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Elizabeth Cascio
Ethan Lewis
Nora Gordon
Sarah Reber

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PAYING FOR PROGRESS: CONDITIONAL GRANTS AND THE DESEGREGATION OF SOUTHERN SCHOOLS*

Elizabeth Cascio
Dartmouth College and NBER

Nora Gordon
UC San Diego and NBER

Ethan Lewis
Dartmouth College

Sarah Reber
UCLA and NBER

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Abstract

This paper examines how a large conditional grants program influenced school desegregation in the American South. Exploiting newly collected archival data and quasi-experimental variation in potential per-pupil federal grants, we show that school districts with more at risk in 1966 were more likely to desegregate just enough to receive their funds. Although the program did not raise the exposure of blacks to whites like later court orders, districts with larger grants at risk in 1966 were less likely to be under court order through 1970, suggesting that tying federal funds to nondiscrimination reduced the burden of desegregation on federal courts.

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I. Introduction

Because the U.S. Constitution reserves powers not explicitly delegated to the federal government for the states, conditional grants are key levers for federal policymakers seeking to affect a broad range of state and local policies. States must implement federally approved speed limits and drinking ages to receive highway funding; universities must provide gender parity in athletic offerings to receive research funding; and states can lose funding if they do not comply with the Clean Air Act. More recently, states and school districts have risked losing federal grants for failure to comply with the accountability requirements of the No Child Left Behind Act. In this paper, we examine whether the threat of withdrawal of this same source of federal education funding induced Southern school boards to make an extremely unpopular decision four decades ago—to desegregate public schools.

Dismantling the dual system of education in the South was, to say the least, contentious. Particularly salient cases, such as those in Little Rock and New Orleans, highlighted extreme white resistance and the need for court intervention, enforced by police or the National Guard, as a "stick" to implement the mandate of *Brown*. The literature has established that the courts played a critical role in desegregating Southern schools, especially after 1968 (e.g., Welch and Light 1987; Reber 2005), and the dotted line in Figure I shows that about half of Southern districts were ultimately under court order to desegregate. There was, however, an historic shift away from desegregation in the mid-1960s, when the extent of court supervision was far more limited. Through the mid-1960s, the likelihood that the average Southern school district was desegregated—had one or more black students in any school with any white students (solid line in Figure I)— outpaced the likelihood that it was under court supervision. There was a particularly noticeable burst of "voluntary" desegregation—that is, desegregation not mandated

by a court—between 1964 and 1966, along with a significant uptick in the share of black students attending desegregated schools (dashed line).

We explore whether the "carrot" of federal funding contributed to voluntary desegregation in the mid-1960s and ultimately reduced the burden that desegregation placed on courts. To receive federal funds, Southern school districts had to comply with the non-discrimination provisions of the Civil Rights Act of 1964 (CRA) by desegregating their schools. Title I of the Elementary and Secondary Education Act of 1965 (ESEA) created large grants for schools, generating significant costs of non-compliance. Researchers have speculated that these policies caused the abrupt rise in desegregation witnessed in aggregate data for the mid-1960s.[1] In previous work, we have shown that high-poverty districts—which stood to gain the most from Title I—were particularly likely to desegregate around this time (Cascio, et al. 2008). However, past work has not been able to separate the effect of conditional grants from the effects of concurrent policy changes, such as the Voting Rights Act of 1965 and the heightened threat of litigation resulting from other provisions of CRA.

To address this identification problem, we exploit idiosyncratic variation across school districts in the amount of federal funding at risk from non-compliance with CRA. The amount of Title I funding a compliant district would have received was based on district-level child poverty and state-level spending; the gap in expected Title I receipts between poor and rich districts was larger in states with higher per-pupil spending prior to ESEA. We examine whether the relatively large difference in funding was matched by a relatively large difference in the likelihood of a district exerting the minimum desegregation effort required to collect federal funds – an intuitive prediction of the simple theoretical framework presented below. The

[1] See, for example, Rosenberg (1991), Boozer, Krueger, and Wolkon (1992), Clotfelter (2004), and Ashenfelter, Collins, and Yoon (2006). In a different context, Almond, Chay and Greenstone (2006) argue that the fund-withholding provisions of the Civil Rights Act, combined with the introduction of Medicare, reduced the black-white gap in infant mortality by desegregating Southern hospitals.

credibility of our inferences is supported by the fact that differences between poor and rich districts in other factors influencing desegregation and in pre-program desegregation outcomes did not vary systematically with a state's prior spending.

We investigate the effects of conditional funding for 1966, the first year of the policy for which appropriate data exist. Districts with larger grants were more likely to desegregate on the margins required for compliance with CRA. The probability of having only token desegregation (which we define as less than 2 percent of blacks in desegregated schools) fell by over 8 percentage points for each additional hundred dollars in potential per-pupil Title I funding (in constant 2007 dollars), with districts moving to slightly higher levels of desegregation (2 to 6 percent of blacks in desegregated schools). Our preferred estimates imply that on average a district would have needed to be paid $1,200 per pupil —72 percent of average per-pupil spending in the South in the early 1960s—to move beyond token desegregation. We find suggestive evidence of similar willingness to pay for teacher segregation. Districts with larger potential grants in 1966 also were less likely to have been under court order both then and through 1970.

Our findings thus suggest that conditional federal grants helped prompt a shift away from the minimal desegregation characteristic of the mid-1960s, thereby reducing the burden placed on federal courts in the years that followed. On the other hand, in 1966, districts were not required to desegregate on margins that would have produced substantial increases in exposure of blacks to whites—particularly in comparison with what court orders required after 1968—and we do not find effects of conditional grants on such margins. But our estimates capture the *marginal* effects of conditional grants in 1966 only, leaving out any aggregate effects of the program's existence or contemporaneous effects in other years, when more intensive desegregation would

have been required to receive funding.[2] Further, the historical record suggests that establishing consistent guidelines for desegregation plans—a critical result of ESEA and CRA implementation during the Johnson administration—promoted a strong judicial role in desegregation in the years that followed. Overall, our analysis shows that districts responded to financial pressure to desegregate in an historically meaningful way and—together with the existing empirical and historical literature—supports the view that dismantling desegregation in Southern schools was facilitated by all three branches of the federal government.

II. Conditional Funding and Desegregation in Theory

We begin by presenting a framework for understanding the effects of conditional federal funding on school desegregation using a modified version of the model Margo (1990) used to understand black-white school spending gaps prior to *Brown*. Because spending on black and white students had greatly converged before the period covered by our study (Card and Krueger 1992; Donohue, Heckman, and Todd 2002; Margo 1990), we depart from Margo and assume that expenditure per pupil did not vary by race within district.[3] We then assume that districts faced a trade-off between expenditure per pupil, e, and student segregation, s, measured as the fraction of black students attending all-black schools. We further assume that decision-making rested in the hands of Southern whites, as few Southern school boards had any black members at this time (U.S. Commission on Civil Rights 1968).

White school boards chose e and s to maximize their utility:

$$U = U(e, s),$$

where the marginal utilities of both spending and segregation are assumed to be positive and

[2] We cannot examine the contemporaneous effects of conditional federal funding in later years, when more was required for CRA compliance, due to changes in program rules.
[3] Differences in spending per pupil in black and white schools did persist through the mid-1960s in Louisiana (Reber 2007). However, allowing for differential spending by race would not change the model's implications.

diminishing ($\partial U/\partial e, \partial U/\partial s > 0$, and $\partial^2 U/\partial e^2, \partial^2 U/\partial s^2 < 0$).[4] Maximization was subject to the constraint that per-pupil expenditure not exceed net per-pupil revenue:

$$e \leq l + m + f - \tau(s),$$

where l, m, and f respectively represent revenue per pupil from local, state, and federal sources. For simplicity, we assume that local and state revenue were fixed, though the substantive implications of the model are unchanged if we relax this assumption by introducing local control over taxation. $\tau(s)$ is the per-pupil expense to the district of segregationist policy s.

$\tau(s)$ has three components. First, segregation may have entailed foregone economies of scale and additional transportation costs. Second, maintaining higher levels of segregation entailed costs associated with deterring and fighting litigation. Finally, requirements for compliance with CRA made receipt of federal funds conditional on reaching some threshold level of student segregation. Federal funds per pupil received by a district can therefore be characterized as $f(s) = f$ if $s \leq \tilde{s}$ and $f(s) = 0$ if $s > \tilde{s}$, where $\tilde{s}$ represents this threshold. Let the first two categories of costs be denoted by λ. The cost of segregation was thus $\tau(s) = s\lambda$ if the threshold was reached and $\tau(s) = s\lambda + f$ if not, generating a discontinuity in the budget constraint at $\tilde{s}$:

$$e \leq \begin{cases} l + m + f - s\lambda \text{ if } s \leq \tilde{s} \\ l + m - s\lambda \text{ if } s > \tilde{s} \end{cases}.$$

The value of $\tilde{s}$ changed over time and was district-specific, as discussed below.

The model suggests a simple test of whether the conditional nature of federal funding influenced districts' segregation policy choices. Figure II provides the intuition, plotting the budget constraints of two hypothetical school districts. Both districts have the same preferences

[4] To the extent that school boards care about exposure of whites to blacks, $\partial U/\partial s$ will be larger in districts with a higher black share in enrollment. For simplicity, we do not incorporate racial composition into the model, but we control for a district's initial racial composition in the empirical analysis.

over segregation and spending, represented by their identical indifference curves, but the district represented in Panel A has a smaller potential grant and therefore a smaller increase in funding at $\tilde{s}$. The graphs show that the district facing a sufficiently large federal grant (Panel B) would have desegregated—just to the point required by CRA, $s = \tilde{s}$—while the district facing the smaller grant would have remained fully segregated (s =1). All else equal, districts with larger grants would have been more likely to cross the threshold to receive their federal funds.

