

VALUING THE VOTE:  
THE REDISTRIBUTION OF VOTING RIGHTS AND STATE FUNDS  
FOLLOWING THE VOTING RIGHTS ACT OF 1965\*

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ABSTRACT

The Voting Rights Act of 1965, called one of the most effective pieces of civil rights legislation in U.S. history, generated dramatic increases in black voter registration across the South. We ask whether the increase in black voting rights was accompanied by an increase in blacks' share of public spending. We exploit a key provision of the Act—removal of literacy tests at registration—for identification. Employing a triple-difference framework over a twenty-year period, we find that counties with higher black population shares in former literacy test states saw greater increases in both voter turnout and state transfers than comparison counties in non-literacy test states, a finding that is consistent with models of distributive politics. *JEL* Codes: D72, H7, I2, J15, N32

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## I. INTRODUCTION

The Voting Rights Act of 1965 (VRA) has been called one of the most effective pieces of civil rights legislation in United States history (Grofman and Handley 1998). The passage and enforcement of the Act dismantled barriers—chiefly literacy tests—that had impeded southern blacks from registering to vote since the 1890s.<sup>1</sup> Southern states that had employed literacy tests saw their black voter registration rates increase an average of 67 percent (from 33.8 to 56.5 percentage points) between 1964 and 1968. In comparison, southern states without literacy tests saw an average increase in black registration of 19 percent (from 60 to 71.4 percentage points) over the same period (Valelly 2004, p. 4).

While the initial increases in black voter registration and later increases in black office holding are noteworthy (Grofman and Handley 1998; Washington 2012), hopes for the VRA were much greater than allowing blacks entrée to voting booths or even elected offices. The franchise was viewed as the gateway to advancement in other aspects of life (Button 1989). “Voting is the foundation stone for political action,” Martin Luther King, Jr. (1965) wrote just months before the Act’s passage. “With it the Negro can eventually vote out of office public officials who bar the doorway to decent housing, public safety, jobs and decent integrated education.” Consistent with Dr. King’s prediction, models of distributive politics suggest that black enfranchisement through the VRA should have strengthened incentives for state elected officials to channel resources toward black communities because of their newfound power to affect election outcomes. And prior history points to a link between the black franchise and black public resource receipt. When black men were granted the constitutional right to vote after the Civil War, for example, blacks saw gains in school funding, civil rights, and labor legislation (Valelly 2004). On the other hand, after Reconstruction, blacks not only lost the ability to vote but also experienced sharp declines in school resources (Margo 1990; Valelly 2004; Naidu 2012).

In this paper, we examine whether the expansion of black voting rights resulting from the VRA’s elimination of literacy tests increased black public resource receipt. To date, the evidence on this question

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<sup>1</sup> We define the South to include the eleven states of the former Confederacy: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

is anecdotal. The historical record suggests that even staunch segregationist politicians in the South, like George Wallace and Strom Thurmond, started to court the black vote following the VRA. And case studies document improvements in street paving, garbage collection, and fire and police services in black neighborhoods in the post-VRA period (Keech 1968; Button 1989). We examine whether this anecdotal evidence is reflective of a causal impact of voting rights on public goods receipt. Our focus is on transfers from state governments to local governments for the provision of local public goods—chiefly education—that civil rights activists thought critical to black advancement.

Our empirical strategy takes advantage of the fact that, despite its name, the southern literacy test was more aptly characterized as a test of skin color than of literacy. The elimination of this test therefore should have had a greater positive impact on enfranchisement in southern counties with higher black population shares. Accordingly, we test for shifts in the distribution of state funds toward counties with higher black population shares in states that had literacy tests—the “treatment” states—from before the VRA to after its passage. To account for the possibility that state funds would have been redistributed toward these communities in the absence of the legislation, we use counties in southern states without literacy tests but with histories of slavery and black disenfranchisement to form a comparison group. While we can never prove our identifying assumption, the treatment and comparison states show similar pre-VRA trends in the relationship between county black population share and per capita state transfers, and the relatively large change in the geographic distribution of state transfers in treatment states is closely timed with the VRA’s passage.

We find that that for each ten percentage point increase in a county’s 1960 black population share, the elimination of the literacy test generated nearly a six percent increase in per capita state transfers over the decade and a half following the VRA. For the average county in a literacy test state, this amounted to a 16.4 percent increase in per capita transfers over the period. This is an economically significant effect, given that nearly 40 percent of local government revenue in the South prior to the VRA came from the state. Although this finding could reflect other (than the suspension of the literacy test) sources of civil rights era pressure on state officials to redirect funds toward areas with higher black

population shares, this finding is substantively unchanged when we allow state transfers to treatment counties to have been more affected by school desegregation, black political activism, and legislative redistricting than transfers to comparison counties with similar black population shares. And although the finding could reflect transfers to non-black (rather than newly enfranchised black) residents living in high-black-share areas, our results are robust to restricting attention to areas where politicians had relatively weak incentives to direct resources to whites. In addition, the same areas that saw increases in transfers also saw increases in the share of black teenagers enrolled and the quality of black schooling. Also consistent both with enfranchisement as the cause of the increased transfers and with previous work (Filer, Kenny, and Morton 1991), we find relatively large and sustained increases in voter turnout in treatment counties with higher black population shares after 1965.

In sum, this paper makes two main contributions. First, our findings complement previous empirical research (e.g., Margo 1990; Jones, Troesken, and Walsh 2012; Naidu 2012) showing that black disenfranchisement starting in the late 19<sup>th</sup> century contributed to reductions in black political power and black receipt of public goods, namely school resources. Instead of focusing on *disenfranchisement*, however, we focus on the *re-enfranchisement* of blacks that happened more than a half century later—a question that has not been addressed to date. Our findings are, however, likely a lower bound on the impact of the VRA as a whole, since our empirical strategy is based on comparisons of southern counties differentially affected by only one provision of the VRA. Outside of the removal of literacy tests, most of the remaining provisions of the Act applied to all counties in the South, and indeed the nation, and may have had substantial impacts that cannot be quantified by our approach.

Second, we provide evidence on an unexplored question in political economy. Previous research on state budgets and enfranchisement, such as Husted and Kenny (1997), Lott and Kenny (1999), and Miller (2008), has focused on how the expansion of the franchise increased the *level* of state spending on programs preferred by those newly eligible to vote. By contrast, we focus on the *distribution* of that

spending.<sup>2</sup> We demonstrate a link between black enfranchisement and black state resource receipt that, as we detail in the next section, is consistent with models of distributive politics.

## II. THEORETICAL MOTIVATION

There are two theoretical channels by which black enfranchisement could have increased the share of state resources targeted to black communities. First, models of identity politics would predict that black voters helped to elect black representatives who redistributed to them because of shared ideology.<sup>3</sup> However, while the number of black state elected officials was on an upward trajectory throughout our sample period, sizable increases in their ranks were slower in coming.<sup>4</sup> In fact, gains in black office holding have more often been attributed to redistricting rule changes that lagged the initial passage of the VRA by as much as 25 years (Handley and Grofman 1994). In regards to the politicians who would have had dominion over state budgets during our sample period, there were no black governors in the South and a very limited presence of black state legislators. What gains were made in terms of black legislators by 1980 were actually greater on a per capita basis in non-literacy test states than in literacy test states.<sup>5</sup>

The second and more applicable theoretical channel is drawn from the distributive politics literature (see, for example, Cox and McCubbins [1986]; Lindbeck and Weibull [1987]; Dixit and Londregan [1996, 1998]), which suggests that black enfranchisement following the VRA should have (weakly) increased public resources flowing to black communities. In these models, politicians or parties distribute resources to clearly identifiable constituent groups in order to maximize votes. Whether the politician should direct more resources to her core supporters or to swing voters is an ongoing debate that

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<sup>2</sup> Our paper is also related to work showing an association between local turnout and government transfers. See, for example, Fleck (1999), Martin (2003), and Strömberg (2004). The crucial distinction between this line of work and our own is that, rather than measure the impact of turnout, we seek to isolate the impact of enfranchisement, which leads to permanent shifts in both actual and, perhaps more importantly, *potential* turnout, a key consideration for politicians in their decision making.

<sup>3</sup> Chattopadhyay and Duflo (2004) and Pande (2003) demonstrate empirically that representatives' personal ideology, proxied by gender and ethnicity, impacts the distribution of public goods.

<sup>4</sup> This slow growth was due in part to barriers (such as redistricting rules) erected by white politicians to prevent enfranchised blacks from electing black representatives. See, for example, Trebbi, Aghion, and Alesina (2008).

<sup>5</sup> Data on black state legislatures from Handley and Grofman (1994) and U.S. Commission on Civil Rights (1968). We focus on state officials because we are interested in the distribution of state resources. While black municipal officials or black school board members may have lobbied for state resources to be directed to their constituents, blacks were also poorly represented amongst local officials. For instance, in 1976, the ratio of the share of total black elected officials—county, municipal, law enforcement, and education officials, in addition to legislators—to black population was 0.15 in states where literacy tests were removed due to the VRA and 0.08 elsewhere in the South (Joint Center for Political Studies 1976).

turns in part on various groups' voting response to political resource receipt. But whether the politician should direct resources to the enfranchised is not in question.

Blacks in the South, following the passage of the VRA, were theoretically an attractive and easily targeted interest group for political patronage. Blacks were both geographically identifiable and tended to vote cohesively (Keech 1968). Blacks also likely had relatively high marginal utility from school, road, or other neighborhood improvements. And the fact that blacks did not comprise a majority of the electorate would not have precluded a causal relationship between their voting eligibility and public goods receipt. Unlike in legislative voting, in which politicians must take a single side of an issue, thereby disappointing voters with the opposing view, politicians may distribute resources such as school and road improvements to several constituent groups in order to build a winning coalition.<sup>6</sup>

While data limitations prevent our proving the distributive politics channel, to the exclusion of all other possible channels for an increase in black communities' receipt of public goods post-VRA, there is much anecdotal evidence in support of it. Following the VRA, some white politicians served white segregationist constituents with rhetoric, while simultaneously and quietly serving the interests of black constituents with goods and services.<sup>7</sup> Other politicians declared the end of their segregationist stance publicly. The most notable examples are Governor George Wallace of Alabama and Senator Strom Thurmond of South Carolina. Wallace infamously declared in his first inaugural address in 1963, "Segregation now! Segregation tomorrow! Segregation forever!" (Leshner 1994, p. 163). In his 1971 inaugural, by contrast, he proclaimed, "Alabama belongs to us all—black and white, young and old, rich and poor alike" (Leshner 1994, p. 457). During that term, ushered in by blacks marching in the inaugural parade for the first time (Leshner 1994), Wallace was:

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<sup>6</sup> The Voter Education Project (1966) argues that, as soon as 1966, southern blacks were marginal voters, potentially important for such coalitions, in a region that was increasing in political competitiveness. The importance of black voters was not lost on black leaders. Said then-Congressman Andrew Young (D-GA) to the Association of Black Mayors in 1974, "And when you need sewer money or money for rural development or money for any kind of program, the first man you should go to is your congressman, because he sees you as a potentially tremendous ally and also as a tremendous threat. Whether you realize your power or not, especially in Democratic areas, any time blacks don't go out and support the party, Republicans now will take over in the South, and these southern Democrats know that, and they know that they can't afford to alienate any of the emerging new black leadership, and they are anxious to serve the needs of your communities" (Bass and De Vries 1976, 46).

<sup>7</sup> Bass and De Vries (1976, p. 149) cite Senator Nunn of Georgia as an example. Black state representative Bobby Hill of Savannah said of Nunn's use of race in his electoral campaigns, "I know when we close the door and get in a smoke-filled room that we can count on him. And I also know that he's got to win for us to [benefit]. And so I understand that."

...actively courting black voters, crowning a black homecoming queen at the University of Alabama and telling a biracial conference of mayors 'we're all God's children. All God's children are equal.' In 1982, Strom Thurmond of South Carolina, the man who led the filibuster against the 1957 Civil Rights Act and who had previously opposed all such legislation, cast votes for extending the Voting Rights Act and making the birthday of Martin Luther King Jr., a national holiday (Swain 1992, p. 293).

Bass and De Vries (1976, p. 12) document the rise in the 1970s of a new type of southern white Democratic governor, who "showed varying degrees of responsiveness to the interest of blacks." One of the new governors, Edwin Edwards of Louisiana, summed up the increase in black political power by arguing that the VRA:

...provided the vehicle to register hundreds of thousands of blacks in the South, and that provided the catalyst for something far more important, black power at the polls... making white politicians sensitive to their needs and desires. That, of course, has served to elevate the status of the black, not only the quality of his schools, but the quality of his roads, and sewer systems and water systems and housing conditions in which he was living (Bass and De Vries, 1976, p. 11-12).

We empirically test the validity of Edwards's assertion.

