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 (VRA) has been called one of the most effective pieces of civil rights legislation in U.S. history, having generated dramatic increases in black voter registration across the South. We show that the expansion of black voting rights in some southern states brought about by one requirement of the VRA – the elimination of literacy tests at voter registration – was accompanied by a shift in the distribution of state aid toward localities with higher proportions of black residents, a finding that is consistent with models of distributive politics. Our estimates imply an elasticity of state transfers to counties with respect to turnout in presidential elections – the closest available measure of enfranchisement – of roughly one.

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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.[1] Southern states that 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 2011), hopes for the VRA were much greater than to allow 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, Naidu 2012, Valelly 2004).

In this paper, we examine whether the expansion of black voting rights resulting from the VRA's elimination of literacy tests increased the flow of state funds to localities with higher black population

[1]We define the South to include the 11 states of the former Confederacy: Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

shares. To date, the evidence on this question 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 the anecdotal evidence is reflective of a causal impact of voting rights on state resource receipt. Our focus is on transfers from state governments to local governments for provision of local public goods – chiefly education[2] – which 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 had a greater positive impact on enfranchisement in southern localities with higher black population shares. Accordingly, we test for shifts in the distribution of state funds toward localities with higher black population shares in states that had literacy tests prior to passage of the VRA – the "treatment" states. 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 as 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 state transfers, and the relatively large changes in the geographic distribution of state transfers in treatment states are closely timed with the VRA's passage.[3]

We find larger shifts in state transfers toward counties with higher black population shares after the VRA in states where literacy tests were removed as a result of the Act. Our estimates imply that the elimination of the literacy test generated an additional five percent increase in per-capita state transfers in the decade and a half following the VRA for each additional ten percentage point increase in a county's

[2] Nearly three-quarters of state transfers to local governments in the South during this period were for education. Another 20 percent were evenly split between funding for highways and general spending. While a small share of state transfers include federal funds that pass through state governments, these federal funds are for local governments, not individuals. Thus, the outcome of interest does not include federal transfers to individuals for retirement, income support, or health care.

[3] Results further withstand trimming to create common support in black share and the propensity to be located in a literacy test state, as well as inverse propensity score weighting.

1960 black population share. For the average county in a literacy test state, this amounted to a 12.4 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. Our substantive findings are unchanged when we allow treatment counties to have been more affected by school desegregation, black political activism, legislative redistricting, budgetary lags, and changes in local need than comparison counties with the same black population shares.

Consistent with an effect on enfranchisement and previous work (Filer, Kenny, and Morton 1991), we also see relatively large and sustained increases in voter turnout in treatment counties with higher black population shares after the VRA. The fact that the same counties that saw an increase in enfranchisement also saw increased resource receipt is consistent with models of distributive politics and the historical record, described in the next section, which indicates that white governors began to target black residents following their enfranchisement. That those additional largely educational resources actually impacted the children of black voters is suggested by our finding that the same areas that saw increases in state transfers also saw increases in the share of black teenagers enrolled in school and the quality of black children's educational experience more generally.

In sum, this paper makes two main contributions. First, our findings complement previous empirical research (e.g., Margo 1990, Naidu 2012) showing that black disenfranchisement starting in the late 19th century contributed to reductions in black receipt of public goods, namely school resources. Instead of focusing on *dis*enfranchisement, 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. Second, we provide evidence on an unexplored question in political economy. While previous research on state budgets and enfranchisement, such as Husted and Kenny (1997), Kenny and Lott (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, we focus on the *distribution* of that spending.[4,5] We

[4] 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 their

demonstrate a link between black enfranchisement and black state resource receipt that is consistent with models of distributive politics.

The remainder of the paper proceeds as follows. In Section II, we provide theoretical motivation. In Section III, we describe the history of voting rights in the South, and in Section IV, we discuss our main data sources. We present baseline results in Section V and verifications of robustness in Section VI. We extend our analysis from funding to school spending and outcomes in Section VII before concluding in Section VIII.

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 could help to elect black representatives who redistribute to black voters because of shared ideology.[6] 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. In fact, gains in black office holding have been more often attributed to redistricting rule changes that lagged the passage of the VRA by as much as 25 years (Handley and Grofman 1994).[7] 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.[8,9]

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focus is on the impact of the decision to exercise the franchise, while our focus in on the impact of gaining it. That is, enfranchisement leads to permanent shifts in both actual and, perhaps more importantly, *potential* turnout, a key consideration for politicians in their decision making.

[5] Our geography-based approach could not be used to study the distributional impacts of the constitutional amendments that enfranchised women and 18-21 year olds, two groups whose spatial distribution is more uniform across localities.

[6] For example, citizen candidate models predict policy divergence between candidates if candidates care about policy outcomes. See for example, Besley and Coate (1997) and Osborne and Slivnski (1996). Chattopadhyay and Duflo (2004) and Pande (2003) demonstrate empirically that a representatives' personal ideology, proxied by gender and ethnicity, respectively, influence policy.

[7] Handley and Grofman (1994) argue that the move to single member districts during the 1970s and 1980s, which all southern states were pressured to make, increased the number of minorities in state legislatures. However, the authors contend that the 1982 renewal of the VRA, which required states to draw districts so as to not fragment black voters, had a much larger impact on black descriptive representation.

[8] The ratio of the share of black elected officials in the state legislative upper house at the end of our sample period (average of 1977 and 1982) to the state's 1980 black population share is on average 0.13 in treatment states and 0.11 in comparison states (weighting by 1960 state population). For the lower house, these figures are 0.28 and 0.48, respectively (Joint Center for Political Studies 1977, 1982). Further, when we run state-level regressions of the change in ratio of black elected officials to population on a literacy test indicator, we find that the increase was slower in literacy test states.

[9] We focus on state officials because we are interested in the distribution of state resources. While black municipal or education officials may have lobbied for state resources to be directed to their constituents, blacks were also poorly represented amongst

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 and 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 of ongoing debate. The answer to the question depends on the modeler's assumptions about the politician's risk aversion and the efficiency of targeting various groups and on the various groups' marginal voting response to political resource receipt (e.g., in terms of turnout and choice). But whether the politician should direct resources to the enfranchised or unenfranchised 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 a relatively high marginal utility of school, road, or other neighborhood improvements. And although blacks did not comprise a majority of the electorate, this would not have precluded a causal relation between their voting eligibility and public goods receipt. Unlike in legislative voting, in which politicians must take a single side of the 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.

And following the VRA, some white politicians in fact served white segregationist constituents with their rhetoric while simultaneously and quietly serving the interests of black constituents with goods and services.[10] 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.

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local officials. The ratio of the share of black elected officials (which include county, municipal, law enforcement and education officials in addition to legislators) to black population in 1976 was .15 in literacy states and .08 in non-literacy states (Joint Center for Political Studies 1976). Unfortunately, we cannot test whether black local officials were more effective than local officials of other races in lobbying for resources for black communities, as the Joint Center provides lists of black local officials, but not the total universe of local positions.

[10] Bass and Devries (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."

Wallace infamously declared in his first inaugural address in 1963, "Segregation now! Segregation tomorrow! Segregation forever!" (Lesher 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" (Lesher 1994, p. 457). During that term, ushered in by blacks marching in the inaugural parade for the first time (Lester 1994), Wallace was:

> *…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).

Politicians who did not modernize their offices in some way generally lost them. Bass and Devries (1976) 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" (p. 12). 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 Devries, 1976, p. 11-12).

We empirically test the relevance of Edwards' 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 15th amendment. 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 over whether the legislation was motivated by racism or partisanship, and over 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).[11] 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 1901-2 Constitutional Convention (Keyssar 2000, p. 113). The targeting was effective: the 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 20th 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 obstacle to the black franchise during our time period. Valelly (2004) points to the falling real value of the poll tax and the fact that six of 11 southern states (four of which also had literacy tests) eliminated them before the 24th amendment in 1964 made them unconstitutional as evidence of the declining significance of poll taxes.[12] Key (1949) argues that given the other disenfranchising laws, the poll tax had not been a binding constraint on blacks for some time. 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,[13] on the other hand, was a significant impediment to black voting for the seventy plus years of its existence. 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.[14] Over time,

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[11] Initially, these rights were limited extra-legally, through violence, intimidation, and voter fraud.
[12] The Supreme Court Case *Harper v. Board of Elections* of 1966 outlawed poll taxes for state elections.
[13] Literacy tests outside the South focused on illiterate 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 in the 1965 VRA, but rather with its 1970 re-authorization.
[14] 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.

