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

C. Kirabo Jackson, *Northwestern University*
Rucker C Johnson, *University of California - Berkeley*
Claudia Persico, *Northwestern University*

THE EFFECT OF SCHOOL FINANCE REFORMS ON THE DISTRIBUTION OF SPENDING, ACADEMIC ACHIEVEMENT, AND ADULT OUTCOMES*

C. KIRABO JACKSON
NORTHWESTERN UNIV & NBER

RUCKER C. JOHNSON
UC-BERKELEY & NBER

CLAUDIA PERSICO
NORTHWESTERN UNIV

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ABSTRACT

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

In Part One, using a newly compiled database of school finance reforms and a recently available long panel of annual school district data on per-pupil spending that spans the period 1967–2010, we present an event-study analysis of the effects of different types of school finance reforms on per-pupil spending in low- and high-income school districts. We find that SFRs have been instrumental in equalizing school spending between low- and high-income districts and many reforms do so by increasing spending for poor districts. While all reforms reduce spending inequality, there are important differences by reform type: adequacy-based court-ordered reforms increase overall school spending, while equity-based court-ordered reforms reduce the spread of spending with little effect on overall levels; reforms that entail high tax prices (the amount of taxes a district must raise to increase spending by one dollar) reduce long-run spending for all districts, and those that entail low tax prices lead to increased spending growth, particularly for low-income districts.

In Part Two, we link the spending and reform data to detailed, nationally-representative data on children born between 1955 and 1985 and followed through 2011 (the Panel Study of Income Dynamics) to study the effect of the reform-induced changes in school spending on long-run adult outcomes. These birth cohorts straddle the period in which most of the major school finance reform litigation accelerated, and thus the cohorts were differentially exposed, depending on place and year of birth. We use the timing of the passage of court-mandated reforms as an exogenous shifter of school spending across cohorts within the same district. Event-study and instrumental variable models reveal that that while there are no effects for children from non-poor families, a 20 percent increase in per-pupil spending each year for all 12 years of public school for children from poor families leads to about 0.9 more years of schooling, 25 percent higher earnings, and a 20 percentage-point reduction in the annual incidence of adult poverty. The magnitudes of these effects are sufficiently large to eliminate between two-thirds and all of the gaps in these adult outcomes between those raised in poor families and those raised in non-poor families. We present several pieces of evidence to support a causal interpretation of the estimates.

* Please direct correspondence to Kirabo Jackson (kirabo-jackson@northwestern.edu) and Rucker Johnson (ruckerj@berkeley.edu). We wish to thank the PSID staff for access to the confidential restricted-use PSID geocode data. We are grateful for helpful comments received from seminar participants at UC-Berkeley and Harvard University.

I. INTRODUCTION

Ensuring equal educational opportunities for all children has long been an American ideal (Strickland, 1991; Browning and Long, 1974). However, the rules that determine school funding have not necessarily lived up to this ideal. In most states, prior to the 1970s, the vast majority of resources spent on K–12 schooling was raised at the local level, primarily through local property taxes (Howell and Miller, 1997; Hoxby, 1996). Because the local property tax base is generally higher in areas with higher home values, and households tend to segregate by income and wealth levels, heavy reliance on local financing contributed 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, and many states have implemented legislative reforms that led to important changes in public education funding.[2] These school finance reforms (SFRs) caused some of the largest changes in the structure of K–12 education spending in United States history.[3]

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

(1) <u>Do existing studies suffer from biases associated with low-quality data?</u> Previous national studies rely on data that were only available every five years starting in 1972.[4] The low frequency of the data both precluded detailed analyses of outcomes surrounding the timing of reforms and rendered authors unable to rule out the possibility that the effects were driven by pre-existing trend differences between reform and non-reform states.

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[1] Note that many low-income urban districts raise local funding from commercial property, so although low-income students typically receive lower levels of funding *on average*, this is not always the case (Hoxby, 1996).

[2] The first of these cases was the well-known California case, *Serrano v. Priest*, decided in 1971.

[3] Furthermore, nine states are currently reforming their school finance rules, and ten states are in legal battles regarding school financing. States that are reforming their school finance rules: Alaska (*Moore v. State of Alaska*), Indiana (*Hamilton Southeastern Schools v. Daniels*), Maine, Michigan (Gov. Rick Snyder's proposal), Minnesota (statewide task force), New Jersey (*Abbott v. Burke*), Ohio, Rhode Island (*Woonsocket School Committee v. Carcieri*), and Washington (*McCleary v. State of Washington).* States that are currently in legal battles: Alabama (*Lynch v. State of Alabama*), California (*Robles-Wong v. State of California*), Colorado (*Lobato v. State of Colorado*), Connecticut (*Connecticut Coalition for Justice in Education Funding v. Rell*), Florida (*Citizens for Strong Schools v. Haridopolos*), Kansas (*Shawnee Mission School District v. State of Kansas*; *Ganon v. State of Kansas*), New Hampshire, New York (*Hussein v. State of New York*), South Carolina (*Abbeville Co. Sch. Dist. v. State of South Carolina*), and Texas (*Texas Taxpayer & Student Fairness Coalition v. Scott*).

[4] Arizona, California,* Idaho, Kansas,* New York, New Jersey,* Washington, and Wisconsin* all had important court decisions that either overturned or upheld the state school finance system before the second possible data point in 1977. States with an asterisk (*) are states in which the status quo was deemed unconstitutional.

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

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

(4) How do changes in school spending caused by SFRs affect the long-run outcomes of affected children? The motivation behind SFRs was to reduce gaps in educational opportunity and subsequent socioeconomic well-being between children from poor and affluent families. However, the extent to which improvements in outcomes for low-income children was achieved is unclear. Hoxby (2001) finds mixed evidence on the effect of increased per-pupil spending associated with SFRs on high-school dropout rates. Card and Payne (2002) find that court-mandated SFRs that reduce inequality in spending are associated with reduced gaps in SAT scores between students from low- and high-income families.[5] In contrast, Downes and Figlio (1998) find that reforms in response to court mandates do not result in significant changes in the distribution of test scores.[6] In addition to the fact that the evidence on student achievement is mixed, there is mounting evidence that focusing on effects on test scores may miss important effects on longer-run outcomes (Heckman, Pinto, & Savelyev, forthcoming; Jackson, 2012). Accordingly, the effect that SFRs may have on long-run outcomes remains unknown.

This paper tackles these four questions through an analysis of the effects of SFRs on the level and distribution of school spending, as well as on subsequent educational and economic attainment outcomes in adulthood. The analysis proceeds in two parts.

In Part One, covered in Sections II and III, we tackle the first three questions and investigate the effects of SFRs on district spending, both in terms of absolute levels and in equalizing spending

[5] As acknowledged by the authors, the data used in this study may suffer from selection to SAT taking.

[6] However, Downes and Figlio (1998) find that plans that impose tax or expenditure limits on local governments reduce overall student performance on standardized tests.

between districts in a state. We address these questions using newly released panel data on per-pupil spending at the school district level going back to 1967, five years before the first reforms, and available annually from 1970 through 2010. We compile a comprehensive inventory of the timing of school finance litigation and legislative changes in state aid formulas that occurred between 1970 and 2010. We also codify reforms into several types, based on the ways the reform influenced the school funding formulas. With the higher-frequency, district-level data (previous studies used data points five or ten years apart), we conduct a detailed analysis of the timing of changes in outcomes in relation to the timing of reforms and assess the degree of pre-existing trends in spending leading up to the enactment of reforms.

Using the longest district-level panel on school spending that has ever been used to analyze these issues, we document the effects of SFRs on spending up to 20 years after reforms. Because many states implemented different aspects of reforms at different times, the high-frequency annual data allow us to distinguish the effects of different types of reforms on school spending. We analyze the effects of different kinds of court-mandated and legislative reforms on school-spending disparities between rich and poor districts and on 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 both for several years before and several years after reforms, and we document the effect of reforms on districts by their percentile of the state income distribution prior to the reforms.

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

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

We use the timing of passage of court-mandated reforms as an exogenous shifter of school spending. To accomplish this, we identify only those changes in school spending at the district level that resulted from court-mandated reform. For each district, we estimate the change in per-pupil spending that occurs after the passage of a court-mandated SFR, net of any underlying state-specific time effects and district trends. This, in essence, identifies those districts that experienced an increase or decrease in per-pupil spending in the years immediately following court-mandated SFR. We then link these district-specific policy-induced spending changes to longitudinal data of individuals born between 1955 and 1985 and followed through 2011 in the PSID. Because our sample includes sets of children from the same districts who were born in different years, some of these children were too old to be affected by reforms at the time of passage (not treated), some were old enough to be treated for some fraction of their school-age years (partially treated), and some were young enough to have entered school after the reforms were passed (fully treated). We combine the variation in exposure across cohorts within districts with the variation across districts in spending increases to implement a triple-difference strategy. The strategy 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 our event-study and instrumental variable models reveal that increases in per-pupil spending, induced by court-mandated school finance reforms, led to significant increases in the likelihood of graduating from high school and educational attainment for poor children, and thereby narrowed adult socioeconomic attainment differences between those raised in poor and affluent families. While we find no effect for children from non-poor families, for poor children, a twenty percent increase in per-pupil spending each year for all 12 years of public school is associated with 0.928 more years of schooling, 24.6 percent higher earnings, and a 19.74 percentage-point reduction in the annual incidence of poverty in adulthood. We present several key patterns that indicate that these improvements reflect the causal effect of these reforms and show that these results persist with controls for other coincident policies (e.g., "War on Poverty" initiatives and related safety-net programs or desegregation).

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

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

PART ONE: EFFECTS OF SFRS ON EDUCATION SPENDING

II. A Discussion of School Reforms and School Finance Data

The centerpiece of equity for children is having the same educational opportunity irrespective of place of residence, race/ethnicity, gender, etc. Toward this aim, starting in the early 1970s there were many court-ordered school finance reforms, legislative actions, and changes to how public schools were financed. To assemble a comprehensive list of reforms, we extract details on the exact timing and type of court-ordered and legislative SFRs from Public School Finance Programs of the United States and Canada[7] (PSFP) and the National Access Network's state-by-state school finance litigation map (2011).[8] We supplement these data with reform descriptions and school funding classifications from Murray, Evans, and Schwab (1998), Hoxby (2001), Card and Payne (2002), Hightower, Mitani, and Swanson (2010), and Baicker and Gordon (2006). In most cases, data from these sources are consistent with each other. Where there are discrepancies, we defer to PSFP and consult state court and legislative records for validation.[9] From these various sources, we compile a comprehensive dataset of each school finance reform between 1970 and 2010.

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

The movement toward school finance reform litigation and the ensuing debates about the constitutionality of local finance systems were based on the legal arguments presented in the successful school desegregation cases (Johnson, 2013). Early 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 U.S. Constitution, as school resources would then be a function of a local communities' wealth. These early challenges to existing local systems of school finance

[7] United States Office of Education [1969, 1972, 1974, 1979] and American Education Finance Association [1988, 1992, 1995].
[8] http://www.schoolfunding.info/states/state_by_state.php3
[9] There were discrepancies in reported timing of overturned court cases in several states: Connecticut (Hoxby states the decision was made in 1978, but Card and Payne report it was made in 1977), Kansas (Hoxby states 1976, but PSFP and ACCESS report 1972), New Jersey (Card and Payne state 1989, but PSFP says 1990), Washington (Murray, Evans, and Schwab, Hoxby, and Card and Payne report 1978, but PSFP reports 1977), Wyoming (Hoxby says 1983, but Card and Payne and Murray, Evans, and Schwab report 1980). We researched each case by name to discover the true date of the decision.

based on Federal Constitutional law were unsuccessful. However, this led to two subsequent waves of successful challenges based on state constitutional law.

These two waves of court-mandated reforms were distinct not only in time but also in motivation (Briffault, 2005). In the first wave, known as "equity cases," proponents of state funding argued that local financing violated the responsibility of the state to provide a quality education to all children. They asserted that public education was a "fundamental interest" for equal protection purposes and thus could not be distributed unequally within a state based on geography absent any "compelling state interest." The motivation was that "poor" school districts had little property wealth to tax in order to support their local schools, while "rich" school districts had much more at their disposal. As such, 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. Cases during the second wave of successful challenges were argued on adequacy grounds. "Adequacy cases" rely on the fact that virtually all states have a constitutional provision requiring the state to provide some level of free education for children (Lindseth, 2004). These cases were argued on the ground that prevailing low levels of educational resources in certain districts (typically low-income areas) violated the state's duty to provide the necessary educational opportunities guaranteed by the state constitution.

Figure 2 presents the number of states that had a court-mandated SFR on equity and/or adequacy grounds for each year between 1967 and 2010. As discussed above, most early court-mandated reforms (1970–1988) were litigated on equity grounds, and most of the later cases (1990–2010) were fought on adequacy grounds. Whether these different kinds of cases have different effects on spending is an empirical question that we address in Section III.

a. Classifying Reforms

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

[1] *PPE = (Local Tax Rate) × (Local Tax Base ÷ number of Students) + (Federal Funding ÷ number of Students) + (State Funding ÷ number of Students)*

Inspection of [1] makes clear that, *all else equal*, districts with higher property tax bases (wealthier districts) will tend to spend more per pupil than districts with low property tax bases (poor districts). It is also apparent that, *all else equal*, districts with higher property tax rates (those that have a high demand for education) will tend to spend more than those with lower property tax rates (those with a lower demand for education). Given residential segregation by income and socioeconomic status, for both these reasons there is a tendency for wealthier districts to spend more per pupil on education than poor districts (Hoxby, 2001).

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

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

Hoxby (2001) argues that these labels may not fully capture the economic incentives associated with the formulas. For example, some plans that would be in the same category in Card and Payne (2002) induce *more* spending by providing more state funding for districts that raise more local funds, while others induce *less* spending on the margin by providing more state funds to districts that raise fewer local funds. Accordingly, Hoxby (2001) advocates classifying reforms based on inverted tax prices. The inverted tax price is the amount of additional funding the district has to spend if it raises tax revenue by one dollar.

An inverted tax price of zero means that a district cannot raise education spending no matter how much it increases its tax revenue (a clear disincentive to raise local funds). This occurs in states that impose spending limits on districts (Downes and Figlio, 1998). An inverted tax price greater than one means that a district can raise education spending by more than one dollar by raising tax revenue by one dollar (a clear inducement to raise local funds). To capture this important feature, Hightower, Mitani, and Swanson (2010) disaggregate variable grant plans into two groups to make a distinction between those plans that focus on school districts' inverted tax prices (*local effort equalization plans*) and those that do not (*equalization plans*). Augenblick, Meyers, and Anderson (1997) aptly refer to these *local effort equalization plans* as "reward for effort" policies. We also use this intuitive label.

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

- *Foundation Plans*: These plans ensure a basic floor to spending. These include foundation plans, foundation grants, and guaranteed minimum tax base plans. These plans establish a foundation level of per-pupil spending, estimate a district's required local contribution to fund this foundation level based on income and wealth levels in the district, and provide the difference between the expected contribution and the foundation level.
    - These plans do not affect tax prices. They provide extra funding to low-income/low-wealth districts while leaving high-income/wealthy districts largely unchanged.
- *Flat Grants*: These plans give aid on a per-pupil basis to all districts.
    - Flat grants do not affect tax prices. They provide similar state funds for all districts and should have little effect on spending inequality, *all else equal*.
- *Equalization Plans*: These plans provide aid to districts based on property values and income levels. They include power equalization plans (which give more money to low-wealth districts), categorical aid schemes (which give money to low-income districts), and other equalization plans that distribute state funds to districts based on wealth or income levels.
    - Because funds are distributed based on wealth and income levels, these plans do not affect tax prices directly (although they may provide incentives to alter the tax base). These plans tend to provide extra funding to low-income/low-wealth districts while possibly taking money away from high-income/wealthy districts.
- *Reward for Effort Plans (inverted tax prices greater than one)*: These schemes seek to promote local efforts to raise school spending by increasing state aid to low-wealth districts that have

high tax rates. The key feature of these plans is that districts receive more state aid when they raise more local taxes.[10]

  - Reward for effort plans promote local efforts to raise education spending by targeting the inverted tax price directly. Such plans typically provide greater incentives for lower-income/low-wealth districts to increase taxes by allowing some districts to have more than one dollar in spending for each dollar raised in taxes. Such policies should increase spending overall, with larger spending increases for low-income districts.

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

b. Changes in School Finance Formulae Over Time

Since 1970, virtually every state has enacted at least one aid formula from among the categories listed above. To provide an overview of the evolution of school finance formulas, Figure 3 plots the number of states that have employed each kind of funding formula in each year. The first notable pattern is that the use of foundation plans was quite high in 1970 and increased slightly during the entire period (from 27 states in 1970 to 36 states in 2010). As more states implemented SFRs, the use of flat grants declined (from 26 states in 1970 to 5 states in 2020), while the use of equalizing plans increased (from 9 states in 1970 to 30 states in 2010). While the reward for effort approach was unpopular in 1970, the number of states employing reward for effort has increased over time (from 0 states in 1970 to 21 states in 2010), as has the number of states imposing

[10] For example, in Georgia, school districts at or below 75 percent of the state average property tax wealth level receive equalization funding in proportion to the number of mills they raise above the required five mill.

spending limits (from 0 states in 1970 to 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. Changes in School Spending Over Time

Data on district and state funding come from the Census of Governments, the Historical Database on Individual Government Finances (INDFIN),[11] and the Common Core of Data (CCD) School District Finance Survey (F-33). The Census of Governments has been conducted every five years since 1967 and records administrative data on school spending for every school district in the United States. This is the data source used in most existing national studies of school finance reforms. We augment this data with annual data from other sources. The INDFIN contains school district finance data annually for a sub-sample of large school districts from 1967 through 1991.[12] After 1992, the CCD School District Finance Survey (F-33) consists of data submitted annually to the National Center for Education Statistics (NCES) and includes data on school spending for every school district in the United States.[13] We combine these data sources to construct a long panel of annual per-pupil spending for school districts in the United States between 1967 and 2010.

This paper focuses on how SFRs affected school spending levels in different local communities, rather than aggregate state-level 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). Note that because quartiles are defined within a state, this plots within-state changes in inequality.

There are a few notable patterns. First, per-pupil spending has been increasing over time in all districts. In 2012 dollars, the average district spent about $4,612 per student in 1967 and

[11] The Historical Database on Individual Government Finances (INDFIN) represents the Census Bureau's first effort to provide a time series of historically consistent data on the finances of individual governments. This database combines data from the Census of Governments Survey of Government Finances (F-33), the National Archives, and the Individual Government Finances Survey.
[12] Per-pupil spending data from before 1992 is missing for Alaska, Hawaii, Maryland, North Carolina, Virginia, and Washington, D.C. Per-pupil spending data from 1968 and 1969 is missing for all states. Spending data for certain years is also missing for the following states: Florida (1975, 1983, 1985–1987, and 1991); Kansas (1977 and 1986); Mississippi (1985 and 1988); Montana (1976); Nebraska (1977); Texas (1991); and Wyoming (1979 and 1984). Where data for only a year or two was missing, it was filled in using linear interpolation.
[13] Both NCES and the Governments Division of the U.S. Census Bureau collect public school system finance data, and they collaborate in their efforts to gather these data.

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

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

III. Event-Study Analysis of Effects on School Spending

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

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

$$[2]\ \$_{dst} = \alpha + \left(Q_d \cdot \sum I_y^{court}\right) \cdot \pi_{q,y}^{court} + \left(Q_d \cdot \sum I_y^{legislate}\right) \cdot \pi_{q,y}^{legislate} + \theta_d + \theta_t + \varepsilon_{dt}$$

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

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

To quantify the effect of these reforms on per-pupil spending, we form linear combinations of the estimated treatment effects for different years. For example, the effect of court-ordered reforms on the spending for the bottom 10 percent of income districts can be estimated by the average of the 5 years after reforms minus the average of the 5 years prior to reforms. This estimate is obtained by computing the following linear combination of coefficient estimates: $(\pi_{q,-5}^{court} + \pi_{q,-4}^{court} + \pi_{q,-3}^{court} + \pi_{q,-2}^{court} + \pi_{q,-1}^{court})/5 - (\pi_{q,5}^{court} + \pi_{q,4}^{court} + \pi_{q,3}^{court} + \pi_{q,2}^{court} + \pi_{q,1}^{court})/5$. Whether this computed difference is statistically significant is determined by testing the statistical significance of the linear combination of the estimated coefficients. We present the results of such tests to accompany the event-study graphs. Note that the standard errors for all the estimates are clustered at the state level.

a. Event-Study Analysis for Court-Mandated Reforms

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

In Figure 5, all the series are centered on 0 during the pre-reform years. During the 10 years prior to reforms (years -10 through -1), districts in reform states saw similar changes in per-pupil spending as districts in non-reform states of the same income level. Within the first five post-reform years however, districts in the bottom quartile (solid black lines) saw increases in per-pupil spending above and beyond comparison districts in non-reform states. Districts between the 25th and 75th percentiles experienced modest increases after reforms (as evidenced by most post-reform data points for these districts being above the pre-reform mean). In contrast, districts in the top 25 percent of incomes in 1962 saw little change in spending within the first 14 years after reforms, and there is evidence of a slight decrease 15 years after reforms for the very highest income districts. Note that the sharp decline in spending at year 8 is due to a compositional change. The lower panel of Figure 5 plots the same dynamic treatment effects, but only using districts that were observed for more than 10 post-reform years. Using this more balanced panel, there is no sudden drop in year 9, but the basic patterns are similar (increased school spending for the lowest-income districts and decreases for the highest-spending districts). Because the more balanced panel includes only older cases, it includes relatively few adequacy cases. We will show that the difference between the top and bottom panels of Figure 5 is due to the fact that the first seven years presented in the top panel include many recent adequacy cases, and these generate somewhat different patterns from the older equity cases.