### III. HISTORY OF BLACK VOTING RIGHTS IN THE SOUTH

Following Reconstruction, southern states developed legal measures that curtailed the voting rights granted to black men by the 15<sup>th</sup> amendment.<sup>8</sup> Beginning in 1890, each of these states enacted a combination of elaborate registration systems, multiple voting-box arrangements, all-white primaries, poll taxes, and literacy tests, among other creative legislation, that prevented blacks from participating in local, state, and federal elections. While historians debate whether the legislation was motivated by racism or partisanship, and whether the resulting disenfranchisement of some poor whites was intentional or not, there is no debate over whether blacks were the primary targets of suffrage restriction (Kousser 1974). In fact, those in favor of such legislation proclaimed their intentions: "I told the people of my county before they sent me here that I intend ...to disenfranchise every negro that I could disenfranchise under the Constitution of the United States, and as few white people as possible," said one participant at Virginia's 1901-1902 Constitutional Convention (Keyssar 2000, 113). The targeting was effective: the

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<sup>8</sup> Initially, these rights were limited extra-legally, through violence, intimidation, and voter fraud. Key (1949), in fact, argues that the legal measures had little effect, since blacks were already *de facto* disenfranchised.

percentage of southern blacks registered to vote remained in the single digits for the next fifty years (Keyssar 2000).

While each southern state enacted a variety of anti-suffrage laws at the turn of the 20<sup>th</sup> century, by 1960, primarily because of federal intervention, there remained only two major legal impediments to voting in the South: the poll tax and the literacy test. Our identification strategy exploits the removal of the literacy test, by far the larger remaining obstacle to the black franchise. Valelly (2004) points to the falling real value of the poll tax and the fact that six of eleven southern states (four of which also had literacy tests) eliminated these taxes before the federal government forcibly eliminated them as evidence of their declining significance.<sup>9</sup> By the late 1950s, the Leadership Conference on Civil Rights—an umbrella organization of civil rights groups across the nation—no longer gave poll tax elimination its “top priority.” Said Arnold Aronson, conference secretary in 1966, “We all recognize that the poll tax is no longer the major impediment to Negro voting and the barriers imposed by literacy tests...are far more significant” (Lawson 1976, p. 145).

The southern literacy test, on the other hand, was a significant impediment to black voting for the seventy plus years of its existence.<sup>10</sup> At the turn of the twentieth century, with southern black illiteracy high, the literacy test was an efficient legal means of disenfranchising the black population. Over time, black literacy increased, and the southern legal disenfranchisement regime suffered assaults (e.g., the Supreme Court declared the grandfather clause unconstitutional in 1915; the all-white primary met the same fate in 1944).<sup>11</sup> In response, states tightened their literacy test requirements, adding tests of character, citizenship, and interpretation. The literacy test endured: all seven southern states that ever

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<sup>9</sup> The 24<sup>th</sup> amendment outlawed poll taxes in federal elections. The Supreme Court case *Harper v. Virginia State Board of Elections* (1966) outlawed poll taxes in state elections.

<sup>10</sup> Literacy tests outside the South were targeted at immigrants instead of blacks, were more fairly administered, and disenfranchised a much smaller share of the population. “In New York and Massachusetts, an illiterate immigrant could gain the franchise by learning to read; for a black man in Alabama education was beside the point” (Keyssar 2000, p. 170). Literacy tests outside the South were not outlawed by the 1965 VRA, but rather by its 1970 re-authorization.

<sup>11</sup> To avoid disenfranchising illiterate whites, states adopted exception clauses to their literacy test requirements. Understanding clauses allowed a man who understood (as judged by the local examiner) a passage read to him to qualify to vote; grandfather clauses permitted those whose ancestors could vote to register without sitting for the literacy test at all.



adopted a literacy test—Alabama, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Virginia—retained the restriction until its forcible removal by the federal government through the VRA.

The endurance of both the test and its efficacy, in the face of rising black literacy rates, is largely attributed to the test's local administration, which "opened the way for discretionary abuse, which was, in fact, the whole point" (Valelly 2004, p. 127). Blacks with college educations were deemed illiterate by white registrars with less than a high school education (*Voices of Civil Rights* 2006); blacks with law degrees were told that their interpretations of legal terms were inadequate (Valelly 2004). In fact, when the United States Commission on Civil Rights held open hearings on disenfranchisement in Alabama in 1958, of 33 black complainants, 26 had high school diplomas, and 10 of those had college degrees while 6 of those had doctorates (Lawson 1976). Registrars failed black applicants for mispronunciations, misspellings, failure to calculate age to the exact day, and poor moral character (United States Commission on Civil Rights 1961). Blacks were asked such nebulous questions as "Who was the Creator?" and "Are all people born alike?" (United States Commission on Civil Rights 1961). A tale went around the black community of a registrar asking a black man the meaning of "habeus corpus." The man replied, "That means this black man ain't gonna register today" (Lawson 1976, p. 86).

Southern blacks understood that education was not a sufficient condition for passing a literacy test. And so did President Lyndon Baines Johnson. Shortly after the March 1965 "Bloody Sunday" televised beating of civil rights activists peacefully marching from Selma to Montgomery, the president introduced voting rights legislation in his "We Shall Overcome" speech to Congress. "And even a college degree cannot be used to prove that [the black applicant] can read and write. For the fact is that the only way to pass these barriers is to show a white skin," the president stated (Johnson 1966, p. 282). Five months later, southern literacy tests were outlawed with the signing of the VRA. The Act, which authorized the president to send federal examiners to register blacks directly, was implemented immediately. And as we noted in the introduction, in the four years from 1964 to 1968, black registration rates in former literacy test states increased nearly 23 percentage points, double the percentage point

increase in the non-literacy test southern states of Arkansas, Florida, Tennessee, and Texas (Valelly 2004, p. 4).

In addition to outlawing the southern literacy test, the VRA forbade discrimination in voting procedures and required selected jurisdictions to receive “preclearance” —approval from the U.S. Department of Justice—for any proposed change in electoral procedure (Section V). Preclearance was aimed at preventing the type of morphing of literacy test laws that happened after the federal government struck down other anti-suffrage legislation. Until the recent Supreme Court ruling in *Shelby County v. Holder* (2013), the seven former literacy test states, as well as Florida and Texas, were subject to preclearance, an important consideration for our analysis.<sup>12</sup> However, during our period of study, the nondiscrimination requirements, like the vast majority of the VRA, applied to all southern states, and, in fact, to all states across the nation, as they continue to today.

#### IV. DATA

Our goal is to ascertain how black enfranchisement following the VRA’s elimination of the southern literacy test affected the distribution of public resources by governors and state legislators. To this end, we test for within-state shifts in the distribution of public resources toward communities with higher black population shares after passage of the VRA—communities where removal of the literacy test should have had a relatively large impact on population enfranchisement. This test requires community-level data on public resources received, voting rights, and demographics.

##### *IV.A. Data on State Transfers to Localities*

Our measure of public resources is per capita transfers from state governments to local governments.<sup>13</sup> The Census of Governments (COG) has collected this information every five fiscal years for decades. We focus on the years 1957 through 1982, a period over which state transfers to local

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<sup>12</sup> The *Shelby County v. Holder* (2013) decision ruled that the Section V coverage formula (actually in Section IVb of the law) was unconstitutional. That formula made jurisdictions subject to preclearance based on past use of an illegal device (e.g., literacy test or in the case of Florida and Texas, failure to provide Spanish language voting materials) and low voter turnout in elections in the 1960s or 1970s.

<sup>13</sup> That is, these state transfers are designed to offset expenditures by local governments, not private individuals.

governments made up about one-third of total state expenditures in the South. One strength of state transfers to local governments as an outcome is that the recipients of these transfers are geographically identifiable, making the measure suitable for a test of distributive politics. Another is the importance of these state transfers for the funding of local public goods, like education, thought to be critical to black economic advancement. In the pre-VRA period, state transfers accounted for about 37 percent of general revenue for local governments in the average southern state (U.S. Department of Commerce 1957, 1962); throughout the sample period, on average 73 percent of these transfers were for education, while general spending and highway funds each constituted 10 percent of the average state total.<sup>14</sup>

We use the COG county area files, which report state transfers to local governments (e.g., counties, municipalities, townships, school districts, and special districts) aggregated to the county level. An advantage of using the county as our unit of analysis is that counties are not political units with endogenous boundaries. County boundaries essentially remain fixed across our 25-year sample period.<sup>15</sup> Another advantage of the county area file is that consistent data are available for all states in the South. Since the structure of local government varies across the South, it would not be possible to use the jurisdiction-level COG without losing data for some states.<sup>16</sup>

We posit that the mechanism linking the VRA to increased resources for black communities was individual enfranchisement. We therefore weight our regressions by 1960 population so that they yield the impact of the removal of literacy tests on the average person. As shown in Table I Panel A, the weighted mean per capita state transfer to local governments in the seven (treatment) states that had literacy tests removed by the VRA was \$359 (2009 dollars) before the VRA (average of the 1957 and

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<sup>14</sup> The remaining transfers were for welfare, health and hospitals, law, sewerage, non-highway transportation, and miscellaneous (U.S. Department of Commerce 1957, 1962, 1977a, and 1982a).

<sup>15</sup> In Virginia, some independent cities and counties combine or split up over time. In these cases, we aggregated the data to the largest unit to which the county or city was party over the sample period. That is, we aggregated data to C if it was created out of a merger of A and B, or if A and B were created from C over the sample period. A history of these reorganizations is available at: [http://publications.newberry.org/ahcbp/documents/VA\\_Consolidated\\_Chronology.htm#Consolidated\\_Chronology](http://publications.newberry.org/ahcbp/documents/VA_Consolidated_Chronology.htm#Consolidated_Chronology). Our estimates are quantitatively similar when these observations, or even the entire state of Virginia, are dropped from the sample.

<sup>16</sup> Most notably, school districts in North Carolina, Tennessee, and Virginia are dependent on higher levels of government. We would therefore lose these states in a school district-level analysis using the COG data.

1962 figures) and \$765 after the VRA (average of the 1977 and 1982 figures).<sup>17</sup> The figures were \$319 and \$670, respectively, in the four other southern states that form our comparison group. Because we are interested in within-state changes in the distribution of this aid, and states vary in their average aid levels, we use the natural log of per capita state transfers in our regression estimation. The growth rate of real per capita state transfers over the twenty-year period was, on average, 81 percent in states with literacy tests and 74 percent in the remainder of the South. Consistent with the targeting of these transfers for education, there were dramatic increases in real per capita education spending across the South over this period, and like the increases in state transfers, the increases in spending were larger in literacy test states.

One complication to the COG state transfer variable is that not all funds that the state reports transferring to local governments actually originate with the state. Some federal “pass-through” money—funds that the federal government provides to local governments through states—is included. In the average southern state, federal pass-through funds accounted for 10 to 11 percent of state transfers in 1957 and 13 to 25 percent of transfers in 1977.<sup>18</sup> To the extent that state governments had discretion over how to distribute these federal funds, their inclusion in transfer totals will not bias our estimates of the amount that state officials decided to transfer to each locality. Rather, the concern is that the flow of nondiscretionary dollars may be correlated with enfranchisement resulting from suspension of the literacy test. Unfortunately, data limitations prohibit subtracting nondiscretionary federal funds from our dependent variable. County-level federal transfer data by program are only available after the VRA and are aggregated across transfers made directly to local governments and transfers funneled through states. Consequently, subtracting off these measures could introduce considerable (non-classical) measurement error into our dependent variable.

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<sup>17</sup> Our estimation sample includes all but 29 counties (aggregated to account for the consolidations and splits in Virginia) in the eleven southern states. We omit two counties for which we lack control variables. We also trim the sample to exclude counties where in any of the years under study, the natural log of presidential turnout or the natural log of per capita state transfers is more than four standard deviations from the state-by-year specific mean of the variable. Doing so decreases the number of literacy test counties remaining in the sample by 13 (from 638 to 625) and the number of non-literacy test counties remaining in the sample by 14 (from 491 to 477). Trimming slightly reduces the estimated elasticity of transfers with respect to turnout and increases its precision. Counties not included in the estimation sample are listed in the Data Appendix.

<sup>18</sup> These are the authors’ calculations based on U.S. Department of Commerce (1957, 1977a). State-level funding data are provided by program, and some programs include both state and federal funding. Our lower bound estimates exclude programs with both a state and federal funding component, while our upper bounds include such programs.