Our empirical models are thus designed to test for an effect of conditional federal funding around $\tilde{s}$. In practice, however, not all districts will be observed at exactly $s = \tilde{s}$ or s =1. While districts could target a level of s with a particular desegregation policy, they could not completely control its realized value. Further, a district with sufficiently weak tastes for segregation and/or sufficiently high costs of segregation may have chosen $s < \tilde{s}$.[5] More generally, variation in s arose from heterogeneity across districts in preferences and costs of segregation and in the available budget; we describe the data we have collected on these district characteristics below. This observation points to the importance of using variation in federal funding that is not correlated with these other key determinants of segregation to identify the effect of conditional funding. The Title I formula generated such exogenous variation.[6]

III. Empirical Strategy

In the earliest years of the program, the Title I allocation for district d in state j was equal to its count of poor children in the 1960 Census ($poor_{d1960}$)[7] multiplied by one-half of average per-

[5] It is also possible that conditional federal funding had a perverse effect on segregation for districts that would have otherwise been segregated less than $\tilde{s}$. If segregation is a normal good, such a district would have consumed more segregation due to the income effect of receiving the grant.

[6] The federal government intended for Title I funding to be used for compensatory programs only, but in practice it was used to finance all types of current education spending (Washington Research Project 1969), suggesting it was as fungible as f in our model.

[7] Specifically, $poor_{d1960}$ is the count of five to seventeen year-olds living in families with incomes less than $2,000 in the 1960 Census. There were Title I eligibles in other categories, but these categories were relatively small in the South. See the Data Appendix for more detail.

pupil expenditure in its state two years prior ($stategrant_{j1}$).[8] The program was thus compensatory: Within a state, districts with more poor children were due more Title I funding. However, two districts with the same poverty count in different states would have had different amounts of Title I funding at risk.

The funding formula motivates a difference-in-differences (DD) estimation strategy, where we compare outcomes for higher- and lower-poverty districts in higher- and lower-spending states. Following the logic of a DD framework, we include functions of the district- and state-specific components of per-pupil Title I funding as controls in our baseline model, as both are strongly correlated with funding, and either may be independently related to segregation outcomes.

Our analysis focuses on potential federal funding and school desegregation during the 1966-67 academic year. The model of interest is:

$$(1) \qquad y_{dj} = \alpha + \theta\ ppti_{dj1966} + g\left(poor_{d1960}/enr_{d1966}\right) + h\left(stategrant_{j1966}\right) + \varepsilon_{dj}\,.$$

The outcome, y_{dj}, is an indicator set to one if district d in state j met a particular desegregation target, and $ppti_{dj1966}$ represents potential Title I funding per pupil in the district in 1966:

$$(2) \qquad ppti_{dj1966} \equiv \frac{stategrant_{j1966}\, poor_{d1960}}{enr_{d1966}},$$

where enr_{d1966} is the district's fall 1966 enrollment.[9] $g(\cdot)$ and $h(\cdot)$ are functions of the district's

[8] Table A.I gives the values of $stategrant_{j1}$ by state for 1966-67, the year used in our analysis. Because the Title I program was not fully funded in 1966-67, the figures reported reflect ratable reductions by state-specific multiplicative constants.

[9] All federal funding was on the line, but we only use Title I funding in the analysis due to data constraints. The parameter θ is thus appropriately interpreted as the reduced-form effect of the conditionality of the Title I program on desegregation. As long as the other categories of federal funding were uncorrelated with the identifying variation in Title I funding, our empirical strategy will produce unbiased estimates of the effect of an additional dollar of federal funding overall. We cannot test this assumption but believe it is likely to hold. ESEA funding was the largest category of aid to elementary and secondary education administered by the Office of Education and was about three times as large as each of the two next-largest categories—aid to federally-impacted areas and the National Defense Education Act programs. Neither of these programs distributed funds based on the interaction of poverty with average state-level spending.

child poverty rate, $poor_{d1960}/enr_{d1966}$, and the 1966-67 state factor in Title I funding, $stategrant_{j1966}$, respectively, and ε_{dj} captures unobserved determinants of the segregation decision. If the requirement that districts meet desegregation targets to receive federal funding affected segregation decisions, the parameter of interest, θ, should be positive.

Although $g(\cdot)$ and $h(\cdot)$ account for many potential confounding factors, OLS estimates of θ may be biased. In particular, while the total Title I grant was determined based only on pre-program district characteristics, the *per-pupil* amount depended on 1966 enrollment, which may have been directly affected by desegregation policy. For example, districts with an unobserved taste for segregation might have both desegregated less and experienced more "white flight." This would generate a negative correlation between $ppti_{dj1966}$ and the error term in equation (1), biasing OLS estimates of θ downward. Conversely, holding preferences constant, school desegregation may have increased white flight, biasing OLS estimates of θ upward. Even if neither of these conditions hold, OLS estimates of θ will be attenuated if current enrollment is measured with error.

We therefore instrument for the actual per-pupil Title I grant with the district's "simulated" per-pupil Title I grant, which holds enrollment constant at pre-Title I levels:

$$(3) \qquad ppti^{SIM}_{dj1966} \equiv \frac{stategrant_{j1966}\, poor_{d1960}}{enr_{d,pre}}.$$

$enr_{d,pre}$ represents average enrollment in district d in years prior to Title I's introduction (specifically, between 1961 and 1963). The simulated grant is thus based entirely on pre-program district characteristics and is itself another noisy measure of Title I funding per pupil, allowing us to address biases from both the endogeneity of enrollment and measurement error. Because the current-year value of the child poverty rate in equation (1) is also potentially endogenous, we use the pre-program child poverty rate, $poor_{d1960}/enr_{d,pre}$, as a control in our primary estimating

equation:

$$(1') \qquad y_{dj} = \alpha + \theta\ ppti_{dj1966} + g\left(poor_{d1960}/enr_{d,pre}\right) + h\left(stategrant_{j1966}\right) + \varepsilon_{dj}.^{10}$$

We also use the least restrictive functions for $g(\cdot)$ and $h(\cdot)$ that our data can accommodate maintaining reasonable precision: dummies for quantiles of the pre-program child poverty rate and state fixed effects. The former fixed effects account for segregation shocks shared by similar-poverty districts in different states, while the latter account for common state-level determinants segregation, such as state policies that affected all districts equally.

Two-stage least squares (TSLS) estimates of θ in equation (1') will be consistent if the instrument, $ppti^{SIM}_{dj1966}$ – the *interaction* between pre-program state average per-pupil expenditure and the pre-program district child poverty rate – is uncorrelated with ε_{dj}. Put differently, it must be the case that unobserved differences in segregation between rich and poor districts do not vary systematically with average state spending on education. This assumption would be violated if state policies in lower-spending states such as Mississippi or South Carolina affected the gap in segregation outcomes between high- and low-poverty districts differently than those of governments in higher-spending and more "progressive" states, such as Florida (see Table A.I).[11] While we cannot entirely rule out this source of bias, historical accounts suggest that the importance of state policies to discourage desegregation had diminished by 1966 and where present such policies were not applied differentially to richer and poorer districts.[12]

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[10] We estimate a linear probability model for ease of implementation and interpretation. The reduced-form, marginal effects of the simulated per-pupil grant on our dichotomous outcomes are similar when estimated using probit.

[11] For example, the correlation between $stategrant_{j1966}$ and the share of a state's electorate that voted for Strom Thurmond in the 1948 presidential election – one measure of segregationist preferences – is negative and statistically significant, suggesting that voters in higher spending states were more progressive on race relations.

[12] One exception is Alabama, where Governor George Wallace pressured districts to flout CRA, providing special assistance funds (to offset Title I losses) to districts that did not comply. When we drop Alabama school districts from our sample, we arrive at very similar estimates (available on request). Outside of Alabama, the most active area of state-level policy towards desegregation was legislation aimed at facilitating the development of all-white private schools. We have not found evidence that these policies were differentially applied to higher- and lower-poverty districts. We have also investigated the empirical relevance of this competing hypothesis by estimating (1') separately

Empirical evidence also supports the identifying assumption. For example, we show below that our instrument, $ppti^{SIM}_{dj1966}$, is uncorrelated with several observed proxies for segregationist preferences and the threat of litigation in our chosen specification, and that the TSLS estimates of θ are not sensitive to the addition of these observables to (1'). We also find no significant "effect" of the Title I funding on desegregation before the program existed, suggesting that $ppti^{SIM}_{dj1966}$ is not correlated with unobserved propensities to desegregate.

IV. Data

We have compiled comprehensive school-district-level data for this analysis from a variety of sources. This section provides a brief overview of these data; see the Data Appendix for more detail and the Data References for specific publications.

IV.A. Title I and Other Explanatory Variables

Table IA shows summary statistics for the explanatory variables used in our analysis. The key variable of interest is Title I funding per pupil in 1966, the numerator of which was collected from Congressional reports. In 1966, this figure was $277 for the average district, about 17 percent of the average per-pupil current expenditure of $1,671 in the early 1960s (both in 2007 dollars). Recall that the simulated Title I grant per pupil is the product of the grant per eligible child, which varied across states (see Table A.I), and the district's pre-program child poverty rate, which was on average 33.8 percent. It is this poverty rate that enters directly and flexibly into equation (1').

Our model also incorporates controls for district and county characteristics that may have been related to segregation. Annual state administrative reports from 1961 to 1963 provide

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for districts in states with high and low Thurmond vote shares. We fail to reject the hypothesis that estimates across subsamples are identical, but the estimates are quite imprecise.

district-level data on average pre-program expenditure per pupil and black share in enrollment.[13] Pre-program expenditure both proxies for a district's potential budget and reflects preferences for spending and segregation. Black share in enrollment would have affected white children's exposure to blacks for any given share of blacks in desegregated schools, thereby affecting white preferences for segregation. County voting records provide data on the share of votes cast for Strom Thurmond in the 1948 Presidential election, another proxy for segregationist preferences. Because larger districts were significantly more likely to have been litigated before 1964 (Cascio, et al. 2008), we use average district enrollment between 1961 and 1963, from the state reports noted above, as a measure of the threat of litigation. Several characteristics of the county population in 1960, taken from the *City and County Data Book* – the percent of the population with a high school degree, the share of employment in agriculture, median family income, and an urban indicator (equal to one if more than half the county's population was urban) – round out our list of controls. Table IA shows that, in the early 1960s, the average district in our sample enrolled just over 6,100 students and was 37.3 percent black. It was in a county with a predominately rural and poorly educated population, where 37.3 percent of workers were agricultural and 35.6 percent of votes were cast for Thurmond in 1948.

