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 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 US history. Between 1971 and 2010, school finance litigation cases were brought forth in 43 states; systems were overturned in 28 of them. This paper presents a comprehensive 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 the first section, using a long panel of annual school district data on per-pupil spending spanning 1962-2010, we present a rigorous event-study analysis of the effects of different types of school finance reforms on state distributions of school spending in both the short and long run. We analyze the efficacy of both court-mandated reforms and legislative reforms to narrow school spending disparities between rich and poor districts and raise the overall level of per-pupil spending. We find that court-ordered school finance reforms have been instrumental in moving toward the goal of equalizing per-pupil spending, and have worked primarily by raising spending at the bottom of the distribution, while spending at the top remained unchanged. Though both legislative and court-mandated reforms reduced inequality in the short run, only court-mandated reforms increased school spending for poor districts in the long run. Differences in effects across types of reforms stem largely from the nature of the spending formulas implemented and their impacts on the tax price of schooling (i.e., the amount by which a district has to raise taxes in order to increase school spending by $1). Formulas that entail high tax prices reduce long-run spending for high-income districts, and those that entail low tax prices lead to long-run increases in school spending, particularly for low-income districts.

In the second section, 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 (PSID)) 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 the passage of court-mandated reforms as an exogenous shifter of school spending. Results from event-study and instrumental variable models reveal that increases in per-pupil spending, induced by court-mandated school finance reform, 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. Some key patterns indicate that these improvements reflect the causal effect of these reforms: (a) no evidence of pre-existing time trends; (b) improvements were experienced only by cohorts within the same school districts that were exposed to the reforms, with impacts constrained only to
school-age years of exposure accompanied by no effects for non-school age years (whether pre-
school ages or beyond age 17); (c) improvements were monotonically increasing in years of
exposure and largest for those exposed to reforms for all 12 years of public school; (d)
improvements were larger in districts that experienced larger reform-induced increases in school
spending; and (e) no effects for children from non-poor families. These results persist with the
inclusion of school district fixed effects and extensive controls for childhood family
characteristics, race- and region-specific birth cohort effects, linear cohort trends in 1960 county
characteristics, and controls for other coincident policies (e.g., “War on Poverty” initiatives &
related safety-net programs; desegregation) that prevailed at the time these children were growing
up. The results imply that a $1,000 increase in per-pupil spending each year for all 12 years of
public school is associated with 0.178 more years of schooling, 14 percent higher earnings, and
an 8 percentage-point reduction in the annual incidence of poverty in adulthood.

I. INTRODUCTION

Ensuring equal educational opportunities for all children has been a long-standing
American ideal (Strickland, 1991; Browning & 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 & Miller, 1997; Hoxby, 1996). Persistently high levels of
residential segregation by race and economic status have often contributed to large inequalities
in per-pupil spending—often, though not always, a result of differences in the size of the local
tax base. For example, in 1970, on the eve of the first successful state litigation case with
regard to school finance, school spending varied dramatically, in multiples, even within the
same state. Because the local property tax base is generally higher in areas with higher home
values, exclusive reliance on localized financing creates a regressive tax system that contributes
to wealthier districts’ ability to spend more per student.1 In response to large within-state
differences in per-pupil spending across wealthy and poor districts, state Supreme Courts
overturned school finance systems in 28 states between 1971 and 2010 (Berry & Wysong,
2010), and many states have implemented legislative reforms that led to important changes in
public education funding.2 These SFRs caused some of the largest changes in the structure of K-
12 education spending in US history. Furthermore, nine states are currently reforming their
school finance rules, and ten states are in legal battles regarding public school financing.3

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 in 1971.
3 States that are reforming their school finance rules: New Jersey (Abbott v. Burke), Indiana (new funding formula
Accordingly, having an accurate description of the effect of these school finance reforms on school spending, educational attainment, and long-run adult outcomes is important.

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

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. As such, researchers were forced to make comparisons across long periods of time. This 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. (Also older studies look at only the first wave of reforms before 1990).

2. Do SFRs lead to enduring changes in spending? There is both empirical and theoretical work indicating that SFRs will affect marginal income tax rates (Anderson and McGuire, 2013), residential sorting (Tiebout, 1956), shifting of income sources for school spending (Brunner and Sonstelie, 2003), and will be capitalized into housing prices (Epple and Ferrerya, 2008). Also, there is evidence that wealthier districts find ways to undo the effects of school reforms by altering voluntary, private contributions (Brunner, & Sonstelie, 2003), and that many states and localities pass laws to tweak or undo the effects of existing reforms (Imazeki and Reschovsky, 2004). For these reasons, the effect of SFRs on school spending inequality in the short run might be quite different from those in the long run.


4 California*, New York, Arizona, New Jersey*, Washington, Wisconsin*, Idaho, and Kansas* 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.


How do different kinds of reforms affect the distribution of school spending in the short and long run? There are substantial differences across states in how they 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.

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 school quality, that arose from per-pupil spending differences, between children from poor and affluent families, which have consequences on socioeconomic well-being in adulthood. However, the extent to which improvements in student outcomes was achieved is largely unknown. Hoxby (2001) finds mixed evidence on the effect of increased per-pupil spending associated with SFRs on high school dropout. The effects on test scores are also decidedly mixed. Card and Payne (2001) 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.7 In contrast, Figlio and Downes (2005) find that finance reforms in response to court mandates do not result in significant changes in either the mean level or the distribution of test scores.8 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 et al, 2013; Jackson, 2013).9 Accordingly, the effect that SFRs may have on long-run outcomes remains unknown.

This paper presents a comprehensive analysis of the effects of SFRs on the level and distribution of school district spending as well as their impacts on subsequent educational and economic attainment outcomes in adulthood. The analysis proceeds in two stages. In the first section, we aim to address the first three questions and investigate the effects of school finance reforms on district spending, both in terms of absolute levels in poor districts, and in equalizing spending between districts in a state. We address these questions using newly-released, school district-level panel data on per-pupil spending that go back to 1962, 10 years before the first reforms, and which are available annually from 1970 through 2010. We compile a

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7 As acknowledged by the authors, the data used in this study may suffer from selection to SAT taking.
8 However, Downes & Figlio (1998) find that plans that impose tax or expenditure limits on local governments reduce overall student performance on standardized tests.
9 See also Lindqvist & Vestman, 2011; Heckman & Rubinstein, 2001; Borghans, Weel, & Weinberg, 2008; Waddell, 2006, Heckman, Pinto, & Savelyev, forthcoming; Deming, Hastings, Kane, & Staiger, forthcoming.
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 codify school finance reform efforts into several types, based on the ways the reform influenced the tax price of schooling (i.e., the amount by which a district has to raise taxes in order to have an increase in school spending of $1) and the proportion of revenue that is generated from local vs state/federal sources. 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 surrounding the timing of reforms and assess the degree of pre-existing trends in spending leading up to the enactment of reforms.

