The Effect of School Finance Reforms on the Distribution of Spending, Academic Achievement, and Adult Outcomes

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ABSTRACT

The school finance reforms (SFRs) that began in the early 1970s and accelerated in the 1980s caused some of the most dramatic changes in the structure of K–12 education spending in U.S. history. We present an analysis of the effects of these reforms on the level and distribution of school district spending, as well as their impacts on subsequent educational and economic attainment outcomes.

In Part One, using a newly compiled database of school finance reforms and a recently available long panel of annual school district data on per-pupil spending that spans the period 1962–2010, we present an event-study analysis of the effects of different types of school finance reforms on per-pupil spending in low- and high-income school districts. We find that SFRs have been instrumental in equalizing school spending between low- and high-income districts and many reforms do so by raising spending for poor districts. While all reforms reduce spending inequality, there are important differences by reform type: adequacy-based court-ordered reforms increase overall school spending, while equity-based court-ordered reforms reduce the spread of spending with little effect on overall levels; reforms that entail high tax prices (the amount of taxes a district must raise to increase spending by one dollar) reduce long-run spending for all districts, and those that entail low tax prices lead to increased spending growth, particularly for low-income districts.

In Part Two, we link the spending and reform data to detailed, nationally representative data on children born between 1955 and 1985 and followed through 2011 (the Panel Study of Income Dynamics) to study the effect of the reform-induced changes in school spending on long-run adult outcomes. These birth cohorts straddle the period in which most of the major school finance reform litigation accelerated, and thus the cohorts were differentially exposed, depending on place and year of birth. We use the timing of the passage of court-mandated reforms as an exogenous shifter of school spending across cohorts within the same district. Event-study and instrumental variable models reveal that that while there are no effects for children from non-poor families, a twenty percent increase in per-pupil spending each year for all 12 years of public school for children from poor families leads to 0.876 more years of schooling, 20.5 percent higher earnings, and an 10.7 percentage point reduction in the annual incidence of adult poverty. The magnitudes of these effects are sufficiently large to eliminate between two-thirds and all of the gaps in these adult outcomes between those raised in poor families and those raised on non-poor families. We present several pieces of evidence to support a causal interpretation of the estimates. The findings present credible evidence that increase in school spending can improve both short- and long-run outcomes for disadvantaged school children.
I. INTRODUCTION

Ensuring equal educational opportunities for all children has been a long-standing American ideal (Strickland, 1991; Browning and Long, 1974). However, the rules that determine school funding have not necessarily lived up to this ideal. In most states, prior to the 1970s, the vast majority of resources spent on K–12 schooling was raised at the local level, primarily through local property taxes (Howell and Miller, 1997; Hoxby, 1996). Because local property tax bases (i.e., home values) and local tax rates were typically higher in areas with wealthier residents, this heavy reliance on localized financing contributed to wealthier districts’ spending more per student.¹ In response to large within-state differences in per-pupil spending across wealthy and poor districts, state supreme courts overruled school finance systems in 28 states between 1971 and 2010, and many states have implemented legislative reforms that led to important changes in public education funding.² These school finance reforms (SFRs) caused some of the largest changes in the structure of K–12 education spending in United States history.³ Accordingly, understanding the effect of these reforms on school spending, educational attainment, and socioeconomic adult welfare of those affected is important.

Existing research indicates that SFRs have led to greater equalization of school spending within states in the short run (Card and Payne, 2002; Murray, Evans, and Schwab, 1998). However, there are four important unresolved questions that remain:

1. Do existing studies suffer from biases associated with low-quality data? Previous national studies rely on data that were only available every five years starting in 1972.⁴ The low

¹ Note that many low-income urban districts raise local funding from commercial property, so although low-income students typically receive lower levels of funding on average, this is not always the case (Hoxby, 1996).
² The first of these cases was the well-known California case, Serrano v. Priest, decided in 1971.
⁴ Arizona, California,* Idaho, Kansas,* New York, New Jersey,* Washington, and Wisconsin* all had important court decisions that either overturned or upheld the state school finance system before the second possible data point in 1977. States with an asterisk (*) are states in which the status quo was deemed unconstitutional.
frequency of the data both precluded detailed analyses of outcomes surrounding the timing of reforms and rendered authors unable to rule out the possibility that the effects were driven by pre-existing trend differences between reform and non-reform states.

(2)  Do SFRs lead to enduring spending changes? Researchers have found that SFRs may affect marginal income tax rates (McGuire and Anderson, 2011), residential sorting (Tiebout, 1956), and shifting of income sources for school spending (Brunner and Sonstelie, 2003); be capitalized into housing prices (Epple and Ferreyra, 2008); and lead to loopholes or subsequent reforms to undo the effects of SFRs (Imazeki and Reschovsky, 2004). Accordingly, the effects of SFRs on school spending in the short run might be quite different from those in the long run.

(3)  How do different kinds of reforms affect the distribution of school spending in the short and long run? There is substantial variation in how different states implement SFRs (Hoxby, 2001). Because policy-makers must choose not only whether to implement reforms but also what kinds of reforms to implement, it is important to know how different kinds of reforms affect the distribution of school spending in both the short and long run.

(4)  How do changes in school spending caused by SFRs affect the long-run outcomes of affected children? The motivation behind SFRs was to reduce gaps in educational opportunity and subsequent socioeconomic well-being between children from poor and affluent families. However, the extent to which improvements in outcomes for low-income children was achieved is unclear. Hoxby (2001) finds mixed evidence on the effect of increased per-pupil spending associated with SFRs on high-school dropout rates. Card and Payne (2002) find that court-mandated SFRs that reduce inequality in spending are associated with reduced gaps in SAT scores between students from low- and high-income families. In contrast, Downes and Figlio (1998) find that reforms in response to court mandates do not result in significant changes in the distribution of test scores. In addition to the fact that the evidence on student achievement is mixed, there is mounting evidence that focusing on effects on test scores may miss important effects on longer-run outcomes (Heckman, Pinto, & Savelyev, forthcoming; Jackson, 2012). Accordingly, the effect that SFRs may have on long-run outcomes remains unknown.

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5 As acknowledged by the authors, the data used in this study may suffer from selection to SAT taking.
6 However, Downes & Figlio (1998) find that plans that impose tax or expenditure limits on local governments reduce overall student performance on standardized tests.
This paper tackles these four questions through an analysis of the effects of SFRs on the level and distribution of school spending, as well as on subsequent educational and economic attainment outcomes in adulthood. The analysis proceeds in two parts.

In Part One, covered in Sections II and III, below, we tackle the first three questions and investigate the effects of SFRs on district spending, both in terms of absolute levels and in equalizing spending between districts in a state. We address these questions using newly released panel data on per-pupil spending at the school district level going back to 1967, five years before the first reforms, and available annually from 1970 through 2010. We compile a comprehensive inventory of the timing of school finance litigation across states and legislative changes in state aid formulas that occurred between 1970 and 2010. We also codify reforms into several types, based on the ways the reform influenced the school funding formulas. With the higher-frequency, district-level data (previous studies used data points that were 5 or 10 years apart), we conduct a detailed analysis of the timing of changes in outcomes in relation to the timing of reforms and assess the degree of pre-existing trends in spending leading up to the enactment of reforms.

Applying the longest district-level panel on school spending that has ever been used to analyze these issues, we document the effects of SFRs on spending up to 20 years after reforms. Because many states implemented different aspects of reforms at different times, the high-frequency annual data allow us to distinguish the effects of different types of reforms on school spending. To document the evolution of school spending before and after reforms, we present a flexible semi-parametric Difference in Difference (DiD) event-study analysis. That is, we show how the year-to-year change in outcomes for districts in reform states differed from those for districts in other states over the same time period. We present estimates both for several years before and several years after reforms, and we document the effect of reforms on districts by their percentile of the state income distribution prior to the reforms.

Both graphical and statistical analyses confirm a structural break around the timing of either legislative or court-mandated reforms that is indicative of a causal effect of SFRs on per-pupil spending. Consistent with previous findings, SFRs tend to reduce inequality in spending between low- and high-income districts. However, different types of reforms have different effects: court-mandated reforms tend to produce greater reductions in spending inequality than legislative reforms. Court-mandated reforms increase spending for low-income districts while
legislative reforms tend to decrease spending. Adequacy-based court-mandated reforms lead to increases in per-pupil spending overall while equity-based court-mandated reforms reduce inequality with little effect overall. Consistent with Hoxby (2001), the effect of reforms on tax prices is important: formulas that impose spending limits and high tax prices on districts reduce spending, particularly for higher income districts; formulas that match district efforts to raise local funds and impose low tax prices increase spending, particularly for lower-income districts.

In Part Two, which includes Sections IV through VII, we address the fourth question by investigating the effects of reform-induced changes in per-pupil spending on long-run educational and economic attainment outcomes. We link our school spending and reform data to detailed longitudinal data on a nationally representative sample of over 15,000 children born between 1955 and 1985 and followed into adulthood through 2011 in the Panel Study of Income Dynamics (PSID). The PSID geocode data are linked with multiple data sources that describe the school quality resources, neighborhood attributes, and coincident policies in order to study the effect of the reform-induced changes in school spending on long-run adult outcomes. These birth cohorts straddle the period in which most of the major school finance reform litigation accelerated, and thus were differentially exposed depending on place and year of birth.

We use the timing of passage of court-mandated reforms as an exogenous shifter of school spending. To accomplish this, we identify only those changes in school spending at the district level that resulted from court-mandated reform. For each district, we estimate the change in per-pupil spending that occurs after the passage of a court-mandated SFR, net of any underlying state-specific time effects and district trends. This, in essence, identifies those districts that experienced an increase or decrease in per-pupil spending in the years immediately following court-mandated SFR. We then link these district-specific policy-induced spending changes to longitudinal data of individuals born between born between 1955 and 1985 and followed through 2011 in the PSID. Because our sample includes sets of children from the same districts who were born in different years, some of these children were too old to be affected by reforms at the time of passage (not treated), some were old enough to be treated for some fraction of their school-age years (partially treated), and some were young enough to have entered school after the reforms were passed (fully treated). This allows for a triple-difference strategy that compares the difference in outcomes between treated and untreated cohorts within
districts (variation in exposure) and across districts with larger or smaller changes in spending due to reforms (variation in intensity).

Results from event-study and instrumental variable models reveal that increases in per-pupil spending, induced by court-mandated school finance reforms, led to significant increases in the likelihood of graduating from high school and educational attainment for poor children, and thereby narrowed adult socioeconomic attainment differences between those raised in poor and affluent families. The results imply that for poor children, a twenty percent increase in per-pupil spending each year for all 12 years of public school is associated with 0.876 more years of schooling, 20.5 percent higher earnings, and a 10.7 percentage-point reduction in the annual incidence of poverty in adulthood. We present several key patterns that indicate that these improvements reflect the causal effect of these reforms and show that these results persist with controls for other coincident policies (e.g., "War on Poverty" initiatives and related safety-net programs; desegregation) that prevailed at the time these children were growing up.

These results provide compelling evidence that the SFRs of the 1970s through 2000s had important effects on the distribution of school spending and the subsequent economic well-being of affected students. Importantly, the results also speak to the broader question of whether money matters. After Coleman (1966), many have questioned whether increased school spending can really help improve the educational and lifetime outcomes of children from disadvantaged backgrounds. The results in this paper suggest that it can.

The remainder of the paper is organized into Part One (containing Sections II and III) and Part Two (containing Sections IV through VII). Section II describes the policy landscape and the data used for the first part of the paper. Section III outlines our main empirical strategy and presents an event-study analysis of the effect of reforms on school spending; it presents regression results to quantify the magnitudes and significance of the estimated effects on school spending and concludes the first part of the analysis. Section IV presents the data used for the second part of the analysis. Section V outlines the triple-difference empirical strategy for identifying the effects of SFRs on long-run outcomes. Section VI presents both event-study and instrumental variables regression results for the effect of school spending on longer run outcomes, and Section VII presents our conclusions.
PART ONE: EFFECTS OF SFRS ON EDUCATION SPENDING

II. A Discussion of School Reforms and School Finance Data

To assemble a comprehensive list of reforms, we extract details on the exact timing and type of court-ordered and legislative SFRs from Public School Finance Programs of the United States and Canada\(^7\) (PSFP) and the National Access Network’s state-by-state school finance litigation map (2011).\(^8\) We supplement these data with reform descriptions and classifications from Murray, Evans, and Schwab (1998), Hoxby (2001), Card and Payne (2002), Hightower, Mitani, and Swanson (2010), and Baicker and Gordon (2006). In most cases, data from these sources are consistent with each other. Where there are discrepancies, we defer to PSFP and consult state court and legislative records for validation.\(^9\) From these various sources, we compile a comprehensive dataset of each school finance reform between 1970 and 2010.