Recall the identifying assumption in our model: in a specification with sufficient controls for its state- and district-level components, the simulated Title I grant per pupil should not be correlated with unobserved determinants of segregation policy. Table II shows that, with the exception of pre-program expenditure per pupil and the county urban indicator, the *observed* district characteristics described above are not significantly related to the instrument in the two specifications employed in our analysis. To mitigate any potential remaining biases and to reduce

[13] In some cases, we do not have data on enrollment by race for these years, so we use data from later in the 1960s; see Data Appendix.

residual variation, we control flexibly for all district characteristics in the specifications estimated below.

IV.B. Outcomes

The main prediction of the model presented in Section II is that school districts with larger potential federal grants would have been more likely to choose levels of student desegregation at or above the threshold for receiving federal funds. To identify where this threshold was — and to develop outcome variables accordingly — it is critical to understand the specific requirements of the law.

Districts with court-supervised desegregation plans were automatically in compliance with CRA. Other districts were required to submit so-called "voluntary" desegregation plans satisfying policy guidelines set out by the Department of Health, Education, and Welfare (DHEW). Most desegregation before 1967 involved transferring black students to formerly all-white schools, and the Guidelines were specified in terms of the share of blacks that had to be transferred. The first Guidelines, for 1965, were vague and ultimately required the transfer of a handful of black students. Many districts did desegregate on the extensive margin—moving at least one black student district-wide into a school with any white students—for the first time in 1965, giving up the principle of separate schools. In 1966, the year of our main analysis, the Guidelines were more stringent and more specific, requiring higher growth in black "transfer rates" for districts which had desegregated fewer blacks the prior year.[14]

In theory, we could identify the threshold level of desegregation for each district in 1966 based on its transfer rate in the prior year, and use an indicator for exceeding that threshold as

[14] Districts with transfer rates (in practice, shares of blacks in desegregated schools) of 8 to 9 percent in 1965 were expected to double their transfer rates; taking the guidelines literally, in 1966 these districts would have been required to have 16 to 18 percent of blacks in desegregated schools. A tripling of transfer rates was expected from districts that had transferred 4 to 5 percent of blacks in 1965, a "proportionally larger" change for districts that had transferred fewer than 4 percent of blacks in 1965, and a "substantial start" from districts with no transfers in 1965 (U.S. DHEW March 1966, p. 8).

our dependent variable. However, we do not have the data on 1965 transfer rates needed to calculate the 1966 threshold for districts in most states. Even if we had these data, construction of the above variable would be impossible since the Guidelines did not specify clear targets for all districts. Moreover, DHEW lacked the black enrollment data—the denominator of the transfer rate—to enforce its own guidelines literally. Despite these challenges, the district-level data we do have suggest that enforcement was generally accurate, but not all noncompliant districts were pursued.[15] Thus, school districts probably had a general idea of their thresholds in 1966 but faced some uncertainty about exactly what was required to receive their funding.

Our analysis of student desegregation therefore focuses on a series of dependent variables that are ultimately *ad hoc*, but arguably capture the relevant margin for the average school district in our sample. We expect that most districts in our sample would have needed more than "token" levels of desegregation but less than about 10 percent of blacks in desegregated schools to meet the targets set out in the Guidelines.[16] Our key dependent variables are therefore indicators for whether a district fell into each of the following categories: less than 2 percent of blacks in desegregated schools (our measure of token desegregation), 2 to 6 percent, 6 to 10, 10 to 20, 20 to 30, 30 to 50, and 50 to 100.[17] We expect to see most of the response to conditional funding in the lower tail of the distribution, from token desegregation to

[15] We have data on both 1965 and 1966 transfer rates for South Carolina. Using these data, we find that all districts in South Carolina where changes in transfer rates from 1965 to 1966 met the criteria outlined in the Guidelines were deemed compliant and received their federal funds on time. About 28 percent of South Carolina districts which appeared noncompliant had their funds deferred. Orfield (1969) explains why not all cases in violation of the Guidelines would be pursued: DHEW had to submit its enforcement actions to the Justice Department's Civil Rights Division, whose strategy was to enforce only those cases in which the Guidelines were violated *and* the district's free-choice plan was either flawed by administrative design or rendered irrelevant due to local intimidation.

[16] According to the 1966 Guidelines, districts with transfer rates greater than zero but less than 4 percent needed to more than triple their level of desegregation activity. Ninety percent of districts in South Carolina – the one state with data available for 1965 that appears representative of the region – had transfer rates above zero but less than 3 percent in 1965, implying they would have had to transfer less than 10 percent of black students in 1966 to meet their targets. Half of South Carolina districts had 1965 transfer rates less than 1 percent, implying 1966 target transfer rates of around 3 percent.

[17] Although we modeled school board preferences as a positive function of segregation (Section II), we specify our dependent variables in terms of desegregation so that our empirical work matches the policy guidance.

slightly higher levels. Consistent with this idea, Table IB shows that 64 percent of districts had less than 10 percent of blacks in desegregated schools in 1966, and over half had less than 6 percent.

We calculate the fraction of blacks in desegregated schools using data from two sources. The number of black students in desegregated schools—any school enrolling at least one student of each race—was published by the Southern Education Reporting Service (SERS), an organization of Southern newspaper editors funded by the Ford Foundation. We estimate the total number of blacks in the district using current year fall enrollment and percent black in enrollment in the early 1960s, both from the state administrative reports referenced above. Table IB shows that, in the average district in our sample, roughly 18 percent of blacks were in desegregated schools in 1966, compared to less than 1 percent in 1964.[18] SERS also recorded data on teacher desegregation, which was required on the extensive margin by the Guidelines starting in 1966. By this point, nearly three-quarters of districts had at least one black teacher in the same school with a white teacher, compared to none two years prior. By 1966, DHEW reported that over 20 percent of districts had had their funds deferred or terminated, confirming that the law was not an empty threat.

Conditional grants may have also reduced the average district's likelihood of resisting desegregation, and therefore the chances it would be sued and ultimately end up under court supervision. To investigate this, we gathered information on the type of plan submitted (court ordered or voluntary) to comply with CRA from a 1966 DHEW report. As shown in Table IB, only 9.6 percent of Southern districts complied with CRA via court order by the fall of 1966.

[18] Theoretically, a district could have a large share of black students in desegregated schools by moving only one white to a black school, but the Guidelines did not contemplate such behavior, and we do not see it in the data. For example, in 1967, in the average district, less than .04 percent of white students attended schools that were more than 90 percent black, and the *maximum* share of whites in schools that were greater than 98 percent black was less than 1 percent. (Data from 1966 do not allow this calculation.)

This share rose over the years that followed (Figure I), and below, we investigate whether having more funding on the line early (in 1966) slowed this trend. The data for this analysis come from comparable DHEW surveys of CRA compliance in later years.

IV.C. Sample

Our sample of school districts is drawn from the states of the former Confederacy, except Texas (which we had to exclude due to incomplete data): Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee and Virginia. Examining the effects of financial incentives in Border states, which also enforced a dual system before *Brown*, would be interesting; unfortunately, the data are less complete for these states. Because school districts both consolidate and split apart during our sample period, we use the state records referenced above to establish a history of reorganizations and aggregate the raw data to the largest unit to which a district was ever a party (see Web Appendix Section I for details). Of these "aggregated" districts, we exclude those for which desegregation was not relevant because they were one-race or nearly one-race.[19] We also exclude districts that were automatically in compliance with CRA in 1966 because they were supervised by a court in 1964 or were missing data. Our main estimation sample includes 916 districts comprising 84 percent of districts that were not one-race or nearly one-race (see Web Appendix Table I).[20]

V. The Effects of Conditional Federal Funding in 1966

V.A. Student and Teacher Desegregation

Table III presents estimates of the effect of potential Title I funding per pupil on two measures of student segregation in 1966: the indicator for a "token" level of desegregation (less

[19] In particular, we drop districts that were less than 3 percent or more than 97 percent black in the early 1960s. The 3 percent cutoff is arbitrary; results are not sensitive to using alternative cutoffs.

[20] Using aggregated district as our unit of analysis, we assume that two districts that may have already split or not yet consolidated in any given year are behaving as one jurisdiction, potentially biasing our estimates downward. Consistent with this, our point estimates tend to be larger and no less precise when we restrict attention to districts that did not split or consolidate over the sample period (results available on request).

than 2 percent of black students attending desegregated schools) and the indicator for having moved just beyond that token level (2 to 6 percent of blacks in desegregated schools). As described above, for the average Southern district, conditional federal funding was likely to have mattered most for student desegregation in this part of the distribution. The four specifications presented for each outcome differ in the choice of function to control for child poverty and the inclusion of additional pre-existing district and county characteristics.[21] All specifications include state fixed effects. Note that the first stage relationship between the actual and simulated per-pupil Title I grants is strong, with a partial F-statistic on the excluded instrument of over 300 across specifications (see Table A.II).