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). 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 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 (The History Channel 2005); blacks with law degrees were told that their interpretations of the legal terms were inadequate (Valelly 2004). In fact, when the Civil Rights Commission held open hearings on disenfranchisement in Alabama in 1958, of 33 black complainants, 26 had high school diplomas, and ten of those had college degrees while six 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 (U.S. Commission on Civil Rights 1961). Blacks were asked such nebulous questions as "Who was the Creator?" and "Are all people born alike?" (U.S. 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 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. Five months later, southern literacy tests were outlawed with the signing of the VRA. The Act, which authorized the president to send in 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 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 literacy tests, the VRA ended discriminatory practices that were prevalent throughout the South, such as redrawing the boundaries of political jurisdictions to prevent blacks from attaining elected office. The nondiscrimination requirements, like the vast majority of the VRA, apply to all southern states – in fact all states across the nation – not just those that had employed literacy tests. Where the law differs by jurisdiction is in regards to Section V, which mandates preclearance by the U.S. Department of Justice for any change in electoral procedure.[15] Preclearance is aimed at preventing precisely the type of morphing of the literacy test law that happened after the federal government struck down other anti-suffrage legislation. The seven former literacy test states as well as Florida and Texas are subject to preclearance.[16]

IV. DATA

A. Data on State Transfers to Localities

Our goal is to ascertain how black enfranchisement following the VRA's elimination of literacy tests impacted how state governors and legislators distributed public resources. To this end, we examine within-state shifts in the distribution of voting rights and public resource receipt across southern communities of varying racial composition over a period surrounding passage of the VRA.

Our key dependent variable is per-capita transfers from state governments to local governments.[17] This information has been collected by the Census of Governments (COG) every five fiscal years for

[15] For example, states under Section V must receive clearance on their redistricting plans before they can use them for state or federal legislative elections. States or localities also must receive permission to change the dates of election, the location of polling places, or the term of an electoral office.

[16] Jurisdictions are subject to Section V preclearance because of 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 turnout. There are also several areas outside of the South that are subject to preclearance, primarily because of failure to provide foreign language voting materials. Only half of the counties in North Carolina are subject to Section V but if any part of a jurisdiction is subject, the whole jurisdiction is subject. Thus, North Carolina must seek preclearance for state, as well as federal, legislative redistricting.

[17] That is, these state transfers are designed to offset expenditures by local governments, not private individuals.

decades. We focus on the years 1957 through 1982. Over this period, state transfers to local governments made up about one-third of total state expenditures in the South. One strength of state transfers to localities as an outcome is that the recipients of these transfers are geographically identifiable, which makes the measure suitable for a test of distributive politics. Another is the importance of state transfers for the funding of local public goods, like education, thought to be critical to black economic advancement. Throughout the sample period, 73 percent of state transfers to local governments in the average southern state were for education; general spending and highway funds each constituted 10 percent of the average state total.[18] Moreover, local governments in the South relied heavily on these state transfers to fund these public services. 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).

The COG files that we use report state intergovernmental transfers to local governments (e.g., counties, municipalities, townships, school districts, and special districts) aggregated to the county (and year) level. An advantage to using the county as our unit of analysis is that counties are not political units with endogenous boundaries that are altered during redistricting.[19] County boundaries essentially remain fixed across our 25 year sample period.[20] 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.[21]

Because we posit that the mechanism linking the VRA to increased resources to black communities is individual enfranchisement, we weight our regressions by 1960 population so that they

[18] Though not every category is reported separately for every state in every year, the remaining transfers are for welfare, health and hospitals, law, sewerage, non-highway transportation, and miscellaneous. The pattern of transfers by funding type does not vary significantly over the sample period (U.S. Department of Commerce 1957, 1962b, 1977, and 1982).
[19] Altering district boundaries was a procedure used by southern states to keep blacks from political office in communities in which the black population was growing. See, for example, Trebbi, Aghion, and Alesina (2008).
[20] 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 at: <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.
[21] 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.

yield the impact of the removal of literacy tests on the average person.[22] As shown in Panel A of Table 1A, the weighted mean per-capita state transfer to local governments in states that had literacy tests prior to the VRA was $356 (2009 dollars) in the pre-period (the average of the 1957 and 1962 figures) and $763 in the post-period (the average of the 1977 and 1982 figures). The figures are $317 and $674 in the other southern states.[23] 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 per-capita state transfers over the twenty-year period is on average 82 percent in states with literacy tests and 76 percent in the remainder of the South. Consistent with the targeting of these transfers for education, there were dramatic increases in 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 state transfer data is that not all funds that the state reports transferring originate with the state. Some federal "pass through" money – funds that the federal government provides to local governments through states – is included, though it is estimated to be less than 15 percent of the total in most states (Ansolabehere, Gerber, and Snyder 2002).[24] To the extent that state governments have discretion over how to distribute these federal funds, their inclusion in transfer totals does not bias our estimates of the amount that state officials decide to transfer to each locality. Rather, the concern is that the nondiscretionary dollars may be correlated with increases in enfranchisement following the VRA. We know, for example, that predominantly black areas were likely to have been allocated more federal funds for education under Title I of the Elementary and Secondary Education Act of 1965 (ESEA) because they were also predominantly poor, and the 1960 county child poverty rate was the primary determinant of

[22] Unweighted specifications would yield the impact of the removal of literacy tests on the average county. This would be a meaningful and appropriate specification if counties were uniformly (across states) political units, and the mechanism posited went through the leaders of these units. However, given that counties are not political units and that the power of the enfranchised increases with their numbers, unweighted specifications unsurprisingly yield attenuated (and insignificant) coefficients. They are also not interpretable as the impact of enfranchisement on state resource receipt.

[23] Our estimation sample includes all counties in the 11 southern states (aggregated to account for the consolidations and splits in Virginia), save two for which we are missing control variables.

[24] Again, because our data set includes transfers to local governments, not individuals, the federal pass through dollars would not include payments under social insurance programs such as Social Security or Medicare. This exclusion may be useful, as federal officials' incentives to target black citizens likely differed from those of state officials both pre- and post-VRA.

formula Title I amounts.[25] We address this issue by controlling in our preferred specifications for this county-level poverty rate.

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.[26] 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[27]; only aggregate statistics are available. As a result, we cannot rule out empirically that the elimination of the literacy test raised white enfranchisement in absolute terms, and indeed some historical accounts suggest that some whites were disenfranchised by literacy tests (Kousser 1974). However, state-level estimates of registration rates by race document larger increases for blacks than whites residing in literacy test states from before to after the passage of the VRA.[28] Existing county level registration data (from administrative records and from surveys) also point to a relatively larger impact of the VRA's removal of literacy tests on black registration.[29]

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

[25] The role of state education agencies (SEAs) in the distribution of Title I ESEA funds was largely symbolic. While SEAs were given authority to approve applications for Title I ESEA funds, the available data suggest that most school districts received their entire formula amounts unless deemed by the federal government to have failed to comply with Title VI of the Civil Rights Act (Cascio et al. 2010). Thus, SEAs appear to have exercised little discretion in the distribution of Title I funds in practice.
[26] Turnout data come from Matt Gentzkow and Jim Snyder and from various editions of *America Votes.*
[27] The November Current Population Survey provides data on turnout by race by region (but not county) beginning in 1964.
[28] 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 U.S. Department of Commerce (1972, 1977, 1982-83, 1987 and 1990.)
[29] 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 test would have eroded this white advantage, but it cannot be confirmed due to the lack of post-VRA registration data at the county level. Stanley (1987) uses survey data spanning the years 1952 to 1984 to show that literacy tests are significant negative predictors of black respondents' reporting having voted. Literacy tests are positively, but insignificantly related to white respondents' reporting having voted.

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 increased 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 results for gubernatorial turnout under all specifications presented below are substantively similar and available on request.

Panel B of Table 1A presents summary statistics on voter turnout by presence of a literacy test, again weighting by 1960 county population. As expected, turnout is lower and more variable in gubernatorial elections. 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 enfranchisement (as proxied by turnout) over time.

C. Other County Characteristics

We draw from a number of other data sources (described in the Appendix) to construct controls for the analysis to follow. These variables are summarized in Table 1B, again weighting by 1960 county population. In 1960, counties in states with literacy tests on average had higher black population shares (29 percent versus 15.3 percent elsewhere in the South), higher child poverty rates (24.3 percent versus 17.5 percent), and lower high school completion rates (32.4 percent versus 37.7 percent). In 1976, the year closest to the end of our sample period with data available, counties in literacy test states were also more likely to be under court order to desegregate (51.4 percent versus 43.8 percent), although counties in the two types of states saw similar receipt of funds under the Emergency School Aid Act (ESAA) of 1972 (39 percent versus 37.6 percent), a federal program intended to facilitate racial integration of schools. While counties in both states were equally likely to be home to black colleges, 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, vote share for segregationist Strom Thurmond in the 1948 presidential election was higher in literacy test states (29.5 percent versus 8.6 percent). The population growth rate between 1960 and 1980 was relatively low in literacy test states, though the two regions experienced similar changes in the demographic composition of their populations over these two decades.

The treatment counties in the literacy test states and the comparison counties elsewhere in the South therefore differ – and significantly so – in their observable characteristics. However, most of the differences in average characteristics between the treatment and comparison counties can be explained by differences in black share and child poverty alone. Indeed, in our application below of inverse propensity score weighting to give more weight to those comparison counties that look like treatment counties, we employ a parsimonious specification, using only these two variables, that substantially mitigates and mostly renders insignificant the differences in average characteristics across treatment and comparison counties.[30] Yet, reweighting does not substantively affect our findings.

V. ENFRANCHISEMENT AND STATE TRANSFERS

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 pre-existing (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 larger 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).

