

Preliminary

**The War on Poverty and Educational Opportunity in the South\***

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**Abstract**

Title I of the 1965 Elementary and Secondary Education Act substantially increased federal aid to school districts, with the goal of improving educational opportunity for poor students. The South was a major beneficiary of the new program. Using newly-collected school finance data and special Census tabulations on educational attainment, we examine the ultimate impact of the program in the South in the years following its introduction. We use initial local child poverty rates, the primary determinant of the grant amount, to provide identifying variation while carefully controlling for other local characteristics, including racial composition and desegregation. We find that increases in federal funding over the second half of the 1960s translated into increases in educational spending of 56 cents on the dollar in the average Southern district – due to crowding out of local revenue that is both significantly positive and less than would be predicted based on the income elasticity alone – and raised spending nearly dollar for dollar in districts with the least scope for fiscal offset. Exploiting this differential fiscal response, we find that increases in school budgets from Title I decreased high school dropout rates for whites, but not for blacks.

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## **I. Introduction**

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, 1977). However, the intent of the granting government may be undermined if receiving jurisdictions crowd out intergovernmental grants by reducing their own effort, as emphasized in the fiscal federalism literature. Furthermore, even in the absence of substantial crowd out the receiving jurisdiction must spend additional funding effectively, which may be nontrivial in policy contexts where the production function for the outcome of ultimate interest is not fully understood.

Perhaps nowhere are these issues more salient than in the evolution of U.S. education finance over the past 50 years. Elementary and secondary education have long been provided by local school districts, but the importance of intergovernmental grants has increased over time as both states and the federal government have contributed a larger share of total revenue. A large empirical literature has explored how state school finance regimes and intergovernmental grants more generally have affected school spending (for example, Fisher and Papke, 2000; Hoxby, 2001). Overall per-pupil expenditure rose dramatically in recent decades and a separate empirical literature examines how school spending during this era has affected student outcomes (see Hanushek, 1986, 1997 for reviews). But few studies, to our knowledge, have examined both issues simultaneously or using variation from their policy origins.<sup>1</sup>

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. Title I ESEA<sup>2</sup> had a dramatic effect on federal education funding: During the fall of 1965, Congress doubled federal expenditure on education by

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<sup>1</sup> 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.

<sup>2</sup> The Elementary and Secondary Education Act of 1965 is formally known as PL 89-10, passed April 11, 1965.

appropriating almost \$1 billion to the new program, equivalent to \$7 billion in 2009. The program was intended to assist school districts in providing programs for “educationally disadvantaged” children, but limited enforcement efforts made the Title I grants much more fungible in practice, allowing them to take the form of general education aid (Martin and McClure, 1969).

We focus the present analysis on the impacts of Title I in the South. The South benefited disproportionately from the Title I funding formula, receiving per-pupil grants through the program that were large relative to existing school spending per student.<sup>3</sup> The relative magnitude of the federal education aid flowing to the region helps to ensure that we have enough precision to explore the impacts of Title I’s introduction on student outcomes even if it did not translate dollar-for-dollar into higher school expenditure. The ultimate fiscal and attainment impacts of Title I’s introduction are also particularly interesting for the South, where vast racial inequities in school resources and educational attainment developed after Reconstruction (Margo, 1990, Card and Krueger, 1992, 1996; Ashenfelter, Collins, and Yoon, 2006), as well as low education spending on Southern whites compared to whites elsewhere in the country. It was stark inequalities such as these that Title I sought to remedy.

We first examine whether the introduction of Title I raised school expenditures. Large state grants and minimum required local contributions for education in the South meant that the education spending decisions of some Southern districts were already constrained, potentially reducing their scope to offset Title I grants through reductions in local effort.<sup>4</sup> We might therefore expect Title I to have raised school spending in the South by more than the income elasticity of demand for education would imply. But we also run the risk overstating the expenditure effects of Title I in the Southern context, since the primary determinant of Title I allocations and the main source of variation available for identification—child poverty—may be positively correlated with

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<sup>3</sup> The change in federal funding from the early to late 1960s was more than a quarter of pre-program spending in the average Southern district.

<sup>4</sup> Baicker and Staiger (2005), Brooks and Phillips (2010), Lutz (2010), and Strumpf (1998) also analyze the importance of institutions, broadly construed, in constraining fiscal responses to intergovernmental grants.

unobserved increases in demand for school funding stemming from school desegregation in the latter half of the 1960s.<sup>5</sup> We minimize any such bias by controlling for initial district black enrollment share – which is positively correlated with both child poverty and with desegregated-induced increases in demand for school spending (Reber, 2011) – and several other pre-existing district observables previously found to predict the pace of desegregation in the South over the 1960s (Cascio, et al., 2008). We find no evidence for other alternative hypotheses for our findings in a series of robustness checks, and we also show that the timing of changes in Southern school finance are closely aligned with the introduction of Title I.

Using newly-collected archival data at the school district level and the child poverty rate implicit in Title I as a source of variation in the change in federal funds, we find that an additional dollar increase in federal revenue from Title I was associated with on average an additional 56 cent increase in per-pupil current expenditures in the average Southern district by the end of the 1960s. This translated into an increase in current expenditures in the six years following implementation of Title I amounting to \$234 more per pupil (in 2009 dollars) than otherwise expected. The crowd-out that occurred in response to receipt of Title I was in local, not state, revenues. Further, institutional factors unique to the South appear to have mediated the fiscal response to Title I: The program increased spending on education significantly more in districts that we expect to have been more constrained – those where the grant was large relative to pre-program local revenue.

We exploit this heterogeneity in fiscal response to estimate the effects of Title I-induced spending increases on changes in high school dropout rates of 18-19 year olds during the 1960s, constructed from county-level tabulations produced for us by the Census Bureau. Among districts with low scope for local offset, spending increased by more and white dropout rates fell faster over

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<sup>5</sup> The introduction of Title I also had a direct impact on desegregation, since receipt of these grants was initially contingent upon non-discrimination under the 1964 Civil Rights Act (CRA). Cascio, et al. (2010) show that Southern districts with more Title I funds at risk were more likely to just meet the (relatively minimal) desegregation requirements to comply with CRA. During the period of our main analysis, the fund-withholding provisions of CRA were not enforced, so there was no direct relationship between Title I funding and desegregation.

the 1960s in higher child poverty districts – which received larger Title I grants – relative to lower poverty districts, but there was no such relationship between child poverty rates and changes in dropout in districts with more scope for local offset. Exploiting the *difference* in the poverty gradients in expenditure across districts with less and more scope to crowd out Title I funds, we estimate that each additional \$100 of Title I induced annual current expenditure per pupil reduced white high school dropout rates by about 3 percentage points by the end of the 1960s.<sup>6</sup> Our estimates imply that Title I induced increases in school expenditure explain about half of the decline in the dropout rate of Southern white 18-19 year olds during the 1960s. The point estimates suggest small detrimental effects for blacks; we cannot rule out moderate beneficial effects for blacks, but we can reject effects as large as those found for whites.

Overall, our findings suggest that Title I increased school spending in South –more so in places where we would expect – and that this spending benefited whites more than blacks. The benefits for whites likely contributed to some regional convergence in educational attainment over the period.<sup>7</sup> But local control over the use of Title I funds may have limited their scope to improve outcomes for blacks, even as black and white schools combined at the end of the decade. It is reassuring that other results we present are consistent with blacks benefiting from desegregation-induced increases in school quality, as Reber (2009) argues was the case in Louisiana. Black dropout rates fell more in districts with higher black enrollment shares, where larger pre-existing black-white resource gaps were closed as schools desegregated.

The remainder of the paper is structured as follows. Section II provides background on the Title I program. Section III describes a simple framework for analyzing intergovernmental grants,

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<sup>6</sup> The relevant cohorts were exposed to the additional spending for about 4 years.

<sup>7</sup> Our calculations from the 1960 and 1970 Integrated Public-Use Microdata Samples (IPUMS) of the U.S. Census (Ruggles et al., 2010) imply that the high school dropout rate of white 18-19 year olds in the South fell by 11.8 percentage points in the 1960s (from 30.1 to 18.3 percent). Outside of the Southern and Border region, the decline in white 18-19 year old dropout rates was around 8.4 percentage points over the decade (from 19.4 to 11 percent).

incorporating important institutional features of Southern school finance. Section IV analyzes the fiscal effects of the program; Section V examines heterogeneity in these effects across districts. In Section VI, we assess the impact of Title I induced expenditure on high school dropout rates by race. Section VII concludes.

## **II. Title I of the Elementary and Secondary Education Act**

Title I funds were first allocated to school districts in the fall of 1965.<sup>8</sup> In keeping with its policy origins as part of the War on Poverty, the Title I formula relied heavily on child poverty counts in determining allocations to school districts. The formula at the program's start was straightforward. Eligible districts received a state-specific payment (the "state factor") per eligible ("formula count") child. The state factor was equal to 50 percent of statewide mean current expenditures, net of federal revenue, per pupil, lagged two years. Starting in fall 1967, variation in the state factor remained only for those states that spent above the national average; the allocation per eligible child in all other states was leveled up to this figure.<sup>9</sup> The formula count was largely determined by the number of 5 to 17 year olds with family income below \$2000 in the 1960 Census.<sup>10</sup> The South's high poverty rates thus translated into unusually high Title I allocations *per pupil*, despite lower state spending on education.

The influx of federal funds to the South from Title I was unprecedented. Figure I shows that the federal government was a negligible source of revenue for Southern school districts prior to passage of ESEA in 1965, with the limited federal funds largely disbursed through Aid to Federally Impacted Areas and the National Defense Education Act. As shown in the first column of Table I, in 1963 and 1964, federal revenue in the average Southern district in our sample only amounted to \$62 per pupil (in real 2009 dollars) – about three percent of per-pupil current expenditures. By the

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<sup>8</sup> See Bailey and Mosher (1968) for a comprehensive description of Title I administration in its early years.

<sup>9</sup> Because Title I was not fully funded after 1965, funding amounts per eligible child were ratably reduced by state-specific multiplicative constants. Table II gives values of the state factor in our sample.

<sup>10</sup> It is also included children with higher family incomes served by AFDC, neglected and delinquent children, and children in foster care. The vast majority of eligible children, particularly in the South, were determined by the Census counts, however.

end of the decade, per-pupil federal revenue had increased dramatically, by \$418 (column (2)), and had come to represent 17 percent of per-pupil expenditure in the average Southern district.

Policymakers were concerned from the start about whether Title I funding would translate into increased compensatory services for poor children. The time period we study here was characterized by weak enforcement of targeting. In the early years of ESEA, regulations specified that Title I funds should “supplement and not supplant” local funds and the Department of Health, Education, and Welfare conducted district-level audits, flagging inappropriate use of funds.<sup>11</sup> 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 regulations and legislative changes that have led to a system that is often criticized today as being overly restrictive and compliance-focused. Even today’s complex regulatory framework and serious enforcement efforts cannot prevent districts from appropriating Title I funds, either for private consumption or for other educational purposes.<sup>12</sup>

### **III. A Framework for Understanding Fiscal Responses to Title I Grants**

Our goal is to understand whether the introduction of the Title I program produced economically significant increases in school spending and if so, whether these increases in spending translated to improved educational attainment. Whether—and how much—federal grants translate into higher spending on education depends on how lower levels of government respond; do they crowd out federal revenue by reducing their own contributions to education spending? Since both local school boards and state governments could have responded to the influx of federal funding, we consider each level of government in turn.

#### *III.A. Local District Responses to Title I Grants*

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<sup>11</sup> The federal government did not ask districts to return misused funds before the 1970s, however.

<sup>12</sup> See, for example, Gordon (2004) on how maintenance of effort requirements do not prevent crowdout of local revenue and Roza (2010) on how comparability requirements do not ensure that Title I funds are concentrated in the poorest schools within districts.

We begin with a standard neoclassical model where a local school district (presumably the median voter) chooses the level of consumption of educational services and all other goods, including private consumption. Districts can reallocate consumption between the two goods by altering the tax rate. In general, Title I funds amounted to a restricted block grant, shifting out a district's original budget constraint horizontally by the amount of aid received. Thus, districts had to spend at least that much money on education, and Title I did not change the slope of the budget constraint.

To understand local responses to Title I in the South, we incorporate into the standard model two important institutional features of Southern state school finance systems during the 1960s: state grants to school districts and minimum required local contributions to education. All of the states in our sample distributed aid primarily through minimum foundation programs at this time.<sup>13</sup> Like Title I, these programs amounted to a restricted block grant from the perspective of school districts.<sup>14</sup> These grants were relatively large in the South, where the typical district received well over half its revenue from the state (Figure I and Table I), compared to about a third outside the South. In addition, school districts could not reduce local revenue below some minimum, since all states had either minimum local contributions for participation in the state finance programs or constitutionally mandated local contributions to education. Both of these factors – the magnitude of state education aid and minimum required local contributions to education – would have limited the scope for Southern school districts to crowd out Title I grants.

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<sup>13</sup> Not all of the programs were referred to as minimum foundation programs, but those that were not called minimum foundation programs had the same general structure described in this section. All of the states in our sample distributed the vast majority (typically upwards of 80 percent) of state aid through these programs throughout the period of study.