Because the same specifications are shown below for other outcomes, we discuss them here in some detail. We begin by estimating θ in equation (1'), controlling flexibly for the state- and district-specific components of the simulated per-pupil Title I grant but omitting other pre-existing district characteristics. The first specification, shown in columns (1) and (5), includes state fixed effects and dummies for twenty quantiles of the district pre-program child poverty rate. To improve the precision of our estimates moving forward, all subsequent models include a more parsimonious set of quantile dummies for the child poverty rate ("restricted" quantiles).[22] We first show the more parsimonious model without additional controls (columns (2) and (6)). We then add controls that capture preferences and components of the budget constraint (percent of votes cast for Thurmond in 1948 and early 1960s expenditure per pupil, both using quintile

[21] Here and in all tables below, standard errors are clustered on county because some of our control variables vary at the county level. Note that our data are a cross-section, and the state fixed effects will account for any unobservable state-specific component of the error term. Of course, the error terms may still be correlated across districts within a state. When we cluster standard errors on state and use the critical values from the t-distribution with 8 degrees of freedom to establish statistical significance (following Monte Carlo simulations done in Cameron, Gelbach, and Miller [2007]), the statistical significance of our key results is largely unchanged. All estimates give each district equal weight; weighting by early 1960s black enrollment yields similar results (available from the authors upon request).

[22] In the "restricted quantile model," we retain dummies for the top two (of twenty) quantiles from the first specification, but replace the rest of the quantile indicators with decile indicators. Estimates with the full set of poverty dummies from the first specification tend to be similar in magnitude, but less precise.

indicators, and early 1960s percent black in enrollment, using decile indicators) and the litigation threat (log of early 1960s enrollment) (columns (3) and (7)). The final specification adds the other socioeconomic indicators available at the county level in the 1960 Census (columns (4) and (8)).

The TSLS estimates, shown in Panel A, suggest that the requirement that districts meet desegregation targets to receive their federal funds did affect behavior, shifting districts from tokenism to somewhat more meaningful desegregation. In the specification with the full set of controls, the TSLS estimates imply that a hundred-dollar increase in Title I funding per pupil was associated with a 12.9 percentage-point increase in the likelihood of having 2 to 6 percent of blacks in desegregated schools (column (8)), and an 8.4 percentage point decline in the likelihood of having less than 2 percent of blacks in desegregated schools (column (4)). We cannot reject the hypothesis that these coefficients are equal in magnitude, but opposite in sign. By comparison, the OLS estimates, shown in Panel B, are the same sign, but smaller in magnitude and mostly not statistically significant. The TSLS estimates may be larger than the OLS estimates because districts that desegregated less experienced larger enrollment declines, or because the denominator of $ppti_{dj1966}$ is measured with error. For all outcomes discussed below, this general pattern of differences between the OLS and TSLS estimates persists.

If our instrumental variables approach is valid, the inclusion of district and county characteristics should not substantively change our point estimates. Comparison of the TSLS estimates across specifications suggests that this is the case. The coefficients on the controls (not shown) are generally in line with our expectations.[23] Notably, however, the controls for per-pupil spending in the early 1960s – the one district characteristic strongly correlated with the instrument (Table II) – do not significantly improve the fit of the model. Furthermore, across

[23] For example, consistent with the findings of Cascio, et al. (2008), districts with higher early 1960s black enrollment were significantly more likely to have engaged in only token desegregation in 1966.

specifications, the coefficients on the poverty dummies indicate that higher poverty districts desegregated less, all else equal. The direction of this correlation works *against* finding any effect of financial incentives on desegregation. Our empirical approach thus tests whether the relationship between poverty and desegregation was less negative in higher spending states, where districts had larger grants holding poverty constant.

Panel A of Table IV presents TSLS estimates of the effect of potential Title I funding per pupil on the full distribution of student segregation in 1966 based on the specification with the most complete set of controls. Columns (1) and (2) repeat columns (4) and (8) of Table III. The estimated effects of Title I funding per pupil are small and statistically insignificant for the rest of the distribution—bins at or above 6 percent (columns (3) through (7)). Redefining the dependent variables as indicators for each two-percentage point bin of student desegregation over its entire support also shows that conditional federal funding significantly affected behavior only in the lower tail of the distribution, as we would expect if districts were desegregating just enough to be in compliance (see Web Appendix Figure I).

The final column of Table IV shows that the effect of Title I funding on the *average* percent of blacks in desegregated schools is positive, but small and not statistically significant.[24] This suggests that while marginal financial incentives induced changes on the regulated margin, they do not account for the large overall reduction in the share of blacks in desegregated schools by 1966 shown in Figure I. A substantial minority of districts contributed to this overall decline by desegregating more than required by the Guidelines, consistent with pressure to desegregate mounting from multiple sources during this period; for example, other provisions of CRA increased the threat of litigation and the Voting Rights Act came into effect. It is therefore not

[24] The estimates in the first two columns suggest that a $100 larger grant per pupil shifted about 8 percent of districts from less than two to 2 to 6 percent of blacks in desegregated schools; using the mid-points of the ranges, this amounts to an increase in the percent of blacks in desegregated schools of about .2 ((4-1)×.08) percentage points. The estimate in column (8) is not precise enough to pick up such an effect.

surprising that some districts were inframarginal with respect to the financial incentives.[25] School boards may have even used these policy changes as political cover to desegregate more than required to receive federal funding, to take advantage of economies of scale, for example.[26]

Consistent with the framework presented in Section II, districts that clearly exceeded the Guidelines' requirements appear to have faced higher costs of maintaining segregation and to have had weaker preferences for segregation. For example, consider the 13 percent of districts with more than half of blacks in desegregated schools in 1966 (first row of Table IV), only two of which were under court order. These districts tended to be relatively small and were therefore more likely to benefit from economies of scale when desegregating. Seventy-seven percent were in the bottom two deciles of black share (less than 17.6 percent black), so that increases in the share of blacks in desegregated schools would have translated into smaller increases in whites' exposure to blacks and would have therefore been less costly. Eighty-two percent of these districts were in the bottom two quintiles of Thurmond vote share.

While conditional federal funding did not yield large effects on blacks' overall exposure to whites in 1966, it did move districts across the regulated margin – just beyond tokenism. The magnitude of our estimates for this margin might be interpreted in two different ways. First, a simple rescaling of the key TSLS coefficients suggests that the average Southern district would have required $1,200 per pupil in 1966 (in 2007 dollars) to move beyond token desegregation.[27] This suggests a substantial willingness to pay for segregated schools, equal to over 70 percent of

[25] Similarly, districts that already had more than 2 percent of blacks in desegregated schools in 1964 were unlikely to have responded to the conditional funding on this margin, and our estimates are essentially unchanged when we omit such districts from our estimation sample (results available on request).

[26] This is similar to Heckman and Payner's (1989) suggestion that South Carolina manufacturers "seized on the new federal legislation and decrees to do what they wanted to do anyway" (p. 174). Put another way, some school boards may have been above their "optimal" level of segregation prior to CRA but felt constrained (for example, by a vocal and politically active minority of whites) to maintain a segregated school system.

[27] Because of the sizeable uncertainty surrounding future DHEW policy guidelines for CRA compliance, the size of future Title I grants, and what the courts would require in future years, we interpret our results as identifying the effect of one year's potential grant amount on one year's segregation policy, rather than as the effect of an expected stream of future payments.

the average per-pupil budget in the South in the early 1960s (Table IA). This estimated willingness to pay for segregation is similar to previous estimates for the South based on preferences revealed through the housing market, both historically (Clotfelter 1975) and in more recent data (Kane, Riegg, and Staiger 2006).[28] Second, our estimates suggest that conditional grants account for about 36 percent of the shift away from token desegregation between 1964 and 1966.[29]

As noted above, receipt of federal funding at this time rested not only on meeting threshold levels of student desegregation, but also on desegregating teaching faculties. If conditional funding mattered, we would therefore expect that districts with more funding at risk were more likely to have desegregated faculties. Table V shows the results of estimating the same models as in Table III with an indicator equal to one if the district had any black teachers on faculties with white teachers as the dependent variable. The TSLS estimates are stable and positive across the four specifications. Our preferred point estimate is significant at the 11 percent level, suggesting that all else equal, each additional $100 in potential per-pupil Title I grant increased the probability that a district's teaching faculty would be desegregated by 6.7 percentage points. This estimate implies a willingness to pay to avoid teacher desegregation ($1,500 per pupil) similar to that to avoid moving beyond token desegregation of students and

[28] Our estimate from the model with all controls is consistent with house prices in a school district with "just enough" (2 to 6 percent of blacks in desegregated schools) desegregation being about 1.6 percent lower compared to a district with token desegregation. (See Web Appendix Section III.) Similarly, using data from Atlanta, Clotfelter (1975) found that a three percentage point increase in black enrollment share in the assigned high school (roughly comparable to the change in black share on the margin we examine) was associated with a 1.4 percent decline in house prices between 1960 and 1970. Investigating willingness to pay for school segregation in Mecklenburg County, North Carolina in the 1990s, Kane, Riegg and Staiger (2006) find that a 3 percentage point increase in the percent of black students at the assigned high school was associated with a 1.3 percent decline in house prices.