[30] The differences remain statistically significant (but small) in the case of presence of another black organization, percent vote for Strom Thurmond, and 1960 to 1980 changes in percent 5-17 and percent unemployed. We discuss the specification of the propensity score model when we employ the propensity-score weighting technique below.

One problem with this approach is that both state aid and enfranchisement may have increased in areas with larger black shares even in the absence of literacy tests being removed by the VRA. For example, civil rights activism, either directly or through an 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 (Reber 2011, Johnson 2011).

We therefore combine the strategy described at the start of this section with the use of a comparison group.[31] That is, we test whether there were *larger* shifts in the distribution of state transfers toward counties with larger black population shares in treatment states than in a group of comparison states, around 1965. Likewise, we should document *larger* gains in turnout for counties with larger black population shares in treatment states before and after the legislation was passed, reflecting enfranchisement. As noted, we limit the comparison group to counties in the four states in the South that did not have literacy tests prior to the VRA – Arkansas, Florida, Tennessee, and Texas – each of which has a history of slavery and of disenfranchising blacks. While the comparison counties on average had lower black population shares in 1960 (Table 1B), 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 both black share and, as mentioned above, to inverse propensity score weighting, which eliminates many of the significant differences in observable characteristics across treatment and comparison counties shown in Table 1B. They are also robust to controlling for proxies for political activism and school desegregation.

B. Event-Study Estimates

[31] An alternative identification strategy would be to limit the sample to county pairs that straddle boundaries between treatment and comparison states and to estimate a model that includes year by county-pair fixed effects. This is the approach used by Naidu (2012) to estimate the effects of implementing literacy tests and poll taxes. Such an approach would account for unobserved determinants of enfranchisement and state transfers at a more local level than our approach. However, it would lower our sample size and is subject to bias from policy-induced migration across state boundaries. Moreover, the effects of disenfranchisement for turnout and public goods in Naidu's study are not particularly sensitive to the inclusion of the year by county-pair fixed effects. These fixed effects matter more for his estimates for land prices, where local unobservables are arguably a greater source of bias.

To set ideas, Figure 1 uses the full sample and 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. The models also include state indicators, to facilitate a within-state interpretation of the coefficient on black share.

Because our identification strategy relies on a relatively larger increase in enfranchisement in higher black share counties of treatment states, it is useful to first consider the estimates for voter turnout rates for presidential elections, our preferred measure of enfranchisement. The solid circle at about -0.01 for 1952 in 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. The change in the slope coefficient on black share in both treatment and comparison counties in 1964 may be due to black voter registration drives across the South[32] or to unusually high interest in the Goldwater-Johnson face-off in counties with higher black shares. Regardless, the co-movement of black share gradients makes clear the need for comparison counties in our estimation strategy.

The effect on enfranchisement is then seen starkly as we move from the 1964 to the 1968 presidential election, the first held after the passage of VRA. While in the elections prior to the passage of the Act, the treatment group dots consistently fell 0.005 log points below those for the comparison group, after the passage of the Act, the solid and hollow circles are nearly atop one another; once literacy tests are removed, the difference in the black share turnout gradient between treatment and comparison states is

[32] Wright (2011) notes that beginning in 1962 the Voter Education Project (VEP), 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. Thus one might wonder whether activist groups alone would have eventually closed the gap in black voter registration between literacy and non-literacy test states. This seems unlikely given the violent resistance that hampered the organization's registration efforts in the Deep South and led the Johnson administration to contemplate federal voting rights legislation even before Bloody Sunday.

removed as well. The elimination of the gap is sustained through the end of the sample period, suggesting that these were permanent gains in turnout, related to changes in black voting eligibility.

We demonstrate the statistical significance of the closing of this gap in Figure 2, Panel A. Here, we plot estimates of the coefficients θ_j (with 95 percent confidence intervals) from the following event-study model:[33]

$$\ln(y_{cst}) = \delta_c + \gamma_{st} + \sum_{j \neq 1960} \mu_j \left(\%bl_c \times D_t^j\right) + \sum_{j \neq 1960} \theta_j \left(lit_s \times \%bl_c \times D_t^j\right) + \varepsilon_{cst} \tag{1}$$

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, δ_c, to account for fixed differences in turnout across counties, and state-by-year fixed effects, γ_{st}, to account for time-varying, state-specific shocks to turnout. Such state-specific shocks might stem from changes in state economic conditions or institutions, like state rules regarding redistricting; the γ_{st} will also capture the impacts of the VRA on aggregate state turnout.[34] 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}), so that the model is identified. The coefficient μ_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.

Figure 2 Panel A thus presents estimates of the *difference* in the black share gradients in turnout in the treatment and comparison states shown in Figure 1 Panel A *relative to* the difference observed in 1960.[35] As the pre-VRA (1952, 1956 and 1964) circles indicate, the treatment-comparison differences in the slope coefficients on black share remained almost unchanged (and statistically indistinguishable from

[33] 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.
[34] One mechanism through which the VRA may have affected turnout is by increasing state political competition (see Besley, Persson, and Strum, 2010). In the analogous regression for per-capita state transfers, these fixed effects will capture the impacts of the VRA on aggregate state transfers to local governments. We know of no study that has estimated this relationship. Husted and Kenny (1997) document increases in state welfare expenditures following the removal of literacy tests, but the vast majority of state transfers to counties in the South during our sample period (roughly 83 percent) were for either education or highways.
[35] That is, the estimates in Figure 2 re-normalize the estimates in Figure 1 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).

one another) in the elections prior to the passage of the Act. Thus, even though the comparison counties have some different observable characteristics from the treatment counties, *trends* in turnout with respect to black population share are almost identical in the two groups of states prior to the VRA being passed, suggestive of the validity of the comparison group. There is a sharp and sustained change beginning in 1968, however, reflecting the relatively large increases in turnout in counties with higher black population shares in states that were no longer allowed to employ literacy tests as a barrier to registration. The 95 percent confidence interval bars indicate that the narrowing of the gap in turnout within treatment states is highly statistically significant, and it remains so over the remainder of our sample period.

In Panel B of Figures 1 and 2, we present analogous estimates for gubernatorial turnout. 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.[36] We omit the interactions with the indicator for the 1957-60 period so that the model is identified.[37] Although these estimates are noisier, they follow a pattern similar to our findings for presidential turnout. The pre-VRA (1953, 1957 and 1961) trend is similar in treatment and comparison counties. Between 1961 and 1965, both series show a steep increase; however, the jump in the treatment series is larger, indicating the relative increase in the black share turnout gradient in treatment states in 1965.

Thus, the first two panels of Figure 1 and Figure 2 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 the figures, we turn to our central question – whether this increase in eligibility was accompanied by an increase in state transfers received per capita. In these panels, the years are marked in five-(fiscal) year intervals beginning with 1957, to match the availability of the COG data. Although we have only two pre-VRA observations, they are quite suggestive of a similar pre-trend: in Figure 1, the treatment and comparison circles are almost exactly on top of one another. Again, this suggests the validity of the comparison group, as it appears to be capturing what would have happened in the treatment states if

[36] In the few states with biennial gubernatorial elections, we use the election following but closest to the beginning of the interval.
[37] We omit the bin that includes the year 1960 for consistency with presidential election results.

literacy tests had remained legal. Having a comparison group is also again critical: the slope coefficient on black population share moves from negative to positive in both series from 1957 to 1962, reflecting civil rights era gains – in this case in terms of funding rather than voting rights – that accrued in predominantly black areas throughout the South.[38]

The estimated impact of the removal of literacy tests with the VRA on funding patterns can then be seen in comparing the post-1965 treatment and comparison series. Because of lags in budgeting and funding, we do not necessarily expect a sharp break in the relative treatment and comparison patterns in 1967, our first post-treatment year. However, a marked divergence between the two series does emerge. The difference intensifies in the 1970s, as state aid becomes sharply redistributive toward areas with higher black populations in the treatment states. Because of the noisiness of the data, the difference is only significant in 1977, as shown in Figure 2. However, the coefficients clearly point to an increase in state transfers to accompany the increase in enfranchisement.

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 subjecting the estimates to a number of specification and robustness checks and of summarizing magnitudes, we now move to the long-difference specification. Because budgetary responses to a change in the electorate may not occur instantaneously (due to lags in assuming office following elections, setting a new budget, and that budget's taking effect), we take a twenty-year difference:

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

where $\Delta\ln(y_{cs})$ is the growth rate in either turnout rates or real per-capita state transfers over a twenty-year period spanning the VRA, x_{cs} is a vector of controls, γ_s is a state fixed effect (a state trend in this

[38] 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 constitutes 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).

difference specification), and all other variables are as previously defined. Model (2) therefore differences model (1) across two data points at the start and end of our sample period, and adds further controls.[39] Thus, the coefficient μ characterizes the (within-state) change in the slope coefficient on black population share in comparison counties, and $\mu + \theta$ does the same for treatment counties; the coefficient of interest is again the difference, θ. Estimates of θ 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 2 provides estimates of θ from equation (2) alongside estimates of the event-study coefficients that were shown visually in the figures, the θ_j from model (1).[40] For the long-difference estimates for per-capita state transfers, we average the two years of data at the beginning and end of our sample period to mitigate budgetary noise; 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 (e.g., 1957-1960 and 1977-1980). The panels of Table 2 pertain to the same respective outcomes as the panels in the two figures.