<sup>14</sup> This is in contrast to many modern state school finance regimes which employ various forms of matching and therefore change relative prices. Grants under state MFPs were not pure block grants in the sense that some district choices could affect the amount of the grant. For example, the grant typically depended on the experience distribution of teachers actually employed in the district. If a district chose to employ more experienced teachers, it would get a larger grant from the state, so the program did change the relative price of teacher experience compared to the pure local finance case. We abstract from these issues here, since they likely had small effects and in any case are unrelated to the Title I grant.



Figure II illustrates the choice problem for a Southern district. 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, including private consumption ( $Y$ ). BC2 accounts for state aid and the minimum local contribution. The budget constraint is shifted out to reflect the additional revenue from the state. A district must spend at least as much as the state grant plus the minimum local contribution, and the maximum spending on  $Y$  is  $I$  less the minimum local contribution. Thus, the dashed part of the budget constraint is inaccessible. The introduction of Title I causes a further parallel shift in the budget constraint to BC3 (the figure depicts a large Title I grant for illustrative purposes). Now the district must spend at least as much as previously required plus the Title I grant on education, so the dashed part of the new budget constraint is inaccessible.

As in the standard model, the Title I grant produces only an income effect. School districts would like to increase their spending on both  $E$  and  $Y$  according to the relevant income elasticities, and the increase in  $E$  corresponding to the change in consumption bundle from point B to point C will be less than the size of the grant. Fisher and Papke (2000) cite estimates for the income elasticity of demand for education spending ranging from 0.40 to 0.65. In our sample, we estimate that an income elasticity of 0.5 would correspond roughly to a 14 cent increase in current expenditure per pupil per additional dollar of Title I revenue per pupil.<sup>15</sup> A district will only increase  $E$  more than this if its optimal bundle is on the inaccessible (dashed) portion of the budget constraint, in which case, it would be forced to the corner (point C). Note that incorporating the

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<sup>15</sup> To calculate the change in current expenditure implied by the income elasticity of 0.5, we use county-level data on pre-program current expenditure, 1969 Title I grants, and median family income in 1960 and 1970. To get counterfactual 1969 current expenditure (without Title I), we assume that per-pupil current expenditure would have increased from pre-program levels according to the observed income growth and the assumed income elasticity. We then divide the total Title I grant by the number of families in the county and compare that to median family income to get the percent change in income associated with the grant. We then multiply that by the assumed income elasticity and the estimated counterfactual 1969 current expenditure to get the increase in current expenditure due to the income elasticity. We then divide that by the grant to get the expected increase in current expenditure per dollar of Title I grant, which is comparable to the TSLs coefficients.

state grant and minimum local contributions expands the inaccessible portion of the budget constraint, increasing the chances that some districts are constrained and increase  $E$  more than the income elasticity would imply. This effect is larger when either the state grant or the minimum local contribution is larger. In addition, this introduces the possibility that a district was already located at the corner, such that spending on  $E$  would increase one-for-one with the federal grant.<sup>16</sup>

The latter case is illustrated in the figure: the district at the corner before the implementation of Title I (point A), would like to move to point B on BC3, but is restricted to choose the new corner, C. The movement from A to B reflects the increase in  $E$  due to the income effect, and the movement from B to C represents the additional increase in  $E$  due to the restrictions imposed by the Title I grant, the state grant, and the required local contribution. In this extreme case, spending on education increases one-for-one with the Title I grant.

Much of the literature on intergovernmental grants rejects the neoclassical model because grants appear to increase spending on the targeted good more than would be implied by the income elasticity of demand—this is referred to as a “flypaper effect,” since the grant disproportionately (i.e. more than would be predicted by the income elasticity of demand) “sticks where it hits.” This “effect” is usually attributed either to some disconnect between the true preferences of voters and spending chosen by governments (e.g., due to “bureaucratic capture,” when a constituency for a specific program develops and is able to prevent voters from reducing spending), or to flawed mental accounting, where voters view spending out of local taxes differently than spending out of intergovernmental grants (see Hines and Thaler (1995) for a review). However, an alternative explanation is that jurisdictions with high demand for spending on a particular good are more likely to receive grants targeted to that good, in which case estimates of the effects of targeted grants on

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<sup>16</sup> Technically, this could happen in a model without the state grant or required minimum local contribution if a district were spending zero on educational services initially; this is not an interesting case when the government under consideration is a local school district.

spending on targeted goods will be biased upward, creating the illusion of a flypaper effect (Knight, 2002).

The framework presented above also suggests that if institutional constraints limit the scope for offset, targeted grants would generate larger increases in spending on the targeted good than predicted by the income elasticity even when assuming a rational and perfectly informed median voter. A sharp test of these assumptions would check whether districts that increased spending on education more than implied by the income elasticity were consuming at the corner. We cannot perform this test directly because in practice we do not have sufficient data to determine precisely where the corner is for each district; in addition, we do not know each district's income elasticity. Instead, we develop a proxy for each district's scope to offset federal funding by reducing local revenue and test for heterogeneous responses along that margin. We discuss this in greater detail in Section V.

### *III.B. State Responses to Title I Grants*

It is possible that state governments might also have reduced their effort in response to the influx of Title I funds, particularly if states have preferences about the distribution of total aid (state plus federal) to school districts. If states cared about the average level of total aid, they could have reduced the state contribution across the board. If they wished to keep the distribution of aid across districts constant, on the other hand, they could have differentially reduced aid to districts with relatively large Title I grants; that is, the state could have responded to the increased progressivity of federal aid by reducing the progressivity of state aid.

Our empirical strategy allows for identification only of the latter type of response – whether states changed how progressively they distributed funds.<sup>17</sup> Allocation of funding under minimum foundation programs typical of the South during this period was formula-based, and the poverty rate did not directly enter the funding formula in any state in our sample; thus, it might appear

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<sup>17</sup> Based on suggestive evidence discussed in the results section, we do not think states responded to Title I in an economically significant way through changes in aggregate education spending.

difficult for states to significantly change the progressivity of funding without undertaking a major reform. On the other hand, the formulas were complex, with inputs that may be correlated with poverty rates. And Reber (2011) shows that the state of Louisiana was able to redistribute state aid substantially in response to desegregation—directing additional funding to high black enrollment share districts—by subtly manipulating the parameters of the existing minimum foundation program. Ultimately, it is an empirical question to what extent states were able and willing to change the progressivity of their funding formulas in response to Title I.

#### IV. Fiscal Responses to Title I

##### IV.A. Methods and Identification

The framework presented above suggests that in general, increases in spending on education will not be one-for-one with the size of the Title I grant, but some districts may be constrained to increase spending more than the income elasticity of education demand would imply. In addition, some literature has found evidence of “flypaper effects” in other contexts. The overall effect of the introduction of Title I therefore depends both on to what degree districts were constrained to increase spending and the extent to which the funds “stuck” to educational spending even in the absence of such constraints. We turn to the data to assess this empirically. We begin in this section by estimating the average effect across districts and gain some insight into possible mechanisms by estimating heterogeneous fiscal responses in the next section.

To estimate the effect of Title I grants on current expenditure for the average district, we begin with the following model:

$$(1) \quad \Delta p p e x p c u r r_{d s} = \theta \Delta p p r e v f e d_{d s} + x_d' \beta + \gamma_s + \varepsilon_{d s},$$

where  $\Delta p p e x p c u r r_{d s}$  represents the change over time in per-pupil current expenditure for district  $d$  in state  $s$  and  $\Delta p p r e v f e d_{d s}$  is the change over time in district  $d$ 's per-pupil federal revenue. We focus on federal revenue overall, since Title I revenue is not observed for all states, and on current expenditure rather than total expenditure, since the latter also includes capital outlays and debt

service, which were not targeted by the Title I program.<sup>18</sup> Because the fiscal variables are in first differences, the coefficient vectors  $\gamma_s$  and  $\beta$  represent expected *trends* in current expenditure across states and across districts with different characteristics,  $x_d$ , respectively. Our estimation focuses on changes between 1963/64 and 1969 – enough time to capture complex state and local responses to the program’s introduction.<sup>19</sup> Table I shows descriptive statistics on the differences for each of the fiscal measures considered for districts in the nine Southern states of our estimation sample; all figures are in 2009 dollars; descriptive statistics on the control variables, described in more detail below, are given in Table II.<sup>20</sup>

The parameter  $\theta$  in equation (1) gives, in dollar-for-dollar terms, the partial relationship between changes in federal revenue and changes in current expenditure. As discussed, this parameter could be close to one, even in a neoclassical framework, in the presence of state aid and minimum local contributions. But it may also be the case that districts with larger increases in per-pupil federal funds might have experienced different changes in per-pupil current expenditure for other reasons. For example, districts with increasing unobserved demand for education may have been more likely to apply for and receive their Title I funds or may have pressured state and federal legislators to provide more funding through other channels. In this case, ordinary least squares (OLS) will yield upward-biased estimates of  $\theta$ .

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<sup>18</sup> Title I accounts for the lion’s share of federal funding of schools by the late 1960s. Current expenditure is also more consistently measured across states and over time and less subject to year-to-year variations.

<sup>19</sup> All years refer to the fall of the school year. We focus on this long-run change because it aligns well with our analysis below of trends in school dropout between spring 1960 and spring 1970. We average 1963 and 1964 to reduce noise. We use 1969 only for the end of the long-run change, since this is after Nixon took office and eased enforcement of the federal-funds-withholding provisions of the Civil Rights Act (Halpern, 1995; Orfield, 2000). Below, we show that our findings would be substantively unchanged if we had used annual data.

<sup>20</sup> These states are Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia; we are not able to incorporate districts from Arkansas and Texas – the two other former Confederate states – due to limitations in available data. Data were hand-entered from print publications. See Data Appendix and References in Cascio, et al. (2010). Our sample here differs from that described in that, to reduce noise, we exclude districts where any one of the fiscal outcomes lies outside 3.5 standard deviations from the state-specific mean. The sampled districts look very similar to all districts on observed dimensions (statistics available on request).

We address this identification problem using the child poverty rate implicit in the Title I formula as an instrument for the change in total federal revenue. While the Title I funding formula does not offer any non-linearities in poverty that would be useful for identification, the availability of extensive controls and high frequency observations in our district-level data enable several tests of the identifying assumption. For example, we can examine the timing of poverty-related changes in school expenditure closely, confirming that the child poverty rate does not predict changes in school funding prior to Title I and that the changes that we do observe are closely timed with the introduction of the program.

The first stage of our two-stage model is given by:

$$(2) \quad \Delta \text{prevfed}_{ds} = \pi_1 \text{child\_poverty60}_d + x_d' \pi_2 + \varphi_s + v_{ds}.$$

The parameter  $\pi_1$  characterizes how the change over time in per-pupil federal funds relates to the 1960 child poverty rate,  $\text{child\_poverty60}_d$ , within state and controlling for other district characteristics. To construct  $\text{child\_poverty60}_d$ , we divide the number of Title I eligible students—determined primarily based on the number of 5 to 17 year-olds with family income below \$2,000 in the 1960 Census—by a district’s 1960 enrollment. This measure captures the effective poverty rate for distribution of Title I funds, had district enrollment not diverged from its 1960 level. The average district in our sample had a child poverty rate of 32 percent, and poverty rates varied considerably across districts (Table II).<sup>21</sup> Two stage least squares (TSLS) estimates will identify  $\theta$  only if  $\text{child\_poverty60}_d$  is uncorrelated with unobserved determinants of trends in per-pupil current expenditure, represented in the error term in equation (1),  $\varepsilon_{ds}$ .

If Title I was the only federal education program distributing aid on the basis of child poverty, estimates of  $\pi_1$  should be close to the average value of the state factor in the Title I formula – the Title I grant that the average district received per eligible child – in our sample during

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<sup>21</sup> Title I eligibles at the district level (the numerator) were estimated using data from Congressional publications (see Cascio, et al. (2010)) and total enrollment in 1960 (the denominator) was drawn from same state publications as the finance variables.

the years of interest. As shown in Table II, the average state factor in 1969 was \$954.<sup>22</sup> Head Start funds could have been correlated with the 1960 child poverty rate, as that was also a program targeted to poor children, the 300 poorest counties received special grant-writing assistance (Ludwig and Miller, 2007), and some Head Start funds flowed through school districts. We therefore control for whether the school district is in one of these counties to avoid confounding fiscal responses to Title I and Head Start.<sup>23</sup> In practice, this has no effect on the estimates, consistent with the small amount of Head Start funding received by school districts.

The most important potential threat to validity is unobserved changes in demand for spending related to desegregation, which proceeded rapidly across the South at the end of the 1960s (Cascio, et al., 2008). Desegregation resulted in a substantial reorganization of schools and may have changed the demand for education spending, though it is *a priori* unclear which direction this would go.<sup>24</sup> On the one hand, dissatisfaction with public schools among whites, white flight to private schools, or an unwillingness of whites to support spending on black students might have reduced demand for school spending. On the other hand, at the start of the 1960s, the black schools in many areas were still significantly lower-quality than the white schools, despite a half-century of progress towards convergence. Black-white school resource gaps were larger in districts with higher black enrollment shares (Margo, 1990; Card and Krueger, 1992; Reber 2011), so maintaining *whites'* school quality would have required larger increases in funding in blacker districts, and we might expect demand for spending to increase more in such districts. Indeed, Reber (2011) shows that blacker districts saw substantially larger increases in funding during this period in Louisiana,

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<sup>22</sup> Data on the state factor in the Title I formula was drawn from county-level data published in a series of Congressional reports. See Data Appendix and References in Cascio, et al. (2010).

