Dartmouth College

Dartmouth Digital Commons

Dartmouth Scholarship

Faculty Work

8-2013

Local Responses to Federal Grants: Evidence from the Introduction of Title I in the South

Elizabeth U. Cascio
Dartmouth College

Nora Gordon
Georgetown University

Sarah Reber
University of California, Los Angeles

Follow this and additional works at: https://digitalcommons.dartmouth.edu/facoa

Part of the Finance Commons, and the Secondary Education Commons

Local Responses to Federal Grants:
Evidence from the Introduction of Title I in the South[†]

By Elizabeth U. Cascio, Nora Gordon, and Sarah Reber*

We analyze the effects of the introduction of Title I of the 1965 Elementary and Secondary Education Act, a large federal grants program designed to increase poor students' educational services and achievement. We focus on the South, the poorest region of the country. Title I increased school spending by $0.50 on the dollar in the average southern school district and by more in districts with less ability to offset grants through local tax reductions. Title I-induced increases in school budgets appear to have reduced high school dropout rates of whites, but not blacks. (*JEL* H52, H75, I21, I28, J15)

Intergovernmental grants are widely used tools that may preserve the efficiency benefits associated with local provision of public goods, while addressing equity concerns and cross-jurisdictional externalities (Musgrave 1959; Oates 1972, 1999). But local control may undermine the intent of the granting government. Receiving jurisdictions may crowd out intergovernmental grants by reducing their own fiscal effort, treating the grant as they would any other source of income. And even when grants increase spending, those dollars may be allocated in unintended or ineffective ways. Understanding local responses to intergovernmental grants is important. For example, state and local governments in the United States received over $500 billion—22 percent of their total revenue—from federal aid in 2009 (Barnett 2011).

Intergovernmental grants are particularly salient in US school finance. Over the past 50 years, both the federal government and the states have dramatically changed the level and distribution of education grants across school districts in an effort to narrow gaps in school spending and achievement. A large empirical literature has explored the effects of changes in state school finance regimes on school spending (e.g., Fisher and Papke 2000; Hoxby 2001). A separate literature has examined how the dramatic increase in school spending over recent decades has affected student

outcomes (see Hanushek 1997 for a review). Few studies have examined the effects of intergovernmental grants on school spending and student outcomes simultaneously, or using variation from their policy origins.[1]

We do so in this paper, analyzing how Title I of the Elementary and Secondary Education Act of 1965 (ESEA) affected school spending and high school dropout rates in the years immediately following its introduction.[2] Title I was—and continues to be—the largest federal program for elementary and secondary education, providing funds to support supplemental educational programs for poor children. The South was allocated large Title I grants at the program's introduction due to its high poverty rate, and serves as the focus of our analysis in part for this reason. The South is also of historical interest, given the vast racial inequities in school resources and educational attainment that developed in the region after Reconstruction (Margo 1990; Card and Krueger 1992a, 1996; Ashenfelter, Collins, and Yoon 2006), and its low school spending and poor educational outcomes by national standards, even for whites.

We begin by estimating the impact of the introduction of Title I on school spending. Existing studies of Title I (Feldstein 1978; Gordon 2004) analyze marginal changes in grant amounts under the existing program, whereas we estimate responses to the larger changes in funding generated by the program's introduction. Our research design combines the timing of the program's introduction with variation across school districts in its intensity, which was increasing in a district's 1960 child poverty rate. Simply put, we test whether the sharp increase in progressivity of federal school revenue in the mid-1960s arising from Title I was accompanied by an increase in the progressivity of school spending. Thus, we determine whether Title I grants exhibited "flypaper effects" by "sticking where they hit" (Hines and Thaler 1995), or were crowded out by state and local governments, as would have been predicted by a neoclassical model in which local governments were accountable to rational median voters.

The main threat to identification in our empirical strategy comes from school desegregation, in two respects. First, the Civil Rights Act of 1964 (CRA) made receipt of Title I funds contingent on meeting desegregation guidelines, and in previous work, we have shown that school districts responded to this financial incentive in the mid-1960s by desegregating just enough to receive their grants (Cascio et al. 2010). If this desegregation affected spending, our approach would still identify a reduced-form effect of Title I on spending, but desegregation would be a causal mechanism. The desegregation requirements for CRA compliance at this time were minimal,[3] however, and their small impact on the overall racial balance of southern

---

[1] Card and Payne (2002) examine how changes in the progressivity of school spending induced by state school finance reforms changed the distribution of SAT scores by income at the school district level. A related literature looks at effects of school finance reforms on local spending and outcomes within individual states: see Clark (2003) on Kentucky, Guryan (2001) on Massachusetts, and Papke (2005, 2008) and Roy (2011) on Michigan.

[2] Our analysis contributes to a growing literature on the effects of the introduction of other "War on Poverty" programs, including Medicare (Almond, Chay, and Greenstone 2006; Finkelstein 2007), Head Start (Ludwig and Miller 2007), and the Food Stamp program (Hoynes and Schanzenbach 2009; Almond, Hoynes, and Schanzenbach 2011).

[3] In 1966, for example, the financial incentive induced districts to move 2 to 6 percent of blacks into schools with whites (Cascio et al. 2010). See Cascio et al. (2008) for more on the timing of southern desegregation.

schools (Cascio et al. 2010) was unlikely to have increased the demand for school spending, as did the more intensive desegregation efforts in the following years (Reber 2011; Johnson 2011). To mitigate concerns that this desegregation affected spending nevertheless, we focus our main analysis on changes in school spending from 1964 to 1969. President Nixon stopped enforcing the fund-withholding provisions of the CRA when he entered office in 1969, and at the end of the 1960s, schools in poorer districts were no more racially balanced than schools in richer districts—a finding of previous research (Cascio et al. 2008, 2010) that we reproduce below.

Second, as Nixon backed off CRA enforcement, the federal courts stepped up their efforts, and districts across the South desegregated substantially between 1968 and 1970 (Cascio et al. 2008). If desegregation increased demand for school spending more in poorer districts, we might mistakenly attribute desegregation-related increases in spending to the introduction of Title I. Indeed, poorer districts tended to have higher black enrollment shares. Districts with higher black enrollment shares, in turn, historically had larger gaps in spending between separate black and white schools, and thereby required larger spending increases to maintain spending on whites when black and white schools combined (Reber 2011). We therefore control linearly for the 1960 black enrollment share in all of our models and confirm that the results are not sensitive to alternative functional forms.

Using newly collected data, we estimate that the introduction of Title I increased school spending by $0.50 on the dollar in the average southern school district, more than double what would have been predicted in a neoclassical model of local government behavior. Title I crowded out local revenue, not state aid. The findings are robust to the inclusion of a range of controls beyond black enrollment share. Further, changes in the relationship between child poverty and fiscal outcomes were closely timed with program implementation, and there was little change in these relationships in the years leading up to it, suggesting that we have identified the causal impact of Title I's introduction. Title I appears to have had larger than predicted spending impacts on average because it increased school spending significantly more in districts where the Title I grant was large relative to what local revenue would have been had pre-ESEA trends continued—districts with what we term "low scope" to offset the grants through reductions in local taxes. Changes in desegregation did not vary along this margin, further diminishing the concern that desegregation is driving the results.

The heterogeneous spending response informs our analysis of Title I-induced spending increases on student outcomes. We ask whether the gap in high school dropout rates between poorer and richer school districts closed by more in districts where Title I funds actually increased spending (those with low scope to offset Title I funds) than in other southern districts. We find that, for whites, each additional $100 of Title I-induced current expenditure per pupil (real 2009 US dollars) received in 1969 (corresponding to about that much per year from 1965 to 1969) was associated with a decrease in high school dropout rates at ages 18 and 19 of about 3.5 percentage points. For blacks, by contrast, the estimates are statistically insignificant and precise enough to rule out effects as large as those found for whites. These estimates are necessarily more speculative than those of the effects of Title I on school spending, because the lack of high frequency data limits the specification checks at our

disposal. Nevertheless, our findings are robust to additional controls and falsification exercises conducted on unaffected cohorts.

Our findings for educational attainment may provide indirect evidence that Title I-induced spending increases were not allocated as the federal government intended. There was little to prevent school districts from directing them to schools attended by whites, which remained largely separate in the South over the period of study. All states in our sample stopped reporting district-level data on spending by race by 1965, and they never reported school-level budgetary data, so we cannot examine this hypothesis directly. However, Martin and McClure (1969) provide extensive anecdotal evidence of such reallocations of Title I funds, which are consistent with longer standing practices in the South of diverting school revenue from black to white students (Margo 1990).

Though Title I had larger than expected spending effects in the South, the relatively weak regulations in place at the time gave school boards considerable latitude over how to allocate the additional education spending across students. Policymakers today continue to grapple with the question of how to optimally regulate Title I—and a host of intergovernmental grant programs across policy functions—with the goal of preventing crowd-out and misuse of funds while retaining local governments' ability to optimally allocate resources. Our analysis contributes to a growing literature suggesting that the answer depends on local institutional context.

I. Title I of the Elementary and Secondary Education Act

In January 1965, President Johnson declared a "national goal of full educational opportunity," expanding the "War on Poverty" to include education. Three months later, following decades of unsuccessful efforts to expand the federal role in education, the ESEA was signed into law.[4] Title I was by far the largest component of the ESEA, authorizing $7 billion (real 2009 US dollars) in new federal funding for programs for poor children and doubling the federal commitment to K–12 education. Title I was reauthorized as the No Child Left Behind Act in 2001 and remains the largest federal program for K–12 education today.

The program marked a historic shift in federal education policy, not only for its magnitude, but also for the control it exerted over schools. The Johnson administration used Title I funds as a "carrot" to encourage school desegregation in the South, by enforcing the requirement of nondiscrimination in federally funded programs under Title VI of the 1964 CRA. Cascio et al. (2010) show that the financial incentives were sufficiently powerful to prompt southern districts to meet the quite limited definition of nondiscrimination enforced in 1965 and 1966—the transfer of a small share of blacks to white schools. Our focus here is on how the additional income provided by the grants affected school spending and high school dropout in the South as of the late 1960s, when Title I funds were no longer being withheld on the basis of insufficient desegregation.

---

[4]While many education historians have focused on the "three Rs" (race, religion, and "Reds"), Kaestle (2001) also attributes previous policy failures to a lack of strong presidential leadership.

The initial formula amounts, allocated to counties for the 1965–1966 school year, were linear in county resident "eligibles"—5–17-year-olds in low-income families as of the 1960 census, plus a small number of children in higher income families that received AFDC in 1962.[5] The slope of the funding formula (the "state factor") initially reflected state efforts, with each eligible child allocated one-half of average education spending per pupil in the state two years prior (net of federal transfers). In 1967, the state factor applied in states spending below the national average (including all of the states in our sample except Florida) was leveled up to the national average (US Department of Health, Education, and Welfare (HEW) 1969). Child poverty was thus essentially the sole determinant of Title I formula amounts for southern counties after 1967.[6]

Throughout the period, states used available subcounty data on child poverty to divide formula grants across school districts within counties. We use the amount each district was entitled to according to these divisions in 1965 (not the amount they actually received) to estimate the number of Title I eligible children in each district, based on data from US Senate Committee on Labor and Public Welfare Subcommittee on Education (1965, 1967). Our key independent variable—the child poverty rate used to determine a district's Title I entitlement—is this estimate of the number of district eligibles (based mostly on 1960 child poverty counts) divided by district enrollment in 1960. (See online Appendix A for details.)

The influx of Title I funds to the South was sizeable. Figure 1 shows trends in average per-pupil current educational expenditure and revenues by source for southern school districts between 1961 and 1969. The underlying sample, which forms the basis of the analysis to follow, includes most school districts in the nine of the eleven states of the former Confederacy (the "South") where annual school finance and enrollment data were available in print publications.[7] As shown in Table 1, federal revenue was negligible in the South prior to passage of the ESEA, accounting for about 3.4 percent of per-pupil current expenditure in 1964, but represented 17.2 percent of per-pupil current expenditure by the end of the decade. Title I accounted for only 8 percent of spending nationally at this point (Snyder and Dillow 2011).

[5] Other categories of eligibility were added over time, but a county's eligibility by the end of the 1960s remained determined mostly on the basis of the 1960 census child poverty counts. Cohen and Moffitt (2009) emphasize the role of the Title I formula itself in garnering political support by spreading funds across Congressional districts rather than concentrating them more intensively in high poverty areas.

[6] Cascio et al. (2010) exploit variation in Title I formula amounts in 1966 across states (from variation in the state factor) as well as across districts (from variation in child poverty). We cannot use this approach due to the change in the formula.

[7] These states are Alabama (Alabama Department of Education 1961–1970), Florida (Florida State Commissioner of Education 1969, 1970, 1971; Florida State Superintendent of Public Instruction 1963, 1965, 1967), Georgia (Georgia Department of Education; Georgia State Department of Education 1967, 1969), Louisiana (State Department of Education of Louisiana 1961–1970), Mississippi (Mississippi State Department of Education 1961, 1963, 1966, 1968; Mississippi State Superintendent of Public Education 1971), North Carolina (North Carolina Education Association 1963–1968; North Carolina Department of Public Instruction 1965–1970), South Carolina (South Carolina State Department of Education 1961–1967; South Carolina Department of Education 1968–1970), Tennessee (Tennessee Department of Education 1961–1970), and Virginia (Virginia State Board of Education 1961–1970). Arkansas did not include ESEA funds in its expenditure or revenue data; Texas did not publish the relevant school finance data for this period. We cannot use the Census of Governments for this analysis since it is available only every five years and, critically, includes Title I funds in state, rather than federal, revenue. See online Appendix A for further description of the data and estimation sample.
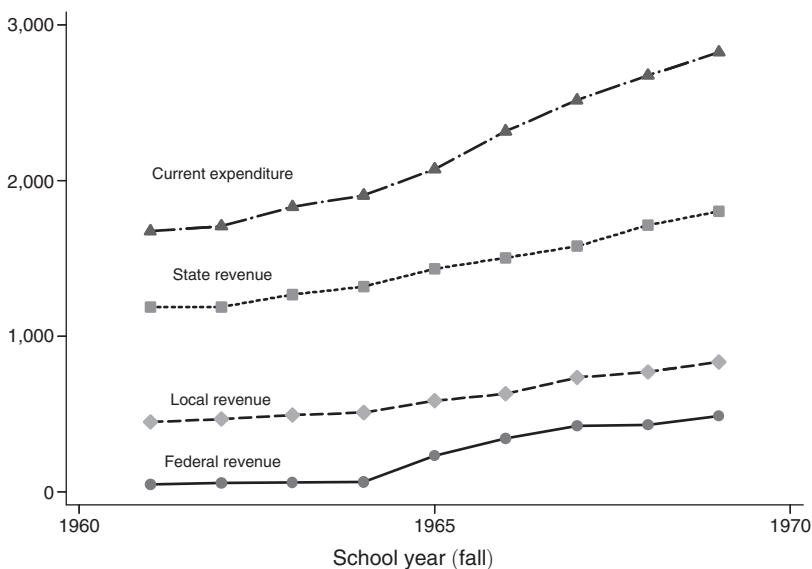
FIGURE 1. TRENDS IN PER-PUPIL EXPENDITURE AND REVENUE BY SOURCE: SOUTHERN STATES, THE 1960S

Notes: All figures are in real 2009 US dollars. Sample consists of 910 school districts from nine southern states: Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia. See online Appendix A for description of estimation sample. Means are weighted.