Armed with the longest district-level panel on spending that has ever been used to analyze these issues, we aim to address the second question by documenting 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 address the third question by distinguishing the effects of different types of reforms on school spending. We analyze the efficacy of both court-mandated reforms and legislative reforms to narrow school spending disparities between rich and poor districts and raise the overall level of per-pupil 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 for several years before as well as 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 – indicative of a causal effect of SFRs on per-pupil spending. Consistent with previous findings, SFRs tend to reduce inequality in spending between low income 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 tend to increase spending overall by increasing spending for low-income districts and somewhat reducing spending for the
highest income districts. These effects reach their maximum after about 15 years and persist over time. In contrast, legislative reforms do reduce spending inequality, but do so primarily by reducing spending for high-income districts, and the effects only persist for about 10 years – suggestive of wealthier district lobbying to undo the effects of SFRs and finding loopholes (ref). Consistent with Hoxby (2010), the differences in the effects by reform type can be largely explained by the kind of funding formulae used and their impacts on the tax price of schooling (i.e., the amount by which a district has to raise taxes in order to increase school spending by $1). Funding formulas that impose spending limits and high tax prices on districts are associated with large reductions in spending particularly for higher income districts; these are more commonly used under legislative reforms. Conversely, funding formulas that match district effort to raise local funds and impose low tax prices are associated with increased spending particularly for lower income districts; these are more commonly used under court-mandated reforms.

In the second part of the paper, we address the final question of the effects of reform-induced changes in per-pupil spending on long-run educational and economic attainment outcomes. In order to address this question, 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 that prevailed at the time these children were growing up, 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. With these linked data, we then analyze the effects of reform-induced changes in school spending by identifying those districts that experienced larger increases in school spending and comparing the adult outcomes of individuals who were exposed, at varying degrees, during childhood to larger versus smaller spending increases due to SFRs.

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10 Adequacy based cases lead to increases in per-pupil spending for all districts (with larger increases for poor districts) while equity-based cases seem to reduce measures of inequality with little effect overall.
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 resultant 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/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 who are from the same districts but were born in different years, there are children who were too old to be affected by reforms at the time of passage (not treated), those who were old enough to be treated for some fraction of their school-age years (partially treated), and those who 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 reform, 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. Some key patterns indicate that these improvements reflect the causal effect of these reforms: (a) no evidence of pre-existing time trends; (b) improvements were only experienced by cohorts within the same school districts that were exposed to the reforms, with impacts constrained only to school-age years of exposure accompanied by no effects for non-school age years (whether pre-school ages or beyond age 17); (c) improvements were monotonically increasing in years of exposure and largest for those exposed to reforms for all 12 years of public school; (d) improvements were larger in districts that experienced larger reform-induced increases in school spending; and (e) no effects for children from non-poor families. These results persist with the inclusion of school district fixed effects and extensive controls for childhood family characteristics, race- and region-specific birth cohort effects, linear cohort trends in 1960 county characteristics, and controls for other coincident policies.
(e.g., "War on Poverty" initiatives & related safety-net programs; desegregation) that prevailed at the time these children were growing up. The results imply that a $1,000 increase in per-pupil spending each year for all 12 years of public school is associated with 0.178 more years of schooling, 14 percent higher earnings, and an 8 percentage-point reduction in the annual incidence of poverty in adulthood.

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

The remainder of the paper is organized as follows. Section II describes the policy landscape and details 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. Section IV 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 V presents the data used for the second part of the analysis. Section VI outlines the triple-difference empirical strategy for identifying the effects of SFRs on long run outcomes. Section VII presents both event study and instrumental variables regression results for the effect of school spending on longer run outcomes, and Section VIII concludes the paper with summary discussion.

II. A Discussion of School Reforms and the School Finance Data

The centerpiece of equity for children is having the same educational opportunity irrespective of place of residence, race/ethnicity, gender, etc. The judicial landmarks of the school desegregation cases provided part of the basis upon which the movement toward school finance reform litigation and debates about the constitutionality of local finance systems would be waged (Johnson, 2013). School finance cases were founded on the basis that existing local systems of school finance violated the Equal Protection Clause of the Fourteenth Amendment of the US constitution or relevant state constitution, as school resources would then be a function of a local communities’ wealth. Supporters of the litigation claimed that local financing
violated the responsibility of the state to provide a quality education to all children. School finance commentators and litigators argued 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. Often, despite the greater tax effort by residents in these poor school districts, they would end up with less money per pupil because of the difference in assessed wealth. Some analysts argue that political factors limit the effectiveness of legislative solutions, as most of the children adversely affected fall into groups who lack sufficient political clout. For example, poor people vote at much lower rates in elections.

Accordingly, achieving fiscal (wealth) neutrality became an explicit goal of many state aid distribution formulas. Most state school finance formulas aim to achieve two main related objectives:

1) account for differences in the costs of achieving equal educational opportunity across schools and districts;
2) 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 legal language used in the case or legislation to classify types of reforms. In contrast, economists studying this topic have emphasized the economics of taxation and redistribution in order to characterize the nature of the income and price incentives embedded in different aid schemes (e.g., Hoxby, 2001). We also take this latter approach.

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\textsuperscript{11} (PSFP), and the National Access Network’s state-by-state school finance litigation map (2011). We supplement these data with reform descriptions and classifications from Murray, Evans, and Schwab (1998), Hoxby (2001), Card and Payne (2002), Hightower et al (2010), and Baicker and Gordon (2004). In most cases, data from these sources are consistent.

with each other. Where there are discrepancies we deferred to *PSFP*, and consulted state court and legislation records for validation.\textsuperscript{12} From these various sources, we compiled a comprehensive dataset of each school finance reform between 1970 and 2010.\textsuperscript{13}

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

Figure 1 presents the total number of states that have 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 have some form of SFR; 23 states had at least one court mandated reform and 32 states had at least one

\textsuperscript{12} There were discrepancies in the timing of overturned court cases in several states: Connecticut (Hoxby stated the decision was made in 1978, but Card & Payne report it was made in 1977), Kansas (Hoxby state 1976, but PSFP and ACCESS report 1972), New Jersey (Card & Payne state 1989, but PSFP says 1990), Washington (Murray, Evans & Schwab, Hoxby, and Card & Payne report 1978, but PSFP report 1977), Wyoming (Hoxby says 1983, but Card & Payne and Murray, Evans & Schwab report 1980). In each case, we researched the court case by name to discover the true date of the decision.