[29] We use our estimates to arrive at this figure by comparing the likelihood of having zero to 2 percent of blacks in desegregated schools with the average potential Title I grant as opposed to no such grant.

suggests that conditional federal funding explains roughly a quarter of the rise in teacher desegregation between 1964 and 1966.[30]

If our empirical strategy has uncovered the causal effects of conditional funding on desegregation, we should find no relationship between potential Title I funding and desegregation *prior to* the program's introduction. Indeed, we find no significant relationship between Title I funds at risk and student desegregation across its entire distribution in 1964, as shown in Panel B of Table IV.[31] However, because all but the first bin were nearly empty in 1964, a stronger test examines whether the potential grant predicted whether a district had *any* black students in school with whites in 1964. This test reveals that districts with larger per-pupil Title I grants were *less* inclined to have desegregated at all by 1964, but insignificantly so, if anything likely biasing against the effects we find.[32]

V.B. Court Supervision

The results above show that school districts with more federal funding on the line were more likely to meet DHEW's desegregation requirements. In 1966, DHEW Guidelines required at least as much desegregation as the typical court-ordered plan.[33] By increasing the probability of meeting DHEW requirements, we expect that larger grants would have made districts less likely to become targets of litigation. Table V shows that conditional Title I funding indeed

[30] We also find a negative but statistically insignificant impact of potential Title I funding on the likelihood that a district had its federal funding deferred or terminated (see Web Appendix Table II). We attribute the relative weakness of this finding to the incomplete and uncertain nature of enforcement, discussed in Section IV.
[31] Unsurprisingly, we find similar TSLS point estimates, but with larger standard errors, when we estimate a model regressing 1964 to 1966 changes in desegregation indicators on the per-pupil Title I grant.
[32] See Web Appendix Table II. We cannot perform a similar robustness check for teacher desegregation because no school districts in our estimation sample had any teacher desegregation in 1964 (see Table IA).
[33] Comprehensive data on the specific requirements of court-ordered plans are not available, but as discussed below, desegregation requirements were typically strengthened first by DHEW and then the courts during the Johnson Administration. The median court-ordered district had only 2.5 percent of blacks in desegregated schools in 1966, compared to 5.6 percent for the median district not under court order. In December 1966, the Fifth Circuit noted "The announcement in HEW regulations that the Commissioner would accept a final school desegregation order as proof of the school's eligibility for federal aid prompted a number of schools to seek refuge in the federal courts. Many of these had not moved an inch toward desegregation." (*United States vs. Jefferson County Board of Education* (372 F.2d 836), 1966). Orfield (1969, 2000) makes a similar point.

reduced the probability of being under court order in 1966. The coefficient changes little in magnitude across specifications and implies that each additional $100 in per-pupil Title I funding reduced the probability of being under court order by 6.6 percentage points (column (4)). The fact that districts with larger grants were no more likely to have been under court order in 1964 – prior to the introduction of the program – again helps rule out the possibility that our identifying variation in grants was correlated with unobserved tastes for segregation (see Web Appendix Table II).[34]

These findings suggest that CRA and ESEA reduced the burden of school desegregation on federal courts. Our estimates imply that without conditional Title I funding, nearly 28 percent of Southern districts not already under court order by 1964 would have required court supervision to achieve the observed shift away from token desegregation between 1964 and 1966 – triple the actual rate of court supervision. We show below that conditional funding continued to reduce the courts' burden through 1970.

VI. Long-Run Effects of Conditional Funding

The results so far show that conditional federal funding mattered for segregation policy choices but did not substantially increase black exposure to whites by 1966. The dual system of education in the South was not eliminated – and dramatic increases in black exposure to whites not achieved – until after 1966, perhaps diminishing the historical importance of CRA and ESEA relative to court-ordered plans.

On the other hand, any assessment of the full impact of CRA, rather than the marginal effects of conditional funding, must emphasize how it changed the role of the courts. Prior to CRA, few Southern districts had been sued and even those under court order had made little progress. Initially, DHEW chose relatively weak guidelines to avoid conflicts with existing court

[34] For this robustness check, we add back into our estimation sample districts that were court supervised in 1964. Our finding therefore also implies that our baseline estimates are not biased by sample selection.

orders. During the remaining years of the Johnson administration, DHEW strengthened its standards in advance of the courts. The Guidelines then helped courts coordinate on more consistent remedies and gave them cover to adopt more stringent requirements. The landmark 1966 Fifth Circuit opinion in *Jefferson* noted that "the HEW Guidelines offer, for the first time, the prospect that the transition from a *de jure* segregated dual system to a unitary integrated system may be carried out effectively, promptly, and in an orderly manner." Back and forth among the Judiciary, Executive, and Legislature and the resulting case law laid the foundation for later Supreme Court decisions. For example, in the landmark 1968 *Green* [35] decision, the Court drew directly from the 1968 DHEW Guidelines, which required school boards to adopt plans so that "there are no Negro or other minority group schools and no white schools – just schools." In this way, CRA and ESEA may have indirectly contributed to later and more dramatic reductions in school segregation.

That the Guidelines became more stringent over time suggests that conditional federal funding may have had direct impacts on policy margins that *did* matter for black exposure to whites. Unfortunately, we are unable to estimate any such impacts, because a change to the Title I funding formula in 1967 eliminated the cross-state variation in grants per poor child central to our identification strategy.[36] However, we can ask whether having a bigger grant in 1966 affected outcomes in later years. That is, did districts with more conditional federal funding early—which we emphasize is not a proxy for a continued stream of bigger grants—follow a permanently different desegregation trajectory?

Table VI examines this possibility, showing TSLS estimates from the preferred specification (with full controls) for segregation and court supervision outcomes for several years

[35] *Green v. New Kent County* (391 U.S. 430)

[36] Similarly, it would be interesting to examine the extensive margin of student desegregation in 1965, which was all the Guidelines required at the time, but we have district-level data only for two states for 1965.

between 1968 and 1976. We now measure segregation using the dissimilarity index, which captures the margins of desegregation relevant in later years and can be interpreted as the share of students who would have had to change schools to replicate the racial composition of the district in each school.[37] The estimates suggest that districts with larger 1966 grants were no more desegregated by this measure in 1968, 1970, 1972, or 1976 (columns (1)-(4)). However, through 1970, districts with more conditional federal funding in 1966 were less likely to require a court order to achieve the higher levels of desegregation shown in the first row of Table VI. The coefficients indicate that each additional $100 of conditional funding reduced the probability of court supervision by 9 percentage points in 1968 and 10 percentage points in 1970 (columns (5) and (6)). Nevertheless, the power of early conditional funding to promote voluntary desegregation faded over time; by 1972, the estimated effect of Title I funding was roughly half of its magnitude in 1970 and no longer statistically significant.

That the importance of courts increased and the role of conditional funding diminished in the early 1970s is not necessarily surprising. As part of the "Southern Strategy," the Nixon administration stopped enforcing the fund-withholding provisions of CRA, eliminating the potential for marginal financial incentives to matter starting in 1969 (Halpern 1995; Orfield 2000). The 1971 Supreme Court decision in *Swann*[38] also strengthened the desegregation requirements for districts under court supervision and specifically sanctioned the use of busing to achieve racial balance. The rate of court supervision increased substantially after 1970 (Figure I and first row of Table VI). The *Swann* standard and court supervision more generally were particularly important in desegregating larger school districts (Reber 2005; Cascio, et al. 2008).

[37] The measure of segregation relevant in 1966—the share of blacks in desegregated schools—increasingly fails to capture the relevant margins of desegregation in later years, as all-black schools were virtually eliminated by 1970 (Figure I). We examine effects on dissimilarity as a measure of racial balance rather than on the exposure of black students to white students because the former is more closely related to a district's desegregation policy, while the latter also depends directly on the district's racial composition. Results are similar for the index for exposure of blacks to whites.

[38] *Swann v. Charlotte-Mecklenburg* (401 U.S. 1)

VII. Conclusions

Today, the federal government uses conditional grants—as complements or substitutes for other mandates—in a variety of contexts. This paper shows that making receipt of the substantial new federal funds offered through Title I ESEA contingent on nondiscrimination through CRA played a role historically in desegregating Southern schools. Districts with more federal funding on the line were more likely to change from behavior that would have clearly been out of compliance with CRA in 1966—having less than 2 percent of their black students in desegregated schools—to behavior for which most districts would have been judged compliant—having 2 to 6 percent of their black students in desegregated schools. CRA and ESEA also contributed to faculty desegregation and reduced the burden that desegregation had long placed on the courts: Districts with larger conditional grants in 1966 were less likely to be under court supervision—but were no less desegregated—through 1970.

Although the extent of desegregation directly induced by conditional funding in 1966 was small compared to what court-ordered plans would achieve in later years, the desegregation that ESEA and CRA induced appears to have been on a margin that whites cared about, as evidenced by Southern school boards' high willingness to pay to avoid it. The policies also represented an historic break from the past and the decade of inaction following *Brown*, giving the courts much-needed backing of the executive and legislative branches for their interventions into the years that followed.

Data Appendix

I. Data on Title I Funding and Child Poverty

Title I funding allocations were made at the county level. States then allocated grants to districts within each county. We do not know data sources used for this purpose, but we do observe district-level Title I allocations in the first year of the program, 1965-66. Using these data, we estimated district-level Title I allocations for 1966-67 assuming that a district was entitled to a constant share of its county allocation. That is, we defined a district's 1966-67 allocation (potential grant) as the share of the county-level allocation that it received in 1965-66 times its 1966-67 county-level allocation. Data on 1965-66 district-level Title I allocations and 1966-67 county-level Title I allocations were entered from U.S. Senate (1967).[39]

U.S. Senate (1965, 1967) give county-level counts of 5-17 year olds eligible for Title I in 1965-66 and 1966-67, respectively. By 1966-67, there were five categories of eligibles: (1) children in families with incomes less than $2000 in 1960 ($poor_{d1960}$); (2) children in families receiving AFDC in excess of $2000; (3) delinquent children; (4) neglected children; and (5) children in foster homes. We estimated district-level counts of Title I eligibles for 1965-66 and 1966-67 ($eligibles_{d1965}$ and $eligibles_{d1966}$, respectively) with the number of county-level eligibles in the relevant year times the share of the county Title I allocation received by the district in 1965-66 (see above). In 1965-66, only counts under categories (1) and (2) were relevant, and these were based entirely on data collected prior to the introduction of Title I. In 1966-67, only category (1) was based on prior data. For this reason, we calculate $ppti^{SIM}_{d1966}$ using (pre-determined) $eligibles_{d1965}$ and (endogenous) $ppti_{d1966}$ using $eligibles_{d1966}$:

$$ppti^{SIM}_{d1966} \equiv \frac{stategrant_{\,j} eligibles_{\,d1965}}{enr_{d,pre}} \text{ and } ppti_{\,d1966} \equiv \frac{stategrant_{\,j} eligibles_{\,d1966}}{enr_{d1966}}.$$ [40]

In practice, this choice makes little difference in the numerators of the actual and simulated Title I grants per pupil, as in nearly all Southern counties, $eligibles_{d1966} \approx eligibles_{d1965} \approx poor_{d1960}$.[41] We then define the pre-program child poverty rate as $eligibles_{d1965}/enr_{d,pre}$; we refer to it as $poor_{d1960}/enr_{d,pre}$ in the text for ease of explanation.