TABLE 1—DESCRIPTIVE STATISTICS ON DISTRICT REVENUE AND EXPENDITURE

	Level			Change	
	1961 (1)	1964 (2)	1969 (3)	1961 to 1964 (4)	1964 to 1969 (5)
Per-pupil federal revenue	47.90 (62.62)	65.12 (73.49)	486.72 (262.51)	17.22 (35.06)	421.60 (270.20)
Per-pupil state revenue	1,187.03 (351.72)	1,318.43 (351.79)	1,803.55 (428.93)	131.40 (94.99)	485.11 (231.73)
Per-pupil local revenue	448.88 (341.59)	509.44 (392.08)	835.63 (586.08)	60.56 (114.57)	326.19 (286.14)
Per-pupil current expenditure	1,675.67 (451.70)	1,905.66 (464.14)	2,825.19 (511.85)	229.99 (215.31)	919.53 (420.69)

Notes: All figures are in real 2009 US dollars. Standard deviations are in parentheses. Sample consists of 910 school districts in nine southern states: Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia. See online Appendix A for description of estimation sample. Means are weighted.

Title I funds were intended to provide supplemental programs to "educationally deprived" poor children, with the goal of raising achievement. However, Title I's early regulation and enforcement did not ensure that the program would translate into more school spending, let alone increased compensatory services. Regulations specified only that Title I funds should "supplement not supplant" local revenues. While HEW conducted district-level audits, it did not require districts to return funds that were misused or penalize districts for violations. The program was criticized as a fungible supplement to general revenue as opposed to a targeted and defined

compensatory intervention, most influentially in a 1969 report by the NAACP Legal Defense Fund (Martin and McClure 1969). Such criticisms sparked a series of changes to Title I in the 1969 amendments to the ESEA that are still embedded in the system today (Brown 2011; Cohen and Moffitt 2009).[8] However, even today's complex regulations and serious enforcement cannot prevent districts from appropriating Title I funds, either for private consumption or for other educational purposes.[9]

We therefore consider Title I to be a restricted block grant to a school district,[10] where spending on public education is the "targeted good." We begin our analysis with an examination of the effects of the program's introduction on education spending at the district level. We then turn to an analysis of its impacts on student outcomes. In light of the above discussion, we consider the impacts of Title I-induced spending increases for all students, not just Title I eligible students. However, we present estimates separately by race to provide indirect evidence on within-district allocation of funds. While blacks should have benefited more from Title I-induced spending increases given their relative poverty, blacks and whites in the South largely attended separate schools through 1968 (Cascio et al. 2008), providing sufficient scope for funds to be diverted from blacks to whites as had been the practice in the South for most of the twentieth century (Margo 1990).

II. Theoretical Background

How much Title I translated into higher school spending depends on how much lower levels of government—both local school districts and states—offset Title I grants by reducing their own contributions for education. A significant empirical literature finds that grants increase spending on the targeted good more than would be expected from a neoclassical model, suggesting that crowd-out might not be a major impediment to redistribution through intergovernmental grants. More recent literature has demonstrated that some of these "flypaper effects" are simply statistical artifacts generated by omitted determinants of grant size, such as local preferences (Knight 2002; Gordon 2004). Other recent papers suggest local institutions play a key role in determining the incidence of intergovernmental grants (Baicker and Staiger 2005; Brooks and Phillips 2010; Lutz 2010; Strumpf 1998).

We also find a role for local institutions in mediating fiscal responses to Title I grants. An important feature of 1960s southern school finance was the small share of total revenue raised locally compared to the rest of the country. This meant that Title I grants were often large relative to local revenue, limiting the ability of some

---

[8] The "supplement not supplant" principle moved from regulations into the law itself, with annual reporting required beginning in July of 1971. Two new provisions aimed to prevent Title I funds from being used as substitutes for state and local funds: "comparability," which requires equitable allocation of state and local resources across schools within districts, and "maintenance of effort," which withholds Title I funds if state and local revenues per pupil fall beneath a (time-varying and often 100 percent) share of previous levels.

[9] Gordon (2004) shows that maintenance of effort requirements did not prevent Title I from crowding out local revenue in the 1990s, and van der Klaauw (2008), Roza (2010), and Heuer and Stullich (2011) show that the comparability provision does not ensure that Title I funds are concentrated in the poorest of a district's schools.

[10] While school districts in the South and elsewhere in the US are often coincident with other local jurisdictions such as counties or cities, they are usually governed by their own elected or appointed school boards. In three states in our sample (North Carolina, Tennessee, and Virginia), school districts are "dependent" on city and county governments for revenue.
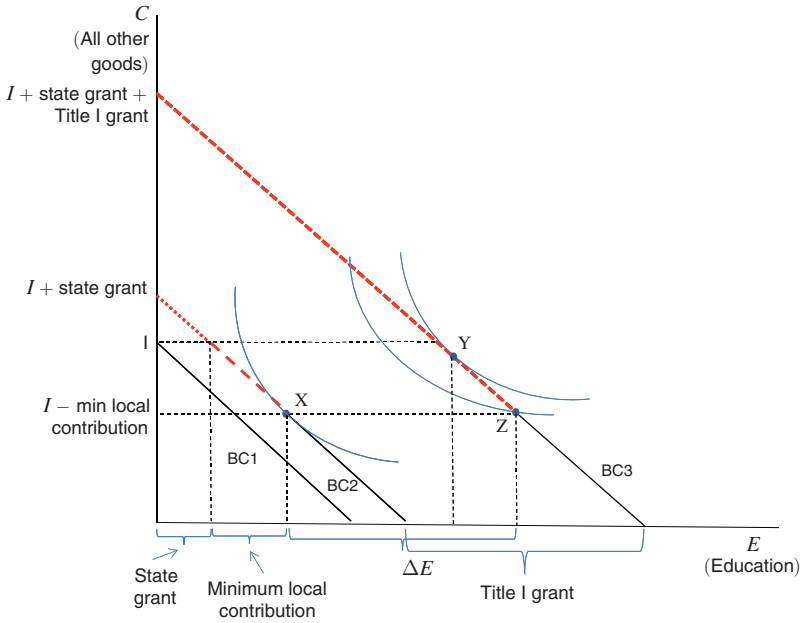
FIGURE 2. LOCAL GOVERNMENT RESPONSES TO TITLE I GRANT

districts to reduce local taxes in response to receiving Title I funds. Local revenue was only 27 percent of the typical southern district's total revenue in the early 1960s, and per-pupil federal revenue in 1969 was almost as large in real terms (Table 1).

Incorporating these features of southern school finance, Figure 2 illustrates the standard neoclassical choice problem for a school district before and after Title I.[11] The innermost budget constraint (BC1) shows the pure local finance case where prices are normalized to one and the district can allocate total income I freely between educational services (E) and all other goods (C), including private consumption. BC2 accounts for state aid and the minimum local contribution (a legally mandated amount districts must raise locally) with a parallel shift of the budget constraint, and represents the scenario before Title I.[12] A district had to spend at least as much as the state grant plus the minimum local contribution on education, so maximum spending on C was I less the minimum local contribution; the dashed part of the budget constraint was inaccessible. The introduction of Title I caused a further parallel shift in the budget constraint to BC3.[13] After Title I, the district had

---

[11] The median voter in the South would have been white, and southern school boards would have been controlled by whites throughout this period in most or all of the districts in our sample (US Commission on Civil Rights 1968). For simplicity, we do not incorporate race into the model explicitly.

[12] Upwards of 80 percent of state aid was distributed through so-called "minimum foundation programs" (MFPs). Grants under state MFPs were not pure block grants in the sense that some district choices (e.g., the distribution of teacher experience) could affect the amount of the grant. We abstract from these issues here, since they likely had small effects and in any case are unrelated to the Title I grant.

[13] Because we focus our empirical analysis on district behavior in 1969, at which point the funds were no longer conditional on desegregation, we abstract from the conditional nature of Title I funds. That is, we do not present segregation as a good consumed by voters, as in Cascio et al. (2010). We also abstract from any possible general equilibrium effects of the program's introduction, in particular on teacher compensation. To the extent that the

to spend at least as much as previously required *plus* the Title I grant on education; the dashed part of the new budget constraint was inaccessible.

How much should have the introduction of Title I increased school spending? Assuming no change in state aid, a given school district would have liked to increase its spending on both E and C according to the relevant income elasticities, in which case its increase in E would have been less than the size of the grant. If its optimal bundle was on the inaccessible (dashed) portion of the budget constraint, however, it would have been forced to the corner (point Z), increasing E by more. The figure illustrates an extreme case where spending increased dollar-for-dollar with Title I: the district at the corner before Title I (point X) would have liked to move to point Y on BC3, but was restricted to choose the new corner, Z. More generally, minimum local contributions and significant state aid in the South would have increased the chances that a district was constrained and increased E more than expected given the income elasticity of demand for education.

In a more realistic model incorporating a probability of detection, fixed costs of changing tax rates, or uncertainty about revenues and expenses, a district would not have needed to be at a corner to have been constrained to spend more on education than desired. While we cannot observe the constraints specific to each school district, we expect that crowd-out would have been more difficult where the grant was large relative to local revenue. Below, we test this idea by estimating heterogeneity in fiscal responsiveness to Title I depending on the magnitude of a district's grant relative to an estimate of what its local revenue would have been in the absence of the program.

State governments might also have offset the new federal funding. The objective function of a state government is more complicated than that of a school district due to its additional policy functions beyond education, and we do not explicitly model its decision here. We can say, however, how a state government would have responded had it wished to replicate the pre-Title I distribution of total (state plus federal) aid across districts after Title I: it would have reduced aid on average, to offset the increase in federal grants for the average district, and reduced aid more for poorer districts, which received larger Title I grants.[14]

Our empirical strategy allows for identification only of the latter type of response—changes to the progressivity of state aid. More generally, our approach focuses on whether the sharp increase in the progressivity of federal education aid in 1965 coincided with a reduction in the school spending gap between poorer and richer districts, with corresponding changes in the poverty gaps in local and state revenue revealing the level of government responsible for any crowd-out.

---

introduction of Title I increased teacher salaries (by increasing the demand for teachers), any increase in overall school spending at the district level would have procured a lesser increase in educational services.

[14] The poverty rate did not directly enter the funding formula in any state under investigation; thus, it might appear difficult for states to change the progressivity of funding without undertaking a major reform. However, the formulas were complex, with inputs that may have been correlated with poverty rates. Reber (2011) shows that the state of Louisiana was able to redistribute state aid substantially in response to desegregation (directing additional funding to districts with higher black enrollment shares) by subtly manipulating the parameters of the existing school finance program.

III. Fiscal Reponses to the Introduction of Title I

A. *Graphical Evidence*

Recall that Title I funds were initially allocated by a linear rule: the formula assigned a constant amount for each Title I eligible student. Title I eligibility was determined primarily based on the number of 5–17-year-olds with family income below $2,000 in the 1960 census. A straightforward way to think about the school spending impacts of Title I's introduction is therefore to explore how the spending gradient in the "initial" district child poverty rate, defined using these eligibility counts, changed over the 1960s, controlling for black enrollment share to account for any contemporaneous impacts of desegregation on school spending (Reber 2011; Johnson 2011), as earlier described. If Title I increased school spending, spending should have become more progressive around 1965. The identifying assumption is that if the Title I program had not been introduced, there would not have been a break in the poverty gradient as of 1965.

We thus begin by estimating parsimonious regressions of per-pupil spending and revenue on the child poverty rate, separately by year (t), controlling for district black enrollment share and state (s) fixed effects:

$$(1) \qquad y_{dst} = \gamma_{st} + \theta_t child\_poverty_d + \beta_t black_d + \varepsilon_{dst}.$$

Initial district child poverty ($child\_poverty_d$) is defined as the ratio of district d's Title I eligibility count in 1965, constructed as described in Section I (and online Appendix A), to its 1960 enrollment. Table 2 shows the average southern district had a child poverty rate of 32 percent and a 1960 black enrollment share ($black_d$) also of 32 percent.[15] The state-year fixed effects, γ_{st}, account for state-specific shocks. The regressions give each district equal weight, so as to capture the average southern school district's fiscal response to Title I; the fiscal responses implied by enrollment weighted estimates are similar.

The circles in Figure 3 show how estimates of θ_t, the child poverty gradient, evolved over time for per-pupil fiscal outcomes, measured in the fall of the year specified and in real 2009 US dollars. Panel A shows that the estimated poverty gradient in per-pupil federal revenue was little changed in the early 1960s but increased sharply in 1965, the first year in which Title I funds were distributed.[16] The estimates for per-pupil current expenditure, shown in panel B, follow the same pattern: the progressivity of school spending trended little in the early 1960s but increased after 1964, suggesting that Title I narrowed the gap in school spending between richer and poorer districts in the South. However, Title I funds did not translate

---

[15] For all states except North Carolina, we constructed this variable from data reported in the school finance publications cited in footnote 7. For North Carolina, we estimated this variable using data from Southern Education Reporting Service (1964, 1966).