\textsuperscript{13} (cite data sources)…put in data appendix.
legislative reform, and 45 has some change in funding formulas. 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.

**Figure 2:** *Type of Cases over time*

These two waves of legislative reforms were distinct not only in time, but also in motivation (Briffault, 2005). The first challenges to state school finance started in the 1960s and were based on federal constitutional law. These challenges were unsuccessful and led to the first wave of successful challenges based on state constitutional law. These cases seek either greater equity in funding among school districts (equity cases) or adequate funding for education (adequacy cases). The first and second wave of SFRs differed in their legal language. Figure 2 presents the number of states that had ever had a court-mandated SFR on equity and adequacy grounds, separately, 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 school spending is an empirical question that we address in Sections III and IV.
a. Classifying Reforms

While different reforms may have been passed 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 spending can be expressed as [1] below, where federal and state funding did not vary much across districts within a state prior to reforms.

\[ \text{per pupil expenditure} = (\text{local tax rate}) \times (\text{Local Tax Base} \div \# \text{ of students}) + \text{Federal} + \text{State} \]

Inspection of [1] makes clear that \textit{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 \textit{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). As such, for both these reasons there is a tendency for wealthier districts to spend more per pupil on education than poor districts (Hoxby 2001). SFRs changed the parameters of this formula to help reduce the strength of the relationship between the level of educational spending and the wealth of the district. Because school finance formulae are complex and there is no single strategy employed by all or even most states, most researchers categorize states’ SFRs based on characteristics of the state’s school financing formula.

Card and Payne (2001) codify formulas into three broad categories: \textit{flat grant plans} that give the same dollar amount per student to all districts in a state; \textit{minimum foundation plans} that set a floor on per-pupil spending. Under such plans the state provides the difference between the minimum amount per pupil and an estimate of how much local revenue a given district can raise; \textit{variable grant plans} that provide different amounts of state aid to districts based both on local property values income levels in addition to how much local revenues are actually raised.

Hoxby (2002) 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 \textit{more} spending by providing more state funding for districts that raise more local funds, while others induce \textit{less} spending on the margin by providing more state funds to districts that raise less 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 they raise tax revenue by 1 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) argue that zero inverted tax prices are an economically important feature on which to classify reforms. An inverted tax price greater than 1 means that a district can raise education spending by more than 1 dollar by raising tax revenue by 1 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). Also, Augenblick et. al. (2007) very 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 five categories. Note that most state funding plans may 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. They are below:

- **Foundation Plans**: Plans that ensure a basic floor to spending. These include foundation plans, foundation grants, guaranteed minimum tax base plans. These plans establish a foundation level of per-pupil spending, estimate a districts’ required local contribution to fund this foundation based on income and wealth levels in the district, and provides the difference between the expected contribution and the foundation level.
  - These plans do not affect tax prices. They provide extra funding to low income/wealth districts while leaving high income/wealthy districts largely unchanged.
- **Flat grant**: Give aid on a per pupil basis to all districts.
  - These plans do not affect tax prices. The plans provide similar state funds for all districts and should have little effect on spending inequality ceteris paribus.
- **Equalization Plans**: Provide aid to districts based on property values, and income levels.
  - These include power equalization plans (give more money to low wealth districts and less to wealthy districts), categorical aid schemes (give money to low-income districts), and other

spending equalization plans that more 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 (they may however provide incentives to alter the tax base). These plans tend to provide extra funding to low income/wealth districts while often taking money away from high income/wealth districts.

**Reward for Effort plans (inverted tax prices greater than 1):** These schemes seek to promote local effort 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 the additional state aid is increasing in local taxes collected.\(^\text{15}\)

- These plans are designed to promote local efforts to raise education spending by targeting the inverted tax price directly. Such plans typically provide greater incentives for lower income/wealth districts to increases taxes by allows some districts to have more than 1 dollar in spending for each dollar raised in taxes. Such policies should increase spending overall. If all districts are equality sensitive to tax prices we should see larger spending increases for low-income districts. However, if low-income districts are less responsive to the inducement (due to a low valuation for education spending on the margin), then increases in spending could be smaller for low-income areas.

**Spending limits (inverted tax price equal to 0):** Under such plans the state imposes a limit on how much a local district can spend on education. Also, some equalization plans might take away all tax revenues raised above a certain amount (i.e. 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 an inverted tax price of zero.

- These plans are designed to limit education spending at the local level for high spending districts. Because high-income districts are also those that 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

\(^{15}\) For example, in Georgia, school districts at or below 75% of the state average property tax wealth level receive equalization funding in proportion to the amount of mills they raise above the required 5mills.
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

Figure 3: Types of Spending Formulae

Since 1970, every state has enacted at least one such aid formula. To provide an overview of the evolution of school finance formulas, Figure 3 plots the number of states that employ 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 has increased slightly during the entire period (27 states in 1970 and 36 states in 2010). As more states implemented SFRs, the use of flat grants declined (26 states in 1970 and 5 states in 2020), while the use of equalizing plans increased (9 states in 1970 and 30 states in 2010). While unpopular in 1970, there is an increase in states employing reward for effort over time (0 states in 1970 and 21 states in 2010) and those imposing spending limits (0 states in 1970 and 12 states in 2010). In Section III we investigate the effects of these different kinds of reforms on the level and distribution of school spending.
c. Spending

Data on district and state funding come from the Census of Governments, the Historical Database on Individual Government Finances (INDFIN)\textsuperscript{16}, the Common Core of Data (CCD) School District Finance Survey (F-33). The Census of Governments was recorded every five years between 1967 until 1992, 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 contain school district finance Data annually for a subsample of large school districts from 1967 through 1991.\textsuperscript{17} After 1992, the Common Core of Data (CCD) School District Finance Survey (F-33) consists of data submitted annually to the National Center for Education Statistics and includes data on school spending for every school district in the United States\textsuperscript{18}. 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.