Of particular interest are the long-difference coefficients for state transfers and for presidential election turnout, our preferred proxy for enfranchisement. The significant 0.00458 in column 2 of Panel A indicates that each one percentage point increase in black population share is associated with a 0.46 percent increase in presidential turnout in treatment counties relative to comparison counties. This estimate implies that half of the 10 percentage point closure of the turnout gap between treatment and comparison counties over this twenty-year period (Table 1A, Panel B) is explained by the removal of literacy tests.[41] The significant 0.00429 coefficient in the final column of the table indicates that each one percentage point increase in black population share is associated with a 0.43 percent increase in per-capita

[39] We choose to add controls in model (2) instead of model (1) for concise exposition, as well as due to the fact that some controls (e.g., those based on decennial census data) are not available in the intervening years.
[40] We continue to weight the long-difference models by 1960 county population. Standard errors are heteroskedasticity robust.
[41] The percent black main effects are about the same size and are statistically significant, shown in Table A2. 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.

state transfers in treatment counties relative to the comparison counties.[42] This estimate implies a 12.4 percent relative increase over the twenty-year period at the mean of 1960 black population share in treatment counties. Normalizing the transfer results by the enfranchisement results formally using a two-stage least squares (2SLS) model, where we instrument for the growth in presidential turnout with lit_s x $\%bl_c$, we estimate that a one percent increase in eligibility-driven turnout increased county residents' state resources by 0.94 percent. We cannot rule out that the elasticity of transfers with respect to enfranchisement is one. We show this 2SLS estimate in the first column of Table 3, after repeating the estimates of θ from model (2) for presidential turnout and real per-capita state transfers, respectively.[43]

The remainder of Table 3 examines the sensitivity of the baseline estimates to variations on the specification. We begin by adding some basic controls. As described above, state education agencies in practice did not deviate from the formula amounts allocated to counties by the federal government under Title I of the ESEA, which are included in our state transfer measure. The formula amounts were the product of two factors. The first was a strong positive correlate of $\%bl_c$ – the county's eligibility rate, determined principally on the basis of the share of its school-aged children living in poverty as of the previous Census. The second was one half of average per-pupil spending on education in the state (net of federal transfers) two years before (the "state factor") – a figure that was slightly higher in the comparison region.[44] Thus Title I formula amounts for counties are *negatively* correlated with lit_s x $\%bl_c$ – and positively correlated with the growth in state transfers – imparting a downward bias on our baseline estimates. To remove this bias, we add to our baseline specification the 1960 child poverty rate used in

[42] This is the average of the 1977 and 1982 coefficients shown in the event-study specification. While we chose a twenty-year difference in part because of the ease with which one can obtain decennial controls at the county level, one can see from comparing the 1972 and 1982 event-study coefficients (Table 2, Panel C) that our results would be nearly identical if we chose to average 1972 and 1977 for our post period instead of 1977 and 1982.

[43] As a point of comparison, the final row in the first column gives the OLS relationship between transfers and 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. The OLS estimate is significantly lower than 2SLS, suggesting 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.

[44] Starting in the third year of the program (fiscal year 1968), the state factors for those states spending below the national average were leveled up to that average, but variation in state factors remained for higher-spending states (see U.S Department of Health, Education, and Welfare 1969, Cascio, et al. 2010, Cascio, Gordon, and Reber 2012). In fiscal year 1977, Florida and Virginia had state factors above the minimum.

the initial Title I formula, individually and interacted with lit_s, in column 2.[45] The new controls enter the state transfer specification with the expected signs (Table A1),[46] and consistent with expectations, the 2SLS estimates increase in magnitude, reflecting an increase in the magnitude of the reduced-form estimate for state transfers. In the remainder of the paper, we treat this specification as our base model.

The remainder of Table 3 is devoted to other specification checks. In column 3, we explore the possibility that the southern literacy test was indeed a test of literacy rather than skin color by adding the 1960 county-level high school completion rate to the column 2 specification, both directly and interacted with lit_s. Neither the completion rate nor the interaction term is a significant predictor of the growth in voter turnout (see Table A2). Our estimates are also little affected, consistent with the historical evidence that the literacy test was more accurately described as a test of race than one of reading ability.

The differences in mean characteristics of treatment and comparison counties (Table 1B) may raise the concern that counties in states without literacy tests are not valid comparisons for counties in states with such tests. Our pre-trend analyses in Figures 1 and 2 support the validity of our comparison group. We provide further support for our identification assumption in the remaining columns of Table 3, where we investigate the possibility of bias due to a lack of common support using three approaches: (1) by restricting the sample to counties with a 1960 black share at least as great as the 10th percentile of the distribution for comparison counties (2.2 percent) and no more than the 90th percentile of the distribution for treatment counties (49.9 percent) (column 4); (2) by trimming the sample to include only those treatment and comparison counties where there is common support in the propensity score (column 5); and (3) by using the estimated propensity scores to weight the regression (column 6).[47] Results are robust

[45] Our estimates are substantively similar when we use the 1970 poverty rate, which would have been used in allocating grants in fiscal years 1977 and 1982, in lieu of the 1960 poverty rate. We choose not to use the 1970 property rate in our main specification since it would have been determined after implementation of the VRA.

[46] The relationship between 1960 child poverty and per-capita state transfers is positive: the federal government's allocation formula for Title I was an increasing function of child poverty. The interaction with lit_s is negative, reflecting the fact that another component of the allocation formula was based on matching state efforts, and states with literacy tests on average spent less on education.

[47] The first restriction eliminates about a quarter of our sample but narrows the difference in average black population share across the two groups; in the restricted sample, the mean 1960 black population share is 26.2 percent in the treatment states and 16.1 percent in the comparison states. Using the second method, where 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, we retain more than 95 percent of our sample. Using the third method, we keep the

to the three approaches. Based on the evidence of Table 3 coupled with the trends presented in Figures 1 and 2, it therefore does not appear that a lack of common support is biasing our estimates.

VI. ROBUSTNESS

We have found robust evidence that the removal of literacy tests increased the growth in per-capita state transfers to localities with higher black population shares. We postulate that the mechanism by which these tests impacted funding was voting eligibility. However, to say that the 1960s and 1970s was a period of great change in the southern United States is of course a gross understatement. Political activism, demographic changes, and federal interventions into education and voting practices in the name of civil rights all 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.

A. Setting-Specific Predictors of State Transfers

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 (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 the 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 of fall 1976, the latest available year. The controls enter with positive significant coefficients in the state transfer specification (Table A1), but as shown in column 2 of Table 4,

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sample from the second and assign comparison counties weights of the odds ratio of the propensity score multiplied by 1960 population. As noted above, the third approach eliminates significant differences across the treatment and comparison counties in most of the characteristics summarized in Table 1B.

adding these controls does not substantively alter our findings from our baseline result (Table 3, column 2), repeated in column 1 of Table 4 for comparison.[48]

In the wake of school desegregation, politicians in treatment states may have also been motivated to direct more state aid 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 for 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 prior to school desegregation – 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) .

While local school boards in treatment states were more likely to have continued this practice of expropriation until schools desegregated, there were treatment and comparison states that had largely abandoned it before then. If our state transfer and school spending findings hold for this subset of states, it would help to rule out this alternative explanation. To perform this test we estimate race-specific pupil-teacher ratios at the county level from school level data on total teachers and enrollment by race, drawn from Office of Civil Rights (OCR) 1968 school survey data.[49] We then limit the sample to states where the black-white difference in pupil-teacher ratios as of 1968 was the least related to 1960 county black share, controlling for the 1960 child poverty rate. This restriction eliminates counties from four treatment states: Alabama, Georgia, Louisiana, and South Carolina. As shown in column 3, though the estimate loses precision, it is similar in magnitude to the full sample estimates shown in the first column.[50]

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[48] Unfortunately, the 1976 OCR district survey data are incomplete for several counties. We determine the share of enrolled students in a county covered by the OCR using basic enrollment information from the 1976 Elementary and Secondary General Information System (ELSEGIS), which covers the universe of school districts during the 1976-77 school year. We then weight the specification of Table 4 column 2 by 1960 county population times this share. Results are similar when we drop counties where the OCR coverage is incomplete (e.g., counties where under 75 percent of pupils are represented).

[49] School level data for 1967 are available but are less incomplete. For earlier in the 1960s, we would need to rely on state records, and most states stopped reporting school resources by race by this time. Given that the first local elections under the VRA took place in the year 1965, that there are budgetary lags, and that not much desegregation had occurred in the South even by 1968 (Cascio et al. 2008), 1968 does not seem like an unreasonable baseline.