[16] Though the two substantive federal grants programs in the early 1960s, Aid to Federally Impacted Areas and the National Defense Education Act, did not explicitly distribute funds based on poverty, the relationship between poverty and federal aid is slightly negative prior to the ESEA. For our identification strategy, the critical finding is that the trend in this relationship is flat prior to the ESEA.

TABLE 2—DESCRIPTIVE STATISTICS ON DISTRICT CHARACTERISTICS

	Mean (1)	SD (2)	Observations (3)
Child poverty rate, 1960	0.32	0.17	910
State factor in Title I grant, 1969	954	6.09	910
Black enrollment share, 1960	0.32	0.22	910
District enrollment, 1960	6,938	11,869	910
One of poorest 300 counties, 1960 (=1)	0.24	0.43	910
Percent voting for Thurmond, 1948	34	30	910
Black voter registration rate, early 1960s (percent)	28	24	812
Black/white dissimilarity index, 1970	0.28	0.22	853

Notes: The state factor in the Title I grant is expressed in real 2009 US dollars. Sample consists of school districts in nine southern states: Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia. See online Appendix A for description of estimation sample.
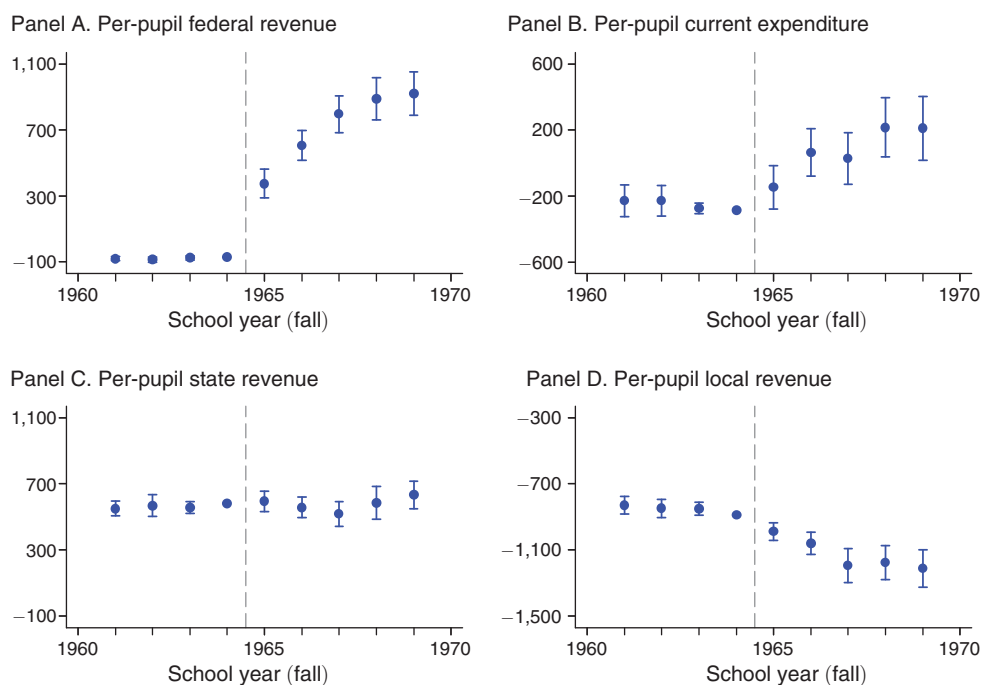
FIGURE 3. YEAR-BY-YEAR GRADIENTS OF DISTRICT FINANCE VARIABLES IN THE 1960 CHILD POVERTY RATE

Notes: All figures are in real 2009 US dollars. The solid dots represent the coefficients on the 1960 district child poverty rate from (unweighted) year-specific regressions that also include state dummies and the 1960 district black enrollment share (equation (1)). The capped lines represent 95 percent confidence intervals on the estimated difference between the poverty gradient in a given year and its value in 1964. To obtain these confidence intervals, we pooled data from 1961 to 1969 and regressed fiscal outcomes on district dummies, state-by-year dummies, and interactions of the 1960 child poverty rate and 1960 black enrollment share with year dummies (for all years except 1964), clustering standard errors on county. The confidence intervals shown are those for the coefficients on the final set of interaction terms, rescaled by the estimate of the 1964 poverty gradient from equation (1). See footnote 17 for more details.

dollar-for-dollar into higher spending: the increase in the poverty gradient for per-pupil current expenditure was less than that for per-pupil federal funding. The poverty gradient in local revenue became more negative after 1965, suggesting that

local effort may have declined in response to Title I, but the moderate progressivity of state aid did not change (panels C and D).

The capped vertical lines around the circles in Figure 3 represent 95 percent confidence intervals on the estimated difference in the poverty gradient between the year specified and 1964.[17] For per-pupil federal revenue, current expenditure, and local revenue, the gradient in child poverty is statistically distinguishable from its 1964 value for each year between 1965 and 1969, but there were neither substantive nor statistically significant changes in the child poverty gradient for state revenue in post-ESEA years. For all variables except local revenue, the differences in the poverty gradients in pre-ESEA years are not just insignificant economically, but also statistically: we cannot reject the null hypothesis that the progressivity of school spending, federal revenue, or state revenue was unchanged across the early 1960s. For per-pupil local revenue, we do reject equality with the 1964 poverty gradient in 1961 and 1963 (in the latter case marginally so), but the differences in the gradients are small relative to the differences seen in the post-ESEA period. This suggests that unobserved determinants of school finance were not meaningfully correlated with child poverty—and the intensity of the Title I program—in the years leading up to Title I implementation, supporting the identification strategy.

Figure 3 thus shows that relative to richer districts, poorer districts experienced larger increases in federal revenue and current expenditure per pupil and larger declines in local revenue per pupil over the 1960s. These changes were closely timed with the introduction of Title I in 1965. These changes were also rapid, suggesting that it took only two to three years for districts to reach a new equilibrium.[18] The remainder of this section establishes the magnitude of the fiscal responses and demonstrates their robustness to additional controls.

B. *Change Regressions*

Differencing (1) evaluated at any two years, we arrive at

$$(2) \qquad \Delta y_{ds} = \tilde{\gamma}_s + \tilde{\theta}\, child\_poverty_d + \tilde{\beta} black_d + \Delta \varepsilon_{ds}.$$

In this differenced model, the state fixed effects, $\tilde{\gamma}_s$, account for trends in fiscal outcomes common to districts in the same state, while the coefficients on $child\_poverty_d$ and $black_d$ give changes over time in the gradients of fiscal outcomes in a district's

---

[17] To obtain these confidence intervals, we pooled the data from 1961 to 1969 and estimated the "stacked" version of equation (1) (again, giving each district equal weight). Formally, we estimated

$$y_{dst} = \delta_d + \gamma_{st} + \sum_{j \neq 1964} \theta_j (child\_poverty_d \times D_t^j) + \sum_{j \neq 1964} \beta_j (black_d \times D_t^j) + \varepsilon_{dst},$$

where D_t^j is an indicator variable set to one if $t = j$, zero otherwise. δ_d is a district-specific intercept. We omit the interactions between each of $child\_poverty_d$ and $black_d$ with the indicator for 1964. The coefficients of interest—the θ_j's—thus measure the difference in the poverty gradient between 1964 and year j. The standard errors are clustered on county because we use county-level information to predict district-level poverty rates (see online Appendix A). In Figure 3, we rescale the confidence intervals on the θ_js by adding the estimate of the 1964 poverty gradient from equation (1).

[18] Gordon (2004) finds that it took about three years for districts to crowd out changes in Title I grants in the 1990s.

1960 child poverty rate and 1960 black enrollment share, respectively. We estimate model (2) for two changes around the introduction of Title I—one entirely in the pre-ESEA period (1961 to 1964) and one that spans the program's introduction (1964 to 1969). Recall that by 1969, the funds were no longer conditional on desegregation activity, reducing the chances that our estimates will reflect a fiscal response to desegregation. Results are nevertheless substantively similar using end-year data as early as 1967. We do not consider any later years because the federal government began providing aid to desegregating districts in 1970 in a way that appears to have been correlated with child poverty. Summary statistics for the dependent variables are presented in the last two columns of Table 1.

The first two columns of Table 3 present estimates of model (2) for the pre-ESEA and pre-to-post changes, respectively, using the same controls included in the regressions plotted in Figure 3 (state indicators and 1960 black enrollment share). A comparison of these columns confirms the intuition from the figure: changes in the poverty gradients of federal revenue, current expenditure, and local revenue from 1964 to 1969 were more significant—economically and statistically—than pre-ESEA changes. Focusing on the pre-to-post change in column 2, the coefficient on the 1960 child poverty rate in the model for per-pupil federal funding (panel A)—$990 (with a standard error of $63)—is, as expected, quite close to the average state factor in the Title I grant formula as of 1969—$954, shown in Table 2.[19] Per-pupil current expenditure increased by $498 more between 1964 and 1969 in a district with only poor children compared to a district with no poor children (panel B). Rescaling by the corresponding change in per-pupil federal revenue, these estimates imply that each additional dollar increase in per-pupil federal revenue generated a $0.50 increase in per-pupil current expenditure.[20] This estimate implies that the introduction of Title I can account for an increase in current expenditure of about $212 per pupil for the average district, or 23 percent of the average increase between 1964 and 1969.

To formalize this rescaling of the estimates into more intuitive dollar-for-dollar terms, we estimate two-stage least squares (TSLS) regressions, using $child\_poverty_d$ as an instrument for the change in federal revenue.[21] The results, reported in column 3, imply that our estimates leave $0.22 of federal revenue unaccounted for: each dollar of additional federal revenue increased current expenditure by $0.50 (panel B), but there was only $0.28 of total crowd-out—$0.33 local (panel D) less $0.05 state (panel C). This difference is statistically significant (p-value = 0.002). It could represent an increase in spending on capital and debt service, which are included in

[19] Data on the 1969 state factor are from US Senate Committee on Labor and Public Welfare Subcommittee on Education (1970). If all districts received their formula amounts under the Title I program (and Title I eligibility counts did not change between 1965 and 1969), the change in the poverty gradient from 1964 to any year from 1965 to 1969 would be equal to the average state factor in that year. The coefficients on $child\_poverty_d$ in 1965 and 1966 are less than this since some southern districts did not receive Title I funds due to noncompliance with Title VI of the CRA.

[20] Feldstein (1978) uses cross-state variation in Title I grants across districts with the same poverty rate in 1970, and finds spending increased by $0.70 per grant dollar. Gordon (2004) exploits formula-based changes in funding in the early 1990s and finds essentially full local crowd-out and no impact on current spending, but her estimates have large standard errors.

[21] We thus assume that there would have been no trend in the poverty gradient between 1964 and 1969 in the absence of the program. If we instead take the trend from 1961 to 1964 as the counterfactual, similar to Finkelstein (2007) in a study of Medicare's introduction, our substantive conclusions are unchanged. Our estimates are also robust to including pre-ESEA (1961 to 1964) changes as controls.

TABLE 3—REDUCED-FORM AND TSLS ESTIMATES OF THE FISCAL RESPONSE
TO THE INTRODUCTION OF TITLE I

	Reduced form		TSLS
	1961 to 1964 (1)	1964 to 1969 (2)	1964 to 1969 (3)
Panel A. Δ Per-pupil federal revenue			
Child poverty rate, 1960	10.12	990.4***	
	(6.702)	(63.15)	
Black enrollment share, 1960	−9.161**	67.20	
	(3.950)	(41.46)	
Panel B. Δ Per-pupil current expenditure			
Child poverty rate, 1960	−59.30	497.5***	
	(45.84)	(92.84)	
Δ Per-pupil federal revenue, 1964 to 1969			0.502***
			(0.0795)
Black enrollment share, 1960	−20.24	369.2***	335.5***
	(49.37)	(71.49)	(67.80)
Panel C. Δ Per-pupil state revenue			
Child poverty rate, 1960	30.64	51.37	
	(21.29)	(40.29)	
Δ Per-pupil federal revenue, 1964 to 1969			0.0519
			(0.0405)
Black enrollment share, 1960	−1.486	83.34**	79.85**
	(19.10)	(33.02)	(34.56)
Panel D. Δ Per-pupil local revenue			
Child poverty rate, 1960	−60.21**	−323.4***	
	(25.89)	(54.37)	
Δ Per-pupil federal revenue, 1964 to 1969			−0.327***
			(0.0586)
Black enrollment share, 1960	−33.32	122.5***	144.4***
	(20.98)	(40.91)	(46.40)
First-stage partial *F*-stat on instrument			245.9
Number of districts	910	910	910

Notes: All dollar figures are in real 2009 US dollars. All specifications include state dummies and 1960 black enrollment share as controls. The TSLS regressions in column 3 use the 1960 child poverty rate as an instrument for the 1964 to 1969 change in per-pupil federal revenue. Standard errors (in parentheses) are clustered on county.

***Significant at the 1 percent level.
**Significant at the 5 percent level.
*Significant at the 10 percent level.

total, but not current, expenditure. When we estimate the model using available data on total expenditure, the results are similar to those for current expenditure but less precise.[22] Technically, this implies that districts used some of the new federal revenue

---

[22] Reporting of total expenditure is not as consistent across states as that of current expenditure and revenue, and because total expenditure includes capital expenditure, it exhibits much more year-to-year variation. In addition, while in theory total revenue should be similar to total expenditure, at least on average and over long time periods, this is not always the case in practice. Capital outlays can generate substantial departures of total expenditure from total revenue in a given year. Finally, at least some states do not count proceeds from bond issues as "revenue," while the capital improvements financed by bond issues are sometimes included in total expenditure; thus, revenue can be persistently lower than expenditure.

to reduce existing debt, acquire less new debt, or build up reserves. This would allow districts either to lower taxes or to increase spending in the future.