The focus of this paper is 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 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).

There are a few notable patterns. First, per-pupil spending has been increasing over time in all districts (recall that these are in 2012 dollars). The average district spent about $4,612 per student in 1967 and spent about $12,772 per student in 2010. This resents a 175 percent increase

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\textsuperscript{16} 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.

\textsuperscript{17} Several states are missing per-pupil spending data from before 1992: Alaska, Hawaii, Maryland, North Carolina, and Virginia. Washington DC is also missing per-pupil spending data prior to 1992. Per-pupil spending data from 1968 and 1969 was also missing in all states. Spending data in Florida was also missing for 1975, 1983, 1985-1987, and 1991. Spending data in Kansas was also missing for 1977 and 1986. Spending data in Mississippi was also missing for 1985 and 1988. Spending data in Wyoming was also missing for 1979 and 1984. Spending data for Montana is missing in 1976, data for Nebraska is missing in 1977, and data for Texas is missing in 1991. Where there was only a year or two missing, we filled in this data using linear interpolation.

\textsuperscript{18} 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.
(in real terms) over 43 years. This increase of about 4 percent annual growth was experienced in both low and high-income districts. Another notable pattern is that the difference between high and low-income districts was wide in the early 70s, narrowed during the late 70s (corresponding to the first wave of reforms), was stable during the 1980s, and then narrowed again in the mid-90s (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.

**Figure 4**

A comparison of Figure 3 and Figure 4 provide suggestive evidence for 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 emphasize adequate spending for low-income districts and relatively rapid increases in the use of reward for effort plans. That is the timing of the reversal...
is coincident with the increased use of reforms that one might ex ante 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 that we investigate in the following section.

III. Event Study Analysis

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 above, 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 the 1976 and 1972 in Illinois is greater than the difference in spending for low-income districts between 1976 and 1972 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 [1] below.

\[
S_{dt} = \alpha + (Q_d \cdot \sum I_y^{court}) \cdot \pi_{q,y}^{court} + (Q_d \cdot \sum I_y^{legis}) \cdot \pi_{q,y}^{legis} \cdot \theta_d + \theta_t + \theta_s + \epsilon_{dt} \tag{1}
\]

In [1], $S_{dt}$ is spending in district $d$ in state $s$ in year $t$, $\theta_d$ is a district fixed effect (which subsumes a state fixed effect), $Q_d$ is an indicator for the percentile of the districts median income in the state distribution in 1962 (this is fixed within a district over time), $\theta_t$ is a year fixed effect, and
\( \varepsilon_{dt} \) 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 \( I_y^{\text{court}} \) and \( I_y^{\text{legislate}} \) which 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 variable map out the dynamic treatment of the two broad types of reforms and are interacted with \( Q_d \) (the percentile of the districts median income in 1962). The coefficients \( \pi_{q,y}^{\text{court}} \) map out the dynamic treatment effect of the first court mandated reform on per-pupil spending for districts in quartile \( q \). Similarly, the coefficients \( \pi_{q,y}^{\text{legislate}} \) map out the dynamic treatment effect of the first legislative reform on per-pupil spending for districts in quartile \( q \). For example, \( \pi_{1,10}^{\text{legislate}} \) is the effect today in a bottom quartile district of implementing the first legislative ten 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 in the bottom quartile five year 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-existing trends in spending and any structural break in outcomes around the timing of reforms.

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. That to estimate the dynamic treatment effect for particular types of funding formulae we can estimate equation (1) where we replace the reform type indicators with \( I_y^{\text{limit}}, I_y^{\text{localeq}}, I_y^{\text{foundation}}, I_y^{\text{equalization}} \) which 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:

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

The test of whether this computed difference is statistically significant is simply testing the statistical significance of a linear combination of the estimated coefficients.\(^\text{19}\) 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 percentile 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 ten years prior to the first court-mandated reforms through twenty years after the reforms. The evolution of spending is presented separately for districts in the bottom 10 percent of median incomes, those in the 10\(^{th}\) to 25\(^{th}\) percentile, those in the 25\(^{th}\) to 50\(^{th}\) percentile, those in the 50\(^{th}\) to 75\(^{th}\) percentile, those in the 75\(^{th}\) to 90\(^{th}\) percentile, and those in the top 10 percent. As such, 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 the districts in the bottom 10 percent of the state income distribution for states that did not have a court mandated reform over the same time period. To allow for one to see how per-pupil spending was affected for all districts on the same scale, each series is re-centered around the average for the ten 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 ten-year average prior to reforms. Also, note that year zero 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 zero during the pre-reform years. However, a skeptical reader be inclined to see a slight upward trend in spending for the bottom 10 percent income districts (we will account for this in our regression estimates). During the 10 years prior

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\(^{19}\) Both the estimates and the standard errors of these estimates can be computed using the “lincom” command in STATA.
to reforms (years -10 through 0), districts of all income levels in reform states saw very similar changes in per-pupil spending as districts in non-reforms states of the same income level. Within the first 5 post reform years however, districts in the bottom quartile (sold black lines) see systematic increases in per-pupil spending above and beyond comparison districts in non-reform states. For districts in the 25th to 75th percentile, one can see 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.

**Figure 5**  *Effect of Court-mandated reforms*

To better quantify the patterns in Figure 5, we estimate the effect of court-mandated reforms as the difference between the average effect in the 10 years prior to reforms and the 10 years post reforms. Based on the linear combination of coefficient estimates, these reforms increased per-pupil spending for the bottom 10% income districts over the first ten 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 so that this represents 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 ten years prior to reforms. This
calculation indicates that after five years these reforms increased per-pupil spending for the bottom 10% income districts by $730.27 (p-value=0.03). This represent a long run increase in spending for low-income districts of about 10 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 ten years by $131.89 (p-value=0.47) and in years five through ten 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 reduce 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 1990’s 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 kinds of cases lead to different kind of reforms that have different effects. This question was investigated empirically by XXXX and XXXX (2002) who found no difference between these two kinds of cases in a simple regression setting. We investigate this question using the more flexible event-study approach. 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 $XXX.XX (p-value=0.0X) after five years post reform. The aims of these cases was to increase spending equity, as such it is clear that reforms induced by these equity based cases achieved the objective.
Figure 7 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 that all children (especially those in low-income districts) received adequate resources for a quality education. Because these cases are much more recent, we present the dynamic treatment for the first 7 years of the reform. As one can see, spending in all districts in states with adequacy cases was relatively stable (relative to non-reform districts) prior to reforms. While there may be a slight uptick in spending 5 years prior to reforms, the trajectory of spending is 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 an increase in spending of over 1000 within the first five years. However, because there are spending increases experienced by all districts adequacy cases have smaller effect of spending gaps than equity cases. About 7 years after and adequacy case, estimates suggest that the gap in spending between the highest- and lowest-income districts is narrowed by only $367 (p-value=0.XX). In sum, consistent with the stated aims of the legal language, equity cases led to greater equity in school spending, while adequacy cases lead to increased school spending overall with particularly large increase for low-income districts.
b. Event Study Analysis for Legislative Reforms