We instead pursue a control-based approach that relies entirely on pre-VRA measures of county characteristics. During our sample period, the three largest federal pass-through programs were authorized under the National School Lunch Act of 1948 (NSLA), Title I of the Elementary and Secondary Education Act of 1965 (ESEA), and the Comprehensive Employment and Training Act of 1973 (CETA). Together, these programs accounted for 75 to 83 percent of pass-through money. These programs targeted localities based on need. Counties with higher child poverty rates received more funds under the NSLA and the ESEA; counties with higher unemployment rates received more CETA funds. To eliminate bias due to the correlation between black share and these measures of need, we control flexibly for both the 1960 child poverty rate and the 1960 unemployment rate in our baseline specification.<sup>19</sup>

#### *IV.B. Data on the Local Electorate*

Ideally, we would start with a “first stage” demonstrating the impact of literacy tests on voter enfranchisement, or *potential* voter turnout. We unfortunately cannot use voter registration by race as a proxy, since registration data are both infrequent and missing for a large number of southern counties (mainly entire states), particularly in the post-VRA period. As a substitute, we consider actual voter turnout as a share of the voting age population at the county level, drawing on data spanning the years 1952 to 1980.<sup>20</sup> The advantages of these data are their universal availability and consistent measurement over time; the key drawback is that turnout is not reported by race, and some historical accounts suggest that some whites were disenfranchised by literacy tests (Kousser 1974). However, state-level estimates of registration rates by race suggest larger increases in registration among blacks than among whites residing in literacy test states from before to after the passage of the VRA.<sup>21</sup> Existing county-level registration data by race (from administrative records and from surveys) also point to a larger impact of the VRA’s

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<sup>19</sup> An additional complication to these data is that in two states—North Carolina and Virginia—state transfer data appear to include transfers not simply to local governments, but also directly to the aged, poor, blind, and disabled individuals. Our state transfer estimates get slightly larger when we drop these two states from the sample.

<sup>20</sup> Turnout data come from Matt Gentzkow and Jim Snyder and from various editions of *America Votes*.

<sup>21</sup> For example, the increase in the black-white registration rate difference between 1964 and 1968, for literacy test states relative to non-literacy test states, was 15 points (21 percentage points versus 6 percentage points). Between 1960 and 1980, this figure is 25 points (25 percentage points in literacy test states and 0 percentage points in non-literacy test states). These are the authors’ calculations, weighted by state population, using data obtained from the U.S. Department of Commerce (1972, 1977b, 1982b, 1983, 1987, and 1990).

removal of literacy tests on black registration than on white registration.<sup>22</sup> Although we cannot rule out empirically that the elimination of the literacy test increased white enfranchisement in absolute terms, the available data thus suggest that it increased less than black enfranchisement in relative terms.

County-level data on aggregate turnout are available for both presidential and gubernatorial elections. Turnout in presidential elections provides the best available measure of enfranchisement, since turnout in presidential elections is higher than in any other electoral contest. Nonetheless, given our focus on state transfers, which are controlled by state elected officials, we also consider turnout rates for gubernatorial elections as a proxy for enfranchisement in state elections. In addition to their lower turnout, a second limitation of gubernatorial elections as a proxy for enfranchisement is their variability. Because these elections vary across states and years in their timing, their procedures, and their competitiveness, they are more difficult to compare across localities than presidential elections, in which the whole country chooses from the same candidates on the same day. Our focus will thus be on presidential turnout, though our conclusions are substantively unchanged if we use gubernatorial turnout.

Table I Panel B presents summary statistics on voter turnout by the presence of a literacy test, again weighting by 1960 county population. As expected, turnout was lower and more variable in gubernatorial elections during the study period. Consistent with the impacts of literacy tests previously estimated applying differences-in-differences to state-by-year data (e.g., Besley and Case 2003), states with literacy tests saw relatively large gains in turnout over time.

#### *IV.C. Other County Characteristics*

We draw from a number of other data sources (described in the Data Appendix) to construct controls. These variables are summarized in Table I Panel C, again weighting by 1960 county population. In 1960, counties in states with literacy tests on average had higher black population shares (28.9 percent versus 15.1 percent elsewhere in the South), higher child poverty rates (24.5 percent versus 17.6 percent),

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<sup>22</sup> Alt (1994) uses pre-VRA county level registration data by race to show that the literacy test increased the numerical advantage in registration of whites over blacks as an increasing function of the county's black population share. It stands to reason that the elimination of literacy tests would have eroded this white advantage, but it cannot be confirmed due to the lack of post-VRA registration data by race at the county level. Likewise, using survey data spanning the years 1952 to 1984, Stanley (1987) shows that literacy tests were significantly negatively related to self-reported voting participation by blacks but not whites.

and lower high school completion rates (32.2 percent versus 37.6 percent), but lower unemployment rates (4.9 percent versus 5.3 percent). In 1976, the year closest to the end of our study period with data available, counties in literacy test states were more likely to be under court order to desegregate (50.4 percent versus 41.9 percent) and to receive funds under the Emergency School Aid Act of 1972 (ESAA) (39.3 percent versus 35.0 percent), a federal program designed to facilitate racial integration of schools. While counties in both literacy test and non-literacy test states were almost equally likely to be home to a black college, NAACP chapters were more often located in counties in states without literacy tests, and other black organizations were more frequently found in counties in states with literacy tests. Suggesting more resistance to civil rights advances, county vote share for segregationist Strom Thurmond in the 1948 presidential election was higher in literacy test states (29.9 percent versus 8.3 percent).

The counties in literacy test states therefore differed from counties elsewhere in the South in terms of observable characteristics, and significantly so in a number of cases. However, most of the differences in average county characteristics between the treatment and comparison states can be explained by differences in 1960 black share and child poverty alone. Indeed, in our application below of inverse propensity score weighting, to give more weight to those counties in comparison states that look like counties in treatment states, we employ a parsimonious specification, using only these two variables, which substantially mitigates all—and renders insignificant most—of the treatment-comparison differences in average county characteristics. Yet, reweighting does not substantively affect our findings.

## V. ENFRANCHISEMENT AND STATE TRANSFERS

### *V.A. Empirical Strategy*

In principle, literacy tests should have been administered to all applicant registrants, but the historical record suggests that they were applied disproportionately to blacks, as described in Section III. A transparent approach to estimating the impact of literacy tests on the within-state distribution of state transfers is therefore to explore how the relationship between preexisting (e.g., 1960) county black share and transfers changed over time within states where literacy tests were forcibly removed by the VRA. If

literacy tests had an impact, we would expect to see a change in the slope coefficient on black share around 1965, i.e., a shift in the distribution of state transfers toward areas with higher black population shares. We should also observe a similar shift in voter turnout to reflect the change in the distribution of the electorate, as documented in previous work (Filer, Kenny, and Morton 1991).

One problem with this approach is that areas with higher black shares may have seen increases in state funding and enfranchisement even in the absence of literacy tests being removed by the VRA. For example, civil rights activism, either directly or through its impact on black enfranchisement, may have yielded rewards in the form of more state aid for localities with higher black shares. School desegregation in the South, which began in earnest after passage of the Civil Rights Act of 1964 and the 1965 ESEA (Cascio et al. 2008), was also associated with larger state funding increases for school districts with higher black enrollment shares (Johnson 2011; Reber 2011).

We therefore combine the strategy described at the start of this section with the use of a comparison group. That is, we test whether there were *larger* post-VRA shifts in the distribution of state transfers toward counties with higher black population shares in treatment states than in a group of comparison states. Likewise, we should document *larger* post-VRA gains in turnout for counties with higher black population shares in treatment states, reflecting enfranchisement. As noted above, we limit the comparison group to counties in the four southern states that did not use literacy tests but did have histories of slavery and of black disenfranchisement—Arkansas, Florida, Tennessee, and Texas.<sup>23</sup> While the comparison counties on average had lower black population shares in 1960 (Table I Panel C), there is significant variation in the geographic distribution of the black population within each region that can be leveraged for identification. Moreover, our findings are robust to trimming the sample to create greater common support in black share.

#### *V.B. Event-Study Estimates*

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<sup>23</sup> We thus limit our comparison states to southern states that, like the literacy tests states, are former members of the Confederacy. Our results are substantively unchanged when we include Delaware, Kentucky, Maryland, and West Virginia—states that fall under the broader census definition of South—as comparison states.



To set ideas, Figure I shows 1960 population-weighted estimates of the coefficient on 1960 black population share from regression models predicting county voter turnout and per capita state transfers, separately by year and treatment status, based on the full sample. Supplemental Appendix Figures I and II show the underlying scatterplots.<sup>24</sup> The models also include state indicators, to facilitate a within-state interpretation of the coefficient on black share, as well as controls for the 1960 child poverty rate and the 1960 unemployment rate to account for federal pass-through funds.

Because our identification strategy relies on a larger increase in the share of the population eligible to vote in higher black share counties of treatment states, it is useful first to consider the estimates for voter turnout rates in presidential elections, our preferred measure of enfranchisement. The solid circle at about -0.01 for 1952 in Figure I Panel A indicates that, in states with literacy tests, each percentage point increase in county black share was associated with a one percent decrease in the turnout rate for the 1952 presidential election. In comparison states (hollow circles), the slope coefficient on black share is also negative in 1952, but not as steep, as might be expected given fewer restrictions on the black franchise in these states at this time. These slopes remain quite stable through the 1960 election, but flatten out in the treatment and comparison regions alike in 1964, the last election before the VRA.<sup>25</sup> The fact that the relationship between black share and voter turnout was already weakening prior to the VRA points to the need for a comparison group, and the co-movement of this relationship in the treatment and comparison states in the pre-VRA period supports the suitability of the comparison states that we have chosen.

The effect of the VRA on voting eligibility is then seen as we move from the 1964 to the 1968 presidential election, the first held after the enactment of VRA. While in the elections prior to the Act, the slope coefficient on black population share for the treatment states was consistently 0.006 or 0.007 log points below that for the comparison states, the gap closed by about half to 0.003 log points in 1968 and

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<sup>24</sup> All Supplemental Appendix materials are available online.

<sup>25</sup> Wright (2013) notes that, beginning in 1962, the Voter Education Project, a coalition of five major civil rights organizations coordinated by the Southern Regional Council, supported local groups in a mass effort throughout the South that registered 700,000 new voters in two-and-a-half years. The shallower slope on black share in 1964 may reflect these registration efforts.

was smaller still from 1972 forward. Once literacy tests were removed, the treatment-comparison difference in the relationship between black share and turnout narrowed. That the difference remained smaller suggests that the outlawing of the literacy test led to permanent gains in turnout by increasing black voting eligibility.

We demonstrate the statistical significance of the closing of this gap in Figure II Panel A. Here, we plot estimates of the coefficients  $\theta_j$  (with 95 percent confidence intervals) from the following event-study model:<sup>26</sup>

$$(1) \quad \ln(y_{cst}) = \delta_c + \gamma_{st} + \sum_{j \neq 1960} \mu_j (\%bl_c \times D_t^j) + \sum_{j \neq 1960} \theta_j (lit_s \times \%bl_c \times D_t^j) + x_{cs}' \beta + \varepsilon_{cst}$$

where  $y_{cst}$  represents the presidential election turnout rate in county  $c$  in state  $s$  in year  $t$ ;  $\%bl_c$  represents percent black in  $c$ 's 1960 population;  $lit_s$  is an indicator variable set to one if state  $s$  had a literacy test that was removed following the VRA, zero else; and  $D_t^j$  is an indicator variable set to one if  $t = j$ , zero else. The model also includes county fixed effects,  $\delta_c$ , to account for fixed differences in turnout across counties, and state-by-year fixed effects,  $\gamma_{st}$ , to account for time-varying, state-specific shocks to turnout stemming from changes in state economic conditions or institutions, such as state rules regarding redistricting, or from the VRA itself.<sup>27</sup> Finally,  $x_{cs}$  is a vector of controls that includes the 1960 unemployment rate and the 1960 child poverty rate, both interacted with year dummies and with year dummies and  $lit_s$  to control for federal pass-through funds. Because the model includes county fixed effects, we omit the interactions with the indicator for one pre-VRA election year (e.g., interactions with  $D_t^{1960}$ ) to identify the model. The coefficient  $\mu_j$  then captures the change in the gradient of turnout in black population share between 1960 and year  $j$  for comparison states, while the sum  $\mu_j + \theta_j$  captures that change for treatment states.

<sup>26</sup> Because the intensity of the treatment varies at the county level (the interaction of the state literacy test laws and the county share black), we cluster standard errors on county.

<sup>27</sup> One mechanism through which the VRA may have affected aggregate state turnout is by increasing state political competition (Besley, Persson, and Sturm 2010). In the analogous regression for per capita state transfers, state-by-year fixed effects capture the impacts of the VRA on aggregate state transfers to local governments.

