

Follow the Money: School Spending from Title I to Adult Earnings



RUCKER C. JOHNSON

Title I funding has been the largest federal program of K–12 education for the past fifty years, the objective being to eliminate the educational disadvantage associated with poverty. I provide new evidence on the long-term effects of school spending from Title I on children's educational and adult economic outcomes. To study effects of Title I, I link school district spending and administrative data on Title I funding to nationally representative data on children born between 1950 and 1977 and followed through 2011. Models include controls for birth cohort and school district fixed effects, childhood family–neighborhood characteristics, and other policies. I find that increases in Title I funding are significantly related to increases in educational attainment, high school graduation rates, higher earnings and work hours, reductions in grade repetition, school suspension or expulsion, incarceration, and reductions in the annual incidence of poverty in adulthood; effects on educational outcomes are more pronounced for poor children.

Keywords: school spending, Title I, educational attainment, poverty

Title I, originally enacted as one of the provisions of the Elementary and Secondary Education Act (ESEA) of 1965, currently accounts for one-third of federal government support for K–12 education. Title I funding represented \$14.3 billion in 2014 appropriations and has been the largest federal program of K–12 education for the past fifty years with an objective of eliminating the educational disadvantage associated with poverty. The program allocates money (nonmatching grants) for compensatory education to school districts based on

child poverty. Currently, more than half of all public schools receive such Title I funding. The program's central goal is to increase achievement of poor students by providing more funding to poor schools. Despite its fiscal importance, evidence on the effectiveness of Title I is mixed (Matsudaira, Hosek, and Walsh 2012; Cascio, Gordon, and Reber 2013; Van der Klaauw 2008). Research has shown that one of the reasons it may not have worked as successfully is the ways in which it may crowd out local funding (Gordon 2004).¹

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1. This relates to the flypaper effect in the public finance literature. For example, Nora Gordon (2004) finds full crowd-out of Title I for the average district during the 1990s.

EXAMINING THE EFFICACY OF TITLE I

This article provides new evidence on the long-term effects of school spending from Title I on children's subsequent educational and adult economic outcomes. To study the effect of Title I, and resultant changes in school spending, on long-term adult outcomes, I link school district spending and administrative data on Title I funding to detailed, nationally representative data on children born between 1950 and 1977 and followed through 2011. In particular, the analysis focuses on the first fifteen years of the roll-out of Title I (from 1965 to 1980) and their long-term impacts for cohorts born between 1950 and 1970 who straddled the period in which federal funding toward low-income districts via Title I rapidly expanded.

With the passage of the Elementary and Secondary Education Act of 1965, school districts with a high percentage of low-income students received additional funding for the first time from the federal government; the regulations gave priority to low-achieving schools. *Poor districts* here refer to those in which a high percentage of students come from low-income families. In some cases, these may be relatively high-spending districts, but they also have high needs. The high degree of residential segregation by economic status and heavy reliance on local property taxes to fund public schools typically leads to a positive correlation between the level of school spending on the one hand, and both childhood family-neighborhood socioeconomic status and student outcomes on the other, in a cross-sectional analysis. However, many of the changes to how schools have been funded since the 1960s have been compensatory, directed to economically disadvantaged districts to narrow educational opportunity gaps driven by funding inequities. Differences in per-pupil spending across wealthy or high-income districts and poor ones are significant, but not all low-income districts are also low-spending, and not all high-spending districts are high-income. The stated program goal of Title I is to improve educational opportunities and outcomes for low-achieving students from schools with high

concentrations of poverty. The program is based on the premise that children from poor families living in high-poverty school districts are doubly disadvantaged: they have fewer nonschool learning opportunities and attend schools with inferior instructional programs relative to children from more economically advantaged backgrounds. This compensatory funding policy likely generates a negative relationship between Title I funding and student achievement that would negatively bias the observed relationship between school spending and student outcomes.²

One of the primary empirical challenges in estimating the effects of Title I funding is that there are many differences between Title I and non-Title I schools and the students attending them (especially the poverty level). The multi-stage allocation procedure for distribution of Title I funds involves the federal government providing money to counties to support K-12 education based on census county poverty counts, and states give money to districts based on these same need-based measures of poverty. The key identification challenge is that, though a district's poverty determines its Title I allotment, poverty also influences both a district's and children's outcomes in a variety of other ways. Thus, separately distinguishing the effects of Title I on state and local revenue from the effects of poverty on Title I, state, and local revenue streams, as well as student outcomes, is extremely difficult. Failure to control for nonrandom assignment of Title I status will generally understate any potential benefits of the program. The research design for this article better addresses the nonrandom allocation of funds and services to facilitate causal inference (that is, attempts to account for the nonrandom selection of students and schools for program participation).

The basic funding structure of the Title I program has remained essentially unchanged over the past fifty years. Although the goal of Title I is to increase school funding in poorer districts to improve student performance, both the intradistrict allocative funding and curricular design of Title I programs to meet

2. An additional concern is whether districts face a disincentive by which improving schools may result in reductions in funding (some scholars have suggested this was the case for some time during the 1980s).

these goals is far from uniform. Some districts target Title I dollars toward disadvantaged children within schools and hold separate classes to provide remedial services to these students and to those identified as at-risk (the pull-out approach). In other districts, the funding is used for schoolwide programs with no targeting of aid at all. Thus, variation is significant in how Title I programs have been implemented across time and geographic areas. Some prior studies of Title I fail to measure whether this additional compensatory funding led to substantive changes in the level or distribution of school funding as well as when and for how long. Without sufficient attention to this first-order question, one cannot definitively conclude whether evidence from Title I can shed light on the role of school spending for student outcomes from poorer districts. That is, if we are to assess the extent to which Title I funding and resultant school spending changes matter, we must first establish how the money is being spent. In the case of no relationship between Title I and outcomes, it is important to understand whether the services funded by Title I are ineffective because they are poorly designed or because they do not represent net service increases.

Other articles that examine the effects of Title I in this issue consider numerous important questions, such as

- Does Title I increase funding of poor schools? Does Title I funding appear to displace other sources of funding? If so, what other sources of funding decline? How does ESEA influence the composition of school spending (expenditures on instruction, the number of guidance counselors, the number of teachers, capital expenditures on school facilities)?
- Does Title I boost measurable school inputs, such as teacher-to-pupil ratios, teacher salary, teacher quality?
- Do schools engage in strategic behavior to gain Title I funding?
- How does the role of socioeconomic status intersect with that of race with regard to ESEA and school functioning in different educational contexts?

This article, however, focuses on whether Title I increases children's long-run socioeconomic attainment, particularly among poor and minority children targeted by Title I programs. That is, has Title I been successful at one of its chief stated objectives in particular?

BACKGROUND AND PRIOR LITERATURE

A higher proportion of Title I resources were allocated to lower grade levels under the assumption that they would have the greatest impact at that level. Such heterogeneity in resource allocation within districts provides an additional rationale to examine a longer time horizon of educational outcomes for cohorts initially exposed during elementary school. For example, Launor Carter (1984) found that Title I students achieved greater gains in the earlier grades than in the upper, and in math programs than in reading.

Related evidence from prior studies shows that schools appear to respond to Title I incentives, possibly by enrolling more eligibles in free-lunch programs (Fisher and Papke 2000). The welfare consequences of such behavior— involving zero-sum competition among poor schools—are likely negative. As well, increases in Title I funding have been shown to be partially offset by local education agency behavior—local funds are redistributed to partially compensate non-Title I schools (Matsudaira, Hosek, and Walsh 2012). Although ESEA explicitly prohibits such substitution that violate the maintenance of effort mandate of the legislation, it is difficult for the federal government to enforce compliance.

During the 1960s and early 1970s, Title I was deemed ineffective because localities did not implement it as intended. Early federal reports of this era explicitly identified major problems in design implementation of Title I, including the misuse of funds and the exclusion of disadvantaged and minority children in low-income areas (McLaughlin 1977), which undermined the program's goals. Ruby Martin and Phyllis McClure's evaluative study (1969) showed how school districts used millions of dollars across the country to make purchases that had little to do with helping poor students. In fact, they documented that districts used Title I funds to continue racial segrega-

tion by offering African American children free food, medical care, shoes, and clothing as long as they remained in predominantly black schools. As well, audits exposed Title I programs whose funds had not been equitably distributed to urban schools but had instead flowed disproportionately to suburban districts. Money intended for poor children was used illegally as a welcome infusion of extra cash to meet overhead expenses, raise teacher pay, and other such general aid. Their investigation helped prompt a flurry of efforts to monitor use of Title I money and ensure that it was being targeted for low-income children and remedial education services.³

Early studies of Title I from this era also charged that the program had perhaps contributed to a dual system of education (and more tracking), in which poor children were systematically subjected to low expectations, watered-down curricula and inexperienced teachers. As documented in an early survey on Title I-funded remedial education programs (Wargo et al. 1972; Rossi et al. 1977), many districts adopted pull-out programs in which reading specialists and teacher aides taught separate compensatory education classes, paid with Title I funds. Although the most popular delivery model for compensatory education was the pull-out program, many educators consider it ineffective and have found it has a stigmatizing effect on students, thus leading to adverse outcomes. As Wilbert Van der Klaauw summarizes it (2008, 754),

Pull-out programs have been found to add little extra instruction time (on average less than 30 minutes a day) and predominantly use drill and practice exercises involving basic thinking skills (Millsap et al. 1993). The additional time Title I students receive in reading and mathematics instruction, replaces the class time that regular students usually receive in more advanced subjects, such as science and social studies (LeTendre 1991). Thus it is not clear that Title I students enjoy much of a net gain in total instruction. . . . Remedial classes, especially those in

high-poverty schools, are often taught by inexperienced teacher aides, the majority of whom do not have college degrees (Millsap et al. 1993; Jendryka 1993). There also have been complaints in the past about a lack of coordination between Title I teachers and regular classroom teachers (Peterson 1987).