<sup>23</sup> We are grateful to Doug Miller and Jens Ludwig for providing us with these data.

<sup>24</sup> Initially, receipt of Title I funding was directly tied to compliance with Department of Health Education and Welfare Guidelines on desegregation, as the Civil Rights Act barred any entity that discriminated on the basis of race from receiving federal funds. Local district choices about desegregation and funding were therefore explicitly joint; however, Nixon did not enforce these provisions, so this does not present a problem for the period we study.

enough to bring resources available in desegregated schools up to the level previously experienced in the white schools.<sup>25</sup>

We have extensive information on the characteristics of school districts that we can incorporate into our model to limit bias. Regardless of which effect dominates, desegregation-related changes in demand for spending are likely related to the initial black enrollment share in the district. We therefore include this as a control, linearly in the baseline specification and more flexibly in robustness checks. Drawing from Cascio, et al. (2008), our baseline specification also controls for the share of the vote cast for Strom Thurmond in the 1948 presidential election, a proxy for pre-existing segregationist preferences, and for 1960 enrollment, a measure of district size, both of which predict the pace of desegregation in the South over the period of study.<sup>26</sup> In robustness checks, we also include controls for the share of non-blacks enrolled in private schools in 1969, the extent of school desegregation in 1970 (equivalent to the change over the period in light of the extremely low initial levels of desegregation), and an indicator for whether the district desegregated under court order. Further, we explore other alternative hypotheses for our findings, such as increases in black political power after the Voting Rights Act of 1965. Table II provides descriptive statistics on the controls that we incorporate into the analysis below. In general, our TSLS estimates are very stable across specifications.

#### *IV.B. Baseline Estimates of Expenditure Response*

Table III, Panel A presents OLS and TSLS estimates of equation (1) from our preferred specification, which includes state fixed effects, 1960 black enrollment share, the natural log of

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<sup>25</sup> On the other hand, whites in blacker districts would have seen their exposure to blacks increase more when schools desegregated, compared to whites in lower black enrollment share districts, and Reber (2011) shows that in Louisiana, white flight was more severe in blacker districts; Clotfelter (1976) finds similar results for Mississippi. If whites in such districts are more dissatisfied with the schools, and enough of them leave, then we might expect demand for spending to increase *less* in blacker districts. The evidence from Louisiana and presented below suggests this was not the case, probably because most whites remained in the public system.

<sup>26</sup> For most states in the sample, district enrollment by race is reported in the same annual state reports from which we draw the finance data. For North Carolina, we use data reported in Southern Education Reporting Service (1964, 1966). Thurmond vote share is from *Electoral Data for Counties in the United States: Presidential and Congressional Races, 1840-1974* (ICPSR Study No. 8611).



1960 enrollment, indicators for quintile of Thurmond vote share, and an indicator for being in one of the 300 poorest counties in 1960. In this table and throughout the remainder of the paper, standard errors are robust to heteroskedasticity and correlation of error terms within county.<sup>27</sup>

The OLS estimate, presented in column (1), suggests that an additional dollar of federal revenue per pupil was associated with a 76 cent increase in current expenditure per student from 1963/64 to 1969. The corresponding TSLS estimate, shown in column (2), implies that an additional dollar of federal revenue generated a 56 cent increase in current spending per pupil. The fact that the OLS estimates exceed the TSLS estimates suggests that districts experiencing increases in other categories of federal funding may have increased spending on schools regardless. The TSLS estimate of 0.56 is significantly less than one, but significantly larger than the income elasticity would imply, suggesting that at least some districts were either constrained by the institutional context to increase spending on education more than they would have liked given income and prices, or that the grant was subject to a flypaper effect.<sup>28</sup>

The first panel in Table IV shows the first stage regressions that underlie these TSLS estimates. The first-stage model is shown in column (1). As expected, the coefficient on the 1960 child poverty rate – \$942 (with a standard error of \$70) – is quite close to the average state factor in the Title I grant formula as of 1969, \$954, shown in Table II. This finding facilitates interpretation of our estimates as deriving from Title I rather than other federal education programs. The second panel of Table IV shows the reduced-form relationship between per-pupil current expenditure and the 1960 child poverty rate. The coefficient on the 1960 child poverty rate implies that current expenditure per-pupil increased by \$526 more in a district with only poor

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<sup>27</sup> We cluster standard errors on county because we use county-level information to predict district-level poverty rates, and because Thurmond vote share is measured at the county level.

<sup>28</sup> For example, if the income elasticity of demand for education spending is 0.5 (the middle of the range of such estimates discussed in Fisher and Papke, 2001), actual changes in median family income at the county level would predict increases of education spending of about 14 cents over this period.

children than in a district with no poor children; rescaling this coefficient by the first stage in Panel A produces the TSLS estimate of 0.56 ( $=\$526/\$942$ ) reported in Table III.

Column (2) of Table IV estimates the same model with pre-program changes in outcomes as the dependent variable. If the identifying assumption is satisfied, these “pre-trends” should be similar on average for districts with different poverty rates. For both federal revenue and current expenditure, the coefficients are much smaller in magnitude and not statistically significant, suggesting the main estimates do not simply reflect a continuation of pre-existing trends. We examine the timing of changes in even more detail using annual data below. Figure III shows these results graphically. The first two panels plot the changes in per-pupil federal revenue and current expenditure following Title I implementation against the 1960 poverty rate; the final panel shows the same thing for pre-program trends in per-pupil current expenditure.

For the purposes of comparison to existing literature, we also show the coefficient on black enrollment share. Consistent with the prediction that demand for funding increased more in blacker districts due to desegregation and with the results for Louisiana reported in Reber (2011), black enrollment share is positively related to changes in per-pupil current expenditure. As expected, the coefficients on the poverty rate would be significantly different than those presented if we failed to control for black enrollment share, given its positive correlation with the 1960 child poverty rate. However, once we control for black share linearly, doing so more flexibly or adding further controls to account for the potential confounding effects of desegregation does not substantively affect the TSLS estimates, as shown below.

As noted, we have focused on current expenditure in part because it is more consistently measured and less noisy than total expenditure, and in part because this is the category of spending targeted by Title I. The TSLS estimates for per-pupil total expenditure, presented in Panel B of Table IV, are noisier than those for per-pupil current expenditure in Panel A, but very similar in magnitude.

#### IV.C. *Baseline Estimates of Revenue Responses*

To the extent that Title I funds were used for consumption of goods other than school spending, which level of government was responsible? The next panels of Tables III and IV present OLS/TSLS and reduced-form estimates of model (1) with changes in per-pupil state and local revenues over 1963/64 to 1969 as dependent variables. The TSLS estimates for state revenue suggest that state governments did not respond to the introduction of Title I by making their funding formulas less progressive: the coefficients on the change in per-pupil federal revenue are small and not statistically significant (Panel C, Table III). Our research design does not allow us to identify the effects of Title I on *average* state aid, but what evidence there is suggests that state offset on this margin was likely minimal.<sup>29</sup> Local school districts did, however, reduce revenue by 27 cents for each federal dollar (Panel D, Table IV).

The estimates in column (2) of Table IV show no relationship between pre-program changes in per-pupil state revenue and the child poverty rate in the early 1960s. Per-pupil local revenue did decline more among poorer districts in the years prior to Title I implementation, but the magnitude of the coefficient is much smaller than in column (1), even accounting for the difference in time horizons in the pre- and post-program periods. To the extent that the estimated relationship between the change in per-pupil local revenue following Title I implementation and the poverty rate partially reflects a continuation of trend, we could be overstating the extent of local crowd-out. On the other hand, we do not see a pre-program trend in current expenditure, and if we include the pre-program change per-pupil local revenue as a control, the estimates become slightly *more negative*, indicating more local crowd-out, as shown below.

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<sup>29</sup> Two pieces of evidence suggest crowd-out on this margin may not have been important. First, as shown in Figure I, there is a strong *upward* trend in state revenue over the late 1960s, although of course this trend may have been even steeper in the absence of Title I. Second, if states that received more Title I funding responded by reducing *average* state funding, we might expect to see a negative relationship between the state-level poverty rate and average state funding. In fact, the state fixed effects in the model for changes in per-pupil state revenues—a measure of the average change in the state—are uncorrelated with the state poverty rate. Ultimately, we do not have a credible source of variation with which to identify the average state response, so we view this evidence as suggestive.

Taken together, the point estimates in Table III imply that each dollar of additional Title I revenue increased total revenue by 77 cents (1 dollar of federal revenue plus 4.3 cents of state revenue less 27 cents of local revenue), but increased total expenditures by only 55 cents. Each dollar of federal revenue thus increased the difference between total revenue and total expenditure by about 22 cents. Technically, this implies that districts used some of the new federal revenue to reduce debt or build up reserves.<sup>30</sup> It is possible that districts chose to “spend” Title I money this way, and this represents a form of local crowd-out. But it is also possible this difference simply reflects noise in the data, particularly the total expenditure variable. The difference is marginally statistically significant (Panel E, Table III).

While the estimates are not precise enough to account for every cent, those for the key outcomes—local revenue, state revenue, and current expenditure—have confidence intervals that allow us to make economically meaningful conclusions about the fiscal impact of federal grants. We can rule out substantial reallocation of state funds, and the confidence intervals for local revenue suggest local districts engaged in economically meaningful offset. The estimate for current expenditure (the targeted good) is significantly different from both zero and one, confirming that on average Title I funds did not increase the targeted category of school expenditures dollar for dollar, nor were they completely offset. We also reject that current expenditure increased by only as much as the income elasticity would imply (14 cents per dollar, according to our back-of-the-envelope estimate). The estimates imply that Title I accounted for approximately a quarter of the overall increase in per-pupil current expenditure over the period and 8 percent of per-pupil current expenditure at the end of the decade.

#### *IV.E. Robustness: Additional Controls*

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<sup>30</sup> In theory, total revenue should be similar to total expenditure, at least on average and over longer time periods. In practice, this is not always the case. Capital outlays (part of total expenditure) can generate substantial departures of total expenditure from total revenue in a given year. In addition, at least some states do not count proceeds from bond issues as “revenue,” while the capital improvements financed by bond issues are included in total expenditure; thus, revenue can be persistently lower than expenditure.

Table V shows that the TSLS coefficients of interest are remarkably insensitive to the inclusion of more controls or more flexible functional forms. The first column of the table repeats the TSLS estimates from our baseline specification shown in Table IV. Columns (2) through (4) show TSLS estimates under alternative specifications of 1960 enrollment and black share in a model that otherwise includes only state fixed effects: Column (2) includes (linear) black enrollment share and the natural log of 1960 enrollment; column (3) interacts both of those with state fixed effects, thus allowing the relationship between these variables and fiscal trends to differ across states; and column (4) is the same as (2) but includes indicators for deciles of black enrollment share in lieu of the linear term. The estimates are very similar to the baseline estimates.

The next columns in Table V add several sets of controls to our baseline specification to account for other coincident policy changes. Column (5) adds several direct measures of desegregation by 1970 –the dissimilarity index and an indicator for whether the district was desegregating under court supervision at that time<sup>31</sup> –as well as the share of the non-black population enrolled in private schools in 1969, which may be related to whites’ demand for spending on public schools.<sup>32</sup> Column (6) examines another alternative hypothesis – that increases in black voter registration rates in the South following enactment of the Voting Rights Act (VRA) of 1965 changed the median voter and demand for school spending. Here, we add a control for the black voter registration rate in the early 1960s – an indicator of how suppressed the black vote was before VRA.<sup>33</sup> Finally, estimates from a specification that controls for changes in federal spending on other programs—including Head Start, public assistance, retirement programs, and health

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<sup>31</sup> Controlling for variables measured after the introduction of Title I raises the possibility of reverse causation, as changes in financing could theoretically influence desegregation, so we do not include these variables in the preferred specification. In this historical context we do not perceive this is a likely or influential mechanism, however.

<sup>32</sup> Ideally, we would use the change in private enrollment, but these data are not available for 1959. Private enrollment rates were historically quite low in the South but increased somewhat during the period of desegregation.

<sup>33</sup> Voting-related changes in blacks’ political power are likely also related to the black share of the population, which is highly correlated with the black share of enrollment already included in the regression.

programs—at the county level are reported in Column (7).<sup>34</sup> Our estimates remain nearly identical when controlling for concurrent policy changes. Thus, while we cannot completely rule out the possibility that changes over the 1960s in both black political power and in the extent of desegregation confound our estimates, controlling for black share alone appears to have largely accounted for the effects of these other policy changes.

Our estimates for the pre-program outcomes generally suggest that higher and lower poverty districts were on similar fiscal trajectories before the implementation of Title I (Table IV), except for relatively small difference in local revenue trends; to address this issue, we include controls for pre-program changes in per-pupil local and state revenue (separately) in Column (8). The estimates are again quite similar to the baseline estimates.