While legislative reforms have received much less attention in the literature than court-mandated reforms, the consensus seems to be that legislative reforms were largely ineffective at increasing school spending for low-income districts or reducing spending inequality. To interrogate this 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 come on the state in 1962, and 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 largely similar trajectory of per-pupil spending as other non-reforms states the few years preceding the reforms. However, in the post reform years there is a clear downward relative trend for all districts. There is also 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 the 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 in addition to inducing slower spending growth overall, legislative reforms reduced spending inequality between low income and high-income districts.

**Figure 6**  *Effect of Legislative Reforms*

![Effect of Legislative Reforms](image)

The point estimates tell a similar story. In the 10 years after reforms the lowest income districts experience a $569.70 reduction in spending (p-value=0.12). Between years five and ten the reduction for these low-income districts is $730.45 (p-value=0.09). Consistent with the persistent slowdown in spending growth, between years ten and fifteen, these reforms reduce spending in low-income districts by $930.41 (p-value=0.02) relative to the ten years prior to reforms. Looking to the top 10 percent of districts, the patterns are very similar. These reforms are associated with an $846.22 in the first ten years post reform (p-value=0.03), and a $1036.58 reduction (p-value= 0.02) between years five and ten. 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 ten years prior to reforms, the spending gap between the top 10 and bottom 10% income districts was reduced by $276.43 (p-value=0.13) in the ten years after reforms. While this the estimated change is not
statistically significant at traditional levels, the effect size is economically important. It represents a 20 percent reduction in the spending gap between high and low-income districts. In fact a parametric model with a simple before/after indicator variable yields a p-value of 0.04 for the change in the spending gap (appendix table X). We conclude from this that legislative changes tended to level down (Hoxby, 2002) and likely has modest effects on spending inequality between low income and high-income districts.

c. Effects by Type of Formula Used

While accurately documenting the effects of these historical reforms is important from an historical point of view, because there are numerous ways that reforms can be constructed, it does not address the policy-relevant question of why different kind of reforms have different effects, or what kinds of reforms policy-maker should try to implement in the future. In fact, it can be argued that what really matters is the kind of funding formula used in a reform that matters 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.

Figure 7 shows the event study graphs for the imposition of spending limits. First one can see that there is little evidence of any differential pre-existing trending 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 very stale 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 for the wealthier districts in the state. The fact that the reductions in spending (relative to the relatively flat trend prior to the change) grow over time is consistent with there being a spending limit that becomes more likely to bind as the underlying level of spending increases for all district to the level of the limit. One would expect it to bind first for the highest spending district, then as overall spending increase it will bind for lower spending districts. This is precisely the pattern observed in the figure; 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 ten years after reforms. However, between yeas ten and twenty 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 ten years after reforms. The reduction increases to $1564.14 between years ten and twenty. 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 five 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.

**Figure 7**  
*Effect of Spending Limits (0 inverted tax prices)*

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 that we refer to as “Reward for Effort” policies. Figure 8 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 (5 out of 7 of the changes are negative realizations for the lowest income districts) there is an upward trend that lasts about 5 years (4 out of 5 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.

Figure 8  Effect of Local Equalization (inverted tax prices > 1)

Because of the pre-existing negative trend, estimating the effects on level with a difference in difference model is unwise because 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 estimating equation [1] 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.
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 five 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 five years, there is not statistically significant difference between the growth rates for port reform years 5 through 10 and the pre-reforms years (both yield p-values above 0.1). However, there is suggestive evidence of increased spending growth for the lowest income districts in the long run such that during port reform years 10 to 20, average annual spending changes were $175.88 more (p-value=0.08) than the pre-reform years. This is consistent what the analysis in levels that reveal 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 (that lasts 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 to consider are foundation plans and equalizing plans (Figure 9). Both kinds of plans generally adjust state spending such that districts with low tax bases (rather than income) receive additional funds from the state. For both these types of reforms spending behaviors were erratic prior more than five years prior. Accordingly, the figures only plot the four years before reforms and all statistical inference are relative to the five years prior to reforms (when behaviors were more stable). The figure reveals that for both kinds of plans low income and high-income districts were on similar trajectories as each other (and districts on other states) for the five years prior to reforms. After reforms both kind of plans experience 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 increase per-pupil spending (relative to the four years prior to reforms) by $464.03 (p-value=0.06) in the ten 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 ten years after reforms. Equalization plans had a very similar effect; there were increases for low-income districts ($529.0792) and small decreases for high-income districts ($47.10) such that the gap in spending was reduced by 576.1793 (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 did so primarily by increases in per-pupil spending for the lowest income districts in the state.

20 For figures that show outcome 10 years prior to reforms, see the appendix.
In sum, the figures reveal that, by and large, school finance reforms achieved the stated objective of reducing inequalities in school spending between low- and high-income districts and increased the level of per-pupil spending in poor communities. The results indicate that 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 affected the distribution of school spending, the remaining question is how changes in school spending cause by these reforms affected the educational and adult outcomes of affected children. This is the topic of part II of this paper.
V. 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 neighborhood block level in which children grew up.\footnote{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 sample today includes numerous adult sibling groupings who have been members of PSID-interviewed families for more than four decades.}

We link our district-level data on school spending and the timing of reforms to the nationally-representative sample of children born between 1955 and 1985 from the PSID. Following Johnson (2011), we then merge neighborhood and school characteristics, and 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, which allow for a rich set of control variables.\footnote{This includes measures from 1968-1988 Office of Civil Rights (OCR) data; 1960, 1970, 1980, 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 entire 1955-1990 period (American Communities Project); and American Hospital Association’s Annual Survey of Hospitals (1946-1990) and the Centers for Medicare Provider of Service data files (dating back to 1960s) to identify the precise date in which a Medicare-certified hospital was established in each county of the US (an accurate marker for hospital desegregation compliance).}

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.\footnote{The PSID maintains extremely high wave-to-wave response rates of 95-98%. Appendix C discusses the extent to which sample selection, including mortality, may bias the reported estimates. Studies have concluded that the PSID sample of heads and wives remains representative of the national sample of adults (Gottschalk et al, 1999; Becketti et al, 1997).} 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 black and low-income families, 59 percent of the sample was poor as children (N=15,353 individuals; 9,035 poor kids; 6,318 non-poor kids).
School Measures. 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)\(^24\). 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.