Evidence on the effectiveness of Title I improving academic achievement has come primarily from two large congressionally mandated studies (mentioned), one meta-analysis of seventeen studies (cited), and two regression-discontinuity studies using recent cohorts of students in a large, urban northeastern city. The latter two recent studies use quasi-experimental research designs to assess the causal effect of Title I (Matsudaira, Hosek, and Walsh 2012; Van der Klaauw 2008). They compare outcomes of schools just above and just below the district-wide poverty threshold. Their approach to distinguish the effect of Title I from the effect of poverty exploits the fact that eligibility for Title I funding within a given district is determined by the poverty rate of each school's attendance area. This formula for eligibility renders schools with a poverty rate below a given cutoff to be ineligible and to receive no funds whatsoever, and all schools with a rate above the cutoff to receive Title I funds. This feature is amenable to a regression discontinuity approach and is valid if they are similar in other characteristics that may affect outcomes. Neither study found a significant effect of Title I funding on either reading or math at the school level for recent student cohorts from the 2000s. Wilbert Van der Klaauw found no Title I effects on school-level spending, and Jordan Matsudaira and his colleagues found very small effects on spending. However, both of these studies analyzed only two to three years of cross-sectional, administrative data from elementary and middle-school records and were not able to use panel-data methods or follow cohorts to examine longer-run educational outcomes beyond short-run test-score gains.

A limitation of studies of the effects of Title

3. For example, see also summary of results of Department of Health, Education, and Welfare (HEW) audits of forty states conducted between 1966 and 1969.

I on achievement is the focus solely on short-run test scores, which are imperfect measures of learning and may be weakly linked to adult earnings and success in life. Indeed, recent studies have documented that effects on long-run outcomes may go undetected by test scores (Heckman, Pinto, and Savelyev 2013; Deming 2009; Jackson 2012; Chetty, Friedman, and Rockoff 2013; Ludwig and Miller 2007; Kemple 2008; Cortes, Goodman, and Nomi 2015). I address the limitations of focusing on test scores as the main outcome by analyzing the effects of Title I school spending on long-run outcomes such as educational attainment and earnings.

DESCRIPTION OF THE LONGER-RUN OUTCOME DATA

I use nationally representative data from the longest-running longitudinal panel in the world—the Panel Study of Income Dynamics (PSID) spanning 1968 through 2011—matched to administrative data about Title I funding at the county level. The study analyzes the life trajectories of original sample children born between 1950 and 1977 and followed through 2011, using the PSID and its supplements on educational experiences from pre-K through grade twelve.⁴ Using the restricted, confidential, geocoded version of the PSID with identifiers at the level of the neighborhood blocks in which children grew up, the PSID data are linked with neighborhood and school characteristics as well as information on other key policy changes (such as the timing of school desegregation, hospital desegregation, rollout

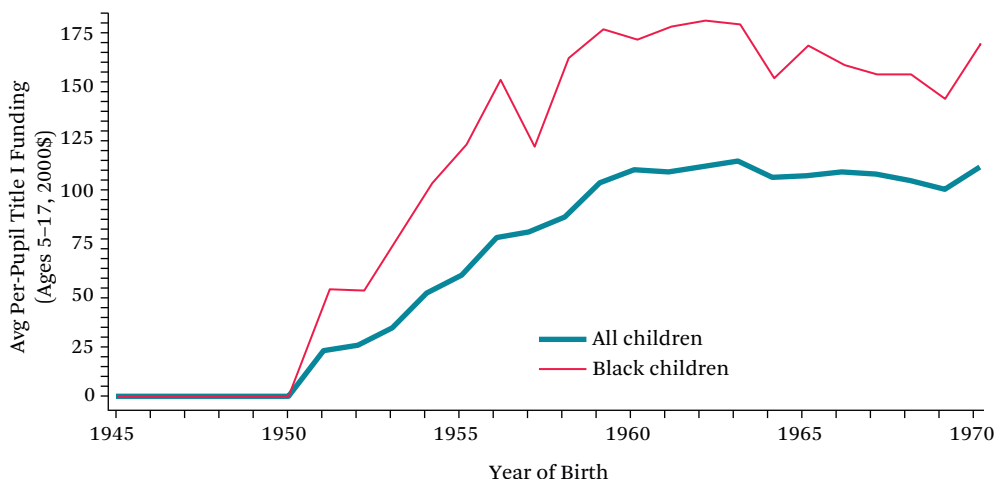
of War on Poverty initiatives, and expansion of safety-net programs) from multiple data sources on the conditions that prevailed when these children grew up. This data construction allows for a rich set of control variables.⁵ Most importantly for this project, these data are linked to federal Title I funding at the county level during the first fifteen years of the program, when these individuals were in their school-age years, acquired from the National Archives and Records Administration (NARA). This historical county-level data enables me to compile an estimate of Title I program expenditures per student and per poor student in the county for all years between 1965 and 1980, and the average Title I per-pupil expenditure during their K–12 school years.

The PSID oversampled low-income and black families, which enables large enough sample sizes of Title I–eligible children among these birth cohorts. The roll-out of Title I and implementation of other War on Poverty policy initiatives during these birth cohorts' childhood provide a unique opportunity to evaluate the long-run impacts of groundbreaking legislation designed to improve educational investment opportunities for poor children.

Figures 1 through 4 show the birth-cohort variation in ESEA–Title I school funding and how the share of individuals exposed to Title I expenditures during childhood increases significantly with birth year over the 1950 through 1970 birth cohorts analyzed in the PSID sample. Specifically, cohorts born in 1950 were the last cohort without access to Title I funding during their school-age years; for cohorts born

4. The PSID began interviewing a national-probability sample of families in 1968. These families were reinterviewed each year through 1997, when interviewing became biennial. All persons in PSID families in 1968 have the PSID “gene,” which means they are followed in subsequent waves. When children with the gene become adults and leave their parents' homes, they become their own PSID “family unit” and are interviewed in each wave. The original geographic-cluster design of the PSID enables comparisons in adulthood of childhood neighbors who have been followed over the life course.

5. The data I use include measures from 1968–1988 Office of Civil Rights (OCR) data; 1960, 1970, 1980, and 1990 Census data; 1962–1999 Census of Governments (COG) data; Common Core Data (CCD) compiled by the National Center for Education Statistics; Regional Economic Information System (REIS) data; a comprehensive case inventory of court litigation regarding school desegregation over the 1955–1990 period (American Communities Project); and the American Hospital Association's Annual Survey of Hospitals (1946–1990) and the Centers for Medicare and Medicaid Services data files (dating back to the 1960s) to identify the precise date in which a Medicare-certified hospital was established in each county of the U.S. (an accurate marker for hospital desegregation compliance).

Figure 1. Birth Cohort Variation in Per-Pupil Title I Funding, All Children

Source: Author's calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA) covering 1965 to 1980 and county-level census counts of the number of children and number of poor children (Surveillance, Epidemiology, and End Results (SEER) data).

in 1960 and through 1970, the average child was in a public school district that received roughly \$110 in per-pupil Title I funding in each year of their school-age years. For the average child, this figure corresponds to about \$950 in Title I funding per poor child received by the public-school district in each year of their school-age years (figures 3 and 4). These increases are especially pronounced for poor and minority children. As shown, the average black child was in a public school district that received roughly \$175 in per-pupil Title I funding in each year of their school-age years (figures 1 and 2), which corresponds to about an average of \$1,150 in Title I funding per poor child during K–12 (figures 3 and 4).

After combining information from these data sources, the main sample used to analyze adult attainment outcomes consists of PSID original sample children born between 1950 and 1977 followed until at least survey wave

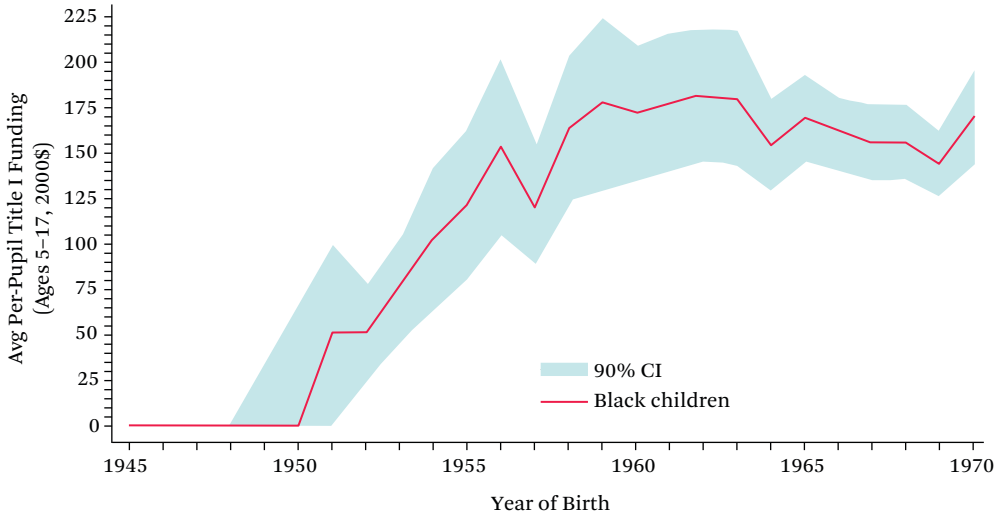
1995 (that is, individuals who were children up to age eighteen in the 1968 wave who have been followed into adulthood).⁶ It includes 7,182 individuals from 2,221 childhood families, 567 school districts, 296 childhood counties, across forty states—a total of 1,572 poor children and 5,610 nonpoor children.⁷ To compare individuals from different cohorts at around the same age, I focus on those adult-economic observations between the ages of thirty and forty. The mean age is thirty-five years for the economic-outcome measures considered.