Figure II Panel A thus presents estimates of the *difference* in the black share gradients in turnout in the treatment and comparison states shown in Figure I Panel A *relative to* the difference observed in 1960.<sup>28</sup> As the pre-VRA (1952, 1956, and 1964) circles indicate, the treatment-comparison differences in the slope coefficients on black share were almost unchanged and statistically indistinguishable from one another in the elections before passage of the Act. There is a significant and sustained change beginning in 1968, however, reflecting the relatively large increases in turnout in counties with higher black population shares in the treatment states. The 95 percent confidence interval bars indicate that the narrowing of treatment-comparison difference in the relationship between black share and turnout from before to after the VRA is highly statistically significant. For gubernatorial turnout (Panel B of Figures I and II), the estimates are noisier but follow a similar pattern.<sup>29</sup>

The first two panels of Figures I and II thus confirm that outlawing literacy tests increased voting eligibility as an increasing function of the county's 1960 black population share. In the final panel of each figure, we turn to our central question—whether this increase in voting eligibility was accompanied by an increase in state transfers received. Figure I Panel C shows that localities with higher black shares received less state funding per capita before the VRA and more so in treatment states. The transfer gap between lower and higher black share counties narrowed in the treatment and comparison states to a similar extent between 1957 and 1962, suggesting that subsequent gains in funding would have been similar for blacks across the South in the absence of the VRA.<sup>30</sup> After the VRA, however, the two series ceased to trend similarly; there were larger increases in the share of transfers going to higher black share counties in treatment states than in comparison states. In fact, by 1972, the pre-VRA pattern had reversed:

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<sup>28</sup> That is, the estimates in Figure II re-normalize the estimates in Figure I so that the difference in the black share slopes between the treatment and comparison states is zero for some specified year prior to the VRA (e.g., 1960 for presidential turnout).

<sup>29</sup> For tractability, we bin gubernatorial elections into four-year periods. Thus, 1953 includes the first gubernatorial election in the state on or after January 1, 1953. In the few states with biennial gubernatorial elections, we use the election following but closest to the beginning of the interval. For consistency with the presidential election results, we omit the interactions with the indicator for the bin that includes 1960 (1957 to 1960) to identify the model.

<sup>30</sup> This is likely the continuation of a much longer-run trend. Starting in the 1940s, school districts with higher black population shares in the South began to benefit relatively more from increases in state aid for education, which, as earlier noted, constituted three-quarters of state transfers to local governments in the South. Initially, these gains in state aid resulted from increases in black teacher salaries associated with NAACP victories in teacher salary equalization cases and tight black teacher labor markets in the South, but they were also undertaken by states in an effort to stave off racial integration of schools (Margo 1990; Donohue, Heckman, and Todd 2002; Ashenfelter, Collins, and Yoon 2006).

counties with higher black shares received relatively more funding in treatment states, but not in comparison states.

*V.C. Long-Difference Estimates*

The graphical evidence is consistent with the elimination of literacy tests having an impact on both enfranchisement and state transfers. To provide a concise means of summarizing magnitudes and of subjecting the estimates to specification and robustness tests, we now move to a triple-difference model. We focus on a model using two data points—one from before the VRA (e.g., 1960) and one well after (e.g., 1980). We omit data from the intervening years for a couple of reasons. First, as suggested by the event-study estimates, funding responses to a change in the electorate may not occur instantaneously due to the timing of elections and following elections, lags in setting a new budget and in that budget’s taking effect. Second, using only data from 1960 and 1980 reduces measurement error in that it does not require interpolation of covariates drawn from the census.

Specifically, we employ a long-difference model of the form:

$$(2) \quad \Delta \ln(y_{cs}) = \gamma_s + \mu \%bl_c + \theta(lit_s \times \%bl_c) + x_{cs}' \beta + v_{cs}$$

where  $\Delta \ln(y_{cs})$  is the growth rate in either turnout rates or real per capita state transfers between (roughly) 1960 and 1980,  $\gamma_s$  is a state fixed effect (a state trend in this difference specification), and  $x_{cs}$  is a vector of controls, which in the baseline specification are the 1960 unemployment rate and 1960 child poverty rate, each entered directly and interacted with  $lit_s$ . All other variables are as previously defined. Model (2) therefore differences model (1) across data points at the start and end of our sample period. Thus, the coefficient  $\mu$  characterizes the (within-state) change in the slope coefficient on black population share in comparison states, and  $\mu + \theta$  does the same for treatment states; the coefficient of interest is again the difference,  $\theta$ . Estimates of  $\theta$  will be identified if, in the absence of the VRA, real per capita state transfers would have grown at the same rate in treatment and comparison states with the same 1960 black population share, adjusting for covariates.

To show how the long-difference estimates relate to the graphical evidence just presented, Table II provides estimates of  $\theta$  from model (2) alongside estimates of the event-study coefficients that were shown visually in Figure II, the  $\theta_j$  from model (1).<sup>31</sup> For the long-difference estimates for per capita state transfers, we average the two years of data at the beginning and the end of our sample period to mitigate noise and better align with the turnout and census data; for turnout, we use returns from the 1960 and 1980 presidential elections and from the gubernatorial elections during the four-year intervals ending in 1960 and 1980 (i.e., 1957 to 1960 and 1977 to 1980). The panels of Table II pertain to the same respective outcomes as the panels in Figure II.

Of particular interest are the long-difference coefficients for state transfers and for presidential election turnout, our preferred proxy for enfranchisement. The significant 0.00587 in column 2 of Panel A indicates that the removal of the literacy test increased presidential turnout by 0.59 percent for each percentage point increase in black population share. This estimate implies that nearly two-thirds of the ten percentage-point increase in presidential turnout in treatment states between 1960 and 1980 (Table I Panel B) can be explained by the removal of literacy tests.<sup>32</sup> Turning to our focal dependent variable, the significant 0.00569 coefficient in the final column indicates that the removal of the literacy test increased per capita state transfers by 0.57 percent for each percentage point increase in black population share. This estimate implies a 16.4 percent relative increase over the twenty-year period for the average treatment county. Normalizing the transfer increase by the increase in presidential turnout formally using a two-stage least squares (2SLS) model, instrumenting presidential turnout growth with  $lit_s \times \%bl_c$ , we estimate that a one percent increase in eligibility-driven turnout increased county residents' state resources by 0.97 percent, or, more succinctly, that the elasticity of transfers with respect to enfranchisement is approximately one. An elasticity of unity in this case is not inherently meaningful, but does provide a useful benchmark for comparing estimates across specifications.

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<sup>31</sup> We continue to weight the long-difference models by 1960 county population. Standard errors are heteroskedasticity-robust.

<sup>32</sup> The percent black main effects are also positive, and economically, as well as statistically, significant, shown in Supplemental Appendix Table I. As treatment counties have a higher black share on average, the general increase in voting access for blacks throughout the South during this time period is also likely a factor in narrowing the gap.

We show this 2SLS estimate in the first column of Table III, after repeating the estimates of  $\theta$  from model (2) for the presidential turnout rate and real per capita state transfers, respectively.<sup>33</sup> The remainder of Table III examines the appropriateness of our baseline specification. Full regression results for many of these specifications and those in subsequent tables are provided in Supplemental Appendix Tables I and II. We also provide the corresponding event-study estimates for these new specifications (and for those in all subsequent tables) in the Supplemental Appendix. Because the event-study estimates do not point to different conclusions, we focus only on the long-difference estimates from this point forward in the interest of brevity.<sup>34</sup>

We first test whether modeling the impact of the removal of the literacy test as a function of county black share alone, rather than black share and educational attainment, is justified. We do so by adding the 1960 county high school completion rate to the baseline specification, both directly and interacted with  $lit_s$ , in column 2. While the completion rate is a marginally significant (positive) predictor of the growth in voter turnout (Supplemental Appendix Table I), its interaction with  $lit_s$  is not. In other words, once we control for black share and its interaction with the literacy test indicator, removal of the literacy test is not more predictive of increases in turnout in areas with lower education levels.

Second, we examine whether modeling the impact of the removal of the literacy test as linear in black share is appropriate. For example, there might have been a discontinuity in the effect once blacks comprised half of the population and could have elected a black representative.<sup>35</sup> While counties are not electoral jurisdictions, one way to assess the potential for such an effect is to include an indicator for

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<sup>33</sup> As a point of comparison, the final row in the first column gives the OLS-estimated elasticity of transfers with respect to presidential turnout. This relationship is close to zero and statistically insignificant. There is great variation in turnout from election to election, much of which is explained by state-year swings in competitiveness in the election. We posit an impact on a county's share of resources through permanent changes in eligibility, not through transitory swings in interest. That the OLS estimate is significantly lower than 2SLS suggests that swings in turnout within counties over time do indeed contain a great deal of variation that is orthogonal to the distribution of state resources.

<sup>34</sup> Likewise, triple-difference estimates using all years of data, where the independent variable of interest is the interaction of  $\%bl$ ,  $lit$ , and a post-VRA period indicator, are somewhat smaller in magnitude than the long-difference estimates but largely still statistically significant. The indirect least squares estimates of the effect of VRA-induced enfranchisement on per capita state transfers from these models are also quite similar to the two-stage least squares estimates from the long-difference specification. (Compare the estimates in Tables III to VI to those in Supplemental Appendix Tables IV to VII.)

<sup>35</sup> For instance, whites may have tried harder to continue to suppress black votes in majority black counties. The historical record, however, indicates that, following the VRA, white elites prevented blacks in largely black areas from obtaining political office largely through redistricting.

majority black county population in 1960 and the interaction of that indicator with *lit<sub>s</sub>*.<sup>36</sup> Majority black counties in literacy test states saw smaller increases in turnout (not significant) and transfers (marginally significant) than majority black comparison counties (Supplemental Appendix Tables I and II). So that we do not use this variation in turnout for identification, the 2SLS model includes *direct* effects of both of the majority black variables on state transfers. The 2SLS estimates are larger than we found at baseline and slightly less precise, but we still cannot reject unit elasticity of state transfers with respect to turnout (Table III, column 3). Dropping majority black counties from the estimation sample altogether produces very similar results (Supplemental Appendix Table III, column 2), as does dropping both majority black counties and the counties with very low black population shares that were more common in comparison states (Table III, column 4).<sup>37</sup> In Supplemental Appendix Table III, we also show that the estimates are robust to allowing for a discontinuity at a lower threshold in black population share (37 percent black, the top quartile of 1960 black share in treatment counties; column 3), as well as to dropping counties with black shares above this lower threshold (column 4). Here, we address the concern that blacks in majority black counties were not necessarily more likely to have been placed in majority black electoral jurisdictions, given that white hostility to the election of blacks may have been greater in these counties.

These results suggest that the inclusion of counties with very high and very low black population shares in model identification biases our 2SLS coefficients downward. Thus, in order to be both conservative and concise, we retain the basic linear specification in black share (using the full sample) for the remainder of the paper.<sup>38</sup> However, differences between treatment and comparison counties in mean characteristics besides black share (Table I Panel C) may raise a third concern—that counties in states without literacy tests are not valid comparisons for counties in states with such tests. While our pre-trend

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<sup>36</sup> Population weighted, nearly 10 (1.2) percent of literacy (non-literacy) test counties were majority black in 1960. We obtain substantively similar results when we allow for a discontinuity at some lower threshold in 1960 county black population share, like the 75<sup>th</sup> percentile of the black share distribution of treatment states (37.2 percent), to address that fact that counties that are not majority black might contain majority black jurisdictions.

<sup>37</sup> In column 4, we restrict the sample to counties with a 1960 black share at least as great as the 10<sup>th</sup> percentile of the comparison county distribution (2.2 percent) and no more than the 90<sup>th</sup> percentile of the treatment county distribution (49.9 percent).

<sup>38</sup> Scatterplots of our outcomes of interest against residual 1960 black share (Supplemental Appendix Figures I and II), by year and by treatment status, generally provide further support for the linear specification and where they do not, they suggest that the specifications described above should have adequately accounted for any non-linearity.

analyses in Figures I and II support the validity of our comparison group, we provide further support for our identification assumption in the remaining columns of Table III. First, we trim the sample to include only those treatment and comparison counties where there is common support in the propensity to be in a literacy test state (column 5). Second, we use the estimated propensity scores both to trim and to weight the regression (column 6). The final approach eliminates significant differences between the treatment and comparison states in 1960 county black population share and in eight out of ten of the other county characteristics listed in the first two sections of Table I Panel C.<sup>39</sup> However, our 2SLS estimates are again larger than at baseline.

## VI. ROBUSTNESS

We have found robust evidence that per capita state transfers to counties with higher black population shares grew relatively faster in southern states where literacy tests were removed by the VRA. We postulate that the underlying mechanism was the increase in black voting eligibility associated with the removal of literacy tests. However, to say that the 1960s and 1970s was a period of great change in the South is, of course, a gross understatement. Political activism and federal interventions into education and voting practices in the name of civil rights occurred during this period. In this section, we explore a number of alternative explanations for our findings related first to our particular setting and second to the distribution of state transfers more generally.