Sixty-six percent of the PSID individuals born between 1955-1985 followed into adulthood grew up in a school district that was subject to a court-mandated school finance reform sometime between 1972 and 2000 (i.e., X,XXX out of 15,353 individuals), with the timing of the court order not necessarily occurring during their school-age years. Eighty-eight percent of the PSID individuals born between 1955-1985 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 (i.e., X,XXX out of 9,035 poor kids). The share of individuals exposed to court-ordered school finance reforms or legislative reforms during childhood increases significantly with birth year over the 1955-1985 birth cohorts analyzed in the PSID sample (Figure 5).

We merged the school district expenditures data, 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 the 5 data sources, the main sample used to analyze adult attainment outcomes consists of PSID individuals born between 1955-1985, and includes 93,022 adult person-year observations of 15,353 individuals (9,035 poor kids; 6,318 non-poor kids) from 1,409 school districts, 1,031 child counties, and from all 50 states. The mean age is about XX for the economic outcome measures considered, with age ranging from 25 to 45, and an average of XX observations per person (of valid adult income observations). The appendices and Appendix Table C0 lists the sources and years of all data elements along with details of the PSID survey questions used to construct key measures. Summary descriptive statistics are presented in Appendix Table C1 broken out by 1962 (pre-reform) district income quartile within the relevant state.

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\(^{24}\) A substantial share of school districts was counties during this period, including more than one-half of Southern school districts.
Outcomes of interest. The set of adult attainments examined chronologically over the life cycle include: 1) educational outcomes—whether graduated from high school, years of completed education, college quality (proxied by 25th and 75th percentiles of SAT scores of the freshman class of college attended); and 2) labor market and economic status outcomes (all expressed in real 2000 dollars)—log wages, family income, annual incidence of poverty in adulthood (ages 25-45). All analyses include men and women with controls for gender, given well-known gender differences in labor market outcomes. This data is combined to provide new evidence on the long-run impacts of school finance reform and school resources.

VI. Empirical Strategy.

Our primary empirical approach uses the staggered timing of court-mandated school finance reforms across districts to implement an “event-study” analysis, which estimates the precise timing of changes in school spending and resultant impacts on adult attainment outcomes, separately by childhood poverty status. This event-study framework also allows one to examine pre-reform trends in outcomes and test for potential endogeneity in the timing of court-ordered reforms. The existence of systematic, pre-reform trends could indicate that the timing of court orders was endogenous to factors affecting post-reform outcomes.

We first build upon the results presented in the first section documenting the reform-induced changes in per-pupil spending patterns by 1962 (pre-reform) district income quartile (within the relevant state). We show that these same patterns of results are found among the subset of districts that overlap the original childhood locations of our PSID cohorts born between 1955 and 1985 (Figure X). In particular, impacts of court-mandated SFR are most pronounced for poor districts, with statistically significant increases in average per-pupil spending during one’s teenage years (ages 12-17) of $1,200 among children from poor districts who were exposed to SFR throughout their school-age years relative to children from the same district who were already beyond school-age when the court mandate first occurred. A few additional important things to note from this figure is that we find no pre-existing time trend in spending and no additional estimated significant spending impacts beyond school-age years of exposure; these patterns affirm the credibility of our research design.
Accordingly, we begin by estimating reduced-form models of adult attainment outcomes on the number of school-age years of exposure to court-mandated SFR, allowing differential effects by 1962 (pre-reform) district income quartile. The district income quartile, in these initial models, proxies the intensity of treatment (i.e., the direction and amount by which reforms affected per-pupil spending) on average (as seen in Figure X). Figure Ya and Yb present these results for the likelihood of high school graduation and educational attainment, respectively, and show impacts for poor and non-poor children on the same graph. Similarly, Figure Za and Zb present these results for adult earnings and family income, respectively. The pattern of results is consistent with a strong relationship between school spending and adult attainment, particularly for poor children, as increases in the duration of SFR exposure in poor districts (which experienced the largest reform-induced changes in spending) are associated with larger improvements in adult socioeconomic attainments. We do not see comparable magnitudes of improvements in outcomes with duration of SFR exposure in districts that experienced modest changes in spending.

Our main event-study models used to analyze the impacts of reform-induced changes in school spending on adult attainment involve estimating equations of the form\(^{25}\):

\[
Y_{icb} = \sum_{t=T-20}^{2} \alpha_{t-T} \cdot I(t_{icb} - T_{c}^* = t - T) \cdot SPEND_c + \sum_{t-T=0}^{12} \theta_{t-T} \cdot I(t_{icb} - T_{c}^* = t - T) \cdot SPEND_c + \sum_{t-T=13}^{20} \delta_{t-T} \cdot I(t_{icb} - T_{c}^* = t - T) \cdot SPEND_c + X_{icb} \beta + Z_{cb} \gamma + (W_{1966c} \ast b) \phi + \eta_c + \lambda_{c} r + \phi_g \ast b + \epsilon_{icb}
\]

(2)

where \(i\) indexes the individual, \(c\) the school district, \(b\) the year of birth, \(g\) the region of birth (defined by 9 census division categories), \(r\) the racial group, \(SPEND_c\) is the SFR-induced change in per-pupil spending in district \(c\); and the indicator variables, \(I(t_{icb} - T_{c}^* = t - T)\), equal one 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 \(c\) \((T_{c}^*)\) equals a value between -20 and 20, which is the full support of years individuals were age 17 relative to initial court order years in the sample.

\(^{25}\) 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.
For example, values for \( t_{icb} - T^* \) 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 zero when the year in which an individual is age 17 (typically, senior year in high school) equals the initial year of court-mandated SFR for the school district in which the person grew up.