To better quantify the patterns in Figure 5, we estimate the effect of court-mandated reform as the difference between the average effect in the 10 years prior to reforms and the 10 years after reforms. Based on the linear combination of coefficient estimates, these reforms increased per-pupil spending for the bottom 10 percent income districts over the first 10 years by $582.81 in 2010 dollars (p-value=0.07). Between 1980 and 1990, the average per-pupil spending for these low-income districts was $6,590.66, representing a relative spending increase of about nine percent. To get a better sense of the longer run effects of spending for these districts, we compute the average effect for years 5 through 10 relative to the 10 years prior to reforms. This calculation indicates that after five years these reforms increased per-pupil spending for the bottom 10 percent income districts by $651.12 (p-value=0.02), an increase in spending for low-income districts of about 11 percent. Similar calculations for the top 10 percent income districts show little effect. The estimated effects suggest that these reforms reduced spending during the first 10 years by $110.41 (p-value=0.27) and in years 5 through 10 by $191.12 (p-value=0.56).

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, after 10 years court-mandated reforms reduced the spending gap between the top-income districts and the bottom-income districts by $842.01 (p-value<0.01). The spending gap between these two groups of districts between 1980 and 1990 was $1,197.33, so that court-mandated reforms reduced this spending gap by about 70 percent on average. The magnitude of these effects, coupled with the rapid increase in the number of court-mandated reforms during the early 1990s, can account for a sizable portion of the spending "catch-up" documented in Figure 4 between the lowest- and highest-income districts.

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

The top panel of Figure 6 presents the dynamic effects of equity-based court-mandated reforms on the level of per-pupil spending. The effects are relative to non-reform states. There is a dip in spending (of about $500) in all districts two years prior to reforms for those states that had their first court-mandated case based on equity grounds relative to similarly affluent districts in non-reform states. While this pre-reform dip makes the effect of such cases on the overall *level* of spending unclear (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 $807.54 (p-value=0.01) after five years post-reform. The aim of these cases was to increase spending equity. Reforms induced by these equity based cases achieved this objective.

The bottom panel of Figure 6 presents the event-study graph for adequacy cases (primarily the second wave of cases). The objective of these cases was not to explicitly reduce inequality in education spending, but rather to ensure that spending permitted all children (especially those in low-income districts) to receive adequate resources for a quality education. Because these cases are more recent, we present the dynamic treatment for the first seven years of the reform. As one can see, spending in all districts in states with adequacy cases was fairly stable (relative to non-reform districts) prior to reforms. The trajectory of spending was quite flat four years prior to reforms. After reforms, there is evidence of an increase in school spending that is most pronounced for the poorest 10 percent of districts. While all districts experience an increase in spending of about $430 within the first five years of reforms, the poorest 10 percent of districts break from the other districts with an increase in spending of over $1,000 within the first five years. Because all districts experienced spending increases, adequacy cases are associated with a smaller reduction in spending gaps than equity cases. Five years after an adequacy case, the gap in spending between the highest- and lowest-income districts is narrowed by $377.25 (p-value=0.02). In sum, consistent with the aims articulated by the courts, equity cases led to greater equity in school spending, while adequacy cases led to increased school spending overall, with particularly large increases for low-income districts.

b. Event-Study Analysis for Legislative Reforms

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

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

The point estimates tell a similar story. In the 10 years after reforms, the lowest-income districts saw a $413.70 reduction in spending (p-value=0.27). Between years 5 and 10, the reduction for these districts was $547.45 (p-value=0.12). These point estimates suggest a persistent slowdown in spending growth even for the lowest-income districts. Looking at districts in the top 10 percent of income, the patterns are similar. These reforms are associated with a $743.66 reduction in the first 10 years post-reform (p-value=0.04) and a $936.64 reduction (p-value= 0.03) between years 5 and 10. Because the reductions in spending are somewhat larger for the high-income districts than the low-income districts, these reforms likely did reduce spending gaps between the top- and bottom-income districts. Our estimates suggest this was the case; relative to the spending gap in the 10 years prior to reforms, the spending gap between the top 10 and bottom 10 percent income districts was reduced by $329.75 (p-value=0.05) in the 10 years after reforms. This represents a 27 percent reduction in the spending gap between high- and low-income districts. We conclude from this that legislative changes did have modest effects on spending inequality within states, but also tended to decrease spending overall (Hoxby, 2002).

c. Effects by Type of Reform Used

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

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

One would expect the spending limit to bind first for the highest-spending districts. Then, as overall spending increases it would bind for lower-spending districts. This is the pattern observed in Figure 8. For the poorest 10 percent of districts, the spending limit reduces spending by $15.39 (p-value=0.946) in the 10 years after reforms. However, between years 10 and 20 post reforms, these low-income districts experience a $910.63 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 $535.91 (p-value<0.01) in the 10 years after reforms. The reduction increases to $1,494.96 (p-value<0.01) between years 10 and 20. Not surprisingly, spending limits are effective at reducing spending inequality: the spending gap between the high- and low-income districts narrows by about $520.15 (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.

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

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

The lower panel of Figure 9 shows the event study 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 five 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 a statistically significant difference between the growth rates for post-reform years 5 through 10 and the pre-reform years (i.e., both yield p-values above 0.1). However, there is evidence suggestive of increased spending growth for the lowest-income districts in the long run such that during post-reform years 10 through 20, average annual spending changes were $175.88 more (p-value=0.08) than during the pre-reform years. This is consistent with the analysis in levels that reveals that reward-for-effort plans reduce the spending gap between low- and high-income districts in the long run by $295.83 (p-value=0.11). Overall, the patterns show an increase in spending and spending growth in the short run (lasting about five 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 12 percent.

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

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

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

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

IV. Description of the Longer-Run Outcome Data

The primary micro dataset utilized to analyze the effects of reform-induced changes in school spending on long-run outcomes is the restricted, confidential geocoded version of the PSID (1968–2011) with identifiers at the level of the neighborhood blocks in which children grew up.[14] We link our district-level data on school spending and the timing of reforms to the nationally-representative sample of children born between 1955 and 1985 from the PSID. Following Johnson (2012), we then merge neighborhood and school characteristics, as well as information on other key policy changes (e.g., the timing of school desegregation, hospital desegregation, rollout of "War on Poverty" initiatives, and expansion of safety net programs) from multiple data sources on the conditions that prevailed when these children were growing up, allowing for a rich set of control variables.[15]

The sample consists of PSID sample members born between 1955 and 1985 who have been followed into adulthood; these individuals were between the ages of 26 and 56 in 2011. We include all information on them for each wave, 1968 to 2011.[16] We include both the Survey Research Center (SRC) component and the Survey of Economic Opportunity (SEO) component, commonly known as the "poverty sample," of the PSID sample. Due to the oversampling of African-American and low-income families, 59 percent of the sample members were poor as children (N=15,353 individuals; 9,035 poor children; 6,318 non-poor children). Sixty-six percent of the PSID individuals born between 1955 and 1985 and followed into adulthood grew up in a school

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

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

[16] The PSID maintains high wave-to-wave response rates of 95–98 percent. Studies have concluded that the PSID sample of heads of households and spouses remains representative of the national sample of adults (Gottschalk et al., 1999; Becketti et al., 1997).

district that was subject to a court-mandated school finance reform sometime between 1972 and 2000, with the timing of the court order not necessarily occurring during their school-age years. Eighty-eight percent of the PSID individuals born between 1955 and 1985 who were poor as children and followed into adulthood grew up in a school district that was subject to a court-mandated school finance reform sometime between 1972 and 2000. Given the patterns in Figure 1, the share of individuals exposed to school finance reforms during childhood increases significantly with birth year over the 1955–1985 birth cohorts analyzed in the PSID sample.

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).[17] Each record is merged with data on school spending for 1960–2000 and the aforementioned school finance variables at the school district level that correspond with the prevailing levels during their school-age years. We also merge information on student-teacher ratios and school segregation indices to the PSID data using the census block/tract contained in the geocode file based on the earliest available address in childhood (or county of birth when census block information is unavailable).

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

[17] Many school districts were counties during this period, including more than one-half of Southern school districts.

V. Empirical Strategy for Estimating Effects on Adult Outcomes

In this section, we aim to determine whether the increased school spending experienced by children in low-income neighborhoods due to SFRs had any lasting effects on their adult socioeconomic well-being. Our empirical approach uses two distinct sources of variation in per pupil spending induced by reforms: First we exploit the staggered timing of court-mandated school finance reforms across districts to implement a cohort level "event-study" analysis (variation in the timing of reforms across cohorts); second, we exploit the fact that the same reform led to different changes in spending across districts (variation in treatment intensity for exposed cohorts). We detail how all this variation is used within a single framework in Section V.b. Because residential mobility across counties and private school attendance are more common among children from affluent parents than those from low-income parents, one might expect larger effects among children from low-income parents.[18] Also, prior research has shown that children from low-income families may be more sensitive to changes in school quality and school-related interventions (e.g., the Tennessee Star class size experiment) than children from more advantaged family backgrounds. As such, we conduct all analyses separately by childhood poverty status. A child is defined as poor if their family income falls below two times the poverty line for any year during their school-age years. As such, this measure captures both the near poor and the persistently poor.

As in the analysis of school spending, we employ a flexible event-study design to map out how adult outcomes evolve over time (i.e. across cohorts) before and after the passage of reforms. This event-study also allows us to examine not only how outcomes vary with years of exposure to the reforms, but also allows us to examine pre-reform trends in outcomes to test for potential endogeneity of the timing of reforms. Also, as in the analysis of spending, we can compare the evolution of outcomes for districts that experienced larger versus smaller increases in per-pupil spending due to reforms. While Part I shows that many reforms change the distribution of school spending, we focus the analysis in Part II on school spending changes associated with the passage of court-ordered reforms. This choice was driven by the fact that court-mandated reforms exhibited

[18] 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.

minimal trending in spending prior to those reforms (suggesting that there might be minimal pre-reform trending in adult outcomes across cohorts), and court-mandated reforms generated large, robust, and statistically significant increases in per-pupil spending for low-income neighborhoods (within which many of the PSID respondents resided). In short, court-mandated reforms have desirable statistical properties and are likely to be reliable exogenous shifters in school spending.

While understanding the effect of school finance reforms on adult outcomes is important, exploiting plausibly exogenous variation in per pupil spending due to reforms allows for an investigation into the broader question of whether increasing school spending can improve the longer run outcomes of affected students. Simply comparing outcomes of students exposed to more or less school spending, even within the same district, could lead to biased estimates of the effect of school spending on student outcomes if there were other factors that affect both student outcomes and school spending simultaneously. For example, a decline in the local economy could depress per-pupil spending (through home prices or tax rates) and also have deleterious effects on student outcomes. This would result in a spurious positive correlation between per-pupil spending and child outcomes. Conversely, an inflow of low-income students might lead to an inflow of compensatory federal funding while simultaneously generating reduced student outcomes. This would lead to a spurious negative relationship between spending and student outcomes. By focusing only on plausibly exogenous changes in school spending within districts associated with reforms, our approach removes potential biases that might exist when simply comparing students who have been exposed to different levels of school spending for reasons unknown to the researcher. In Section VI, we show that isolating exogenous variation in school spending leads one to very different conclusions than simple comparisons that do not account for the possibility that changes in school spending might be endogenous to student outcomes.

a. Hypothesized Effects Across Cohorts

There are two natural tests of whether spending changes associated with school finance reforms have a causal effect on adult outcomes. The first test is whether exposed cohorts in those districts that experienced increases in per pupil school spending also had improved outcomes relative to unexposed cohorts in the same district. The second test is whether the improvements observed for exposed cohorts (relative to unexposed cohorts) are larger for districts that experience larger increases in per pupil school spending. Because not all cohorts within a district are equally treated (some are exposed to spending increases for more of their school years than others), and not all districts experience the same changes in spending after reforms (some districts experience larger spending increases than others), both of these tests can be implemented within a single event-study framework. We lay out the cross-cohort and cross-district patterns in outcomes one should observe in an event-study analysis if there is a causal effect of increased spending due to reforms on adult outcomes.

If there is a causal effect of increased school spending on adult outcomes, and there are no pre-existing cohort trend differences across districts that experience increases in spending, then an event-time figure across cohorts for a given increase in school spending should follow patterns similar to the stylized patterns presented in Figure 11. On the x-axis is the years of expose to the reform for a given cohort, and on the y-axis is the cohort-level mean of some outcome for which higher values are better.

For those cohorts who were too old to be exposed to any reform-induced spending increases (to the left of 0 such that they were 18 or older at the time of the passage of reforms), there should be no systematic increase or decrease in the outcome across cohorts because none of these cohorts was exposed. As such, an event-study graph of outcomes by cohort should be relatively flat across cohorts that were too old to be affected by the reforms. Also, because pre-reform cohorts are not exposed to any spending changes, outcomes should be similar across the pre-reform cohorts both in districts that experienced large increases in school spending due to reforms and those that experienced small increases in school spending due to reforms.

For those cohorts who were of school-going age when reforms were implemented (i.e. those who were between the ages of five and 17, indicated by relative years 0 to 12 on the x-axis), outcomes should both be better than those for the unexposed cohorts and increasing in the number of years of exposure. That is, cohorts that are exposed to increased spending for a longer period of time should have better outcomes than cohorts exposed to the same spending increase but for a shorter period of time (variation in timing). Also, for a given duration of exposure, individuals in districts with larger increases in spending should have larger improvement in outcomes than those in districts that experienced smaller increases in spending (variation in intensity). As such, the relationship between years of exposure and good outcomes should be positive and it should be more positive for districts that experience larger increases in spending. This is depicted in the two upward sloping segments for the partially exposed cohorts, where the dashed line is steeper for larger increases in spending.

Finally, among more recent cohorts (i.e., those who were younger than 5 or unborn at the passage of reforms) all 12 of the school-age years were post-reform, As such, these cohorts should have better outcomes than the partially exposed cohorts, and there should be no systematic increase or decrease in the outcome among these fully treated cohorts. As with the partially exposed cohorts, for a given duration of exposure individuals in districts with larger increases in spending should have larger improvements in outcomes than those in districts that experienced smaller increases in spending. This leads to better outcomes (relative to untreated cohorts) for the fully treated cohorts in high-increase districts than low-increase districts.

In sum, if (a) there is a causal effect of spending on outcomes and (b) the district-level spending increases due to reforms are exogenous to changes in the outcomes, then the plot of the event-time indicator variables for districts that experience small and large spending increases due to reforms should follow the stylized patterns in Figure 11. That is, outcomes should be improving in years of exposure to reforms (variation in time) and the relative improvements should be larger in districts that experienced larger increases in school spending (variation in intensity).

b. Analyzing the Effect of School Spending on Adult Outcomes

To show evidence of causal relationships, we test for the specific patterns hypothesized in Figure 11 semi-parametrically across a variety of adult outcomes. While looking for differences across cohorts can be achieved with a flexible event-study analysis, testing for differences across districts that saw larger or smaller increases in spending requires a good measure of the court-mandated reform-induced increase in school spending. The event-study analysis documented that districts in the bottom quartile of the state's income distribution in 1962 experienced larger increases in school spending than those in high-income quartiles. As such, the quartile of the district in the income distribution could serve as a proxy for the extent to which reforms increased funding in the district. However, this is a relatively weak proxy for increases in spending at the individual district level because (a) not all court-mandated reforms had the same effect on all districts, and (b) not all reforms had the same distributional effects on districts within a state. As such, to test for whether those districts that experienced larger increases in school spending were those that experienced larger improvement in adult outcomes requires having a good measure of the increases in school spending associated with the implementation of a court-mandated reform at the individual district level.

To obtain a measure of district-specific changes in spending caused by court-mandated reforms, we regress the natural log of district per-pupil spending on district fixed effects, year fixed effects, and an indicator variable denoting a post-reform year interacted with each school district. To isolate spending changes associated with the court-ordered reforms, we also include controls for a variety of potentially confounding policies. The period under study overlaps other important policy changes (Johnson, 2013; Chay, Guryan, & Mazumder, 2009; Hoynes, Schanzenbach, & Almond, 2012). To account for the effect of these policy changes, we include county by year measures of school desegregation, hospital desegregation, community health centers, and state funding for kindergarten, in addition to per capita expenditures on Head Start, per capita expenditures at age four, Title I school funding, and average childhood spending on food stamps, Aid to Families with Dependent Children (AFDC), Medicaid, and unemployment insurance.[19] The coefficients on the district indicators interacted with the post-reform indicator provide the regression estimate of the change in per-pupil spending associated with the passage of a court-mandated reform for each district (net of the effect of a myriad of other policies). For each district, we take the interaction of the post-reform indicator with that district as our time-invariant, district-specific, court-mandated reform effect on spending ("$SPEND_c$"). Using this measure of treatment intensity (the increase in per-pupil spending that the district will experience after the passage of reforms) allows for a direct test of our key hypothesis—that exposed cohorts in those districts that experienced the largest increases in school spending also experienced the greatest improvement in outcomes relative to unexposed cohorts in the same district.[20]

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

$$\begin{aligned} Y_{icb} = & \sum_{t-T=-20}^{-2} \alpha_{t-T} \cdot 1\left(t_{icb} - T_c^* = t-T\right) \cdot SPEND_c + \sum_{t-T=0}^{12} \theta_{t-T} \cdot 1\left(t_{icb} - T_c^* = t-T\right) \cdot SPEND_c \\ & + \sum_{t-T=13}^{20} \delta_{t-T} \cdot 1\left(t_{icb} - T_c^* = t-T\right) \cdot SPEND_c + X_{icb}\beta + Z_{cb}\gamma + (W_{1960c} * b)\varphi + \eta_c + \lambda_b^r + \phi_g^r * b + \varepsilon_{icb} \end{aligned} \quad [3]$$

[19] The data sources used to compile these measures are detailed in Johnson (2013).

[20] Consistent with the flexible district-specific effects picking up much of the variability associated with the income quartiles, it is much more positive for lower-income districts. However, one can only explain four percent of the variability across districts in $SPEND_c$ with the income category variables. Using all the observable variables to describe reforms from Part One interacted with the quartile of income can predictably explain about one-third of the variability in $SPEND_c$.

[21] This part of the research design is similar in setup to a recent study by Johnson (2011) on the long-run impacts of court-ordered school desegregation.

where i indexes the individual, c the school district, b the year of birth, g the region of birth (defined by nine census division categories), and r the racial group. The variable η_c is a school district fixed effect, and $SPEND_c$ is the SFR-induced change in per-pupil spending in district c. The flexible timing indicators, $1\left(t_{icb} - T_c^* = t - T\right)$, equal 1 if the year the individual from school district c turned age 17 (t_{icb}) minus the year of the initial SFR court order in school district c (T_c^*) equals a value between -20 and 20. For example, values for $\left(t_{icb} - T_c^*\right)$ 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. The event-study year (t - T) is 0 when the year in which an individual was age 17 (typically, a high-school senior) equals the initial year of court-mandated SFR for the school district in which the person grew up.

Estimation of equation [3] provides a flexible description of the subsequent adult attainment outcomes in relation to the cohort- and district-specific timing of reform-induced changes in school spending. This allows us to test for the patterns described in Figure 11. The estimates of the post-reform year indicator variables interacted with the reform-induced increase in spending, θ_{t-T} in equation [3], map out difference in outcomes across cohorts that experienced a 100 percent increase in per-pupil spending after the passage of reforms.

These estimates provide precise pictures of the exact timing of any changes in attainment outcomes in relation to the number of school-age years of exposure to SFR and its resultant changes in spending. Because the validity of our empirical design depends critically on the assumption that those districts that saw increases in school spending due to reforms were not already on a differential trajectory of improving outcome, we also present the flexible time indicators interacted with the increase in spending for years prior to reforms. A plot of the estimates of the pre-reform indicator time dummies interacted with the reform-induced increase in spending, α_{t-T}, provides a visual portrait of whether there were systematic time trends in outcomes preceding enactment of court-ordered SFR in districts that would have experienced increases or decreases in school spending after reforms. The former uses the specific timing and intensity of changes to test for causal effects of school spending; the latter provides a test of endogeneity in the timing and scope of the initial court orders. Note that in addition to testing for trending in the pre-reform cohorts, estimated effects beyond the maximum 12 school-age years of exposure (δ_{t-T} , for event-study years (t-T)>12) provide an additional specification test, as these should not exhibit significant trends in outcomes because these additional years do not represent any change in school-age exposure.[22]

This model can be viewed as a triple-difference strategy that compares the difference in outcomes between cohorts within the same district exposed to reforms for different amounts of time (variation in exposure) across districts with larger or smaller changes in school spending due to reforms (variation in intensity). Because the intensity variable $SPEND_c$ is invariant within a district and all models include district fixed effects, the validity of the research design relies upon the exogeneity of the timing of passage of court-mandated SFRs, which is addressed and supported by the model specification in several ways. First, the model includes school district fixed effects (η_c), race-specific birth year fixed effects (λ_b^r), and race-by-region of birth cohort trends ($\varphi_g^r * b$), and it controls for an extensive set of child and childhood family characteristics (X_{icb}: parental education and occupational status, parental income, mother's marital status at birth, birth weight, child health insurance coverage, and gender). To account for effect of the other policies discussed above when predicting effects on outcomes, we include county by year of birth level measures of school desegregation, hospital desegregation, community health centers, and state funding for kindergarten, in addition to per capita expenditures on Head Start, per capita expenditures at age four, Title I school funding, and average childhood spending on food stamps, Aid to Families with Dependent Children (AFDC), Medicaid, and unemployment insurance, (Z_{cb}).[23] Few studies simultaneously account for so comprehensive a set of policies. Models that analyze the economic outcomes use all available person-year observations for ages 25-45 and control for a cubic in age to avoid confounding life cycle and birth cohort effects. To control for trends in factors hypothesized to influence the timing of SFR, equation [3] also includes interactions between 1960 characteristics of the county of birth and linear trends in the year of birth ($W_{1960c} * b$: 1960 county poverty rate, percent black, average education level, percent urban, and population size). Standard errors are all clustered at the school district level.