#### *IV.F. Timing of Fiscal Responses*

We have shown that per-pupil current expenditure increased and per-pupil local revenue declined more in higher poverty districts following the introduction of Title I; this relationship is robust to a range of specifications. We can also use annual data to pin down the timing of changes in fiscal outcomes more precisely, showing that the observed changes in the relationship between the poverty rate and fiscal outcomes were closely timed with the introduction of Title I. For each of the fiscal outcomes, we estimate year-by-year regressions on *child\_poverty<sub>d</sub>* and the controls from the preferred specification. The coefficients on *child\_poverty<sub>d</sub>* are plotted in Figure IV. The dependent variables are in *levels*, not changes or differences, as above. Thus, changes in the coefficients on *child\_poverty<sub>d</sub>* over time indicate the extent to which funding became more or less progressive.

Panels A and B show that the poverty gradients in per-pupil federal revenue and per-pupil

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<sup>34</sup> Data underlying the variables described in this paragraph were drawn from the following sources. The dissimilarity index is calculated from school level data collected by the Office for Civil Rights (OCR); district level information provided in the same OCR survey provide information on whether the district was complying with the Civil Rights Act through a court order. See Data Appendices of Cascio, et al. (2008, 2010) for more details. We are grateful to James Alt for providing the voting data; see Alt (1995). TSpending on Head Start is from National Archives data generously provided by Jens Ludwig and Doug Miller. The data on spending on other programs comes for Regional Economic Information System and was generously provided by Doug Almond, Hilary Hoynes, and Diane Schanzenbach.

expenditure were unchanged in the early 1960s but increased sharply after Title I was implemented in 1965.<sup>35</sup> Panel C shows that, controlling for other characteristics, the relationship between the poverty rate and per-pupil state revenue was stable throughout the 1960s. There is evidence of a slight downward trend for per-pupil local revenue immediately before implementation of Title I, with a larger decline in the poverty-local-revenue gradient after 1965.<sup>36</sup>

The values of the coefficients themselves also illuminate how Title I transformed school finance in the South. Before Title I, poorer districts raised less revenue locally, as predicted, but also received more than rich districts through state grants; existing state funding formulas were redistributive. On net, however, a district with only poor children spent \$300-\$400 less per pupil than a district with no poor children in the early 1960s – a differential that was reinforced by regressive distribution of funds under existing federal programs. The distribution of federal funds became sharply progressive starting in 1965, and the gap in per-pupil expenditure between rich and poor districts began to close. By the late 1960s, initially poorer districts spent per student more on average, reversing the pattern seen earlier in the decade.

## **V. Heterogeneous Fiscal Responses to Title I Grants**

For the average Southern school district, each dollar of federal revenue distributed through Title I in the 1960s increased current expenditure by more than 50 cents – much more than what reasonable estimates of the income elasticity of demand for education would imply. Is this difference driven by the institutional context of large state block grants and minimum local contributions, or is it evidence of bureaucratic capture or flawed intuitive accounting? The model presented in Section III suggests that current expenditure would increase more (and local revenue decline less) in districts that would have liked to consume on the restricted portion of the budget

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<sup>35</sup> Some districts did not receive funding due to non-compliance with the Civil Rights Act, so it is not surprising that the coefficients are slightly smaller in 1965 and 1966.

<sup>36</sup> We do not think this reflects an anticipatory response to the program. ESEA was debated and implemented fairly quickly, and it is difficult to imagine districts understood precisely how they would be affected long before final passage of the legislation.

constraint after receiving Title I funds, but were forced to the corner by large state block grants and minimum local contributions (BC3 in Figure II).

In this section, we examine whether our aggregate estimate of Title I's fiscal impact is attributable to a non-trivial share of Southern districts with limited ability to offset the grants. We identify districts that are *more likely* to have been constrained to consume at the corner after Title I, given their prior local effort and expected Title I funds, and test whether increases in current expenditure were larger and local offset less pronounced among such districts. We also examine whether the response to Title I depended on the district's black enrollment share, which may be independently interesting in light of race relations in the South at the time. Black enrollment share is also correlated with the scope for local offset, so those results could be confounded if we do not take racial composition into account.

#### *V.A. Measuring the Scope for Local Offset*

To understand our measure of "scope for local offset," it is useful to return to Figure II. First, a district would have had lower ability to offset Title I if the Title I grant was larger, all else equal. Put differently, a larger Title I grant would have shifted out BC3 by more, increasing the probability that a district preferred to consume on its inaccessible portion. Second, districts with low initial local revenue would have had less scope to offset the Title I grant by lowering local revenue, all else equal. In part, this is because they would have been more likely to hit the minimum local contribution. In the extreme, districts initially at the corner –those at point A on BC2 – would not have been able to lower local revenue at all after Title I, as noted above. Low initial local revenue also implies low desired spending on education, all else constant.

Thus, the districts most likely to have been constrained were those that received large Title I grants relative to the local revenue *they would have raised in the absence of the program* in the same year. We cannot observe this counterfactual local revenue, but it is likely highly correlated with local revenue prior to Title I. We therefore use the ratio of the district's 1969 Title I to pre-



program local revenue as a proxy for the scope for local offset. This proxy orders districts by their scope for offset but has no natural scale, so we group districts by quartiles. The results are somewhat noisy but suggest that the top quartile is different from the rest of districts; we therefore focus on this division and refer to the top quartile as “low scope for offset” districts. Because low scope for offset districts received larger Title I grants, these districts were also poorer on average, but there is significant variation in the poverty rate within each category. If this were not the case, we would not be able to estimate effects separately for the two groups with any precision.

*V.B Heterogeneous Responses: Results*

Table VI presents the results on heterogeneous responses for the changes in per-pupil current expenditure, state revenue, and local revenue between 1963/64 and 1969. In panel A, the change in per-pupil federal revenue is interacted with an indicator for being a low scope for offset district, and the excluded instruments are the poverty rate and the poverty rate interacted with the low scope for offset indicator. The direct effect of low scope for offset and the other controls in Table III are also included. To improve precision and facilitate hypothesis testing, we estimated a pooled model including interactions of the low scope for offset indicator only with key controls (black share in enrollment, district enrollment, and state dummies), but the results are similar if we simply split the sample and estimate the regressions separately for high and low scope for offset.

The standard errors are moderately large, but the results suggest that current expenditure increased significantly more in low scope for offset districts – 94 cents for each dollar of federal revenue, compared to 38 cents for districts without low scope for offset (column (1)). There are no significant differences across these two groups of districts in state revenue responses (column (2)), though the standard errors do not allow us to rule out economically interesting effects. The results for local revenue (column (3)) suggest that the quarter of districts with low scope for offset do not reduce local revenue at all in response to federal funding, while the remaining districts reduce local revenue by 43 cents for each dollar of federal revenue. The coefficients actually suggest crowd *in*

among low scope for offset districts, but we cannot reject zero local revenue response for this group.

If other district characteristics are correlated with low scope for offset, and those characteristics also influence the response to Title I, our estimates of heterogeneity by scope for offset could be biased. In particular, blacker districts are more likely to have low scope for offset, since they received larger Title I grants and had lower initial local revenue. Unlike with scope for offset, where we have a prediction based on the framework, it is not obvious *ex ante* how black enrollment share might affect the response to Title I: perhaps voters in blacker districts were less averse to spending on education out of federal dollars than local taxes – “flawed mental accounting” – or the income elasticity of demand might have depended on racial composition.

To examine this, we estimate models allowing for heterogeneous responses by black share alone as well as models allowing heterogeneity by black share and scope for offset simultaneously. Panel B shows estimates analogous to those in Panel A, but stratifying districts by black share. Again, we divided the sample into quartiles, and the top quartile appeared to be most different from the rest of the distribution, so we define districts in the top quartile (corresponding to approximately 50 percent) as “high black enrollment share.”<sup>37</sup> The results suggest that current expenditure increased by about twice as much per dollar of federal revenue in high black enrollment share districts; this is due to lower local offset.

Panel C presents estimates of the specification with both interactions include to determine whether racial composition or scope for offset is responsible for the differential response. The estimates are somewhat imprecise, but the coefficients on the low scope for offset interaction are quite similar to those in Panel A, when the interaction with high black share is excluded, and maintain significance at the 5 percent level. The coefficients on the interactions with high black share are a little more than half as large as in Panel B and not significant.

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<sup>37</sup> Results are similar but less stark if we define high black enrollment share as above the median.

While we cannot make definitive statements in light of our standard errors, we take the fact that the scope for offset interaction coefficients are relatively stable across specifications, combined with the theoretical prediction that scope for offset should matter for fiscal responses, as suggestive evidence that scope for offset, and not racial composition, was an important mediator of fiscal responses to Title I. The direction of the heterogeneous response is consistent with institutional constraints' limiting local revenue responses, but it is also possible that districts we characterize as "low scope for offset" were not legally constrained to the corner solution, but rather were disproportionately subject to bureaucratic capture or flawed mental accounting. The point estimates can be squared with a neoclassical model with no flypaper effects only if a non-trivial share of districts faced significant institutional constraints on reducing local revenue.<sup>38</sup>

Regardless of the mechanism, however, the fact that Title I increased spending more in districts we characterize as having low scope for offset provides a useful source of variation for estimating the effects of Title I-induced increases in spending, to which we now turn.

## **VI. Title I and Educational Outcomes**

Did the increases in school spending that resulted from the introduction of Title I improve student outcomes? Educators have been skeptical of Title I's ability to improve outcomes, criticizing it as a "funding stream" rather than a well-defined and intensive intervention (see Cohen and Moffitt, 2009). But we have shown that the introduction of Title I did increase current educational spending, which could have improved outcomes for at least some students within a

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<sup>38</sup> Our measure of scope for offset does not have a natural scale, but the point estimates indicate one-for-one increases in spending with federal revenue and no offset for low scope for offset districts, implying that a quarter of districts would have consumed at the corner both with and without the Title I grant in the context of the neoclassical model. And among the remaining districts, the increase in spending is more than even an income elasticity of demand for education spending of one would predict. In some states, we have more detailed breakdowns of local revenue, and based on the limited data we have, there do not appear to be districts that were raising only the required minimum contribution for participation in the state minimum foundation program. However, we cannot be sure that this is the actual required minimum contribution overall because we do not know the precise school finance rules in each state.

district, regardless of whether they were specifically targeted by Title I services.<sup>39</sup> As such, this analysis not only contributes to the literature on the effects of Title I, but also to the extensive (but not always well-identified) literature on the effects of school spending on student outcomes more generally (see Hanushek (1997) for a review).

We exploit the heterogeneity of fiscal response documented above to estimate the effects of Title I-induced spending on educational outcomes. Some districts in the South had less scope to reduce local revenue in response to Title I and therefore increased spending more. If “money mattered,” it then should be the case that Title I’s introduction improved student outcomes more in districts that were less able to crowd out the new federal funding due to institutional constraints. We begin by testing this prediction using a newly-created dataset on age-specific dropout rates. We then exploit the heterogeneity of fiscal responses across districts to construct an instrumental variables estimate of the effect of school spending on dropout rates.

#### *VI.A. Reduced-Form Effects of Child Poverty*

To confirm the first prediction, we examine how changes in high school dropout rates between 1960 and 1970 related to the 1960 child poverty rate; if Title-I-related money mattered, we should see larger reductions in dropout rates in places with lower scope for local offset. Dropout rates are measured at the county level and were constructed from county-level tabulations of 1960 and 1970 census data produced for us by the Census Bureau; we focus our attention on 18 to 19 year-olds, where the estimates are relatively precise.<sup>40</sup> Unlike test scores, dropout rates are a measure of school output that can be consistently observed at a local level both before and after

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<sup>39</sup> 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. Identifying these effects is difficult, given the negative selection of participants by design. However, analyses of participation are also incomplete, since Title I may not provide much in the way of additional services to participants (Puma et al. (1997) describe Title I as “an add-on program operating at the margins” having “limited instructional focus”), and Title I designated spending may have impacts on students who are not disadvantaged.

<sup>40</sup> These tabulations give counts of total population and of population not enrolled in school *and* a highest grade completed less than grade twelve, by county, race, single year of age (15 to 19), and census year. Counts were constructed by the Census Bureau from census sample detail files.

Title I. Dropout rates are also an important outcome for this period, particularly in the South, where 30 percent of whites and 43 percent of blacks aged 18 and 19 were high school dropouts in 1960.

Before turning to the estimates for dropout rates, we confirm in the first column of Table VII that the results for the effects of Title I funding on per-pupil current expenditure are similar when estimated at the county, rather than district, level.<sup>41</sup> We restrict attention to counties where districts in our estimation sample represent at least 90 percent of total county enrollment in 1960; county aggregates were generated from the district data weighting by 1960 district enrollment.<sup>42</sup> We present separate models by race, weighting by race-specific county population of 18-19 year olds in 1960, so that the estimates represent the effect of Title I for the average student; we also include the 1960 race-specific dropout rate of 18-19 year olds as a control to account for the possibility that the trend in dropout rate depended on the starting point.<sup>43</sup> The specification otherwise includes the same controls as that in the first panel of Table VI.