Figure 1 presents the total number of states that ever had a legislative SFR, a court-mandated SFR, or a substantive change in the school funding formula for each year between 1967 and 2005. A few patterns are apparent. First, even though most studies focus on court-mandated reforms, many states had legislative SFRs or substantive changes in how schools were funded that were not court mandated. Indeed, in 1996, while only 19 states had a court-mandated reform, 31 states had some kind of legislative SFR, and 45 states had experienced some kind of change to school funding formulas. Second, by 2005, most states had some form of SFR: 23 states had at least one court-mandated reform, 32 states had at least one legislative reform, and 45 had some change in funding formula. Third, there were two distinct waves of court-ordered SFRs, the first starting in 1971 and going through 1980 and the second between 1989 and 1997.

The movement toward school finance reform litigation and the ensuing debates about the constitutionality of local finance systems were based on the landmark school desegregation

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\(^8\) http://www.schoolfunding.info/states/state_by_state.php3

\(^9\) There were discrepancies in reported timing of overturned court cases in several states: Connecticut (Hoxby states the decision was made in 1978, but Card and Payne report it was made in 1977), Kansas (Hoxby states 1976, but PSFP and ACCESS report 1972), New Jersey (Card and Payne state 1989, but PSFP says 1990), Washington (Murray, Evans, and Schwab, Hoxby, and Card and Payne report 1978, but PSFP reports 1977), Wyoming (Hoxby says 1983, but Card and Payne and Murray, Evans, and Schwab report 1980). We researched each case by name to discover the true date of the decision.
The first challenges to existing local systems of school finance were based on the Equal Protection Clause of the Fourteenth Amendment of the U.S. Constitution. These challenges were unsuccessful, but they led to two subsequent waves of successful challenges based on state constitutional law.

These two waves of court-mandated reforms were distinct not only in time but also in motivation (Briffault, 2005). In the first wave, known as “equity cases,” proponents of state funding argued that local financing violated the responsibility of the state to provide a quality education to all children. They asserted that public education was a “fundamental interest” for equal protection purposes and thus could not be distributed unequally within a state based on geography absent any “compelling state interest.” The motivation was that “poor” school districts had little property wealth to tax in order to support their local schools, while “rich” school districts had much more at their disposal.

The successful challenges in the second wave were argued on adequacy grounds. “Adequacy cases” rely on the fact that virtually all states have a constitutional provision requiring the state to provide some level of free education for children (Lindseth, 2004). These cases were argued on the ground that prevailing low levels of educational resources in certain districts (typically low-income areas) violated the state’s duty to provide the necessary educational opportunities guaranteed by the state constitution. Figure 2 presents the number of states that had a court-mandated SFR on equity and/or adequacy grounds for each year between 1967 and 2010. While most early court-mandated reforms (1970–1988) were litigated on equity grounds, most of the later cases (1990–2010) were fought on adequacy grounds. Whether these different kinds of cases have different effects on spending is an empirical question that we address in Section III.

a. Classifying Reforms

While different reforms may have been implemented with different motivations in mind, to describe how reforms might affect per-pupil spending, most economics studies describe reforms in terms of how they change school finance formulas. To a first order approximation, district per-pupil expenditure (PPE) can be expressed as equation [1] below, where federal and state funding did not vary much across districts within a state prior to reforms.

\[
PPE = (\text{local tax rate}) \times \left( \frac{\text{Local Tax Base}}{\# \text{ of students}} \right) + \text{Federal} + \text{State}
\]
Inspection of [1] makes clear that, all else equal, districts with higher property tax bases (wealthier districts) will tend to spend more per pupil than districts with low property tax bases (poor districts). It is also apparent that, all else equal, districts with higher property tax rates (those that have a high demand for education) will tend to spend more than those with lower property tax rates (those with a lower demand for education). Given residential segregation by income and socioeconomic status, for both these reasons there is a tendency for wealthier districts to spend more per pupil on education than poor districts (Hoxby 2001).

Achieving fiscal (wealth) neutrality was a goal of many changes to state aid distribution formulas. That is, SFRs changed the parameters of spending formulas to reduce the strength of the relationship between the level of educational spending and the wealth of the district. Most changes in school finance formulas due to reforms aim to (a) account for differences in the costs of achieving equal educational opportunity across schools and districts; and (b) account for differences in the ability of local public school districts to cover those costs. The design of state aid formulas to meet these goals, however, is far from uniform. Legal scholars often rely on the language used in the case or legislation to classify types of reforms. In contrast, economists have emphasized how reforms affect the income and price incentives embedded in the state’s school financing formula (e.g., Hoxby, 2001). We also take this latter approach.

Card and Payne (2002) codify formulas into three broad categories: flat grant plans, which give the same dollar amount per student to all districts in a state; minimum foundation plans, which set a floor on per-pupil spending (the state provides the difference between the minimum amount per pupil and an estimate of how much local revenue a given district can raise); and variable grant plans, which provide different amounts of state aid to districts based on local property values, income levels, and how much local revenues are actually raised.

(Hoxby (2001) argues that these labels may not fully capture the economic incentives associated with the formulas. For example, some plans that would be in the same category in Card and Payne (2002) induce more spending by providing more state funding for districts that raise more local funds, while others induce less spending on the margin by providing more state funds to districts that raise fewer local funds. Accordingly, Hoxby (2001) advocates classifying reforms based on inverted tax prices. The inverted tax price is the amount of additional funding the district has to spend if it raises tax revenue by one dollar.
An inverted tax price of zero means that a district cannot raise education spending no matter how much it increases its tax revenue (a clear disincentive to raise local funds). This occurs in states that impose spending limits on districts (Downes and Figlio 1998). An inverted tax price greater than one means that a district can raise education spending by more than one dollar by raising tax revenue by one dollar (a clear inducement to raise local funds). To capture this important feature, Hightower, Mitani, and Swanson (2010) disaggregate variable grant plans into two groups to make a distinction between those plans that focus on school districts’ inverted tax prices (local effort equalization plans) and those that do not (equalization plans). Augenblick, Meyers, and Anderson (1997) aptly refer to these local effort equalization plans as “reward for effort” policies. We also use this intuitive label.

We combine these approaches to create the following five categories. Note that many state funding plans fall into more than one category. While any approach to summarize numerous different reforms into a manageable number of variables will be imperfect, we believe that our classification captures the key elements highlighted in the literature.

- **Foundation Plans**: These plans ensure a basic floor to spending. These include foundation plans, foundation grants, and guaranteed minimum tax base plans. These plans establish a foundation level of per-pupil spending, estimate a district’s required local contribution to fund this foundation level based on income and wealth levels in the district, and provide the difference between the expected contribution and the foundation level.
  - These plans do not affect tax prices. They provide extra funding to low-income/low-wealth districts while leaving high-income/wealthy districts largely unchanged.

- **Flat Grants**: These plans give aid on a per-pupil basis to all districts.
  - Flat grants do not affect tax prices. They provide similar state funds for all districts and should have little effect on spending inequality, all else equal.

- **Equalization Plans**: These plans provide aid to districts based on property values and income levels. They include power equalization plans (which give more money to low-wealth districts), categorical aid schemes (which give money to low-income districts), and other spending equalization plans that distribute state funds to districts based on wealth or income levels.
  - Because funds are distributed based on wealth and income levels, these plans do not affect tax prices directly (although they may provide incentives to alter the tax base).
These plans tend to provide extra funding to low-income/low-wealth districts while possibly taking money away from high-income/wealthy districts.

- **Reward for Effort Plans (inverted tax prices greater than one):** These schemes seek to promote local efforts to raise school spending by increasing state aid to low-wealth districts that have high tax rates. The key feature of these plans is that districts receive more state aid when they raise more local taxes.\(^\text{10}\)
  
  - Reward for effort plans promote local efforts to raise education spending by targeting the inverted tax price directly. Such plans typically provide greater incentives for lower-income/low-wealth districts to increase taxes by allowing some districts to have more than one dollar in spending for each dollar raised in taxes. Such policies should increase spending overall, with larger spending increases for low-income districts.

- **Spending Limits (inverted tax price equal to zero):** Under such plans, the state imposes a limit on how much a district may spend on education. In addition, some equalization plans take away all tax revenues raised above a certain amount (i.e., if there is a recapture provision). The key feature of such plans is that districts are unable to increase school spending above some limit—that is, around the limit districts face a zero inverted tax price.
  
  - Spending limits are designed to limit education spending at the local level for high-spending districts. Because high-income districts also tend to have more spending, one would expect such policies to reduce spending for all districts, with a more pronounced effect for high-income districts. Such policies likely do reduce inequality, but at the expense of lower overall education spending. Because education spending tends to increase over time as spending levels rise to that of the limit, spending limits may reduce spending for all school districts.

b. **Changes in Formulae Over Time**

Since 1970, virtually every state has enacted at least one aid formula from among the categories listed in subsection a, above. To provide an overview of the evolution of school finance formulas, Figure 3 plots the number of states that have employed each kind of funding formula in each year. The first notable pattern is that the use of foundation plans was quite high in 1970 and increased slightly during the entire period (from 27 states in 1970 to 36 states in

\(^{10}\) For example, in Georgia, school districts at or below 75 percent of the state average property tax wealth level receive equalization funding in proportion to the number of mills they raise above the required five mill.
2010). As more states implemented SFRs, the use of flat grants declined (from 26 states in 1970 to 5 states in 2020), while the use of equalizing plans increased (from 9 states in 1970 to 30 states in 2010). While the reward for effort approach was unpopular in 1970, the number of states employing reward for effort has increased over time (from 0 states in 1970 to 21 states in 2010), as has the number of states imposing spending limits (from 0 states in 1970 to 12 states in 2010). In Section III, below, we investigate the effects of these different kinds of reforms on the level and distribution of school spending.

c. Changes in School Spending Over Time

Data on district and state funding come from the Census of Governments, the Historical Database on Individual Government Finances (INDFIN),11 and the Common Core of Data (CCD) School District Finance Survey (F-33). The Census of Governments has been conducted every five years since 1967, and records administrative data on school spending for every school district in the United States. This is the data source used in most existing national studies of school finance reforms. We augment this data with annual data from other sources. The INDFIN contains school district finance data annually for a sub-sample of large school districts from 1967 through 1991.12 After 1992, the CCD School District Finance Survey (F-33) consists of data submitted annually to the National Center for Education Statistics (NCES) and includes data on school spending for every school district in the United States.13 We combine these data sources to construct a long panel of annual per-pupil spending for school districts in the United States between 1967 and 2010.

This paper focuses on how SFRs affected school spending in different local communities, rather than measures of spending inequality over time. As such, we classify school districts based on their median income levels in 1962. To show how per-pupil spending has changed for neighborhoods that were low and high income in 1962, Figure 4 plots the mean

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11 The Historical Database on Individual Government Finances (INDFIN) represents the Census Bureau’s first effort to provide a time series of historically consistent data on the finances of individual governments. This database combines data from the Census of Governments Survey of Government Finances (F-33), the National Archives, and the Individual Government Finances Survey.
12 Per-pupil spending data from before 1992 is missing for Alaska, Hawaii, Maryland, North Carolina, Virginia, and Washington, D.C. Per-pupil spending data from 1968 and 1969 is missing for all states. Spending data for certain years is also missing for the following states: Florida (1975, 1983, 1985–1987, and 1991); Kansas (1977 and 1986); Mississippi (1985 and 1988); Montana (1976); Nebraska (1977); Texas (1991); and Wyoming (1979 and 1984). Where data for only a year or two was missing, we filled it in using linear interpolation.
13 Both NCES and the Governments Division of the U.S. Census Bureau collect public school system finance data, and they collaborate in their efforts to gather these data.
per-pupil spending each year between 1976 and 2010 for district by their quartile in the state income distribution in 1962. This figure depicts the evolution of per-pupil spending over time for districts with different income levels in 1962 (before any SFRs). Note that because quartiles are defined within a state, this plots within-state changes in inequality.