I begin the analysis by examining the relationship between the 1960 county-poverty rate and Title I funding, then I estimate models of Title I effects on per-pupil spending. The set of adult outcomes examined chronologically over the life cycle include educational outcomes—whether ever repeated a grade, whether ever placed in a gifted program, whether ever suspended or expelled from school, whether grad-

6. The PSID maintains high wave-to-wave response rates of 95 to 98 percent. Studies have concluded that the PSID sample of heads of households and spouses remains representative of the national sample of adults (Fitzgerald, Gottschalk, and Moffitt 1998).

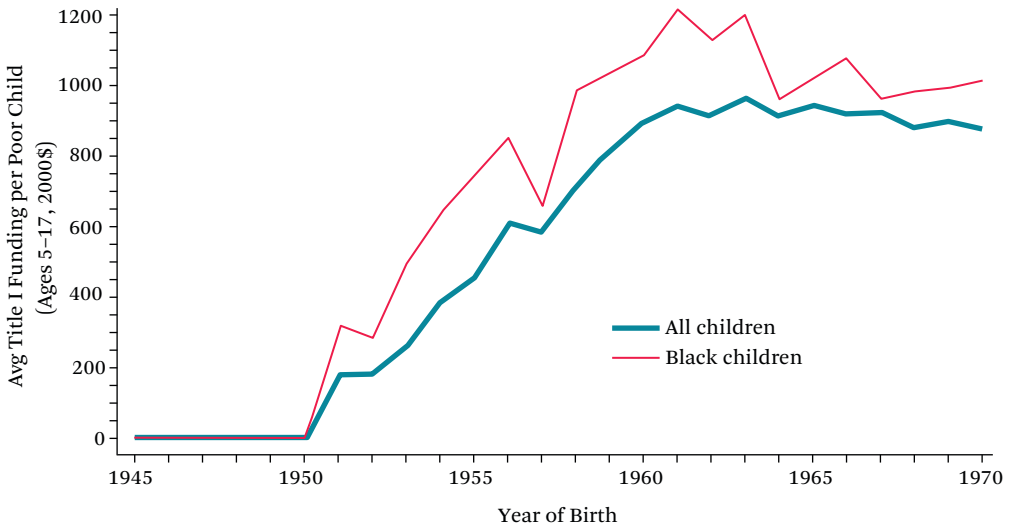
7. The average school district has thirteen PSID sample children: half have at least nine, fewer than 6 percent only one, three-quarters at least three, and one-quarter at least twenty-five.

Figure 2. Birth Cohort Variation in Per-Pupil Title I Funding, 90 Percent CI

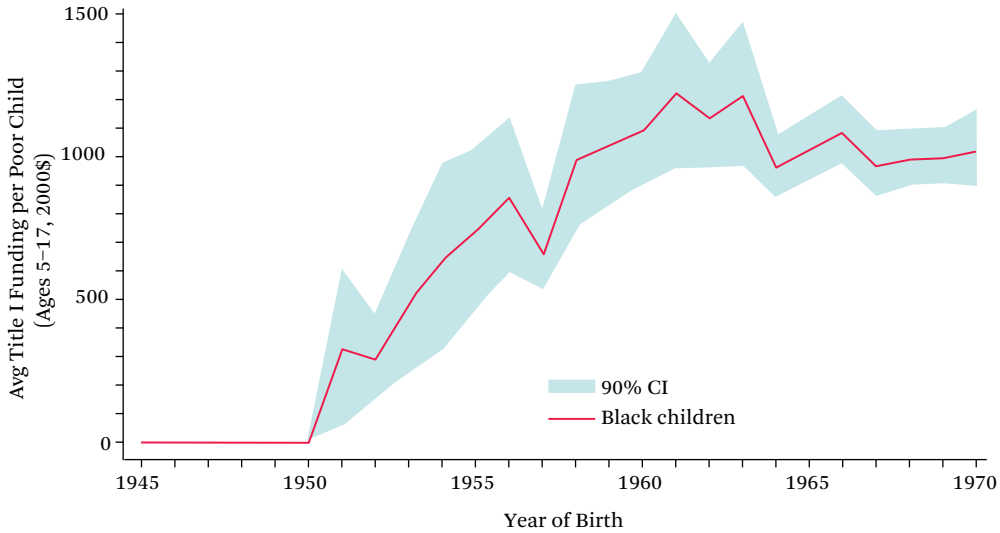


Source: Author’s calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA) covering 1965 to 1980 and county-level census counts of the number of children and number of poor children (Surveillance, Epidemiology, and End Results (SEER) data).

Figure 3. Birth Cohort Variation in Title I Funding per Poor Child, All Children



Source: Author’s calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA) covering 1965 to 1980 and county-level census counts of the number of children and number of poor children (Surveillance, Epidemiology, and End Results (SEER) data).

Figure 4. Birth Cohort Variation in Title I Funding per Poor Child, 90 Percent CI

Source: Author's calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA) covering 1965 to 1980 and county-level census counts of the number of children and number of poor children (Surveillance, Epidemiology, and End Results (SEER) data).

uated from high school,⁸ years of completed education; labor-market and economic-status outcomes (all in 2000 dollars)—wages, annual work hours, earnings, family income, and annual incidence of poverty in adulthood (ages thirty through forty); and whether ever incarcerated by age thirty-five. The child behavioral outcomes (ever repeated a grade, ever suspended or expelled) are important to examine in part because early manifestations of problem behavior in children have been shown to often be a risk factor for dropping out of high school and a precursor to more serious involvement in deviant behavior in adolescence and criminal involvement in adulthood. All analyses include men and women with controls for gender. Childhood family poverty status is assessed by matching a child's total family-income average in the period between ages twelve and seventeen with the corresponding poverty thresholds based on income and fam-

ily size. Summary statistics are presented in table 1.

Spells of incarceration are recovered from information on PSID respondents collected in each survey that includes whether a respondent was incarcerated at the time of the interview. This data on incarceration alone has limitations. Among the most important is that the survey identifies incarceration in a given year only if ongoing at the time of the interview. As a result, we are likely to miss individuals serving shorter sentences that did not coincide with the time of the interview. The 1995 wave added an education and crime-history module to the PSID, including several key questions I use to augment and obtain more precise information about the timing and duration of incarceration and minimize measurement error. In particular, information was collected for all adults in the 1995 wave on grade repetition, placement in gifted programs, whether respon-

8. High school graduate here refers to earning a high school diploma. Individuals earning GEDs are treated as high school dropouts here, following Heckman's work showing that the economic returns to GEDs are closer to that of dropouts than those who earn high school diplomas.

Table 1. Effects of Title I Funding on School District Spending

	Dependent Variable: Per-Pupil Spending, Average, Ages Five to Seventeen
County ESEA per-pupil spending, average ages five to seventeen	0.9976* (0.5647)
County ESEA per-pupil spending, average ages five to seventeen*(1960 county poverty rate - 45)	0.1292* (0.0777)
School district fixed effects?	yes
Race-specific year of birth fixed effects?	yes
Race*census division FE*year of birth?	yes
Childhood family and neighborhood controls?	yes
School desegregation controls?	yes
Other local/state/federal government expenditure programs?	yes
Number of individuals	6,817
Number of childhood families	1,920
Number of school districts	518

Source: Author's calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1955 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA) and school district per-pupil spending from the Individual Government Finances Database and Census of Governments.

Note: Robust standard errors in parentheses (clustered on childhood county).

Sample: PSID original sample children born 1955 to 1977 followed to adulthood.

* $p < .10$; ** $p < .05$; *** $p < .01$

dents had ever been expelled or suspended from school, been booked or charged with a crime, been placed in a juvenile correctional facility, or served time in jail or prison, as well as the number of times and the month and year of release as respectively applicable. For the adult incarceration outcome, the sample consists of PSID children born between 1950 and 1970 followed into adulthood who answered the criminal history questions in the 1995 wave of the PSID or were positively identified as incarcerated in any wave of the survey between 1968 and 2011.

I use the census block as the definition of neighborhood, which is a smaller geographic area than most previous studies use, and I match childhood residential address histories to blocks and school-district boundaries that prevailed in 1969 (the algorithm is outlined in the appendix).⁹ Each record is merged with

data on school spending for 1960 through 2000, the Title I funding information at the county level, neighborhood-level variables from the 1970, 1980, and 1990 census that corresponds with the prevailing levels during their school-age years. I use the census block or tract contained in the geocode file based on the 1968 residential location—the earliest available address in childhood (or county of birth when census block information is unavailable)—to avoid potential bias from endogenous residential mobility in response to Title I-induced school spending changes.

EMPIRICAL STRATEGY FOR ESTIMATING TITLE I EFFECTS ON ADULT OUTCOMES

The central aim of the empirical analysis is to investigate whether Title I funding, and resultant changes in school spending, have long-term impacts on adult outcomes. Particular at-

9. Many school districts were counties during this period, including more than half of southern school districts.

tention is given to determine whether the increased Title I funding experienced by children in lower-income communities had any lasting effects on their adult socioeconomic well-being. The empirical approach uses changes in Title I expenditures across cohorts from the same district, and differences in Title I expenditures among observationally similar children and families in different districts, experienced during one's school-age years, to isolate the effect of Title I as distinct from the effects of childhood poverty and other trends and coincident policies.