Consistent with our earlier findings, Title I had a much greater impact on school spending for students residing in counties with lower scope for local offset. For whites (Panel A), among *high scope for offset* counties, those with only poor children in 1960 experienced on average a marginally significant \$372 greater increase in spending by fall 1969 compared to those with no poor children in 1960; among *low scope for offset* counties, this figure amounted to \$1,570: much more of the federal funding gains translated into spending.<sup>44</sup> The \$1,198 difference in poverty gradients across groups is highly statistically significant. The estimates for blacks, in the first column of Panel B, are

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<sup>41</sup> We could have alternatively looked at the 1960/61 to 1969 difference in per pupil expenditure to match the period over which the difference in dropout rates is measured. Doing so yields substantively similar findings. We focus on the 1963/64 to 1969 difference because it matches the analysis presented above.

<sup>42</sup> There are 820 districts represented in the 638 counties in the sample. In cases where not all districts within a county have low scope for offset, we coded the county as not having low scope for offset; though all discussion of impact on dropout refers to counties, recall that the relevant fiscal decisions are made at and aggregated from the district level. The estimates are qualitatively similar if we define low scope for offset at the county level using the 1960 enrollment weighted fraction of districts so classified.

<sup>43</sup> We unfortunately cannot examine trends in dropout at the county level during the 1950s.

<sup>44</sup> The county-level TSLS estimates are also similar to those reported in Table VI at the district level.

substantively similar. The first columns of Figures Va and Vb, which plot the regression-adjusted relationship between 1960 poverty rate and changes in per-pupil expenditure by race and scope for offset, show the differences across groups graphically.

The second column of Table VII presents the corresponding reduced-form estimates for the change in the dropout rate over the 1960s; the second columns of Figures Va and Vb provide a graphical representation. Among counties with relatively high scope for offset, regardless of race, 18 and 19 year olds in poorer counties saw no greater reduction in dropout over the 1960s than their counterparts in richer counties. Coefficients on the 1960 poverty rate are actually slightly positive though not statistically significant, at 4.76 (3.85) for whites (Panel A) and 5.09 (4.81) for blacks (Panel B). We may have expected to these coefficients to be negative, given the positive relationship between 1960 child poverty rates and spending trends in column (1). However, initial child poverty may have been related to trends in dropout through channels besides Title I-induced increases in school funding. It is for this reason that we focus on the *difference* in poverty gradients across low and high offset counties, thereby allowing poverty to have directly affected dropout trends.

Indeed, for whites, the reduction in high school dropout over the 1960s was strongly negatively related to 1960 child poverty in low scope for offset counties. As shown in the second column of Panel A, the coefficient on the interaction between the 1960 child poverty rate and the low scope for offset indicator is negative, larger in magnitude than the direct effect of poverty, and statistically significant (-37.32 (11.69)). Thus, the relatively large increases in expenditure on whites in low offset counties were accompanied by relatively large reductions in their dropout rates. This was not the case for blacks, however: the coefficient on the interaction term for blacks is positive and statistically insignificant (12.47 (10.13)) (Panel B). These reduced-form estimates imply that an additional \$100 increase in per-pupil current expenditure over the second half of the

1960s corresponded to a 3.1 percentage point reduction in dropout for whites ( $=(-37.32/\$1,198)*100$ ) and a 1.2 percentage point increase in dropout for blacks ( $=(12.47/\$1,039)*100$ ).

#### VI.B. *TSLS Estimates of the Effect of Current Expenditure*

Table VIII presents the TSLS estimates of the effect of current expenditure on high school dropout by race, where we instrument for the change in per-pupil current expenditure with the *interaction* of 1960 child poverty with low scope for offset. TSLS estimation is useful not only for establishing magnitudes, but also for making transparent the assumptions under which we identify a causal effect of school funding on student outcomes. By using the *interaction* between the 1960 child poverty rate and the low scope for offset indicator as the excluded instrument, we can allow the 1960 child poverty rate to have an effect on dropout through channels other than educational expenditure. Identification does *not*, therefore, rely in on the assumption that unobserved determinants of dropout trends be uncorrelated with the 1960 child poverty rate; rather, it relies on the weaker assumption that unobserved determinants of dropout trends be correlated with 1960 child poverty *in the same way* across counties with high and low scope for offset.

The second column of Table VIII presents the TSLS estimates that correspond to the estimates given in Table VI.<sup>45</sup> As anticipated, the TSLS estimates imply that each additional \$100 increase in current expenditure by 1969 was associated with a statistically significant 3.1 percentage point reduction in the likelihood of white dropout and an insignificant 1.2 percentage point increase in the likelihood of black dropout over the 1960s. OLS estimates from the same specification, presented in column (1), are much smaller in magnitude than TSLS, and for whites, are positive and statistically significant, though small. The discrepancy between the OLS and TSLS estimates suggests that school spending was rising relatively more in places where white (black) dropout rates were falling relatively less (more) dramatically over the decade for unobserved reasons.

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<sup>45</sup> Column (1) of Table VII represents the first stage, and column (2) of Table VII represents the reduced form of the two-stage model in Table VIII, column (2).

The point estimates suggest that whites benefited from Title I-induced changes in educational expenditure, but blacks did not. The standard errors on the estimates allow us to reject that the effects are the same for the two groups, but the estimates are sufficiently imprecise that we cannot reject moderate beneficial effects for blacks. It is also worth noting the coefficient on 1960 fraction black. Recall that, separate from the effects of the introduction of Title I, spending increased more in counties with higher black enrollment shares (Table IV, Panel B), possibly due to desegregation. Consistent with Reber (2009), Table VIII, Panel B shows that there was a corresponding disproportionate reduction in dropout rates among blacks in blacker counties; and the relationship between black enrollment share and the change in dropout did not depend on the scope for offset. For whites, the coefficient on black enrollment share is positive, imprecisely estimated, and more sensitive to specification. We return to this issue in the discussion of magnitudes and interpretation of the results below.

Columns (3) and (4) of Table VIII test the robustness of our findings to additional controls not included in our baseline specification. In column (3), we include changes in transfer payments to the county over the 1960s through other federal programs, including Head Start and Medicaid. In column (4), we also add county aggregates of the two measures of desegregation as of 1970 introduced above and of the 1969 nonblack private school enrollment rate.<sup>46</sup> In practice, these variables are not significantly correlated with either trends in dropout rates or the instrument, so their addition to the model affects TSLS point estimates little but raises their standard errors.

A more compelling exploration of identification comes from estimation of the same TSLS model for trends in high school dropout for older individuals – namely, those whose secondary education would have been completed prior to 1965. Presumably, unobserved shocks to trends in educational attainment – through changes in economic conditions or migration, for example –

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<sup>46</sup> Note that we lose some counties when we add these controls, which are missing for some districts in our estimation sample. As above, a county is included in the analysis if at least 90 percent of 1960 county enrollment is represented in the district level data.



would have affected these individuals as well. If the identifying assumption is satisfied, we would therefore expect to see no significant difference across high and low offset counties in the poverty gradient in dropout trends for this older group. We conduct a preliminary version of this test using 1960 and 1970 county-level data on the percent of the overall population aged 25 and older without a high school degree. The resulting TSLS estimate is slightly positive and not statistically significant (results not shown).<sup>47</sup>

*VI.C. Interpreting the Magnitudes of the Effects on High School Dropout*

Our estimates suggest that among whites, an additional \$100 change in per-pupil current expenditure reduced dropout rates by an additional 3.1 percentage points. The change in spending was permanent, so these estimates should not be compared to programs involving one-time expenditures. The cohorts in our analysis were exposed to about 5 years of Title I-induced spending increases before we observe their dropout outcomes in 1969. Title I grants were slightly smaller in the early years (and some districts did not receive their grants due to non-compliance with the Civil Rights Act), so the 3.1 percentage point decline in dropout corresponds to exposure to somewhat less than \$500 of additional cumulative spending between 1965 and 1969.

It is difficult to compare our estimates of the effects of school spending on educational outcomes. With the exception of Reber (2009), most existing studies that measure outcomes with educational attainment – studies which, like this one, focus on individuals schooled before 1970 – measure school inputs directly (i.e., with pupil-teacher ratios or teacher salaries) rather than with educational expenditure (Card and Krueger, 1992a, 1992b, 1996; Ashenfelter, Collins, and Yoon, 2006). Such studies find positive effects of measured inputs on educational attainment and wages, but it is difficult to compare the magnitudes to our estimates. On the other hand, existing work

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<sup>47</sup> Data are from the City and County Data Book. We will ultimately conduct a stronger test by using high school dropout rates of the population aged 25 and over by race, which we can construct from published Census data once hand-entered.

estimating the effects of educational spending tends to use test scores as the educational outcome of interest (Hanushek, 1997).

We therefore consider what our estimates imply about Title I's contribution to the substantial reductions in high school dropout over the 1960s. The average white 18 or 19 year old saw a \$148 increase in current expenditure related to the introduction of Title I by the late 1960s, implying a 4.6 ( $=\$148 \times -3.1$ ) percentage point reduction in the probability of dropout. Spending increases stemming from Title I thus account for roughly 45 percent of the 10 percentage point reduction in white high school dropout rates over the 1960s. The high school dropout rate of black 18-19 year olds fell even more than that for whites – by 14 percentage points – in the 1960s. Our point estimates for black dropout are insignificant and positive. We therefore use the (negative) lower bound on the 95 percent confidence interval to calculate the *most* Title I-induced spending increases could account for: 23 percent of the overall trend.<sup>48</sup>

The differential role of Title I in explaining black and white dropout trends over this period is unsurprising given increases in school quality for blacks related to desegregation. Districts with higher black enrollment shares in 1960 saw larger increases in per-pupil current expenditure over the second half of the 1960s (Table III); Reber (2011) argues that this additional funding in blacker districts was needed to “level up” quality in desegregated schools to that previously available only in the white schools. Under the (relatively strong) assumption that the effects of initial black enrollment share on black dropout worked only through such desegregation-related increases in spending,<sup>49</sup> our estimates imply a 3.35 percentage point greater reduction in black dropout for each

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<sup>48</sup> This calculation uses the lower bound on the 95 percent confidence interval on the TSLS estimate for blacks presented in Table VII, column (2) (-1.27). For the average black, an additional federal dollar raised current expenditure by 63 cents. Given a \$404 dollar increase in federal revenue for the average black, this implies \$255 more spending as a result of Title I, and a 3.24 percentage point reduction in the black dropout rate ( $=2.55 \times -1.27$ ).

<sup>49</sup> Potential exposure of blacks to whites after desegregation is the other mechanism through which initial black enrollment share may have mattered for black outcomes (Reber, 2009).

additional \$100 increase in per-pupil current expenditure growth.<sup>50</sup> Increases in school spending through this alternative channel would, in turn, account for about 45 percent (6.2 percentage points) of the overall decline in dropout among blacks during the 1960s.

Taken together, the results suggest that Title I-induced increases in spending benefited whites but not blacks, while desegregation-related changes in school quality benefited blacks (but not whites). It is worth noting that we cannot rule out moderate beneficial effects of Title I for blacks based on the 95 percent confidence intervals, but still, the estimates for whites are clearly larger. We observe changes in funding only at the *district* level, but it may be the case that what these funding changes imply about school quality experienced by different types of students depends on the source of additional funding. It is possible, for example, that within districts, Title I-induced spending increases were spent in ways that favored whites, which seems possible in light of weak enforcement of even nominal targeting of Title I funding to the most disadvantaged students within districts was weak (Martin and McClure, 1969).

Although we find independent effects of poverty (operating through Title I) and fraction black on funding, there is significant correlation between the two, and it would seem odd that the same districts would target Title I-induced funding to whites and desegregation-induced funding to blacks. However, the differential increases in spending related to black enrollment share may not reflect additional expenditure targeted to blacks *per se*, but instead reflect the differential changes in school quality experienced by blacks in different districts as schools desegregated. Before desegregation, the black enrollment share was positively related to black-white school-resource gaps; thus, blacks in heavily black districts would see bigger increases in school resources when

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<sup>50</sup> For this calculation, we use the coefficient on 1960 black enrollment share in Table VII, Panel B column (2) (-14.73) and the coefficient on 1960 black enrollment share in a regression of the long difference in per pupil expenditure that weights by the 1960 population of black 18-19 year olds and includes their 1960 dropout rate as a control (\$439.6). While the magnitudes of the black and white estimates, we hesitate to directly compare them since the point-in-time spending increases have different origins and thus may represent different increases in resources over the school career. Nevertheless, it is useful to note that these estimates are broadly consistent with those presented in Reber (2009) for Louisiana.

black and white schools combined, compared to blacks in whiter districts. (Conversely, whites in blacker districts would have seen declines in resources relative to their counterparts in whiter districts.) If resources matter, the coefficient on fraction black would show a negative association with the change in high school dropout for blacks (and conversely for whites), *even if additional resources did not flow disproportionately to blacker districts*. In fact, our estimates indicate that spending increased more in blacker districts. Reber (2011) finds something similar in her analysis of Louisiana, and based on more detailed data than are available for all states in our sample, argues that this differential increase in spending for blacker districts was about what was needed to “hold harmless” whites. In that case, we would expect to see an even larger differential decline in dropout among blacks in blacker districts, but weakly positive or no relationship between black enrollment share and dropout among whites. This is consistent with what we find. To the extent that the coefficient on fraction black in the spending regression represents the additional money needed in blacker districts to hold harmless whites, it is an indication of how pre-existing black-white school resource gaps varied with black enrollment share and therefore how desegregation-related improvements in resources varied with black enrollment share.