### *VI.A. Setting-Specific Predictors of State Transfers*

As discussed earlier, nearly seventy-five percent of funds transferred from states to localities during our study period were earmarked for education. Thus, the first alternative mechanism we consider is funding for a major development in education over our study period—school desegregation. Southern districts with higher black enrollment shares were more likely to desegregate under court order over our

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<sup>39</sup> The differences remain statistically significant in the case of presence of another (aside from the NAACP) black organization and percent vote for Strom Thurmond. We estimate the propensity score using a logit regression of an indicator for a literacy test prior to the VRA on 1960 black share, 1960 child poverty, and the interaction of the two variables, weighting by 1960 population. Using the second method (column 6), we assign comparison counties weights of the odds of being in a literacy test state multiplied by 1960 population.



sample period (Cascio et al. 2008), and school districts that desegregated under court order received more state education aid (Johnson 2011). While we have no reason to believe that counties in treatment states were more likely to be under court order than counties in comparison states with the same black population share, we examine this possibility by controlling for both the percent of the county’s school enrollment in districts under court order to desegregate and the percent of the county’s enrollment in districts receiving federal funds under the ESAA, both measured as late in our sample period as possible (fall 1976). Both controls enter with positive coefficients in the state transfer specification (Supplemental Appendix Table II), but as shown in column 2 of Table IV, the resulting estimates do not substantively deviate from our baseline findings, repeated in column 1 for comparison.<sup>40</sup>

In the wake of school desegregation, politicians in treatment states may have also been motivated to direct more state funds to counties with higher black population shares to “hold harmless” whites. When black and white schools were separate, and blacks were disenfranchised, white school boards tended to expropriate money allocated by the state to black pupils for white schools. Where there were more black students relative to whites, there was greater scope for white pupils to profit (Bond 1934; Margo 1990). As a result, racial gaps in school resources before schools desegregated—and the funding necessary to “level up” spending on black students afterward—tended to be larger the higher a district’s black enrollment share (Reber 2011). In an attempt to rule out this explanation for our findings, we limit the sample to the seven states that appear to have largely abandoned this practice of expropriation by the VRA, as evidenced by relatively weak relationships between the county-level black-white gap in the pupil-teacher ratio in 1968 and 1960 black share, controlling for 1960 child poverty and unemployment rates.<sup>41</sup> This restriction eliminates counties from four treatment states: Alabama, Georgia, Louisiana, and

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<sup>40</sup> The 1976 school district survey data from which we obtained these variables are incomplete for several counties. We determine the share of enrolled students in a county covered by this survey using basic enrollment information from the 1976 Elementary and Secondary General Information System (ELSEGIS), which covers all school districts during the 1976-77 school year. We then weight the specification of Table IV column 2 by 1960 county population times this share. Results are similar when we drop counties where the survey coverage is incomplete (i.e., counties where under 75 percent of pupils are represented).

<sup>41</sup> Ideally, we would have performed this test using race-specific pupil-teacher ratio data (or more generally, race-specific school resource data) from immediately before the implementation of the VRA. Unfortunately, such data are only available for two southern states—Louisiana and Virginia (Cascio, Gordon, and Reber forthcoming)—both of which had literacy tests. Instead, we

South Carolina. Although the estimate in this smaller sample is less precise (column 3), it is similar in magnitude to the full-sample estimates shown in the first column.<sup>42</sup>

Our estimates may also be confounded by political activism. For example, pro-civil rights forces may have been more likely to target counties with higher black shares in literacy test states. On the other hand, state officials may have been more likely to target resources to areas with anti-civil rights activism, again in an effort to appease whites. We investigate these possibilities in column 4. As proxies for civil rights activism, we control for indicators for whether the county had an NAACP chapter, some other black organization, or a black college, all measured in 1960. As a proxy for the strength of anti-civil rights sentiment, we employ the county vote share won by segregationist Strom Thurmond in his 1948 presidential bid. Suggesting that some of the post-VRA increase in turnout was a white protest vote, Thurmond vote share is a strong positive predictor of growth in presidential turnout (Supplemental Appendix Table I). However, the coefficient of interest, that on  $lit_s \times \%bl_c$ , is not attenuated in the turnout specification by the inclusion of this control, suggesting that that the coefficient reflects increases in black enfranchisement. Likewise, while the growth in state transfers is positively and significantly related to the presence of the NAACP or a black college (Supplemental Appendix Table II), controlling for these variables does not affect the coefficient on  $lit_s \times \%bl_c$ . Thus, the 2SLS elasticity estimate is little changed.

The extent of black political power in the South may also have been affected not only by contemporaneous political activism, but also by the vestiges of a major prior event—namely, the mechanization of cotton and the concomitant reduction in reliance of local economies on black labor. Black population share is in fact a stronger predictor of reliance on cotton production in the treatment states than in the comparison states, suggesting that our estimates could be picking up such a phenomenon. To address this possibility, we added the percent of county land devoted to harvested cotton

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estimated race-specific pupil-teacher ratios at the county level from school-level data on total teachers and enrollment by race from 1968, the earliest year in which representative data are available for the South (see Data Appendix for more details).

<sup>42</sup> We weight regressions by 1960 county population times share of a county's enrollment represented in the school survey data used to calculate pupil-teacher ratios in 1976 (determined by comparison with the 1976 ELSEGIS), so as to match the sample in a specification presented in Table VI. Results are similar when we drop counties where the school survey coverage is incomplete (i.e., counties where under 75 percent of pupils are represented). Results are also robust (and actually of greater statistical significance) when we change the sample inclusion rule from the seven states to the four or nine states with the weakest relationships between the black-white gap in the pupil-teacher ratio in 1968 and 1960 black share.

in 1959 ( $\%cot_c$ ) and its interaction with  $lit_s$  to our basic model, thus allowing counties with different levels of cotton penetration as of 1959 to have different trends in outcomes and for these differential trends to vary between the treatment and comparison states. As shown in the last column of Supplemental Appendix Table III, the  $lit_s \times \%cot_c$  interaction enters insignificantly in both the turnout and transfer specifications, and the  $lit_s \times \%bl_c$  coefficients are essentially unchanged.

Two events contemporaneous with our study period that expanded the political power of some segments of the population may also have affected blacks disproportionately. First, the 1962 Supreme Court decision in *Baker v. Carr* required equalization of legislative district populations, both at the state and federal level. Using the same COG data that we use here but for the entire country, Ansolabehere, Gerber, and Snyder (2002) show that counties underrepresented in state legislatures as of 1960 saw relatively large increases in state transfers between 1957/62 and 1977/82, as the representation gap was eliminated. We reproduce this finding (Supplemental Appendix Table II) by controlling for the same measure of initial county representation that they employed—the “relative representation index” (RRI), defined as the number of legislative seats per person in the county, divided by that same figure for the state overall, as of 1960 (David and Eisenberg 1961). However, adding this control for the redistributive effects of mandatory redistricting leaves our estimates substantively unchanged, as shown in column 5.

Second, as explained in Section III, poll taxes were eliminated in three treatment states and two comparison states at almost the same time as literacy tests. We focus on the removal of literacy tests under the VRA because the historical evidence suggests that the poll tax was no longer a significant obstacle to the black vote as of the 1960s. In column 6 of Table IV, we account for possible omitted variables bias due to failure to account for the impacts of the elimination of the poll tax by adding a control for the interaction between an indicator for having a poll tax in the pre-VRA period and 1960 black population share. Our results are again substantively unchanged, consistent with previous research findings that the elimination of the poll tax did not have a disproportionate effect on black voting.<sup>43</sup>

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<sup>43</sup> Filer, Kenny, and Morton (1991) find no evidence that the poll tax had larger effects on turnout in counties with higher nonwhite population shares.

Finally, not only were there events concurrent with the VRA that may have impacted black political power, but the VRA itself consisted of more than just a mandate to cease the use of literacy tests. The VRA brought increased federal scrutiny to the southern electoral process, particularly in states that had employed literacy tests, through the assignment of federal examiners to register voters and oversee elections, and by requiring preclearance by the Department of Justice for any change in voting rules for a large share of southern electoral jurisdictions. Determining whether state politicians were motivated to redistribute to areas with higher black shares because of fear of this federal intervention or fear of black political power may not be possible as these two phenomena went hand-in-hand. Constitutionally, blacks had the right to vote for nearly 100 years before the VRA; what gave blacks newfound political power in 1965 was that right being backed by federal interest and intervention.

Nevertheless, several pieces of evidence suggest that an attempt to avoid federal intervention is an unlikely mechanism for our findings. First, the assignment of examiners was largely limited to the first two years after passage of the VRA.<sup>44</sup> Focusing on a long difference thus allows us to estimate outcomes beyond the period of direct federal intervention. Second, while all of the treatment states became subject to Section V preclearance in 1965, two comparison states—Florida and Texas—became subject to the requirement with the 1975 renewal of the VRA. While it might have taken more than seven years for the effects of Section V in Florida and Texas to reach fruition, one potential way to rule out preclearance as a mechanism is to limit our estimation sample to “covered” jurisdictions. When we do this (final column), the reduced-form coefficient of interest in both the turnout and state transfer specifications nearly doubles. Our 2SLS estimate of the impact of enfranchisement-induced turnout is however unchanged.

#### *VI.B. General Predictors of State Transfers*

The literature on the distribution of public goods acknowledges a role for political considerations in the distribution decision. This literature further argues that demographic characteristics can play a part in determining public goods receipt. For example, the bureaucratic model of decision-making

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<sup>44</sup> Ninety-six percent of those enrolled by federal examiners were enrolled in the first two years after the VRA (Valelly 2004).

hypothesizes that public goods are distributed to the neediest communities based on bureaucratic formulas, not politics. Consistent with this theory, previous literature has found that residents' age and poverty are correlates of a community's public goods receipt.<sup>45</sup> Population growth may also impact per capita funding if budgeting lags population change. Ansolabehere, Gerber, and Snyder (2002) find evidence of this phenomenon in the country overall in the same data used here.

Testing such alternative hypotheses for our findings is potentially fraught, since increases in state transfers toward newly enfranchised blacks could have themselves caused changes in a county's population characteristics. Areas with higher initial black population shares in literacy test states in fact experienced significant shifts in their populations over the period of study toward demographic groups that should have benefited disproportionately from any new state funding. For example, while such counties suffered relative decreases in total population between 1960 and 1980, their black population shares increased,<sup>46</sup> as did various demographic markers of their need, such as the shares school-aged and elderly in the population, as well as the share of families in poverty. Interestingly, although increases in state transfers might have encouraged relatively more blacks to remain in an area (or move to it), the treatment areas did not deviate significantly from comparison areas in terms of labor market outcomes, as measured by either the share unemployed or the share employed in agriculture.<sup>47</sup>

In Table V, we look to demographic change as a potential alternative explanation for our findings, repeating our baseline specification (from Table III, column 1) in column 1 for the purposes of comparison. Because residential mobility is a channel through which the VRA arguably affected per capita state transfers, these estimates are unsurprisingly smaller than our baseline estimates. Consider first the estimates when we control for population growth, in column 2. The coefficient on population growth

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<sup>45</sup> See, for example, Boyle and Jacobs (1982), Cingranelli (1981), Koehler and Wrightson (1987), Lee (1994), Miranda and Tunyavong (1994), and Mladenka and Hill (1978).

<sup>46</sup> That the same areas that saw increased state transfers saw increased black population shares suggests that the state transfer funds were in fact following black voters and provides further evidence that transfers were not aimed at appeasing white voters.

<sup>47</sup> That is, in a model of the form of (2) in which we employ the change in log population from 1960 to 1980 as our dependent variable, we find a negative and significant coefficient on  $lit_s \times \%bl_c$ . In a model in which we employ the change in the percent black as the dependent variable, we find a positive and significant coefficient on  $lit_s \times \%bl_c$ .  $lit_s \times \%bl_c$  also significantly predicts the change in county population shares that are school aged or elderly, as well as the change in the share of a county's families living in poverty but not changes in the county unemployment rate or the share employed in agriculture.

in the model for state transfers is negative and significant (Supplemental Appendix Table II), consistent with Ansolabehere, Gerber, and Snyder's (2002) findings for the entire country, and the reduced-form state transfer and 2SLS estimates are attenuated accordingly. However, the 2SLS estimate remains significant at the 5 percent level. Likewise, when we test in column 3 for the possibility that bureaucratic rules explain our findings, by controlling for changes in the shares of the county population that are school-aged (5 to 17), elderly (65 and over), in poverty, or unemployed between 1960 and 1980—most of which are significantly associated with growth in per capita state transfers in the expected direction (Supplemental Appendix Table II)—the reduced-form state transfer and 2SLS estimates are smaller than at baseline but still statistically significant.

## VII. ENFRANCHISEMENT AND EDUCATIONAL SPENDING AND OUTCOMES

Thus far, we have produced robust evidence of shifts in state transfers after the VRA toward counties with higher black population shares in states where literacy tests were outlawed by the Act. We have provided a variety of indirect evidence—both qualitative (from the historical record) and quantitative (shifts in state funding align with increases in black enfranchisement over the same period)—that the increases in black voting eligibility that accompanied the elimination of the literacy test were responsible for these shifts in funding patterns. And we have provided evidence against several alternative explanations drawn from both the public goods literature and contemporaneous history. That the same areas that saw greater enfranchisement saw increased state transfers is consistent with models of distributive politics—state politicians targeting newly enfranchised blacks to maximize vote share.