II. Data on Other District- and County-Level Covariates

District-level data on total enrollment, enrollment by race, and current expenditure prior to 1964 were entered from annual reports of state departments or superintendents of education.[42] Fall 1966 enrollment (enr_{d1966}) was drawn from the same source. Pre-program enrollment ($enr_{d,pre}$) is

[39] A complete citation of this publication and all referenced below are provided under Data References.
[40] Notice that the numerator of $ppti_{d1966}$ is equivalent to the estimated district-level grant for 1966-67 described in the first paragraph of this section.
[41] In our estimation sample, the average, median, and minimum ratios of $poor_{d1960}$ to $eligibles_{d1966}$ are .984, .995, and .865, respectively.
[42] Alabama Department of Education (various years), Arkansas Department of Education (various years), Florida State Superintendent of Public Instruction (various years), Georgia State Department of Education (various years), North Carolina Education Association (various years), South Carolina State Department of Education (various years), State Department of Education of Louisiana (various years), Tennessee Department of Education (various years), Virginia State Board of Education (various years).

average fall enrollment based on data from all years where reported between 1961 and 1963. Pre-program per-pupil current expenditure and pre-program percent black in enrollment are these variables averaged across all years where reported between 1961 and 1963.[43] For states where enrollment by race was not reported, we estimated pre-program percent black using either district-level data from SERS (1964, 1966) (for North Carolina), county-level data on the racial breakdown of the 5-17 year-old population from special tabulations of the 1960 Census (for Florida, where district boundaries correspond to counties), or school-level data on enrollment by race for 1967 from U.S. DHEW (1969) (for Arkansas).

The county-level percent of votes cast for Strom Thurmond in the 1948 Presidential election was drawn from ICPSR Study No. 8611 (Clubb, Flanigan, and Zingale 2006). Data on 1960 median family income, share with a high school degree, share employed in agriculture, and urban status at the county level were drawn from ICPSR Study No. 7736 (U.S. Department of Commerce 1999).

III. Data on Desegregation Outcomes, 1966 and Later

District-level data on the number of blacks in desegregated public schools and the presence of any teacher desegregation for fall 1966 were entered from SERS (1967). Most data were from computer printouts provided by the Office of Education from its first survey of Southern school desegregation. The survey response rate was 80 percent; SERS was able to fill in data for some missing districts. For districts listed, we set the student desegregation indicator equal to one if any blacks were reported to be in school with whites. We estimated the total number of blacks in the district (not reported) with fall 1966 enrollment times pre-program fraction black. We then used this measure along with the number of blacks in desegregated schools to construct the percent of blacks in desegregated schools. Using the information on teacher desegregation, we also constructed an indicator equal to one if any blacks taught on desegregated faculties.

Of the 995 districts meeting all other data requirements for the analysis, 131 did not have SERS data sufficient to directly calculate the percent of blacks in desegregated schools for fall 1966. Of these, 39 had their funds deferred or terminated in that year (see below) and were assumed to have had less than 2 percent of blacks in desegregated schools.[44] Thirteen districts did not appear in SERS (1967) and had not desegregated at all in later years (see below); we assume these districts had not desegregated in 1966 and assign them zero for the percent of blacks in desegregated schools. Our estimation sample for models using data on the percent of blacks in desegregated schools therefore includes 916 school districts.[45]

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[43] We use the enrollment measure most consistently reported within the state over time. All states except Arkansas, Georgia, and North Carolina report fall enrollment or registration or average daily membership. To make these states' enrollment figures more comparable to those for other states, we multiply the enrollment concept reported (average daily attendance, or ADA) by the statewide average ratio of fall enrollment to ADA reported in U.S. DHEW (1967).

[44] Where observed, almost three quarters of districts with funds deferred or terminated have desegregated less than 2 percent of blacks; non-reporting districts likely had less desegregation. We impute the fraction of black students in all-black schools to be .001 for these districts. Because it would require stronger assumptions given the 1966 DHEW guidelines at the time, we do not impute values for the dichotomous indicators of any student or teacher desegregation based on having funds deferred or terminated.

[45] The estimates tend to be stronger when we drop districts with imputed outcomes (available on request).

U.S. DHEW (1966) provides fall 1966 data for all Southern school districts on the type of desegregation plan submitted to comply with CRA and whether the plan was approved by DHEW. We set the court order indicator to one for districts with approved court-ordered plans and zero otherwise. Using other information reported, we created an indicator for whether federal funds to the district had been deferred or terminated by fall 1966.

For fall 1968 and later, data on student desegregation and status of compliance with CRA (type of plan) were drawn from school-level surveys conducted by the Office for Civil Rights. See Cascio, et al. (2008) for more detail on these data and sources.

IV. Data on Desegregation Outcomes, 1964 and Earlier

For 1956 through 1964, we have entered district-level data on desegregation and type of plan from SERS (various years).[46] These publications give, for all districts desegregated "in policy or in practice,"[47] the number of blacks attending public school with whites, the total number of black children enrolled in public schools, and whether desegregation was court-ordered or undertaken voluntarily by the local school board. Using these data, we are able to construct the percent of blacks attending desegregated schools and indicators for whether the district had a court-ordered desegregation plan or any blacks enrolled in public schools with whites for 1964 and earlier.

For districts not listed in these publications, we have set all of these variables to zero.[48] It is difficult to assess the credibility of this assumption, since no other agencies collected data on desegregation over the period of interest. SERS's data collection strategy is also unclear. However, since there were such low rates of desegregation during the period, it was most likely not very onerous to collect the data. SERS was also a trusted source, as it supplied desegregation data to the U.S. Commission on Civil Rights by contractual agreement in 1964 (U.S. Commission on Civil Rights 1966; p. 30). The SERS state-level summaries of desegregation activity are also considered the best available data by social scientists and have been previously cited in academic research (e.g., Rosenberg 1991; Orfield 2000).

[46] We use data presented in the following versions of this publication: April 15, 1957 (for fall 1956), November 1957 (for fall 1957), October 1958 (for fall 1958), May 1960 (for fall 1959), November 1960 (for fall 1960), November 1961 (for fall 1961), November 1962 (for fall 1962), 1963-64 (for fall 1963), and November 1964 (for fall 1964).
[47] Districts desegregated in policy but not in practice had freedom of choice plans, where blacks' option to apply to white schools was not exercised, or court orders that had not yet taken effect.
[48] We compiled lists of districts by state and year from the annual reports cited in Section II of this Appendix.

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Table IA
Descriptive Statistics: Potential Title I Funding in 1966 and Other District and County Characteristics

	Mean	Std. dev.
	A. Potential Title I Funding, 1966	
Title I Per Pupil (current enrollment, $2007)	277	149
Simulated Title I Per Pupil (early 1960s enrollment, $2007)	274	137
	B. Pre-Existing District and County Characteristics	
Early 1960s Child Poverty %	33.8	17.5
Early 1960s Black Enrollment %	37.3	20.2
1948 Thurmond Vote %	35.6	28.8
Early 1960s Enrollment	6,121	10,977
Early 1960s Expenditure per Pupil ($2007)	1,671	427
1960 County Characteristics:		
% with High School Degree	26.36	7.35
% Employed in Agriculture	37.32	24.03
Median Family Income ($2007)	23,308	6,877
=1 if Urban	0.20	0.40
Number of Districts	916	

Notes: Table gives descriptive statistics on key explanatory variables for the full estimation sample in 1966. The unit of observation is the school district. The sample includes school districts in 10 states of the former Confederacy (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia) and is restricted to districts in these states that had black enrollment shares between 0.03 and 0.97 on average between 1961 and 1963, were not under court order to desegregate in 1964, have complete data on the characteristics listed, and have the percent of blacks in desegregated schools observed in 1966. For more information, see text and Data Appendix. "Early 1960s" corresponds to an average taken over 1961 to 1963.

Table IB
Descriptive Statistics: Measures of Segregation

	1964	1966
	A. Segregation Outcomes	
Percent of Blacks in Desegregated Schools	0.8	18.1
(std. dev.)	(5.4)	(28.6)
= 1 if % Black Students in Desegregated Schools is:		
Less than 2%	0.947	0.305
At least 2% but less than 6%	0.021	0.247
At least 6% but less than 10%	0.012	0.087
At least 10% but less than 20%	0.011	0.122
At least 20% but less than 30%	0.003	0.052
At least 30% but less than 50%	0.002	0.060
50% or more	0.003	0.127
Number of Districts	905	916
=1 if Any Black Teachers Work with White Teachers	0.000	0.725
Number of Districts	916	881
	B. CRA Compliance	
Funds Deferred or Terminated	not applicable	0.204
Under Court Order	not applicable	0.096
Number of Districts	not applicable	916

Notes: See notes to Table IA for description of 1966 sample; 1964 sample is limited to districts in the 1966 sample. See Data Appendix for a description of how the variables are constructed.

Table II
Potential Title I Funding in 1966 and Other Determinants of Segregation Policy

	Two-Stage Least Squares Coefficient (Standard Error) on Title I Funding Per Pupil (in 100s of $2007)	
Dependent Variable	(1)	(2)
	A. Proxies for Preferences	
1948 Thurmond %	-1.573	-0.610
	(1.741)	(1.494)
Early 1960s Black Enrollment %	-1.443	0.329
	(1.636)	(1.452)
	B. Proxy for Litigation Threat	
Ln Early 1960s Enrollment	-0.153*	-0.117
	(0.079)	(0.078)
	C. Potential School Budget	
Early 1960s Expenditure per Pupil (100s of $2007)	0.805***	0.485**
	(0.230)	(0.243)
	D. County Characteristics	
1960 % with High School Degree	-0.129	-0.741
	(0.958)	(0.932)
1960 % Employed in Agriculture	-2.880	-0.692
	(3.280)	(3.150)
1960 Median Family Income ($2007)	515.2	-359.6
	(787.2)	(823.3)
1960 Urban Indicator	0.123**	0.086*
	(0.054)	(0.051)
Controls:		
State Fixed Effects	X	X
Early 1960s Child Poverty %:		
Dummies for 20 Quantiles	X	
Restricted Quantile Effects†		X

Notes: Each entry gives the TSLS coefficient (standard error) on Title I funding per pupil (100s of $2007) in a model predicting the district or county characteristic listed. The instrument for Title I funding per pupil is simulated Title I funding per-pupil (also in 100s of $2007); see text. All regressions contain 916 district-level observations and also include as an explanatory variable whether the district had any student desegregation in 1964. Standard errors are clustered on county. *** p<0.01, ** p<0.05, * p<0.1

† Dummies for nine deciles and the top two of the twenty quantiles in the first specification.

