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The Effects of School Spending on Educational and Economic Outcomes: Evidence from School Finance Reforms

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Abstract

Since Coleman (1966), many have questioned whether school spending affects student outcomes. The school finance reforms that began in the early 1970s and accelerated in the 1980s caused some of the most dramatic changes in the structure of K–12 education spending in US history. To study the effect of these school-finance-reform-induced changes in school spending on long-run adult outcomes, the researchers link school spending and school finance reform data to detailed, nationally representative data on children born between 1955 and 1985 and followed through 2011. They use the timing of the passage of court-mandated reforms, and their associated type of funding formula change, as an exogenous shifter of school spending and they compare the adult outcomes of cohorts that were differentially exposed to school finance reforms, depending on place and year of birth. Event-study and instrumental variable models reveal that a 10 percent increase in per-pupil spending each year for all 12 years of public school leads to 0.27 more completed years of education, 7.25 percent higher wages, and a 3.67 percentage-point reduction in the annual incidence of adult poverty; effects are much more pronounced for children from low-income families. Exogenous spending increases were associated with sizable improvements in measured school quality, including reductions in student-to-teacher ratios, increases in teacher salaries, and longer school years.

I. INTRODUCTION

US K-12 public schools vary significantly in quality, as has been documented in a broad range of studies.[1] These differences are often cited as a major contributor to achievement gaps by parental socioeconomic status and race/ethnicity. Moreover, education is one of the largest single components of government spending, amassing 7.3% of GDP across federal, state, and local expenditures (OECD 2013 report). Accordingly, understanding the role (if any) of school spending, and the roles of school resource inputs, as determinants of school quality and student outcomes are of first-order significance. In this paper we present fresh evidence on the enduring question of whether, how, and why school spending affects student outcomes. The objectives of this paper are threefold: we aim to (1) isolate exogenous changes in district per-pupil spending that are unrelated to unobserved determinants of student outcomes, (2) document the relationship between exogenous changes in school spending and the adult outcomes of affected children, and (3) shed light on underlying mechanisms by documenting the changes in observable school inputs through which any education spending effects might emerge.

Since Coleman (1966), researchers have questioned whether increased school spending actually improves student outcomes. The report—the first national, large-scale quantitative analysis of the role of schools—employed data from a cross-section of students in 1965-66 and showed that variation in school resources, as measured by per-pupil spending and student-to-teacher ratios, was unrelated to variation in student achievement on standardized tests. Since then, how school spending affects student academic performance has been extensively studied. Hanushek (1986) reviews this recent literature and his conclusions echo those of Coleman (1966).

Given that adequate school funding is a necessary condition for the provision of a quality education, the lack of an observed positive relationship between school spending and student outcomes is surprising.[2] However, there are two key attributes of previous national studies that might limit the ability to draw firm conclusions from their results. The first limitation is that test scores are imperfect measures of learning and may be weakly linked to adult earnings and success in life. Indeed, recent studies have documented that effects on long-run outcomes may go

[1] For example, adult earnings has been found to vary significantly by high school attended even after controlling for childhood family background characteristics (Betts, 1995; Grogger, 1996).

[2] Potential explanations that have been put forth to explain why there is no link found between school spending and student outcomes for cohorts educated since the 1950s include: (a) diminished returns to school spending as levels of spending have increased over time (relative to earlier cohorts); (b) deterioration of the quality of the teaching workforce; (c) increased waste and ineffective allocation of resources to school inputs (see Betts, 1996).

undetected by test scores (e.g. Heckman, Pinto, & Savelyev, 2014; Deming 2009; Jackson, 2012; Chetty, Friedman and Rockoff, 2013; Ludwig and Miller, 2007). We address the limitations of focusing on test scores as our main outcome by focusing on the effect of school spending on long-run outcomes such as educational attainment and earnings.

The second limitation of previous work is that most national studies correlate actualized changes in school spending with changes in student outcomes. This is unlikely to yield real causal relationships because many of the changes to how schools have been funded since the 1960s would lead to biases that weaken the observed association between *changes* in school resources and student outcomes. For example, with the passage of the Elementary and Secondary Education Act of 1965, school districts with a high percentage of low-income students receive additional funding, and the regulations give priority to low-achieving schools. Such policies likely generate a mechanical negative relationship between school spending and student achievement that would negatively bias the observed relationship between school spending and student outcomes.[3] Additionally, because localities face tradeoffs when allocating finite resources, positive effects of endogenous increases in school spending could be offset by reductions in other kinds of potentially productive spending. We overcome the biases inherent in relying on potentially endogenous observational changes in school resources by documenting the relationship between exogenous quasi-experimental shocks to school spending and long run adult outcomes.