There are a few notable patterns. First, per-pupil spending has been increasing over time in all districts. In 2012 dollars, the average district spent about $4,612 per student in 1967 and about $12,772 per student in 2010. This represents a 175 percent increase (in real terms) over 43 years. This increase of about 4 percent annual growth was experienced in both low- and high-income districts. A second notable pattern is that the difference between high- and low-income districts was wide in the early 1970s, narrowed during the late 1970s (corresponding to the first wave of reforms), was stable during the 1980s, and then narrowed again in the mid-1990s (corresponding to the second wave of reforms). One unexpected pattern is that per-pupil spending in the lowest income districts (in 1962) was always below that of other districts until the mid-1990s, when spending in the poorest districts rose to levels above that of the middle-income districts. While districts in the lowest income group spent about 8 percent less than the median income district in 1967, by 2010, the districts that were in the lowest-income group in income spent 7 percent more than those in the middle-income groups in 1962.

A comparison of Figure 2 through 4 suggests why this reversal may have taken place during the late 1990s. The timing of the increases in education spending for the low-income districts are very much in line with the timing of the second wave of court-mandated reforms that emphasized adequate spending for low-income districts and relatively rapid increases in the use of reward for effort plans. The timing of the reversal is coincident with the increased use of reforms that one might expect to lead to a disproportionate increase in school spending in these low-income areas. Of course, the extent to which these reforms actually had the expected effects is an empirical question, which we investigate in the following section.

III. Event-Study Analysis of Effects on School Spending

Our empirical approach to estimating the effect of SFE on the distribution of per-pupil spending across income levels is to analyze data using a differences-in-differences (DiD) methodology. Using the district-by-year data as described in Section II, we can compare the spending in low- or high-income districts (districts with low or high median incomes in 1962)
before implementation of a SFR to the spending in the same district after implementation. Because there may be a tendency for spending to increase over time, we use the difference in spending for low- or high-income districts across the same years in states that did not implement any reforms over that time period as a basis for comparison.

To give an example, Illinois implemented its first SFR in 1973, while Missouri implemented its first SFR in 1977. One can compare spending for low-income districts in Illinois in 1972 (the year before the reform) to that in 1976 (four years post-reform). Because there may have been some national changes that affected spending in all districts between 1972 and 1976, one can use the difference in spending for low-income districts between 1972 and 1976 in Missouri (both pre-reform years in MO) as an estimate of what the change in spending would have been for low-income districts in Illinois absent reforms. If reforms increase spending for low-income districts, we should see that the difference in spending for low-income districts between 1972 and 1976 in Illinois is greater than the difference in spending for low-income districts between 1972 and 1976 in Missouri. The same logic can be applied to spending in medium- and high-income districts. This is the logic of the DiD estimator. One can implement this DiD strategy within a regression framework by estimating equation [2], below.

\[
S_{dst} = \alpha + (Q_d \cdot \sum I_{y}^{court}) \cdot \pi_{q,y}^{court} + (Q_d \cdot \sum I_{y}^{legislate}) \cdot \pi_{q,y}^{legislate} + \theta_d + \theta_t + \epsilon_{dt}
\]

In equation [2], $S_{dst}$ is spending in district $d$ in state $s$ in year $t$, $Q_d$ is an indicator for the percentile of the district’s median income in the state distribution in 1962 (this is fixed within a district over time), $\theta_d$ is a district fixed effect (which subsumes a state fixed effect), $\theta_t$ is a year fixed effect, and $\epsilon_{dt}$ is a district-by-year level error term. Because some states had multiple reforms, we estimate treatment effects for the first reform of each type. The main treatment variables for the first reforms are $I_{y}^{court}$ and $I_{y}^{legislate}$. These are indicator variables equal to 1 if state $s$ will implement its first court-mandated reform or legislative reform in $y$ years, and 0 otherwise. These indicator variables map out the dynamic treatment of the two broad types of reforms and are interacted with $Q_d$. The coefficients $\pi_{q,y}^{court}$ map out the dynamic treatment effect of the first court-mandated reform on per-pupil spending for districts in quartile $q$. Similarly, the coefficients $\pi_{q,y}^{legislate}$ map out the dynamic treatment effect of the first legislative reform on per-pupil spending for districts in quartile $q$. For example, $\pi_{4,10}^{legislate}$ is the effect today in a bottom
income quartile district of implementing the first legislative reform 10 years in the future, and \( \pi_{1,5}^{\text{legislate}} \) is the effect today in a bottom quartile district of having implemented the first legislative reform in the bottom quartile 5 years ago. We plot the estimated treatment effects to illustrate how per-pupil spending evolves in the years before, during, and after the first legislative and court-mandated reforms. A visual inspection of this event-study plot should reveal any pre-reform trends in spending and any structural break in outcomes.

Because different kinds of reforms may have different effects, we also estimate dynamic treatment effects for different aspects of each reform by coding up the first year that a district uses a formula with (a) spending limit, (b) local equalization, (c) foundation plan, or (d) equalization plan. To estimate the dynamic treatment effect for particular types of funding formulae, we can use equation \([2]\) while replacing the reform-type indicators with \( I_{y}^{\text{limit}} \), \( I_{y}^{\text{localeq}} \), \( I_{y}^{\text{foundation}} \), and \( I_{y}^{\text{equalization}} \). These are indicator variables equal to 1 if state \( s \) will implement its first spending limit, local equalization, foundation plan, or equalization plan in \( y \) years, and 0 otherwise. One can then plot the coefficients on these indicators interacted with the district quartile to observe how district per-pupil spending evolved before, during, and after the changing of the school finance formulas in these specific ways.

To quantify the effect of these reforms on per-pupil spending, we form linear combinations of the estimated treatment effects for different years. For example, the effect of court-ordered reforms on the spending for the bottom 10 percent of income districts can be estimated by the average of the 5 years after reforms minus the average of the 5 years prior to reforms. This estimate is obtained by computing the following linear combination of coefficient estimates:

\[
\frac{1}{5} \left( \pi_{q,-5}^{\text{court}} + \pi_{q,-4}^{\text{court}} + \pi_{q,-3}^{\text{court}} + \pi_{q,-2}^{\text{court}} + \pi_{q,-1}^{\text{court}} \right) - \pi_{q,5}^{\text{court}} - \frac{1}{5} \left( \pi_{q,4}^{\text{court}} + \pi_{q,3}^{\text{court}} + \pi_{q,2}^{\text{court}} + \pi_{q,1}^{\text{court}} \right)
\]

Whether this computed difference is statistically significant is determined by testing the statistical significance of the linear combination of the estimated coefficients. We present the results of such tests to accompany the event-study graphs. Note that the standard errors in all the estimates effects are clustered at the state level.

### a. Event-Study Analysis for Court-Mandated Reforms

Much of the empirical literature of SFR has focused on court-mandated reforms. Figure 5 presents the event-study graph for court-mandated reforms for school districts in different
percentiles of the income distribution in 1962 (a year before any reforms were implemented). The figure depicts how district-level per-pupil spending evolved annually from 10 years prior to the first court-mandated reforms through 20 years after the reforms. The evolution of spending is presented separately for districts in the bottom 10 percent of median incomes, those in the 11th to 25th percentile, those in the 26th to 50th percentile, those in the 51st to 75th percentile, those in the 76th to 90th percentile, and those in the top 10 percent. The series for the bottom 10 percent depicts how per-pupil spending evolved for districts in the bottom 10 percent of the state income distribution over time in states with a court-mandated reform, relative to such districts in states without a court-mandated reform over the same time period. To show how per-pupil spending was affected for all districts on the same scale, each series is re-centered around the average for the 10 years prior to reforms. This means that a value of 0 in a given year would mean that spending in that year was the same as the 10-year average prior to reforms. Also, note that year 0 is the year of the reform. As such, if reforms increase spending relative to pre-reform years, we should see positive values for years 1 through 20, and if reforms decrease spending, we should see negative values for these years.

In Figure 5, all the series are centered on 0 during the pre-reform years. During the 10 years prior to reforms (years -10 through -1), districts in reform states saw similar changes in per-pupil spending as districts in non-reform states of the same income level. Within the first 5 post-reform years however, districts in the bottom quartile (solid black lines) saw systematic increases in per-pupil spending above and beyond comparison districts in non-reform states. Districts between the 25th and 75th percentiles experienced systematic increases about 8 years after reforms. In contrast, districts in the top 10 percent of incomes in 1962 saw little change in spending within the first 14 years after reforms. There is some evidence, however, of a slight decrease 15 years after reforms for the very top districts. Note that the decline in spending at year 9 is due to a compositional change. The lower panel of Figure 5 plots the same dynamic treatment effects, but only using districts that were observed for more than 9 years post reform. Using this more balanced panel, there is no sudden drop in year 9, but the basic patterns are the same. Because the more balanced panel includes only older cases, it includes relatively few adequacy cases. We will show that the difference between the top and bottom panels of Figure 5 is due to the fact that the first 8 years presented in the top panel include many adequacy cases, and these generate somewhat different patterns from equity cases.
To better quantify the patterns in Figure 5, we estimate the effect of court-mandated reform as the difference between the average effect in the 10 years prior to reforms and the 10 years post reform. Based on the linear combination of coefficient estimates, these reforms increased per-pupil spending for the bottom 10 percent income districts over the first 10 years by $636.37 (p-value=0.04). Between 1980 and 1990, the average per-pupil spending for these low-income districts was $6,985.04, representing a relative spending increase of about 9 percent. To get a better sense of the longer run effects of spending for these districts, we compute the average effect for year 5 through 10 relative to the 10 years prior to reforms. This calculation indicates that after 5 years these reforms increased per-pupil spending for the bottom 10 percent income districts by $730.27 (p-value=0.03), an increase in spending for low-income districts of about 11 percent. Similar calculations for the top 10 percent income districts show little effect. The estimated effects suggest that these reforms reduced spending during the first 10 years by $131.89 (p-value=0.47) and in years 5 through 10 by $108.12 (p-value=0.32).

In sum, court-mandated reforms increased spending in the lowest-income districts by about 10 percent and had little effect for the highest-income districts. Using the estimates, court-mandated reforms reduced the spending gap between the top-income district and the bottom-income districts by $839.01 (p-value<0.01). The spending gap between these two groups of districts between 1980 and 1980 was $1269.53, so that court-mandated reforms reduced this spending gap by about two-thirds on average. The magnitude of these effects, coupled with the rapid increase in the number of court-mandated reforms during the early 1990s, can account for a sizable portion of the spending “catch-up” documented in Figure 4 between the lowest- and highest-income districts.

There are two types of court-mandated reforms: those argued on equity grounds and those argued on adequacy grounds. One might wonder if these different kind of cases lead to different kind of reforms that have different effects. This question was investigated empirically by Springer, Liu, and Guthrie (2009) and Berry (2007), who found no difference between these two kinds of cases in simple regression settings. We investigate this question using the more flexible event-study approach.

The top panel of Figure 6 presents the dynamic effects of equity-based court-mandated reforms on the level of per-pupil spending. The effects are relative to non-reform states. As one can see, there is a downward trend in spending for those states that had their first court-
mandated case based on equity grounds relative to similarly affluent districts in non-reform states. While it is unclear what the effect of such cases are on the overall level of spending (because it is unclear what the trajectory of school spending would have looked like absent reforms), it is apparent that equity cases do lead to greater equity in spending: while the top-income and bottom-income districts are on very similar trajectories prior to reforms, such that the spending gap was stable in the pre-treatment years, the spending gap narrowed by $856.34 (p-value=0.01) after five years post-reform. The aim of these cases was to increase spending equity. Reforms induced by these equity based cases achieved the objective.