While our estimates do not account for every cent of Title I revenue, they are estimated precisely enough to draw economically meaningful conclusions about the program's fiscal impacts. We can rule out substantial reallocation of state funds, and the confidence interval for the local revenue estimate suggests that the average school district engaged in economically meaningful offset. The estimate for current expenditure (the targeted good) is significantly different from both one and zero, and also significantly larger than the response that existing estimates of the income elasticity of education demand would imply—$0.12 to $0.19 per dollar in the average district, according to our back-of-the-envelope estimate.[23]

C. Robustness

As we have discussed, the primary threat to identification in our empirical strategy comes from desegregation-related changes in school finance. Although Title I receipt and desegregation were no longer explicitly linked by the late 1960s, schools across the South desegregated substantially in the late 1960s (Cascio et al. 2008). If desegregation increased demand for school spending more in poorer districts than in richer districts, those effects could be difficult to distinguish from those associated with the introduction of Title I.[24] Our main control for the effects of desegregation is black enrollment share. Table 3 presents coefficients on black share from estimates of model (2), for comparison with the existing literature. Black enrollment share is positively related to changes in per-pupil current expenditure, state revenue, and local revenue between 1964 and 1969, consistent with demand for funding having increased more in districts with higher black enrollment shares due to desegregation and with the results for Louisiana reported in Reber (2011).

The first columns of Table 4 show the stability of the coefficients of interest as we include more controls that correlate with desegregation or more flexible functional forms for black enrollment share. Column 1 repeats the TSLS estimates from column 3 of Table 3 for the purposes of comparison. In column 2, we interact black enrollment share with state dummies, thus allowing its effects to differ across states, and in column 3, we include indicators for deciles of black enrollment share in lieu of the linear term in column 1. Column 4 returns to the baseline linear specification and adds more district characteristics that Cascio et al. (2008) identified as predictive of the path to desegregation in the South—the share of the vote cast in the county for Strom Thurmond in the 1948 presidential election (Clubb, Flanigan, and Zingale 2006), a proxy for segregationist preferences, and (the natural log of) 1960 enrollment, a measure of district size. The TSLS coefficients are little changed.

---

[23] See online Appendix B for details. Put differently, the income elasticity would have had to have been about 1.7 to explain our estimates of the Title I-induced increase in education spending. Fisher and Papke (2000) cite estimates of the income elasticity of demand for education spending ranging from 0.40 to 0.65.

[24] Theoretically, desegregation could have decreased demand for school spending. For example, dissatisfaction among whites, increases in private school enrollment, or falling property values may have reduced local support for schools (Clotfelter 1976; Baum-Snow and Lutz 2011). However, the available empirical evidence suggests that desegregation increased demand for spending (Reber 2011; Johnson 2011).

Table 4—Sensitivity of the Estimated Long-Run Fiscal Responses to the Introduction of Title I

	Baseline	Change functional form of black share[a]		Desegregation[b]
	(1)	(2)	(3)	(4)
Panel A. Δ Per-pupil current expenditure				
Δ Per-pupil federal revenue	0.502***	0.482***	0.526***	0.462***
	(0.0795)	(0.0822)	(0.0950)	(0.0828)
Root MSE	289.6	288.2	290.0	290.1
Panel B. Δ Per-pupil state revenue				
Δ Per-pupil federal revenue	0.0519	0.0218	−0.00485	0.0672
	(0.0405)	(0.0402)	(0.0479)	(0.0423)
Root MSE	145.8	142.8	145.9	144.7
Panel C. Δ Per-pupil local revenue				
Δ Per-pupil federal revenue	−0.327***	−0.320***	−0.273***	−0.310***
	(0.0586)	(0.0595)	(0.0672)	(0.0608)
Root MSE	217.8	217.8	215.0	217.2
First-stage partial F-stat on instrument	245.9	240.3	187.0	225.6
Number of districts	910	910	910	910

	Other concurrent policy changes			All preexisting characteristics[b,d]
	Voting Rights Act[c]	Head Start[d]	Other federal programs[e]	
	(5)	(6)	(7)	(8)
Panel A. Δ Per-pupil current expenditure				
Δ Per-pupil federal revenue	0.499***	0.501***	0.485***	0.464***
	(0.0922)	(0.0950)	(0.0838)	(0.0982)
Root MSE	294.4	289.8	289.2	290.2
Panel B. Δ Per-pupil state revenue				
Δ Per-pupil federal revenue	0.0457	0.0368	0.0634	0.0529
	(0.0488)	(0.0484)	(0.0430)	(0.0492)
Root MSE	149.7	146.0	145.0	144.8
Panel C. Δ Per-pupil local revenue				
Δ Per-pupil federal revenue	−0.304***	−0.336***	−0.334***	−0.321***
	(0.0653)	(0.0737)	(0.0613)	(0.0741)
Root MSE	218.1	218.4	217.1	218.0
First-stage partial F-stat on instrument	179.5	174.4	230.2	168.9
Number of districts	812	910	909	910

Notes: Changes in fiscal variables correspond to 1964 to 1969 and are in real 2009 US dollars. Each column and panel represents a different TSLS regression. Unless otherwise noted, all regressions include state dummies and 1960 black enrollment share as controls. The instrument for the change in per-pupil federal revenue is the 1960 child poverty rate. See online Appendix A for a detailed description of control variables and data sources. Standard errors (in parentheses) are clustered on county.

[a]Column 2 interacts 1960 black enrollment share with state dummies. Column 3 replaces 1960 black enrollment share with dummies for deciles of 1960 black enrollment share.

[b]ln (1960 district enrollment) and dummies for quintiles of 1948 Thurmond vote share added as controls.

[c]Black voter registration rate in early 1960s added as a control.

[d]Dummy for one of the 300 poorest counties in 1960 added as a control.

[e]Changes in transfers for other federal programs added as controls.

***Significant at the 1 percent level.

Below, we also show that child poverty does not predict the intensity of desegregation, as measured by the dissimilarity index, and that the spending response was

larger but the level of desegregation no higher for districts more limited in their ability to offset Title I by reducing local revenue.

Taken together, these results suggest that the relative increase in spending among poorer districts during this period was not due to desegregation. The remainder of the table explores other potential confounds. Increases in black political power following the Voting Rights Act of 1965 (VRA) may have resulted in larger increases in education spending for higher poverty districts. Controlling for black enrollment share should account for these impacts to a large extent, but as another test, we include the black voter registration rate in the early 1960s as a proxy for how suppressed the vote was before the VRA.[25] The estimates are little affected (column 5). The estimates are likewise little changed in column 6, where we include an indicator for whether the district was in one of the 300 poorest counties in 1960. These counties received special grant-writing assistance in the 1960s for Head Start (Ludwig and Miller 2007), another program for poor children implemented in the mid-1960s that sometimes channeled funds through school districts. In column 7, we include Head Start spending at the county level as of 1968 along with changes in federal outlays to counties between 1962 and 1969 for other programs—public assistance, retirement programs, and health programs.[26] The estimates are quite similar to the baseline specification. Controlling simultaneously for all preexisting characteristics available for the full sample (column 8) also generates similar estimates as our baseline specification.

IV. Heterogeneous Fiscal Responses to Title I

We expect that school districts reduced local revenue less in response to Title I, thereby increasing spending more, when grants were large relative to local revenue. As discussed in Section II, we expect less crowd-out in districts that would have liked to consume on the restricted portion of the budget constraint. However, even if they did not consume literally at the corner, districts with large Title I grants relative to local revenue may also have been more likely constrained to offset less than they would have liked. Given the underlying increase in funding from all sources over this period (Figure 1), reducing local revenue *relative to the counterfactual* without actively reducing tax rates would have been relatively quick and easy for districts where the grant was small relative to local revenue: they could have simply increased local revenue somewhat less than they otherwise would have. In districts where the grant was large relative to counterfactual local revenue, on the other

---

[25] James Alt generously provided the data for all states except Virginia. We entered data for Virginia from US Commission on Civil Rights (1968). The exact year that black voter registration is measured differs across states subject to data availability (see Alt 1994). Black voter registration rates were on average lower in southern states with literacy tests that were abolished by the VRA (Alabama, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, and Virginia). Cascio and Washington (2012) show that counties with higher black population shares in these states saw larger increases in voter turnout and state transfers (largely for education) after the VRA than counties with higher black population shares elsewhere in the South. The specification in Table 4, column 2 embodies this finding, allowing the effects of black share to differ by state.

[26] Jens Ludwig and Douglas Miller generously provided the list of the 300 poorest counties and Head Start allocations for 1968. Douglas Almond, Hilary Hoynes, and Diane Schanzenbach generously provided the data on changes in federal outlays for other programs.

hand, actual reductions in local revenue, or significantly more time, may have been required to offset Title I.

These observations motivate our investigation of differential fiscal responses depending on the ratio of the Title I grant relative to (counterfactual) local revenue, which we expect to be negatively related to the ease with which a district could offset the grant—the "scope for local offset." This analysis serves as a test of the internal validity of our research design; if our estimates are causal, the spending effects should be larger where we expect them to be in theory. But the analysis serves two important additional purposes. First, because incentives to desegregate (and as we show below, realized desegregation) did not vary with scope for local offset, the results provide an additional way to rule out desegregation as a mechanism or confounder of the results presented in the previous section. Second, the existence of differential spending responses to Title I related to scope for local offset gives us an intuitively appealing way to estimate the effects of Title I-induced spending on educational outcomes.

We would like to know how large the Title I grant was relative to what local revenue would have been in 1969 in the absence of the program; this would tell us by what percent the district would have had to reduce local revenue, relative to the counterfactual, to offset the grant fully. We do not observe counterfactual local revenue, so we estimate it using a district-specific linear extrapolation of the pre-ESEA trend in local revenue from 1961 to 1964.[27] The magnitude of this ratio is much more sensitive than the rank order of districts that it implies to how we estimate counterfactual local revenue.[28] We therefore exploit the rank order of districts by dividing the sample into quartiles according to this measure, rather than interpreting the magnitudes as described above. Although this proxy for scope for offset is noisy, it appears to carry signal throughout its distribution: the magnitudes of the TSLS estimates for changes in per-pupil local revenue and current expenditure are monotonic in the expected direction in quartiles of the proxy (see Table A1). For simplicity, we split the sample at the seventy-fifth percentile and consider the top quartile to encompass "low scope for local offset" districts. Results are substantively similar, but the differences in spending effects less dramatic, if we split the sample at the median of this measure.

Districts with low scope for offset were poorer, blacker, and smaller, on average, compared to the rest of the sample (Table A2). This is not surprising since these districts had larger Title I grants, lower local revenue, or both. To estimate the effects of Title I on fiscal outcomes separately for these two groups of districts, we need to assume that the identification assumption described above holds in each subsample;

---

[27] In a few instances, the prediction for per-pupil local revenue for 1969 was negative. In these cases, we recoded per-pupil local revenue to a small positive number to ensure that the district was coded as having "low scope for local offset" in the analysis to follow. Our substantive results are unchanged if we predict per-pupil local revenue in 1969 on the basis of pre-ESEA trends in the natural log of per-pupil local revenue (or in percentage terms), to ensure that the 1969 prediction of per-pupil local revenue is always positive. Likewise, we obtain similar results when the denominator of this measure is instead a prediction of combined per-pupil local and state revenue arrived at using the same approach.

[28] For example, an alternative approach would be to base our counterfactual on 1964 local revenue only. The rank order of districts would be the same regardless of what we assumed for the (common) growth rate of local revenue between 1964 and 1969, though the magnitude of scope for local offset would vary widely.

Panel A. Per-pupil federal revenue

Panel B. Per-pupil current expenditure

Panel C. Per-pupil state revenue

Panel D. Per-pupil local revenue

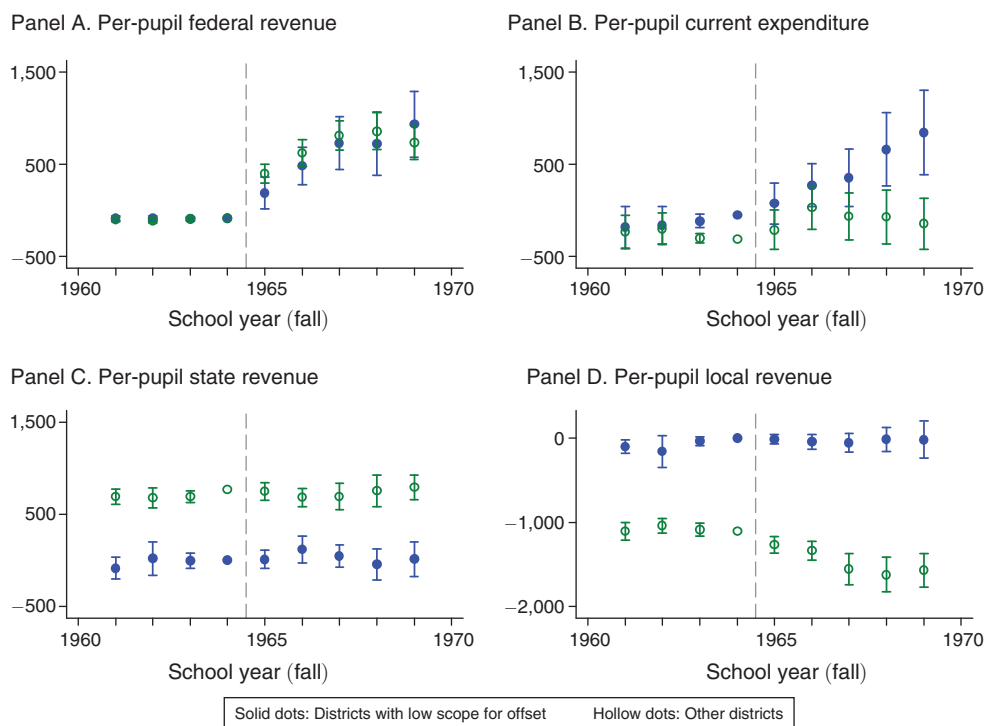Solid dots: Districts with low scope for offset   Hollow dots: Other districts

FIGURE 4. YEAR-BY-YEAR GRADIENTS OF DISTRICT FINANCE VARIABLES
IN THE 1960 CHILD POVERTY RATE, BY SCOPE FOR LOCAL OFFSET

Notes: All figures are in real 2009 US dollars. A district is classified as having "low scope for local offset" if it ranked in the top quartile of the ratio of predicted per-pupil Title I grant to predicted per-pupil local revenue for 1969. Predicted per-pupil local revenue assumes the district-specific linear trend from 1961 to 1964 would have continued to 1969. The dots represent the coefficients on the 1960 district child poverty rate from (unweighted) year-specific regressions, estimated separately for districts with and without low scope for local offset that also include state dummies, the 1960 district black enrollment share, the natural log of 1960 district enrollment, dummies for quintiles of county vote share for Strom Thurmond in the 1948 presidential election, and a dummy for whether the district was in one of the 300 poorest counties in 1960 as controls. The capped lines represent 95 percent confidence intervals on the estimated difference between the poverty gradient in a given year and its value in 1964. To obtain these confidence intervals, we estimated a regression using pooled data from 1961 to 1969 for the subsample in question that included district dummies, state-by-year dummies, and interactions between year dummies (for all years except 1964) and each of the control variables, clustering standard errors on county. The confidence intervals shown are those for the coefficients on the interactions between the 1960 child poverty rate and year dummies, rescaled by the subsample-specific estimates of the 1964 poverty gradient. See footnote 17 for more details.

that is, absent the program, there would not have been a sharp break in the poverty gradient for the outcomes of interest around 1965. Figure 4 shows trends in the poverty gradients separately for low scope for offset districts (solid dots) and other districts (hollow dots). The change in the poverty gradient in federal revenue was the same for both samples (panel A), as expected based on the Title I formula, but increases in federal revenue appear to have translated into higher spending at a much higher rate in low scope for offset districts (panel B). This is because there was significantly more local offset in other districts (panel D). Thus, while the initial *levels* of the poverty gradients sometimes differ across the two samples, there is no apparent pre-ESEA trend in the poverty gradient for any of the outcomes, consistent with the results presented in Figure 3 for the full sample and supportive of the identification assumption.