[22] Only in the case in which SFR plans became more effective with time would we expect a significant relationship between outcomes and event-study years beyond 12, which we explore.
[23] The data sources used to compile these measures are detailed in Johnson (2013).

One potential parental response to the presence of school quality differences across public schools is to move to a different city or enroll children in a private school.[24] Because we did not want to include endogenous residential moves, we identified the neighborhood and school of upbringing based only on the earliest childhood address (in most cases, 1968).[25] As such, one can interpret our estimates as intention to treat. Results are similar when the sample is restricted to individuals who lived in their childhood residence prior to the initial court orders. The latter part of Section VI provides more discussion of falsification and specification tests performed.

We present graphical plots, based on equation [3], that show the response of outcomes to reform-induced effects on per-pupil spending. This allows us to test for any increase with years of exposure and the resultant amount of spending change. To present both the time variation and the intensity variation on the same graph, we present the estimated event-study effects for a 10 percent increase, a 20 percent increase and a 25 percent increase. If there is a real causal effect of increased spending on adult outcomes, the event study plot should follow the patterns in Figure 11. The aim of the event-study analysis is to clearly illustrate the patterns in the data. To provide point estimates and statistical inference tests we turn to a regression analysis.

c. Regression Estimates of the Effect of School Spending on Adult Outcomes

In addition to presenting the visual evidence on the causal effects of increased school spending on outcomes using the event-study analysis, we also present regression estimates based on the same sources of exogenous variation that are used to quantify these relationships. The basic empirical approach to identifying the effect of school spending on longer-run outcomes is to compare outcomes of individuals who were exposed to different levels of school spending when they were young. Our measure of exposure to school spending is $PPE_{5\text{-}17}$, the average per-pupil spending in an individual's birth district during the years when that individual was ages five through 17 (school age years). A doubling of this average can be interpreted as a doubling of per pupil spending for all 12 years of an individual's school career. Because such large increases are very rare, to allow for a marginal increase in this variable to have a more realistic interpretation, we take the natural log of this average and multiply it by five (i.e. we use $\ln(PPE_{5\text{-}17})*5$), so that the coefficient on $\ln(PPE_{5\text{-}17})*5$ in a regression is the effect of a 20 percent increase in per pupil

[24] After SFRs in California, the share of students attending private schools rose about 50 percent (Downes & Schoeman, 1998), and educational foundations grew tremendously (Brunner & Sonstelie, 2003). Privatization grew disproportionately in districts constrained by the SFR formula to spend less than they traditionally had.
[25] Among original sample children in the PSID, the average proportion of childhood spent growing up in the 1968 neighborhood was roughly two-thirds.

spending for all 12 years of one's school age years. The standard deviation of the district-specific spending increases is 0.15, so that a 20 percent increase is somewhat larger than the typical increase. However, this treatment is well within the range of the data such that one-quarter of districts in reforms states experience reform-induced spending increases this large or larger.

As discussed previously, using all changes in school spending might introduce endogeneity bias, so we isolate plausibly exogenous variation in this measure due to the exogenous variation in school spending due to reforms. Changes in this measure across cohorts from the same district are sensitive to both the years of exposure to reforms and the size of the districts increase in spending due to reforms. Accordingly, to use only the variation in school spending associated with reforms we use the number of years of exposure interacted with the districts specific increase in spending as our exogenous instrument for exposure to school spending. Specifically, we estimate the following system of equations by two-stage least squares (2SLS).

$$\text{[4]} \qquad \begin{aligned} \widehat{PPE}_{5\text{–}17} &= \pi_1(t_{icb} - T_c^*) \cdot SPEND_c + X_{icb}\pi_2 + Z_{cb}\pi_3 + (W_{1960c} * b)\pi_4 + \eta_{c1} + \lambda_{b1}^r + \phi_{g1}^r * b \\ Y_{icb} &= \delta \cdot \widehat{PPE}_{5\text{–}17} + X_{icb}\beta + Z_{cb}\gamma + (W_{1960c} * b)\varphi + \eta_c + \lambda_b^r + \phi_g^r * b + \varepsilon_{icb} \end{aligned}$$

All variables are defined as in [3]. The difference between [3] and [4] is that we replace the event time indicator variables interacted with the district-specific effect with a single measure of expose to per pupil spending, $PPE_{5\text{-}17}$, in the second stage regression. In the first stage regression, we instrument for $PPE_{5\text{-}17}$, with a parameterized version of the event time indicators (i.e. a linear in years of exposure) interacted with the district-specific reform induced spending increase, $(t_{icb} - T_c^*) \cdot SPEND_c$. Standard errors are clustered at the school district level.

The instrumental variables models exploit both the variation in timing and intensity of school spending changes due to court-mandated reforms to obtain clean causal estimates of the effect of increased school spending on adult outcomes. The coefficient δ from the instrumental variables regressions should uncover the casual effect of increased school spending on adult outcomes so long as the timing of court mandated SFRs is exogenous to changes in outcomes across birth cohorts within districts that saw larger versus smaller increases in school spending due to the reforms. Both the event study analysis and additional placebo tests show that this is likely the case. For comparison purposes, we also present results from a naïve ordinary least squares specification that does not instrument for per-pupil spending.

VI. Estimated Effects on Longer-Run Outcomes

Educational Attainment. Figure 12 presents the semi-parametric event-study model results of the effects of reform-induced changes in per-pupil spending on the probability of graduating from high school. These are shown separately for poor (left) and non-poor (right) children We obtained the individual event time indicator variables interacted with the district specific increase in spending and plot the estimated event time graph for a 10 percent, 20 percent, and 25 percent spending increase (these roughly correspond to $500, $1,000, and $1,250 increases in per-pupil spending). All estimates are centered on the effect in year 0 — the year an individual was 17 years old. As detailed in Section V, all models include school district fixed effects, race-specific region, and year of birth effects; controls for linear cohort trends in 1960 county characteristics; controls at the county level for the timing of school desegregation and hospital desegregation, roll-out of the "War on Poverty," and related safety-net programs; and childhood family characteristics.

Looking first at children from poor families, the event-study plots for a 10, 20, and 25 percent increase all follow a similar broad pattern. Districts that saw increases in school spending exhibit no discernible trending in high school graduation for the pre-treatment cohorts (those that were 18 or older at the time of the reforms). Importantly, the pre-reform year effects are very similar for districts that experienced a 10, 20, and 25 percent spending increase. That is, districts that had large spending increases after reforms were on the same trajectory as districts that saw small increases or reductions in school spending after reforms. This indicates that the timing of the reforms was exogenous to changes in high school graduation rates in a given district and that the size of the eventual spending increase was unrelated to the pre-reform trends in outcomes. This lends credibility to our empirical design and the resulting instrumental variables estimates.

Looking at partially exposed cohorts, the results are consistent with real causal effects on exposed cohorts from poor families. That is, cohorts with more years of exposure to spending increases have higher high school graduation rates than unexposed cohorts and cohorts with fewer years of exposure. Also, the increases associated with exposure are larger in districts that experienced the largest increases in spending. Both the patterns in timing and intensity support the hypothesis that the increased school spending associated with the court-mandated SFRs increased high school graduation rates. Looking to the fully treated cohorts, the results are somewhat noisier, but there is a clear pattern of better outcomes for those fully treated cohorts (than untreated cohorts) in districts that saw larger increases in school spending.

The estimates for non-poor children reveal a very different pattern from those of poor children. To allow for an easy direct comparison, the event study plots for poor and non-poor children are presented on the same scale. For non-poor children, there is suggestive evidence of a rather *slight* increase in high school graduation after the passage of reforms. Exposed cohorts do appear to have slightly higher high school graduation rates than the pre-reform cohorts, and districts that experienced larger spending increases do seem to have somewhat better high school graduation rates than those with smaller spending increases for the exposed cohorts. While the pattern of results might indicate small effects for children from non-poor families, the magnitudes of these effects are much smaller than those for children from poor families.

Looking beyond high school graduation to overall years of education reveals very similar patterns to those for high school completion. Figure 13 presents the event study plots for a 10, 20, and 25 percent spending increase on years of educational attainment. As with high school graduation, there is no trending in outcomes for the pre-reform cohorts. For children from poor families (left), years of education is increasing in years of exposure and the increases are larger for those districts that experienced the largest spending increases. As with high school graduation, for non-poor families there is very weak evidence of somewhat positive effects.

These results are consistent with the conclusions that spending increases only improve educational outcomes for those who are exposed during their school-age years and that the benefits associated with improved spending are concentrated among children from poor families. That is, while outcomes are largely similar across exposed and unexposed cohorts for children from non-poor families, for children from poor families we see that (a) increases in educational outcomes occur only for exposed children during school-age years, (b) improvements are monotonically increasing in years of exposure, (c) improvements are larger with larger spending increases, (d) the timing of improvements in outcomes track the timing of the increases in spending, and (e) there are no differential pre-reform trends in outcomes for districts that experience increases or decreases in spending.

Having established that there are real policy-induced improvements in long-run educational attainment associated with larger school spending increases for exposed cohorts, we now quantify the relationship between spending increases and longer-run educational attainment. For this we turn to the instrumental variable regression results that use the event study patterns to predict changes in childhood exposure to per-pupil spending. Putting all the variation together, the instrumental variables 2SLS models provide a direct estimate of the effect of increased school spending on adult outcomes and allow for tests of statistical significance.

The regression estimates are presented in Table 2. The main outcomes are the educational attainment measures and the variable of interest is the natural log of average per-pupil spending during an individual's school age years times five. The interpretation of a unit change in this variable is the effect of increasing school spending by 20 percent over all 12 of an individual's school age years. The excluded instrument for this spending variable is the number of school-age-years of exposure to reforms interacted with the respective school district's reform induced change in school spending. The first stage F-statistic is greater than 50 in all models. For comparison purposes, we also show estimates from ordinary least squares (OLS) regression models that do not account for the possible endogeneity of school spending.

Column 4 in the top panel of Table 2 presents the 2SLS regression results based on variation presented in Figures 12 for children from poor families. The 2SLS coefficient on the log of school spending*5 is 0.2293 (se=0.0727). This means that for children from poor families, increasing per pupil spending by 20 percent in all 12 school-age years increases the likelihood of graduating high school by 23 percentage points. This estimate is statistically significant at the one percent level and the 95 percent confidence interval is between 8.7 and 37 percentage points. To put these high school graduation estimates in perspective, the high school graduation rates for non-poor and poor children were 79 and 92 percent, respectively. Increasing per-pupil school spending by 20 percent over the entire schooling career of a cohort of low-income children will increase the high school graduation rate for those children by between 11 and 46 percent. In fact, the effects are large enough to completely eliminate the high school graduation gap between children for poor and non-poor families. Consistent with Figure 12, there is a small statistically insignificant effect for children from non-poor families (top panel column 6). The 2SLS coefficient on the log of school spending*5 for children from non-poor families is 0.0647 (se=0.0526). While this estimate is positive, it is not statistically significantly different from zero at the 10 percent level.

The lower panel presents the regression estimates for years of education. For children from poor families (lower panel column 4), the 2SLS coefficient on the log of school spending*5 is 0.9292 (se=0.2872). This means that for children from poor families, increasing per pupil spending by 20 percent in all 12 school age years increases educational attainment by 0.928 years. This estimate is statistically significantly different from zero at the one percent level and the 95 percent confidence interval is between 0.36 and 1.49 years. The education gap between the children from poor and non-poor families is 1.01 years. As such, the estimated effect for poor children is large enough to almost completely eliminate the education gap between children from poor and non-poor families. Looking to children from non-poor families, there is a small statistically insignificant effect for children from non-poor families (top panel column 6). The 2SLS coefficient on the log of school spending*5 on years of education for children from non-poor families is 0.2959 (se=0.3259). While this estimate is positive, it is not statistically significantly different from zero at the 10 percent level.

In sum, both the visual evidence and the regression evidence indicate that increased school spending caused by school finance reforms had a causal positive effect on the educational outcomes of affected children from poor families. Both analyses suggest that there is little to no effect for children from non-poor families. The magnitude of these effects for children from poor families are large enough to eliminate the high-school completion gap and years of educational attainment gap between children from poor and non-poor families. We present tests for robustness in section VI.2.

Labor Market Outcomes, *Adult Family Income, and Poverty Status*. The next series of results reveals large, significant effects of school spending on poor children's subsequent adult economic status and labor market outcomes, using the same model specifications. Figures 14, 15, and 16 present school spending effects by childhood poverty status on adult economic outcomes (ages 25–45), including wages (Figure 14), annual family income (Figure 15), and the annual incidence of poverty (Figure 16). In light of the parallel set of findings across all of these long-run economic outcomes, the results are discussed in succession below.

As with the educational outcomes, the economic outcome patterns are similar to those hypothesized in Figure 11 for poor children and are indicative of the causal effects of increases in school spending induced by court-mandated reforms. We first discuss the earnings outcomes. For both the log of earnings and family income (Figures 14 and 15), there is no evidence of trend differences prior to reforms between districts that saw larger or smaller increases in school spending after reforms. In contrast, for children from poor families, both earnings outcomes exhibit improvements across cohorts associated with more years of exposure to a spending increase. For children from poor families the increases are only associated with the school-age years, and there is no systematic difference in outcomes across cohorts born at different times but with the same number of years of exposure – consistent with a causal effect of spending increases. The results by intensity are also consistent with real casual effects. For both adult earnings and family income the increases for exposed cohorts are larger for those in districts that experience larger increases in spending. The differences by spending increase are more pronounced for family income (Figure 15) than for earnings (Figure 14), but for both outcomes, the event study for a 10 percent increase (dashed line) lies below that of a 25 percent increase (sold grey line) for the exposed cohorts.

The figures for children from non-poor families tell a different story than that for children from poor families. For children from non-poor families, there is a tendency for the exposed cohort to have somewhat worse outcomes than the unexposed cohorts. While the time variation might be suggestive of slight negative effects for children from non-poor families, the effects by treatment intensity do not support this conclusion. If the reduced effects for children from non-poor families were due to increased spending one should see that the reductions are larger for a 25 percent increase than a 10 percent increase. For income this is not the case, and for family income exposed children fared *better* when the district saw larger increases in school spending. This is inconsistent with increased spending reducing family incomes for children from non-poor families. This conclusion is supported by the regression results.

The regression estimates for adult economic outcomes are presented in Table 3. As before, the interpretation of a unit change in this variable is the effect of increasing school spending by 20 percent over all 12 of an individual's school age years. The top panel of Column 4 presents the 2SLS effects for the natural log of earnings. The estimated coefficient of 0.2460 (se=0.107) for children from poor families means that increasing per pupil spending by 20 percent in all 12 school age years increases the adult wages of such children by 24.6 percent. The 95 percent confidence interval is between 3.4 and 45 percent. The point estimate of 0.246 implies an elasticity of wages with respect to per-pupil spending of close to one. However, standard errors support a range of elasticities between 0.17 and 2.28. As one might expect, the 2SLS coefficient on per pupil spending for children from non-poor families (top panel of column 6) is negative. However, the p-value associated with the hypothesis that this negative point estimate is different from zero is 0.466. The results suggest that the effect of increasing school spending by 20 percent in all school age years is large enough to eliminate the wage gap between children from low- and high-income families.

The estimates for the natural log of family income are similar to those of other outcomes. The middle panel of Column 4 presents the effects for the natural log of family income. The estimated coefficient of 0.522 (p-value<0.01) for children from poor families means that for children from poor families, increasing per pupil spending by 20 percent in all 12 school age years increases family income by 52.2 percent. The 95 percent confidence interval is between 17.4 and 86 percent. Unlike the effects for wages, the 2SLS coefficient on per pupil spending for children from non-poor families (middle panel of column 6) is positive. However, the point estimate is not statistically significantly different from zero at traditional levels. The positive effects on family income and the negative effect for wages (neither of which is statistically significant) suggest that no systematic effect for children from non-poor families. As with wages, the results suggest that the effect of increasing school spending by 20 percent in all school age years is large enough to completely eliminate the family income gap between children from low income families and those from non-poor families.

The last outcome is adult poverty. Because this is an undesirable outcome, estimates should be interpreted such that lower numbers are better. The event study is presented in Figure 16. As with the other outcomes, there is strong evidence of a causal effect of school spending on outcomes for children from poor families and no effect for children from non-poor families. The left panel of Figure 16 shows that there is no pre-reform trending in outcomes across unexposed cohorts for non-poor families. However, the exposed cohorts have steady declines in adult poverty that is increasing in both years of exposure and the size of the districts' increase in spending. In stark contrast to that for poor children, the event study for children from non-poor families (right) shows no systematic change in outcomes across cohorts/timing or treatment intensity. The regression results (lower panel of Table 3) are consistent with this. The 2SLS point estimate for children from poor families is -0.1974 (se=0.0587). This means that means that for such children, increasing per pupil spending by 20 percent in all 12 school age years decreases the likelihood of falling into poverty as an adult by 19.7 percentage points. This estimated effect is statistically significantly different from zero at the one percent level and the 95 percent confidence interval is between 8.23 and 31 percentage points.

Taken together, the event study graphs and the instrumental variables regression estimates based on exogenous changes in school spending present a compelling case that increased school spending caused by school finance reforms has meaningful causal effects on adult earnings, family income, and poverty status. We now present a few more robustness tests and discuss the findings in the context of others in the literature.

a. Robustness Checks

Falsification Tests: We probed the robustness of these 2SLS estimates further in several ways. First, as a placebo falsification test using the 2SLS models, we estimate the marginal effect of school spending during non-school-age years. That is, we estimate 2SLS models similar to equation [4] where in addition to including school spending between the ages of five and 17, we also include school spending between the ages of zero and four (when there should be no effect) and school spending between the ages of 20 and 24 (when there should also be no effect). Note that because some students do remain in school through age 19 we did not include school spending during ages 18 and 19 in the falsification test. To isolate exogenous changes in school spending for the different age ranges we use an instrument for exposure during the respective age ranges. As before, we instrument for school spending between ages five and 17 (school-age years) with the number of years of exposure between ages five and 17 interacted with the district-specific increase in spending. We instrument for school spending between ages zero and four 4 (pre-school-age years) with the number of years of exposure between ages zero and four interacted with the district specific increase in spending, and we instrument for school spending between ages 20 and 24 (post-school-age years) with the number of years of exposure between ages 20 and 24 interacted with the district specific increase in spending. The first stage F-statistic for each of the three endogenous regressors is greater than 50. If the effects documented for poor children are real, there should be effects during school age years and no effects for non-school-age years.

Regression estimates from this placebo test are presented in Table 4 for all long-run outcomes. Note that these placebo tests are estimated for children from poor families only. For all outcomes, there are statistically significant effects of school spending during school-age years and no statistically significant effect of school spending for non-school-age years. As further evidence of no effect for the non-school-age years, the placebo estimates are in different directions for the various outcomes showing that there was no tendency toward improving or deteriorating outcomes among unexposed cohorts in districts that saw larger or smaller increases in school spending. These placebo estimates support the visual evidence of real casual effects presented in the figures.

Validating Using Other Data: While the tests thus far show that our estimates are internally valid, readers might wonder how these patterns might generalize to districts that are not included in the PSID. To speak to this issue, we replicated the analyses for high school graduation using the Common Core Data (CCD)—Local Education Agency Universe Survey and Non-Fiscal Survey Database—for all school districts in the US for available years 1987-2010 with the preferred research design, as reported in Appendix B. We find a similar pattern of results for the effects of reform-induced school spending changes on district-level graduation and high school dropout rates (these effects are not broken up by poverty level). Using a variant of the models (based only on state level variation), we are able to replicate the patterns of our main findings using the Intergrated Public Use Microdata Series from the Census for educational attainment and adult earnings (Appendix C). The similar pattern of the PSID, CCD, and Census results demonstrate that the findings are generalizable and representative for these birth cohorts, and assuage concerns that the results are specific to the PSID.

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 based on whether, how long, and how much SFR influenced per-pupil spending during their K–12 school years. The evidence is not consistent with alternative counter-explanations or causes. Based on the robustness of the results, such an alternative cause would have to meet the following 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 effects are constrained only to school-age years of exposure (given the evidence showing no effects for non-school-age years for both pre-school ages and beyond age 17); (c) it had the largest effects on poor children in communities where SFR resulted in the largest changes in school spending; and (d) it had no effects on individuals from non-poor-childhood families. Because we can think of no such counter-example, and because we are careful to control for a variety of potentially confounding policies and effects, we are confident that these effects can be taken as causal.

b. The Importance of Using Exogenous Variation

As mentioned previously, comparing outcomes of individuals exposed to different levels of school spending without accounting for changes in school spending that may be the result of other factors that also directly affect the outcomes of interest, could lead to bias. One of the benefits of our framework is that we only exploit plausibly exogenous variation in school spending that is driven by the reforms. To gauge the extent to which this matters, we also estimated naïve OLS regression for all our models.