However, a weakness of our data is in its aggregation to the county level, which does not allow us to show directly that funds transferred to areas with higher black shares actually targeted black voters. In this section, we therefore shift our focus from funds received by localities to funds localities expended on blacks, or, rather, our best available proxies thereof. Specifically, we employ race-specific data on pupil-teacher ratios and teenagers' school attendance rates to show that the same areas that saw increases in

state transfers also saw increases in the share of black teenagers educated and the quality of black children's educational experience more generally.<sup>48</sup>

Before we present these findings, we provide support for the plausibility of an impact of the VRA on black schooling by demonstrating that the removal of literacy tests impacted education spending. First, recall that nearly three-quarters of the funds were earmarked for education. Second, in Table VI, after repeating our basic specification for the long difference in per capita state transfers for comparison (column 1), we present estimates of the long difference in per capita school expenditures (column 2). The 0.58 elasticity of per capita county education expenditures with respect to presidential turnout rates implies a 63 cent increase in overall education spending for every dollar increase in state transfers.<sup>49</sup>

As suggestive evidence that these additional education dollars were spent on black children, we first examine their effects on the black-white differential in pupil-teacher ratios. To do so, we return to the same school-level data used to define the estimation sample in the third column of Table IV. The year 1976 is the latest in our sample period in which district coverage in these data is high, so we focus on the change in pupil-teacher ratios between 1968 and 1976. We run models of the form of (2) in which the outcome is the difference in the growth of the pupil-teacher ratio between 1968 and 1976 between the schools attended by blacks and the schools attended by whites; to the extent that the additional educational aid was spent on blacks, blacks should have experienced faster class-size declines (or slower class-size increases) than whites.<sup>50</sup> To reduce the possibility that redistribution motivated by school desegregation confounds these estimates, we limit the sample to that used in Table IV, column 3—states

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<sup>48</sup> An alternative approach to determining whether blacks were receiving relatively more public goods—examining relative changes in housing prices in predominantly black and white neighborhoods—is not feasible largely because of data limitations. The census is our only known source of housing value data for the time period and it asks owners, but not renters, to estimate housing value. Thus, a housing value analysis would suffer from selection bias, due not only to a black-white gap in homeownership rates, but a decrease in the gap over our time period (Collins and Margo 2001).

<sup>49</sup> Assuming that all treatment area increases in education spending are a direct result of VRA-induced transfers, the implied elasticity of education spending with respect to state transfers is the ratio of their turnout elasticities ( $0.594=0.58/0.968$  in our preferred specification). We convert this elasticity into a dollar-for-dollar figure using treatment county means of the education spending and state transfers variables. We can do a similar exercise with local taxes. The results imply a 63 cent increase in per-capita education spending and a 31 cent reduction in real per-capita local total tax revenues for each dollar increase in real per capita state transfers. We can thus account for 94 cents of each transfer dollar through increases in education spending and reductions in local taxation alone. We cannot do a complete accounting of each transfer dollar because we do not have data for all counties (and in some cases for any county) for the remaining expenditure categories.

<sup>50</sup> The substantive findings are unchanged if we do not take the natural log. We take the natural log to have an elasticity interpretation for 2SLS, consistent with our 2SLS estimates for state transfers and education spending.

in which 1960 county black share was a relatively weak predictor of the 1968 black-white pupil-teacher ratio differential.

The results (column 3) indicate that the relative pupil-teacher ratio fell significantly faster in treatment counties than in comparison counties with the same 1960 black population share. Thus, the same areas that saw a relatively large increase in state transfers saw a faster increase in the relative quality of public schools that blacks attended. Reassuringly, desegregation—a potential alternative explanation for this finding—was progressing at a similar speed between 1968 and 1976 in treatment and comparison counties with the same 1960 black population shares. We show this in column 4, measuring school racial composition with the county dissimilarity index—roughly, the fraction of students who would have to switch schools to achieve the same racial balance within schools as within the county as a whole (see Data Appendix for the dissimilarity index formula).

The second suggestive piece of evidence that educational spending increased for black children comes from examining the quantity—rather than the quality—of black education. For this we turn to county-level data from the Census of Population specially tabulated by the Census Bureau, which afford us more complete data for a greater number of counties in our restricted subset of states and over the full time frame of our main analysis. We run our basic model, employing the black-white difference in the growth of the school enrollment rates at ages 16 to 17 between 1960 and 1980 as our dependent variable.<sup>51</sup> As shown in the final column of Table VI, the elimination of the literacy test was associated with a relative increase in black school enrollment rates.

In summary, Table VI demonstrates the same higher black share areas in literacy test states that received more funds also saw gains in the share of black teenagers enrolled in school and the quality of black children's educational experience more generally. These findings provide further support for our contention that the Act led state politicians to target newly enfranchised black constituents (and their children) for political patronage. However, these tests are not dispositive: the marginal dollar may

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<sup>51</sup> More precisely, the variable measures enrollment in public or private school, or completion of at least 12 years of education. We focus on these older school-aged children (rather than 14 and 15 year olds), because they are the more likely to drop out. We do not focus on older individuals (such as 18 or 19 year olds) because of concerns about selective migration.



purchase more in terms of black education than white; money may be spent in ways not captured by pupil-teacher ratios or students educated. For these reasons, we view these estimates—and the implication of black welfare gains due to the VRA—as only suggestive.

## VIII. CONCLUSION

The VRA removed literacy tests as a barrier to black citizens' political participation in seven of eleven southern states. As a result, black enfranchisement increased markedly. Because of residential segregation, changes in the share of county residents who were newly enfranchised varied within state across localities—spatial variation that we exploit to examine the impact of enfranchisement on the distribution of state resources. We test for larger post-VRA shifts in the distribution of state transfers toward localities with higher black population shares in southern states where literacy tests were suspended in 1965 than in other states in the former Confederacy. We demonstrate that not only did enfranchisement in higher black share localities increase relatively more in treatment states following the Act's passage, but that state transfers to these localities increased relatively more, as well. In other words, the same black communities that saw an increase in enfranchisement-driven turnout saw an increase in their share of the state resource pie. We rule out competing explanations—drawn both from contemporaneous historical events and from the public resource distribution literature—for the change in state transfers. We also present evidence of increases in the relative share of black teenagers enrolled in school and improvements in the quality of black children's educational experiences generally, further supporting the contention that these funds were targeted toward black citizens.

We posit that the causal link runs from enfranchisement to resource receipt, a conclusion that is consistent with theoretical models of distributive politics in which politicians target resources to identifiable and politically persuadable interest groups to earn their electoral support. That enfranchisement was accompanied by increased resource receipt suggests that the VRA provided substantive, rather than merely symbolic, political gains for southern blacks. While political gain does not

necessarily equate to welfare gain, the fact that this political gain was accompanied by increases in the quality and quantity of black education suggests that the win may have been welfare improving.

We note that our accounting of the impacts of the VRA is incomplete. Through Section V and nondiscrimination requirements that applied to all sample counties, the VRA may have led to additional political or economic gains not captured by our comparison of counties in literacy and non-literacy test states in the South.

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## DATA APPENDIX

### A. *State Transfers Data*

Our analysis uses data on transfers from state governments to local jurisdictions. These data were drawn from the Census of Governments (COG) Historical Data Base on County Area Finances Since 1957, which we downloaded from <ftp://ftp2.census.gov/pub/outgoing/govs/special60/>. The variable that we use is Total State IG (intergovernmental) Revenue, which is reported every five years starting in 1957 and continuing through 1982. In Table VI, we also use the variable Direct Expenditure on Education. Real per capita figures were created by dividing these figures by population from the same year as reported by the same source, then converting to real 2009 dollars using the CPI-U.

### B. *Voter Turnout Data*

The majority of the presidential and gubernatorial turnout data were provided by Matthew Gentzkow and James Snyder. The remaining observations were hand-entered or scanned using various volumes of *America Votes*. We converted these figures to county voter turnout rates by dividing them by the voting age population in the county (ages 21+ in 1970 and prior and ages 18+ in 1971 and later). We obtained the county-level voting age populations for 1950 and 1960 from National Historical Geographic Information System (NHGIS) (Minnesota Population Center 2004) and for 1970 and 1980 from special tabulations of county population by race, gender, and age that we obtained directly from the Census Bureau. We linearly interpolated voting age population in the intercensal years.

### C. *Data on 1960 County Characteristics*

1. *Black Population Share (1960)*: Percent black in the 1960 county population is from the *1960 County and City Data Book Consolidated File, County Data 1947-1977* (U.S. Department of Commerce 1978).
2. *Child Poverty Rate (1960)*: The numerator of the 1960 child poverty rate is the 1960 Census report of the number of 5 to 17 year olds in the county living in families with incomes less than \$2000 in 1959, which was hand-entered from U.S. Senate (1965). This was the primary determinant of a county's Title I eligibility in the 1960s.<sup>1</sup> The denominator is the number of 5 to 17 year olds in the county as of 1960, which we downloaded from the NHGIS (Minnesota Population Center 2004).
3. *Unemployment Rate (1960)*: We use the 1960 unemployment rate reported in the *1960 County and City Data Book*.
4. *High School Completion Rate (1960)*: Percent of the county population with at least 12 years of education is from the *1960 County and City Data Book*.
5. *Relative Representation Index (RRI) (1960)*: The 1960 RRI was hand-entered from a table in David and Eisenberg (1961) titled, "Index Values of the Right to Vote for Members of the Legislature, by Counties, 1910, 1930, 1950, and 1960, as Percentage of the State-Wide Average." In our analysis, we divided the number reported for 1960 by 100.

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<sup>1</sup> There was an additional category of Title I eligibility in 1965: children in families receiving AFDC in excess of \$2000 in 1962. Other categories of eligibility were introduced over time (e.g., foster children, neglected children, and delinquent children), but the main determinant of Title I eligibility over our sample period remained the census-based child poverty count.

6. *Proxies for Civil Rights Activism (1960)*: We constructed our proxies for black political activism—indicators for the presence of an NAACP chapter, another black race organization, and a black college in the county—from data used in Matthews and Prothro (1963), available at: <http://www.rochester.edu/College/psc/signorino/courses/>. This data set provides neither FIPS county codes nor county names but rather numbers counties consecutively within state. To ascertain the identity of each observation, we obtained a county list from the 1950 Census and numbered counties consecutively exactly as they fell with a sort on county FIPS code. All counties merged, and the 1950 black population shares reported in the Matthews and Prothro data matched those that we drew from county population figures reported by NHGIS (Minnesota Population Center 2004) in nearly all instances.
7. *Strom Thurmond Vote Share (1948)*: For all southern states except Alabama and Texas, the county-level percent of votes cast for Strom Thurmond in the 1948 Presidential election was drawn from ICPSR Study No. 8611 (Clubb, Flanigan, and Zingale 2006). For Alabama and Texas, we use data purchased from *Dave Leip's Atlas of U.S. Presidential Elections* (<http://uselectionatlas.org/>).
8. *Percent of Acreage Devoted to Harvested Cotton (1959)*: The percent of a county's land area devoted to harvested cotton was calculated by dividing Item 175 (Harvested acreage: cotton (acres), 1959) by Item 3 (Approximate land area, all farms (acres), 1959) from Part 90 (1959 Data Set I (County and State)) of ICPSR Study No. 2896 (Haines and Inter-university Consortium for Political and Social Research 2010).

D. *Data on Changes in County Characteristics*

1. *Population Growth Rate, 1960-1980*: We use the 1960 population figure reported in the 1960 City and County Data Book and the 1980 population figure from the *1944–1983 Editions of the County and City Data Books*, downloaded from the University of Virginia library (<http://www2.lib.virginia.edu/ccdb/>).
2. *Change in the Percents of the Population School-Aged and Elderly, 1960-1980*: The 1960 population percentages of school age (ages 5-17) and elderly (ages 65 and over) were constructed using data on population by age from NHGIS (Minnesota Population Center 2004). Values for 1980 were constructed using data on population by age from the *1944–1983 Editions of the County and City Data Books*.
3. *Change in the Percent of Families below the Poverty Line, 1960-1980*: For 1960, the poverty rate is the percentage of families in the county reporting incomes less than \$3000 in 1959, drawn from the *1960 County and City Data Book*. For 1980, we construct the poverty rate using data on the number of families below the poverty line and the number of families from the *1944–1983 Editions of the County and City Data Books*.
4. *Change in the Unemployment Rate, 1960-1980*: We use the 1960 unemployment rate reported in the *1960 County and City Data Book*. The unemployment rate for 1980 was constructed using the BLS report of the number unemployed and the number in the civilian labor force from the *1944–1983 Editions of the County and City Data Books*.