The bottom panel of Figure 6 presents the event-study graph for adequacy cases (primarily the second wave of cases). The objective of these cases was not to explicitly reduce inequality in education spending, but rather to ensure that spending permitted all children (especially those in low-income districts) to receive adequate resources for a quality education. Because these cases are much more recent, we present the dynamic treatment for the first seven years of the reform. As one can see, spending in all districts in states with adequacy cases was fairly stable (relative to non-reform districts) prior to reforms. While there may have been a slight uptick in spending five years prior to reforms, the trajectory of spending was quite flat four years prior to reforms. After reforms, there is visual evidence of an increase in school spending that is most pronounced for the poorest 10 percent of districts. While all districts experience an increase in spending of about $430 within the first five years of reforms, the poorest 10 percent of districts break from the other districts with an increase in spending of over $1000 within the first five years. Because all districts experienced spending increases, adequacy cases are associated with smaller reduction in spending gaps than equity cases. Five years after an adequacy case, the gap in spending between the highest- and lowest-income districts is narrowed by $367.28 (p-value=0.02). In sum, consistent with the aims articulated by the courts, equity cases led to greater equity in school spending, while adequacy cases led to increased school spending overall, with particularly large increases for low-income districts.

b. Event-Study Analysis for Legislative Reforms

Legislative reforms have received much less attention in the literature than court-mandated reforms, and the consensus seems to be that legislative reforms were largely ineffective at increasing school spending for low-income districts or reducing spending
inequality. To investigate this conclusion further, Figure 7 plots the change in district per-pupil spending over time for states that experienced legislative reforms relative to similarly affluent districts in non-reform states. As in Figure 5, the series are presented for districts that were at different points in the distribution of median income in 1962, and they are re-centered around the mean for the 10 years prior to reforms. Similar to states that passed court-mandated reforms, those states that passed legislative reforms were on a trajectory of per-pupil spending similar to that of non-reform states during the few years preceding the reforms. However, in the post-reform years, there is a clear downward relative trend in spending for all districts.

Figure 7 also provides visual evidence that legislative reforms reduce spending inequality. The three series in black are districts above the median and those in grey are districts below the median. Prior to reforms, the black and grey series move together, and no single series is systematically above the other. In contrast, in the post-reform years, districts below the median income (black series) are on top and those above the median income (gray series) are on the bottom. This suggests that legislative reforms induced slower spending growth but also reduced spending inequality between low- and high-income districts.

The point estimates tell a similar story. In the 10 years after reforms, the lowest-income districts experienced a $569.70 reduction in spending (p-value=0.12). Between years 5 and 10, the reduction for these low-income districts was $730.45 (p-value=0.09). Consistent with the persistent slowdown in spending growth, between years 10 and 15 these reforms were associated with a reduction in spending in low-income districts by $930.41 (p-value=0.02) relative to the 10 years prior to reforms. Looking at the top 10 percent of districts, the patterns are similar. These reforms are associated with an $846.22 reduction in the first 10 years post-reform (p-value=0.03) and a $1036.58 reduction (p-value=0.02) between years 5 and 10. Because the reductions in spending are somewhat larger for the high-income districts than the low-income districts, these reforms likely did reduce spending gaps between the top- and bottom-income districts. Our estimates suggest this was the case, but not conclusively so: relative to the spending gap in the 10 years prior to reforms, the spending gap between the top 10 and bottom 10 percent income districts was reduced by $275.43 (p-value=0.09) in the 10 years after reforms. While the estimated change is only marginally statistically significant, the size of the effect is economically important. It represents a 20 percent reduction in the spending gap between high- and low-income districts. We conclude from this that legislative changes
tended to level down spending (Hoxby, 2002) and likely had modest effects of reducing spending inequality between low-income and high-income districts.

c. Effects by Type of Reform Used

While accurately documenting the effects of these past reforms is important from an historical point of view, it does not address the policy-relevant question of why different kind of reforms have different effects or what kinds of reforms policy-makers should try to implement in the future. There are numerous ways that reforms can be constructed, and it can be argued that what really matters is the kind of funding formula used in a reform, rather than why or how the reform was implemented. Furthermore, as illustrated in Figure 1, there are many more funding changes that may affect the distribution of school spending that are not tied to specific legislative or court-mandated reforms. This motivates an event-study analysis of the four most commonly introduced types of reforms. Because flat grants were not introduced over time, but rather replaced with new reforms, we do not estimate the effects of introducing flat grants.

Figure 8 shows the event-study graphs for the imposition of spending limits. There is little evidence of any differential pre-existing trend in school spending for districts that imposed tax limits and those that had no change in their tax prices. It is also apparent that spending gaps across income levels were stable prior to reforms. Consistent with theoretical predictions, spending limits reduce per-pupil spending for all districts in the long run, with the most pronounced effect in the wealthier districts of a state. The fact that reductions in spending (relative to the relatively flat trend prior to the change) grow over time is consistent with a spending limit that becomes more likely to bind as the underlying level of spending increases for all districts to the level of the limit.

One would expect the spending limit to bind first for the highest spending district. Then, as overall spending increased it would bind for lower spending districts. This is precisely the pattern observed in the Figure 8: for the poorest 10 percent of districts, the spending limit is associated with a reduction in spending of $2.39 (p-value=0.992) in the 10 years after reforms. However, between years 10 and 20 after reforms, these low-income districts experience a $954.18 relative reduction in spending (p-value=0.01). For higher-income districts, the reductions in spending are much more immediate. For the most affluent 10 percent of districts, the spending limit is associated with a reduction in spending of $555.03 (p-value<0.01) in the
10 years after reforms. The reduction increases to $1564.14 between years 10 and 20. Not surprisingly, spending limits are effective at reducing spending inequality: the spending gap between the high- and low-income districts narrows by about $637.21 (p-value<0.01) after 5 years. This is a non-trivial reduction in the spending gap, but it appears to come at the expense of slower spending growth for all districts. The decreases in spending are consistent with the theoretical prediction that decreases in inverted tax prices will tend to decrease the overall level of school spending.

On the other side of the policy spectrum are policies that promote school spending by encouraging local districts to increase per-pupil spending with matching funds. We refer to these as “reward for effort” policies. Figure 9 provides the event study for this kind of reform. Unlike other kinds of reforms, there is clear evidence of a downward trend in per-pupil spending for those states that implemented local equalizing policies. This is consistent with the notion that the kinds of policies states employ are not random and that one must be careful to consider pre-existing trends when analyzing the effects of such policies. Despite the existence of a negative trend, there is clear evidence of a structural break at the exact time of passage of reforms. While spending is clearly declining in all districts in the pre-reform years (five out of seven of the changes are negative realizations for the lowest-income districts), there is an upward trend that lasts about five years (four out of five first post-reform realizations is positive for the lowest-income districts). The fact that this negative to positive change is experienced for all districts suggests that this is not merely a statistical artifact. After this five-year period of increased spending, however, spending reverts to the pre-existing downward trend.

Because of the pre-existing negative trend, estimating the effects on a level with a DiD model is unwise because the common trends assumption is clearly violated for spending levels. However, the common trends assumption they may be valid for spending growth. If so, one can estimate credible effects on spending growth by applying equation [2], on the one-year change in spending rather than the level of spending. This allows for the estimation of the effect of reward for effort reforms on spending growth because it takes into account differences in spending growth between reform and non-reform districts.

The lower panel of Figure 9 shows the dynamic treatment effect for changes in school spending. It is clear that while the common trends assumption was violated for levels, it appears to be satisfied for year-to-year changes in spending. The figure shows that during the first 5
years after the introduction of a reward for effort reform, all districts experienced increased spending growth relative to the previous 10 or 5 years; low-income districts experienced an increase in the year-to-year increase in spending of $131.13 (p-value=0.01); and high-income districts experienced an increase in the year–to-year increase in spending of $126.10 (p-value=0.03). Consistent with a reversion to the pre-reform growth rate after about 5 years, there is not a statistically significant difference between the growth rates for post-reform years 5 through 10 and the pre-reform years (i.e., both yield p-values above 0.1). However, there is evidence suggestive of increased spending growth for the lowest-income districts in the long run such that during post-reform years 10 through 20, average annual spending changes were $175.88 more (p-value=0.08) than during the pre-reform years. This is consistent with the analysis in levels that reveals that reward for effort plans reduce the spending gap between low- and high-income districts in the long run by $331.41 (p-value=0.07). Overall, the patterns show an increase in spending and spending growth in the short run (lasting about 5 years after reforms) for all districts, with a possible permanent increase in spending growth for the poorest districts. Results suggest that these policies increase the growth of spending (particularly for low-income districts) and reduce spending gaps between high- and low-income districts by about 13 percent.

The last two kinds of reforms are foundation plans and equalizing plans (Figure 10). Both kinds of plans generally adjust state spending such that districts with low tax bases (rather than low income) receive additional funds from the state. For both these types of reforms, spending behaviors were erratic more than five years prior. Accordingly, the figures only plot the four years before reforms, and all statistical inferences are relative to the five years prior to reforms (when behaviors were more stable). The figures reveal that for both kinds of plans, low-income and high-income districts were on similar trajectories (and as were districts in other states) for the five years prior to reforms.

After reforms, both kinds of plans caused increased spending for the lowest-income districts and had small effects for the highest-income districts. Foundation plans increased spending for all districts below the 90th percentile in median income. For the lowest-income districts, equalizing plans increased per-pupil spending (relative to the 4 years prior to reforms) by $464.03 (p-value=0.06) in the 10 years post-reform. However, there was a slight decrease of $84.47 (p-value=0.74). The gap in spending associated with these reforms between the low- and
high-income districts was reduced by $548.21 (p-value<0.001) in the 10 years after reforms. Equalization plans had a very similar effect: there were increases for low-income districts ($529.07) and small decreases for high-income districts ($47.10) such that the gap is spending was reduced by $576.18 (p-value=0.03). In sum, both equalizing plans and foundation plans appear to have reduced the spending gap between high- and low-income districts by about one-third, and to have done so primarily by increases in per-pupil spending for the lowest income districts in the state.

The figures reveal that, by and large, school finance reforms achieve the stated objective of reducing inequalities in school spending between low- and high-income districts and increase the level of per-pupil spending in poor communities. Both equalization plans and foundation plans are effective at reducing spending gaps between low- and high-income areas. The results also indicate that plans that aim to increase equality by reducing spending for the highest-income districts achieve this objective, but with the unintended impact of also reducing spending in low-income districts in the long run. In contrast, plans that promote greater education spending through matching tend to have a positive effect on the growth of school spending for all districts, with particularly large effects for low-income districts. Having established to what extent and how SFRs change the distribution of school spending, the remaining question is how changes in school spending caused by these reforms affect the educational and adult outcomes of children. This is the topic of Part Two, below.

**PART TWO: EFFECTS OF SCHOOL SPENDING ON LONG-RUN OUTCOMES**

**IV. Description of the Longer-Run Outcome Data**

The primary micro dataset utilized to analyze the effects of reform-induced changes in school spending on long-run outcomes is the restricted, confidential geocoded version of the PSID (1968–2011) with identifiers at the level of the neighborhood blocks in which children grew up.\(^{14}\) We link our district-level data on school spending and the timing of reforms to the

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\(^{14}\) The PSID began interviewing a national probability sample of families in 1968. These families were re-interviewed each year through 1997, when interviewing became biennial. All persons in PSID families in 1968 have the PSID “gene,” which means that 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. Moreover, the genealogical design implies that the PSID
nationally representative sample of children born between 1955 and 1985 from the PSID. Following Johnson (2012), we then merge neighborhood and school characteristics, as well as information on other key policy changes (e.g., 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 in the 1960s, ’70s, ’80s, and ’90s, when these children were growing up, allowing for a rich set of control variables.\textsuperscript{15}

The sample consists of PSID sample members born between 1955 and 1985 who have been followed into adulthood; these individuals were between the ages of 26 and 56 in 2011. We include all information on them for each wave, 1968 to 2011.\textsuperscript{16} We include both the Survey Research Center (SRC) component and the Survey of Economic Opportunity (SEO) component, commonly known as the “poverty sample,” of the PSID sample. Due to the oversampling of African-American and low-income families, 59 percent of the sample members were poor as children (N=15,353 individuals; 9,035 poor children; 6,318 non-poor children). Sixty-six percent of the PSID individuals born between 1955 and 1985 and followed into adulthood grew up in a school district that was subject to a court-mandated school finance reform sometime between 1972 and 2000, with the timing of the court order not necessarily occurring during their school-age years. Eighty-eight percent of the PSID individuals born between 1955 and 1985 who were poor as children and followed into adulthood grew up in a school district that was subject to a court-mandated school finance reform sometime between 1972 and 2000. Given the patterns in Figure 1, the share of individuals exposed to school finance reforms during childhood increases significantly with birth year over the 1955–1985 birth cohorts analyzed in the PSID sample.