For all outcomes and subsamples, the OLS estimates are orders of magnitude smaller than the 2SLS estimates and only one of the 15 OLS estimates is statistically significantly different from zero. Looking to poor children, where we find sizable effects in 2SLS models, the OLS estimates are all economically insignificant and not statistically significant from zero at the 10 percent level. The stark contrast between the OLS and the 2SLS estimates underscores the importance of relying on exogenous variation in school spending. Importantly, the contrast between the OLS and the 2SLS estimates in our data provides an explanation for why these estimates might differ from other influential studies in the literature (e.g., Coleman et al. 1966, Betts 1995, Hanushek 1996, and Grogger 1996). We suspect some prior studies that lacked a compelling research design to isolate causal effects of spending may have produced modest estimated effects of school spending due to unresolved endogeneity biases.

c. Exploring the Mechanisms.

A limitation of the results on the long-term impacts of school spending is their reduced-form nature. We cannot separately identify the pathways through which various types of K-12 education spending, and the composition of school expenditures, impacts subsequent adult attainments. We did, however, explore these issues using extensions of our main model specifications to examine the impacts of SFRs on instructional spending, school support services, physical capital and school building expenditures. To speak to these issues, we employ data from the CCD on the types of school spending (available for years 1992 through 2010) and student staff ratios (available for years 1986 through 2010). The earliest CCD data start in 1986 so that we do not have detailed data for the same cohorts that are exposed to the early reforms in the PSID. However, an analysis of mechanisms for the more recent cohorts might be instructive. To determine how each additional dollar associated with reforms was spent, we employ instrumental variables models similar to equation [4] where the main outcomes are capital expenditure, expenditure on instruction, and instruction on services. To gain an understanding of how these reforms affected student-staff ratios we employ the same instrumental variables models similar to equation [4] where the main outcomes are students per school, students per teacher, students per counselor, and students per administrator. The endogenous regressor is the level of per pupil spending and the excluded instrument is the number of years of exposure to reforms interacted with the district specific spending increase. Results are presented in Table 5.

SFRs led to increases in all categories of spending. When a district sees an increase in school spending of $1,000 due to reforms, spending on capital increases by $86, spending on instruction increases by $559, and spending on support services increases by $405 on average. Relative to mean levels, these increases are roughly proportional to the allocation of funds on average – suggesting that schools simply increased spending in all categories with little effect on the allocation of funds across categories. The increases for instruction and support services (which includes expenditures to hire more teachers and/or increase teacher salary and also funds to hire more guidance counselors and social workers) are consistent with the positive effects for those from low-income families.

We also estimate effects on student-staff ratios. For these models the endogenous regressor is the natural log of school spending. Districts that experience a 20 percent increase in spending due to reforms see reductions in school size and fewer students per teacher. Both of these have been found to benefit students in general, with larger effects for children from disadvantaged backgrounds (e.g., Krueger and Whitmore 2001, Bloom and Unterman 2013). We also find that schools in these districts have fewer students per counselor and fewer students per administrator. These have also been found to improve student outcomes (e.g., Reback 2010, Carell and Carell 2006). While there may be other mechanisms through which increased school spending may improve student outcomes, results suggest that the positive effects may be driven, at least in part, by reductions in class size and having more adults per student in schools. Other possible mechanisms include changes in peer composition and changes in teacher quality.[26] Separately identifying and disentangling the mechanisms underlying the overall causal impact of spending is very difficult with available data and is left for future work.

VII. Summary Discussion and Conclusion

One of the distinguishing features of the U.S. public education system is its heavy reliance on the local property tax base for school district funding. This paper highlights the important role played by the courts and legislators in school finance reform during the past four decades, and documents their long-term effects. We first investigated the effects of school finance reforms on district spending, both in terms of absolute levels in poor districts and in narrowing the spending gaps between the poor and affluent districts in a state. Consistent with prior research, we find that court-mandated reforms were effective at reducing spending inequality between high- and low-income districts within a state and that this was achieved by increasing spending for the lowest-income districts. However, we document new and important differences between various kinds of court-mandated reforms. Equity-based court-mandated reforms are effective at reducing spending differences between high- and low-income areas, but they appear to do so primarily through

[26] For example, if high-income parents were less likely to send their children to private school in districts that experienced increases in school spending (Downes & Schoeman, 1998), children in the public school system may have also experienced changes in peer quality. Districts that saw spending increases may have been able to attract better teachers through increases in salaries, improvements in non-pecuniary characteristics, or changes in the composition of students (Jackson, 2009; Jackson, forthcoming).

redistributing school spending. Adequacy-based school finance reforms are also effective at reducing spending gaps, but they do so by increasing school spending in all districts, with larger increases for low-income districts. Looking to legislative reforms, our findings differ from many others in that we find that legislative reforms were somewhat effective at reducing spending gaps. We document important differences in the effect of reforms based on how they affect funding formulas. Both foundation plans and equalization plans reduce spending inequality with respect to income, with ambiguous effects on the overall level of spending. Reward for effort plans (that lead to low tax prices in order to promote school spending) lead to increased spending growth for about five years after reforms and reduce spending gaps in the long run. Plans that impose spending limits reduce spending for all districts and lead to particularly large reductions in relative spending for high-income districts. Thus, the first part of our paper makes important contributions to the public finance literature on the most effective designs of K-12 school funding formulas to narrow spending gaps between rich and poor districts while increasing overall average spending levels.[27]

The second part of our paper presents new evidence on the long-term productivity of education spending. The results make important contributions to the human capital literature and highlight how improved access to school resources can profoundly shape the life outcomes of economically disadvantaged children, and thereby significantly reduce the intergenerational transmission of poverty. We investigated the reform-induced effects of school spending increases on adult educational attainment, earnings, family income, and poverty status. We find that there are no discernable effects of increased school spending on children from non-poor families. However, our results indicate that for children from poor families, increasing per-pupil spending by 20 percent for a child’s entire K-12 schooling career increases high school completion by 22.9 percentage points, increases the overall number of years of education by 0.928, increases adult earnings by about 24.6 percent, increases annual family income by 52.2 percent, and reduces the incidence of adult poverty by 19.7 percentage points. All of these effects are statistically significant and are robust to a rich set of controls for confounding policies and trends. The magnitudes of these effects are sufficiently large to eliminate between two-thirds and all of the gaps in these adult outcomes between those raised in poor families and those raised in non-poor families.

[27] Today, most of the variation in per-pupil spending is across states, rather than within state. Thus, the effect of school finance reforms can be limited, as far as equalization of spending across the U.S. is concerned.

Our results indicate a causal relationship between per-pupil spending and student outcomes. However, the reform-induced spending changes we examine occurred at a time when average school spending levels were much lower (roughly $4,500) as compared with average per-pupil school spending in 2013 in excess of $10,000. Because education spending likely exhibits diminishing marginal productivity, at prevailing levels of school spending one might require much larger increases in spending to achieve the same effects as those found in this paper. That being said, after Coleman (1966), many have questioned whether increased school spending can really help improve the educational and lifetime outcomes of children from disadvantaged backgrounds. Our findings show that it can.

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Tables and Figures

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

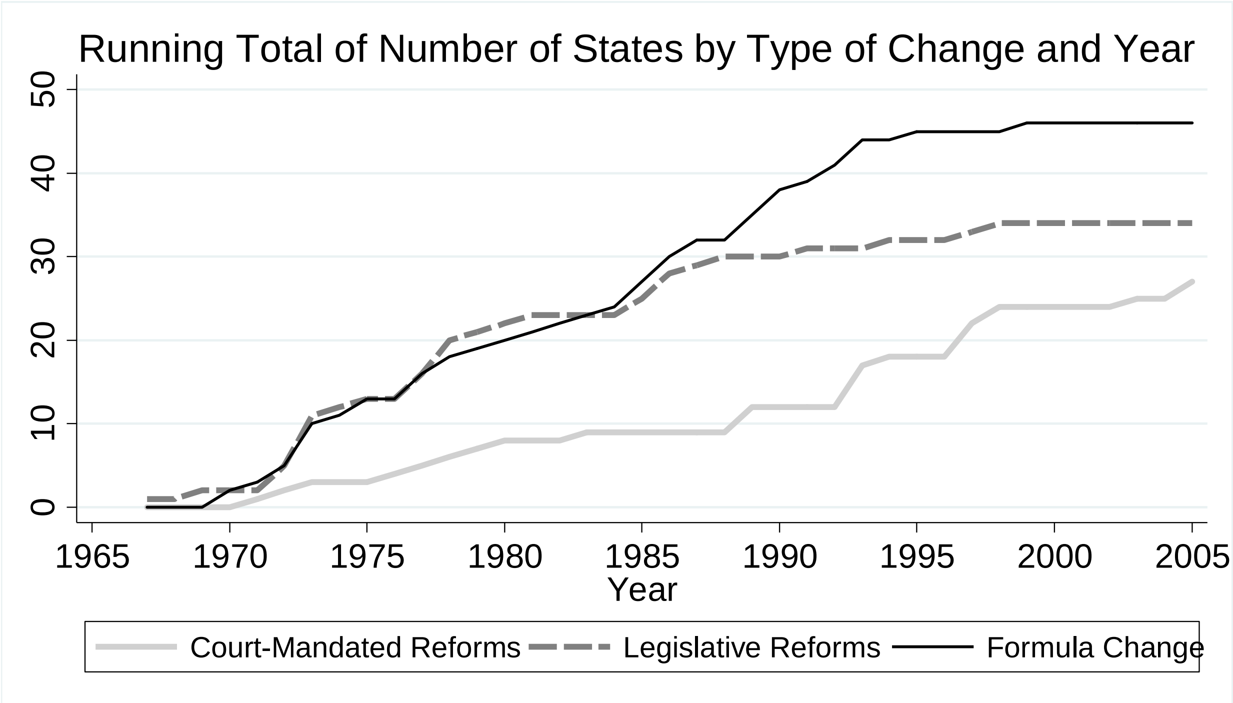


Figure 2: *Type of Cases over Time*

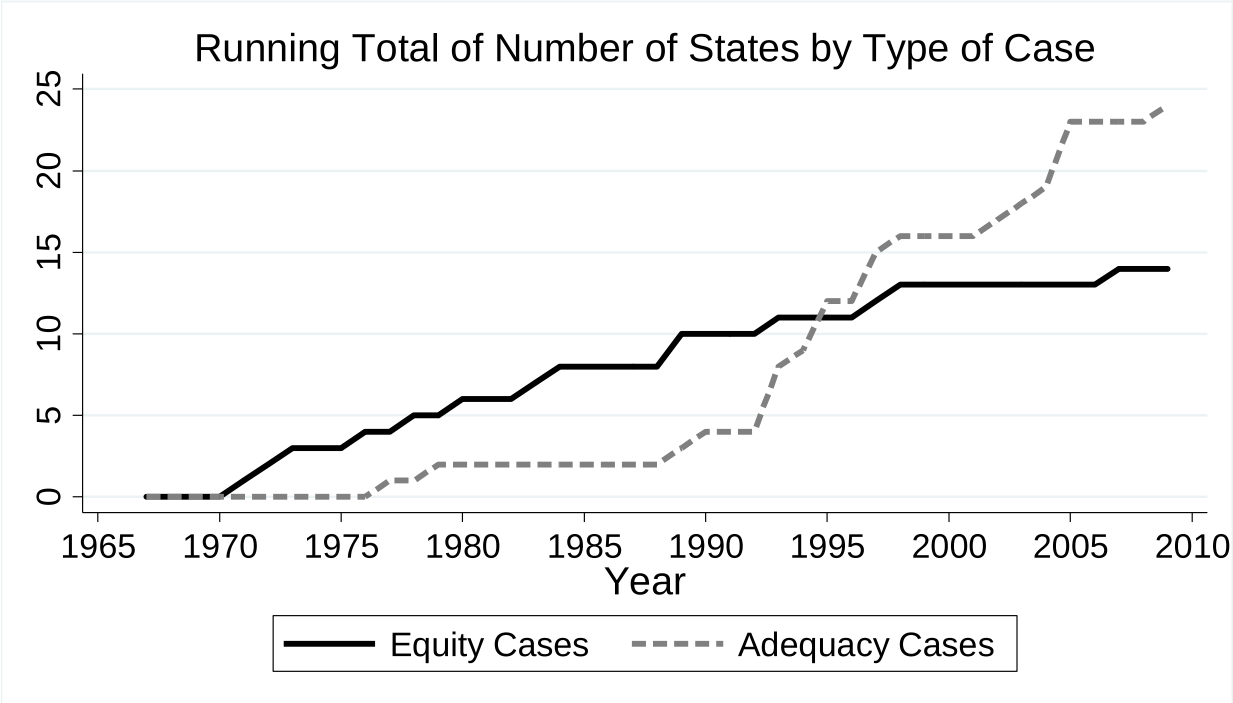


Figure 3: *Types of Spending Formulae*

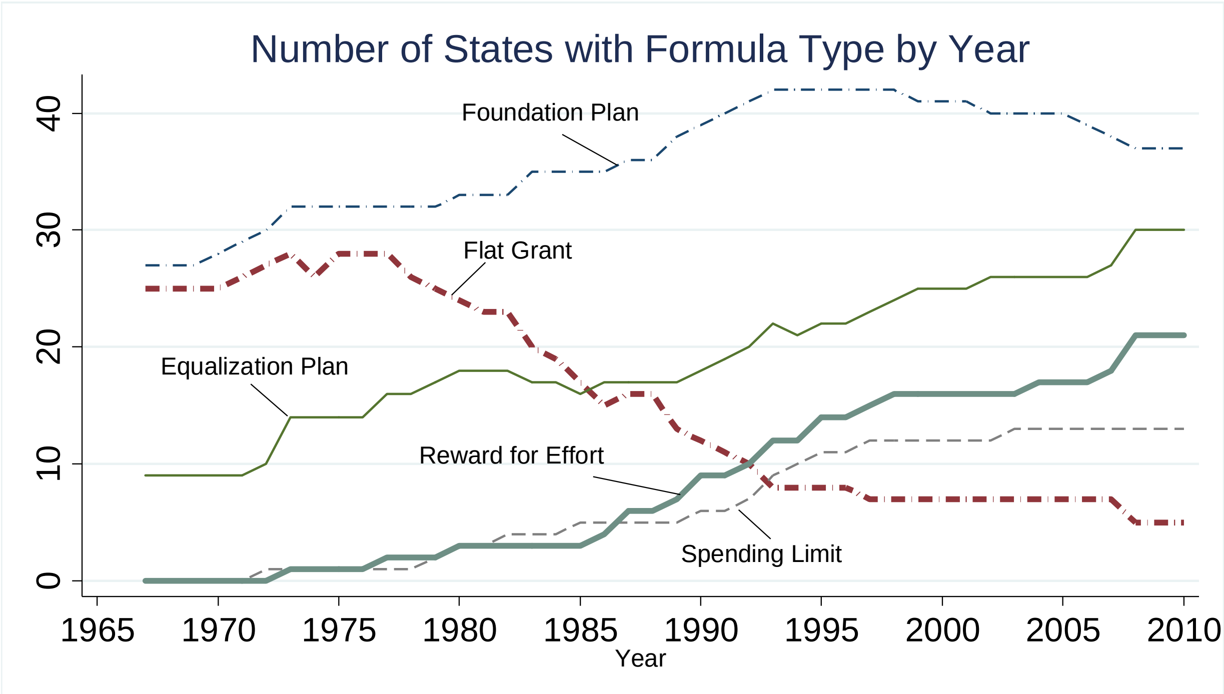


Figure 4: *Per-Pupil Spending by Percentile of Income within State in 1967*

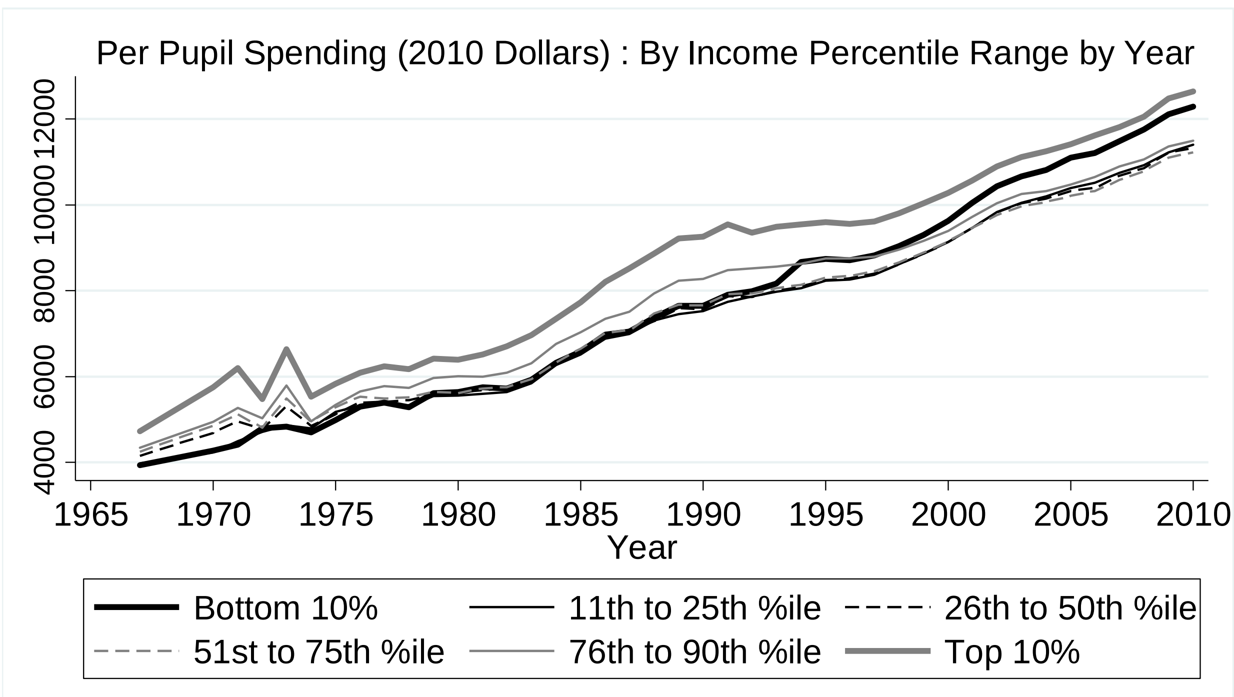
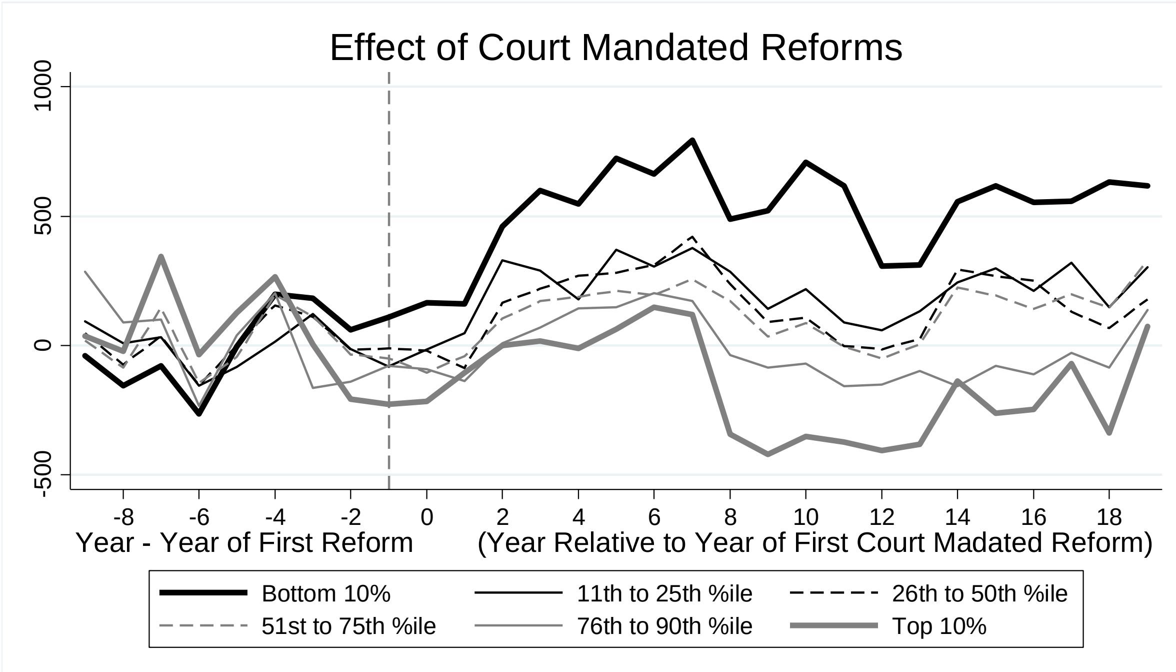
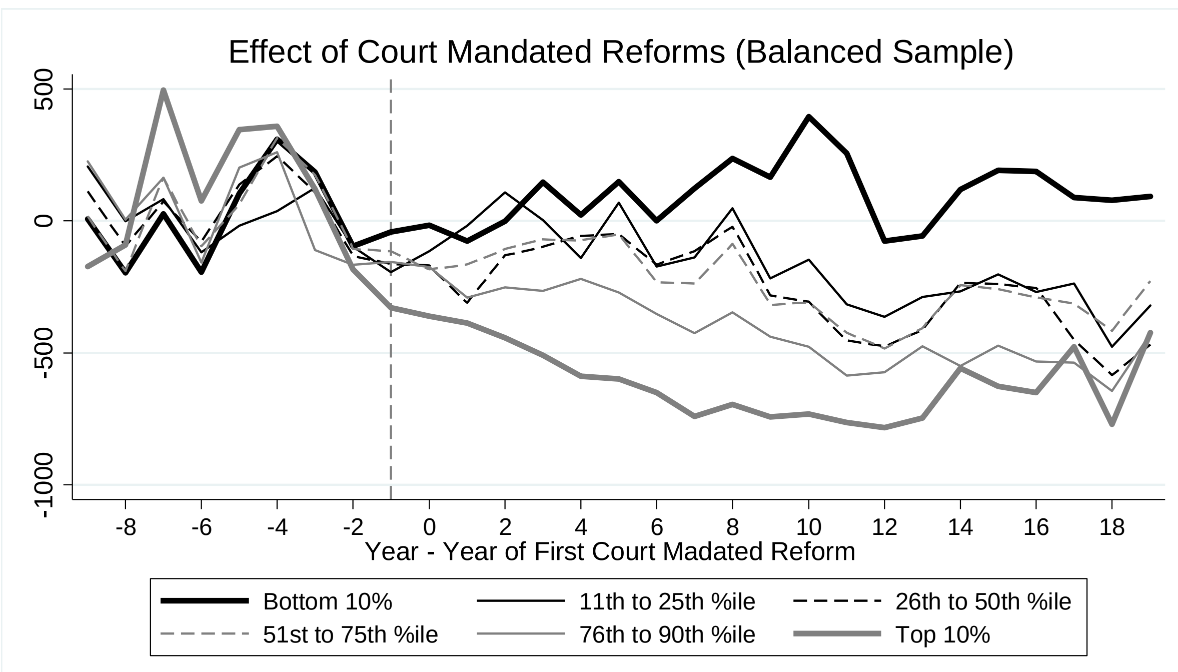


Figure 5 *Effect of Court-Mandated Reforms*



a.



b.