We do not have data (or a research design) to test this hypothesis directly. It is also possible that the relationships between black enrollment share and changes in spending and dropout rates are not related to desegregation (or each other), so this explanation is necessarily speculative. The fiscal response to desegregation might also have been different in the absence of the influx of Title I funds. For example, districts and states may have chosen to increase funding less in response to desegregation in the absence of Title I, though again, we cannot evaluate this empirically. If this was the case, Southern blacks, particularly those in blacker districts, benefited indirectly from Title I.

## **VII. Conclusion**

Our analysis indicates that school districts did offset part of Title I grants by collecting less local revenue than they would have in the program’s absence, but spending increased substantially

nonetheless. On average, current expenditure rose by \$0.56 for each dollar of Title I funding. In districts with relatively low scope for local offset, those with large grants relative to local revenue, Title I grants increased current expenditure nearly dollar-for-dollar. Although the return to each federal dollar, in terms of educational outcomes, would have been greater in the absence of the observed crowd-out, even in its presence the program was an important contributor to the substantial decline in high school dropout rates during the 1960s among whites, but not for blacks. The results show that “money mattered” for educational attainment during this period, though it is worth noting that baseline spending was substantially lower than it is today.

Our findings are in contrast to the existing literature on Title I, which has concluded 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 non-participants (as in Carter, 1984; Puma et al., 1997). In addition to facing difficult selection programs due to the targeting of services to the most disadvantaged, existing research on Title I does not account for the possibility that participating students would have received at least some of the services in the absence of the program –another form of crowd-out – or that non-participating students may also benefit, either due to non-compensatory uses of program funds or to positive spillovers from appropriately targeted services. While Title I was more revenue than a “program” during our period of study – and schools and districts were relatively unconstrained in how they spent the funds – than is the case today, understanding the effects of Title I for all students today remains an important consideration.

Highly publicized instances of crowding out during this historical episode drew strong reactions and led to an increasingly complex set of regulations regarding the use of Title I funds. These regulations are intended to improve targeting but may have the unintended consequence of inefficiently constraining how local governments allocate funds across programs and inputs,

particularly in the context of the many other categorical programs and corresponding compliance requirements that have emerged since Title I's inception. Furthermore, the benefits of policies designed to prevent offsetting of funds, such as maintenance of effort requirements in Title I and many other government programs today, may be limited -- governments are required to maintain effort at past levels or some share of them, but the counterfactual throughout most of Title I's history was increasing rather than stable local revenue collections. In a context in which programs are sufficiently cost-effective to "cover" the cost of crowd out, the limitations of policies designed to fight it suggest that it may be best viewed as a cost of doing business. Our results suggest that where local institutions reduce the scope for offset, the trade-off between intensively regulating to attempt to prevent expropriation of funds versus more freely allowing local policies to reflect unique preferences and policy contexts is less severe. While our analysis cannot solve the difficult problem of designing and regulating intergovernmental grants optimally, that the results point to the importance of considering actual expenditure levels, rather than revenue streams, when conducting program evaluations.

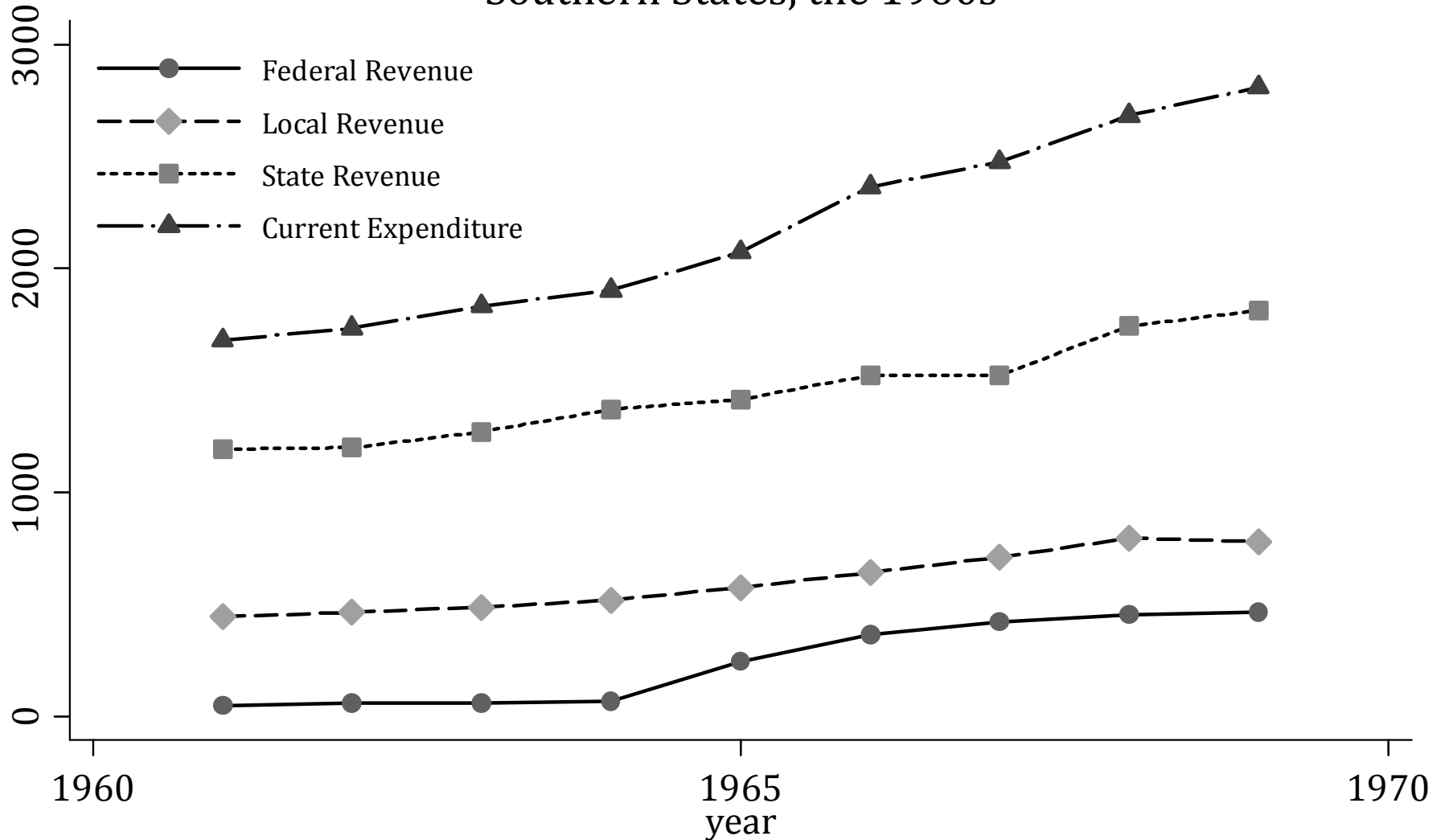
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Figure I. Trends in Per-Pupil Expenditure and Revenue by Source:  
Southern States, the 1960s



Note: All figures are in 2009 dollars. Unweighted means were calculated from our district-level sample from 9 Southern states: AL, FL, GA, LA, MS, NC, SC, TN, and VA.

Figure II. Local Government Responses to Title I Grant

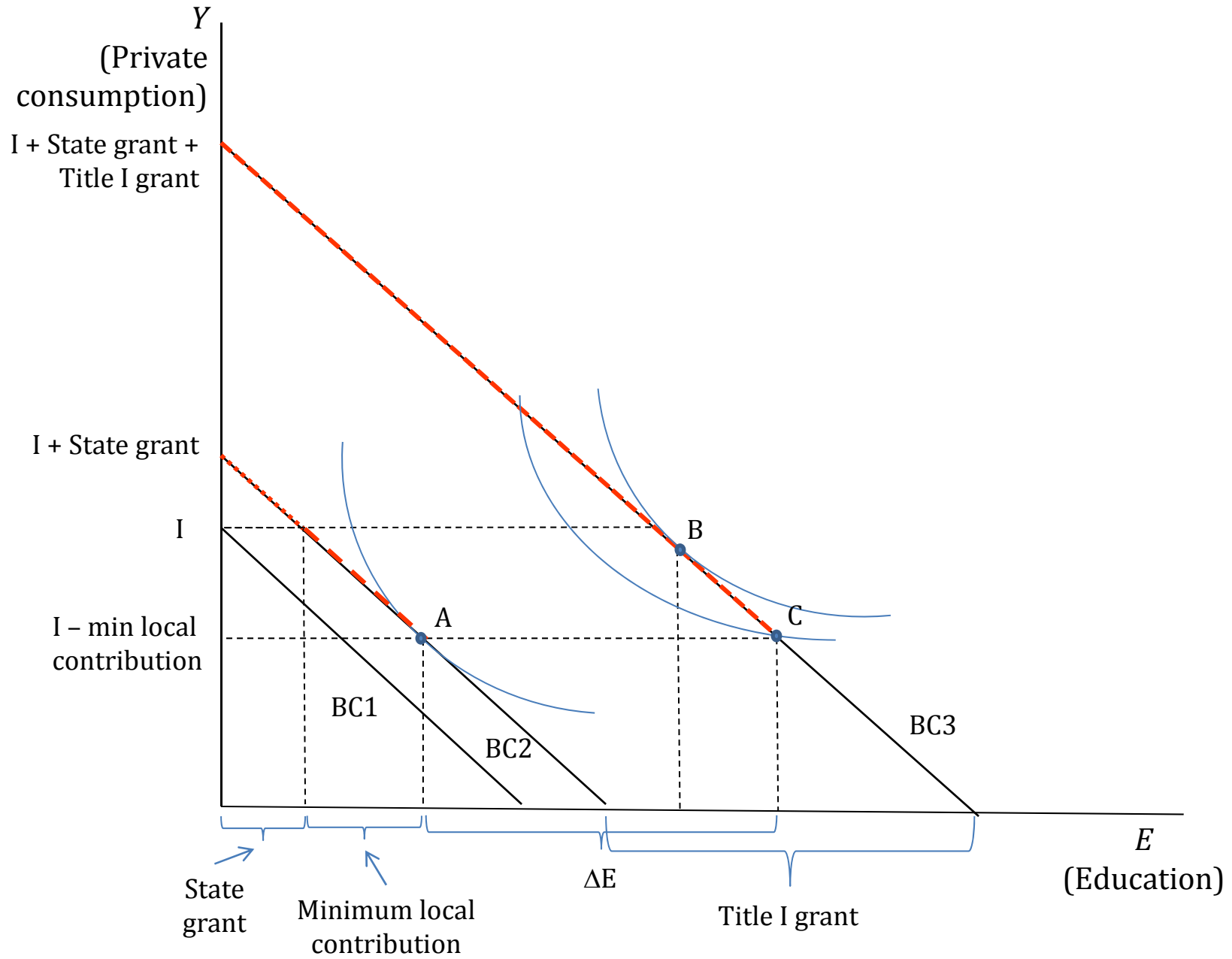
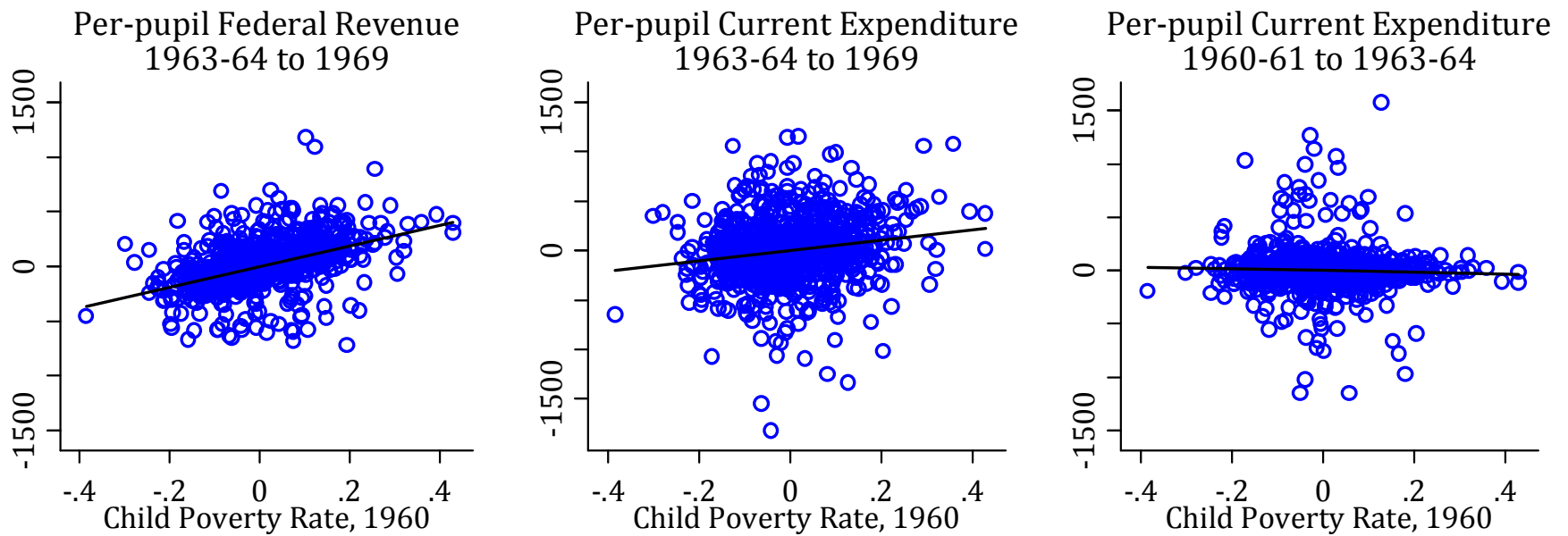
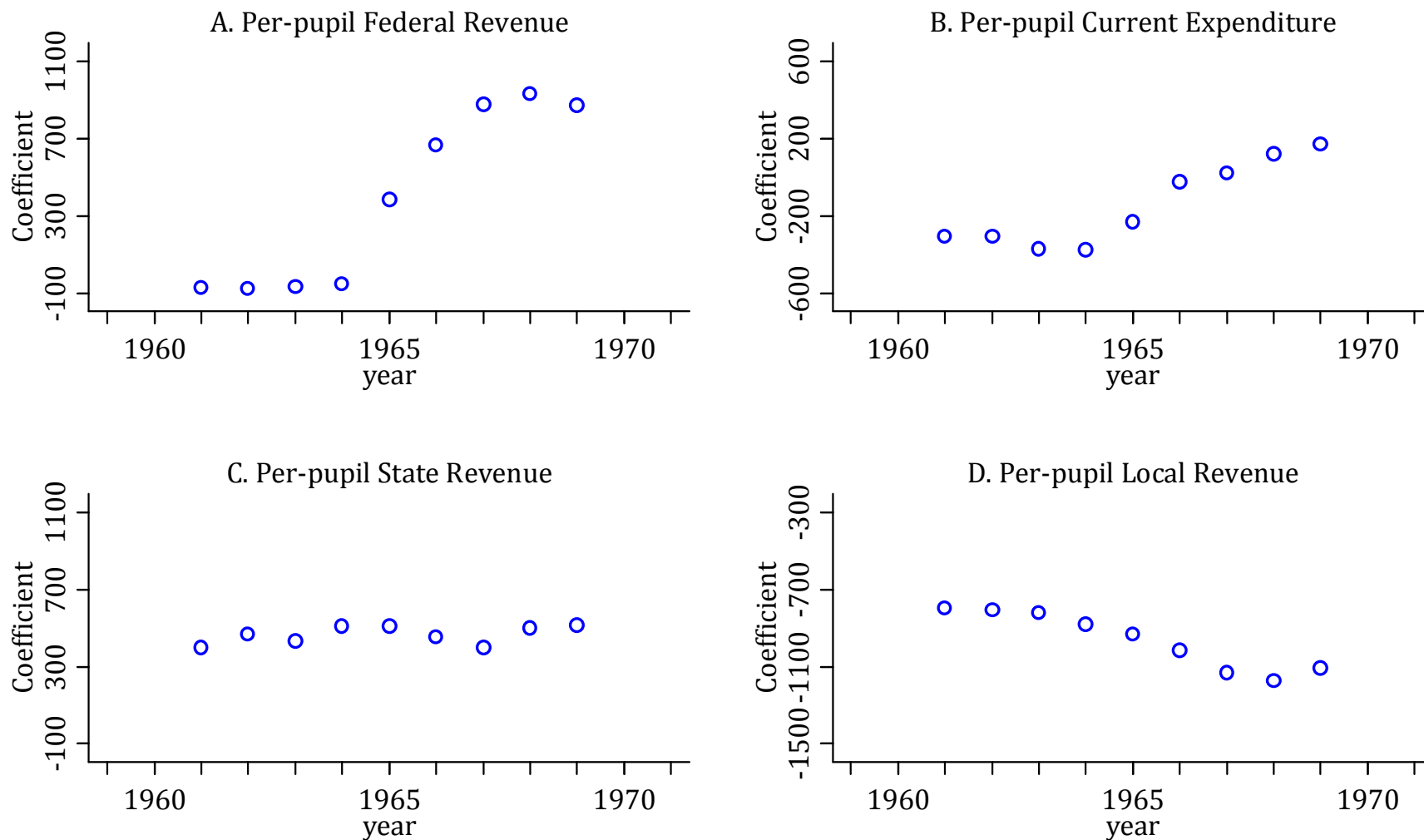