\textsuperscript{15} The data we 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).

\textsuperscript{16} The PSID maintains high wave-to-wave response rates of 95–98 percent. Appendix C discusses the extent to which sample selection, including mortality, may bias the estimates. Studies have concluded that the PSID sample of heads of households and spouses remains representative of the national sample of adults (Gottschalk et al., 1999; Becketti et al., 1997).
We use the census block as the definition of neighborhood, which comprises a smaller geographic area than most previous studies utilize, and we match childhood residential location address histories to blocks and school district boundaries that prevailed in 1969 (the algorithm is outlined in Appendix A). Each record is merged with data on school spending for 1960–2000 and the aforementioned school finance variables at the school district level that correspond with the prevailing levels during their school-age years. We also merge information on student-teacher ratios and school segregation indices to the PSID data using the census block/tract contained in the geocode file based on the earliest available address in childhood (or county of birth when census block information is unavailable).

After combining information from these data sources, the main sample used to analyze adult attainment outcomes consists of PSID individuals born between 1955 and 1985. It includes 93,022 adult person-year observations of 15,353 individuals (9,035 poor children; 6,318 non-poor children) from 1,409 school districts, 1,031 counties, and all 50 states and the District of Columbia. Given the structure of the data, the oldest cohort is observed at age 56, while many cohorts are observed at age 30. As such, to allow us to compare individuals from different cohorts at around the same age, we focus on those adult observations between the ages of 25 and 45. The mean age is 32.9 years for the economic outcome measures considered. The set of adult attainments examined chronologically over the life cycle include (a) educational outcomes—whether graduated from high school, years of completed education; and (b) labor market and economic status outcomes (all expressed in real 2000 dollars)—wages, family income, and annual incidence of poverty in adulthood (ages 25–45). All analyses include men and women with controls for gender. These data are combined to provide new evidence on the long-run impacts of school finance reform and school resources. Summary statistic are presented in Table 1.18

V. Empirical Strategy for Estimating Effects on Adult Outcomes

In the second part of the adult outcome analysis, we aim to determine whether the increased school spending experienced by children in low-income neighborhoods due to SFRs had any lasting effects on their adult socioeconomic well-being. Our primary empirical

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17 Many school districts were counties during this period, including more than one-half of Southern school districts.
18 The appendices and Appendix Table C list the sources and years of all data elements, along with details of the PSID survey questions used to construct key measures.
approach uses the staggered timing of court-mandated school finance reforms across districts to implement an “event-study” analysis. That is, we estimate the precise timing of changes in school spending associated with the passage of reforms and the resultant effects on adult attainment outcomes. Because residential mobility across counties and private school attendance are much more common among children from affluent parents than those from low-income parents, one might expect larger effects among children from low-income parents. Also, prior research has shown that children from low-income families may be more sensitive to changes in school quality and school-related interventions (e.g., TN Star experiment) than children from more advantaged family backgrounds. As such, we conduct this analysis separately by childhood poverty status.

As in the analysis of school spending, we employ a flexible event-study design to map out how adult outcomes evolve over time (across cohorts) before and after the passage of reforms. This event study also allows us to examine not only how outcomes vary with years of exposure to the reforms but also pre-reform trends in outcomes, and to test for potential endogeneity in the timing of reforms. While many reforms change the distribution of school spending, we focus on school spending changes associated with the passage of court-ordered reforms. This choice was driven by the fact that court-mandated reforms exhibited minimal trending in spending prior to those reforms (suggesting that there might be minimal trending in adult outcomes across cohort), and court-mandated reforms generated large, robust, and statistically significant increases in per-pupil spending for low-income neighborhoods (within which many of the PSID respondents resided). In short, court-mandated reforms have desirable statistical properties and are likely to be reliable exogenous shifters in school spending.

It is worth pointing out that simply comparing outcomes of students exposed to more or less school spending within the same district could lead to biased estimates of the effect of school spending on student outcomes if there were other factors that affected both student outcomes and school spending simultaneously. For example, a decline in the local economy could depress per-pupil spending (through home prices or tax rates) and also have deleterious

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19 Prior research has demonstrated that while residential instability is significantly greater for poor families, and they experience intra-county moves more frequently, they most often move to neighborhoods of similar observable quality (Johnson, 2009; Kunz, Solon et al., 2008; Mare et al., 2008). Poor families are far less mobile, as measured by upward residential mobility patterns, and are less responsive to policy changes due to the greater residential location constraints they face.
effects on student outcomes. This would result in a spurious correlation and would lead one to wrongly infer that reduced per-pupil spending has negative effects on child outcomes. Conversely, an inflow of low-income student populations might lead to an inflow of compensatory federal funding while simultaneously generating reduced student outcomes. This would lead to a spurious negative relationship between spending and student outcomes. By focusing only on changes in school spending within districts associated with reforms, the event-study approach removes potential biases that might exist when simply comparing students who have been exposed to different levels of school spending for reasons unknown to the researcher.

a. Hypothesized Effects Across Cohorts

A natural test of whether spending changes associated with school finance reforms have a causal effect on adult outcomes is to observe whether exposed cohorts in those districts that experienced the largest increases in school spending also experienced the greatest improvement in outcomes relative to unexposed cohorts in the same district. Because not all cohorts within a district are equally treated (some are exposed to spending increases for more of their school years than others), it is instructive to lay out the cross-cohort patterns in outcomes one should observe if there is a causal effect of increased spending due to reforms on adult outcomes.

If there is a causal effect of increases in school spending on adult outcomes, and there are no pre-existing cohort trend differences across districts that see increases in spending, then an event-time figure across cohorts for a given increase in school spending should follow a pattern similar to that hypothesized presented in Figure 11. That is, for those who were too old to be exposed to any reform-induced spending increases (to the left of 0 such that they were 18 or older at the time of the passage or reforms), there should be no systematic increase or decrease in the outcome across cohorts because none of these cohorts was exposed. For those who were of school-going age when reforms were implemented (i.e. those who were between the ages of 5 and 17, indicated by relative years 0 to 12), outcomes should both be better than those for the unexposed cohorts and increasing in the number of years of exposure. Finally, among more recent cohorts for whom all the school-age years were post-reform (i.e., those who were either not yet born or younger than 5 at the passage of reforms), they all had 12 years of exposure. As such, these cohorts should have better outcomes than the partially exposed cohorts, and there should be no systematic increase or decrease in the outcome among these fully treated cohorts. If (a) there is a causal effect of spending on outcomes and (b) the spending
increases predicted by the district-specific estimates are exogenous to changes in the outcomes, then the plot of the event-time dummies interacted with the increase in spending (for any positive increase in spending) should follow the stylized pattern in Figure 11. Moreover, the patterns presented in Figure 11 should be more pronounced for larger increases in spending.

b. **Estimating the Effect of School Spending on Outcomes**

To look for the hypothesized patterns discussed above, we test for the specific pattern presented in Figure 11 semi-parametrically across a variety of adult outcomes in the data. Importantly, we also test whether the differences across cohorts were larger for districts that experienced larger increases in spending due to reforms. While looking for differences across cohorts can be achieved with a flexible event-study analysis, testing for differences across districts that saw larger or smaller increases in spending requires a good measure of the court-mandated reform-induced increase in school spending.

The event-study analysis documented that districts in the bottom income quartile of the state’s income distribution in 1967 experienced larger increases in school spending than those in high-income quartiles. As such, the quartile of the district in the income distribution could serve as a proxy for the extent to which reforms increased funding in the district. However, this is a relatively weak proxy for increases in spending at the individual district level because (a) not all court-mandated reforms had the same effect on all districts, and (b) not all reforms had the same distributional effects on districts within a state. As such, to directly test for whether those districts that experienced larger increases in school spending were those that experienced larger improvement in adult outcomes requires having a measure of the increases in school spending associated with the implementation of a court-mandated reform at the individual district level.

To obtain a measure of district-specific changes in spending, we regress district per-pupil spending on district fixed effects, years fixed effects, and an indicator variable denoting a post-reform year interacted with each school district. The coefficients on the district indicators interacted with the post-reform indicator provide the regression estimate of the change in per-pupil spending associated with the passage of a court-mandated reform for each district. For each district, we take the interaction of the post-reform indicator with that district as our time-invariant, district-specific, court-mandated reform effect on spending (“\( SPEND_c \)”).

Testing for differences in the evolution of outcomes across districts with higher or lower levels of \( SPEND_c \) imposes the condition that the effect of school finance reforms on outcomes
in a district be monotonically related to the effect of school finance reforms on spending in that district. This is exactly the hypothesis we aim to test. That is, using this single measure of treatment intensity (the increase in per-pupil spending that the district will experience after the passage of reforms) allows for a direct test of our key hypothesis—that exposed cohorts in those districts that experienced the largest increases in school spending also experienced the greatest improvement in outcomes relative to unexposed cohorts in the same district. It is worth noting that one could test for this relationship indirectly by estimating the effect of reforms on adult outcomes by income categories and then comparing the changes in spending in each income group to the changes in outcomes for those same income groups. We present such estimates in Appendix A as a robustness check. The patterns from this indirect test are consistent with those from our more parametric approach but, as one would expect, much less precise.\(^{20}\)

While we impose a monotonic relationship between increases in spending and the adult outcomes with our parameterization of the treatment intensity variables, we remain flexible in our estimation of the timing of effects across cohorts using an event-study design. The main event-study models used to analyze the impacts of reform-induced changes in school spending on the difference in adult attainment between treated and untreated cohorts involve estimating equations of the form [3], below:\(^{21}\)

\[
Y_{icb} = \sum_{t-T=20}^{2} \alpha_{t-T} \cdot 1(t_{icb} - T^*_c = t - T) \cdot \text{SPEND}_c + \sum_{t-T=0}^{12} \theta_{t-T} \cdot 1(t_{icb} - T^*_c = t - T) \cdot \text{SPEND}_c
\]

\[
+ \sum_{t-T=13}^{20} \delta_{t-T} \cdot 1(t_{icb} - T^*_c = t - T) \cdot \text{SPEND}_c + X_{icb} \beta + Z_{icb} \gamma + (W_{1960c} \cdot b) \varphi + \eta_c + \lambda_b + \phi_g + \varepsilon_{icb}
\]

where \(i\) indexes the individual, \(c\) 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 variable \(\eta_c\) is a school district fixed effect, and \(\text{SPEND}_c\) is the SFR-induced change in per-pupil spending in district \(c\).

The flexible timing indicators, \(1(t_{icb} - T^*_c = t - T)\), equal 1 if the year the individual from school district \(c\) turned age 17 (\(t_{icb}\)) minus the year of the initial SFR court order in school district \(c\) (\(T^*_c\)).

\(^{20}\) Consistent with the flexible district-specific effects picking up much of the variability associated with the income quartiles, it is much more positive for lower-income districts. However, one can only explain 4 percent of the variability across districts in \(\text{SPEND}_c\) with the income category variables. Using all the observable variables to describe reforms from Part One interacted with the quartile of income can predictably explain about one-third of the variability in \(\text{SPEND}_c\).

\(^{21}\) This part of the research design is similar in setup to a recent study by Johnson (2011) on the long-run impacts of court-ordered school desegregation.
\( T_c^* \) equals a value between -20 and 20. For example, values for \( t_{ich} - T_c^* \) between -20 and -2 represent pre-treatment years; a value of -1 represents an individual who was 18 when court-mandated SFR was first enacted and thus was not exposed, which is used as the reference group category; values between 0 and 12 represent school-age years of SFR exposure; and values greater than 12 represent years beyond school-age exposure years. The event-study year \((t - T)\) is 0 when the year in which an individual was age 17 (typically, a high-school senior) equals the initial year of court-mandated SFR for the school district in which the person grew up.