[50] Because the OCR school surveys do not cover all districts in a county, we weight regressions by the 1960 county population times share of a county's enrollment represented in the OCR as of 1976 (determined by comparison of the 1976 OCR with the

It is also possible that our estimates are 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 this possibility in column 4. As proxies for civil rights activism, we control for indicators for whether the county had an NAACP, whether the county had some other black organization, and whether the county had 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. The Strom Thurmond vote share is a strong positive predictor of growth in presidential turnout (Table A2), which is perhaps an indication that some of the post-VRA increase in turnout was in the form of a white protest vote. The lit_s x $\%bl_c$ coefficient is however not attenuated in the turnout specification by the inclusion of this control, lending credence to the supposition that the coefficient reflects increased black enfranchisement. In the state transfer specification, the Strom Thurmond vote share enters insignificantly, but the presence of the NAACP, another black organization, and a black college are all positive and significant predictors of the change in state transfers (Table A1). That our basic result is robust to the inclusion of all of these controls provides evidence against activism and further evidence against politicians' appeasement of whites as alternative mechanisms for the increased transfers to counties with higher black shares.

There are three other potential alternative contemporaneous explanations for our findings, all political in nature. 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 corrected. We reproduce this finding (Table A1) when we control for the same measure of initial county representation that they employed – the "relative

1976 ELSEGIS). Results are similar when we drop counties where the OCR school survey coverage is incomplete (e.g., counties where under 75 percent of pupils are represented).

representation index" (RRI), defined as the number of legislative seats per person in the county, divided by that same figure for the state 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 almost contemporaneously with 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 the sixth column of Table 4, we account for possible omitted variables bias stemming from failure to account for elimination of the poll tax by adding to the regression the interaction between an indicator for having a poll tax in the pre-period and 1960 black population share. Our results are again substantively unchanged, consistent with previous research findings suggesting that the elimination of the poll tax did not have a disproportionate effect on black voting.[51]

Finally, not only were there events concurrent with the VRA that may have impacted state funding to localities, 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 political process, particularly in former literacy test states. However, an attempt to avoid scrutiny or federal intervention seems an unlikely mechanism for the increased state funding over our twenty-year period, as the additional scrutiny was largely limited to the first two years after passage of the VRA.[52] Thus focusing on a long difference allows us to examine outcomes beyond the period of direct federal intervention.

A second way in which the VRA led to increased oversight of literacy test states is through Section V, which as described earlier, mandates that localities request permission from the U.S. Department of Justice to change any of their voting rules. All of the treatment states have been subject to Section V since 1965. We test for the impact of pre-clearance on state transfers by noting that two

[51] Filer, Kenny, and Morton (1991) find no evidence that the presence of a poll tax had larger effects on turnout in counties with higher nonwhite population shares.
[52] Ninety-six percent of those enrolled by federal examiners were enrolled in the first two years after the VRA, with 75 percent in the first year (Valelly, 2005).

comparison states, Florida and Texas, have been subject to pre-clearance since the 1975 renewal of the VRA. Thus, to account for the impact of pre-clearance, we drop Arkansas and Tennessee from our sample, relying on only Florida and Texas as the comparison states and thereby limiting the sample to counties in states subject to Section V. As shown in the final column of Table 4, the reduced-form coefficient of interest in both the turnout and state transfer specifications nearly doubles in this restricted sample. Our 2SLS estimate of the impact of enfranchisement-induced turnout is however unchanged. We cannot reject an elasticity of one.

While suggestive, this test is admittedly imperfect. If the effects of pre-clearance take more than seven years to reach fruition, then our comparison states are imperfect controls. And in fact determining whether state politicians were motivated to redistribute to areas with higher black shares because of fear of 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 new found political power in 1965 was that right being backed by the federal government.

B. General Predictors of State Transfers

The literature on the distribution of public goods acknowledges a role for political considerations in the distribution decision. But this literature has also identified determinants of local public goods receipt aside from politics. For example, counties with higher population growth may see less growth in per-capita funding if budgeting lags population growth. Ansolabehere, Gerber, and Snyder (2002) find such a phenomenon in examining the same data used here, but for the entire country. Because of a concern that residential mobility may be endogenous to the VRA, we do not control for population growth over our timeframe in our preferred estimates. We do so in column 2 of Table 5 to test for this possibility. As in Table 4, the first column repeats our preferred specification from Table 3 column 2 for ease of comparison. The coefficient on population growth in the model for state transfers is negative (Table A1), consistent with expectations. Although results are attenuated, the 2SLS estimate remains significant at the 10 percent level and we continue to be unable to reject an elasticity of state transfers with respect to turnout of one.

The bureaucratic model of decision-making hypothesizes that public goods are distributed to the neediest communities based on bureaucratic formulas, not politics. Consistent with this theory, the literature finds that population age and poverty status are correlates of public good receipt.[53] Once again we do not want to control for changes in county demographics in our basic specification because of a concern that these characteristics may be endogenous; however, we test the possibility that bureaucratic rules explain our findings by controlling for changes in the shares of the county population that is school-aged (5 to 17), elderly (65 and over), in poverty, or unemployed between 1960 and 1980. These variables generally enter the state transfer regression with the expected signs: increases in school aged children, unemployment and poverty, all markers of greater need, are significantly associated with growth in state transfers (Table A1). Because treatment counties with larger 1960 black shares experienced significantly larger increases in the school-aged and low-income shares over the 20 year period, it is not surprising that the reduced-form coefficients in both our turnout and state transfer specifications fall in magnitude with the addition of these controls. Nonetheless, even controlling for need, our 2SLS estimates, shown in column 3 of Table 5, remain statistically indistinguishable from one.

VII. ENFRANCHISMENT AND EDUCATIONAL SPENDING AND OUTCOMES

Thus far, we have produced robust evidence of shifts in state transfers toward more predominantly black counties in states where literacy tests were outlawed by the VRA, following the Act's passage. We have provided both qualitative evidence (from the historical record) and quantitative evidence (shifts in state funding align with increases in black enfranchisement over the same period) consistent with enfranchisement as the mechanism through which areas with larger black share received larger state transfers. And we have provided evidence against several alternative explanations for these funding patterns drawn from the public goods literature generally and from contemporaneous historical events more specifically. That the same areas that saw increased enfranchisement saw increased state

[53] See for example Boyle and Jacobs (1982), Cingranelli (1981), Koehler and Wrightson (1987), Lee (1994), Miranda and Tunyavong (1994), and Mladenka and Hill (1978).

transfers is consistent with models of distributive politics—state politicians targeting newly enfranchised blacks in order to maximize vote share.

But just as our historical time and place led to questions about politicians' motives to target blacks, readers may also wonder – given the layers of bureaucracy separating the governor from the school teacher – whether the funds, largely earmarked for education, actually reached the children of black voters. In this section, 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.[54]

Before we present these findings, we demonstrate that the removal of literacy tests had an impact on education spending. In Table 6, after first repeating our basic specification for the long difference in per-capita state transfers for comparison, we present estimates of the coefficient on lit_s x $\%bl_c$ for the long difference in per-capita school expenditures in column 2. The coefficient is positive and statistically significant at the 10 percent level. The implied 0.58 elasticity of per-capita county education expenditures with respect to presidential turnout rates implies a 48 cent increase in education spending for every dollar increase in state transfers.[55,56]

[54] An alternative approach to determining whether blacks were receiving relative 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 only 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).

[55] The implied elasticity of education spending with respect to state transfers is the ratio of their turnout elasticities (0.469=0.58/1.125 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. Note that the resulting 48 cent figure is a lower bound on the increase in education spending for each additional dollar of *education* aid from the state. If we instead assume that 73 percent of the average increase in state transfers was targeted for education, as is true on average in the South, the elasticity estimate implies a 66 cent increase in education spending for every additional dollar of state education aid received. As a point of comparison, Cascio, Gordon, and Reber (2012) find that a dollar increase in Title I funding in the South over the second half of the 1960s was associated with about a 50 cent increase in school spending.

[56] In fact, reductions in local tax burdens and educational spending alone account for at least 82 cents of each dollar transferred. Our estimates imply a 34 cent reduction in real per-capita local total tax revenues for each dollar increase in real per-capita state transfers, which coupled with the 48 cent increase in education spending accounts for 82 cents of each transfer dollar. If instead we assume that 73 percent of the average increase in state transfers was targeted for education, we can account for the entire transfer dollar (a 34 cent reduction in real per-capita total tax revenues and a 66 cent increase in education spending; see previous footnote.) 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 spending categories.

As suggestive evidence that these additional education dollars were spent on black children, we first demonstrate that treatment counties experienced a greater reduction over time in the black-white differential in pupil-teacher ratios than comparison counties with same black population shares. To do so, we return to the same OCR school-level data used to define the estimation sample in the third column of Table 4. The year 1976 is the latest in our sample period in which district coverage in the OCR school survey is high, so we focus on the change in pupil-teacher ratios from 1968 to 1976. We run models of the form of equation (2) in which the outcome is the black-white difference in the 1968 to 1976 change in log pupil-teacher ratio.[57] To improve the plausibility of our identifying assumption that these two sets of counties would have seen similar growth in the black-white differential in pupil-teacher ratios in the absence of the VRA, we limit the sample to that used in Table 4, column 3 – states in which 1960 county black share is a relatively weak predictor of the 1968 black-white pupil-teacher ratio differential.

The results (Table 6, column 3) indicate that the relative pupil-teacher ratio fell significantly faster in treatment areas. Thus, the same areas that saw an increase in state transfers saw a faster increase in the relative quality of black public education. 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 district as a whole. (See Appendix for the dissimilarity index formula.).