TABLE 5—THE DESEGREGATION RESPONSE TO THE INTRODUCTION OF TITLE I
AND HETEROGENEITY IN THE FISCAL AND DESEGREGATION RESPONSE
TO THE INTRODUCTION OF TITLE I BY SCOPE FOR LOCAL OFFSET

| | Δ (1964 to 1969) in per-pupil: | | | |
Dependent variable:	Current expenditure (1)	State revenue (2)	Local revenue (3)	1970 dissimilarity index (4)
Panel A. Full sample				
Δ Per-pupil federal revenue, 1964 to 1969	0.464*** (0.0982)	0.0529 (0.0492)	−0.321*** (0.0741)	8.77e-05 (6.30e-05)
First-stage partial *F*-stat on instrument	168.9	168.9	168.9	146.6
Number of districts	910	910	910	853
Panel B. By scope for local offset				
1. Districts with low scope for offset				
Δ Per-pupil federal revenue, 1964 to 1969	0.881*** (0.167)	0.0101 (0.0886)	−0.0262 (0.101)	0.000151 (0.000121)
First-stage partial *F*-stat on instrument	35.29	35.29	35.29	32.79
Number of districts	227	227	227	207
2. Other districts				
Δ Per-pupil federal revenue, 1964 to 1969	0.208 (0.154)	0.0326 (0.0777)	−0.564*** (0.127)	−1.05e-05 (8.99e-05)
First-stage partial *F*-stat on instrument	86.20	86.20	86.20	73.71
Number of districts	683	683	683	646
3. Difference				
Δ Per-pupil federal revenue × low scope for offset	0.673*** (0.226)	−0.0225 (0.117)	0.538*** (0.161)	0.000162 (0.000149)
Number of districts	910	910	910	853

Notes: All dollar figures are in real 2009 US dollars. Each column and panel presents coefficient estimates from a TSLS regression. All regressions include state dummies, 1960 black enrollment share, ln(1960 district enrollment), an indicator that the district is in one of the 300 poorest counties in 1960, and indicators for quintiles of the Thurmond vote share as controls. In panels A, B1, and B2, we instrument for the 1964 to 1969 change in per-pupil federal revenue with the 1960 child poverty rate. In panel B3, we instrument for the 1964 to 1969 change in per-pupil federal revenue with the 1960 child poverty rate, and we instrument for the 1964 to 1969 change in per-pupil federal revenue interacted with the low scope for offset indicator with the 1960 child poverty rate interacted with low scope for offset indicator. A district is classified as having "low scope for local offset" if it ranked in the top quartile of the ratio of predicted per-pupil Title I grant to predicted per-pupil local revenue for 1969. Predicted per-pupil local revenue assumes the district-specific linear trend from 1961 to 1964 would have continued to 1969. Standard errors (in parentheses) are clustered on county.

    ***Significant at the 1 percent level.

To facilitate the dollar-for-dollar interpretation, we estimate TSLS regressions separately for the two samples and present results in Table 5 analogous to those in Table 4. All specifications include controls for all preexisting variables that can be measured for all districts (column 8 of Table 4). Columns 1 to 3 of Table 5 show the effect of the 1964 to 1969 change in per-pupil federal revenue on the 1964 to 1969 changes in per-pupil spending, per-pupil state revenue, and per-pupil local revenue, respectively, separately for districts with low scope for local offset (panel B1) and for all other districts (panel B2). Estimates for the full sample in the same specification are repeated in panel A for comparison.

The estimates by scope for offset align with our expectations and the results shown graphically above. Current expenditure increased substantially more between 1964 and 1969 in districts with low scope for local offset—a statistically significant $0.88 for each dollar of federal revenue, compared to an insignificant $0.21 on the dollar

for the remaining districts (column 1). The difference in these estimates is a statistically significant $0.67 (panel B3). There is no significant state revenue response for either group of districts, and no significant difference in state revenue responses across the two subsamples (column 2). However, for the quarter of districts with low scope for offset, there was indeed no reduction in local revenue in response to Title I, while the remaining districts reduced local revenue by $0.56 on the dollar. The difference in local revenue responses across the two groups is a statistically significant $0.54, which can account for a substantial fraction of the difference in spending effects across groups.[29]

If our estimates are confounded by desegregation, we would expect to see a different relationship between the poverty rate and desegregation in the low scope for offset districts, where Title I funding increased spending more, compared to the rest of the sample. In column 4, we show results from the same specification as in columns 1 to 3, but using as a dependent variable the realized level of desegregation as measured by the dissimilarity index in 1970.[30] In fact, the 1960 poverty rate is not related to the realized level of desegregation in the full sample (panel A) or either subsample (panel B). The estimates are statistically insignificant and substantively small,[31] suggesting that desegregation-related changes in demand for school spending are not biasing the estimated effects of Title I on fiscal outcomes.

V. Title I and Educational Attainment

The results thus far suggest that Title I raised school spending in the South, and considerably more so in districts where the grants were large relative to estimated counterfactual local revenue, where the ability to offset Title I funds was likely constrained. Did these increases in school spending improve student outcomes? Data on educational outcomes at the county or district level are limited for this time period, and we do not have annual measures of educational outcomes as we did for the

[29] One might be concerned that these estimates reflect variation in the fiscal response along some other dimension correlated with low scope for offset, like having a high black enrollment share or a high poverty rate. Potential heterogeneity by black enrollment share is particularly interesting because of the key role that race played in southern politics and school finance during this period. When we estimate fully interacted versions of the models estimated for Table 5, panel B3 to check for heterogeneous fiscal responses by low scope for local offset and high black enrollment share simultaneously, the estimates for scope for offset are statistically and substantively unchanged, and there is no significant heterogeneity by black enrollment share. Separately identifying differential responses by low scope for offset and child poverty is more difficult. When we allow higher poverty districts to have a differential response, the coefficient on the low scope for offset interaction falls in the specifications predicting changes in current expenditure and local revenue, and the standard errors increase. Neither interaction coefficient is individually significant, though they are jointly significant. While it is therefore possible that Title I grants were "stickier" in low scope for offset districts because they were "stickier" in poor districts, we do not have the power to distinguish between these hypotheses.

[30] We calculated the dissimilarity index using school-level data on enrollment by race for fall 1970 reported in HEW (1972). We use data from 1970 rather than 1969 because district coverage is more complete, and the legal environment was similar in both years. Results are similar for 1969. In results not reported, as an alternative way to address the concern that desegregation could be confounding the results, we added the 1970 dissimilarity index as well as an indicator for the presence of a court order in 1970 to the specifications reported in Table 4. This did not affect the coefficients of interest, but we do not report these results since both of these variables are potentially endogenous to the Title I grant.

[31] A typical major court-ordered school desegregation plan reduced the dissimilarity index by about 0.22 (Reber 2005). By comparison, the lower bound of the 95 percent confidence interval for the coefficient in panel A suggests that an additional $100 Title I-induced increase in federal revenue was associated with a reduction in the dissimilarity index of only 0.004.

analysis of fiscal outcomes. While we think it is instructive to analyze trends in educational attainment for this period to shed light on the potential effects of Title I, we therefore consider these results to be significantly more speculative than the results presented above.

Recall there was limited enforcement of even nominal targeting to "educationally deprived" students during this era, so Title I-induced increases in school expenditure could easily have benefited other children. We therefore examine how Title I affected all students, and view our analysis as being more closely related to the literature on the effects of school spending on student outcomes than the literature on the achievement effects of Title I.[32] We do, however, present separate estimates for blacks and whites to explore the possible importance of race in the within-district allocation of funds. All states discontinued publication of spending by race by the mid-1960s, so we cannot examine this directly. Up to that time, school boards allocated state funding disproportionately to white schools (Margo 1990; Reber 2011); the same local school boards controlled the disbursal of Title I funds to schools, which were still largely identifiable by race during our study period. Prior to legislative and regulatory changes in 1970, school boards could comply with the letter but not the spirit of Title I by giving the new federal dollars to poorer and blacker schools while simultaneously redistributing other revenue streams to continue to favor white schools on net.

A. *Graphical and Regression Evidence*

Our outcome of interest is the change in the high school dropout rate of 18–19-year-olds over the 1960s, where the high school dropout rate is defined as the share of individuals who are neither enrolled in school nor have 12 years of completed schooling.[33] High school dropout, unlike other student outcomes of interest, can be consistently observed at a local level both before and after Title I. It was also a relevant margin of attainment for the South at this time, as 32 percent of whites and 45 percent of blacks aged 18 and 19 were high school dropouts in 1960.[34] We purchased special tabulations of the 1960 and 1970 censuses of population to estimate dropout rates for those years at the lowest possible level of geographic disaggregation—the county.[35] Because counties vary dramatically in size, and school districts, not counties, are the relevant decision makers, we weight the analysis by county population

---

[32] Several major studies—the Sustaining Effects Study (Carter 1984) and the Prospects study (Puma et al. 1997)—attempt to estimate the effects of participating in Title I programs on student outcomes. These studies implicitly assume that participation in a Title I program reflects additional resources to the participating student (not full crowd-out), and that nonparticipating students are unaffected (no spillovers). Identifying these effects is difficult, given the negative selection of participants by design (see Borman and D'Agostino (1996) for a review).

[33] Technically speaking, the high school dropout rate so defined may include individuals who never started high school, as students commonly exited the educational system before high school during this period (Collins and Margo 2006).

[34] Reducing high school dropout rates would be unambiguously good if graduation standards were clearly defined and followed. If schools graduate more students because of pressure to do so, rather than increasing shares of students achieving some set level of competency, dropout rates do not reveal underlying changes in true levels of human capital.

[35] Our sample includes 838 districts in 647 counties. We restrict attention to counties where districts in our estimation sample represent at least 90 percent of total county enrollment in 1960. Though all discussion of impacts on high school dropout refers to counties, recall that the relevant fiscal decisions are made at and aggregated from the district level.

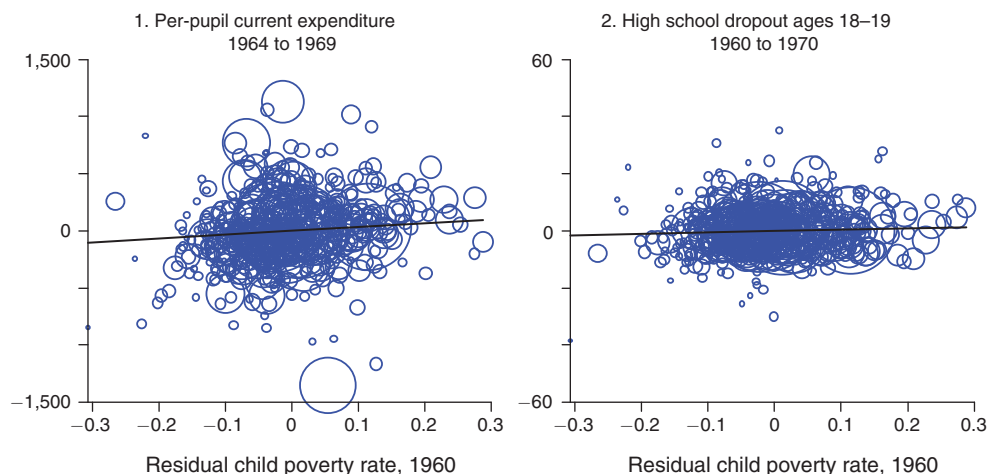Panel A. Counties with low scope for local offset
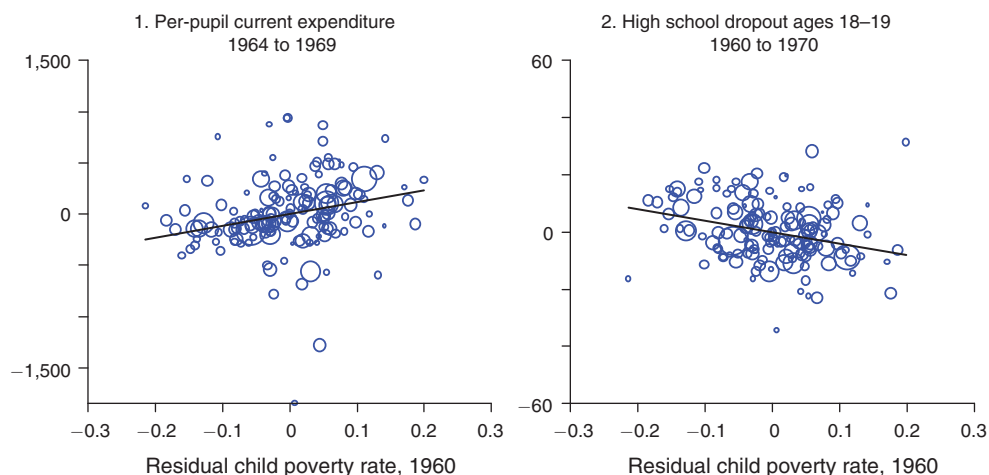
Figure 5. The 1960 Child Poverty Rate and Differences in Current Expenditure and White High School Dropout Rates over the 1960s, by Scope for Local Offset

Notes: All figures are in real US 2009 dollars. A county is classified as having "low scope for local offset" if all districts in the county ranked in the top quartile of the ratio of predicted per-pupil Title I grant to predicted per-pupil local revenue for 1969. Predicted per-pupil local revenue assumes the district-specific linear trend from 1961 to 1964 would have continued to 1969. The hollow dots represent the residuals from regressions, estimated separately for counties with and without low scope for local offset, that also include state dummies, the 1960 county black enrollment share, the natural log of 1960 county enrollment, dummies for quintiles of county vote share for Strom Thurmond in the 1948 presidential election, a dummy for whether the county was one of the 300 poorest in 1960, and the high school dropout rate of white 18–19-year-olds in the county in 1960. The regressions are weighted (and the dot sizes represent) the 1960 population of white 18–19-year-olds.