Estimation of equation [3] provides a flexible description of the subsequent adult attainment outcomes in relation to the cohort- and district-specific timing of reform-induced changes in school spending. This allows us to test for the patterns described in Figure 11. The estimates of the post-reform indicator time dummies interacted with the reform-induced increase in spending, \( \theta_{t-T} \) in equation [3], map out difference in outcomes across cohorts that experienced a one dollar increase in per-pupil spending after the passage of reforms. These estimates provide precise pictures of the exact timing of any changes in attainment outcomes in relation to the number of school-age years of exposure to SFR and its resultant changes in spending. Because the validity of our empirical design depends critically on the assumption that those districts that saw increases in school spending due to reforms were not already on a differential trajectory of improving outcome, we also present the flexible time indicators interacted with the increase in spending for years prior to reforms. A plot of the estimates of the pre-reform indicator time dummies interacted with the reform-induced increase in spending, \( \alpha_{t-T} \), provides a visual portrait of whether there were systematic time trends in outcomes preceding enactment of court-ordered SFR in districts that would have experienced increases or decreases in school spending after reforms. The former uses the specific timing and intensity of changes to test for causal impacts of school spending; the latter provides a test of endogeneity in the timing and scope of the initial court orders. Note that in addition to testing for trending in the pre-reform cohorts, estimated effects beyond the maximum 12 school-age years of exposure \( (\delta_{t-T} \text{, for event-study years } (t-T)>12) \) provide an additional specification test, as these should
not exhibit significant trends in outcomes because these additional years do not represent any change in school-age exposure.\textsuperscript{22}

This model can be viewed as a triple-difference strategy that compares the difference in outcomes between cohorts within the same district exposed to reforms for different amounts of time (variation in exposure) across districts with larger or smaller changes in school spending due to reforms (variation in intensity). Because the intensity variable $\text{SPEND}_i$ is invariant within a district and all models include district fixed effects, the validity of the research design relies upon the exogeneity of the timing of passage of court-mandated SFRs, which is addressed and supported by the model specification in several ways. First, the model includes school district fixed effects ($\eta_{g}$), race-specific birth year fixed effects ($\lambda_b$), and race-by-region of birth cohort trends ($\phi_g \cdot b$), and it controls for an extensive set of child and childhood family characteristics ($X_{ich}$: parental education and occupational status, parental income, mother’s marital status at birth, birth weight, child health insurance coverage, and gender).

The period under study overlaps other important policy changes (Johnson, 2013; Chay, Guryan, & Mazumder, 2009; Hoynes, Schanzenbach, & Almond, 2012). To account for these policy changes, we include county by year of birth level measures of school desegregation, hospital desegregation, community health centers, and state-funding for kindergarten, in addition to per capita expenditures on Head Start, per capita expenditures at age four, Title I school funding, and average childhood spending on food stamps, Aid to Families with Dependent Children (AFDC), Medicaid, and unemployment insurance, ($Z_{ch}$).\textsuperscript{23} Few studies simultaneously account for so comprehensive a set of policies. Models that analyze the economic outcomes use all available person-year observations for ages 25-45 and control for a cubic in age to avoid confounding life cycle and birth cohort effects. To control for trends in factors hypothesized to influence the timing of SFR [3] also included interactions between 1960 characteristics of the county of birth and linear trends in the year of birth ($W_{1960c} \cdot b$: 1960 county poverty rate, percent black, average education level, percent urban, and population size).

\textsuperscript{22} Only in the case in which SFR plans became more effective with time would we expect a significant relationship between outcomes and event-study years beyond 12, which we explore.

\textsuperscript{23} The data sources used to compile these measures are detailed in Johnson (2013).
One potential parental response to the presence of school quality differences across public schools is to move to a different city or enroll one’s child in a private school. Because we did not want to include endogenous residential moves, we identified the neighborhood and school of upbringing based only on the earliest childhood address (in most cases, 1968). The resultant potential measurement error of school spending will tend to attenuate coefficients toward zero. Results are very similar when the sample is restricted to individuals who lived in their childhood residence prior to the initial court orders. The latter part of Section VI, below, provides more discussion of falsification and specification tests performed.

We present graphical plots, based on equation [3], that form the response function of outcomes to reform-induced effects on per-pupil spending. This allows us to test for any increase with years of exposure and the resultant amount of spending change. We also fit a parametric spline of the model above that provides simple summaries of the magnitudes of impacts of SFRs and school spending. The structure imposed is sufficiently flexible to allow several important specification tests to examine whether the detected impacts support a causal interpretation of school spending. The spline specification, informed by Figure 11, is below:

\[
Y_{icb} = \theta_0 (t_{icb} - T_{c}^*) \cdot D_{cb} \cdot 1(t_{icb} - T_{c}^* < 0) \cdot SPEND_c + \theta_1 (t_{icb} - T_{c}^*) \cdot D_{cb} \cdot 1(0 \leq t_{icb} - T_{c}^* \leq 12) \cdot SPEND_c + \theta_2 (t_{icb} - T_{c}^*) \cdot D_{cb} \cdot 1(t_{icb} - T_{c}^* > 12) \cdot SPEND_c + X_{icb} \beta + Z_{cb} \gamma + (W_{1960} \ast b) \phi + \eta_c + \lambda_t + \phi_t \ast b + \epsilon_{icb}
\]

The variable \( \theta_0 \) captures the pre-period linear trend in outcomes across districts that will experience a one dollar increase in school spending after a SFR; \( \theta_1 \) represents the estimated effect of each additional year of SFR exposure for a one dollar increase in district per-pupil spending, ranging from 0 to 12 years of exposure; \( \theta_2 \) captures the post-SFR linear trend for years beyond school age for a one dollar district-level increase in per-pupil spending. All models are estimated separately by child poverty status.

Finally, we impose further structure on the model (i.e. we impose a monotonic relationship between school spending and outcomes) to efficiently summarize the event-study graphs and provide a direct estimate of the effect of increased spending (experienced by each child) associated with the reforms on adult outcomes. To achieve this, we estimate instrumental

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24 After SFRs in California, the share of students attending private schools rose about 50 percent (Downes & Schoeman, 1998), and educational foundations grew tremendously (Brunner & Sonstelie, 2003). Privatization grew disproportionately in districts constrained by the SFR formula to spend less than they traditionally had.

25 Among original sample children in the PSID, the average proportion of childhood spent growing up in the 1968 neighborhood was roughly two-thirds.
variables regressions (two-stage least squares) to predict adult outcomes based on [3] where our endogenous regressor is the natural log of the average per-pupil spending in a student’s district when they were between the ages of 5 and 17. To facilitate direct comparison with the event-study estimates (that show a 20 percent spending increase) we multiply this by five so that the estimated coefficient on this variable \( \ln(ppe_{age 5-17})/(0.2) \) is the marginal effect of a twenty percent spending increase. We use \( \sum_{t-T=0}^{12} \theta_{t-T} \cdot 1(t_{ich} - T_{c}^* = t - T) \cdot SPEND_c \), the school age years of exposure indicator variables interacted with our district-specific court-mandated reform spending increase, as excluded instruments (i.e. exogenous shifter in school spending). All other variables in [3] are included. Results from the instrumental variables models exploit both the variation in timing and intensity of school spending changes due to court mandated reforms to obtain clean causal estimates of the effect of increased school spending on adult outcomes. All standard errors are clustered at the school district level.

VI. Estimated Effects on Longer-Run Outcomes

Educational Attainment. Figures 12a and 12b present the non-parametric and parametric event-study model results for poor and non-poor children on the same graph for the effects of reform-induced changes in per-pupil spending on the probability of graduating from high school and years of completed schooling, respectively. We present the estimated effects of a 20 percent increase in per-pupil spending—this is about $1000.00 and is roughly the standard deviation of the change in spending associated with a court-mandated reform. As detailed in Section V, above, all models include school district fixed effects, race-specific region, and year of birth effects; controls for linear cohort trends in 1960 county characteristics; controls at the county level for the timing of school desegregation and hospital desegregation, roll-out of the "War on Poverty," and related safety-net programs; and childhood family characteristics.

For both the probability of high school graduation and overall years of completed education, both the event-study and spline estimates show that there were no statistically significant effects for non-poor children while there were statistically significant effects for poor children. This is consistent with the notion that children from poor families are more responsive to policy changes than children from higher-income families. Looking only at poor children, the patterns are quite similar to the stylized patterns presented in Figure 11. Before reforms, there is
no evidence of any systematic trending differences between those districts that will see a 20 percent spending increase after reforms and other districts (the parametric spline model cannot reject the null hypothesis of any differential pre-reform trending at the 10 percent level for either education outcome). Both educational outcomes are increasing in the number of years of exposure (the parametric model rejects the null hypothesis of no positive trend post-reforms at the 5 percent level for both outcomes), and the increasing trend stops after the maximum number of school-age years (12).

These results are consistent with the conclusion that spending increases only improve outcomes for those who are exposed during their school-age years. That is, we see that (a) increases in educational outcomes occur only for exposed children during school-age years, (b) improvements are monotonically increasing in years of exposure, (c) improvements are larger with larger spending increases, (d) the timing of improvements in outcomes track the timing of the increases in spending, and (e) there are no differential trends in outcomes for districts that experience increases or decreases in spending due to reforms prior to such reforms.

Having established that there are real policy-induced improvements in long-run educational attainment associated with larger school spending increases, it is helpful to quantify the relationship between school spending increases and longer-run educational attainment. The Figure 12 shows that being exposed to a 20 percent increase in school funding each year for all 12 years of one’s schooling will increase the likelihood of graduating high school by about 15 percentage points and will increase the number of years of education by 0.8. To put these high school graduation estimates in perspective, in 1990 (when many in the sample would have been school age), the national dropout rate was 12.1 percent, while that for families in the lowest income quartile (those for whom we find effects) was about 25 percent (NCES). As such, the results indicate that increasing per-pupil school spending by 20 percent over the entire schooling career of a cohort of low-income children will reduce the dropout rate for those children by 66 percent.

Having established above that the increases in school spending during the school age years are likely exogenous to other changes in outcomes, we now present the instrumental variables (IV) estimates. Putting all the variation together, the instrumental variables 2SLS

models provide a direct estimate of the effect of increased school spending on adult outcomes, and allow for clear tests of statistical significance. The coefficient on the natural log of total per-pupil across an individual’s twelve years of public schooling all times 5 is the effect of a twenty percent increase in school spending each year for all twelve years of public schooling (about a $1000.00 increase in per pupil spending each year during all twelve school-age years).

The IV estimates in Table 2 show that a twenty percent increase in per-pupil spending across all 12 school-age years leads to a 0.11 percentage point increase (p-value<0.01) in high school graduation for the full sample. Because this estimate includes both poor and non-poor children, the effects are likely larger among children from poor families. The second and third columns present estimates for poor and non-poor children separately. While there is a small statistically positive effect on non-poor children of 0.048 percentage points, this effect is well-within sample variability and is not statistically significant (consistent with the event-study graphs). In stark contrast, among for children from poor families a twenty percent increase in per-pupil spending across all 12 school-age years leads to a 17.46 percentage point increase (p-value<0.01) in high school graduation. The instrumental variables estimates for years of completed education in Table 3 tell a very similar story. A twenty percent increase in per-pupil spending across all 12 school-age years leads to 0.357 more years of schooling (p-value<0.05). As expected, this estimate masks large effects for poor children. While there is a small statistically insignificant effect on educational attainment for non-poor children, a twenty percent increase in per-pupil spending across all 12 school-age years leads to 0.877 more years of schooling (p-value<0.01) for children from poor families. It is important to note the magnitude of these effects for children from poor families, because these effects are large enough to eliminate the high-school completion gap between children from poor and non-poor families and eliminate about 85 percent of the gap in average years of educational attainment.

Labor Market Outcomes, Adult Family Income, and Poverty Status. The next series of results reveals large, significant effects of school spending on poor children’s subsequent adult economic status and labor market outcomes, using the same model specifications. Figures 13 and 14 present school spending effects by childhood poverty status on adult economic outcomes (ages 25–45), including wages (Figure 13a), annual family income (Figure 13b), and the annual
incidence of poverty (Figure 14). In light of the parallel set of findings across all these long-run economic outcomes, the results are discussed in succession below.