A 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 tabulations from the Census of Population, which afford us more complete data for a greater number of counties in our restricted subset of states over a longer (and our more standard) period of time. We run our basic model employing the 1960 to 1980 change in the black-white differential in (the natural

[57] The substantive findings are unchanged if we do not take the natural log. We only take the natural log to have an elasticity interpretation for 2SLS, consistent with our 2SLS estimates for state transfers and education spending.

log of) school enrollment rates of 16-17 year olds as our dependent variable.[58] As shown in the final column of Table 6, the relative convergence of black to white school enrollment rates is statistically significant.

In summary, Table 6 demonstrates the same high black share areas in literacy test states not only received more funds, but saw gains in the share of black teenagers enrolled in school and the quality of black children's educational experience more generally. We recognize that these tests are not dispositive—the marginal dollar may purchase more in terms of black education than white; money may be spent in ways not captured by pupil-teacher ratios or students educated.[59] Nonetheless, Table 6 supports the contention that money targeted to communities with higher black populations following the VRA reached newly enfranchised blacks (and their children).

VIII. CONCLUSION

The VRA removed literacy tests as a barrier to black citizens' political participation in seven of 11 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 post-VRA shifts in the distribution of state transfers toward localities with larger black populations in literacy test relative to non-literacy test southern states. We demonstrate that not only did enfranchisement in higher black share localities increase disproportionately in treatment states following the Act's passage, but that state transfers to these localities increased 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 on improvements in the relative share of black teenagers

[58] 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 older individuals (like 18 or 19 year olds) because of concerns about selective migration.
[59] For example, Cascio, Gordon and Reber (2012) show that increases in school budgets in the South resulting from the introduction of Title I decreased high school dropout rates for whites but not for blacks.

enrolled in school and 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 political support. That enfranchisement was accompanied by increased resource receipt suggests that 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. The welfare impact is a question whose full measure we leave for future research.[60]

[60] We are not the first to assert the importance of this question. Noting the surge in voter registration following the VRA, Wright (1999) urged economic historians to examine the connection between enfranchisement and economic advancement.

APPPENDIX

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 6, 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* (Washington, DC: Elections Research Center, Congressional Quarterly). 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 City and County 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.[1] The denominator is the number of 5 to 17 year olds in the county as of 1960, which we downloaded from the National Historical Geographic Information System (Minnesota Population Center 2004).

3. *High School Completion Rate (1960):* Percent of the county population with at least 12 years of education is from the 1960 City and County Data Book.

4. *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.

[1]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.

5. *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): 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.

6. *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 David Leip's Atlas of US Presidential Elections (http://uselectionatlas.org/).

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 1980 City and County Data Book, 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 1980 City and County Data Book.

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 City and County 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 1980 City and County Data Book.

4. *Change in the Unemployment Rate, 1960-1980*: We use the 1960 unemployment rate reported in the 1960 City and County 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 1980 City and County Data Book.

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* (Office for Civil Rights 1970) and *Fall 1976 Elementary and Secondary School Civil Rights Survey* (Office for Civil Rights 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:

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

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.

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Table 1A. Descriptive Statistics: Census of Governments and Election Turnout Outcomes

	With Literacy Test		Without Literacy Test	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
		A. Funding		
Per-Capita State Transfers ($2009), 1957/62	356.4	116.0	317.2	91.4
Per-Capita State Transfers ($2009), 1977/82	762.6	192.3	674.0	197.2
Δ ln(Per-Capita State Transfers)	0.82	0.33	0.76	0.34
Per-Capita Education Spending ($2009), 1977/82	513.4	130.10	612.9	188.21
Per-Capita Education Spending ($2009), 1957/62	931.2	196.77	1000.1	244.73
Δ ln(Per-Capita Education Spending)	0.61	0.24	0.50	0.23
		B. Election Turnout		
Presidential Election Turnout Rate (%), 1960	37.2	13.2	44.8	8.3
Presidential Election Turnout Rate (%), 1980	46.9	8.1	47.8	6.1
Δ ln(Presidential Election Turnout Rate)	0.27	0.34	0.08	0.20
Gubernatorial Election Turnout Rate (%), 1957-60	22.3	18.6	24.5	14.0
Gubernatorial Election Turnout Rate (%), 1977-80	35.1	12.2	31.2	7.5
Δ ln(Gubernatorial Election Turnout Rate)	0.77	0.83	0.37	0.46
Observations (counties)	638		491	

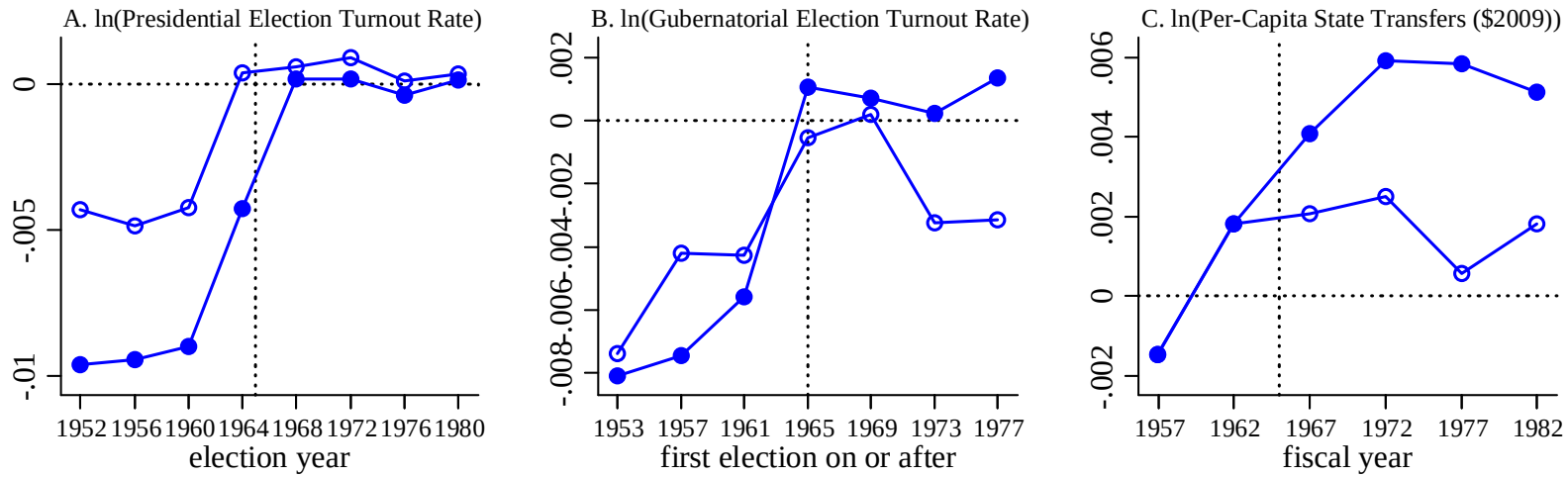
Notes: The estimation sample includes all but two counties in the South (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, and VA). States with literacy tests still in place immediately prior to the VRA are AL, GA, LA, MS, NC, SC, and VA. Gubernatorial turnout is for the election within the stated four-year interval closest to the interval's starting point. Statistics are weighted by 1960 county population. See Appendix for sources.

Table 1B. Descriptive Statistics: County Characteristics

	With Literacy Test		Without Literacy Test	
	Mean	SD	Mean	SD
	(1)	(2)	(3)	(4)
Percent Black, 1960	29.0	16.2	15.3	11.3
Child Poverty Rate, 1960	24.3	14.6	17.5	11.1
Percent Completed High School, 1960	32.4	10.7	37.7	9.9
Percent of County Enrollment in Districts:				
Under Court Order to Desegregate, 1976	51.2	47.3	43.8	42.7
Receiving ESAA funds, 1976	39.7	45.6	37.6	38.4
=1 if Has NAACP Chapter, 1960 (x100)	53.8	49.9	65.5	47.6
=1 Has Other Black Organization, 1960 (x100)	6.2	24.1	0.1	3.8
=1 Has Black College, 1960 (x100)	24.1	42.8	24.2	42.9
Percent Vote for Strom Thurmond, 1948	29.5	30.2	8.6	12.1
Relative Representation Index, 1960	1.0	0.6	1.0	0.8
1960 to 1980 Change in:				
ln(Population)	22%	26%	39%	30%
Percent of Population Ages 5 to 17	-5.2	1.9	-4.1	1.5
Percent of Population Ages 65 and over	3.2	1.5	3.2	2.6
Percent of Families below Poverty Line	-23.1	11.3	-20.0	10.9
Percent Unemployed	5.4	3.2	3.4	3.4
Observations (counties)†	638		491	

Notes: The estimation sample includes all but two 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 Appendix for sources. † For the school desegregation variables, there are 635 and 490 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.