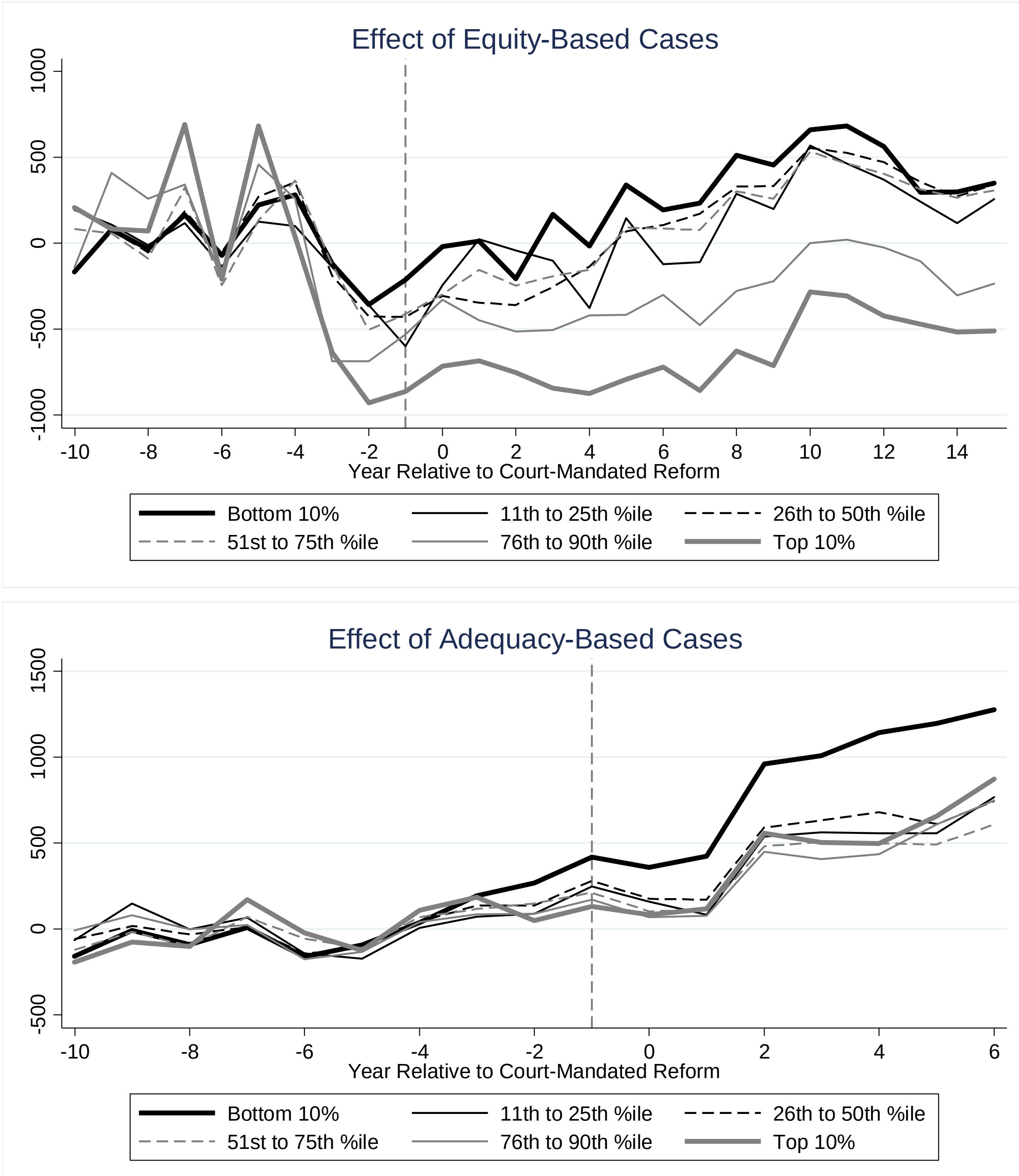
a: all districts

b: only districts observed for more than 10 years after reforms

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

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

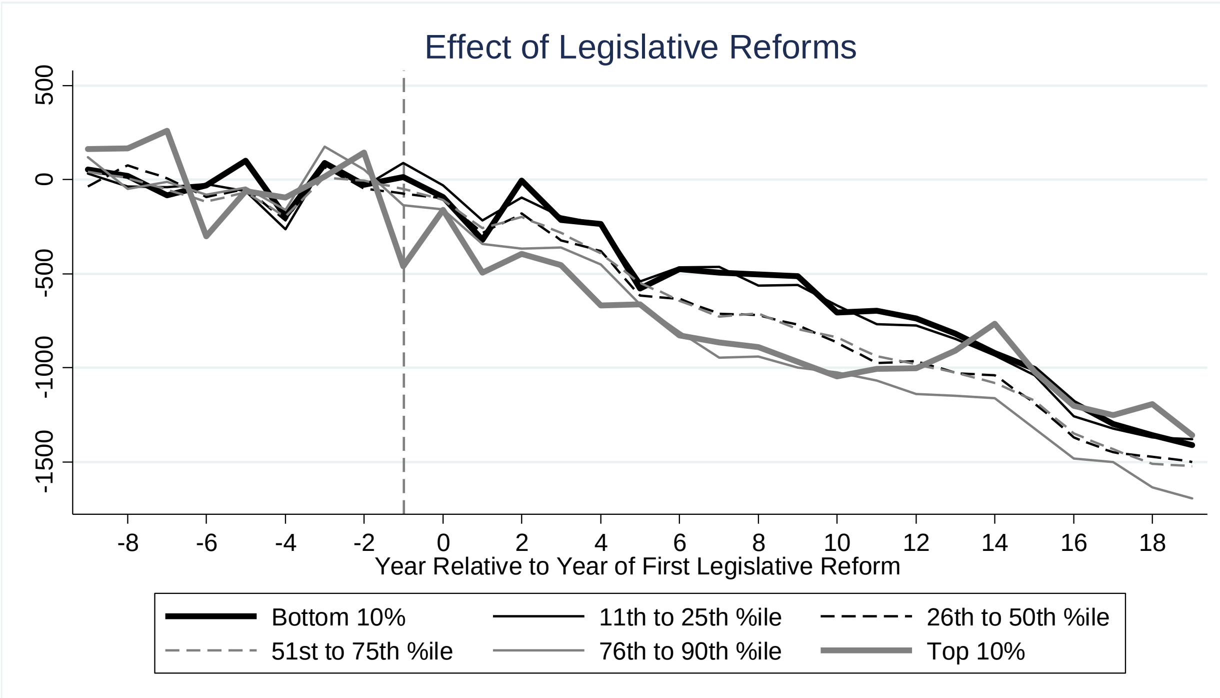
Figure 6 *Effect of Equity and Adequacy Cases*



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

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

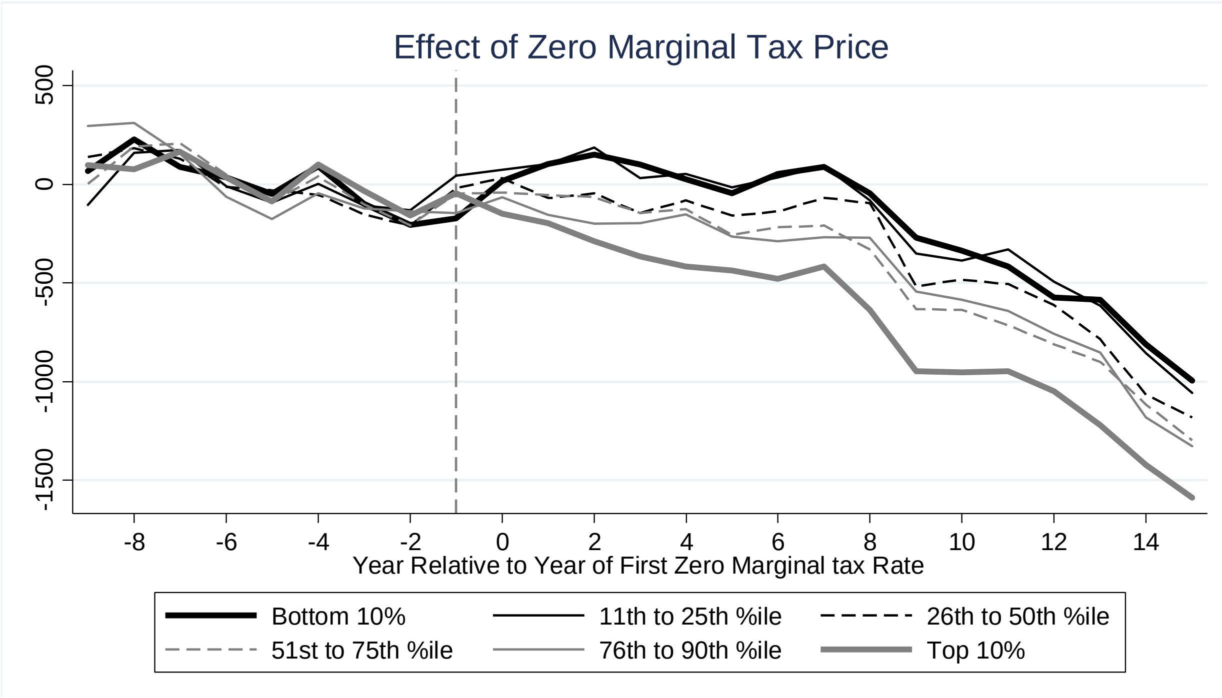
Figure 7: *Effect of Legislative Reforms*



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

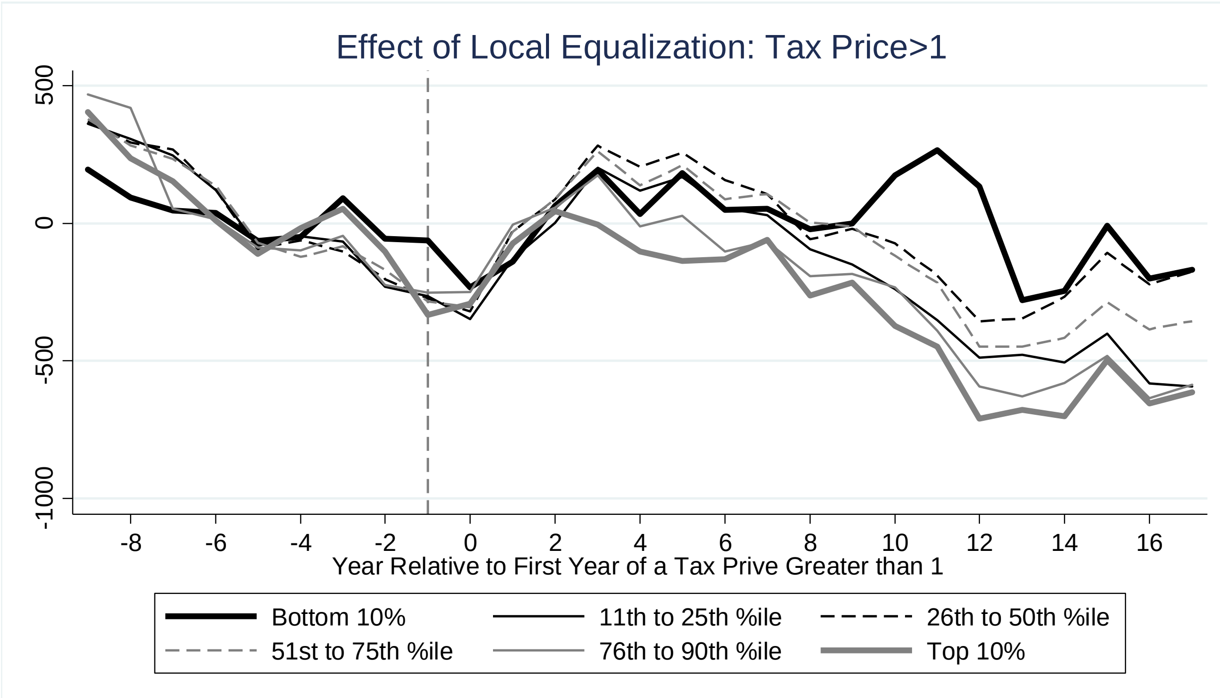
Model: These plots present the estimated coefficients of a regression on per-pupil spending at the district level on year fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables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

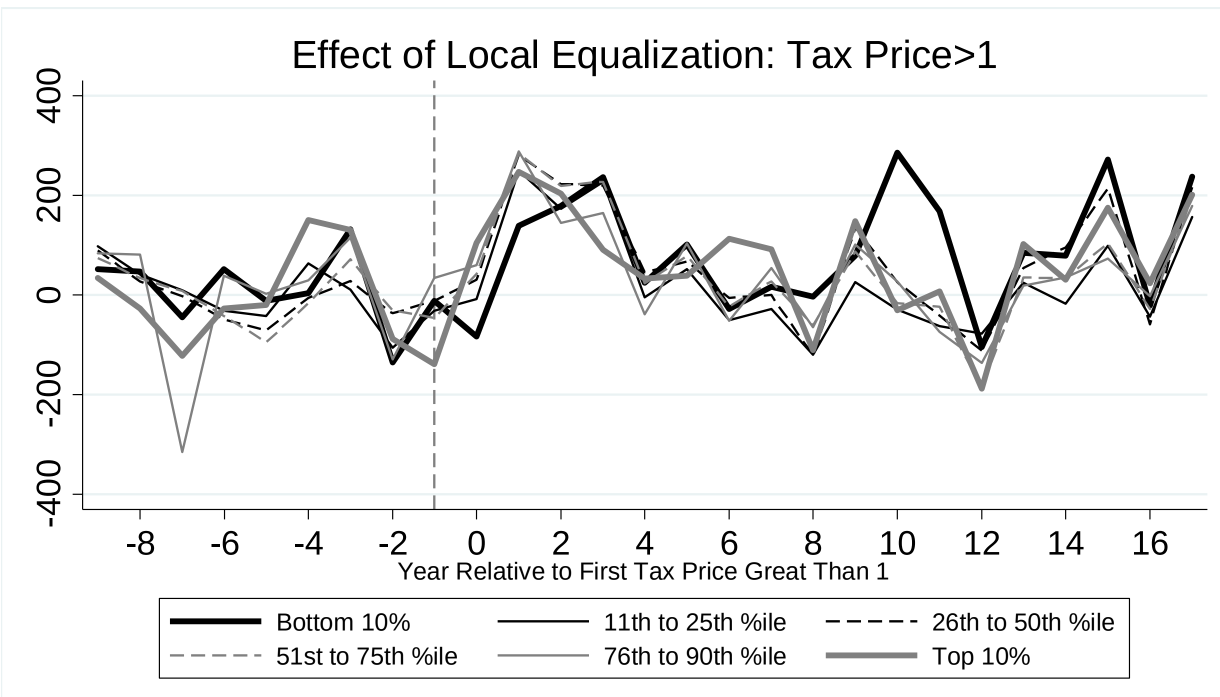
Figure 8 *Effect of Spending Limits (zero inverted tax prices)*



<u>Data:</u> The sample includes all school districts in the United States between the years of 1967 and 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.
<u>Model:</u> These plots present the estimated coefficients of a regression on per-pupil spending at the district level on year fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables from 10 years prior to 19 years after reforms (for reforms that impose spending limits and reward for effort plans simultaneously). Standard errors are adjusted for clustering at the state level.

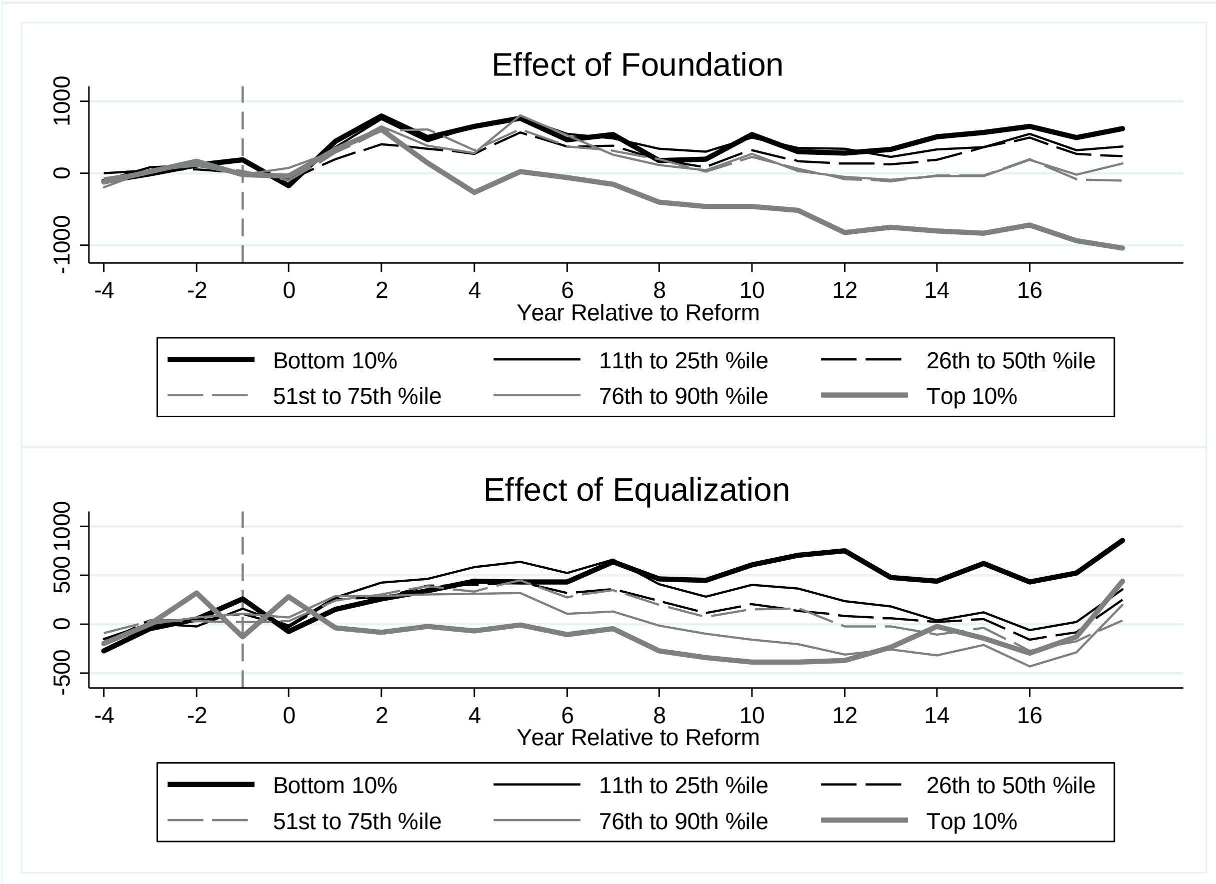
Figure 9 *Effect of Reward for Effort Plans (inverted tax prices greater than one)*





Data: The sample includes all school districts in the United States between the years of 1967 and 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.
Model: These plots present the estimated coefficients of a regression on per-pupil spending at the district level on year fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables from 10 years prior to 19 years after reforms (for reforms that impose spending limits and reward for effort plans simultaneously). Standard errors are adjusted for clustering at the state level.

Figure 10 *Effect of Foundation and Equalization Plans*



Data: The sample includes all school districts in the United States between the years of 1967 and 2010 (unless stated otherwise). The sample is made up of 483,047 district-year observations.
Model: These plots present the estimated coefficients of a regression on per-pupil spending at the district level on year fixed effects, district fixed effects, and the percentile group of the district in the state distribution of median income interacted with a full set of event-time indicator variables from 10 years prior to 19 years after reforms (for equalization plans, foundation plans and flat grant plans, simultaneously). Standard errors are adjusted for clustering at the state level.