Table III
The Effect of Potential Title I Funding on Student Desegregation, 1966

	= 1 if % Black Students in Desegregated Schools is:							
	Less than 2%, 1966				At least 2% but less than 6%, 1966			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of Dependent Variable	0.305				0.247			
	A. Two-Stage Least Squares							
Title I Per Pupil, 1966 (in 100s of $2007)	-0.101**	-0.0747*	-0.0866*	-0.0837*	0.134***	0.151***	0.140***	0.129***
	(0.0483)	(0.0442)	(0.0449)	(0.0449)	(0.0471)	(0.0432)	(0.0438)	(0.0434)
First Stage Partial *F*-Stat for Excluded Instrument	306.2	408.5	433.0	400.1	306.2	408.5	433.0	400.1
RMSE	0.370	0.370	0.364	0.365	0.419	0.420	0.403	0.402
	B. Ordinary Least Squares							
Title I Per Pupil, 1966 (in 100s of $2007)	-0.0660*	-0.0541	-0.0606*	-0.0577	0.0256	0.0416	0.0531	0.0456
	(0.0384)	(0.0365)	(0.0352)	(0.0354)	(0.0345)	(0.0326)	(0.0328)	(0.0321)
RMSE	0.370	0.370	0.364	0.365	0.417	0.418	0.402	0.401
R-Squared	0.375	0.370	0.401	0.402	0.094	0.083	0.168	0.176
Controls:								
State Fixed Effects	X	X	X	X	X	X	X	X
Early 1960s Child Poverty %:								
Dummies for 20 Quantiles	X				X			
Restricted Quantile Effects†		X	X	X		X	X	X
Early 1960s Black Enrollment % (Decile Dummies)			X	X			X	X
1948 Thurmond Vote % (Quintile Dummies)			X	X			X	X
Ln Early 1960s Enrollment			X	X			X	X
Early 1960s Exp. per Pupil (Quintile Dummies)			X	X			X	X
1960 County Characteristics‡				X				X
Number of Districts	916	916	916	916	916	916	916	916

Notes: Each column in each panel gives results from a different regression. In Panel A, the instrument for Title I funding per pupil is simulated Title I funding per-pupil (also in 100s of $2007); see text. The unit of observation is a school district; see text and Data Appendix for descriptions of the sample. In addition to the controls listed, all models include as an explanatory variable an indicator for whether the district had any student desegregation in 1964. Standard errors, in parentheses, are clustered on county. *** p<0.01, ** p<0.05, * p<0.1

† Dummies for nine deciles and the top two of the twenty quantiles in the first specification.

‡ %with high school degree, % employed in agriculture, median family income ($2007), indicator for urban.

Table IV
TSLS Estimates of the Effect of Potential Title I Funding in 1966 on the Distribution of Student Desegregation, 1964 and 1966

	= 1 if % Black Students in Desegregated Schools is:							% of Blacks in
	< 2%	2 to < 6%	6 to <10%	10 to <20%	20 to <30%	30 to <50%	50%+	Desegregated Schools
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				A. 1966				
Mean of Dependent Variable	0.305	0.247	0.0873	0.122	0.0524	0.0600	0.127	18.1
Title I Per Pupil, 1966 (in 100s of $2007)	-0.0837*	0.129***	0.00154	-0.00139	-0.0210	-0.00794	-0.0168	0.951
	(0.0449)	(0.0434)	(0.0227)	(0.0338)	(0.0203)	(0.0311)	(0.0201)	(1.480)
RMSE	0.365	0.402	0.278	0.317	0.218	0.231	0.244	0.178
Number of Districts	916	916	916	916	916	916	916	916
				B. 1964				
Mean of Dependent Variable	0.947	0.0210	0.0122	0.0110	0.00331	0.00221	0.00331	0.8
Title I Per Pupil, 1966 (in 100s of $2007)	0.00244	0.00523	-0.00600	-0.00434	0.00539	0.00192	-0.00464	0.247
	(0.0140)	(0.00958)	(0.00653)	(0.00777)	(0.00364)	(0.00193)	(0.00414)	(0.300)
RMSE	0.197	0.138	0.106	0.103	0.0576	0.0469	0.0569	0.0509
Number of Districts	905	905	905	905	905	905	905	905

Notes: Each column in each panel gives results from a different TSLS regression. The instrument for Title I funding per pupil is simulated Title I funding per-pupil (also in 100s of $2007); see text. The unit of observation is a school district; see text and Data Appendix for descriptions of the sample. The specification is the same as that shown in columns (4) and (8) of Table III. In addition to the controls listed, all models in Panel A include as an explanatory variable an indicator for whether the district had any student desegregation in 1964. The partial F-statistic on the excluded instrument in the underlying first-stage regressions is 400.1 in both Panels A and B. Standard errors, in parentheses, are clustered on county. *** p<0.01, ** p<0.05, * p<0.1

Table V
The Effect of Potential Title I Funding on Teacher Desegregation and Court Supervision, 1966

	= 1 if Any Black Teachers in Same School as White Teachers, 1966				= 1 if Under Court Order, 1966			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of Dependent Variable	0.725				0.0961			
	A. Two-Stage Least Squares							
Title I Per Pupil, 1966 (in 100s of $2007)	0.0633	0.0694	0.0645	0.0673	-0.0552*	-0.0627**	-0.0643**	-0.0658**
	(0.0462)	(0.0422)	(0.0432)	(0.0421)	(0.0288)	(0.0265)	(0.0273)	(0.0267)
First Stage Partial *F*-Stat for Excluded Instrument	289.2	382.7	409.1	383.0	306.2	408.5	433.0	400.1
RMSE	0.371	0.370	0.368	0.368	0.255	0.255	0.248	0.248
	B. Ordinary Least Squares							
Title I Per Pupil, 1966 (in 100s of $2007)	-0.0138	-0.00502	0.00277	0.00570	-0.0324*	-0.0394**	-0.0263	-0.0280
	(0.0382)	(0.0357)	(0.0363)	(0.0355)	(0.0188)	(0.0177)	(0.0184)	(0.0182)
RMSE	0.369	0.369	0.368	0.368	0.255	0.255	0.248	0.247
R-Squared	0.339	0.334	0.352	0.356	0.275	0.269	0.324	0.329
Controls:								
State Fixed Effects	X	X	X	X	X	X	X	X
Early 1960s Child Poverty %:								
Dummies for 20 Quantiles	X				X			
Restricted Quantile Effects†		X	X	X		X	X	X
Early 1960s Black Enrollment % (Decile Dummies)			X	X			X	X
1948 Thurmond Vote % (Quintile Dummies)			X	X			X	X
Ln Early 1960s Enrollment			X	X			X	X
Early 1960s Exp. per Pupil (Quintile Dummies)			X	X			X	X
1960 County Characteristics‡				X				X
Number of Districts	881	881	881	881	916	916	916	916

Notes: Each column in each panel gives results from a different regression. In Panel A, the instrument for Title I funding per pupil is simulated Title I funding per-pupil (also in 100s of $2007); see text. The unit of observation is a school district; see text and Data Appendix for descriptions of the sample. In addition to the controls listed, all models also include as an explanatory variable an indicator for whether the district had any student desegregation in 1964. Standard errors, in parentheses, are clustered on county. *** p<0.01, ** p<0.05, * p<0.1

† Dummies for nine deciles and the top two of the twenty quantiles in the first specification.

‡ % with high school degree, % employed in agriculture, median family income ($2007), indicator for urban.

Table VI
TSLS Estimates of the Effect of Potential Title I Funding in 1966 on Long-Term Outcomes

	Dissimilarity Index				=1 if Under Court Order			
	1968	1970	1972	1976	1968	1970	1972	1976
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Mean of Dependent Variable	0.693	0.247	0.214	0.204	0.304	0.327	0.516	0.553
Title I Per Pupil, 1966 (in 100s of $2007)	0.0106	-0.00760	-0.0196	-0.0193	-0.0907**	-0.0963***	-0.0578	-0.0203
	(0.0184)	(0.0173)	(0.0143)	(0.0149)	(0.0369)	(0.0368)	(0.0368)	(0.0429)
First-Stage Partial *F*-stat for Excluded Instrument	453.7	335.3	339.3	318.1	453.7	335.3	339.3	318.1
RMSE	0.180	0.154	0.129	0.123	0.310	0.346	0.377	0.401
Controls:								
State Fixed Effects	X	X	X	X	X	X	X	X
Early 1960s Child Poverty %:								
Dummies for 20 Quantiles	X				X			
Restricted Quantile Effects†		X	X	X		X	X	X
Early 1960s Black Enrollment % (Decile Dummies)			X	X			X	X
1948 Thurmond Vote % (Quintile Dummies)			X	X			X	X
Ln Early 1960s Enrollment			X	X			X	X
Early 1960s Exp. per Pupil (Quintile Dummies)			X	X			X	X
1960 County Characteristics‡				X				X
Number of Districts	914	993	996	916	914	993	996	916

Notes: Each column gives results from a different TSLS regression. The instrument for Title I funding per pupil is simulated Title I funding per-pupil (also in 100s of $2007); see text. The unit of observation is a school district. For columns (5) - (8), the sample is limited to districts for which the dissimilarity index is observed in the same year. In addition to the controls listed, all models also include as an explanatory variable an indicator for whether the district had any student desegregation in 1964. Districts that experienced a boundary change between 1969 and 1976 are excluded from the analysis. Standard errors, in parentheses, are clustered on county. *** p<0.01, ** p<0.05, * p<0.1

† Dummies for nine deciles and the top two of the twenty quantiles in the first specification.