As with the educational outcomes, the economic outcome patterns are similar to those hypothesized in Figure 11 for poor children and are indicative of a real causal effect of increases in school spending caused by court-mandated reforms. For both earnings and family income, there is no evidence of trend differences prior to reforms between districts that saw larger or smaller increases in school spending after reforms. (Neither model can reject the absence of differential pre-trends at the 10 percent level.) In contrast, both outcomes exhibit statistically significant improvements across cohorts associated with more years of exposure to a 20 percent spending increase. As with the other outcomes, the increases are only associated with the school-age years, and there is no systematic difference in outcomes across cohorts born at different times but with the same number of years of exposure. Note that because adult poverty is an undesirable outcome, all the estimates should be interpreted such that lower numbers are better. The estimated effects on adult poverty are consistent with all the other outcomes. However, there is some evidence of a trend of deteriorating outcomes prior to reforms for this outcome only. This implies that exposed cohorts fared better in terms of poverty despite being on a possible pre-reform trajectory of worsening outcomes over time. This suggests that our regression estimates of the effect on poverty could not be the continuation of pre-existing trends and are likely to be biased toward zero, if at all.

Figure 13a shows that exposure to a 20 percent increase in school funding each year for all 12 years of one’s schooling will increase a poor child’s future earnings by roughly 20 percent. For family income, the figure suggests that exposure to a 20 percent increase in school funding each year for all 12 years of one’s schooling will increase poor children’s future family income by roughly $15,000. Relative to the mean family income in the United States in 2010 of about $50,000, this represents a 30 percent increase. Finally, the figure for poverty suggests that exposure to a 20 percent increase in school funding each year for all 12 years of one’s schooling will reduce adulthood poverty among children from poor families by about 13 percentage points. Taken together, these results present a compelling case that increased school spending caused by school finance reforms has meaningful causal effects on adult earnings, family income, and poverty.
The instrumental variables estimates (Tables 6, 5, and 6) are consistent with patterns in the event study graphs. Specifically, for none of the outcomes is there a statistically significant relationship between increased school spending and adult outcomes for non-poor children. In contrast, for children from poor families a twenty percent increase in per-pupil spending across all 12 school-age years leads to 20.5 percent higher earnings (p-value<0.01), 25.67 percent higher family income (p-value<0.01), and 10.66 percentage points lower annual incidence of adulthood poverty (p-value<0.01).

The results support a causal interpretation of the effects of per-pupil spending by uncovering sharp differences in the estimated long-run effects on cohorts born within a fairly narrow window of each other as to whether, how long, and how much SFR influenced per-pupil spending during their K–12 school years. The evidence collectively is not consistent with alternative counter-explanations (i.e., other factors that happen to be changing at the same time these SFRs are enacted). Based on the robustness of the results, such an alternative explanation would have to be a cause that meets the following very strict criteria: (a) it closely follows the timing of passage of court-mandated SFRs (given the evidence showing no pre-existing time trends); (b) its impacts are constrained only to school-age years of exposure (given the evidence showing no effects for non-school-age years, whether pre-school ages or beyond age 17); (c) it had the largest impacts on poor children in communities where SFR resulted in the largest changes in school spending; and (d) it had no effects on individuals from non-poor-childhood families. Because we can think of no such counter-example, and because we are careful to control for a variety of potentially confounding policies and effects, we are confident that these effects can be taken as causal.

VII. SUMMARY DISCUSSION AND CONCLUSION

One of the distinguishing features of the U.S. public education system is its heavy reliance on the local property tax base for school district funding. This paper highlights the important role played by the courts and legislators in school finance reform during the past four decades, and documents their long-term effects.

We first investigated the effects of school finance reforms on district spending, both in terms of absolute levels in poor districts and in narrowing the spending gaps between the poor and affluent districts in a state. Consistent with prior research, we find that court-mandated
reforms were effective at reducing spending inequality between high- and low-income districts within a state and that this was achieved by increasing spending for the lowest-income districts. However, we document new and important differences between various kinds of court-mandated reforms. Equity-based court-mandated reforms are effective at reducing spending differences between high- and low-income areas, but they appear to do so primarily through redistributing school spending. Adequacy-based school finance reforms are also effective at reducing spending gaps, but they do so by increasing school spending in all districts, with larger increases for low-income districts. Looking to legislative reforms, our findings differ from many others in that we find that legislative reforms were somewhat effective at reducing spending gaps. We document important differences in the effect of reforms based on how they affect funding formulas. Because flat grant plans were generally not introduced during reforms, but rather removed as reforms were introduced, we do no analyze the effects of introducing flat grants. Both foundation plans and equalization plans reduce spending inequality with respect to income, with ambiguous effects on the overall level of spending. Reward for effort plans (that lead to low tax prices in order to promote school spending) lead to increased spending growth for about five years after reforms and reduce spending gaps in the long run. Plans that impose spending limits reduce spending for all districts and lead to particularly large reductions in relative spending for high-income districts.

Next we investigated the effects of spending increases caused by these school finance reforms on adult educational attainment, earnings, family income, and poverty status. We find that there are no discernable effects of increased school spending on children from non-poor households. However, our results indicate that for children from poor families, increasing per-pupil spending by 20 percent for a child’s entire schooling career increases high school completion by 17.46 percentage points, increases the overall number of years of education by 0.876, increases adult earnings by about 20.5 percent, increases annual family income by 25.6 percent, and reduces the incidence of adult poverty by 10.6 percentage points. All these effects are statistically significant at the 1 percent level, and are robust to a rich set of controls for confounding policies and trends. The magnitudes of these effects are sufficiently large to eliminate between two-thirds and all of the gaps in these adult outcomes between those raised in poor families and those raised on non-poor families.
Our results indicate a causal relationship between per-pupil spending and student outcomes. However, the improved outcomes could operate through a variety of mechanisms that may warrant further investigation beyond the scope of this research. For example, if high-income parents were less likely to send their children to private school in districts that experienced increases in school spending (Downes and Schoeman, 1998), children in the public school system may have also experienced changes in peer quality. Districts that saw spending increases may have been able to attract better teachers through increases in salaries, improvements in non-pecuniary characteristics, or changes in the composition of students (Jackson, 2009; Jackson, forthcoming). As such, the results should not be seen as the causal effect of more money per se, but rather as a clear demonstration of how increased school spending can have important long-run implications for children. After Coleman (1966), many have questioned whether increased school spending can really help improve the educational and lifetime outcomes of children from disadvantaged backgrounds. Our findings show that it can.

References


Tables and Figures

Figure 1: *Number of States with Reforms over Time*

Running Total of Number of States by Type of Change and Year

Figure 2: *Type of Cases over Time*

Running Total of Number of States by Type of Case
Figure 3: *Types of Spending Formulae*

Number of States with Formula Type by Year

- Foundation Plan
- Flat Grant
- Equalization Plan
- Reward for Effort
- Spending Limit

Year


Figure 4: *Per-Pupil Spending by Percentile of Income within State in 1967*

Per Pupil Spending: By Percentile Range by Year

- Bottom 10%
- 25th to 50th
- 75th to 90th
- 10th to 25th
- 50th to 75th
- Top 10%

Year

Figure 5  Effect of Court-Mandated Reforms

Data: The sample includes all school districts in the United States between the years of 1967 through 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.

Model: These plots present the estimated coefficients of a regression on per-pupil spending at the district level on years fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables from 10 years prior to 19 years after reforms (for both court-mandated reforms and legislative reforms simultaneously). Standard errors are adjusted for clustering at the state level.
Figure 6  
*The Effect of Equity and Adequacy Cases*

Data: The sample includes all school districts in the United States between the years of 1967 through 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.

Model: These plots present the estimated coefficients of a regression on per-pupil spending at the district level on years fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event time indicator variables from 10 years prior to 19 years after reforms (for equity based court-mandated reforms, adequacy based court-mandated reforms, and legislative reforms simultaneously). Standard errors are adjusted for clustering at the state level.
**Figure 7: Effect of Legislative Reforms**

Data: The sample includes all school districts in the United States between the years of 1967 through 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.

Model: These plots present the estimated coefficients of a regression on per-pupil spending at the district level on years fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables from 10 years prior to 19 years after reforms (for court-mandated reforms and legislative reforms simultaneously). Standard errors are adjusted for clustering at the state level.
Figure 8  Effect of Spending Limits (0 inverted tax prices)

Data: The sample includes all school districts in the United States between the years of 1967 through 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.

Model: These plots present the estimated coefficients of a regression on per-pupil spending at the district level on years fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables from 10 years prior to 19 years after reforms (for reforms that impose spending limits and reward for effort plans simultaneously). Standard errors are adjusted for clustering at the state level.
Figure 9  
*Effect of Reward for Effort Plans (inverted tax prices >1)*

**Effect of Local Equalization: Tax Price >1**

**Effect of Reward for Effort on Spending Growth: Tax Price >1**

**Data:** The sample includes all school districts in the United States between the years of 1967 through 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.

**Model:** These plots present the estimated coefficients of a regression on per-pupil spending at the district level on years fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables from 10 years prior to 19 years after reforms (for reforms that impose spending limits and reward for effort plans simultaneously). Standard errors are adjusted for clustering at the state level.
Figure 10  
*Effect of Foundation and Equalization Plans*

**Effect of Foundation**

![Graph showing the effect of foundation on per-pupil spending over time for different percentile groups.](image)

**Effect of Equalization**

![Graph showing the effect of equalization on per-pupil spending over time for different percentile groups.](image)

**Data:** The sample includes all school districts in the United States between the years of 1967 through 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.

**Model:** These plots present the estimated coefficients of a regression on per-pupil spending at the district level on years fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables from 10 years prior to 19 years after reforms (for equalization plans, foundation plans and flat grant plans, simultaneously). Standard errors are adjusted for clustering at the state level.
Figure 11: Hypothesized Patterns with a Causal Effect of an Exogenous Increase in Spending

We might expect a similar pattern for adult outcomes.
Should be more pronounced for larger spending changes.

Year age 17 relative to year of initial court order
Figure 12:  *Effect of a 20% Reform-Induced Spending Increase on Educational Attainment*

**Data:** PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011. (N=15,353 individuals (9,035 poor children; 6,318 non-poor children) from 1,409 school districts, 1,031 child counties, 50 states).

**Models:** Results are based on event-study models—both non-parametric and parametric (w/CI) estimates—that include school district fixed effects, race-specific year of birth fixed effects, and census division–specific linear cohort trends; controls at the county-level for the timing of school desegregation, hospital desegregation, roll-out of "War on Poverty," and related safety-net programs (community health centers, county expenditures on Head Start (at age four), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded kindergarten introduction), controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election (proxy for segregationist preferences)), each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Standard errors are clustered at the childhood county level. Main school finance reform variables are allowed to affect outcomes through both the amount of induced school spending changes and the duration of school-age years of exposure to reform-induced spending changes (i.e., models include intercept and slope terms of intensity of treatment (district spending change) and interaction terms of "school spending change exposure years" in order to capture dose of treatment in terms of both an individual's school-age years of exposure to school finance reform and the district's change in per-pupil spending induced by reform). Results for non-poor children are not statistically significant from zero.
**Figure 13:** *Effect of a 20% Reform Induced Spending Increase on Earnings and Family Income*

Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011. (N=15,353 individuals (9,035 poor children; 6,318 non-poor children) from 1,409 school districts, 1,031 child counties, 50 states).

Models: Results are based on event-study models--both non-parametric and parametric (w/CI) estimates--that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" and related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Standard errors are clustered at the childhood county level. Main school finance reform variables allowed to affect outcomes through both the amount of induced school spending changes and the duration of school-age years of exposure to reform-induced spending changes (i.e., models include intercept and slope terms of intensity of treatment (district spending change) and interaction terms of "school spending change*exposure years" in order to capture dose of treatment in terms of both an individual's school-age years of exposure to school finance reform and the district's change in per-pupil spending induced by reform). Results for non-poor children not statistically significant from 0.
Figure 14:  Effect of a 20% Reform Induced Spending Increase on Poverty as an Adult

Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011. (N=15,353 individuals (9,035 poor children; 6,318 non-poor children) from 1,409 school districts, 1,031 child counties, 50 states).