E. *Data on School Desegregation and Race-Specific Pupil-Teacher Ratios*

We constructed the fraction of county enrollment in school districts under court order to desegregate or receiving Emergency School Aid Act (ESAA) funds using school district level data from the *Fall 1976 Elementary and Secondary School Civil Rights Survey* (Office for Civil Rights 1978). We used a version of the data housed at UCLA and converted from binary to ASCII format by Ben Denckla and Sarah Reber. A district was classified as being under court order if it answered yes (=1) to the question “School System under Court Order to Desegregate” and as receiving ESAA funds if it answered “yes” (=1) to the question “Is this an ESAA district?” County-level figures are the weighted averages of these dummy variables, where the weights are the sum of total male and female pupils in membership in the district. To ascertain the fraction of total county enrollment in 1976 covered by the 1976 OCR district survey, we merged on the *Elementary and Secondary General Information System (ELSEGIS): Public School District Universe Data, 1976-1977* (U.S. Department of Education 1977).

We constructed race-specific pupil-teacher ratios and the dissimilarity index at the county level using school-level data from the *Directory of Public Elementary and Secondary Schools in Selected Districts* (U.S. Department of Health, Education, and Welfare 1970) and *Fall 1976 Elementary and Secondary School Civil Rights Survey* (U.S. Department of Health, Education, and Welfare 1978). We used versions of the data housed at UCLA and converted from binary to ASCII format by Ben Denckla and Sarah Reber. County-level race-specific pupil teacher ratios were calculated as weighted averages of school-level pupil-teacher ratios, with weights equal to school-level, race-specific enrollment. The formula for the dissimilarity index is given by:

$$\frac{\sum_{i \in d} enr_i |\%bl_i - \%bl_c|}{2 \times enr_c \times \%bl_c \times (1 - \%bl_c)}$$

where  $enr_i$  is total enrollment in school  $i$ ,  $enr_c$  is the enrollment in county  $c$  (summing across all schools),  $\%bl_i$  is the percent of total enrollment in school  $i$  that is black, and  $\%bl_c$  is the black enrollment percent in county  $c$ . To ascertain the fraction of total county enrollment in 1976 covered by the 1976 OCR school survey, we merged on the *ELSEGIS Public School District Universe Data, 1976-1977* (U.S. Department of Education 1977); comparable data are not available for 1968.

#### F. *Data on School Enrollment Rates*

We constructed the 1960 to 1980 change in race-specific school enrollment rates of 16 and 17 year olds using county-level tabulations of population characteristics by single year of age, race (white, black/African American, other), and year, produced by the Census Bureau using the sample detail files from the 1960 and 1980 Censuses. The numerator of the enrollment rate in a given Census year is the number of 16 and 17 year old residents of a county of a given race who either are currently enrolled in school (public or private) or have more than twelve years of completed education. The denominator is the total number of 16 and 17 year old residents of that county of that race with education reported.

#### G. *Estimation Sample*

Our estimation sample includes all but 29 counties in the 11 former Confederate states (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia). We trim from the estimation sample those counties where, in any year under study, the natural log of turnout in the presidential election or the natural log of per-capita state transfers deviate by more than 4 standard deviations from the state-by-year specific mean. This trimming rule results in us dropping 13 counties (or independent cities, in the case of Virginia) in literacy test (treatment) states (Macon County, AL; Mobile County, AL; Chattahoochee County, GA; Pointe Coupee Parish, LA; St. James Parish, LA; Vernon Parish, LA; West Feliciana Parish, LA; Stone County, MS; Cumberland

County, NC; Onslow County, NC; James City, VA; Prince George County, VA; Harrisonburg City, VA) and 14 counties in non-literacy test (comparison) states (Jefferson County, AR; Broward County, FL; Gadsden County, FL; Hillsborough County, FL; Union County, FL; Fayette County, TN; Armstrong County, TX; Coryell County, TX; Foard County, TX; Kendall County, TX; Kenedy County, TX; Maverick County, TX; Orange County, TX; Randall County, TX). For the remaining two southern counties not in our sample (Issaquena County, MS; Chesapeake City, VA), we lack data on control variables.

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Table I.  
Descriptive Statistics

	With Literacy		Without Literacy	
	Test		Test	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
<i>Panel A. Funding (real 2009 dollars)</i>				
Per-Capita State Transfers, 1957/62	358.5	114.8	318.5	89.5
Per-Capita State Transfers, 1977/82	764.9	188.9	670.3	196.9
$\Delta \ln(\text{Per-Capita State Transfers})$	0.81	0.33	0.74	0.29
Per-Capita Education Spending, 1977/82	527.5	131.06	621.6	154.83
Per-Capita Education Spending, 1957/62	933.6	180.10	1001.9	247.42
$\Delta \ln(\text{Per-Capita Education Spending})$	0.58	0.21	0.48	0.19
<i>Panel B. Election Turnout</i>				
Presidential Election Turnout Rate (%), 1960	37.5	13.2	44.8	8.2
Presidential Election Turnout Rate (%), 1980	47.2	7.7	47.9	6.1
$\Delta \ln(\text{Presidential Election Turnout Rate})$	0.27	0.34	0.08	0.20
Gubernatorial Election Turnout Rate (%), 1957-60	22.5	18.8	23.8	13.6
Gubernatorial Election Turnout Rate (%), 1977-80	35.3	12.2	31.1	7.4
$\Delta \ln(\text{Gubernatorial Election Turnout Rate})$	0.77	0.83	0.39	0.45
<i>Panel C. County Characteristics</i>				
Percent Black, 1960	28.9	16.2	15.1	11.0
Child Poverty Rate (%), 1960	24.5	14.7	17.6	11.1
Unemployment Rate (%), 1960	4.9	1.6	5.3	1.8
Percent Completed High School, 1960	32.2	10.7	37.6	9.9
Percent of County Enrollment in Districts:				

Under Court Order to Desegregate, 1976	50.4	47.2	41.9	42.1
Receiving ESAA funds, 1976	39.3	45.4	35.0	37.1
=1 if Has NAACP Chapter, 1960 (x100)	54.2	49.9	64.5	47.9
=1 Has Other Black Organization, 1960 (x100)	4.9	21.5	0.2	3.9
=1 Has Black College, 1960 (x100)	24.1	42.8	25.1	43.4
Percent Vote for Strom Thurmond, 1948	29.9	30.3	8.3	11.9
Relative Representation Index, 1960	1.0	0.6	1.0	0.8
1960 to 1980 Change in:				
ln(Population) (x 100)	21	26	38	29
Percent of Population Ages 5 to 17	-5.3	1.9	-4.1	1.5
Percent of Population Ages 65 and over	3.2	1.5	3.1	2.4
Percent of Families below Poverty Line	-23.3	11.4	-20.0	11.1
Percent Unemployed	5.4	3.2	3.4	3.4
Observations (counties)†	625		477	

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*Notes:* The estimation sample includes all but 29 counties in the South (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, and VA). States with literacy tests still in place immediately prior to the VRA are AL, GA, LA, MS, NC, SC, and VA. Desegregation statistics are weighted by 1960 county population X the fraction of 1976 county enrollment covered by the desegregation data; all other statistics are weighted by 1960 county population. See Data Appendix for sources.

† For the school desegregation variables, there are 622 and 476 observations for the literacy test and non-literacy test subsamples, respectively. For Strom Thurmond vote share, there are 636 and 491 observations for the literacy test and non-literacy test subsamples, respectively.

Table II.

## Baseline Event-Study and Long-Difference Estimates

Panel A. ln(Presidential Election Turnout Rate)		Panel B. ln(Gubernatorial Election Turnout Rate)		Panel C. ln(Per-Capita State Transfers)	
Long Diff.		Long Diff.		Long Diff.	
County X	1960 to	County X	1957-60 to	County X	1957/62 to
Year	1980	Year	1977-80	Year	1977/82
(1)	(2)	(3)	(4)	(5)	(6)
1960 percent black		1960 percent black		1960 percent black	
X literacy test indicator		X literacy test indicator		X literacy test indicator	
X indicator for:		X indicator for:		X indicator for:	
year=1952	0.000114 (0.00104)				
year=1956	-0.000558 (0.00123)	1953>=year>1957	-0.000554 (0.00288)	year=1957	0.000368 (0.00115)
year=1960	-	1957>=year>1961	-	year=1962	-
year=1964	0.000929 (0.000781)	1961>=year>1965	0.00341 (0.00338)	year=1967	0.00204 (0.00161)
year=1968	0.00334*** (0.00102)	1965>=year>1969	0.00633** (0.00270)	year=1972	0.00549** (0.00240)
year=1972	0.00525*** (0.00123)	1969>=year>1973	0.00529* (0.00307)	year=1977	0.00769*** (0.00223)
year=1976	0.00456***	1973>=year>1977	0.0108***	year=1982	0.00405

	(0.00112)		(0.00380)		(0.00266)
year=1980	0.00587***		1977>=year>1980	0.0114***	
	(0.00125)			(0.00343)	
1960 percent black	0.00587***	1960 percent black	0.0114***	1960 percent black	0.00569***
X literacy test indicator	(0.00117)	X literacy test indicator	(0.00318)	X literacy test indicator	(0.00215)
Observations	8,816	1,102	7,713	1,102	6,612
R-squared	0.896	0.739	0.924	0.793	0.916
					1,102
					0.480

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*Notes:* Regressions in the odd-numbered columns include county fixed effects, state indicators X year indicators and —each individually, interacted with year indicators, and interacted with year indicators and the literacy test indicator—1960 county percent black, the 1960 county child poverty rate, and the 1960 county unemployment rate; standard errors are robust for heteroskedasticity and for correlation in error terms within county over time. Regressions in the even-numbered columns include state indicators and—each individually and interacted with the literacy test indicator—1960 county percent black, the 1960 county child poverty rate, and the 1960 county unemployment rate; standard errors are heteroskedasticity-robust. The estimation sample includes counties in the South (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, and VA). The literacy test indicator is set to one for states with literacy tests still in place immediately prior to the VRA (AL, GA, LA, MS, NC, SC, and VA). Regressions are weighted by 1960 county population. \*\*\*, \*\*, and \* indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table III.

## Long-Difference Estimates: Sensitivity to Specification

	Nonlinearities in 1960 percent black?					Common support in p-score + inverse p-score weighting
	Common support in:		Common support in:		Common support in p-score + inverse p-score weighting	
	A test of literacy or of race?	Remove ID from majority black counties	Propensity to be in literacy test state (p-score)	Propensity to be in literacy test state (p-score)		Propensity to be in literacy test state (p-score)
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. $\Delta \ln$ (Presidential Election Turnout Rate), 1960 - 1980						
<i>First-stage</i>						
1960 percent black	0.00587***	0.00565***	0.00546***	0.00608***	0.00501***	0.00357***
X literacy test indicator	(0.00117)	(0.00122)	(0.00142)	(0.00165)	(0.00122)	(0.00131)
R-squared	0.739	0.754	0.744	0.699	0.703	0.723
Panel B. $\Delta \ln$ (Per-Capita State Transfers), 1957/62 - 1977/82						
<i>Reduced-form</i>						
1960 percent black	0.00569***	0.00708***	0.00677***	0.00646**	0.00585***	0.00451**
X literacy test indicator	(0.00215)	(0.00226)	(0.00250)	(0.00285)	(0.00223)	(0.00200)
R-squared	0.480	0.492	0.482	0.494	0.476	0.584
<i>Two-stage Least Squares</i>						
<i>(Instrument: 1960 percent black X literacy test indicator)</i>						

$\Delta \ln$ (pres. election turnout rate)	0.968***	1.254***	1.241**	1.063**	1.169**	1.262*
	(0.369)	(0.463)	(0.491)	(0.499)	(0.464)	(0.673)
Root MSE	0.269	0.292	0.292	0.269	0.284	0.279
<i>Ordinary Least Squares</i>						
$\Delta \ln$ (pres. election turnout rate)	0.0923	0.0803	0.0905	0.0337	0.0817	0.122
	(0.0895)	(0.0945)	(0.0925)	(0.111)	(0.0985)	(0.0781)
R-squared	0.473	0.479	0.473	0.484	0.467	0.581
Root MSE	0.230	0.229	0.230	0.222	0.230	0.219
Observations (counties)	1,102	1,102	1,102	805	1,040	1,040
Controls:						
1960 high school completion rate		X				
1960 high school completion rate						
X literacy test indicator		X				
1960 majority black (=1)			X			
1960 majority black (=1)						
X literacy test indicator			X			

*Notes:* All models include state indicators and 1960 county percent black, the 1960 county child poverty rate, and the 1960 county unemployment rate, each entered directly and interacted with the literacy test indicator. The full estimation sample includes counties in the South (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, and VA). The literacy test indicator is set to one for states with literacy tests still in place immediately prior to the VRA (AL, GA, LA, MS, NC, SC, and VA). The sample in column (4) consists of counties with 1960 percent black at least as high as the 10<sup>th</sup> percentile of the percent black distribution for states without literacy tests (2.2%) and no more than the 90<sup>th</sup> percentile of the percent black distribution for states with literacy tests (49.9%). The sample in column (5) drops counties in treatment states with propensity scores above the maximum propensity score in comparison states and counties in comparison states with propensity scores below the minimum for treatment states. For the regressions in column (6), treatment counties



are weighted by 1960 county population and, comparison counties are weighted by 1960 county population  $X p/(1-p)$ , where  $p$  represents the estimated propensity score.