(of 18–19-year-olds, by race) in 1960. The estimates are substantively similar but less precise when we estimate the model without weights, suggesting that weighting primarily corrects for heteroskedasticity. On average, southern blacks experienced a 14.1 percentage point reduction in likelihood of high school dropout at ages 18 to 19 between 1960 and 1970; for whites, this figure was 10 percentage points.

TABLE 6—REDUCED-FORM RELATIONSHIP BETWEEN 1960 CHILD POVERTY RATE
AND CHANGES IN CURRENT SCHOOL EXPENDITURE
AND HIGH SCHOOL DROPOUT RATES AT THE COUNTY LEVEL

Dependent variable:	Δ Per-pupil current expenditure, 1964–1969 (1)	Δ White HS dropout ages 18–19, 1960–1970 (2)	Δ Per-pupil current expenditure, 1964–1969 (3)	Δ Black HS dropout ages 18–19, 1960–1970 (4)
Panel A. Sample: Counties with low scope for local offset				
Child poverty, 1960	1,156.32***	−39.98***	1,181.56***	7.75
	(287.77)	(10.59)	(321.19)	(11.58)
Number of counties	152	152	135	135
Panel B. Sample: Other counties				
Child poverty, 1960	338.89	4.76	564.98***	6.69
	(244.81)	(3.37)	(172.87)	(5.05)
Number of counties	495	495	470	470
Panel C. Full sample: Difference				
Child poverty, 1960 × low scope for local offset	817.43**	−44.74***	616.58*	1.06
	(372.11)	(10.78)	(354.43)	(12.23)
Number of counties	647	647	605	605
Weight	Whites ages 18–19, 1960		Blacks ages 18–19, 1960	

Notes: The change in per-pupil current expenditure (columns 1 and 3) is in real 2009 US dollars. Each column and panel presents estimates from a different regression. All regressions are weighted by the race-specific population of 18–19-year-olds in 1960. All regressions include state dummies, 1960 black enrollment share, ln(1960 district enrollment), an indicator that the county was one of the 300 poorest counties in 1960, indicators for quintiles of the Thurmond county vote share, and the race-specific high school dropout rate of 18–19-year-olds in 1960 as controls; in panel C, these controls are interacted with the low scope for offset indicator. Throughout, attention is restricted to counties where districts in our estimation sample represent at least 90 percent of total county enrollment in 1960. A county is classified as having "low scope for local offset" if all districts in the county ranked in the top quartile of the ratio of predicted per-pupil Title I grant to predicted per-pupil local revenue for 1969. Predicted per-pupil local revenue assumes the district-specific linear trend from 1961 to 1964 would have continued to 1969. Standard errors (in parentheses) are heteroskedasticity robust.

***Significant at the 1 percent level.
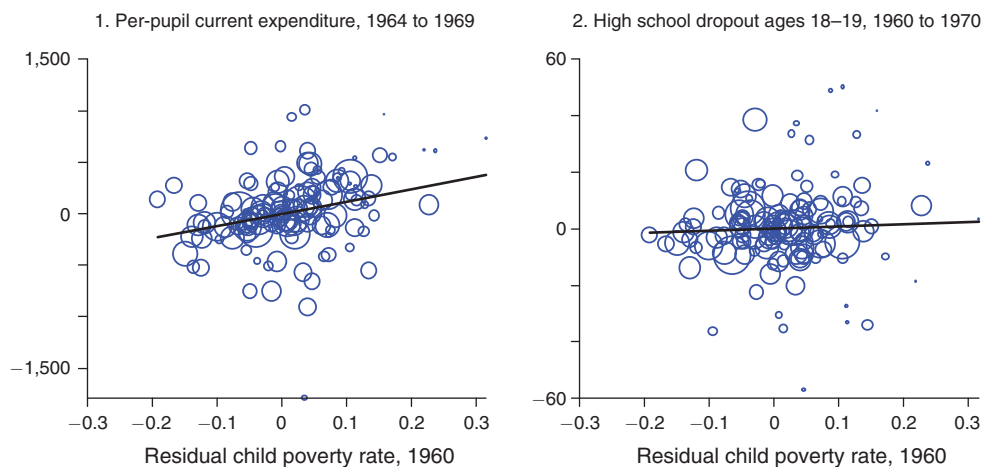**Significant at the 5 percent level.
*Significant at the 10 percent level.

We exploit the heterogeneity in the spending response, shown above, as a source of identification. To set ideas, the first column in Figure 5 shows the regression-adjusted relationship between 1960 child poverty rates and the change in per-pupil current expenditure between 1964 and 1969, separately in counties where all districts had low scope for local offset (panel A) and in the remaining counties (panel B); the slope estimates are presented in the first column of the same respective panels of Table 6. The regressions were estimated using county aggregates of the district-level finance data and are otherwise similar to the reduced-form specifications that underlie the district-level TSLS estimates in Table 5.[36] The dot sizes represent the

---

[36] County aggregates were generated from all district-level data weighting by 1960 district enrollment. The specification also includes the 1960 race-specific high school dropout rate of 18–19-year-olds to account for the possibility that the trend in high school dropout depended on the starting point. We unfortunately cannot examine (or control for) trends in high school dropout by race at the county level during the 1950s.

Panel A. Counties with low scope for local offset

1. Per-pupil current expenditure, 1964 to 1969

2. High school dropout ages 18–19, 1960 to 1970

Panel B. Other counties

1. Per-pupil current expenditure, 1964 to 1969

2. High school dropout ages 18–19, 1960 to 1970

Residual child poverty rate, 1960
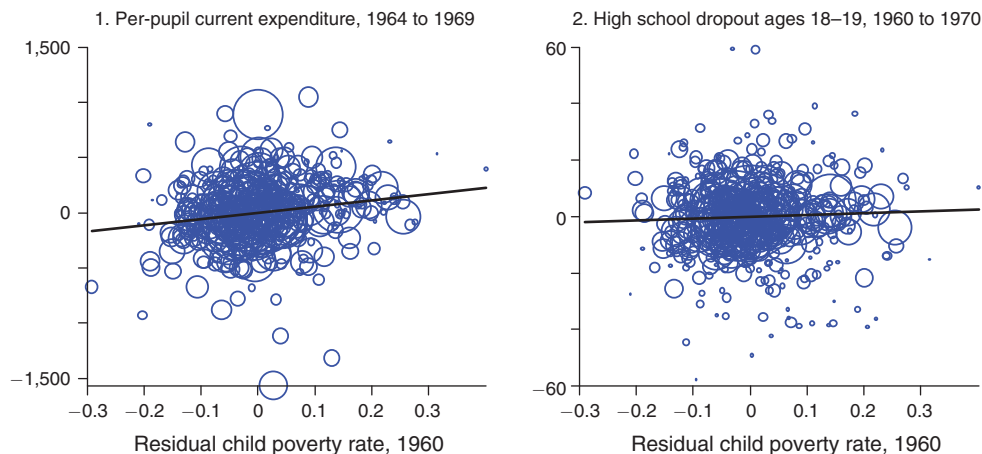
Residual child poverty rate, 1960

FIGURE 6. THE 1960 CHILD POVERTY RATE AND DIFFERENCES IN CURRENT EXPENDITURE
AND BLACK HIGH SCHOOL DROPOUT RATES OVER THE 1960S, BY SCOPE FOR LOCAL OFFSET

Notes: All figures are in 2009 US dollars. A county is classified as having "low scope for local offset" if all districts in the county ranked in the top quartile of the ratio of predicted per-pupil Title I grant to predicted per-pupil local revenue for 1969. Predicted per-pupil local revenue assumes the district-specific linear trend from 1961 to 1964 would have continued to 1969. The hollow dots represent the residuals from regressions, estimated separately for counties with and without low scope for local offset, that also include state dummies, the 1960 county black enrollment share, the natural log of 1960 county enrollment, dummies for quintiles of county vote share for Strom Thurmond in the 1948 presidential election, a dummy for whether the county was one of the 300 poorest in 1960, and the high school dropout rate of black 18–19-year-olds in the county in 1960. The regressions are weighted (and the dot sizes represent) the 1960 population of black 18–19-year-olds.

size of the county's 18–19-year-old white population in 1960 to reflect weighting of the regression fit. Consistent with findings reported in Table 5, school spending became much more progressive over the second half of the 1960s in the subsample of counties where the scope for local offset by districts was low. The finding is similar in the subsample of counties with black populations and weighting by initial

county black population, as shown in the first column of Figure 6 and the third column of Table 6.

If "money mattered," we would expect to see greater convergence in educational attainment between poorer and richer counties over the 1960s in the subsample where the scope for local offset was low, or where Title I translated into more spending at a higher rate. We explore this in the second column of each figure using the same specification as in the first column, but replacing the dependent variable with the 1960 to 1970 change in high school dropout rates of 18–19-year-olds. For whites (column 2 of Table 6), the reduction in high school dropout over the 1960s was much larger in poorer counties than richer counties in the subsample with low scope for offset (panel A), but not elsewhere (panel B), suggesting that Title I-induced spending increases improved white educational outcomes. This was not the case for blacks, however (column 4).

We rescale the reduced-form estimates for high school dropout by the corresponding reduced-form estimates for the 1964 to 1969 change in annual per-pupil spending to make their magnitudes more interpretable. For example, the estimates in panel A imply that each additional $100 increase in per-pupil school spending was associated with a 3.46 percentage point decrease in high school dropout for whites $(= -39.98/(1,156.32/100))$. We take this approach to maintain consistency with our empirical analysis thus far, but note that scaling by the *cumulative* increase in spending resulting from Title I may be more appropriate. The cohorts in our analysis were exposed to about five years of Title I-induced spending increases before we observe high school dropout in 1970 (the 1969–1970 school year). Title I grants were slightly smaller in the early years (and some districts did not receive their grants due to noncompliance with the CRA), so an additional $100 in spending by fall 1969 likely corresponds to somewhat less than $500 of additional cumulative spending between 1965 and 1969. To think about the effects of an additional $100 of *cumulative* spending exposure, the estimates presented would therefore need to be scaled down by about a factor of around five.

We present TSLS estimates of the effects of changes in current expenditure on changes in high school dropout in Table 7. In panel A, we instrument for the change in per-pupil current expenditure (in hundreds of real 2009 US dollars) with the 1960 child poverty rate, limiting the sample to counties with low scope for offset, where the spending response to Title I for both blacks and whites was statistically significant. This is the formalization of the example described intuitively above. The identifying assumption is that, in the absence of the Title I, there would have been no change in the poverty gradient in outcomes in these counties. In panel B, we instrument for the change in per-pupil current expenditure with the 1960 child poverty rate *interacted with* an indicator for whether the county's districts had low scope for offset using the full sample. These estimates rescale the reduced-form estimate for the change in high school dropout in panel C of Table 6 by the corresponding reduced-form estimate for the change in per-pupil current expenditure. In this specification, we thus allow the 1960 child poverty rate to have an effect on high school dropout through channels other than educational expenditure. The identifying assumption here is that, in the absence of the program, changes in the poverty gradient in high school dropout would have been the same in both sets of counties.