The main regression models used to analyze the impacts of Title I on the difference in adult attainment between treated and untreated cohorts involve estimating equations of the form

$$Y_{idb} = \beta_1 TitleISpend_{db} + \beta_2 TitleISpend_{db} * NonPoorKid_{idb} + X_{idb}\theta + Z_{db}\gamma + (W_{1960d} * b) \phi + \eta_d + \lambda_b^r + \varphi_g^r * b + \varepsilon_{idb}$$

where i indexes the individual, d the school district, b the year of birth, g the region of birth (defined by nine census division categories), and r the racial group. The measure of exposure to Title I funding is $TitleISpend_{5-17}$, the average per-pupil Title I expenditure in an individual's birth district during the individual's school-age years (ages five through seventeen). A doubling of this average can be interpreted as a doubling of Title I per-pupil spending for all twelve years of an individual's school career. In alternative specifications, I also examine the average Title I expenditures per poor child in the county during K-12 as the key explanatory variable. The rationale for this latter measure is that, if Title I funding is targeted toward resources and services for disadvantaged students within the district, then the effective school resources this funding supports would partly depend on how many of those students are in the district. No information is available on intradistrict resource allocation of Title I

funding toward specific school inputs nor on the extent to which it is targeted.

I test for differential Title I effects by childhood poverty status, as prior research has shown that children from low-income families may be more sensitive to changes in school quality and school-related interventions (such as the Tennessee Star class-size experiment) than children from more advantaged family backgrounds. Furthermore, because both residential mobility across counties and private school attendance are more common among children from affluent families than those from low-income ones, one might expect larger effects among children from low-income families.¹⁰ The equation includes school-district fixed effects (η_d), race-specific birth year fixed effects (λ_b^r), and race-by-region of birth cohort trends ($\varphi_g^r * b$), and it controls for an extensive set of child and childhood family-neighborhood characteristics (X_{idb}): parental education and occupational status, parental income, mother's marital status at birth, birth weight, child health insurance coverage, gender, neighborhood poverty, neighborhood racial composition, and neighborhood average-education level).

To account for the effect of the other coincident policies, I include county-by-birth-year measures of school desegregation, community health centers, and state funding for kindergarten, in addition to per capita expenditures on Head Start (at age four), and average childhood spending on food stamps, Aid to Families with Dependent Children (AFDC), Medicaid, and unemployment insurance (Z_{cb}) (for the data sources used to compile these measures, see Johnson 2011). To control for trends in factors hypothesized to influence Title I funding, the equation also includes interactions between 1960 characteristics of the county of birth and linear trends in the year of birth ($W_{1960d} * b$): 1960 county poverty rate, percent black, average education level, percent urban, and population size). Standard errors are all clustered at the childhood county level.

10. Prior research has demonstrated that though residential instability is significantly greater for poor families, and they experience intracounty moves more frequently, they most often move to neighborhoods of similar observable quality (Johnson 2009). Poor families are far less mobile, as measured by upward residential-mobility patterns, and less responsive to policy changes due to the greater residential location constraints they face.

Ideally, one would want information on Title I funding at the school or district level, but for this historical time period, per-pupil Title I funding and Title I funding per poor child at the county level are the most detailed measures one can construct from the available National Archives Record Administration (the only data source one can use to compile this information for this period). Although this limitation undoubtedly results in some measurement error, including school-district fixed effects and controls for detailed neighborhood characteristics will minimize potential bias. Moreover, many school districts were counties during this period, including more than half of southern school districts.

One potential parental response to existing school quality differences across public schools is to move to a different city or enroll children in a private school. Moreover, changes in how local school expenditures are financed may affect local residential-sorting patterns and property values. For example, it is possible that, instead of affecting learning per se, a school's Title I status may affect who enrolls and leaves a school. In that case, the estimated effects could be due to changes in the composition of students attending a school. This would constitute a true causal effect of Title I receipt on average educational outcomes, but obviously would have very different policy implications. If Title I services actually lead to improved educational outcomes, one would expect attrition rates (the main component of mobility rates) to be lower instead of higher in Title I schools. Because I did not want to include endogenous residential moves (that is, selective mobility of students to non-Title I schools during these years), I identi-

fied the neighborhood and school of upbringing based only on the earliest childhood address, 1968, which predates most of the major increases in Title I funding.¹¹ Because of this limitation, we can interpret the results as providing intention-to-treat estimates of the impacts of Title I school spending. The analysis aims to capture the quality of the public schools potentially attended by a given individual, rather than simply the quality of schools and classes actually attended; this approach also lessens measurement error and helps circumvent issues of endogeneity of both school choice and intradistrict resource allocation. In particular, I examine district-level measures of school resource inputs that reflect the quality of the overall school system available to an individual during their K-12 years, based on the district lived in that corresponds with the earliest residential address.¹² By using the earliest residential address of children, I minimize potential bias from endogenous residential mobility.¹³

RESULTS

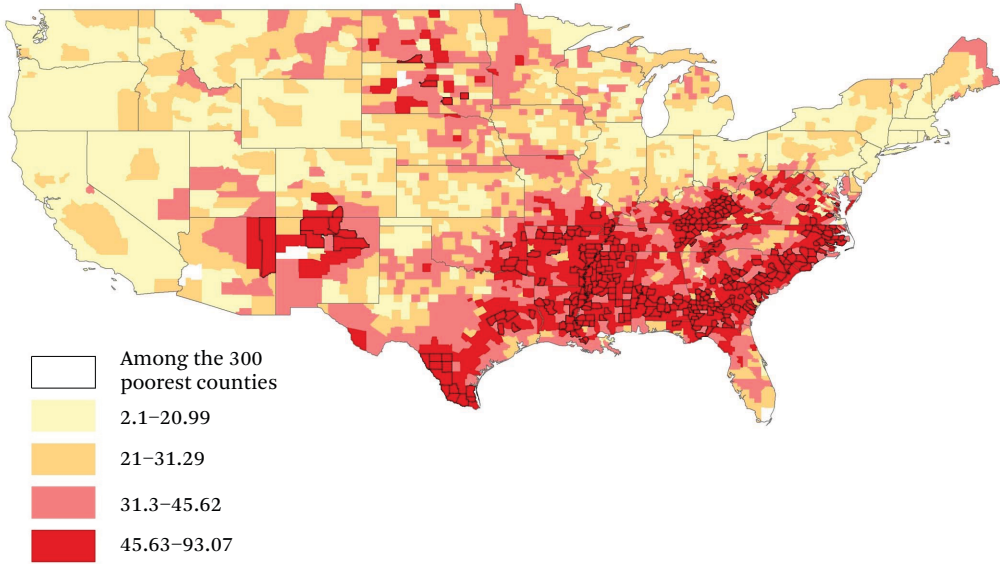
To put things in context and provide perspective before proceeding to the regression results, I present a set of descriptive statistics compiled for this historical period. In a series of maps, I first display the geographic variation in county-poverty rates in 1960, which highlights the substantial concentration of poverty in the South during the time period leading up to ESEA (figure 5). Figure 6 shows the geographic variation in the racial composition in 1960, which demonstrates the concentration of blacks in the South that overlaps the high-poverty counties. Geographic variation in Title

11. Among original sample children in the PSID, the average proportion of childhood spent growing up in the 1968 neighborhood was roughly 65 percent.

12. We recognize that classroom sizes and teacher characteristics vary even within districts and schools, and some children move across school systems, which will induce some measurement error. However, if this measurement error is of a classical variety, the resultant attenuation bias will lead us to understate the importance of school spending. Districts are not typically required to report school-level expenditures.

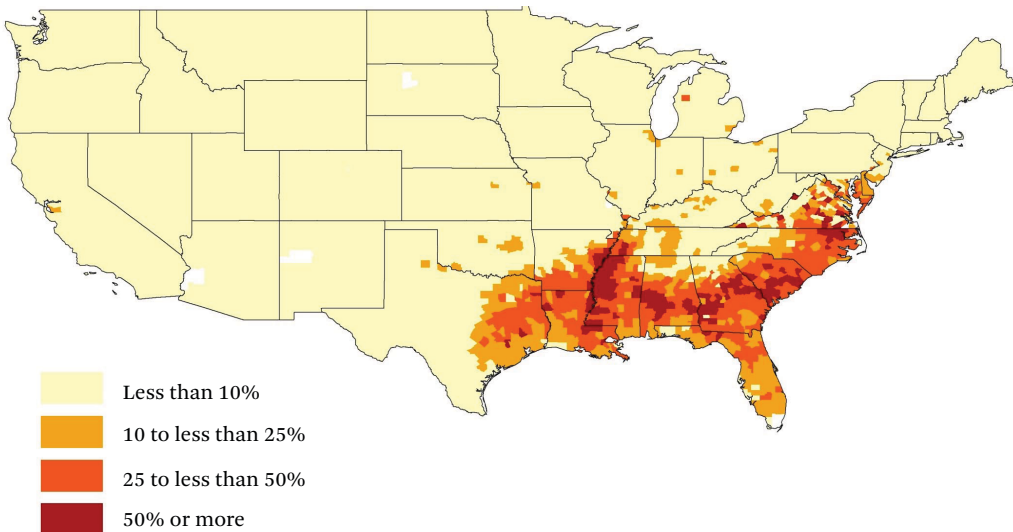
13. I find a similar pattern of results (with larger point estimates for Title I effects) among the subsample of cohorts born between 1963 and 1977 for whom the earliest residential address information used predates their school-age years. This suggests that endogenous residential mobility is not a significant source of bias and is not likely a factor that would result in an overstatement of Title I effects in this analysis.