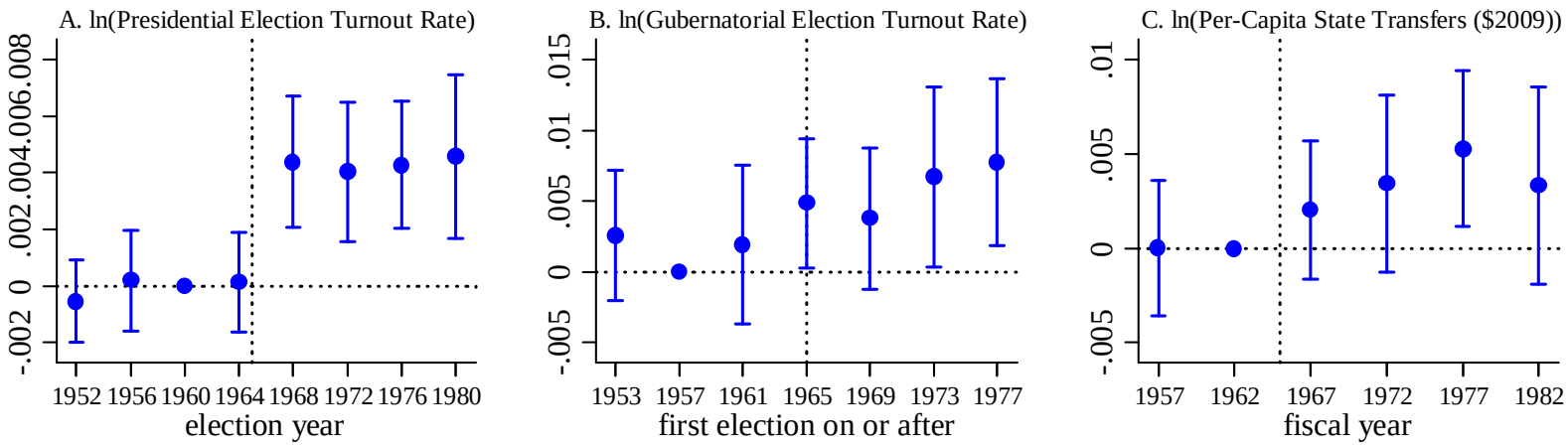
Figure 1. Trends in the Gradients of Voter Turnout Rates and Per-Capita State Transfers in 1960 County Percent Black, by Presence of a Literacy Test prior to the VRA



Solid Circles: With Literacy Test. Hollow Circles: Without Literacy Test

Note: Each graph shows coefficients on county % black in 1960 by year, separately for states with literacy tests (AL, GA, LA, MS, NC, SC, VA) and for states in the South without literacy tests (AR, FL, TN, TX), prior to the VRA. The unit of observation is a county. Regressions also include state fixed effects and are weighted by 1960 county population. The dotted vertical line is at 1965, the year that the VRA was passed.

Figure 2. 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



Note: Each graph shows coefficients (95% confidence intervals) on the interactions presence of a literacy test (indicator) X year (indicator) X county % black in 1960. Interactions with indicators for 1960, 1957-60, and 1962 are omitted in Panels A, B, and C, respectively, so that the model is identified. The unit of observation is the county. All specifications include county and state-by-year fixed effects and year indicators interacted with county %black in 1960. The estimation sample includes all but two counties in the South (AL, AR, FL, GA, LA, MS, NC, SC, TN, TX, VA). Specification is weighted by 1960 county population. The dotted vertical line is at 1965, the year that the VRA was passed.

Table 2. Baseline Event-Study and Long-Difference Estimates

A. ln(Presidential Election Turnout Rate)			B. ln(Gubernatorial Election Turnout Rate)			C. ln(Per-Capita State Transfers)		
	County X Year	Long Diff. 1960 to 1980		County X Year	Long Diff. 1957-60 to 1977-80		County X Year	Long Diff. 1957/62 to 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	-0.000537							
	(0.000735)							
year=1956	0.000194		1953>=year>1957	0.00255		year=1957	1.34e-05	
	(0.000900)			(0.00232)			(0.00181)	
year=1960	-		1957>=year>1961	-		year=1962	-	
year=1964	0.000121		1961>=year>1965	0.00194		year=1967	0.00203	
	(0.000890)			(0.00283)			(0.00184)	
year=1968	0.00439***		1965>=year>1969	0.00486**		year=1972	0.00345	
	(0.00116)			(0.00230)			(0.00237)	
year=1972	0.00403***		1969>=year>1973	0.00376		year=1977	0.00527**	
	(0.00124)			(0.00252)			(0.00208)	
year=1976	0.00428***		1973>=year>1977	0.00672**		year=1982	0.00332	
	(0.00114)			(0.00321)			(0.00265)	
year=1980	0.00458***		1977>=year>1980	0.00775***				
	(0.00147)			(0.00297)				
1960 percent black		0.00458***	1960 percent black		0.00775***	1960 percent black		0.00429**
X literacy test indicator		(0.00137)	X literacy test indicator		(0.00275)	X literacy test indicator		(0.00176)
Observations	9,032	1,129		7,902	1,129		6,774	1,129
R-squared	0.894	0.708		0.920	0.788		0.869	0.425
Controls:								
1960 percent black	X	X		X	X		X	X
X year indicators	X			X			X	
State indicators	X	X		X	X		X	X
X year indicators	X			X			X	
County fixed effects	X			X			X	

Notes: The estimation sample includes all but two 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. Standard errors in columns (1), (3), and (5) are robust for heteroskedasticity and for correlation of error terms within county over time. Standard errors in columns (2), (4) , and (6) are heteroskedasticity robust. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table 3. Long-Difference Estimates: Sensitivity to Specification

	(1)	Child poverty (2)	A test of literacy or of race? (3)	Common support in: 1960 black share (4)	Common support in: propensity to be in literacy test state (5)	Common support in p-score + inverse p-score weighting (6)
			A. Δ ln (Presidential Election Turnout Rate), 1960 - 1980			
			First-stage			
1960 percent black	0.00458***	0.00468***	0.00423***	0.00612***	0.00497***	0.00343***
X literacy test indicator	(0.00137)	(0.00153)	(0.00162)	(0.00209)	(0.00146)	(0.00122)
R-squared	0.708	0.708	0.724	0.674	0.703	0.715
			B. Δln (Per-Capita State Transfers), 1957/62 - 1977/82			
			Reduced-form			
1960 percent black	0.00429**	0.00583***	0.00604***	0.00735**	0.00661***	0.00520**
X literacy test indicator	(0.00176)	(0.00208)	(0.00215)	(0.00291)	(0.00213)	(0.00203)
R-squared	0.425	0.429	0.430	0.444	0.435	0.532
			Two-stage Least Squares			
			(Instrument: 1960 percent black X literacy test indicator)			
Δ ln (pres. election turnout rate)	0.937**	1.245**	1.428**	1.200**	1.331**	1.518**
	(0.434)	(0.560)	(0.725)	(0.546)	(0.531)	(0.757)
Root MSE	0.296	0.323	0.338	0.308	0.325	0.320
			Ordinary Least Squares			
Δ ln (pres. election turnout rate)	0.0658	0.0694	0.0744	0.0347	0.0734	0.139*
	(0.0809)	(0.0811)	(0.0880)	(0.104)	(0.0879)	(0.0736)
R-squared	0.420	0.421	0.422	0.434	0.424	0.528
Root MSE	0.258	0.258	0.258	0.251	0.256	0.236
Observations (counties)	1,129	1,129	1,129	822	1,090	1,090
Controls:						
1960 child poverty rate		X	X	X	X	X
" X literacy test indicator		X	X	X	X	X
1960 high school completion rate			X			
" X literacy test indicator			X			

Notes: All models include state indicators and 1960 percent black. The full estimation sample includes all but two 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 10th percentile of the percent black distribution for states without literacy tests and no more than the 90th percentile of the percent black distribution for states with literacy tests. 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 4. 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)
				A. Δ ln (Presidential Election Turnout Rate), 1960 - 1980			
				Reduced-form			
1960 percent black	0.00468***	0.00481***	0.00450**	0.00548***	0.00471***	0.00464***	0.00905***
X literacy test indicator	(0.00153)	(0.00150)	(0.00194)	(0.00152)	(0.00155)	(0.00149)	(0.00243)
R-squared	0.708	0.709	0.759	0.717	0.709	0.709	0.738
				B. Δln (Per-Capita State Transfers), 1957/62 - 1977/82			
				Reduced-form			
1960 percent black	0.00583***	0.00514***	0.00579**	0.00574***	0.00499***	0.00576***	0.0123***
X literacy test indicator	(0.00208)	(0.00198)	(0.00274)	(0.00187)	(0.00178)	(0.00202)	(0.00328)
R-squared	0.429	0.473	0.391	0.476	0.506	0.431	0.394
				Two-stage Least Squares			
				(Instrument: 1960 percent black X literacy test indicator)			
Δ ln (pres. election turnout rate)	1.245**	1.068**	1.289*	1.046***	1.059**	1.241**	1.361**
	(0.560)	(0.469)	(0.736)	(0.392)	(0.452)	(0.564)	(0.532)
Root MSE	0.323	0.297	0.338	0.295	0.289	0.323	0.342
Observations (counties)	1,129	1,125	509	1,127	1,129	1,129	959
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	

Notes: All models include state indicators, 1960 percent black, and the 1960 child poverty rate, 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 24th 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 OCR district survey. 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 5. Long-Difference Estimates: Robustness to General Predictors of State Transfers