TABLE 7—TSLS ESTIMATES OF THE EFFECT OF SCHOOL SPENDING ON HIGH SCHOOL DROPOUT BY RACE

Δ White high school dropout (%), 1960–70:	Ages 18–19 (1)	Ages 18–19 (2)	Ages 25+ (3)
Mean of dependent variable	−10.0	−10.0	−7.7
Panel A. Instrument is 1960 child poverty rate (sample is counties with low scope for local offset)			
Δ Per-pupil current expenditure ($100s), 1964 to 1969	−3.46***	−4.52**	0.55
	(1.25)	(1.88)	(0.41)
Root MSE	15.42	17.74	4.252
First-stage partial *F*-stat on instrument	16.15	9.78	20.39
Number of counties	152	152	152
Panel B. Instrument is 1960 child poverty rate × low scope for local offset (full sample)			
Δ Per-pupil current expenditure ($100s), 1964 to 1969	−5.47*	−6.28*	0.48
	(2.80)	(3.72)	(0.56)
Root MSE	17.26	19.31	3.63
First-stage partial *F*-stat on instrument	4.826	3.536	4.346
Number of counties	647	646	646

Δ Black high school dropout (%), 1960–70:	Ages 18–19 (4)	Ages 18–19 (5)	Ages 25+ (6)
Mean of dependent variable	−14.1	−14.1	−7.9
Panel A. Instrument is 1960 child poverty rate (sample is counties with low scope for local offset)			
Δ Per-pupil current expenditure ($100s), 1964 to 1969	0.66	1.17	0.39
	(1.05)	(1.09)	(0.24)
Root MSE	10.28	10.51	2.35
First-stage partial *F*-stat on instrument	13.53	13.54	13.49
Number of counties	135	135	124
Panel B. Instrument is 1960 child poverty rate × low scope for local offset (full sample)			
Δ Per-pupil current expenditure ($100s), 1964 to 1969	0.17	0.98	−0.29
	(2.01)	(1.82)	(0.46)
Root MSE	8.159	8.569	2.430
First-stage partial *F*-stat on instrument	3.026	4.081	3.180
Number of counties	605	604	541
Additional controls?		X	

Notes: The change in per-pupil current expenditure is in hundreds of real 2009 US dollars. Each column and panel presents estimates from a different regression. All regressions are weighted by the race-specific 1960 county population of the relevant age group. All regressions include state dummies, 1960 black enrollment share, ln (1960 district enrollment), an indicator that the county was one of the 300 poorest counties in 1960, indicators for quintiles of the Thurmond county vote share, and race-specific 1960 high school dropout rates of the relevant age group as controls; in panel B, these controls are interacted with the low scope for offset indicator. "Additional controls" include changes in transfers to the county for other federal programs from 1962–1969, which are interacted with the low scope for offset indicator in panel B. Throughout, attention is restricted to counties where districts in our estimation sample represent at least 90 percent of total county enrollment in 1960. Standard errors (in parentheses) are heteroskedasticity robust.

*** Significant at the 1 percent level.
 ** Significant at the 5 percent level.
  * Significant at the 10 percent level.

Columns 1 and 4 of Table 7 present the TSLS estimates for the baseline specification for changes in white and black high school dropout rates, respectively. The estimates in panel A, which come from the first identification strategy, imply that each additional $100 increase in annual current expenditure per pupil between 1964 and 1969 was associated with a statistically significant 3.46 percentage point decrease in the likelihood of white high school dropout, and an insignificant 0.66 percentage point increase in the likelihood of black high school dropout over the 1960s.

Allowing for a direct effect of 1960 poverty by using other counties as a comparison group, in panel B, we find slightly more negative estimates—a marginally statistically significant −5.47 for whites, and an insignificant 0.17 for blacks. The first stage is unsurprisingly weaker in these models, which are more demanding of the data.[37] Nevertheless, these models suggest that failure to allow for a direct effect of 1960 child poverty on high school dropout rates may bias the models in panel A against finding an effect. Indeed, though generally not statistically significant, the coefficients on the 1960 child poverty rate in the second specification (not shown) are positive.

The estimates are robust to several specification tests. For example, in columns 2 and 5, we include changes in transfer payments to the county over the 1960s through other federal programs, including Head Start and Medicaid, and see if anything an increase in the magnitude of the estimates for whites.[38] Perhaps more compelling, we see no effect of spending on high school dropout rates of individuals whose secondary education would have been completed prior to 1965, using as a dependent variable the change between 1960 and 1970 in the percent of a county's whites and blacks aged 25 and older without a high school degree (columns 3 and 6).[39] Presumably, unobserved shocks to educational attainment in the county population—through migration, for example—would have affected this older age group as well.

We do not have similar county-level data for 1950, so we cannot directly conduct the "placebo test" on pre-ESEA trends. We have located county-level data for 1950 and 1960 on the school enrollment rates of 16–17-year-olds, however (Historical Census Browser 2004). These data cover a different age group and, more importantly, are not reported separately by race. Nevertheless, the trend in this measure should reflect general trends in educational attainment of young people during the 1950s. The coefficients of interest reported in Table 7 are not affected by the inclusion of the change in this variable between 1950 and 1960. When we estimated the specifications in columns 1 and 4 with the 1950 to 1960 change in enrollment rates as the dependent variable, the point estimate on the change in per-pupil current expenditure from 1964 to 1969 was always insignificant and small relative to the estimates for white high school dropout over the 1960s, with large standard errors; furthermore, the coefficient was negative in three out of four cases, suggesting, if anything, *increases* in dropout over the 1950s.

Because spending was low and high school dropout rates high during the period of study, the effects on educational attainment that we estimate may be larger than would be expected from modern-day spending increases. It is difficult to compare

---

[37] Note that the first-stage coefficients reported in Table 7 should be compared to the reduced-form coefficients in Table 5. For example, the reduced-form F-stat on the instrument from the district-level TSLS models linking federal revenue to spending in column 1 of Table 5, panel B1 is 16.56. The first-stage F-stats from the county-level TSLS models linking spending to high school dropout reported in Table 7, panel A are slightly smaller because the county-aggregated data offer less variation in the key explanatory variables and fewer observations, reducing power.

[38] These estimates are similarly unaffected by the inclusion of the 1970 dissimilarity index as a control. The estimates in panel B are also substantively similar but less precise when allow for heterogeneity in the direct effects of child poverty by whether all districts in the county had high (top quartile) 1960 black enrollment shares.

[39] We calculated high school completion rates from data published in US Department of Commerce (1963) and downloaded from the National Historical Geographic Information System (Minnesota Population Center 2004).

these estimates to those from studies that estimate the impacts of school spending for more recent cohorts, because such studies tend to measure achievement with test scores. Even comparing our findings to those for earlier cohorts is difficult: while most existing studies of earlier cohorts measure achievement with educational attainment, they also tend to measure school inputs directly (i.e., with pupil-teacher ratios), instead of with spending.[40]

We can think about the magnitudes of the estimates in several ways. First, a back-of-the-envelope cost-benefit analysis focusing solely on labor market returns to an additional year of schooling for whites implies that the value of the social benefits was larger than the spending increase (see online Appendix B for details). Second, the point estimate for whites implies that Title I can explain 37 percent of the 10 percentage point decrease in their high school dropout rate over the decade. By contrast, using the 95 percent confidence interval, we estimate that Title I can account for at most 24 percent of the 14 percentage point decrease in black high school dropout over the 1960s.[41]

B. *Evidence on Within-District Allocation*

Our estimates suggest that whites benefited from Title I-induced changes in educational expenditure, but blacks did not. The estimates are imprecise enough that we cannot reject moderate beneficial effects for blacks, but we reject that the effects are the same for the two groups.

One possible explanation for these findings is that Title I-induced spending increases were disproportionately directed toward whites. Because race-specific spending data are not available, we cannot directly examine to what extent within-district allocations of Title I funds targeted white students. And our analysis of changes in race-specific pupil-teacher ratios before and after Title I for the two states where data was available was uninformative. However, our analysis of changes in *overall* pupil-teacher ratios between 1964 and 1969 for five states in our estimation sample implies that Title I-induced reductions in pupil-teacher ratios can account for 36 to 40 percent of the spending increase.[42] Given the strong link between pupil-teacher ratios and black educational attainment in slightly earlier cohorts (e.g., Card

---

[40] See, for example, Card and Krueger (1992a, 1992b, 1996) and Ashenfelter, Collins, and Yoon (2006). Such studies find positive effects of inputs on educational attainment and wages. Reber (2010) estimates the effects of desegregation-induced changes in spending on educational attainment for blacks and finds somewhat smaller effects. Existing work estimating the effects of educational spending tends to use test scores as the educational outcome of interest (Hanushek 1997).

[41] These calculations scale up the coefficient on the change in per-pupil funding (or its upper bound) in the baseline specification of panel A of Table 7 by the predicted change in per-pupil spending from Title I for the average child, then divide by the (weighted) mean of the dependent variable. The predicted change in per-pupil spending from Title I is calculated separately by race, weighting by the 1960 race-specific population of 18–19-year-olds and controlling for their high school dropout rate. We focus on the panel A estimates because they are more precise.

[42] The states with pupil-teacher ratio data are Alabama, Florida, Louisiana, Tennessee, and Virginia. Using the same specification as in Table 3, we estimate a positive and statistically significant coefficient on child poverty for the change in teacher-pupil ratios (0.00518 (0.00130)). Given average teacher salaries in these states in 1964 and 1969 ($36,423 and $43,617, respectively, in real 2009 US dollars) and the corresponding coefficient estimate on child poverty for the change in per-pupil current expenditure in this sample ($561.6), these estimates imply that reductions in pupil-teacher ratios can account for about 36 to 40 percent of the spending increase induced by Title I.

and Krueger 1992a), we expect blacks would have benefited from these reductions in pupil-teacher ratios had they actually experienced them.

Moreover, targeting of Title I-induced spending increases toward whites was plausible. Substantial desegregation occurred only after 1968,[43] making it possible for districts to continue directing resources to white schools as they had historically done (Margo 1990). Targeting of Title I funds was also weak at the time, as discussed in Section I. Martin and McClure (1969) present many examples of districts using funds in schools not designated as Title I recipients.[44] But such blatant misuses of funds *identified as Title I* are only one way in which non-Title I schools could benefit from the program. More easily and without violating any laws or regulations at the time, districts could have allocated more state and locally generated revenue to non-Title I schools, using Title I funds "correctly" in Title I schools. Martin and McClure's (1969) documentation of such behavior lent political pressure for the increased regulation of the use of Title I funds starting with the 1969 ESEA amendments.

VI. Conclusion

This paper examines the fiscal and educational impacts of the introduction of Title I of the Elementary and Secondary Education Act in the South. Combining variation in the program's intensity across school districts with the timing of its introduction in 1965, we find evidence of an important role for Title I in increasing the progressivity of spending on southern schools during the 1960s. School districts responded to the influx of Title I funding by significantly reducing their own fiscal effort, and more so where Title I grants were small relative to what local revenue would have been had pre-ESEA trends continued. "Money mattered," but only where the introduction of Title I increased spending, and only for whites.[45] Despite this, the program appears to have been cost-effective overall in the South on the basis of our estimates.

Our analysis contributes to understanding of the impact of Title I in its earliest years, but necessarily falls short of a full assessment of Title I's legacy. The introduction of Title I likely also had other benefits—on other educational or social outcomes, on other cohorts, and through the increased consumption of other goods that crowd-out represents—that are not easily quantified. The effects of Title I's

---

[43] A growing literature examining the effects of policy efforts to narrow black-white school quality gaps and to desegregate schools consistently concludes that such programs were beneficial for black educational attainment. See, for example, Reber (2010); Johnson (2011); Card and Krueger (1992a); Lutz (2011); Ashenfelter, Collins, and Yoon (2006); and Guryan (2004). Similar to Reber (2010) and consistent with the idea that desegregation-induced spending helped blacks, we find that spending and educational attainment for blacks both increased more in districts with higher black enrollment shares.

[44] For example: "In Oxford, Mississippi, a curriculum and materials center is located at a non-Title I school, near a police station, reportedly for fear of burglary. Furthermore, the Title I coordinator in Oxford is principal of a non-Title I, white school" (Martin and McClure 1969, 6). An HEW audit of Louisiana school districts covering Title I expenditures in the program's first year found that 23 counties "loaned" equipment costing $645,624 to schools that were ineligible to participate in Title I programs. The auditors noted that much of the "loaned" equipment was "set in concrete or fastened to the plumbing" (Martin and McClure 1969, 9).

[45] Our findings are in contrast to the existing literature on Title I, which has concluded that the program is largely ineffective. However, we ask a different question by estimating returns to increases in education spending induced by Title I for all students in a district, rather than comparing outcomes of participants in whatever was called a Title I program at a particular point in time to those for nonparticipants (as in Carter 1984; Puma et al. 1997).

introduction might well have been different outside of the South. These questions are important ones for future research.

Our analysis also makes a more general point. In the same vein as Baicker and Staiger's (2005) analysis of Medicaid Disproportionate Share Hospital funding and van der Klaauw's (2008) school-level analysis of Title I programs in New York City, our findings emphasize the usefulness of examining the impacts of intergovernmental grants on a jurisdiction's finances alongside any evaluation of their impact on the ultimate outcome of interest. Indeed, the introduction of Title I improved educational attainment only where it increased education spending.

REFERENCES

Alabama Department of Education. 1961–1970. *Annual Report for the Scholastic Year Ending … and for the Fiscal Year Ending … : Statistical and Financial Data*. Alabama Department of Education. Montgomery, AL.

Almond, Douglas, Kenneth Y. Chay, and Michael Greenstone. 2006. "Civil Rights, the War on Poverty, and Black-White Convergence in Infant Mortality in the Rural South and Mississippi." Massachusetts Institute of Technology (MIT) Department of Economics Working Paper 07-04.

Almond, Douglas, Hilary W. Hoynes, and Diane Whitmore Schanzenbach. 2011. "Inside the War on Poverty: The Impact of the Food Stamps on Birth Outcomes." *Review of Economics and Statistics* 93 (2): 387–403.

Alt, James E. 1994. "The Impact of the Voting Rights Act on Black and White Voter Registration in the South." In *Quiet Revolution in the South: The Impact of the Voting Rights Act, 1965–1990*, edited by Chandler Davidson and Bernard Grofman, 351–77. Princeton: Princeton University Press.

Ashenfelter, Orley, William J. Collins, and Albert Yoon. 2006. "Evaluating the Role of Brown v. Board of Education in School Equalization, Desegregation, and the Income of African Americans." *American Law and Economics Review* 8 (2): 213–48.

Baicker, Katherine, and Douglas Staiger. 2005. "Fiscal Shenanigans, Targeted Federal Health Care Funds, and Patient Mortality." *Quarterly Journal of Economics* 120 (1): 345–86.

Barnett, Jeffrey. 2011. *State and Local Government Finances Summary: 2009*. U.S. Census Bureau Department of Commerce Governments Division Brief G09-ALFIN. Washington, DC, October.

Baum-Snow, Nathaniel, and Byron F. Lutz. 2011."School Desegregation, School Choice, and Changes in Residential Location Patterns by Race." *American Economic Review* 101 (7): 3019–46.

Borman, Geoffrey D., and Jerome V. D'Agostino. 1996. "Title I and Student Achievement: A Meta-Analysis of Federal Evaluation Results." *Educational Evaluation and Policy Analysis* 18 (4): 309–26.