Figure 5. U.S. County Poverty Rates in 1960

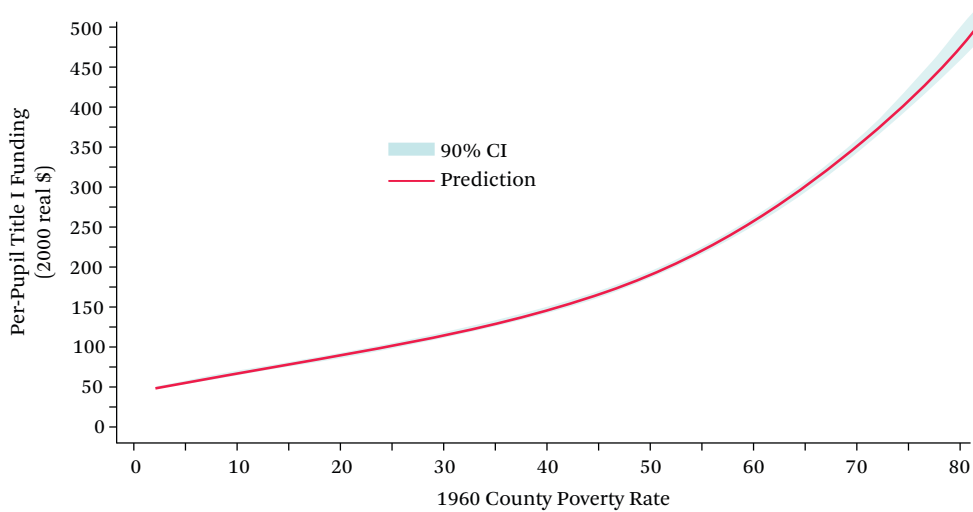


Source: Author's calculations based on county-level poverty rates for all U.S. counties in 1960 based on 1960 census data and data from the National Archives and Records Administration (NARA). Calculations verified from a study conducted by Jens Ludwig and Doug Miller (2007).

Figure 6. County Population: Percent African American, 1960



Source: Author's calculations based on county-level racial composition for all U.S. counties in 1960 based on 1960 census data.

Figure 7. Per-Pupil Title I Funding and 1960 County Poverty Rate

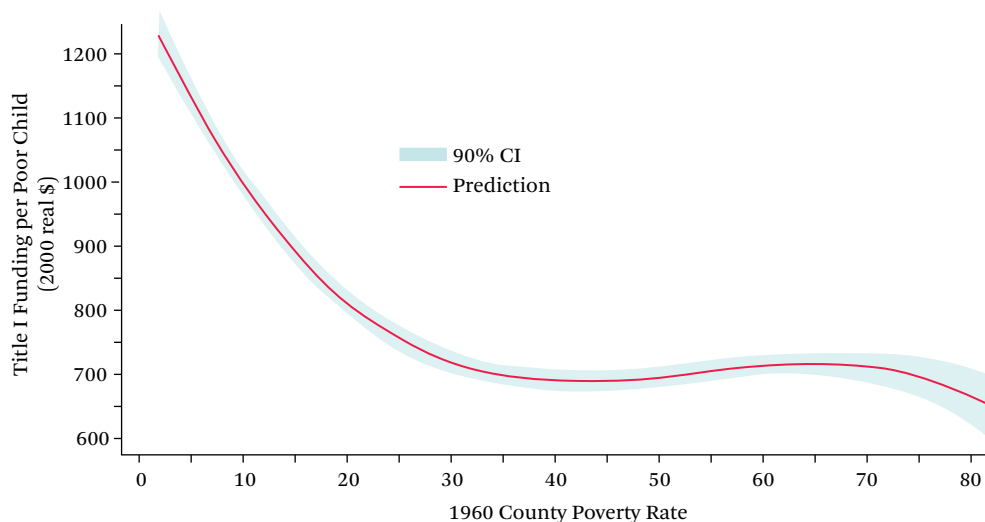
Source: Author's calculations based on annual school district data of the full universe of U.S. public school districts from the Individual Government Finances Database and Census of Governments, 1970–1980. These data are matched with county-level Title I funding information (NARA) and county-level demographic characteristics from census data.

Note: District spending and Title I funding CPI-U deflated in real 2000 dollars. Analysis sample includes 10,735 school districts; forty-five states. Results are based on regression models of per-pupil Title I funding on 1960 county poverty rates (quadratic specification allowing flexible functional form) that include year fixed effects as controls.

I funding and its increases in successive years between 1965 and 1980 is substantial, as shown in figures 1 through 4.

To investigate this variation further, I assembled annual school-district panel data for 1967 through 1990 (NBER 2003) matched with county-level Title I funding information (NARA) and other county characteristics that include the universe of public school districts in the United States (N=10,735 school districts across forty-five states). The district spending and Title I funding measures are CPI-U deflated in real 2000 dollars (for further details on data construction and sources, see appendix). Using these data, figures 7 and 8 present the strong relationship between 1960 county poverty rates and Title I funding that generates much of the geographic variation in Title I expenditures. Results are based on regression models of per-pupil Title I funding on 1960 county poverty rates (quadratic specification allowing flexible functional form) that include year fixed ef-

fects as controls to account for national time trends (figure 7). Per-pupil Title I funding increased rapidly with 1960 county poverty rates. In particular, although counties with 1960 poverty rates of less than 20 percent received less than \$100 per-pupil Title I funding annually on average between 1970 and 1980, those with rates in excess of 50 percent received more than \$200, and the poorest counties received \$500. Figure 8 presents the relationship between 1960 county poverty rates and Title I funding per poor child in the county using the same model. As shown, counties with 1960 county poverty rates in excess of 30 percent received \$700 Title I funding per poor child annually on average between 1970 and 1980. Title I funding per poor child decreases roughly linearly with 1960 county poverty rates for low- and moderate-poverty counties (that is, county poverty rates up to 25 percent), going from roughly \$1,200 for counties with less than 10 percent to \$800 for counties with 20 percent. These increases

Figure 8. Title I Funding per Poor Child and 1960 County Poverty Rate

Source: Author's calculations based on annual school district data of the full universe of U.S. public school districts from the Individual Government Finances Database and Census of Governments, 1970–1980. These data are matched with county-level Title I funding information (NARA) and county-level demographic characteristics from census data.

Note: District spending and Title I funding CPI-U deflated in real 2000 dollars. Analysis sample includes 10,735 school districts; forty-five states. Results are based on regression models of per-pupil Title I funding on 1960 county poverty rates (quadratic specification allowing flexible functional form), estimating the same model as figure 7 with Title I funding per poor child as the dependent variable.

are significant, given that the average total per-pupil spending in 1967 was about \$2,900 (in 2000 dollars). Whether per-pupil Title I spending or Title I spending per poor child is the most important measure to consider depends in part on the degree of targeting of Title I-funded resources to the most disadvantaged children within the district.

The descriptive summary statistics for the PSID sample are presented in table A1 (see appendix). Some additional descriptive statistics are drawn directly from congressional reports and surveys conducted in the first decade when Title I funding was first distributed (Wargo et al. 1972), supplemented with my own analyses:

In 1970, 13.5 percent of school-age children in the United States were from families with incomes below the poverty line; moreover, 39 percent of all black school-age children were poor but only 9.5 percent of all white

children were. These rates were substantially higher in the South.

As of 1969, the end of the study period, minority children made up 20 percent of public school enrollment. However, 77 percent of all black students were enrolled in schools with minority concentrations above 50 percent and 98 percent of white children attended schools with minority proportions below 49 percent.

More black (24.2 percent) than white children (16.5 percent) were achieving below grade level in one or more subjects, and fewer blacks (65 percent) than whites (74 percent) were at grade level. Although 14 percent of all elementary school children had severe reading problems, 20 percent of children from low-income families and 25 percent of students enrolled in large urban schools had such problems.

Descriptive statistics on the Title I program's operational context follow:

Poor children tended to be enrolled in a relatively few large districts that had low to moderate regular per-pupil expenditures and high concentrations of low-income children.

As of 1969, the majority of low-income children were enrolled in Title I schools, 90 percent of them concentrated in districts with low (under \$1,885) to moderate (\$1,885 to \$2,770) regular per-pupil expenditures (2000 dollars), and 68 percent were enrolled in 12 percent of the Title I participating districts.

In 1968, 29 percent of children enrolled in Title I elementary schools were from minority groups. Within Title I elementary schools, 83 percent of students were assigned to classrooms in which 90 percent or more of the children were of one race, white or black. Only 17 percent were in classrooms where the racial composition corresponded to that of the student population in Title I schools, and even fewer were in classrooms with a racial composition corresponding to national standards for integration (Wargo et al. 1972). In stark contrast, only 0.2 percent of white children in Title I elementary schools were enrolled in classrooms with a 90 percent or higher concentration of blacks.

The extensive segregation by race and class reflected by these descriptive statistics was partially due to a tendency to group minority and poor children by ability in classrooms, and white children and children from more economically advantaged families by subject.

Approximately 75 percent of children attended schools with low concentrations of poor children, but 9 percent were in schools where more than half of children were from poor families, and these were disproportionately located in large urban cities.