Figure III. Reduced-Form Relationships between Federal Revenue and Current Expenditure Per-Pupil and the 1960 Child Poverty Rate



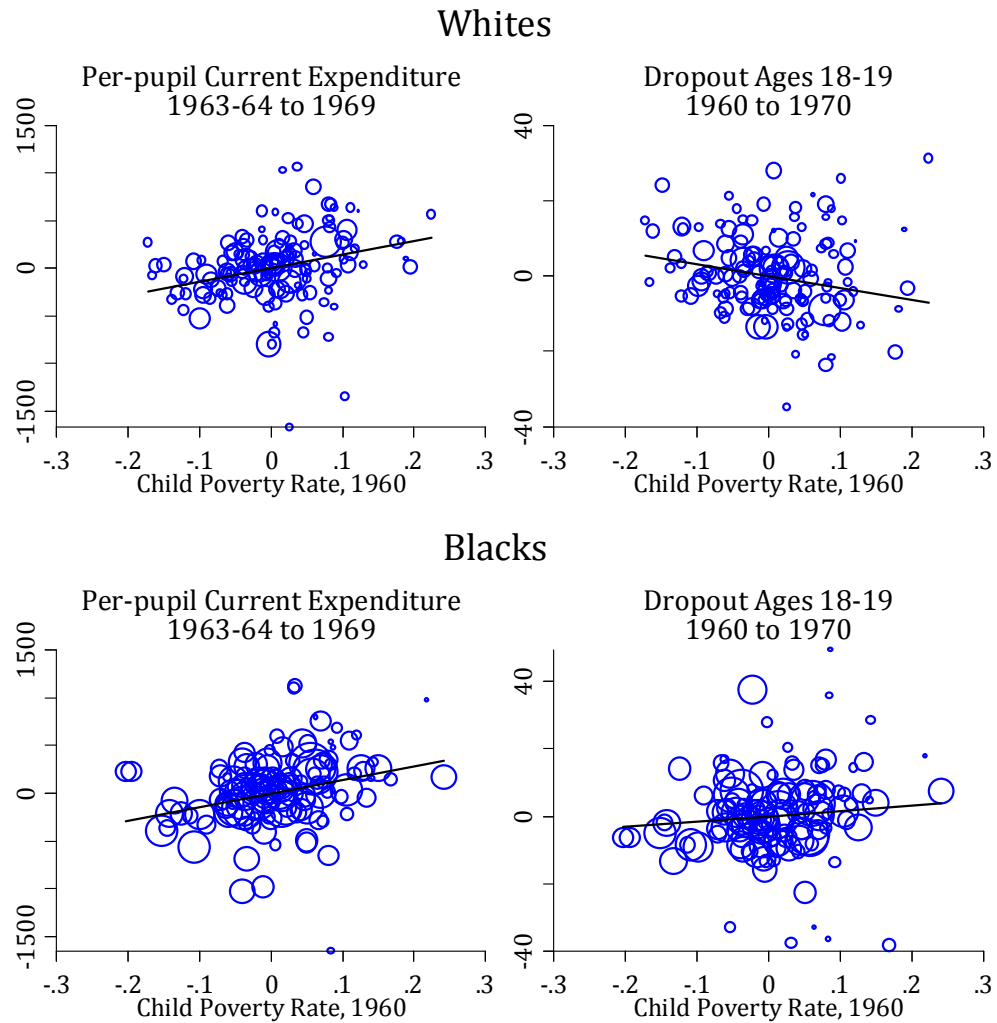
Note: Plotted are residuals from regressions on state dummies, 1960 black share in enrollment, ln 1960 enrollment, dummies for county quintile in 1948 Thurmond vote share, and dummy for among poorest 300 counties in 1960

# Figure IV. Year-by-Year Gradients of District Finance Variables in the 1960 Child Poverty Rate



Note: Plotted are slopes on the 1960 child poverty rate from year-specific regressions on state dummies, 1960 black share in enrollment, ln 1960 enrollment, dummies for county quintile in 1948 Thurmond vote share, and dummy for among poorest 300 counties in 1960.

Figure Va. The 1960 Child Poverty Rate and Differences in Current Expenditure and Dropout Rates by Race over the 1960s  
 Counties with Low Scope for Offset

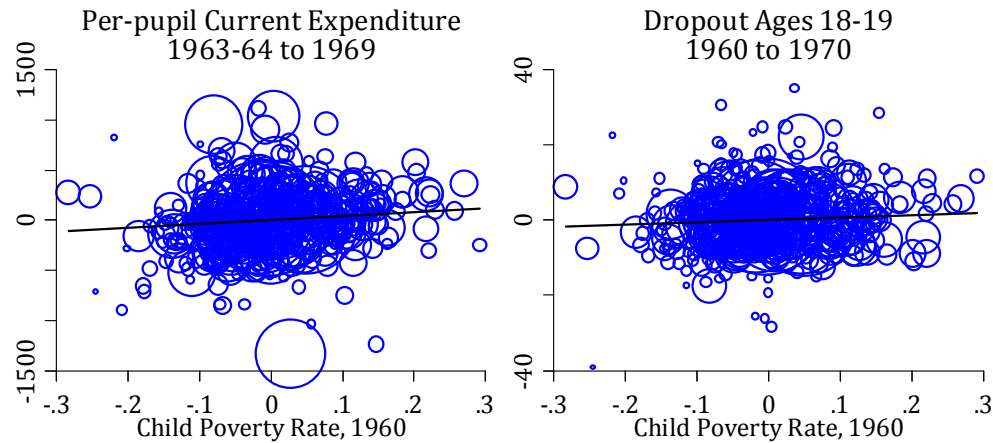


Note: Plotted are residuals from regressions on state dummies, 1960 black share in enrollment, ln 1960 enrollment, dummies for quintile of 1948 Thurmond vote share, dummy for among poorest counties in 1960, the race-specific 1960 dropout rate of 18-19 year olds. The unit of observation is the county, and regressions are weighted by the race-specific 1960 population of 18-19 year olds.

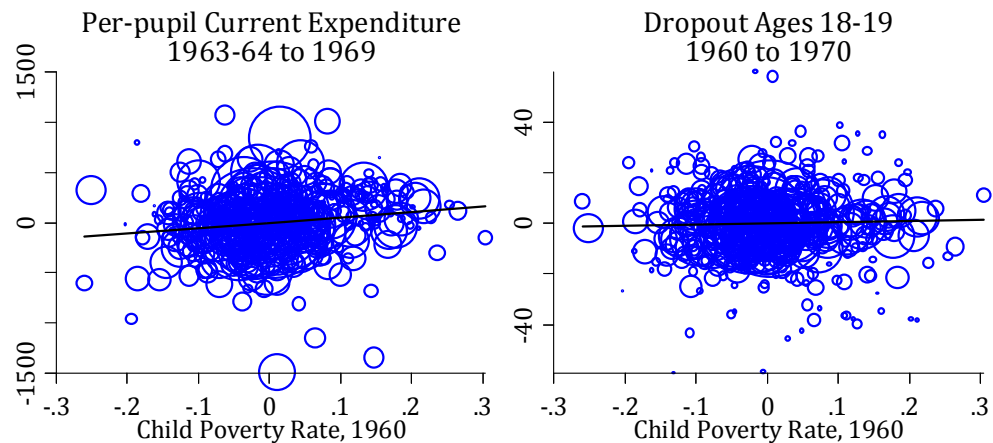
# Figure Vb. The 1960 Child Poverty Rate and Differences in Current Expenditure and Dropout Rates by Race over the 1960s

Counties without Low Scope for Offset

## Whites



## Blacks



Note: Plotted are residuals from regressions on state dummies, 1960 black share in enrollment, ln 1960 enrollment, dummies for quintile of 1948 Thurmond vote share, dummy for among poorest counties in 1960, the race-specific 1960 dropout rate of 18-19 year olds. The unit of observation is the county, and regressions are weighted by the race-specific 1960 population of 18-19 year olds.

Table I. Descriptive Statistics: Dependent Variables

	Level	Changes	
	1963-64 (1)	1963-64 to 1969 (2)	1960-61 to 1963-64 (3)
Per-pupil federal revenue	62.34 (70.31)	418.21 (262.74)	10.51 (34.62)
Per-pupil state revenue	1,306.24 (350.03)	500.31 (246.65)	114.43 (134.63)
Per-pupil local revenue	497.58 (387.31)	285.04 (230.41)	58.63 (107.25)
Per-pupil current expenditure	1,870.55 (465.26)	962.18 (428.76)	191.86 (212.11)

*Notes:* All figures are in real 2009 dollars. Standard deviations are in parentheses. Sample consists of 90 districts in 9 southern states: Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia.