Figure 11: *Hypothesized Patterns with a Causal Effect of an Exogenous Increase in Spending*

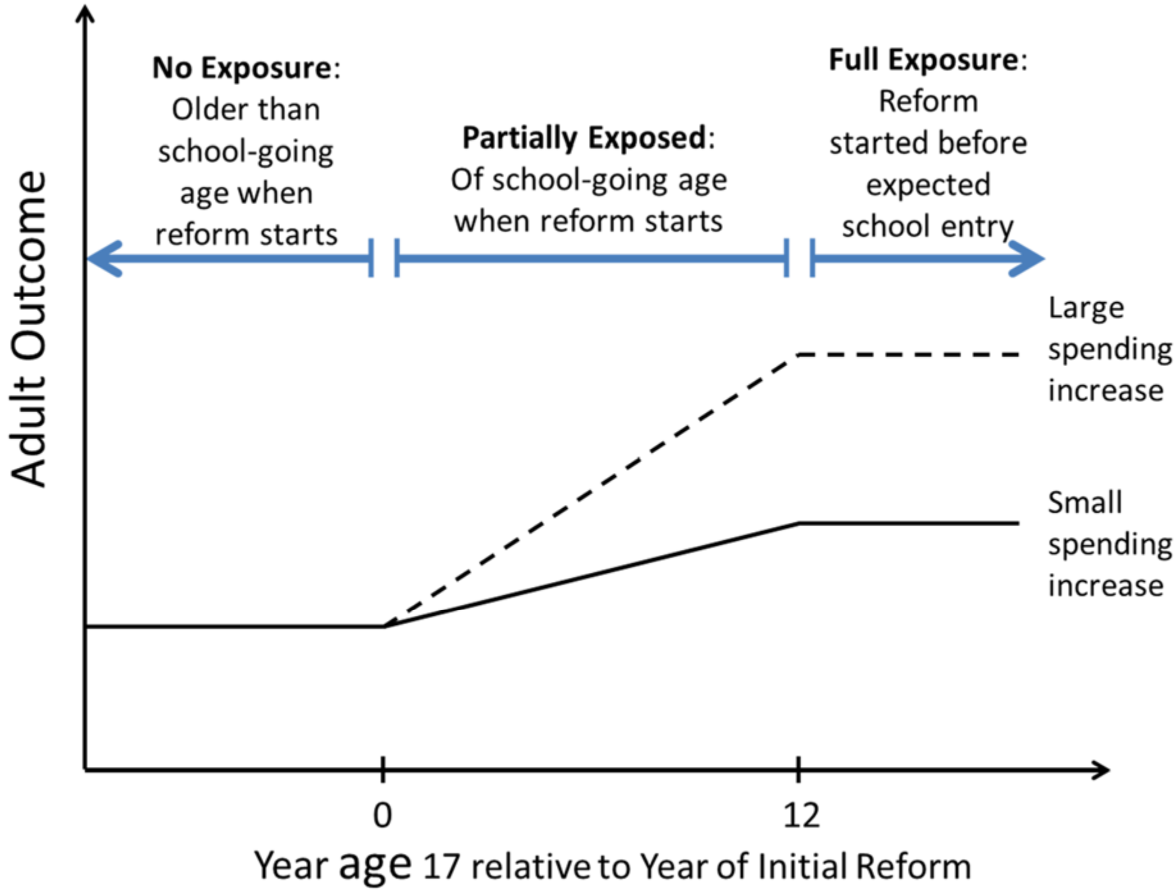
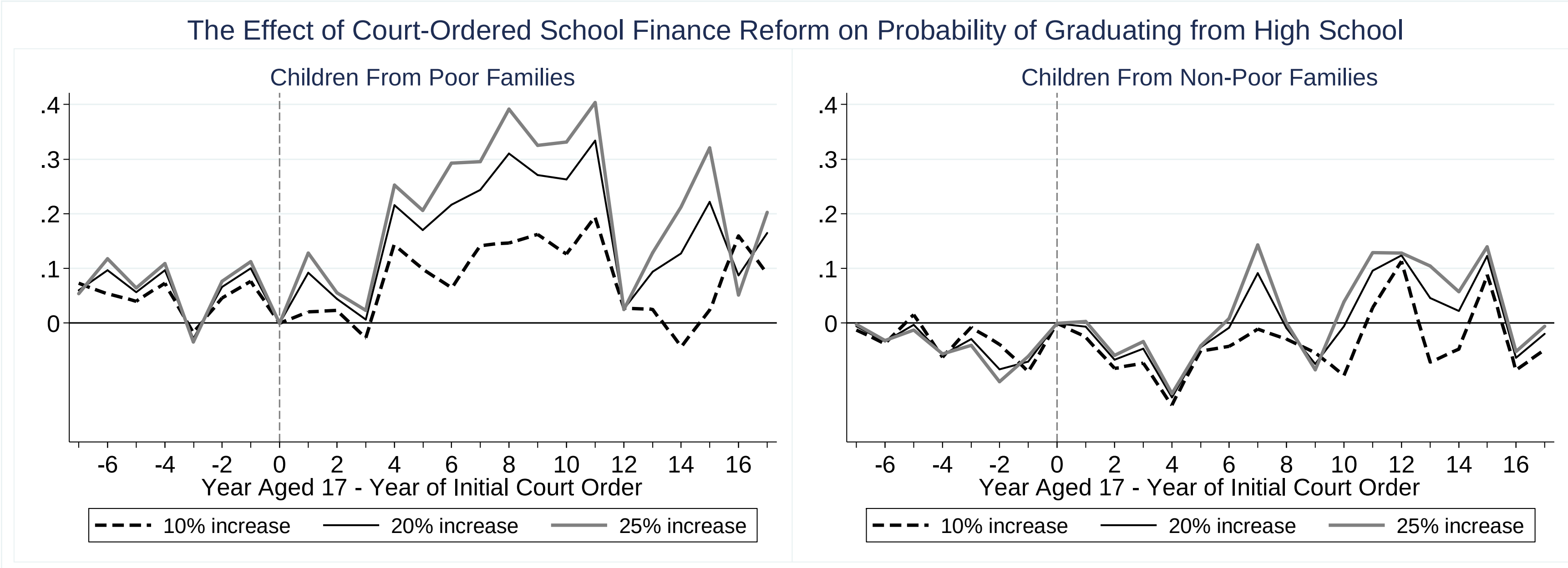


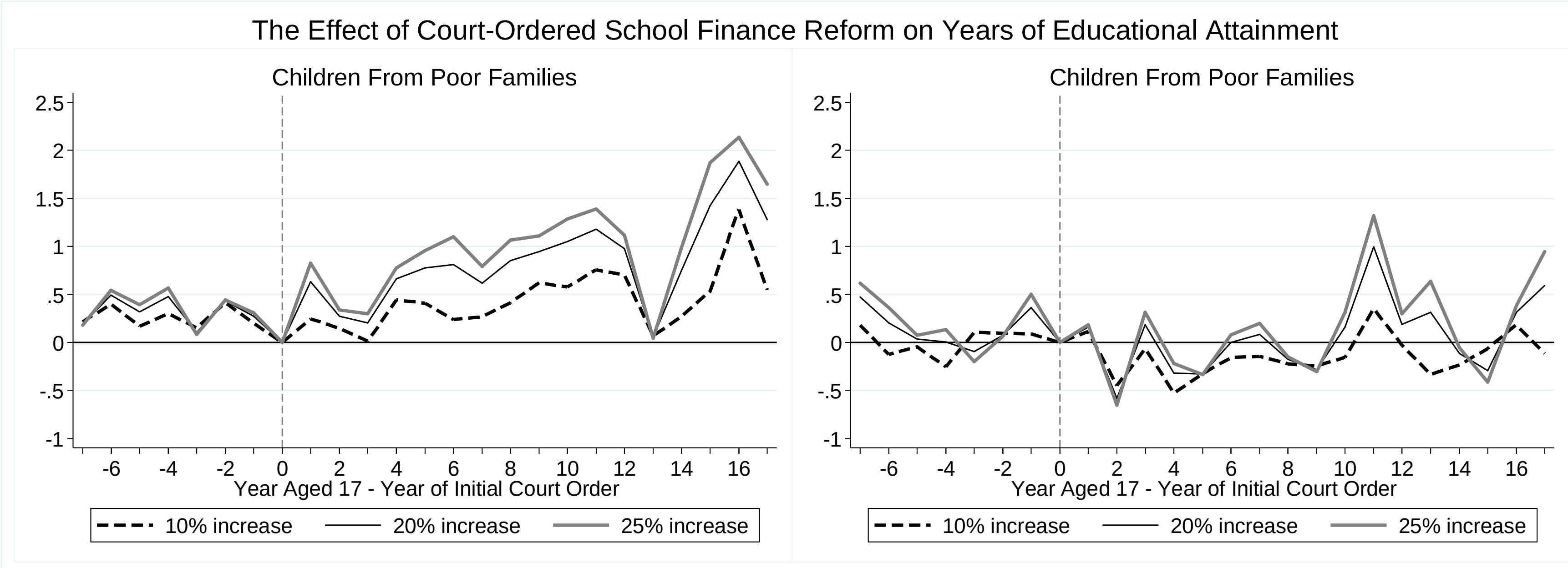
Figure 12: *Event Study of Effects on Probability of High School Graduation by Spending Increase by Childhood Poverty Status*



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 non-parametric event-study models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends, controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded Kindergarten), 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 significantly different from zero.

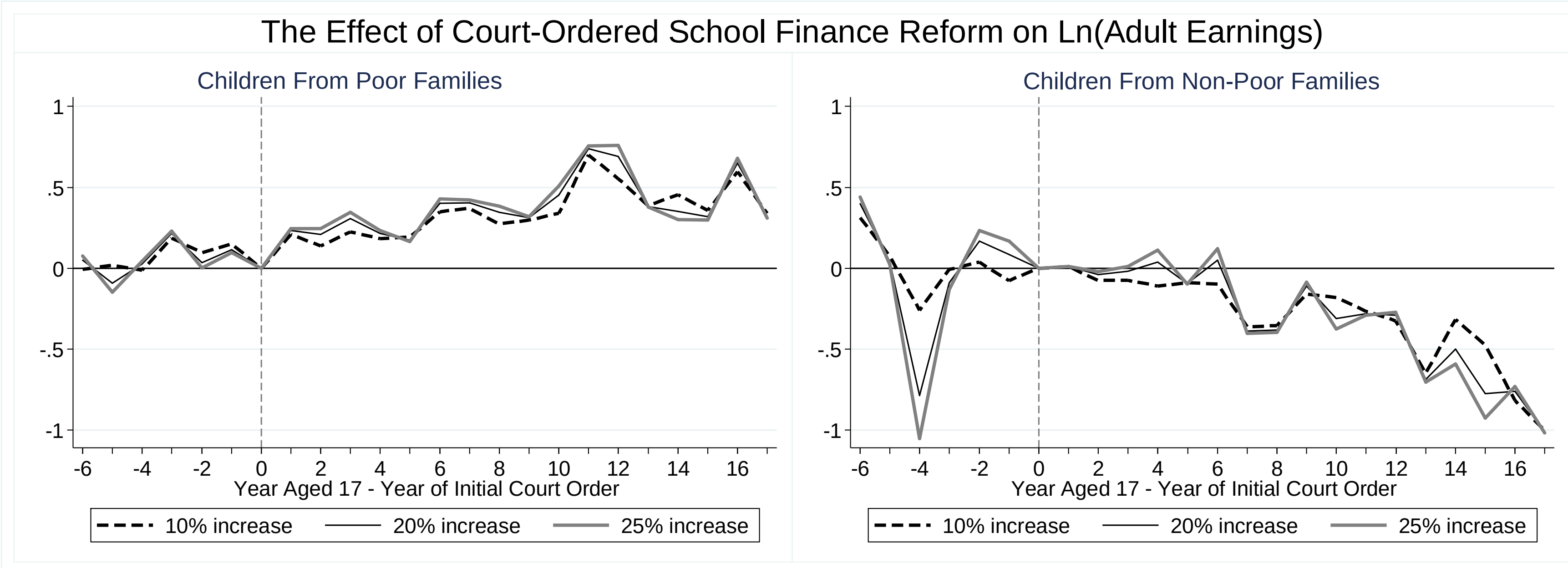
Figure 13: *Event Study of Effects on Years of Education by Spending Increase by Childhood Poverty Status*



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 non-parametric event-study models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends, controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded Kindergarten), 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 significantly different from zero.

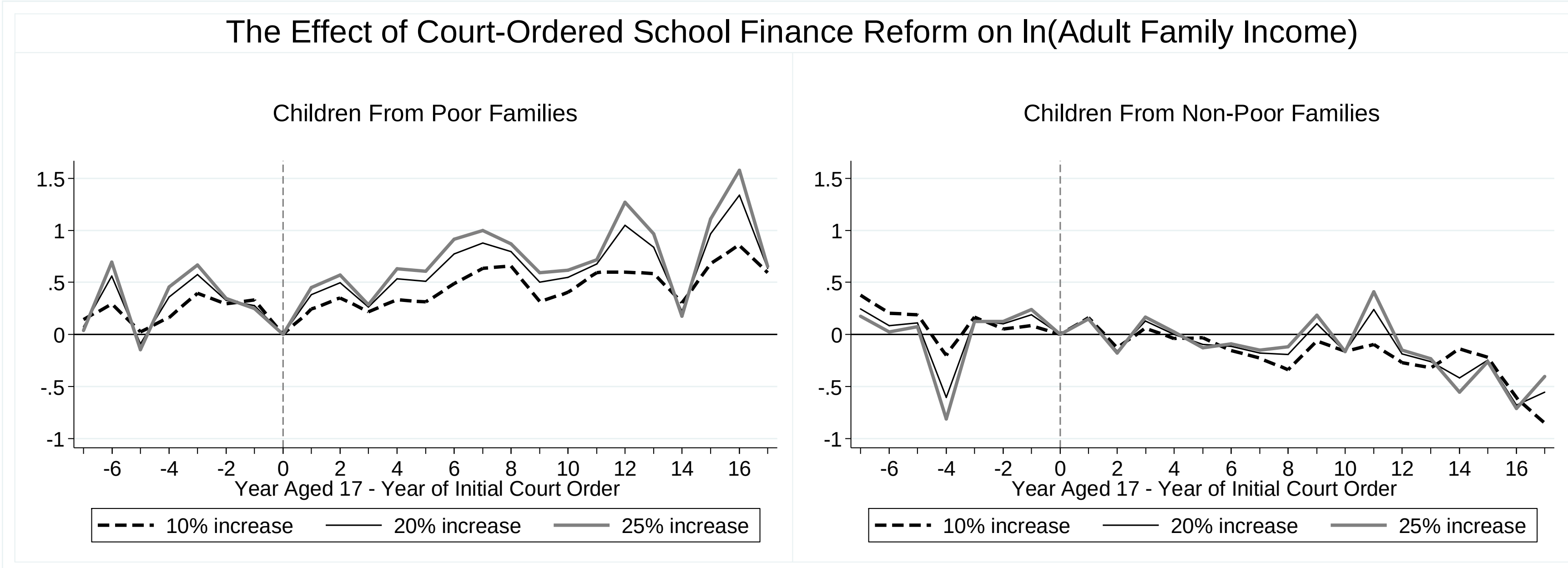
Figure 14: *Event Study of Effects on log of Adult Earnings (ages 25 to 45) by Spending Increase by Poverty Status*



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 non-parametric event-study models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends, controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded Kindergarten), 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 significantly different from zero.

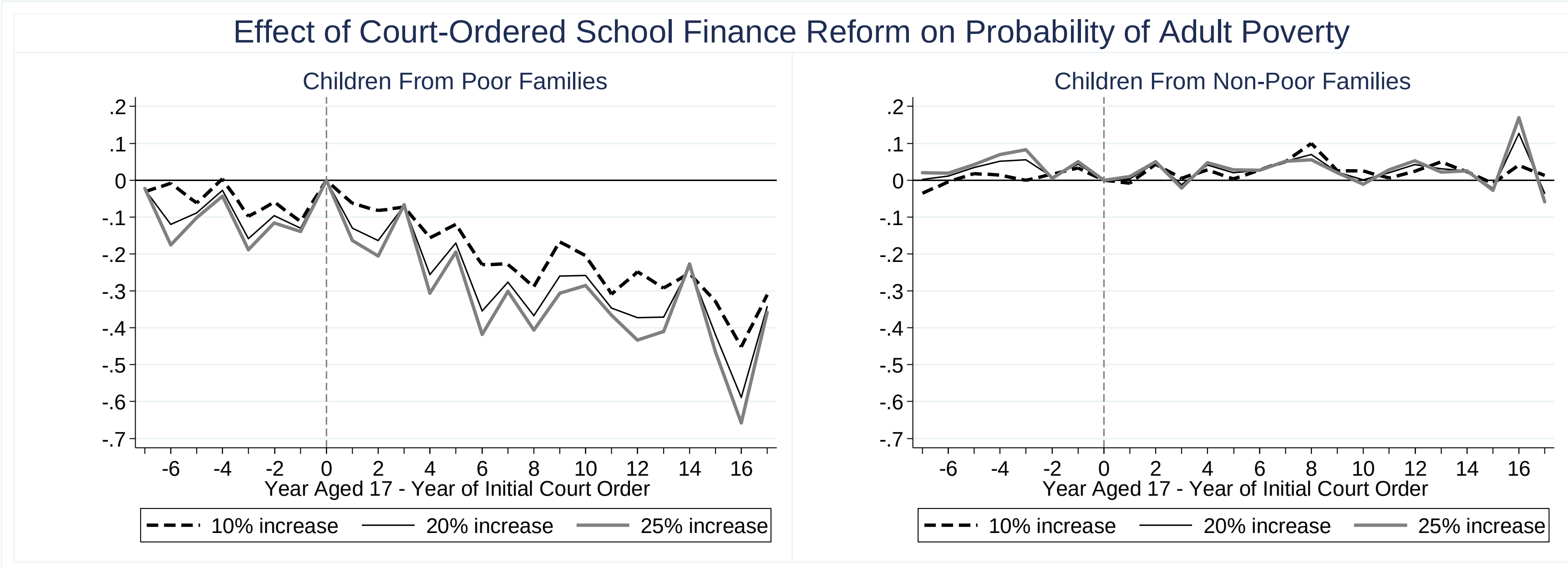
Figure 15: *Event Study of Effects on Log of Family Income (ages 25 to 45) by Spending Increase by Childhood Poverty Status*



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 non-parametric event-study models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends, controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded Kindergarten), 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 significantly different from zero.

Figure 16: *Event Study of Effects on Probability of Adult Poverty (ages 25 to 45) by Spending Increase by Childhood Poverty Status*



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 non-parametric event-study models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends, controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded Kindergarten), 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 significantly different from zero.

Table 1: *Descriptive Statistics by Childhood Poverty Status*

	All (N=15,353)	Poor Child (N=9,035)	Non-Poor Child (N=6,318)
Adult Outcomes:			
High School Graduate	0.86	0.79	0.92
Years of Education	13.18	12.63	13.64
Ln(Wages), at age 30	2.51	2.36	2.61
Adult Family Income, at age 30	$49,308	$35,212	$55,324
In Poverty, at age 30	0.08	0.13	0.04
Age (range: 20-57)	32.9	32.6	33.2
Year Born (range: 1955-1985)	1969	1970	1968
Female	0.44	0.43	0.44
Black	0.14	0.23	0.07
Childhood School Variables:			
Per-pupil Spending (avg., ages 5-17)	$4,463	$4,436	$4,486
Any Court-ordered School Finance Reform, age 5-17	0.53	0.53	0.53
Years of Exposure to School Finance Reform, age 5-17	4.35	4.46	4.27
1960 District Poverty Rate (%)	22.09	24.75	19.88
Childhood Family Variables:			
Income-to-needs Ratio (avg., ages 12-17):	3.17	1.64	3.77
Mother's Years of Education	12.05	11.32	12.66
Father's Years of Education	12.05	10.91	12.93
Born into Two-parent Family	0.62	0.55	0.68
Low Birth Weight (<5.5 pounds)	0.07	0.08	0.06
Childhood Neighborhood Variables:			
Neighborhood Poverty Rate	0.11	0.16	0.08
Residential Segregation Dissimilarity Index	0.72	0.71	0.72

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

Table 2: *OLS vs 2SLS Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Educational Attainment: by Childhood Poverty Status*

	All Children (Population weighted)		Children from Poor Families		Children from Non-Poor Families	
	OLS	2SLS/IV	OLS	2SLS/IV	OLS	2SLS/IV
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Prob(HS Grad)					
Ln(School District Per-pupil Spending)$_{(age\ 5\text{-}17)}$ /.2	**0.0095***	**0.1475*****	0.0010	**0.2293*****	0.0044	0.0647
	(0.0052)	**(0.0410)**	(0.0069)	**(0.0726)**	(0.0065)	(0.0526)
	Dependent variable: Years of Education					
Ln(School District Per-pupil Spending)$_{(age\ 5\text{-}17)}$ /.2	0.0077	**0.7599*****	-0.0247	**0.9282*****	-0.0135	0.2959
	(0.0325)	**(0.2483)**	(0.0314)	**(0.2872)**	(0.0373)	(0.3284)
Number of Individuals	14,670	14,670	8,639	8,639	6,031	6,031
Number of School Districts	1,288	1,288	918	918	978	978

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

Robust standard errors in parentheses (clustered at school district level)

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

Models: Results are based on OLS and 2SLS/IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). PSID sample weights are used to account for oversampling of poor families to produce nationally-representative estimates for models with all kids. The first-stage model includes as predictors the school-age years of exposure to school finance reform interacted with the respective school district's reform-induced change in school spending. There exists a significant first-stage for both poor and non-poor kids.