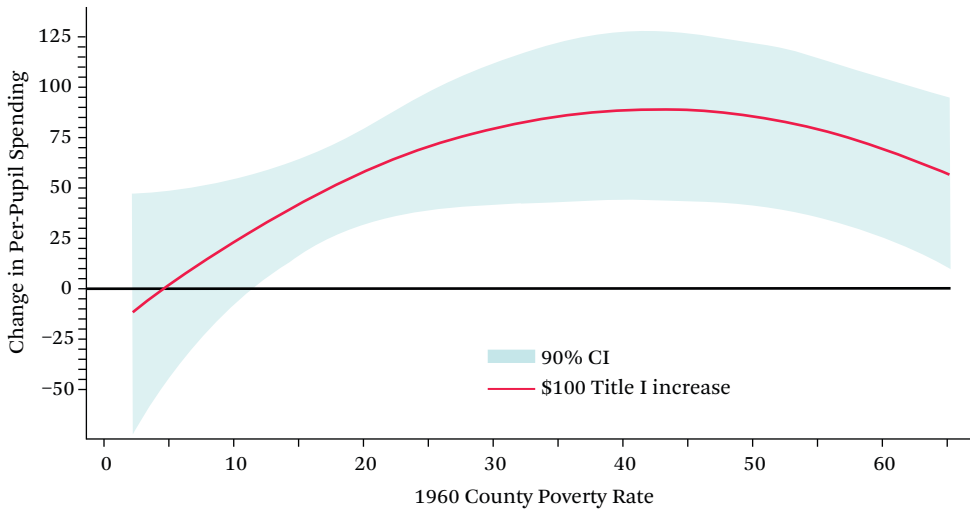
Between 1966 and 1969, participation intensity in compensatory education programs averaged about only one hour per day (Wargo et al. 1972).

REGRESSION RESULTS

Figure 9 presents estimated effects of per-pupil Title I funding on per-pupil spending using panel data of the universe of school districts. The model includes an interaction term between per-pupil Title I funding and the 1960 county poverty rate to test for differential responsiveness of districts to a given increase in Title I depending on the district's poverty level, using school-district fixed effects and year fixed effects as controls. Figure 9 highlights the differential effect of a \$100 dollar increase in per-pupil Title I funding on per-pupil spending by county poverty rate, wherein we find significant crowd-out for low- and moderate-poverty counties. In contrast, for high-concentrated poverty areas, particularly in the range of 35 to 55 percent poverty, there is only limited crowd-out and every additional Title I dollar translates into between \$0.75 to \$1 in additional total school district spending. For example, the results indicate that a \$100 dollar increase in per-pupil Title I funding leads to an \$88 dollar increase in total per-pupil spending for county poverty rates of 40 to 50 percent, a \$79 increase for county poverty rate of 30 percent, a \$59 increase for poverty rate of 20 percent, but only a (statistically insignificant) \$26 increase for a county with a 10 percent rate. Thus, on average, not only did higher concentrated poverty areas receive more Title I funds, but they also experienced less crowd-out, which led to larger increases in total district spending for a given Title I funding increase.

Table 1 presents results of a similar analysis of the estimated effects of Title I on average per-pupil spending between the ages of five and seventeen using the subset of districts that overlap the PSID sample. The pattern of results is similar. Despite significant increases in Title I funding, they led to very modest increases in per-pupil spending in low- and moderate-poverty districts due to significant crowd-out of local revenue for public schools. However, in high-concentrated poverty areas, a \$100 increase in per-pupil Title I funding is associated with about a \$100 increase in average per-pupil spending, which corresponds to roughly a 3 percent increase for a county with a 45 percent poverty rate. I find that per-pupil spending increased by about twice as much per dollar of

Figure 9. Effect of \$100 Increase in Per-Pupil Title I Funding on Per-Pupil Spending by 1960 County Poverty Rate



Source: Author's calculations based on annual school district data of the full universe of U.S. public school districts from the Individual Government Finances Database and Census of Governments, 1970–1980. This data is matched with county-level Title I funding information (NARA) and county-level demographic characteristics from census data.

Note: District spending and Title I funding CPI-U deflated in real 2000 dollars. Analysis sample includes 10,735 school districts; forty-five states. Results are based on regression model of district per-pupil spending on per-pupil Title I funding interacted with 1960 county poverty rates (quadratic specification allowing flexible functional form) that include school district fixed effects and year fixed effects as controls.

federal revenue in both high-poverty districts and high black-enrollment-share districts, due to less local offset. Federal guidance on how school districts are to use Title I funds is imprecise, especially during the first twenty years after ESEA was enacted. Over time, local guidance on the use of Title I funds has become far more specific, and the degree to which Title I funds are restricted varies by district. Title I did not impose a student performance requirement in its original 1965 enactment, and the requirement was introduced in 1994 and was not fully implemented until the No Child Left Behind Act (NCLB) of 2001. In the current Title I policy context within school accountability systems, even if being a Title I school does not lead to a significant increase in average per-pupil expenditures, the Title I program itself could still have an effect, as it may put restrictions on the minimum amount of resources to be spent on low-achieving students, on the way

it is spent, and also makes the school accountable for its students' achievements.

ESTIMATED EFFECTS ON LONGER-RUN OUTCOMES

Educational Attainment

Differences are large and significant in educational attainment by child poverty status, as evidenced in the summary statistics: the high school graduation rate among poor children in the sample was 0.63 but among nonpoor children 0.87; poor children completed 11.9 years of education and nonpoor children 13.4 years on average (table A1). Tables 2 and 3 present the results for the effects of Title I funding on the likelihood of high school graduation and years of completed education, respectively. The results indicate that a \$100 increase in per-pupil Title I funding experienced throughout one's K–12 years (which is

Table 2. Effects of Title I Funding on the Likelihood of Graduating from High School

	Dependent Variable: Prob(HS Graduate)		
County Title I per-pupil spending (00s), average ages five to seventeen	0.0225** (0.0093)		
County Title I per-pupil spending (00s), average ages five to seventeen*poor child		0.0533*** (0.0164)	
County Title I per-pupil spending (00s), average ages five to seventeen*nonpoor child		0.0087 (0.0096)	
County Title I spending per poor child (00s), average ages five to seventeen*poor child			0.0121*** (0.0036)
County Title I spending per poor child (00s), average ages five to seventeen*nonpoor child			0.0008 (0.0013)
School district fixed effects?	yes	yes	yes
Race-specific year of birth fixed effects?	yes	yes	yes
Race*census division FE*year of birth?	yes	yes	yes
Childhood family and neighborhood controls?	yes	yes	yes
School desegregation controls?	yes	yes	yes
Other local/state/federal government expenditure programs?	yes	yes	yes
Number of individuals	7,035	7,035	7,035
Number of childhood families	2,171	2,171	2,171
Number of school districts	563	563	563

Source: Author's calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA).

Note: Robust standard errors in parentheses (clustered on childhood county).

* $p < .10$; ** $p < .05$; *** $p < .01$

the average increase across districts and also represents a standard deviation change) is associated with a 2.2 percentage point increase in the likelihood of graduating from high school (table 2, column 1) and a 0.25 increase in completed years of education (table 3, column 1) on average among all children. The estimated effects are both statistically significant at the 0.05 level. Furthermore, we find statistically significant differences in these effects for the likelihood of high school graduation by childhood poverty status; effects are more pronounced for poor children. In particular, for poor children the estimated effect of a \$100 increase in per-pupil Title I funding leads to a 5.3 percentage point increase in the likelihood of graduating from high school. Title I spending has a negligible and statistically insignificant effect for nonpoor children,

however (table 2, column 2). The estimated effects of per-pupil Title I funding for poor and nonpoor children are similar for years of education (table 3, column 2).

The results indicate that for poor children, a \$1,000 increase in Title I funding per poor child (a standard deviation change, the average increase across districts being \$800) is associated with a 12.1 percentage point increase in the likelihood of high school graduation and 0.6 more years of completed education, all of which are statistically significant at the 0.01 level (tables 2 and 3, column 3). The corresponding effects for nonpoor children are roughly half the magnitude found for poor children on years of education, and the effects for non-poor children are small and insignificant for high school graduation (tables 2 and 3, column 3).

Table 3. Effects of Title I Funding on Educational Attainment

	Dependent Variable: Years of Education		
County Title I per-pupil spending (00s), average ages five to seventeen	0.2459** (0.0531)		
County Title I per-pupil spending (00s), average ages five to seventeen*poor child		0.2763*** (0.0680)	
County Title I per-pupil spending (00s), average ages five to seventeen*nonpoor child		0.2225*** (0.0557)	
County Title I spending per poor child (00s), average ages five to seventeen*poor child			0.0600*** (0.0151)
County Title I spending per poor child (00s), average ages five to seventeen*nonpoor child			0.0297*** (0.0107)
School district fixed effects?	yes	yes	yes
Race-specific year of birth fixed effects?	yes	yes	yes
Race*census division FE*year of birth?	yes	yes	yes
Childhood family and neighborhood controls?	yes	yes	yes
School desegregation controls?	yes	yes	yes
Other local/state/federal government expenditure programs?	yes	yes	yes
Number of individuals	7,035	7,035	7,035
Number of childhood families	2,171	2,171	2,171
Number of school districts	563	563	563

Source: Author’s calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA).

Note: Robust standard errors in parentheses (clustered on childhood county).