‡ % with high school degree, % employed in agriculture, median family income ($2007), indicator for urban.

Table A.I
The State Grant Component of Title I Funding, 1966

State	$stategrant_{1966}$ ($2007)	Number of Districts in 1966 Estimation Sample
Alabama	798	83
Arkansas	871	121
Florida	1161	56
Georgia	906	146
Louisiana	891	59
Mississippi	578	99
North Carolina	862	126
South Carolina	647	86
Tennessee	840	67
Virginia	844	73
Total		916

Notes: See Section III and Data Appendix for a description of the Title I funding formula and the text and Data Appendix for a description of the estimation sample.

Table A.II
First Stage Regressions: All Specifications, 1966 Estimation Sample

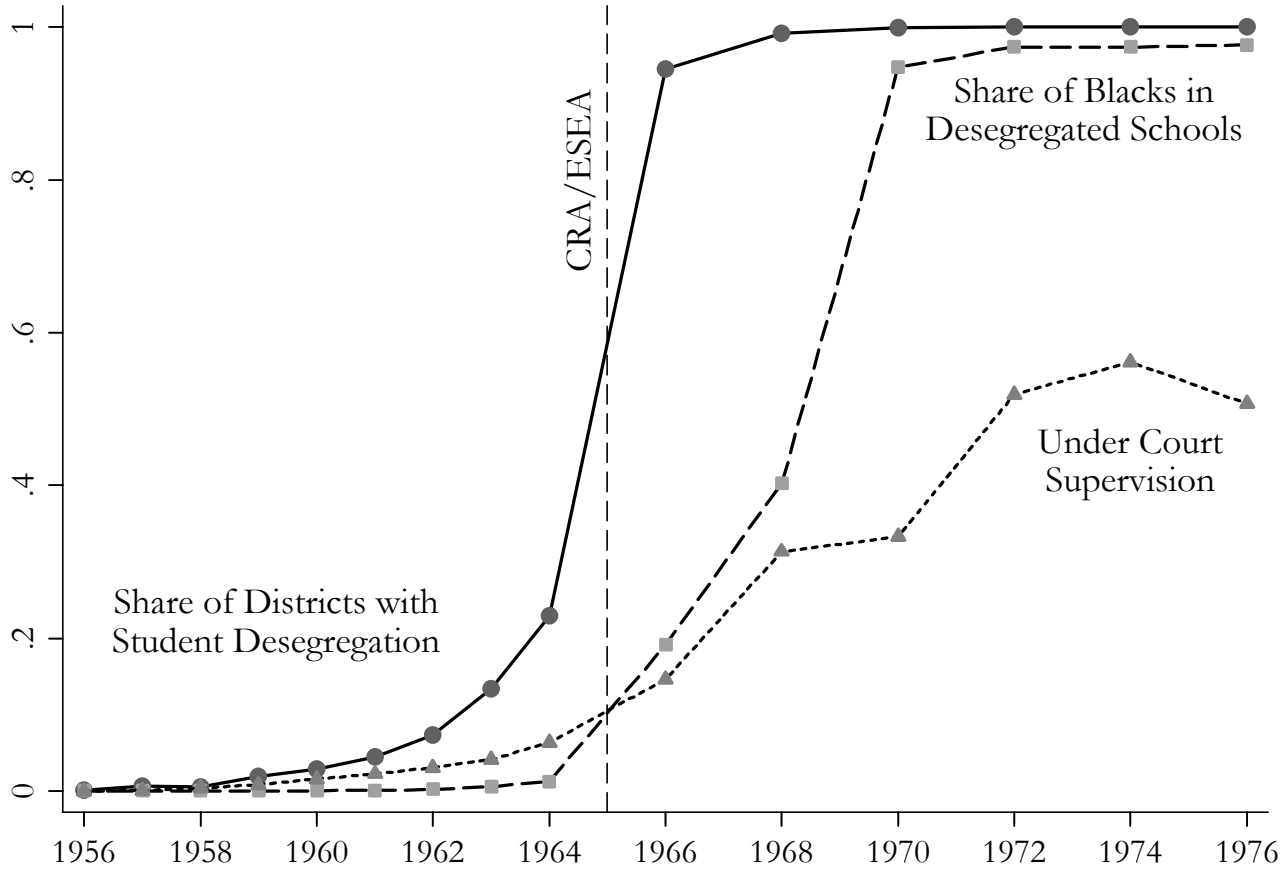
	Per-Pupil Title I Grant, 1966			
	(1)	(2)	(3)	(4)
Simulated Per-Pupil Title I Grant, 1966	1.129***	1.122***	1.127***	1.125***
	(0.0645)	(0.0555)	(0.0542)	(0.0563)
R-Squared	0.973	0.972	0.974	0.974
Partial *F*-Stat for Excluded Instrument	306.2	408.5	433.0	400.1
Controls:				
State Fixed Effects	X	X	X	X
Early 1960s Child Poverty %:				
Dummies for 20 Quantiles	X			
Restricted Quantile Effects†		X	X	X
Early 1960s Black Enrollment % (Decile Dummies)			X	X
1948 Thurmond Vote % (Quintile Dummies)			X	X
Ln Early 1960s Enrollment			X	X
Early 1960s Exp. per Pupil (Quintile Dummies)			X	X
1960 County Characteristics‡				X
Number of Districts	916	916	916	916

Notes: Each column in each panel gives results from a different regression. Both the simulated and actual per-pupil Title I grants are in 100s of $2007. The unit of observation is a school district; see text and Data Appendix for descriptions of the 1966 estimation sample. In addition to the controls listed, all models include as an explanatory variable an indicator for whether the district had any student desegregation in 1964. Standard errors, in parentheses, are clustered on county. *** p<0.01, ** p<0.05, * p<0.1

† Dummies for nine deciles and the top two of the twenty quantiles in the first specification.

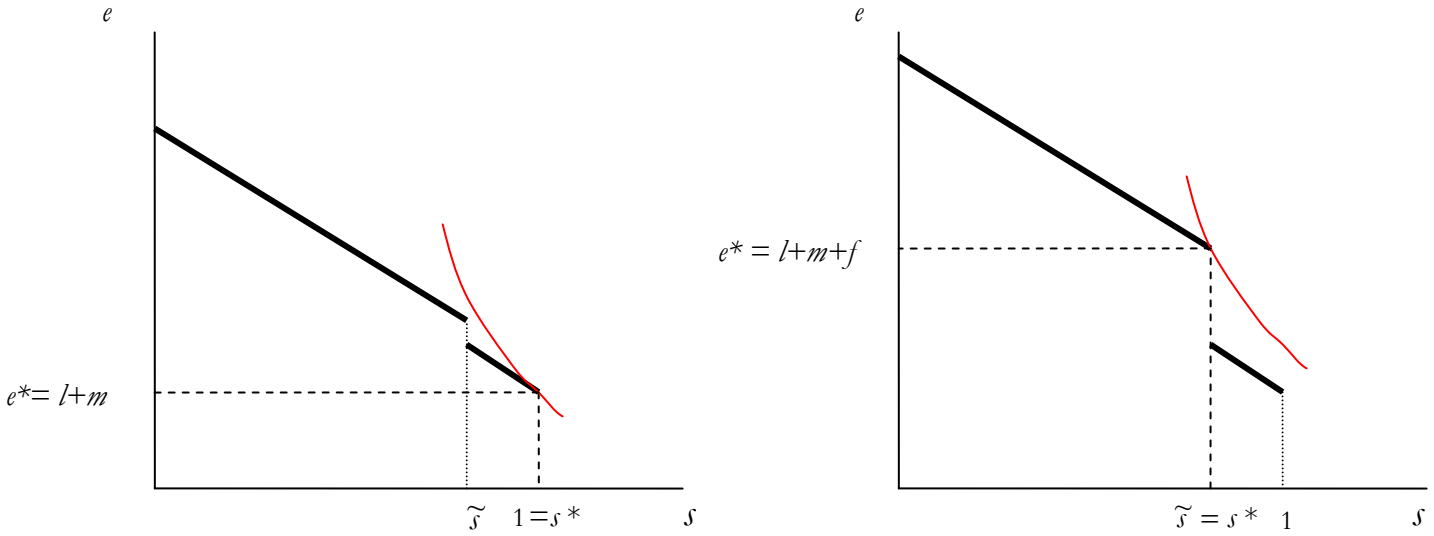
‡ % with high school degree, % employed in agriculture, median family income ($2007), indicator for urban.

Figure I. District-Level Trends in Desegregation and Court Orders in the Former Confederacy



Notes: Authors' calculations based on Southern Education Reporting Service (SERS), Department of Health Education and Welfare, and Office of Civil Rights data. Sample includes unbalanced panel of school districts in all states of the former Confederacy, except Texas (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, and Virginia), where between 3 percent and 97 percent of student enrollment was black on average between 1961 and 1963. Trends for a balanced panel are broadly similar. A school is considered desegregated if it had any blacks in school with whites; a district is desegregated if it contained any desegregated schools. A district is considered under court supervision if was on SERS's list of districts desegregating under court order (1956 to 1964) or if it complied with the Civil Rights Act by submitting a court-ordered plan (1966 to 1976). All school districts are given equal weight. Trend breaks between 1964 and 1966 are less dramatic but still apparent when tabulations are weighted by average black enrollment between 1961 and 1963. See Data Appendix for details.

Figure II. Theoretical Predictions



A. District with small federal grant available

B. District with large federal grant available

Notes: e represents expenditure per pupil; l, m and f represent local, state, and federal revenue, respectively; s represents segregation; and $\tilde{s}$ represents the threshold level of student segregation at which federal funds are received.