This can be viewed as 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 school spending due to SFR (variation in intensity). The event study framework allows one to inspect whether districts that underwent larger changes in school spending resultant from SFR exhibited differential trends in outcomes preceding the enactment of court orders, which we use as an additional specification test.

For each district, we compute the change in school district per-pupil spending induced by the court order from the year preceding enactment to the first several years following it. In particular, we use the estimated district-specific SFR-induced change in per-pupil spending from expanded models estimated in Section II that are net of any underlying state-specific time effects, district trends, and a host of other coincident policy changes. We then exploit variation in the intensity and variation in exposure years of SFR to assess whether there is evidence of a dose-response effect of school spending improvements on subsequent education and economic attainment outcomes.

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 \((\eta_c)\), race-specific birth year fixed effects \((\lambda_r^g)\), race-by-region of birth cohort trends \((\varphi^r*b)\), and controls for an extensive set of child and childhood family characteristics \((X_{icb}: \text{parental education and occupational status, parental income, mother’s marital status at birth, birth weight, child health insurance coverage, gender})\). The set of controls also involve interactions between 1960 characteristics of the county of birth and linear trends in the year of birth \((W_{1960}^c*b): 1960 \text{ county poverty rate,}\)
percent black, average education level, percent urban, population size), as further controls for trends in factors hypothesized to influence the timing of SFR.

The period in which school finance reform began overlaps other important coincident policy changes, including the implementation of school desegregation (Johnson, 2011), hospital desegregation in the South (Chay, Guryan, Mazumder, 2011), and the roll out and significant expansion of the safety net via War on Poverty and Great Society programs and initiatives (Almond, Hoynes, Schazenbach, 2013). To account for these policy changes, we directly include county-level measures that capture the geographic timing of school desegregation and hospital desegregation (exposure based on place and year of birth), roll out of community health centers, state-funded initiatives for kindergarten introduction, Head Start per-capita expenditures at age 4, per-capita expenditures from Title-I school funding, and per-capita expenditures on food stamps, AFDC, Medicaid, unemployment insurance, each averaged over the individual’s childhood years ($Z_{cb}$). The data sources used to compile these measures are detailed in the Data Appendix. While this work draws heavily from prior research that have examined these other policy impacts, few studies have attempted to simultaneously account for such a comprehensive set of policies, in this case to isolate the causal impact of school finance reform-induced changes in per-pupil spending. The models that analyze the economic and health status outcomes of interest use all available person-year observations in adulthood (for ages 25-45) with controls for age, age squared and age cubed to avoid confounding life cycle and birth cohort effects.

Estimation of equation (2) provides an unrestricted description of the subsequent adult attainment outcomes in relation to the cohort- and district-specific timing of reform-induced changes in school spending, $\theta_{t,T}$. The estimates of $\theta_{t,T}$ 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; while the estimates of $\alpha_{t,T}$ provide a precise visual portrait of whether there are systematic time trends preceding enactment of court-ordered SFR. 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.
We present graphical plots, based on equation (2) estimates that form the response function of reform-induced effects of per-pupil spending to test for any dose-response with years of exposure and resultant amount of spending change. Furthermore, estimated effects beyond the maximum 12 school-age years of exposure (\( \delta_{r-T} \), for event study years \((t-T)>12\)) provide an additional specification test, as these should not exhibit significant trends in outcomes since these additional years do not represent any change in school-age exposure. This motivates fitting a parametric version of the unrestricted event study model above that provides simple summaries of the magnitudes and statistical significance of impacts of SFR and school spending. A spline specification is used to place some structure on the relationship between court-ordered SFR exposure and adult outcomes to improve precision, but the structure imposed is flexible enough to allow several important specification tests to examine whether the detected impacts support a causal interpretation of school spending. The chosen spline specification, informed by the non-parametric event study models, is:

\[
Y_{icb} = \theta_0 \left(t_{icb} - T_c^* \right) \cdot D_{cb} \cdot 1 \left(t_{icb} - T_c^* < 0 \right) \cdot \text{SPEND}_c \\
+ \theta_1 \left(t_{icb} - T_c^* \right) \cdot D_{cb} \cdot 1 \left(0 \leq t_{icb} - T_c^* \leq 12 \right) \cdot \text{SPEND}_c \\
+ \theta_2 \left(t_{icb} - T_c^* \right) \cdot D_{cb} \cdot 1 \left(t_{icb} - T_c^* > 12 \right) \cdot \text{SPEND}_c \\
+ X_{icb} \beta + Z_{ib} \gamma + (W_{1960} \cdot b) \phi + \eta_c + \lambda_i + \phi_g \cdot b + \epsilon_{icb}
\]

The specification allows a partial test of the identifying assumption through its test of pre-existing time trends in outcomes prior to court orders and a break in this trend once SFR mandates go into effect. \( \theta_0 \) captures the pre-period linear trend in outcomes prior to SFR; \( \theta_1 \) represents estimated impact of each additional year of SFR exposure, ranging from 0 to 12 years of exposure; \( \theta_2 \) captures the post-SFR linear trend for years beyond school-age (i.e., this represents years of exposure during pre-school years or prior to birth, and thus, no difference in actual exposure during school-age years). Figure A6 illustrates a graphical representation that motivates the spline specification, and the patterns we may expect to see if consistent with a causal impact of per-pupil spending.

These models are estimated separately by child poverty status as well. There are several reasons why we hypothesize long-run impacts of reform-induced changes in spending may be

---

26 Except 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.
greater for poor children. First, poor children typically reside in poorer districts that underwent larger increases in per-pupil spending due to SFR. Secondly, prior research has often 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. Thirdly, endogenous residential mobility in response to school quality and/or parental decisions to send their kids to private schools is far more common among middle-class and affluent families. 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.

As aforementioned, 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.\textsuperscript{27} Because we did not want to include endogenous residential moves, this analysis does not incorporate information of family moves across school districts during the child’s school-age years. Instead, we identify the neighborhood and school of upbringing based on the earliest childhood address (in most cases, 1968).\textsuperscript{28} The resultant potential measurement error of school spending will tend to lead to attenuation bias of coefficients toward zero. Appendix Table A9 shows similar results when the sample is restricted to individuals who lived in their childhood residence prior to the initial SFR court orders. The analysis does capture school district characteristics that changed significantly from year to year. The latter part of Section VII provides more discussion of various falsification and specification tests performed.