* $p < .10$; ** $p < .05$; *** $p < .01$

Other School-Related Outcomes

To examine intermediate educational outcomes leading up to high school graduation, looking beyond overall years of education reveals a similar pattern of significant results for the likelihood of grade repetition and other school-related outcomes. Among poor children, 20 percent repeated a grade at some point, and 28 percent were suspended or expelled from school. Among nonpoor children, 11 percent were held back and 17 percent were suspended or expelled. The results presented in table 4 indicate that a \$100 increase in per-pupil Title I funding leads to a 2 percentage point reduction in the likelihood of grade repetition (column 1, marginally significant), a 2 percentage point increase in the likelihood of

placement in an advanced or gifted class (column 2), and roughly a 2 percentage-point reduction in the likelihood of ever being suspended or expelled from school (column 3, marginally significant). These results are particularly noteworthy because, as mentioned, grade repetition and suspension or expulsion from school are often early antecedents to high school dropout and behavior problems. They are also key risk factors for subsequent criminal involvement among individuals raised in high-poverty, high-crime neighborhoods. The final column of table 4 shows that a \$100 increase in per-pupil Title I funding is significantly associated with a 0.44 percentage-point reduction in the likelihood of ever being incarcerated by age thirty-five, which corre-

Table 4. Effects of Title I Funding on Other School-Related Outcomes

	Dependent Variable:			
	Prob(Ever Grade Repetition)	Prob(Ever Placed in Gifted Program)	Prob(Ever Suspended/ Expelled)	Prob(Ever Incarcerated) by Age Thirty-Five
County Title I per-pupil spending (00s), average ages five to seventeen	-0.0197* (0.0138)	0.0196* (0.0118)	-0.0196* (0.0127)	-0.0044* (0.0025)
School district fixed effects?	yes	yes	yes	yes
Race-specific year of birth fixed effects?	yes	yes	yes	yes
Race*census division FE*year of birth?	yes	yes	yes	yes
Childhood family and neighborhood controls?	yes	yes	yes	yes
School desegregation controls?	yes	yes	yes	yes
Other local/state/federal government expenditure programs?	yes	yes	yes	yes
Number of individuals	5,715	5,715	5,715	9,446
Number of childhood families	1,939	1,939	1,939	2,415
Number of school districts	538	538	538	589

Source: Author's calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA).

Note: Robust standard errors in parentheses (clustered on childhood county).

* $p < .10$ (one-tailed test); * $p < .10$ (two-tailed test); ** $p < .05$; *** $p < .01$

sponds with about a 10 percent reduction in the risk, on average, for poor children. Recall that a \$100 increase in per-pupil Title I funding corresponds with about an \$800 increase in Title I funding per poor child for the average district.

Labor Market Outcomes, Adult Family Income, and Poverty Status

As with educational achievement, differences are stark in adult economic attainments by childhood poverty status. Among poor children, the average family income at age thirty was \$33,169 and 26 percent were in poverty at age thirty. Among nonpoor children, the average family income at age thirty was \$48,736 and only 5 percent were in poverty at age thirty (see table A1). The next series of results reveals modest, but significant, effects of Title I funding on children's subsequent adult economic status and labor market outcomes, using the

same model specifications. As shown in table 5, the results indicate that a \$100 increase in per-pupil Title I funding throughout the school-age years leads to a 6.1 percent increase in adult wages, 49 additional annual work hours, 7.9 percent increase in annual labor market earnings, a 5.5 percent increase in annual family income, and a 1.7 percentage-point reduction in the annual incidence of adult poverty at ages thirty to forty. All but one of the estimated effects are statistically significant at the 0.05 level and many at the 0.01 level.

Table 6 presents the estimated effects of Title I funding per poor child by child poverty status across the main socioeconomic outcomes considered. The broad pattern is that the estimated effects of Title I funding per poor child are between one and a half and two times larger for poor children than for nonpoor children on years of education, likelihood of place-

Table 5. Effects of Title I Funding on Adult Economic Outcomes

	Dependent Variable:				
	Ln(Wage), Ages Thirty to Forty	Annual Work Hours (Include Os), Ages Thirty to Forty	Ln(Annual Earnings), Ages Thirty to Forty	Ln(Annual Family Income), Ages Thirty to Forty	Annual Incidence of Poverty, Ages Thirty to Forty
County Title I per-pupil spending (00s), average ages five to seventeen	0.0610*** (0.0204)	49.1535* (29.7015)	0.0793*** (0.0292)	0.0553** (0.0269)	-0.0168*** (0.0062)
School district fixed effects?	yes	yes	yes	yes	yes
Race-specific year of birth fixed effects?	yes	yes	yes	yes	yes
Race*census division FE*year of birth?	yes	yes	yes	yes	yes
Childhood family and neighborhood controls?	yes	yes	yes	yes	yes
School desegregation controls?	yes	yes	yes	yes	yes
Other local/state/federal government expenditure programs?	yes	yes	yes	yes	yes
Number of person-year observations	30,979	36,389	31,095	36,948	37,079
Number of individuals	4,734	4,932	4,740	4,966	4,970
Number of childhood families	1,903	1,929	1,903	1,931	1,933
Number of school districts	509	517	509	518	518

Source: Author’s calculations based on nationally-representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA).

Note: Robust standard errors in parentheses (clustered on childhood county).

* $p < .10$; ** $p < .05$; *** $p < .01$

ment in a gifted program, likelihood of incarceration, and adult wages. For example, the results indicate that, on average, a \$1,000 increase in Title I funding per poor child during the school-age years is associated with a 10.8 percent increase in adult wages and a 5.8 percent increase for nonpoor children (table 6, column 6).

Discussion of Magnitudes in Perspective

Given the small increase—between \$100 and \$400 per student—in school spending, what is a reasonable expectation of the effect of Title I? A few caveats on how to interpret the magnitudes of the estimates are important. Given the lack of information on the extent of targeting of Title I programs, the effects of Title I

funding on treated students remains unclear. That is, whether one considers the estimated effects as large or small would ideally be informed by the extent to which funds are targeted within schools. For example, if funds are targeted to 20 percent of students, expected effects should be five times as large. Targeting issues, and the effective progressivity of school spending, include not only whether resources are targeted to poor schools within districts, but also whether they are targeted to educationally disadvantaged students within schools. Early reports of blatant misappropriation of funds and large-scale violations in the operation of the program are referenced in early reports.

The precise interpretation of the results de-

Table 6. Effects of Title I Funding on Socioeconomic Outcomes, by Child Poverty Status

	Prob (HS Graduate)	Years of Education	Prob(Ever Placed in Gifted Program)	Prob(Ever Suspended/ Expelled)	Prob(Ever Incarcerated) by Age Thirty-Five	Ln(Wage), Ages Thirty to Forty
County Title I spending per poor child (00s), average ages five to seventeen*poor child	0.0121*** (0.0036)	0.0600*** (0.0151)	0.0078** (0.0032)	-0.0089*** (0.0031)	-0.0016* (0.0009)	0.0108* (0.0062)
County Title I spending per poor child (00s), average ages five to seventeen*nonpoor child	0.0008 (0.0013)	0.0297*** (0.0107)	0.0038** (0.0017)	-0.0013 (0.0024)	-0.0010*** (0.0002)	0.0058* (0.0034)
School district fixed effects?	yes	yes	yes	yes	yes	yes
Race-specific year of birth fixed effects?	yes	yes	yes	yes	yes	yes
Race*census division FE*year of birth?	yes	yes	yes	yes	yes	yes
Childhood family and neighborhood controls?	yes	yes	yes	yes	yes	yes
School desegregation controls?	yes	yes	yes	yes	yes	yes
Other local/state/federal government expenditure programs?	yes	yes	yes	yes	yes	yes
Number of person-year observations	—	—	—	—	—	30,979
Number of individuals	7,035	7,035	5,715	5,715	9,446	4,734
Number of childhood families	2,171	2,171	1,939	1,939	2,415	1,903
Number of school districts	563	563	538	538	589	509

Source: Author's calculations based on nationally representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA).

Note: Robust standard errors in parentheses (clustered on childhood county).

* $p < .10$ (two-tailed test); ** $p < .05$; *** $p < .01$

depends on the source and nature of the variation in a school's Title I status over time and its links with the distribution of school budget and expenditure patterns in these years. One of the lessons from decades of education evaluation research on the effectiveness of school reforms is that the how matters more than the what—that is, how programs are implemented must be carefully considered and understood. That concept applies for the context of this study as well, though a lack of data availability prohibits focus on these issues. Decisions on how the money is spent are left to local governance. There is no single Title I treatment effect because of the diverse ways in which federal spending was used and the program implemented across districts. The results section discussed the average effects, but substantial heterogeneity is likely in treatment effects across geographic areas because of the diverse ways in which Title I funding was implemented. Relatively small samples prohibit exploring this heterogeneity in detail in this study given substantial precision issues and the lack of available information on how funds were used.¹⁴

An alternative way to gauge the magnitudes of Title I effects is to consider them alongside impacts of other major K–12 education interventions, such as class size reductions. For example, the Project Star experiment (a randomized experiment investigating the effects of small class size) cost about \$3,800 per student. This large-scale experiment reduced class size by seven students from a base of twenty-two for several early-elementary grades and increased student achievement by between 0.2 and 0.25 standard deviations. A reasonable starting benchmark may be to assume that effects are linear in program costs, so Title I effects may be slightly less than one-tenth of Project Star's effects. Following this logic, one may reasonably expect effects of roughly a 0.7 reduction in student-teacher ratios, and a 0.02

to a 0.025 increase in test scores—a very small effect indeed.

Other recent studies use quasi-experimental designs to analyze the impacts of school inputs and school spending on long-run outcomes (see Jackson, Johnson, and Persico 2015; Chetty et al. 2011; Fredriksson, Ockert, and Oosterbeek 2013). Raj Chetty and his colleagues (2011) report that Project Star students who were randomly assigned to a kindergarten teacher with more than ten years of experience earn 6.9 percent higher income on average at age twenty-seven relative to students with less experienced teachers. They also find that assigning students to a classroom that is one standard deviation better than average in kindergarten (where class quality captures the combined influences of peer effects, teacher effects, and all other classroom characteristics that affect test scores) generates a 9.6 percent increase in annual earnings at age twenty-seven. Kirabo Jackson, Rucker Johnson, and Claudia Persico (2015), using evidence from court-ordered school finance reforms, find that, for children from low-income families, a 10 percent increase in per-pupil spending throughout one's K–12 years leads to 0.46 additional years of completed education, 9.6 percent higher earnings, and a 6.1 percentage-point reduction in the annual incidence of adult poverty. In light of the fact that these reforms generated substantially larger changes in spending and school inputs than Title I, and back-of-the-envelope calculations, the results on long-run outcomes presented here may be viewed as sizeable.¹⁵

That the Title I effects on long-term attainment outcomes documented here are larger than previous effects highlights the importance of evaluating educational interventions. It also raises concerns about accountability policies that rely exclusively on test scores, because long-run analyses of educational interventions may yield very different conclusions

14. I explored heterogeneous effects by 1960 county poverty rate, initial level of local revenue, region (South versus non-South), and race, but small sample sizes resulting in significant precision issues failed to produce useful evidence along these lines.