Table 3: *OLS vs 2SLS Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Long-Run Economic Outcomes: by Childhood Poverty Status*

	All Children (Population		Children from Poor		Children from Non-	
	OLS	2SLS/IV	OLS	2SLS/IV	OLS	2SLS/IV
	(1)	(2)	(3)	(4)	(5)	(6)
	Dependent variable: Ln(Wage), ages 25-45					
Ln(School District Per-pupil Spending)$_{(age\ 5\text{-}17)}$ /.2	-0.0135	0.1255	-0.0047	**0.2460****	-0.0190	-0.1122
	(0.0118)	(0.0822)	(0.0098)	**(0.1077)**	(0.0142)	(0.1540)
	Dependent variable: Ln(annual Family Income), ages 25-45					
Ln(School District Per-pupil Spending)$_{(age\ 5\text{-}17)}$ /.2	-0.0050	0.1710	-0.0056	**0.5220*****	-0.0087	0.2239
	(0.0136)	(0.1223)	(0.0160)	**(0.1775)**	(0.0137)	(0.1984)
	Dependent variable: Prob(Poverty), ages 25-45					
Ln(School District Per-pupil Spending)$_{(age\ 5\text{-}17)}$ /.2	-0.0003	-0.0179	0.0037	**-0.1974*****	0.0045	0.0056
	(0.0023)	(0.0254)	(0.0047)	**(0.0587)**	(0.0033)	(0.0328)
Number of Individuals	14,670	14,670	8,639	8,639	6,031	6,031
Number of School Districts	1,288	1,288	918	918	978	978

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

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

Models: Results are based on OLS and 2SLS/IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). PSID sample weights are used to account for oversampling of poor families to produce nationally-representative estimates for models with all kids. The first-stage model includes as predictors the school-age years of exposure to school finance reform interacted with the respective school district's reform-induced change in school spending. There exists a significant first-stage for both poor and non-poor kids.

Table 4: *2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Long-Run Outcomes: Placebo Tests for Non-school Ages (Children from Poor Families Only)*

	Prob(High School Grad)	Years of Education	Ln(Wage), age 25-45	Ln(Family Income), age 25-45	Prob(Poverty), age 25-45
	1	2	3	4	5
Ln(School District Per-pupil Spending)(age 5-17) /.2	**0.1795*** **	**0.8105*** **	**0.1908***	**0.4579*** **	**-0.1526*** **
	(0.0625)	**(0.2409)**	**(0.1023)**	**(0.1589)**	**(0.0577)**
Ln(School District Per-pupil Spending)(age 20-24) /.2	0.0474	-0.0444	0.0266	-0.0758	0.0395
	(0.1765)	(0.6956)	(0.1546)	(0.2019)	(0.0762)
Ln(School District Per-pupil Spending)(age 0-4) /.2	0.0588	-0.4241	0.2030	0.3261	-0.1010
	(0.1034)	(0.6042)	(0.1936)	(0.2799)	(0.0833)
Number of Individuals	8,284	8,284	8,284	8,284	8,284
Number of School Districts	788	788	788	788	788

Robust standard errors in parentheses (clustered at school district level)

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

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

Models: Results are based on 2SLS/IV models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends; controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs (community health centers, county expenditures on Head Start (at age 4), food stamps, Medicaid, AFDC, UI, Title-I (average during childhood years), timing of state-funded Kindergarten intro); controls for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, percent voted for Strom Thurmond in 1948 Presidential election*race (proxy for segregationist preferences)) each interacted with linear cohort trends; and controls for childhood family characteristics (parental income/education/occupation, mother's marital status at birth, birth weight, gender). The first-stage model include as predictors the years of exposure to school finance reform (for relevant ages 5-17; 20-24; 0-4) interacted with the respective school district's reform-induced change in school spending. There exists a significant first-stage for both poor and non-poor kids.

Table 5: *Evidence on Mechanisms (CCD Data)*

	1	2	3	5	6	7	8	9
	Spending by Category (1992-2010)			Student to Staff Ratios (1986 -2010 Data)				
	Capital	Instruction	Support Services	Students per School	Students per Teacher	Students per Councilor	Students per Aide	Students per Admin
Spending	**0.0862****	**0.559****	**0.405****					
	(0.00853)	**(0.0233)**	**(0.0154)**					
Ln(Per Pupil Spending)				**-117.91****	-4.775	**-138.36***	-59.146	**-167.78****
				(10.44)	(0.226)	**(63.51)**	(38.02)	**(30.219)**
Mean of Dependent Var	840.9	6224	3503	360.91	12.33	377.8	92.33	239.5

Event time trend + Year + District Effects in all models

Standard errors in parenthesis adjusted for clustering at the school district level

** p<0.01, * p<0.05, + p<0.1

Appendix A: *Matching PSID Individuals to their Childhood School Districts*

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

To construct demographic information on 1970-definition school districts, we compile census data from the tract, place, school district and county levels of aggregation for 1960, 1970, 1980 and 1990. We construct digital (GIS) maps of 1970 geography school districts using the 1969-1970 School District Geographic Reference File from the Census. This file indicates the fraction by population of each census tract that fell in each school district in the country. Those tracts split across school districts we allocated to the school district comprising the largest fraction of the tract's population. Using the resulting 1970 central school district digital maps, we allocate tracts in 1960, 1980 and 1990 to central school districts or suburbs based on the locations of their centroids. The 1970 definition central districts located in regions not tracted in 1970 all coincide with county geography which we use instead.

Appendix B: *Validating the PSID with CCD data*

To assuage concerns that our findings are driven by the sampling design of the PSID, we replicate our analysis for high school completion using the CCD data. We focus on dropout rates (grades 9-12) because this is the most reliable data that can be compared across time. Our data span the years 1991-92 to 2008-09. The dropout data from 1991-2001 and 2005-2008 come from the Common Core of Data Local Education Agency Universe Survey for all school districts in the United States. For years 2002-2004, dropout data in the CCD are only available for districts enrolling over 1,000 students. We also compiled a long panel of high school completion for years 1989 through 2010 by counting the number of graduates in a given year per 100 eighth graders four years before. This is a measure of the percentage of 8th graders who graduate.

To validate our PSID analysis, we compute district specific spending increases using the same method as that employed for the PSID data. Using the district specific effects to the graduation data and high school completion data from the CCD by year. We then estimate the effect of increased school spending due to reforms on the district dropout rate and our measure of the high school completion rate.

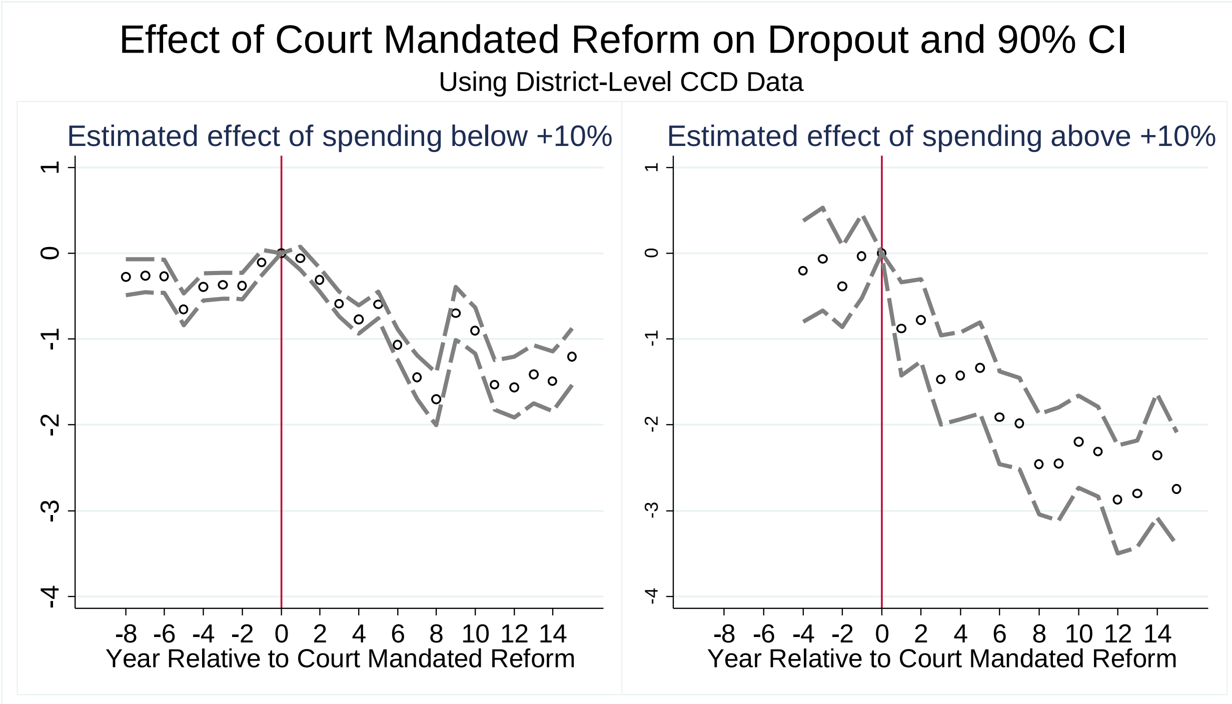
It is important to note that while one might expect the patterns in the CCD to be similar to those in the PSID, there are numerous reasons to expect some differences between the results presented in the PSID and the CCD samples. First, because these data are at the district level rather than the individual student level and because the CCD data are based on the school district attended (rather than the school district of birth) any effects might reflect changes in school composition that occur as a result of changes in per-pupil spending associated with reforms. Second, note that the CCD data span a different time period from the sample analyzed in the PSID. While the PSID analysis is based on individuals who were of school age between 1960 and 1992, the CCD data span individuals who would have been school age between 1980 and 2008. Finally, while we analyze the effect of exposure to school spending for an individual over their entre 12 years of public schooling in the PSID, in the CCD we analyze the effect of contemporaneous spending in a given year. In sum, there are numerous reasons to expect differences between the results presented in the PSID and the CCD samples. However, should the results be similar between the CCD data and the PSID sample, this robustness check would indicate that our findings are robust and generalizable.

First, we show the event-study graph for the passage of a court-mandated reform for districts that experienced a 10 percent increase in spending and all other districts separately. We run a regression of the dropout rate on a set of indicator variables denoting the number of years since a court-mandated reform (ranging from -8 to 14). To show the dynamic effect for districts that saw larger increases in spending versus other districts, we interact these dynamic event-time dummies with an indicator equal to one if the districts saw more than a 10 percent increase in per-pupil spending. The event study graphs are presented for the two district types in Figure B.1.

The event-study graphs are consistent with spending increases associated with reforms reducing the dropout rate. For both types of districts there is minimal evidence of trending in outcomes prior to reforms (note that the data can only support estimating effects for four years prior to reforms for districts with large spending increases). For both groups of districts there is a steady decrease in the dropout rate after reforms that plateau after about 12 years of exposure. Finally, in regards to dosage, the decrease in the dropout rate is largest in those district that experience larger increases in per-pupil spending. In sum, both the patterns in timing and intensity support a causal effect on dropout in the CCD sample.

To quantify these effects, we implement the same instrumental variables strategy from Section V on the CCD sample. We instrument for per-pupil spending with *SPENDj* interacted with the number of years of exposure (going from zero to 12), in a regression that includes district fixed effects, time fixed effects, and also a linear in event time. We present a model based on the level of spending in dollars and also models that are based on the natural log of per-pupil school spending. In all models there is a strong first stage (F-statistic>50). For both models the OLS regression yields no relationship between spending and dropout, while the 2SLS models show a statistically significant negative relationship between per-pupil spending and dropout. The 2SLS estimates indicate that increasing per-pupil spending by $1,000 will reduce dropout by about one percentage point and a doubling of per-pupil spending would reduce the dropout rate by 10.77 percentage points. Note that this estimate is not directly comparable to that from the PSID sample because this estimate is based on annual spending at the district level, not the cumulative effect of a sustained spending increase (experienced at the student level) for all 12 years of a student's life. Because we expect the later to be much larger, the results from the CCD data are consistent with those from the PSID. Looking at the number of graduates per 100 8th graders tells a similar story. The OLS results yield a negative relationship between school spending and graduation rates. This would suggest that increasing per-pupil spending actually reduces graduation rates. However, in 2SLS models that rely only on exogenous variation the results have the expected sign. The coefficient on spending ($1,000) is 0.725 and that on the log of spending is 6.96 (both is significant at the five percent level). These point estimates suggest that increasing per-pupil spending by $1,000 would increase the number of graduates per 100 eighth graders by 0.725, and that doubling per-pupil spending would increase the graduates per 100 eighth graders by about 7 students. As with the dropout outcome, this estimate is not directly comparable to that from the PSID sample because this estimate is based on annual spending at the district level, not the cumulative effect of a sustained spending increase (experienced at the student level) for all 12 years of a student's life. Because we expect the latter to be much larger, the results from the CCD data are consistent with those from the PSID.

Figure B.1: *Event Study Graph for Effects on Graduation in CCD Data*



<u>Data</u>: District level Common Core Data (1991-2010) matched with per-pupil spending data and estimated district specific school finance reform spending increases from Section V.

<u>Models</u>: Results are based on event-study models that include: school district fixed effects, and year effects The figure plots the estimated years of exposure to school finance reform interacted with the an indicator variable connoting whether the respective school district's reform-induced change in school spending is less than (Left) or more less than (Right) 10 percent.

Table B1: *Regression Estimates of the Effect of School Spending on District High School Dropout (CCD data)*

	1	2	3	4	5	6	7	8
	Dropouts Rate	Graduates per 100 8th graders	Dropouts Rate	Graduates per 100 8th graders	Dropouts Rate	Graduates per 100 8th graders	Dropouts Rate	Graduates per 100 8th graders
	OLS	OLS	OLS	OLS	2SLS	2SLS	2SLS	2SLS
Per Pupil Spending (1000)	0.00641	-0.126**			-1.046**	0.725*		
	[0.00617]	[0.0248]			[0.276]	[0.296]		
ln(Per Pupil Spending (1000))			-0.103	-1.519**			-10.77**	6.967**
			[0.0779]	[0.287]			[1.466]	[2.138]
Observations	111,065	91,169	111,065	91,169	80,394	91,112	80,394	91,112
R-squared	0.595	0.537	0.595	0.537	-0.745	0	-0.373	0.008
Mean of Dep Var	3.581	82.09	3.581	82.09	3.581	82.09	3.581	82.09

Robust standard errors in brackets

** p<0.01, * p<0.05, + p<0.1

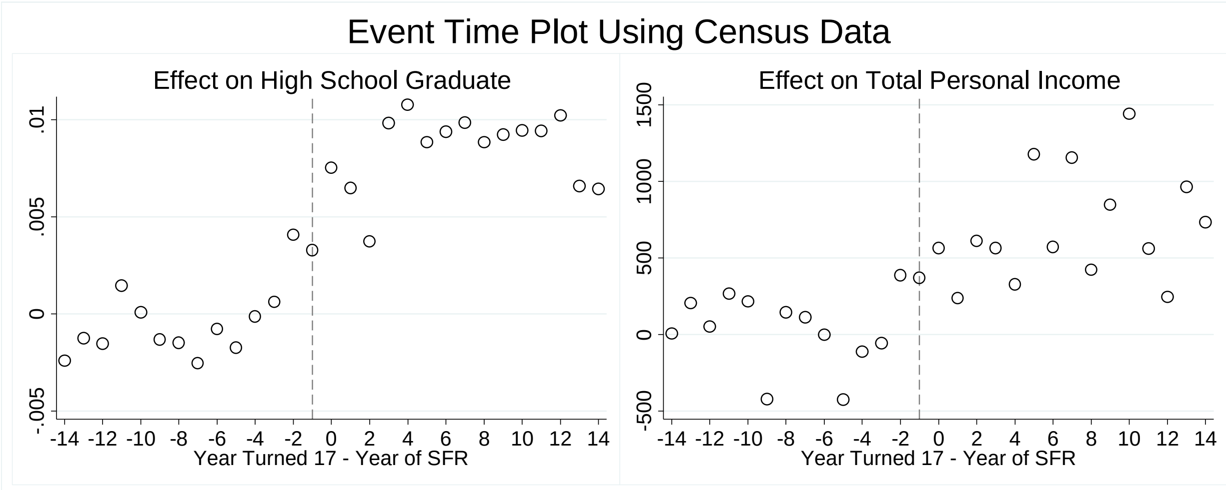
Appendix C: *Validating the PSID with Census IPUMS Data*

To provide further evidence of the generalizability of our results we also explore only the state level variation in per-pupil spending associated with the passage of court-mandated school finance reforms. Because the individual level Census data does not include the precise location of birth but does include the state of birth we can only analyze patterns in the data at the state level. If increases in per-pupil spending improved outcomes, given that court-mandated reforms are associated with increased school spending on average, one might expect that cohorts within states that are of school age after reform years should have better outcomes than individuals from the same state who already completed school at the passage of the reforms.

To interrogate this using the Census data, we look at all individuals born between 1955 and 1985 (the same dates of birth as our PSID sample) and between the ages of 25 and 45 (the same ages as our PSID sample). We then estimate an event-study regression on high school completion and annual personal income. That is, we regress these outcomes on a series of indicator variables denoting the year an individual turned 17 minus the year that a reform was passed in the individuals' state of birth. As such, individuals who were 17 at the years of reforms are at 0 years of exposure, while those in the negative range were 18 or older at the time the reforms were passed in their state of birth. We present the event time plots in figure A. If there is a causal effect, we should see minimal trending for the cohorts that were too old to be exposed and improving outcomes with increased years of exposure.

Consistent with a causal effect, for both high school graduation and income, outcomes are increasing in years of exposure and there is no pre-existing trend difference for the outcomes. To provide a test of statistical significance, we take the plotted dynamic treatment effect (i.e. the treatment effect for each cohort) and then fit a simple linear model in event time with an intercept at zero (i.e. cohort that was 18 at the time of the reform) and a differential post intervention time trend (a differential trend for cohorts that were 17 or younger at the time of reforms). The F-statistic on the post reform intercept and the post reform linear trend provides a test of whether there is a statistically significant structural break that occurs for the treated cohorts. For both outcomes, the two-sided p-value associated with the F-statistic for the joint significance of the post intercept and the post trend is smaller than 0.05 – indicating that relative to the pre-existing trend in outcomes, the improved high school completion and income observed for the exposed cohorts is larger than would be expected by random change (under the simple linear model).

Figure C.1: *Patterns in the Census Data*



Data: Individual Census IPUMS Data (1970, 1980, 1990, and 2000-2012) matched with the timing of court mandated reforms by date of birth and state of birth.

Models: Results are based on event-study models that include: state of birth fixed effects, year of birth effects, age and age squared, and gender and ethnicity interacted with a linear cohort trend. The figure plots the estimated years of exposure to school finance reform for high school graduation (Right) and total personal income (Left).

Appendix D: Data Sources for Part 1

Per Pupil Spending Data

Previous historical data on per pupil expenditures was only available in a readily usable format via the *Census of Governments: School System Finance (F-33) File* (U.S. Bureau of the Census, Department of Commerce). The Census of Governments previously was only conducted in years that end in a two or seven, so at the time when many important papers on SFRs were written, there were many years of missing data. In addition, until recently the earliest available F-33 data was for the year 1972. As a result, it was previously impossible to model per pupil spending and spending inequality annually over time, so many authors (e.g., MES, Card and Payne), operating under the Common Trends Assumption, assumed that trends in per pupil spending were linear. Due to these limitations, previous papers on school finance reforms were also unable to look at how the exact timing of reforms affected per pupil expenditure and spending inequality within a state.

Our data from 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*. The School District Finance Data FY 1967-91 is available annually from 1967 through 1991. It contains over one million individual local government records, including counties, cities, townships, special districts, and independent school districts. The INDFIN database frees the researcher from the arduous task of reconciling the many technical, classification, and other data-related changes that have occurred over the last 30 years. For example, this database includes corrected statistical weights that have been standardized across years, which had not been done previously. Furthermore, although most governments retain the ID number they are assigned originally, there are circumstances that result in a government's ID being changed. Since a major purpose of the INDFIN database is tracking government finances over time, it is critical that a government possess the same ID for all years (unless the ID change had a major structural cause). For example, All Alaska IDs were changed in the 1982 Census of Governments. In addition, new county incorporations, where governments in the new county area are re-assigned an ID based on the new county code (e.g., La Paz County, AZ), cause ID changes. Thus, if a government ID number was changed, the ID used in the database is its current GID number, including those preceding the cause of the change, so that the ID is standardized across years.

In addition to standardizing the data, the Census Bureau has corrected a number of errors in the INDFIN database that were previously in other sources of data. For example, for fiscal years 1974, 1975, 1976 and 1978 the school district enrollment data that had previously been released were useless (either missing or in error for many records). Thus, in August 2000, these missing enrollment data were replaced with those from the employment survey individual unit files. This enables us to more accurately compute per pupil expenditures for those years. In addition, source files before fiscal 1977 were in whole dollars rather than thousands. This set a limit on the largest value any field could hold. If a figure exceeded that amount, then the field contained a special "overflow" flag (999999999). Few governments exceeded the limit (Port Authority of NY and NJ and Los Angeles County, CA are two that did). For the INDFIN database, actual data were substituted for the overflow flag. Finally, in some cases the Census revised the original data in source files for the INDFIN database. In some cases, official revisions were never applied to the data files. Others resulted from the different environment and operating practices under which source files were created. Finally, some extreme outliers were identified and corrected (e.g., a keying error for a small government that ballooned its data).