Regressions in all other columns are weighted by 1960 county population. Standard errors (in parentheses) are heteroskedasticity-robust. \*\*\*, \*\*, and \* indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table IV.

## Long-Difference Estimates: Robustness to Setting-Specific Predictors of State Transfers

	Baseline	School Desegregation	Political activism	Mandatory redistricting	Elimination of the poll tax	Preclearance Provision	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. $\Delta \ln$ (Presidential Election Turnout Rate), 1960 - 1980							
<i>First-stage</i>							
1960 percent black	0.00587***	0.00595***	0.00626***	0.00614***	0.00585***	0.00591***	0.0105***
X literacy test indicator	(0.00117)	(0.00113)	(0.00152)	(0.00109)	(0.00116)	(0.00118)	(0.00145)
R-squared	0.739	0.739	0.800	0.748	0.739	0.739	0.769
Panel B. $\Delta \ln$ (Per-Capita State Transfers), 1957/62 - 1977/82							
<i>Reduced-form</i>							
1960 percent black	0.00569***	0.00517**	0.00522*	0.00586***	0.00519***	0.00549***	0.0126***
X literacy test indicator	(0.00215)	(0.00210)	(0.00275)	(0.00191)	(0.00182)	(0.00205)	(0.00324)
R-squared	0.480	0.516	0.470	0.530	0.552	0.483	0.452
<i>Two-stage Least Squares</i>							
<i>(Instrument: 1960 percent black X literacy test indicator)</i>							
$\Delta \ln$ (pres. election turnout rate)	0.968***	0.869**	0.833*	0.954***	0.887***	0.928***	1.199***
	(0.369)	(0.340)	(0.426)	(0.316)	(0.330)	(0.351)	(0.367)
Root MSE	0.269	0.254	0.262	0.260	0.250	0.265	0.294

Observations (counties)	1,102	1,098	498	1,100	1,102	1,102	934
Sample:	Full	Counties w/ Desegregation Data in 1976	Counties in AR, FL, MS, NC, TN, TX, and VA w/ PT Ratio Data	Counties w/ Thurmond Vote Share	Full	Full	Drop states not subject to Section V †

Additional Controls:

% enr. under court order, 1976	X						
% enr. receiving ESAA funds, 1976	X						
=1 if NAACP chapter, 1960				X			
=1 if other black org, 1960				X			
=1 if black college, 1960				X			
% voting for Strom Thurmond, 1948				X			
relative representation index, 1960					X		
poll tax indicator X % black, 1960						X	

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*Notes:* All models include state indicators and 1960 county percent black, the 1960 county child poverty rate, and the 1960 county unemployment rate, each entered directly and interacted with the literacy test indicator. The poll tax indicator is set to one for states with poll taxes eliminated subsequent to ratification of the 24<sup>th</sup> amendment in 1964 (AL, AR, MS, TX, and VA). Regressions in all columns except (2) and (3) are weighted by 1960 county population. Regressions in column (2) are weighted by 1960 county population X the fraction of total 1976 county enrollment represented in the 1976 Office for Civil Rights (OCR) district survey (U.S. Department of Health, Education, and Welfare 1978). Regressions in column (3) are weighted by 1960 county population X the fraction of total 1976 county enrollment represented in the 1976 OCR school survey. Standard errors (in parentheses) are heteroskedasticity-robust. \*\*\*, \*\*, and \* indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

† States not subject to Section V are AR and TN.

Table V.

## Long-Difference Estimates: Robustness to General Predictors of State Transfers

	Baseline	Infrequent updating of funding formulas	Changes in categorical eligibilty for state funds
	(1)	(2)	(3)
Panel A. $\Delta \ln$ (Presidential Election Turnout Rate), 1960 - 1980			
<i>First-stage</i>			
1960 percent black	0.00587***	0.00602***	0.00506***
X literacy test indicator	(0.00117)	(0.00113)	(0.00104)
R-squared	0.739	0.740	0.752
Panel B. $\Delta \ln$ (Per-Capita State Transfers), 1957/62 - 1977/82			
<i>Reduced-form</i>			
1960 percent black	0.00569***	0.00378**	0.00292**
X literacy test indicator	(0.00215)	(0.00186)	(0.00138)
R-squared	0.480	0.551	0.620
<i>Two-stage Least Squares</i>			
<i>(Instrument: 1960 percent black X literacy test indicator)</i>			
$\Delta \ln$ (pres. election turnout rate)	0.968***	0.627**	0.577**
	(0.369)	(0.297)	(0.286)
Root MSE	0.269	0.229	0.220
Observations (counties)	1,102	1,102	1,102
Additional Controls (all 1960-1980):			

$\Delta \ln(\text{population})$	X	
$\Delta \% \text{ of population aged 5-17}$		X
$\Delta \% \text{ of population aged 65+}$		X
$\Delta \% \text{ of families in poverty}$		X
$\Delta \% \text{ unemployed}$		X

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*Notes:* All models include state indicators and 1960 county percent black, the 1960 county child poverty rate, and the 1960 county unemployment rate, each entered directly and interacted with the literacy test indicator.

Regressions are weighted by 1960 county population. Standard errors (in parentheses) are heteroskedasticity-robust.

\*\*\*, \*\*, and \* indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table VI.

## Long-Difference Estimates: Education Spending, Quality and Quantity

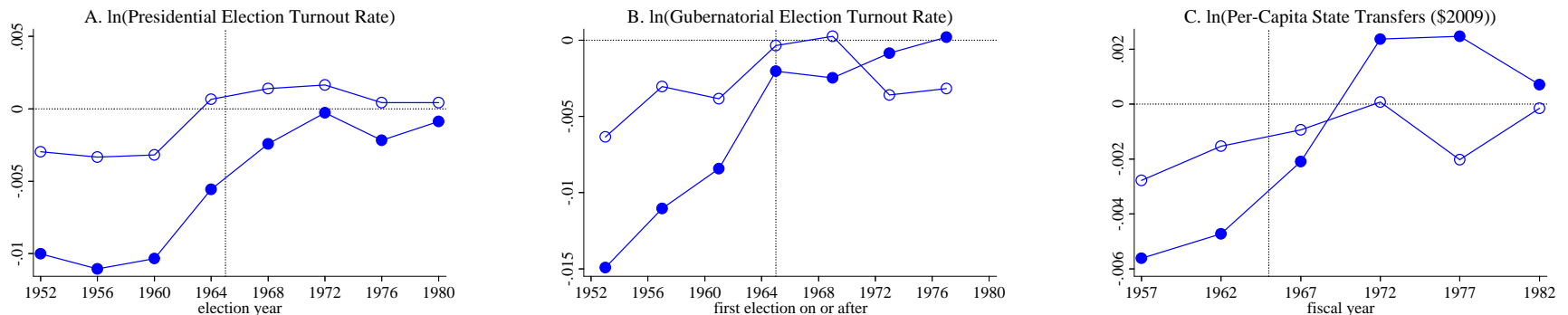
	$\Delta \ln$ (Per-Capita Finance Outcomes) 1957/62-1977/82		Black-White Diff. $\Delta \ln$ (PT Ratio) 1968-1976	$\Delta$ Dissim. Index 1968-1976	Black-White Diff. $\Delta \ln$ (Enr. Rate, Ages 16-17) 1960-1980
<i>Dependent variable:</i>	State Transfers (1)	School Spending (2)	1968-1976 (3)	1968-1976 (4)	1960-1980 (5)
Panel A. $\Delta \ln$ (Presidential Election Turnout Rate), 1960-1980					
<i>First-stage</i>					
1960 percent black	0.00587***	0.00587***	0.00626***	0.00626***	0.00540***
X literacy test indicator	(0.00117)	(0.00117)	(0.00152)	(0.00152)	(0.00127)
R-squared	0.739	0.739	0.800	0.800	0.791
Panel B. $\Delta$ Outcome					
<i>Reduced-form</i>					
1960 percent black	0.00569***	0.00338**	-0.00161**	9.74e-05	0.00299**
X literacy test indicator	(0.00215)	(0.00145)	(0.000661)	(0.00169)	(0.00152)
R-squared	0.480	0.163	0.294	0.566	0.107
<i>Two-stage Least Squares</i>					
<i>(Instrument: 1960 percent black X literacy test indicator)</i>					

$\Delta \ln$ (pres. election turnout rate)	0.968*** (0.369)	0.575** (0.268)	-0.258** (0.117)	0.0156 (0.270)	0.554* (0.309)
Root MSE	0.269	0.215	0.0726	0.153	0.182
Observations (counties)	1,102	1,102	498	498	623
Sample:	Full	Counties in AR, FL, MS, NC, TN, TX, and VA with: PT ratios by race available      PT ratios by race available      enrollment by race available			

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*Notes:* All models include state indicators and 1960 county percent black, the 1960 county child poverty rate, and the 1960 county unemployment rate, each entered directly and interacted with the literacy test indicator. Regressions in columns (1), (2), and (5) are weighted by 1960 county population; all other regressions are weighted by 1960 population X the fraction of total 1976 county enrollment represented in the school-level 1976 OCR data (U.S. Department of Health, Education, and Welfare 1978). Standard errors (in parentheses) are heteroskedasticity-robust. \*\*\*, \*\*, and \* indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Figure I  
Trends in the Gradients of Voter Turnout Rates and Per-Capita State Transfers in 1960 County Percent Black,  
By Presence of a Literacy Test Prior to the VRA



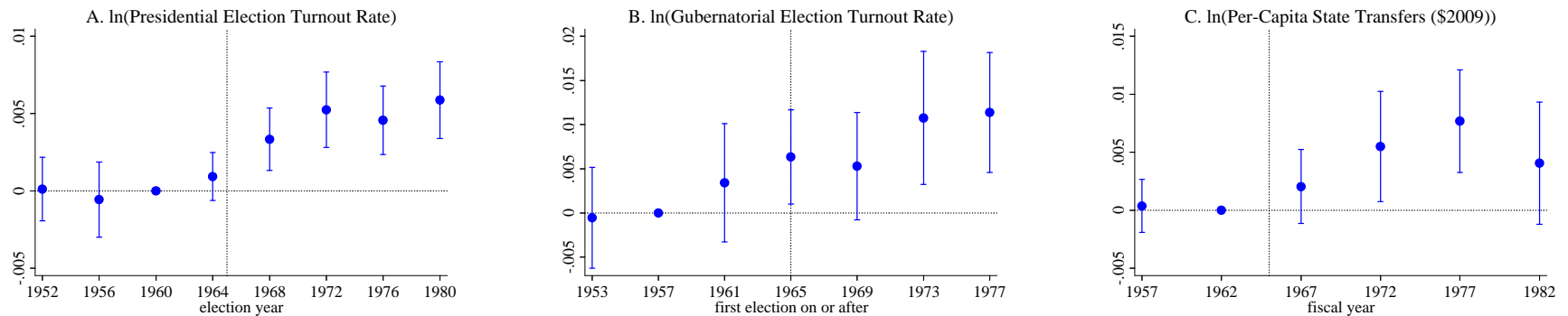
Solid Circles: With Literacy Test.

Hollow Circles: Without Literacy Test.

*Notes:* Each graph shows coefficients on 1960 county percent black by year, separately for states in the South with literacy tests prior to the VRA (AL, GA, LA, MS, NC, SC, VA) and for states in the South without literacy tests (AR, FL, TN, TX). The unit of observation is county. The underlying year- and subsample-specific regressions also include state indicators, the 1960 county child poverty rate, and the 1960 county unemployment rate as controls and are weighted by 1960 population. The estimation sample includes all but 29 counties in the South; see Data Appendix for further description. The dotted vertical line is at 1965, the year that the VRA was enacted.



**Figure II**  
**Test for Differential Trends in the Gradients of Voter Turnout Rates and Per-Capita State Transfers in 1960 County Percent Black, By Presence of a Literacy Test Prior to the VRA**



*Notes:* Each graph shows coefficients (95% confidence intervals) on interactions between the literacy test indicator, a year indicator, and 1960 county percent black. All specifications include county fixed effects, state-by-year fixed effects, and year indicators interacted with 1960 county percent black. The model also includes the 1960 county child poverty rate and the 1960 county unemployment rate, interacted with year indicators and with both year indicators and the literacy test indicator. Interactions with indicators for 1960, 1957-60, and 1962 are omitted in Panels A, B, and C, respectively, to identify the model. The estimation sample includes all but 29 counties in the South (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, VA); see Data Appendix for further description. Specifications are weighted by 1960 county population. The dotted vertical line is at 1965, the year that the VRA was enacted.