15. I thank Jordan Matsudaira (Cornell) for helpful discussions about alternative ways of thinking through the magnitudes of the estimated Title I effects.

than short-run analyses that focus on test scores.

Other Unresolved Issues and Directions for Future Research

A dynamic analysis of whether a state responds to its poor districts' receipt of large Title I grants by redirecting money away from education aid in poor districts toward welfare (public assistance), criminal justice, health care, or tax reduction could have implications for educational outcomes. Such analysis could provide greater insight into the dynamics of the flypaper effect and shed light on the form the crowd-out responses to Title I might take and how they affect educational outcomes.

The results presented here suggest that the incidence of benefits and costs of Title I-induced changes in school expenditure patterns favor children from poor families. One unanswered question is whether this relationship operates similarly or differently by race. Other related questions include how the effectiveness of Title I is related to the way Title I funds are spent, whether participation in Title I narrows educational achievement differences between program participants and (otherwise similar) nonparticipants, and whether this gap would widen without the existence of Title I services (that is, whether Title I students learn more than they would have without Title I).

CONCLUSION

This article provides fresh evidence on the long-term effects of school spending from Title I on children's subsequent educational and adult economic outcomes, focusing on the first fifteen years of the roll-out of Title I. To isolate the effect of Title I on long-run adult outcomes, all models include controls for school-district fixed effects, race- and region-specific birth-cohort trends, an extensive set of childhood, family, and neighborhood characteristics, and other coincident policies (such as desegregation and War on Poverty initiatives and related safety-net programs). I find that increases in Title I funding are significantly related to increases in the likelihood of graduating from high school, reductions in both the likelihoods of grade repetition and school suspension or expulsion, more years of completed education,

higher earnings and work hours, a reduction in the annual incidence of poverty in adulthood (ages thirty to forty), and a reduction in the likelihood of ever being incarcerated by age thirty-five. The effects on educational outcomes are more pronounced for children from poor families. Although the magnitudes of these effects are modest, they are economically important and noteworthy because a significant amount of Title I funding led to only modest increases in school district spending due to crowd-out of local funding.

One of the factors undermining the effectiveness of ESEA is that it crowds out local provision of school funding (Gordon 2004). This suggests an alternative matching-funds school-finance formula design that rewards local effort so that overall spending in poor districts is increased and per-pupil spending between poor and affluent districts narrowed. Such formulas are effective because they affect taxes directly, allowing such districts to have more than a dollar in spending for each dollar raised in taxes. The lesson from state school finance reform is that design features are central to effectiveness both in narrowing disparities in spending and in improving the long-term educational and adult economic outcomes (Jackson, Johnson, and Persico 2015).

Today, a majority of the per-pupil spending disparities by socioeconomic status occur between states rather than between districts within a state. This is in large measure due to the role of court-mandated school finance reforms and legislative reforms at the state level (Jackson, Johnson, and Persico 2014). On a federal level, additional questions remain related to the efficiency of fifty fragmented state school finance systems.

The effectiveness of school spending is a perennial issue in education policy and has spawned a large literature and contentious debate. Concerns that school spending inequalities undermine the provision of equal educational opportunities fueled the initial passage of ESEA in 1965 and movement toward school finance reform litigation and legislation over the past several decades. Money alone may not be enough, but provision of adequate and equitable distribution of spending is a necessary condition. These efforts may need to be ad-

joined with accountability systems that help ensure spending is allocated to its most productive uses to narrow gaps in educational opportunity, which may be key sources of the growing socioeconomic status gaps in student achievement. Other nonmonetary factors may also influence school quality, such as school accountability policies, curricular standards, role of tracking and ability grouping, improving incentives in schools, and competition between schools for students.

One of the main factors that may moderate the influence of school spending on student outcomes is how the money is spent. An unresolved question and high-priority issue for future research concerns the relative efficacy of specific school resource inputs that the marginal dollar of targeted educational spending can determine, such as smaller classes versus higher teacher salaries versus capital or building expenditures and other spending categories. Arguably, ensuring a quality education for all students has never been more important given the substantial increases in the labor market returns to skills. A collage of recent evidence paints a collective picture that, with well-designed and targeted incentives, Title I alongside school finance reform policies can be a part of the solution to reduce the intergenerational transmission of poverty.

APPENDIX

Data on Title I Funding

I compiled administrative data about federal outlays for Title I acquired from the National Archives and Records Administration (NARA) for 1965 through 1980. The information (of historical annual county-level Title I funding) was culled from NARA records by searching program titles and program codes. I identified the pool of grants potentially for ESEA (included string searches in ESEA grant titles). For most records, ESEA spending are listed by community and funding, and information on “stock” of programs at a particular time allows verification of accuracy of grant flows. Historical annual data of county-level age-specific population counts from 1965 to 2000 from SEERS data are used to put the county-level federal ESEA program outlay measures in per-pupil terms.

To verify that the Title I spending information I compiled was accurate, I was able to successfully match the NARA data to published figures for total (ESEA) federal spending and Title I spending at the national and state levels.

Although data on school-level Title I spending are unavailable (because districts are not required to report intradistrict resource allocation) and the use of county-level Title I expenditures leads to some measurement error, no other data source or analysis has investigated long-run impacts or used data at this level of geographic detail, particularly for this historical period.

Matching PSID Individuals to Their Childhood School Districts

Using GIS mapping techniques, I was able to match childhood addresses to the school district boundaries that prevailed in 1969 (to avoid complications arising from endogenously changing district boundaries over time). I obtained addresses for the full universe of elementary and secondary schools in the United States in 1969 along with a geocoded match file of district boundaries and census tract codes. I have done this in my prior work that examines the long-run effects of school desegregation, school finance reforms, and Head Start (separate papers).

To limit the possibility that school district boundaries were drawn in response to pressure for SFRs, we use 1969 school district geographies. The “69-70 School District Geographic Reference File” (U.S. Bureau of Census 1970) relates census tract and school district geographies. For each census tract in the country, it provides the fraction of the population that is in each school district. Using this information, I aggregated census tracts to 1970 district geographies with Geographic Information Systems (GIS) software. I assigned census tracts from 1960, 1980, and 1990 to school districts using this resulting digital map based on their centroid locations.

To construct demographic information on 1969–1970 school districts, I compiled census data from the tract, place, school district and county levels of aggregation for 1960, 1970, 1980, and 1990. I constructed digital (GIS) maps of 1970 geography school districts using the

Table A1. Descriptive Statistics by Childhood Poverty Status

	All (N=7,182)	Poor (N=1,572)	Nonpoor (N=5,610)
Adult Outcomes			
High school graduate	0.84	0.63	0.87
Years of education	13.29	11.87	13.44
Ever repeated a grade	0.11	0.20	0.11
Ever suspended/expelled	0.17	0.28	0.17
Ever placed in gifted program	0.19	0.09	0.20
Ln(wages), at age thirty	2.60	2.13	2.64
Annual work hours (includes 0s), at age thirty	2,112	1,858	2,132
Adult family income, at age thirty	\$47,605	\$33,169	\$48,736
In poverty, at age thirty	0.07	0.26	0.05
Ever incarcerated, by age thirty-five	0.02	0.04	0.01
Year born (range: 1950–1977)	1963	1962	1963
Female	0.52	0.57	0.51
Black	0.12	0.50	0.08
Childhood school variables			
Per-pupil spending average ages five to seventeen	\$4,187	\$3,739	\$4,232
Per-pupil Title I funding average ages five to seventeen	\$96	\$128	\$93
Title I funding per poor child average ages five to seventeen	\$805	\$744	\$812
1960 district poverty rate (%)	22.49	33.80	21.31
Childhood family variables			
Income-to-needs ratio average ages twelve to seventeen:	3.12	0.71	3.37
Mother's years of education	12.05	11.32	12.66
Father's years of education	12.05	10.91	12.93
Born into two-parent family	0.89	0.61	0.92
Low birth weight (<5.5 pounds)	0.07	0.08	0.06
Childhood neighborhood variables			
Neighborhood poverty rate	0.11	0.16	0.10
Residential segregation dissimilarity index _{county}	0.72	0.71	0.72

Source: Author's calculations based on nationally representative data from the Panel Study of Income Dynamics (PSID) of original sample children born between 1950 and 1977 followed into adulthood (1968–2011), matched with administrative data of county-level Title I funding from the National Archives and Records Administration (NARA) and school district per-pupil spending from the Individual Government Finances Database and Census of Governments.

Note: All descriptive statistics are sample weighted to produce nationally representative estimates of means. Dollars are CPI-U deflated in real 2000 dollars.

1969–1970 School District Geographic Reference File from the Census. This file indicates the fraction by population of each census tract that fell in each school district in the country. Those tracts split across school districts I allocated to the school district comprising the

largest fraction of the tract's population. Using the resulting 1970 central school district digital maps, I allocated tracts in 1960, 1980, and 1990 to central school districts or suburbs based on the locations of their centroids. The 1970 definition central districts located in regions not

tracted in 1970 all coincide with county geography that I use instead.

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