The Common Core of Data (CCD) School District Finance Survey (F-33) consists of data submitted annually to the National Center for Education Statistics (NCES) by state education agencies (SEAs) in the 50 states and the District of Columbia. The purpose of the survey is to provide finance data for all local education agencies (LEAs) that provide free public elementary and secondary education in the United States. 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. The Census of Governments, which was recorded every five years until 1992, records administrative data on school spending for every district in the United States. After 1992, the Public Elementary-Secondary Education Finances data were recorded annually with data available until 2010. We combine these data sources to construct a long panel of annual per pupil spending for each school district in the United States between 1967 and 2010.

Per-pupil spending data from before 1992 is missing for Alaska, Hawaii, Maryland, North Carolina, Virginia, and Washington, D.C. Per-pupil spending data from 1968 and 1969 is missing for all states. Spending data 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 of missing per pupil expenditure data, we filled in this data using linear interpolation.

Data on School Finance Reforms

Due to great interest on the topic, the timing of school finance reforms (SFRs) has been collected in various places. Data on the exact timing and type of court ordered and legislative SFRs was obtained from Public School Finance Programs of the Unites States and Canada (PSFP), National Access Network's state by state school finance litigation map (2011), from Murray, Evans, and Schwab (1998), Hoxby (2001), Card and Payne (2002), Hightower et al (2010), and Baicker and Gordon (2004). The most accurate information on school finance laws can be derived from the PSFP, which provides basic information and references to the legislation and court cases challenging them (Hoxby, 2001). In most cases, data from these sources are consistent with each other. Where there are discrepancies we often defer to PSFP, but also consulted LexisNexis and state court and legislation records.

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

Using a policy survey conducted during the 2008-2009 school year, a recent study by Hightower et al (2010) provides a description of state finance policies and practices. This study was used to verify whether there had been any changes to state funding formulas between 1998 and 2009. We only collected information on the first five court cases per state in which the state found the school funding system unconstitutional. There were only three states with five or more court cases overruling the funding system (New Hampshire, New Jersey, and Texas). In addition, we only collected information on the first four court cases per state in which states upheld the school funding system. There were only four states with four or more court cases in which the school funding system was upheld (Illinois, New York, Oregon, and Pennsylvania).

Information on whether or not a state funding formula had a MFP, flat grant formula, variable matching grant scheme, recapture provision, spending limit, power equalization scheme, local-effort equalization scheme, or full state funding came from *PSFP* (1998) and was verified using Card and Payne (2002) and Hightower et al (2010). We defined MFPs, flat grant formulas, and variable matching grant schemes in the same way as Card and Payne did in their 2002 study. We defined power equalization, local-effort equalization, and full state funding in the same way as the EPE study (Hightower, Mitani and Swanson, 2010). Each element of a state funding formula was coded as a dichotomous variable. For example, MFP is a dichotomous variable that is equal to one in the year and all subsequent years in which a state's finance system had a MFP plan in place. MFP was set equal to zero in all years prior to the state's funding system having a MFP in place, or if a state never implemented a MFP. Information on adequacy and equity reforms came from Berry (2005) and Springer, Liu and Guthrie (2009)'s Table 1. Following Springer, Lui and Guthrie (2009), we define two dichotomous variables for equity and adequacy reforms. *Adequacy* is a dichotomous variable set to one in the year, and all subsequent years, in which a state's finance system was overturned on adequacy grounds. *Adequacy* is set to zero in all years prior to a school funding mechanism being overturned, or if a state's finance system was never ruled unconstitutional. Similarly, *equity* is a dichotomous variable that is set to one in the year, and in all subsequent years, in which a state's finance system was ruled unconstitutional on equity grounds. *Equity* was set to zero in all years prior to the state funding mechanism being declared unconstitutional, or if a state's funding mechanism was never overturned.

Information on the timing of tax limits came from Downes and Figlio (1998). In addition, information on the foundation property tax rate and the maximum/minimum inverted tax price of marginal local spending was obtained from Hoxby (2001) and defined in the same way. Thus, we defined a variable, limit2, which equals one if there is a zero tax price according to Hoxby, a recapture provision, or a spending cap. We defined another variable, g1taxprice, as equal to one if the inverted tax price was greater than one (which should promote spending).

Table D.1: Supreme Court Rulings on the Constitutionality of School Finance Systems, SFR Legislation, and State Finance Plans, 1967-2010

State	Case Name / Legislation (without a court case)	Year	Constitutionality of finance system	Type of Reform	Funding Formula before Reform	Funding Formula after Reform
Alabama	Alabama Coalition for Equity v. Hunt; Harr v. Hunt	1993	Overturned	Adequacy	MFP	MFP + EP
Alaska	Kasayulie v. Alaska	1999	Overturned	Adequacy	MFP	MFP + EP
Arizona	Shofstall v. Hollins	1973	Upheld		FG	FG
	Legislation	1980		Legislative	FG	MFP + EP + LE + SL
	Roosevelt v. Bishop	1994	Overturned	Adequacy	MFP + EP + LE + SL	MFP + EP + LE + SL
	Roosevelt v. Bishop	1997	Overturned	Adequacy	MFP + EP + LE + SL	MFP + EP + LE + SL
	Roosevelt v. Bishop	1998	Overturned	Adequacy	MFP + EP + LE + SL	MFP + EP + LE + SL
	Flores v. Arizona	2007	Overturned	Adequacy	MFP + EP + LE + SL	MFP + EP + LE + SL
Arkansas	Dupree v. Alma School District No. 30	1983	Overturned	Equity	FG + SL	MFP + SL
	Lake View v. Arkansas	1994	Overturned	Equity	MFP + SL	MFP + EP + SL
	Lake View School District, No. 25 v. Huckabee	2002	Overturned	Adequacy	MFP + EP + SL	MFP + EP + SL
	Lake View School District, No. 25 v. Huckabee	2005	Overturned	Adequacy	MFP + EP + SL	MFP + EP + SL
California	Serrano v. Priest	1971	Overturned	Equity		FG + MFP
	Serrano v. Priest	1977	Overturned	Equity	FG + MFP	FG + MFP + SL
	Serrano v. Priest	1986	Upheld		FG + MFP + SL	FG + MFP + SL
	Proposition 98	1988		Legislative	FG + MFP + SL	FG + MFP + SL
Colorado	Lujan v. Colorado State Board of Education	1982	Upheld		FG + EP	FG + EP
	Public School Finance Act of 1994	1994		Legislative	FG + EP	EP + MFP
Connecticut	Horton v. Meskill	1978	Overturned	Equity	FG	EP
	Horton v. Meskill	1982	Overturned	Equity	EP	EP

	Horton v. Meskill	1985	Upheld		EP	EP + MFP
	Sheff v. O'Neill	1995	Overturned	Adequacy	EP + MFP	EP + MFP
	Coalition for Justice in Education Funding, Inc v. Rell	2010	Overturned	Adequacy	EP + MFP	EP + MFP
Delaware	No litigation or legislative reform				EP + FG	EP + FG
Florida	Florida Education Finance Program	1973		Legislative	MFP + FG	MFP + EP + LE
	Coalition for Adequacy and Fairness in School Funding v. Chiles	1996	Upheld		MFP + EP + LE	MFP + EP + LE
	School Board of Miami-Dade County v. King	2006	Upheld		MFP + EP + LE	MFP + EP + LE
	Schroeder et al v. Palm Beach Co School Board et al	2009	Upheld		MFP + EP + LE	MFP + EP + LE
Georgia	McDaniel v. Thomas	1981	Upheld		MFP	MFP
	Legislation	1986		Legislative	MFP + EP + LE	MFP + EP + LE
Hawaii	No litigation or legislative reform				FS	FS
Idaho	Thompson v. Engleking	1975	Upheld		MFP	MFP
	Frazier et. al. v. Idaho	1990	Upheld		MFP	MFP
	Idaho Schools for Equal Educational Opportunity v. State	1998	Overturned	Adequacy	MFP	MFP
	Idaho Schools for Equal Educational Opportunity v. State	2005	Overturned		MFP	FS
Illinois	Blase v. Illinois	1973	Upheld			MFP + EP + FG
	Legislation	1973		Legislative		MFP + EP + FG
	EPople v. Adams	1976	Upheld		MFP + EP + FG	MFP + EP + FG
	Legislation	1980		Legislative	MFP + EP + FG	MFP + EP + FG
	The Committee for Educational Rights v. Edgar	1996	Upheld		MFP + EP + FG	MFP + EP + FG
	Public Act 90-548	1999		Legislative	MFP + EP + FG	MFP + EP + FG
Indiana	Legislation	1993		Legislative	MFP + FG	MFP + EP + FG
Iowa	Legislation	1972		Legislative		MFP + FG + SL
	Code Chapter 257	1992		Legislative	MFP + FG + SL	MFP + SL
Kansas	Knowles v. State Board of Education	1972	Overturned	Equity		EP

	Knowles v. Kansas	1981	Upheld		EP	EP
	School District Finance and Quality EPrformance Act	1992		Legislative	EP	MFP + EP + LE + SL
	Unified School District No. 299 v. Kansas	1994	Upheld		MFP + EP + LE + SL	MFP + EP + LE + SL
	Montoy v. State	2005	Overturned	Adequacy	MFP + EP + LE + SL	MFP + EP + LE + SL
Kentucky	Rose v. The Council for Better Education, Inc.	1989	Overturned	Adequacy	MFP + FG	MFP + FG + LE
	Young v. Williams	2007	Upheld		MFP + FG + LE	MFP + FG + LE
Louisiana		1976	Upheld		MFP	MFP
	School Board v. Louisiana	1987	Upheld		MFP	MFP
	Legislation	1992		Legislative	MFP	MFP + EP
	Charlet v. Legislature of State of Louisiana	1998	Upheld		MFP + EP	MFP + EP
Maine	Legislation	1978		Legislative	MFP	MFP
	The School Finance Act	1985		Legislative	MFP	MFP + EP
	M.S.A.D. #1 v. Leo Martin	1995	Upheld		MFP + EP	MFP + EP
	The School Finance Act - LD958	1996		Legislative	MFP + EP	MFP + EP
	Essential Programs and Services Funding Act	2004		Legislative	MFP + EP	MFP + EP + LE
Maryland		1972	Upheld		MFP	MFP
	Somerset County Board of Education et al. v. fIornbeck et al.	1983	Upheld		MFP	MFP
	Legislation	1987		Legislative	MFP	MFP + FG + LE
	Bradford v. Maryland State Board of Education	1996	Upheld		MFP + FG + LE	MFP + FG + LE
	Bradford v. Maryland State Board of Education	2002	Upheld		MFP + FG + LE	MFP + FG + LE + EP
	Thornton Commission's new finance system	2002		Legislative	MFP + FG + LE	MFP + FG + LE + EP
	Bradford v. Maryland State Board of Education	2005	Overturned	Adequacy	MFP + FG + LE + EP	MFP + FG + LE + EP

Massachusetts	Collins-Boverini plan	1978		Legislative	EP + FG	EP
	Legislation	1985		Legislative	EP	EP
	Mc Duffy v. Secretary of the Executive Office of Education	1993	Overturned		EP	MFP
	Hancock vs. Commissioner of Education	2005	Upheld		MFP	MFP
Michigan	Milliken v. Green	1973	Upheld			MFP + FG + EP
	Legislation	1973		Legislative		MFP + FG + EP
	East Jackson Public Schools v. State of Michigan	1984	Upheld		MFP + FG + EP	MFP + FG + EP
	PA 145	1994		Legislative	MFP + FG + EP	EP + FG + SL
	Durant vs State of Michigan	1997	Overturned	Adequacy	EP + FG + SL	EP + FG + SL
Minnesota		1971	Upheld			
	Legislation	1973		Legislative		MFP + FG
	General Education Revenue Program	1989		Legislative	MFP + FG	MFP
	Skeen v. Minnesota	1993	Upheld		MFP	MFP
Mississippi	Mississippi Adequate Education Program of 1997	1997		Legislative	MFP + FG	MFP + LE
Missouri	Legislation	1977		Legislative	MFP	MFP + FG + EP
	Committee for Educational Equality v. Missouri	1993	Overturned	Adequacy	MFP + FG + EP	MFP + FG + EP + LE
Montana	Helena Elementary School District No. 1 v. State of Montana	1989	Overturned	Equity	MFP	MFP + EP
	Montana Rural Ed. Association v. Montana	1993	Overturned	Equity	MFP + EP	MFP + EP + SL
	Columbia Falls Public Schools v. State	2005	Overturned	Adequacy	MFP + EP + SL	MFP + EP + SL
	Montana Quality Education Coalition v Montana	2008	Overturned	Adequacy	MFP + EP + SL	MFP + EP + SL
Nebraska	School Foundation and Equalization Act	1967		Legislative		MFP + FG
	LB 1059 (TEEOSA)	1990		Legislative	MFP	MFP + SL
	Gould v. Orr	1993	Upheld		MFP + SL	MFP + SL
	LB 806	1997		Legislative	MFP + SL	MFP + EP + SL

	Nebraska Coalition for Educational Equity and Adequacy v. Heineman	2007	Upheld		MFP + EP + SL	MFP + EP + SL
Nevada	No litigation or legislative reform				MFP	MFP
New Hampshire	Legislation	1985		Legislative	MFP + FG	MFP
	Claremont New Hampshire v. Gregg	1993	Overturned	Adequacy	MFP	MFP
	Claremont v. Governor	1997	Overturned	Adequacy	MFP	MFP
	Claremont v. Governor	1999	Overturned	Adequacy	MFP	MFP + EP
	Claremont v. Governor	2002	Overturned	Adequacy	MFP + EP	MFP + EP + SL
	Londonderry School District v.New Hampshire	2006	Overturned	Adequacy	MFP + EP + SL	MFP + EP + SL
New Jersey	Robinson v. Cahill	1973	Overturned	Equity	EP	FG + EP
	Robinson v. Cahill	1976	Overturned	Equity	FG + EP	FG + EP
	Abbott v. Burke	1990	Overturned	Adequacy	EP	MFP + EP
	Abbott v. Burke	1991	Overturned	Adequacy	MFP + EP	MFP + EP
	Abbott v. Burke	1994	Overturned	Adequacy	MFP + EP	MFP + EP
New Mexico	Public School Finance Act	1974		Legislative	MFP + FG	MFP
	Zuni School District v. State	1998	Overturned	Equity	MFP	MFP + EP
New York		1972	Upheld		MFP + FG	MFP + FG
	Board of Education, Levittown v. Nyquist	1982	Upheld		MFP + FG	MFP + FG
	Board of Education City School District, Rochester v. Nyquist	1987	Upheld		MFP + FG	FG + EP
	REFIT v. State of New York	1993	Upheld		FG + EP	FG + EP
	CFE v. State	2003	Overturned	Adequacy	FG + EP	FG + EP
	CFE v. State	2006	Overturned	Adequacy	FG + EP	MFP + EP
North Carolina	Britt v. State Board	1987	Upheld		FG	FG + EP
	Leandro v. State	1994	Upheld		FG + EP	FG + EP
	Leandro v. State	1997	Overturned	Adequacy	FG + EP	FG + EP
	Leandro v. State	2004	Overturned	Adequacy	FG + EP	FG + EP

North Dakota	Bismark Public Schools v. North Dakota	1993	Upheld		MFP	MFP
	Legislation	2007		Legislative	MFP	EP + LE
Ohio	Legislation	1975		Legislative		FG + EP
	Board of Education of the City School District of Cincinnati v. Walter	1979	Upheld		FG + EP	MFP
	Howard v. Walter	1991	Upheld		MFP	MFP
	DeRolph v. Ohio	1997	Overturned	Adequacy	MFP	MFP
	DeRolph v. Ohio	2000	Overturned	Adequacy	MFP	MFP
	DeRolph v. Ohio	2002	Overturned	Adequacy	MFP	MFP + EP
	Legislation	2010			MFP + EP	MFP + EP
Oklahoma	Legislation	1981		Legislative	MFP + FG + EP	MFP + EP
	Fair School Finance Council of Okla. v. Oklahoma	1987	Upheld		MFP + EP	MFP + EP
Oregon	Olsen v. Oregon	1976	Upheld		MFP + FG	MFP + FG
	Legislation	1978		Legislative	MFP + FG	MFP + SL
	Coalition for Education Equity v. Oregon	1991	Upheld		MFP + SL	MFP + EP + SL
	Legislation	1991	Upheld	Legislative	MFP + SL	MFP + EP + SL
	Withers v. State	1995	Upheld		MFP + EP + SL	MFP + EP + SL
	Withers v. State	1999	Upheld		MFP + EP + SL	MFP + EP + SL
	EPndleton School District v. State of Oregon	2009	Overturned	Adequacy	MFP + EP + SL	MFP + EP + SL
EPnnsylvani a		1975	Upheld		EP + FG	EP + FG
	Dansen v. Casey	1979	Upheld		EP + FG	EP
	Dansen v. Casey	1987	Upheld		EP	EP
	EPnnsylvania Association of Rural and Small Schools v. Case	1991	Upheld		EP	EP
	Legislation	1991		Legislative	EP	EP
	Legislation	2008		Legislative	EP	MFP + EP + LE
Rhode Island	Legislation	1985		Legislative	EP + FG	EP

	City of Pawtucket v. Sundlun	1995	Upheld		EP	EP
	Legislation	1996		Legislative	EP	EP
South Carolina	Education Finance Act of 1977	1977		Legislative	FG	MFP + LE
	Education Improvement Act	1984		Legislative	MFP + LE	MFP + LE
	Richland County v. Campbell	1988	Upheld		MFP + LE	MFP + LE
	Lee County v. South Carolina	1993	Upheld		MFP + LE	MFP + LE
	Abbeville County School District v. State	2005	Overturned	Adequacy	MFP + LE	MFP + LE
Tennessee	Legislation	1977		Legislative	MFP	MFP
	Tennessee Small School Systems v. McWheter	1993	Overturned	Equity	MFP	MFP
	Tennessee Small School Systems v. McWheter	1995	Overturned	Adequacy	MFP	MFP
	Tennessee Small School Systems v. McWheter	2002	Overturned	Adequacy	MFP	MFP
Texas		1973	Upheld		MFP + FG	MFP + FG
	Legislation	1986		Legislative	MFP + FG	MFP + FG
	Edgewood IndeEPndent School District v. Kirby	1989	Overturned	Equity	MFP + FG	MFP + FG
	Edgewood v. Kirby	1991	Overturned	Equity	MFP + FG	MFP + FG
	Carrollton-Farrners v. Edgewood	1992	Overturned	Equity	MFP + FG	MFP + EP + LE + SL
	Edgewood v. Meno et al. and Bexar Co. Education District et al.	1995	Upheld		MFP + EP + LE + SL	MFP + EP + LE + SL
	West Orange-Cove Consolidated ISD v. Nelson	2004	Overturned	Adequacy	MFP + EP + LE + SL	MFP + EP + LE + SL
Utah	Legislation	1973		Legislative	MFP	MFP
	Capital Outlay Foundation Program	1993		Legislative	MFP	MFP + EP
Vermont	Legislation	1969		Legislative	FG + EP	FG + EP
	Legislation	1982		Legislative	FG + EP	FG + EP
	Legislation	1987		Legislative	FG + EP	FG + MFP
	Lamoille County v. State of Vermont	1994	Upheld		FG + MFP	FG + MFP

	Brigham v. State	1997	Overturned	Equity	FG + MFP	MFP + EP + SL
	Act 68	2003		Legislative	EP + SL	FS + SL
Virginia	Standards of Quality legislation	1972		Legislative	MFP + FG	MFP + FG
	Legislation	1975		Legislative	MFP + FG	MFP + FG
	Revision of SOQ	1989		Legislative	MFP + FG	MFP + EP + LE
	Scott v. Virginia	1994	Upheld		MFP + EP + LE	MFP + EP + LE
Washington	Northshore v. Kinnear	1974	Upheld		MFP	MFP
	Seattle School District No. 1 of King County v. State	1977	Overturned	Adequacy	MFP	FS + EP
	Seattle II	1991	Overturned	Adequacy	FS + EP + LE	FS + EP + LE + SL
	Federal Way School District v. State of Washington	2007	Overturned	Equity	FS + EP + LE + SL	FS + EP + LE + SL
West Virginia	Pauley v. Kelly	1979	Overturned	Adequacy	MFP	MFP
	Pauley v. Bailey	1984	Overturned	Equity	MFP	MFP + LE
	Pauley v. Gainer	1995	Overturned	Adequacy	MFP + LE	MFP + LE
	WV code §18-2E-5	1998		Legislative	MFP + LE	MFP + LE
Wisconsin	Legislation	1973		Legislative		EP
	Buse v. Smith	1976	Overturned	Equity	EP	EP
	Kukor v. Grover	1989	Upheld		EP	EP + LE
	Vincent v. Voight	2000	Upheld		EP + LE	EP + LE
Wyoming	Washakie v. Herschler	1980	Overturned	Equity	MFP	MFP
	Campbell v. State	1995	Overturned	Adequacy	MFP	MFP + EP + SL
	Campbell II	2001	Overturned	Adequacy	MFP + EP + SL	MFP + EP + SL

Notes: The state funding formulas may include: flat grants (FG), minimum foundation plans (MFP), equalizations plans (EP), local effort equalizations (LE), spending limits (SL), and full state funding (FS).