\section*{VII. Estimated Effects on Longer-Run Outcomes}

\textit{Educational Attainment}. Figures 6A and 6B present the non-parametric and parametric event study model results for poor and non-poor children on the same graph for the effects of

\footnotesize{\textsuperscript{27} In California after its SFR, the share of students attending private schools rose about 50 percent (Downes and Schoeman, 1998), and educational foundations grew tremendously (Brunner and Sonstelie, 1996). Privatization grew disproportionately in districts that were constrained by the SFR formula to spend less than they traditionally had.

\textsuperscript{28} Among original sample children in the PSID, the average proportion of childhood spent growing up in the 1968 neighborhood was roughly two-thirds.}
reform-induced changes in per-pupil spending on the probability of graduating from high school and years of completed schooling, respectively. As detailed in Section VI, 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*race, roll-out of "War on Poverty" & related safety-net programs; and childhood family characteristics.

**College Quality.** Equally important impacts of school spending may extend beyond improvements in the quantity of years of completed education to the quality of education received (in both absolute and relative terms). Accordingly, I next examine school spending effects on college quality. There is a growing body of evidence that demonstrates significant labor market returns to college quality (Andrews et al., 2011; Hoekstra, 2009). I use information collected on college name reported by respondents between 1975 and 2009 and match it with the Integrated Post-secondary Education Data System (IPEDS) in order to link respondents with college quality indicators for the college attended. I use the 25th and 75th percentiles of the SAT math and verbal scores of the freshman class to which the individual attended college as markers of college quality.

Figure 7 presents the non-parametric and parametric event study model estimates for poor and non-poor children on the same graph for the effects of reform-induced changes in per-pupil spending on these measures of college quality.

**Labor Market Outcomes, Adult Family Income and Poverty Status.** The next series of results reveal large, significant effects of school spending on poor children’s subsequent adult economic status and labor market outcomes, using the same model specifications. Figures 8-10 present school spending effects by childhood poverty status on adult economic outcomes (ages 25-45), including wages (Figure 8), annual family income (Figure 9A), and the annual incidence of poverty (Figure 9B). In light of the parallel set of findings across all these long-run economic outcomes, the results are discussed in succession.

The results indicate that….

[Summarize 2SLS/IV models here]. Similar patterns of results emerge from both event-study and 2SLS/IV estimates of the impacts of school spending on adult socioeconomic attainment.
The evidence collectively is not consistent with alternative omitted-variables counter-explanations of the results (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) had the largest impacts on poor children in communities where SFR resulted in the largest changes in school spending; and finally d) had no effects on individuals from non-poor childhood families. 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 that differ in whether and how long and how much SFR influenced per-pupil spending during their K-12 school years.

VIII. SUMMARY DISCUSSION AND CONCLUSION

One of the distinguishing features of the US 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 in school-related cases on school finance reform during the past 3 decades, and documents their long-term impacts. We analyzed the efficacy of both court-mandated school finance reforms and legislative reforms to narrow school spending disparities between rich and poor districts and raise the overall level of per-pupil spending. 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.

[go back & expand this concluding section]
Childhood exposure to School Finance Reform

- already an adult when reform starts
- partially exposed
- born post reform start

Childhood exposure to School Finance Reform

Yr age 17 relative to yr of initial court order.
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 (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" & 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). Standard errors are clustered at the childhood county level. 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.
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 kids; 6,318 non-poor kids) 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" & 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). 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 kids not statistically significant from 0.
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 kids; 6,318 non-poor kids) from 1,409 school districts, 1,031 child counties, 50 states).

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Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Full analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011. (N=15,353 individuals (9,035 poor kids; 6,318 non-poor kids) from 1,409 school districts, 1,031 child counties, 50 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). 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 kids not statistically significant from 0.
### Table 1a. 2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Educational Attainment

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<thead>
<tr>
<th>Dependent variable:</th>
<th>Prob(HS Grad)</th>
<th>Years of Education</th>
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</thead>
<tbody>
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<td></td>
<td>Prob(HS Grad)</td>
<td>Years of Education</td>
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<td>(2)</td>
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<tr>
<td>School District Per-pupil Spending (000s) (age 12-17)</td>
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<td>0.1736*</td>
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<tr>
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<td>(0.0203)</td>
<td>(0.0940)</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>13,220</td>
<td>13,220</td>
</tr>
<tr>
<td>Number of School Districts</td>
<td>1,265</td>
<td>1,265</td>
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</table>
Table 1b. 2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Educational Attainment

<table>
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<th>Second Stage</th>
<th>Prob(HS Grad)</th>
<th>Years of Education</th>
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<td>Ln(School District Per-pupil Spending)_{age 12-17} / .25</td>
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<td>(2)</td>
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<tr>
<td></td>
<td><strong>0.1214</strong>*</td>
<td><strong>0.2243</strong>*</td>
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<tr>
<td></td>
<td>(0.0306)</td>
<td>(0.1417)</td>
</tr>
<tr>
<td>Number of Individuals</td>
<td>13,220</td>
<td>13,220</td>
</tr>
<tr>
<td>Number of School Districts</td>
<td>1,265</td>
<td>1,265</td>
</tr>
</tbody>
</table>
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=70,117 person-year adult observations of 11,260 individuals (6,507 poor kids; 4,753 non-poor kids) from 1,339 school districts, 980 childhood 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" & 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). 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 kids not statistically significant from 0.
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=93,022 person-year adult observations of 12,682 individuals (7,424 poor kids; 5,258 non-poor kids) from 1,362 school districts, 997 childhood 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" & 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). 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 kids not statistically significant from 0.
Data: PSID geocode Data (1968-2011), matched with childhood school and neighborhood characteristics. Full analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011. (N=93,022 person-year adult observations of 12,682 individuals (7,424 poor kids; 5,258 non-poor kids) from 1,362 school districts, 997 childhood counties, 50 states).

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Effect of Court-Ordered School Finance Reform on School Spending & Likelihood of High School Graduation

Effect of Court-Ordered School Finance Reform on School Spending & Educational Attainment

Effect of Court-Ordered School Finance Reform on School Spending & Earnings in Adulthood

Effect of Court-Ordered School Finance Reform on School Spending & Family Income in Adulthood
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=93,022 person-year adult observations of 12,682 individuals (7,424 poor kids; 5,258 non-poor kids) from 1,362 school districts, 997 childhood 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" & 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). 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 kids not statistically significant from 0.