	Baseline (1)	Infrequent updating of funding formulas (2)	Changes in categorical eligibilty for state funds (3)
	A. Δ ln (Presidential Election Turnout Rate), 1960 - 1980		
	First-stage		
1960 percent black	0.00468***	0.00470***	0.00379***
X literacy test indicator	(0.00153)	(0.00146)	(0.00141)
R-squared	0.708	0.708	0.720
	B. Δln (Per-Capita State Transfers), 1957/62 - 1977/82		
	Reduced-form		
1960 percent black	0.00583***	0.00407**	0.00317**
X literacy test indicator	(0.00208)	(0.00189)	(0.00155)
R-squared	0.429	0.506	0.567
	Two-stage Least Squares		
	(Instrument: 1960 percent black X literacy test indicator)		
Δ ln (pres. election turnout rate)	1.245**	0.866*	0.835*
	(0.560)	(0.478)	(0.493)
Root MSE	0.323	0.274	0.268
Observations (counties)	1,129	1,129	1,129
Additional Controls (all 1960-80):			
Δ ln(population)		X	
Δ % of population aged 5-17			X
Δ % of population aged 65+			X
Δ % of families in poverty			X
Δ % unemployed			X

Notes: All models include state indicators, 1960 percent black, and the 1960 child poverty rate, 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 6. Long-Difference Estimates: Education Spending, Quality, and Quantity

	Δln (Per-Capita Finance Outcomes) 1957/62-1977/82		Black-White Diff. Δ ln(PT Ratio) 1968-1976	Δ Dissim. Index 1968-1976	Black-White Diff. Δ ln(Enr. Rate, Ages 16-17) 1960-1980
Dependent variable:	State Transfers	School Spending			
	(1)	(2)	(3)	(4)	(5)
			A. Δ ln (Presidential Election Turnout Rate), 1960-1980		
			Reduced-form		
1960 percent black	0.00468***	0.00467***	0.00450**	0.00450**	0.00466***
X literacy test indicator	(0.00153)	(0.00153)	(0.00194)	(0.00194)	(0.00169)
R-squared	0.708	0.708	0.759	0.759	0.756
			B. Δ Outcome		
			Reduced-form		
1960 percent black	0.00583***	0.00272*	-0.00140**	-0.000258	0.00301**
X literacy test indicator	(0.00208)	(0.00142)	(0.000640)	(0.00174)	(0.00153)
R-squared	0.429	0.158	0.285	0.538	0.104
			Two-stage Least Squares		
			(Instrument: 1960 percent black X literacy test indicator)		
Δ ln (pres. election turnout rate)	1.245**	0.582*	-0.311*	-0.0575	0.647*
	(0.560)	(0.335)	(0.178)	(0.383)	(0.385)
Root MSE	0.323	0.219	0.0764	0.156	0.189
Observations (counties)	1,129	1,128	509	509	638
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

Notes: All models include state indicators, 1960 percent black, and the 1960 child poverty rate, 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. Standard errors (in parentheses) are heteroskedasticity robust. ***, **, and * indicate statistical significance at the 1, 5, and 10 percent levels, respectively.

Table A1. Full Reduced-Form Regression Results forPer-Capita State Transfers:
Selected Specifications from Tables 3, 4, and 5

	Dependent variable: Δln (Per Capita State Transfers)								
Specification	Table 3, column 1	Table 3, column 2	Table 3, column 3	Table 4, column 2	Table 4, column 4	Table 4, column 5	Table 4, column 6	Table 5, column 2	Table 5, column 3
1960 percent black	0.00429**	0.00583***	0.00604***	0.00514***	0.00574***	0.00499***	0.00576***	0.00407**	0.00317**
X literacy test indicator	(0.00176)	(0.00208)	(0.00215)	(0.00198)	(0.00187)	(0.00178)	(0.00202)	(0.00189)	(0.00155)
1960 percent black	0.00103	0.000881	0.000804	-0.00169	-0.00278	0.000744	0.00204	0.000231	-0.00154
	(0.00161)	(0.00168)	(0.00177)	(0.00193)	(0.00183)	(0.00147)	(0.00199)	(0.00170)	(0.00130)
1960 child poverty rate		-0.00349	-0.00451	-0.00289	-0.00393*	-0.00457**	-0.00378	-0.00287	-0.00129
X literacy test indicator		(0.00232)	(0.00590)	(0.00207)	(0.00220)	(0.00197)	(0.00236)	(0.00233)	(0.00182)
1960 child poverty rate		0.000866	0.00136	0.00340**	0.00638***	0.00720***	0.000943	-0.00148	0.0176***
		(0.00173)	(0.00568)	(0.00168)	(0.00191)	(0.00167)	(0.00170)	(0.00199)	(0.00184)
1960 high school completion rate			-0.00148						
X literacy test indicator			(0.00759)						
1960 high school completion rate			0.000650						
			(0.00745)						
% enr. rec. ESAA funds, 1976				0.00165***					
				(0.000454)					
% enr. under court order, 1976				0.000676*					
				(0.000408)					
=1 if NAACP chapter, 1960					0.131***				
					(0.0290)				
=1 if other black org, 1960					0.121**				
					(0.0499)				
=1 if black college, 1960					0.121***				
					(0.0399)				
% vote for Strom Thurmond					0.000365				
					(0.00137)				
relative rep. index, 1960						-0.156***			
						(0.0263)			
1960 percent black							-0.00199		
X poll tax indicator							(0.00150)		
Δ ln(population)								-0.397***	
								(0.0563)	
Δ % of population aged 5-17									0.0366***
									(0.00752)
Δ % of population aged 65+									-0.0173
									(0.0110)
Δ % unemployed									0.00678**
									(0.00304)
Δ % of population in poverty									0.0194***
									(0.00192)
R-squared	0.425	0.429	0.430	0.473	0.476	0.506	0.431	0.506	0.567
Observations	1,129	1,129	1,129	1,125	1,127	1,129	1,129	1,129	1,129

Notes: All models include state indicators. The estimation sample includes all but two 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 poll tax indicator is set to one for states with poll taxes eliminated subsequent to ratification of the 24th amendment in 1964 (AL, AR, MS, TX, and VA). 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 A2. Full Reduced-Form Regression Results for Voter Turnout in Presidential Elections:
Selected Specifications from Tables 3, 4, and 5

	Dependent variable: Δln (Presidential Election Turnout)								
Specification	Table 3, column 1	Table 3, column 2	Table 3, column 3	Table 4, column 2	Table 4, column 4	Table 4, column 5	Table 4, column 6	Table 5, column 2	Table 5, column 3
1960 percent black	0.00458***	0.00468***	0.00423***	0.00481***	0.00548***	0.00471***	0.00464***	0.00470***	0.00379***
X literacy test indicator	(0.00137)	(0.00153)	(0.00162)	(0.00150)	(0.00152)	(0.00155)	(0.00149)	(0.00146)	(0.00141)
1960 percent black	0.00459***	0.00454***	0.00393***	0.00431***	0.00279**	0.00454***	0.00522***	0.00454***	0.00437***
	(0.00118)	(0.00131)	(0.00138)	(0.00129)	(0.00139)	(0.00130)	(0.00138)	(0.00134)	(0.00114)
1960 child poverty rate		-0.000379	-0.000351	-0.000811	-9.41e-05	-0.000337	-0.000548	-0.000386	0.000820
X literacy test indicator		(0.00212)	(0.00241)	(0.00214)	(0.00212)	(0.00211)	(0.00217)	(0.00210)	(0.00214)
1960 child poverty rate		0.000281	0.00420**	0.000625	0.000368	3.04e-05	0.000327	0.000305	0.00321
		(0.00194)	(0.00207)	(0.00182)	(0.00163)	(0.00193)	(0.00194)	(0.00187)	(0.00199)
1960 high school completion rate			0.00111						
X literacy test indicator			(0.00372)						
1960 high school completion rate			0.00516						
			(0.00342)						
% enr. rec. ESAA funds, 1976				-0.000329					
				(0.000233)					
% enr. under court order, 1976				0.000331					
				(0.000220)					
=1 if NAACP chapter, 1960					-0.00418				
					(0.0179)				
=1 if other black org, 1960					0.0563				
					(0.0515)				
=1 if black college, 1960					0.0121				
					(0.0366)				
% vote for Strom Thurmond					0.00298***				
					(0.000939)				
relative rep. index, 1960						0.00617			
						(0.0122)			
1960 percent black							-0.00118		
X poll tax indicator							(0.00124)		
Δ ln(population)								0.00400	
								(0.0437)	
Δ % of population aged 5-17									0.0215***
									(0.00707)
Δ % of population aged 65+									0.00284
									(0.00463)
Δ % unemployed									-0.000409
									(0.00330)
Δ % of population in poverty									0.00278*
									(0.00163)
R-squared	0.708	0.708	0.724	0.709	0.717	0.709	0.709	0.708	0.720
Observations	1,129	1,129	1,129	1,125	1,127	1,129	1,129	1,129	1,129

Notes: All models include state indicators. The estimation sample includes all but two 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 poll tax indicator is set to one for states with poll taxes eliminated subsequent to ratification of the 24th amendment in 1964 (AL, AR, MS, TX, and VA). 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.