Models: Results are based on event-study models--both non-parametric and parametric (w/CI) estimates--that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" and related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Standard errors are clustered at the childhood county level. Main school finance reform variables allowed to affect outcomes through both the amount of induced school spending changes AND the duration of school-age years of exposure to reform-induced spending changes (i.e., models include intercept and slope terms of intensity of treatment (district spending change) and interaction terms of "school spending change*exposure years" in order to capture dose of treatment in terms of both an individual's school-age years of exposure to school finance reform and the district's change in per-pupil spending induced by reform). Results for non-poor children not statistically significant from 0.
Table 1. Descriptive Statistics by Childhood Poverty Status

<table>
<thead>
<tr>
<th></th>
<th>All (N=15,353)</th>
<th>Poor Child (N=9,035)</th>
<th>Non-Poor Child (N=6,318)</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Adult Outcomes:</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>High School Graduate</td>
<td>0.86</td>
<td>0.79</td>
<td>0.92</td>
</tr>
<tr>
<td>Years of Education</td>
<td>13.18</td>
<td>12.63</td>
<td>13.64</td>
</tr>
<tr>
<td>Ln(Wages), at age 30</td>
<td>2.51</td>
<td>2.36</td>
<td>2.61</td>
</tr>
<tr>
<td>Adult Family Income, at age 30</td>
<td>$49,308</td>
<td>$35,212</td>
<td>$55,324</td>
</tr>
<tr>
<td>In Poverty, at age 30</td>
<td>0.08</td>
<td>0.13</td>
<td>0.04</td>
</tr>
<tr>
<td>Age (range: 20-57)</td>
<td>32.9</td>
<td>32.6</td>
<td>33.2</td>
</tr>
<tr>
<td>Female</td>
<td>0.44</td>
<td>0.43</td>
<td>0.44</td>
</tr>
<tr>
<td>Black</td>
<td>0.14</td>
<td>0.23</td>
<td>0.07</td>
</tr>
<tr>
<td><strong>Childhood school variables:</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Per-pupil spending (avg, ages 5-17)</td>
<td>$4,463</td>
<td>$4,436</td>
<td>$4,486</td>
</tr>
<tr>
<td>Any court-ordered school finance reform, age 5-17</td>
<td>0.53</td>
<td>0.53</td>
<td>0.53</td>
</tr>
<tr>
<td># of exposure yrs to school finance reform, age 5-17</td>
<td>4.35</td>
<td>4.46</td>
<td>4.27</td>
</tr>
<tr>
<td>1960 District Poverty Rate (%)</td>
<td>22.09</td>
<td>24.75</td>
<td>19.88</td>
</tr>
<tr>
<td><strong>Childhood family variables:</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Income-to-needs ratio (avg, ages 12-17):</td>
<td>3.17</td>
<td>1.64</td>
<td>3.77</td>
</tr>
<tr>
<td>Mother's years of education</td>
<td>12.05</td>
<td>11.32</td>
<td>12.66</td>
</tr>
<tr>
<td>Father's years of education</td>
<td>12.05</td>
<td>10.91</td>
<td>12.93</td>
</tr>
<tr>
<td>Born into two-parent family</td>
<td>0.62</td>
<td>0.55</td>
<td>0.68</td>
</tr>
<tr>
<td>Low birth weight (&lt;5.5 pounds)</td>
<td>0.07</td>
<td>0.08</td>
<td>0.06</td>
</tr>
<tr>
<td><strong>Childhood neighborhood variables:</strong></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Neighborhood poverty rate</td>
<td>0.11</td>
<td>0.16</td>
<td>0.08</td>
</tr>
<tr>
<td>Residential segregation dissimilarity index&lt;sub&gt;county&lt;/sub&gt;</td>
<td>0.72</td>
<td>0.71</td>
<td>0.72</td>
</tr>
</tbody>
</table>

**Note:** All descriptive statistics are sample weighted to produce nationally-representative estimates of means. Dollars are CPI-U deflated in real 2000 $.
Table 2. 2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on the Likelihood of High School Graduation, by Child Poverty Status

<table>
<thead>
<tr>
<th></th>
<th>Prob(HS Grad)</th>
<th></th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Second Stage</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Ln(School District Per-pupil Spending)_{age 5-17}/.2</td>
<td></td>
<td>All Kids (1)</td>
<td>Poor Kids (2)</td>
<td>Non-Poor Kids (3)</td>
</tr>
<tr>
<td></td>
<td></td>
<td>0.1115***</td>
<td>0.1746***</td>
<td>0.0481</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.0317)</td>
<td>(0.0627)</td>
<td>(0.0347)</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>13,220</td>
<td>7,662</td>
<td>5,558</td>
<td></td>
</tr>
<tr>
<td>Number of School Districts</td>
<td>1,265</td>
<td>883</td>
<td>957</td>
<td></td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses (clustered at childhood county level)

*** p<0.01, ** p<0.05, * p<0.10

Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011.

Models: Results are based on 2SLS/IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). There exists a significant first-stage for both poor & non-poor kids.
Table 3. 2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Educational Attainment, by Child Poverty Status

<table>
<thead>
<tr>
<th>Dependent variable: Years of Education</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Ln(School District Per-pupil Spending)_{age 5-17} / .2</strong></td>
</tr>
<tr>
<td>All Kids</td>
</tr>
<tr>
<td>(1)</td>
</tr>
<tr>
<td><strong>0.3575</strong></td>
</tr>
<tr>
<td>(0.1472)</td>
</tr>
<tr>
<td>Number of Individuals</td>
</tr>
<tr>
<td>Number of School Districts</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses (clustered at childhood county level)

*** p<0.01, ** p<0.05, * p<0.10

Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011.

Models: Results are based on 2SLS/IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). There exists a significant first-stage for both poor & non-poor kids.
Table 4. 2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Earnings in Adulthood, by Child Poverty Status

<table>
<thead>
<tr>
<th>Dependent variable:</th>
<th>Ln(Wages), ages 25-45</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Second Stage</td>
</tr>
<tr>
<td></td>
<td>All Kids (1)</td>
</tr>
<tr>
<td>Ln(School District Per-pupil Spending)(age 5-17)/.2</td>
<td>0.0720*</td>
</tr>
<tr>
<td>(0.0428)</td>
<td>(0.0650)</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>13,220</td>
</tr>
<tr>
<td>Number of School Districts</td>
<td>1,265</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses (clustered at childhood county level)
*** p<0.01, ** p<0.05, * p<0.10

Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011.

Models: Results are based on 2SLS/IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender) and age. There exists a significant first-stage for both poor & non-poor kids.
Table 5. 2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Family Income in Adulthood, by Child Poverty Status

Dependent variable:

<table>
<thead>
<tr>
<th>Ln(School District Per-pupil Spending)_{age 5-17}/.2</th>
<th>All Kids (1)</th>
<th>Poor Kids (2)</th>
<th>Non-Poor Kids (3)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ln(annual Family Income), ages 25-45</td>
<td>0.1563***</td>
<td>0.2567***</td>
<td>-0.0210</td>
</tr>
<tr>
<td></td>
<td>(0.0517)</td>
<td>(0.0881)</td>
<td>(0.0567)</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>13,220</td>
<td>7,662</td>
<td>5,558</td>
</tr>
<tr>
<td>Number of School Districts</td>
<td>1,265</td>
<td>883</td>
<td>957</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses (clustered at childhood county level)
*** p<0.01, ** p<0.05, * p<0.10

Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011.

Models: Results are based on 2SLS/IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender) and age. There exists a significant first-stage for both poor & non-poor kids.
Table 6. 2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on the Annual Incidence of Poverty in Adulthood, by Child Poverty Status

<table>
<thead>
<tr>
<th></th>
<th>Prob(Adult Poverty), ages 25-45</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Second Stage</td>
</tr>
<tr>
<td></td>
<td>All Kids (1)</td>
</tr>
<tr>
<td>Ln(School District Per-pupil Spending)_{age 5-17}/.2</td>
<td>-0.0543***</td>
</tr>
<tr>
<td></td>
<td>(0.0175)</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>13,220</td>
</tr>
<tr>
<td>Number of School Districts</td>
<td>1,265</td>
</tr>
</tbody>
</table>

Robust standard errors in parentheses (clustered at childhood county level)

*** p<0.01, ** p<0.05, * p<0.10

Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011.

Models: Results are based on 2SLS/IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender) and age. There exists a significant first-stage for both poor & non-poor kids.
Appendix A: Reduced Form Effects of Court Mandated Reforms by Districts Income Level

Figure A1: Reduced Form Effect on Spending (PSID Sample) by District Income Level

Data: School District Data (1962-2000) matched with PSID geocode Data (1968-2011); district spending CPI-U deflated in real 2000 dollars. Analysis sample includes all school districts in which PSID individuals born 1955-1985 grew up and whom have been followed into adulthood. (N=13,220 individuals from 1,265 school districts, 894 counties, 46 states).

Models: Results are based on event-study models--parametric estimates--that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Main school finance reform variables include both intercept and slope terms to allow reforms to immediately affect district per-pupil spending following court mandate and to subsequently influence the trajectory of spending in the years following reform. Results presented by 1962 district income quartile within the relevant state to capture distributional impacts.
Figure A2: Reduced Form Effect of Court Mandated Reforms on High School Graduation by District Income Level

Data: School District Data (1962-2000) matched with PSID geocode Data (1968-2011); district spending CPI-U deflated in real 2000 dollars. Analysis sample includes all school districts in which PSID individuals born 1955-1985 grew up and whom have been followed into adulthood. (N=13,220 individuals from 1,265 school districts, 894 counties, 46 states).

Models: Results are based on event-study models--parametric estimates--that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Main school finance reform variables include both intercept and slope terms to allow reforms to immediately affect district per-pupil spending following court mandate and to subsequently influence the trajectory of spending in the years following reform. Results presented by 1962 district income quartile within the relevant state to capture distributional impacts.
**Figure A3: Reduced Form Effect of Court Mandated Reforms on Years of Education by District Income Level**

Data: School District Data (1962-2000) matched with PSID geocode Data (1968-2011); district spending CPI-U deflated in real 2000 dollars. Analysis sample includes all school districts in which PSID individuals born 1955-1985 grew up and whom have been followed into adulthood. (N=13,220 individuals from 1,265 school districts, 894 counties, 46 states).

Models: Results are based on event-study models--parametric estimates--that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Main school finance reform variables include both intercept and slope terms to allow reforms to immediately affect district per-pupil spending following court mandate and to subsequently influence the trajectory of spending in the years following reform. Results presented by 1962 district income quartile within the relevant state to capture distributional impacts.
**Figure A4:** Reduced Form Effect of Court Mandated Reforms in Low Income Districts on Educational Attainment by Childhood Poverty Status

**Graphs:**
- **Effect of Court-Ordered School Finance Reform on the Likelihood of High School Graduation, Among Poor Districts by Child Poverty Status**
- **Effect of Court-Ordered School Finance Reform on Educational Attainment, Among Poor Districts by Child Poverty Status**

**Data:** School District Data (1962-2000) matched with PSID geocode Data (1968-2011); district spending CPI-U deflated in real 2000 dollars. Analysis sample includes all school districts in which PSID individuals born 1955-1985 grew up and whom have been followed into adulthood. (N=13,220 individuals from 1,265 school districts, 894 counties, 46 states).

**Models:** Results are based on event-study models--parametric estimates--that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Main school finance reform variables include both intercept and slope terms to allow reforms to immediately affect district per-pupil spending following court mandate and to subsequently influence the trajectory of spending in the years following reform. Results presented by 1962 district income quartile within the relevant state to capture distributional impacts.
Figure A4: **Reduced Form Effect of Court Mandated Reforms in Low Income Districts on Adult Family Income and Adult Poverty by Childhood Poverty Status**

**Data:** School District Data (1962-2000) matched with PSID geocode Data (1968-2011); district spending CPI-U deflated in real 2000 dollars. Analysis sample includes all school districts in which PSID individuals born 1955-1985 grew up and whom have been followed into adulthood. (N=13,220 individuals from 1,265 school districts, 894 counties, 46 states).

**Models:** Results are based on event-study models--parametric estimates--that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, medicaid, AFDC, UI, Title-I (average during childhood yrs), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). Main school finance reform variables include both intercept and slope terms to allow reforms to immediately affect district per-pupil spending following court mandate and to subsequently influence the trajectory of spending in the years following reform. Results presented by 1962 district income quartile within the relevant state to capture distributional impacts.