Brooks, Leah, and Justin H. P**hillips.** 2010. "An Institutional Explanation for the Stickiness of Federal Grants." *Journal of Law, Economics, and Organization* 26 (2): 243–64.

Brown, Cynthia G. 2011. "Fractured Governance of Resources and the Need for a Coherent and Fair System of Funding to Support High Quality Public Schools." Paper presented at Rethinking Education Governance in the Twenty-First Century Conference, Thomas B. Fordham Institute and Center for American Progress, Washington, DC, December 1.

Card, David, and Alan B. Krueger. 1992a. "School Quality and Black-White Relative Earnings: A Direct Assessment." *Quarterly Journal of Economics* 107 (1): 151–200.

Card, David, and Alan B. Krueger. 1992b. "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." *Journal of Political Economy* 100 (1): 1–40.

Card, David, and Alan B. Krueger. 1996. "School Resources and Student Outcomes: An Overview of the Literature and New Evidence from North and South Carolina." *Journal of Economic Perspectives* 10 (4): 31–50.

Card, David, and A. Abigail Payne. 2002. "School finance reform, the distribution of school spending, and the distribution of student test scores." *Journal of Public Economics* 83 (1): 49–82.

Carter, Launor F. 1984. "The Sustaining Effects Study of Compensatory and Elementary Education." *Educational Researcher* 13 (7): 4–13.

Cascio, Elizabeth, Nora Gordon, Ethan Lewis, and Sarah Reber. 2008. "From Brown to busing." *Journal of Urban Economics* 64 (2): 296–325.

Cascio, Elizabeth, Nora Gordon, Ethan Lewis, and Sarah Reber. 2010. "Paying for Progress: Conditional Grants and the Desegregation of Southern Public Schools." *Quarterly Journal of Economics* 125 (1): 445–82.

Cascio, Elizabeth U., Nora Gordon, and Sarah Reber. 2013. "Local Responses to Federal Grants: Evidence from the Introduction of Title I in the South: Dataset." *American Economic Journal: Economic Policy*. http://dx.doi.org/10.1257/pol.5.3.126.

Cascio, Elizabeth U., and Ebonya L. Washington. 2012. "Valuing the Vote: The Redistribution of Voting Rights and State Funds Following the Voting Rights Act of 1965." National Bureau of Economic Research (NBER) Working Paper 17776.

Clark, Melissa A. 2003. "Education Reform, Redistribution, and Student Achievement: Evidence from the Kentucky Education Reform Act." PhD diss. Princeton University.

Clotfelter, Charles T. 1976. "School Desegregation, 'Tipping' and Private School Enrollment." *Journal of Human Resources* 11 (1): 28–50.

Clubb, Jerome M., William H. Flanigan, and Nancy H. Zingale. 2006. *Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840–1972.* [Computer file], Compiled by Jerome M. Clubb, William H. Flanigan, and Nancy H. Zingale, ICPSR08611-v1. Ann Arbor, MI: Inter-university Consortium for Political and Social Research [producer and distributor], 2006-11-13. doi:10.3886/ICPSR08611.v1.

Cohen, David K., and Susan L. Moffitt. 2009. *The Ordeal of Equality: Did Federal Regulation Fix the Schools?* Cambridge, MA: Harvard University Press.

Collins, William J., and Robert A. Margo. 2006. "Historical Perspectives on Racial Differences in Schooling in the United States." In *Handbook of the Economics of Education*, Vol. 1, edited by Eric Hanushek and Finis Welch, 107–54. Amsterdam: Elsevier.

Elementary and Secondary Education Act Amendments of 1969. 1970. Pub. L. No. 91–230, 84 Stat. 121, April 13.

Feldstein, Martin. 1978. "The Effect of a Differential Add-on Grant: Title I and Local Education Spending." *Journal of Human Resources* 13 (4): 443–58.

Florida State Superintendent of Public Instruction. 1963, 1965, 1967. *Biennial Report.* Superintendent of Public Instruction State of Florida. Tallahassee.

Florida State Commissioner of Education. 1969. *Biennial Report.* Commissioner of Education State of Florida. Tallahassee.

Florida State Commissioner of Education. 1970, 1971. *State of Florida Commissioner of Education Report.* Tallahassee.

Finkelstein, Amy. 2007. "The Aggregate Effects of Health Insurance: Evidence from the Introduction of Medicare." *Quarterly Journal of Economics* 122 (1): 1–37.

Fisher, Ronald C., and Leslie E. Papke. 2000. "Local Government Responses to Education Grants." *National Tax Journal* 53 (1): 153–68.

Georgia Department of Education. *1968–69 Georgia Public Schools Cost per ADA Child.*

Georgia State Department of Education. 1967, 1969. *Annual Reports of the Department of Education to the General Assembly of the State of Georgia.* Georgia State Department of Education. Atlanta, GA.

Georgia State Superintendent of Schools. 1963, 1965. *Report on Georgia Schools.* State Department of Education.

Gordon, Nora. 2004. "Do federal grants boost school spending? Evidence from Title I." *Journal of Public Economics* 88 (9–10): 1771–92.

Guryan, Jonathan. 2001. "Does Money Matter? Regression-Discontinuity Estimates from Education Finance Reform in Massachusetts." National Bureau Economic Research (NBER) Working Paper 8269.

Guryan, Jonathan. 2004. "Desegregation and Black Dropout Rates." *American Economic Review* 94 (4): 919–43.

Hanushek, Eric A. 1997. "Assessing the Effects of School Resources on Student Performance: An Update." *Educational Evaluation and Policy Analysis* 19 (2): 141–64.

Heuer, Ruth, and Stephanie Stullich. 2011. *Comparability of State and Local Expenditures Among Schools Within Districts: A Report From the Study of School-Level Expenditures.* U.S. Department of Education Office of Planning, Evaluation and Policy Development Policy, and Program Studies Service. Washington, DC, November.

Hines, James R., and Richard H. Thaler. 1995. "The Flypaper Effect." *Journal of Economic Perspectives* 9 (4): 217–26.

Historical Census Browser. 2004. University of Virginia, Geospatial and Statistical Data Center: http://mapserver.lib.virginia.edu (accessed April 12, 2012).

Hoxby, Caroline M. 2001. "All School Finance Equalizations are Not Created Equal." *Quarterly Journal of Economics* 116 (4): 1189–1231.

Hoynes, Hilary W., and Diane Whitmore Schanzenbach. 2009. "Consumption Responses to In-Kind Transfers: Evidence from the Introduction of the Food Stamp Program." *American Economic Journal: Applied Economics* 1 (4): 109–39.

Johnson, Rucker C. 2011. "Long-run Impacts of School Desegregation and School Quality on Adult Attainments." NBER Working Paper 16664.

Kaestle, Carl F. 2001. "Federal Aid to Education Since World War II: Purposes and Politics." In *The Future of the Federal Role in Elementary and Secondary Education*, edited by Jack Jennings, 13–36. Washington, DC: Center on Education Policy.

Knight, Brian. 2002. "Endogenous Federal Grants and Crowd-out of State Government Spending: Theory and Evidence from the Federal Highway Aid Program." *American Economic Review* 92 (1): 71–92.

Ludwig, Jens, and Douglas L. Miller. 2007. "Does Head Start Improve Children's Life Chances? Evidence from a Regression Discontinuity Design." *Quarterly Journal of Economics* 122 (1): 159–208.

Lutz, Byron. 2010. "Taxation with Representation: Intergovernmental Grants in a Plebiscite Democracy." *Review of Economics and Statistics* 92 (2): 316–32.

Lutz, Byron. 2011. "The End of Court-Ordered Desegregation." *American Economic Journal: Economic Policy* 3 (2): 130–68.

Margo, Robert A. 1990. *Race and Schooling in the South, 1880–1950: An Economic History*. Chicago: University of Chicago Press.

Martin, Ruby, and Phyllis McClure. 1969. *Title I of ESEA: Is it Helping Poor Children?* National Association for the Advancement of Colored People (NAACP). Washington, DC, December.

Minnesota Population Center. 2004. "National Historical Geographic Information System: Pre-release Version 0.1." Minneapolis: University of Minnesota. http://www.nhgis.org (accessed April 15, 2011).

Mississippi State Superintendent of Public Education. 1961, 1963, 1966, 1968. *Biennial report and recommendations of the State Superintendent of Public Education to the legislature for the scholastic year*. Mississippi State Department of Education. Jackson, MS.

Mississippi State Superintendent of Public Education. 1971. *Annual Report and Recommendations of the State Superintendent of Public Education to the legislature of Mississippi 1969–70*. State Department of Education of Mississippi. Jackson, MS.

Musgrave, Robert A. 1959. *The Theory of Public Finance: A Study in Political Economy*. New York: McGraw-Hill.

North Carolina Education Association. 1963–1968. *Per Pupil Expenditures for Current Expense: Information Provided by Division of Statistical Services State Department of Public Instruction*. North Carolina Education Association. Raleigh, NC.

North Carolina Department of Public Instruction. 1965–1970. *Current Expenditures by Source of Funds*. Raleigh, NC.

Oates, Wallace E. 1972. *Fiscal Federalism*. New York: Harcourt, Brace, Jovanovich.

Oates, Wallace E. 1999. "An Essay on Fiscal Federalism." *Journal of Economic Literature* 37 (3): 1120–49.

Papke, Leslie E. 2005. "The effects of spending on test pass rates: evidence from Michigan." *Journal of Public Economics* 89 (5–6): 821–39.

Papke, Leslie E. 2008. "The Effects of Changes in Michigan's School Finance System." *Public Finance Review* 36 (4): 456–74.

Puma, Michael J., Nancy Karweit, Cristofer Price, Anne Ricciuti, William Thompson, and Michael Vaden-Kiernan. 1997. *Prospects: Final Report on Student Outcomes*. U.S. Department of Education Planning and Evaluation Service. Washington, DC, April.

Reber, Sarah J. 2005. "Court-Ordered Desegregation: Successes and Failures Integrating American Schools since Brown versus Board of Education." *Journal of Human Resources* 40 (3): 559–90.

Reber, Sarah J. 2010. "School Desegregation and Educational Attainment for Blacks." *Journal of Human Resources* 45 (4): 893–914.

Reber, Sarah J. 2011. "From Separate and Unequal to Integrated and Equal? School Desegregation and School Finance in Louisiana." *Review of Economics and Statistics* 93 (2): 404–15.

Roy, Joydeep. 2011. "Impact of School Finance Reform on Resource Equalization and Academic Performance: Evidence from Michigan." *Education Finance and Policy* 6 (2): 137–67.

Roza, Marguerite. 2010. *Educational Economics: Where Do School Funds Go?* Washington, DC: Urban Institute Press.

Snyder, Thomas D., and Sally A. Dillow. 2011. *Digest of Education Statistics, 2010*. U.S. Department of Education National Center for Education Statistics Institute of Education Sciences. Washington, DC, April.

South Carolina State Department of Education. 1961–1967. *Annual Report of the State Superintendent of Education of the State of South Carolina*. South Carolina State Department of Education. Columbia, SC.

South Carolina Department of Education. 1968–1970. *Annual Report*. Superintendent of Education. Columbia, SC.

Southern Education Reporting Service. 1964. *A Statistical Summary, State By State, of School Segregation-Desegregation in the Southern and Border Area from 1954 to the Present*. Fourteenth revision. Southern Education Reporting Service. Nashville, November.

Southern Education Reporting Service. 1967. *A Statistical Summary, State By State, of School Segregation-Desegregation in the Southern and Border Area from 1954 to the Present*. Sixteenth revision. Southern Education Reporting Service. Nashville, February.

State Department of Education of Louisiana. 1961–1970. *Annual Report*. State Department of Public Education. Baton Rouge, LA.

Strumpf, Koleman S. 1998. "A predictive index for the flypaper effect." *Journal of Public Economics* 69 (3): 389–412.

Tennessee Department of Education. 1961–1970. *State of Tennessee Annual Statistical Report of the Department of Education.* Tennessee Department of Education. Nashville.

U.S. Commission on Civil Rights. 1968. *Political Participation: A Study of the Participation of Negroes in the Electoral and Political Processes in 10 Southern States Since Passage of the Voting Rights Act of 1965*. U.S. Commission on Civil Rights. Washington, DC, May.

U.S. Department of Commerce Bureau of the Census. 1963. *Census of Population: 1960 Volume I: Characteristics of the Population*. Washington, DC: U.S. Government Printing Office.

U.S. Department of Health, Education, and Welfare Office of Education. 1969. *History of Title I ESEA*. U.S. Department of Health, Education and Welfare. Washington, DC, June.

U.S. Department of Health, Education, and Welfare Office of Education. 1972. *Directory of Public Elementary and Secondary Schools in Selected Districts: Enrollment and Staff by Racial/Ethnic Group, Fall 1970*. Washington, DC: U.S. Government Printing Office.

U.S. Senate Committee on Labor and Public Welfare Subcommittee on Education. 1965. *Maximum Basic Grants—Elementary and Secondary Education Act of 1965* (*Public Law 81-874, Title II, and Public Law 89-10, Title I*). Washington, DC: U.S. Government Printing Office.

U.S. Senate Committee on Labor and Public Welfare Subcommittee on Education. 1967. *Notes and Working Papers Concerning the Administration of Programs Authorized Under Title I of Public Law 89-10, The Elementary and Secondary Education Act of 1965 as Amended by Public Law 89-750*. Washington, DC: U.S. Government Printing Office.

U.S. Senate Committee on Labor and Public Welfare Subcommittee on Education. 1970. *Allocation of Funds Under Title I of the Elementary and Secondary Education Act of 1965, as Amended, Fiscal Year 1970*. Washington, DC: U.S. Government Printing Office.

van der Klaauw, Wilbert. 2008. "Breaking the link between poverty and low student achievement: An evaluation of Title I." *Journal of Econometrics* 142 (2): 731–56.

Virginia State Board of Education. 1961–1970. *Annual Report of the Superintendent of Public Instruction of the Commonwealth of Virginia*. Virginia State Board of Education. Richmond, VA.