Table II. Descriptive Statistics: Independent Variables

	Mean	Std. dev.	Obs.
	(1)	(2)	(3)
Child poverty rate, 1960	0.32	0.17	903
State factor, 1969	954	6.16	903
Black share in enrollment, 1960	0.32	0.22	903
District enrollment, 1960	6,969	11,849	903
Poorest 300 counties, 1960	0.24	0.43	903
Percent voting for Thurmond, 1948	34	30	903
Black voter registration rate, 1960	28	24	813
Nonblack private school enrollment share, 1969	0.04	0.07	882
Black/white dissimilarity, 1970	0.28	0.22	849
District under court supervision, 1970	0.35	0.48	863

*Notes:* All dollar figures are in real (2009) terms. Sample consists of districts in 9 southern states: Alabama, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia.



Table III. OLS and TSLS Estimates of the Fiscal Response to Title I

	Dependent Variable is Change: 1963-64 to 1969	
	OLS	TSLS
	(1)	(2)
A. Per-pupil Current Expenditure		
Δ Per-pupil Federal Revenue	0.757*** (0.0437)	0.558*** (0.0959)
Root MSE	279.5	282.7
B. Per-pupil Total Expenditure		
Δ Per-pupil Federal Revenue	0.915*** (0.0790)	0.547*** (0.151)
Root MSE	494.9	501.1
C. Per-pupil State Revenue		
Δ Per-pupil Federal Revenue	0.0727*** (0.0239)	0.0416 (0.0539)
Root MSE	163.8	163.9
D. Per-pupil Local Revenue		
Δ Per-pupil Federal Revenue	-0.0430 (0.0271)	-0.268*** (0.0687)
Root MSE	194.6	200.5
E. PP Total Revenue less PP Total Exp		
Δ Per-pupil Federal Revenue	0.114 (0.0729)	0.226* (0.130)
Root MSE	461.1	461.7
First-stage Partial <i>F</i> -stat on instrument	-	182.4
Number of districts	903	903

*Notes:* All dollar figures are in real (2009) terms. All specifications include as controls 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. Standard errors (in parentheses) are clustered on county. . \*\*\*, \*\*, \* represent statistical significance at the 1, 5, and 10 percent levels, respectively.

Table IV. Reduced-form Fiscal Responses to Title I

	Dependent Variable is Change:	
	1963-64 to 1969	1960-61 to 1963-64
	(1)	(2)
A. First Stage: Per-Pupil Federal Revenue		
Child Poverty Rate, 1960	942.5*** (69.80)	9.193 (7.924)
Fraction Black, 1960	69.48 (42.58)	-0.516 (5.090)
R-squared	0.500	0.605
B. Reduced Form: Per-Pupil Current Expenditure		
Child Poverty Rate, 1960	526.2*** (109.0)	-73.72 (54.54)
Fraction Black, 1960	330.5*** (76.08)	-24.07 (44.75)
R-squared	0.460	0.095
C. Reduced Form: Per-Pupil State Revenue		
Child Poverty Rate, 1960	39.23 (51.33)	46.61 (37.95)
Fraction Black, 1960	73.32* (37.79)	11.74 (25.87)
R-squared	0.563	0.377
D. Reduced Form: Per-Pupil Local Revenue		
Child Poverty Rate, 1960	-252.8*** (60.35)	-71.84** (28.42)
Fraction Black, 1960	139.5*** (40.92)	-39.52* (22.49)
R-squared	0.311	0.284
Number of districts (all panels)	903	903

*Notes:* All dollar figures are in real (2009) terms. In addition to the 1960 poverty rate and 1960 black enrollment share, all specifications include state dummies,  $\ln(1960$  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. Standard errors (in parentheses) are clustered on county. . \*\*\*, \*\*, \* represent statistical significance at the 1, 5, and 10 percent levels, respectively.

Table V. Sensitivity of the TSLS Estimates of the Long-Run Fiscal Response to Title I

	Baseline	Change Functional Form of Black Share			Concurrent Policy Changes			Lags
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. $\Delta$ Per-pupil Current Expenditure								
$\Delta$ Per-pupil Federal Revenue	0.558*** (0.0959)	0.592*** (0.0786)	0.548*** (0.0808)	0.622*** (0.0896)	0.561*** (0.0978)	0.558*** (0.105)	0.558*** (0.0983)	0.555*** (0.0974)
Root MSE	282.7	282.3	280.9	282.7	282.4	286.2	282.3	283.1
B. $\Delta$ Per-pupil State Revenue								
$\Delta$ Per-pupil Federal Revenue	0.0416 (0.0539)	0.0881** (0.0431)	0.0513 (0.0424)	0.0554 (0.0513)	0.0656 (0.0525)	0.0408 (0.0618)	0.0546 (0.0575)	0.0594 (0.0515)
Root MSE	163.9	164.1	149.2	163.7	162.3	169.0	163.6	156.6
C. $\Delta$ Per-pupil Local Revenue								
$\Delta$ Per-pupil Federal Revenue	-0.268*** (0.0687)	-0.271*** (0.0566)	-0.260*** (0.0570)	-0.239*** (0.0622)	-0.282*** (0.0669)	-0.266*** (0.0728)	-0.262*** (0.0691)	-0.304*** (0.0696)
Root MSE	200.5	200.3	198.3	199.2	201.3	201.5	199.1	198.3
First-stage Partial $F$ -stat on Instrument	182.4	276.1	256.5	217.2	182.2	141.3	172.5	179.3
Number of districts	903	903	903	903	840	813	903	903
Controls:								
Black share in enrollment, 1960	X	X	X		X	X	X	X
ln(district enrollment, 1960)	X	X	X	X	X	X	X	X
Poorest 300 counties indicator	X				X	X	X	X
Thurmond vote, 1948 (quintile dummies)	X				X	X	X	X
State dummies	X	X	X	X	X	X	X	X
x black share in enr, 1960			X					
x ln(district enr, 1960)			X					
Deciles of black share in enrollment, 1960				X				
School segregation measures, 1970					X			
Nonblack private school enrollment share, 1969					X			
Voter Registration, 1960						X		
Change spending on other public programs							X	
Lagged change in state and local revenue								X

*Notes:* Each column in each panel represents a different TSLS regression. The instrument for the change in per-pupil federal revenue is the 1960 child poverty rate. Changes in fiscal variables correspond to 1963/64 to 1969 and are in real (2009) dollars. Standard errors (in parentheses) are clustered on county. . \*\*\*, \*\*, \* represent statistical significance at the 1, 5, and 10 percent levels, respectively.

Table VI. Heterogeneous Fiscal Responses to Title I

	Dependent Variable is Change from 1963-64 to 1969		
	Current		
	Expenditure (1)	State Revenue (2)	Local Revenue (3)
A. Scope for Offset (Title I Grant/Pre-program Local Revenue)			
$\Delta$ Per-pupil Federal Revenue	0.377*** (0.143)	0.0177 (0.0802)	-0.434*** (0.117)
X Low Scope for Offset (bottom quartile)	0.558** (0.236)	-0.0815 (0.123)	0.627*** (0.145)
B. Black Enrollment Share			
$\Delta$ Per-pupil Federal Revenue	0.418*** (0.118)	0.0159 (0.0638)	-0.362*** (0.0811)
X High Black Share (top quartile)	0.463** (0.201)	0.0705 (0.110)	0.415*** (0.120)
C. Scope for Offset and Black Share			
$\Delta$ Per-pupil Federal Revenue	0.294* (0.152)	-0.0106 (0.0853)	-0.487*** (0.117)
X Low Scope for Offset (bottom quartile)	0.569** (0.270)	-0.0268 (0.144)	0.533*** (0.176)
X High Black Share (top quartile)	0.252 (0.265)	0.0401 (0.152)	0.277 (0.185)
Number of districts	903	903	903
Controls:			
Interacted Treatment Variable	X	X	X
Interacted with Treatment Interaction Variable:			
Black share in enrollment, 1960	X	X	X
ln(district enrollment, 1960)	X	X	X
State dummies	X	X	X
Poorest 300 counties indicator	X	X	X
Thurmond vote, 1948 (quintile dummies)	X	X	X

*Notes:* All dollar figures are in real (2009) terms. Excluded instruments are 1960 poverty rate and 1960 poverty rate interacted with the relevant interacted treatment variable. Standard errors (in parentheses) are clustered on county. . \*\*\*, \*\*, \* represent statistical significance at the 1, 5, and 10 percent levels, respectively.

Table VII. Reduced Form Heterogenous Effect Estimates for Current Expenditure and Dropout Rates at the County Level

	Dependent variable:	
	1963/64 to 1969 Diff. in Per-Pupil Current Expenditure (1)	1960 to 1970 Diff. in Dropout Rate of 18-19 y.o.'s (x 100) (2)
A. Whites		
Child Poverty Rate, 1960	372.1*	4.76
	(223.8)	(3.85)
X Low Scope for Offset	1,198***	-37.32***
	(439.9)	(11.69)
R-squared	0.601	0.467
Number of Observations	638	638
Weight	white 18-19 y.o.'s, 1960	
B. Blacks		
Child Poverty Rate, 1960	505.8***	5.09
	(186.4)	(4.81)
X Low Scope for Offset	1,039***	12.47
	(400.1)	(10.13)
R-squared	0.58	0.495
Number of Observations	598	598
Weight	black 18-19 y.o.'s, 1960	
Controls:		
Low Scope for Offset	X	X
Interacted with Low Scope for Offset:		
Black share in enrollment, 1960	X	X
ln(district enrollment, 1960)	X	X
State dummies	X	X
1960 Race-Specific Dropout Rate Ages 18-19	X	X
Poorest 300 counties indicator	X	X
Thurmond vote, 1948 (quintile dummies)	X	X

*Notes:* Each column and each panel represent a different regression. The unit of analysis is a county. We restrict attention to counties where districts in our estimation sample represent at least 90 percent of total county enrollment in 1960. County aggregates of per-pupil current expenditure (2009 dollars) were generated from the district data weighting by 1960 district enrollment. In cases where not all districts within a county have low scope for offset, we coded the county as not having low scope for offset. The change in high school dropout rates of 18-19 year olds is measured at the county level. Standard errors (in parentheses) are heteroskedasticity robust. . \*\*\*, \*\*, \* represent statistical significance at the 1, 5, and 10 percent levels, respectively.

Table VIII. OLS and TSLS Estimates of the Effect of Current Expenditure on Dropout Rates

Dependent Variable:	1960-1970 Diff in Dropout Rate of 18-19 Year Olds (x100)			
	OLS (1)	TSLS (2)	TSLS (3)	TSLS (4)
A. White				
Mean of Dependent Variable	-10.12	-10.12	-10.12	-10.18
1963-64 to 1969-70 Difference in Per-pupil Current Expenditure (\$100s)	0.15** (0.07)	-3.11** (1.54)	-3.38* (1.76)	-3.13* (1.76)
Child Poverty Rate, 1960	2.05 (3.73)	16.35 (11.62)	13.25 (11.27)	14.46 (11.94)
Fraction Black, 1960	-1.73 (3.15)	16.01 (10.90)	11.19 (9.57)	5.15 (9.02)
X Low Scope for Offset	-0.19 (5.11)	-4.45 (9.13)	-3.17 (9.23)	-5.06 (9.37)
RMSE	5.620	11.24	11.59	10.96
First-stage partial <i>F</i> -stat on Excl. Inst.	-	7.42	6.33	6.09
Number of Counties	638	638	638	591
B. Black				
Mean of Dependent Variable	-14.08	-14.08	-14.08	-13.99
1963-64 to 1969-70 Difference in Per-pupil Current Expenditure (\$100s)	-0.01 (0.12)	1.20 (1.10)	1.47 (1.29)	1.99 (1.73)
Child Poverty Rate, 1960	7.08 (4.56)	-0.98 (8.88)	-2.78 (9.16)	-4.62 (11.69)
Fraction Black, 1960	-8.27** (3.70)	-14.73** (6.91)	-14.09** (6.65)	-17.41** (7.45)
X Low Scope for Offset	-1.34 (6.19)	-0.67 (6.79)	0.29 (7.12)	0.77 (8.27)
RMSE	8.065	8.744	9.061	9.807
First-stage partial <i>F</i> -stat on Excl. Inst.	-	6.74	5.82	4.77
Number of Counties	598	598	598	568
Excluded Instrument	-	Child Poverty, 1960 X Low Scope for Offset		
Controls:				
Low Scope for Offset	X	X	X	X
Interacted with Low Scope for Offset:				
Black share in enrollment, 1960	X	X	X	X
ln(district enrollment, 1960)	X	X	X	X
State dummies	X	X	X	X
1960 Race-Specific Dropout Rate Ages 18-19	X	X	X	X
Poorest 300 counties indicator	X	X	X	X
Thurmond vote, 1948 (quintile dummies)	X	X	X	X
Change spending on other public programs			X	X
Nonblack private school enrollment share, 1969				X
School segregation measures, 1970				X

*Notes:* Each column and each panel represent a different regression. The unit of analysis is a county. We restrict attention to counties where districts in our estimation sample represent at least 90 percent of total county enrollment in 1960. County aggregates of per-pupil current expenditure (2009 dollars) were generated from the district data weighting by 1960 district enrollment. In cases where not all districts within a county have low scope for offset, we coded the county as not having low scope for offset. The change in high school dropout rates of 18-19 year olds is measured at the county level. Standard errors (in parentheses) are heteroskedasticity robust. . \*\*\*, \*\*, \* represent statistical significance at the 1, 5, and 10 percent levels, respectively.