As documented in Murray, Evans, and Schwab (1998), Hoxby (2001), Card and Payne (2002) and Jackson, Johnson, and Persico (2014a), 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 US history. To isolate plausibly exogenous changes in school resources we investigate the effects of changes in per-pupil spending, due only to the passage of court-mandated school finance reforms, on long-run educational and economic outcomes. We link detailed data on school reforms and school spending to longitudinal data on a nationally-representative sample of over 15,000 children born between 1955 and 1985 and followed into adulthood in the Panel Study of Income Dynamics (PSID). These birth cohorts straddle the period in which SFR implementation accelerated, and thus were differentially exposed

[3] Similarly, the wave of school finance reforms that started in 1972 changed how public schools were funded in 45 states (Jackson, Johnson, and Persico 2014a). School finance reform-induced changes in school spending are largely comprised of additional school funding that is allocated by compensatory formulas, whereby school resources are disproportionately targeted at lower-income districts and least-able students (lower-performing students).

to reform-induced changes in school spending depending on place and year of birth.

We use both the timing of passage of court-mandated reforms and the type of funding formula introduced by that reform as exogenous shifters of school spending. Specifically, for each district we predict the spending change that the district would experience after the passage of court-mandated school finance reform based on the experiences of similar districts facing similar reforms in different states. We then see if "treated" cohorts (those young enough to have been in school during or after the reforms were passed) have better outcomes relative to "untreated" cohorts (children who were too old to be affected by reforms at the time of passage) in districts predicted (based on the experiences of similar districts in other states) to experience larger reform-induced spending increases. Correlating outcomes with only the predicted reform-induced variation in spending, rather than all actualized spending, removes the confounding influence of unobserved factors that may both determine actualized school spending and also affect student outcomes.

In related work, Card and Payne (2002) find that court-mandated SFRs reduce SAT-score gaps between low- and high-income students. However, Hoxby (2001) finds mixed evidence on the effect of increased spending due to SFRs on high-school dropout rates, and Downes and Figlio (1998) find no significant changes in the distribution of test scores.[4] Looking at individual states, Guryan (2001), Papke (2005) and Roy (2011) find that reforms improved test scores in low-income districts in Massachusetts and Michigan.[5] Overall, the evidence on the effects of SFRs on academic outcomes is mixed, and the effects on long run economic outcomes is unknown.

Our event-study and instrumental variables models reveal that increased per-pupil spending, induced by SFRs, increased the high school graduation rates and educational attainment for low-income children, and thereby narrowed adult socioeconomic attainment differences between those raised in low- and high-income families. While we find small effects for children from affluent families, for low-income children, a 10 percent increase in per-pupil spending each year for all 12 years of public school is associated with 0.43 additional years of completed education, 9.5 percent higher earnings, and a 6.8 percentage-point reduction in the annual incidence of adult poverty. In fact, a 25 percent increase over all school age years is sufficiently large to eliminate the attainment gaps between children from low- and high-income families. We

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

[5] In a recent working paper, Hyman (2014) analyzes the same Michigan reform and finds that it increased college going for non-poor children in low income districts.

present several patterns to support a causal interpretation, and show our results are robust to the inclusion of controls for many coincident policies (e.g., desegregation and safety-net programs).

To shed light on mechanisms we document that reform-induced school spending increases were associated with a reduction in the student-to-teacher ratio, longer school years, and increased teacher salaries–suggesting that reductions in class size, increases in instructional time and improvements in teacher quality improve student outcomes. These findings stand in contrast to studies finding little effect of measured school inputs on student outcomes for cohorts educated after 1950 (Betts, 1995; Betts, 1996; Hanushek, 2001) and are in line with studies that find that school inputs matter for older cohorts educated between 1920 and 1950 (Card and Krueger, 1992; Loeb and Bound, 1996) and studies on recently educated cohorts using randomized and quasi-random variation in school inputs (e.g. Chetty *et al*, 2013; Fredriksson *et al*, 2012).

We reconcile our results with the existing literature by showing that actualized increases in school spending are associated with disadvantaged family characteristics and unrelated to improvements in observable measures of school quality, while exogenous spending increases due to reforms is uncorrelated with family background and is strongly associated with better school inputs. Accordingly, our findings may differ from existing studies for two distinct reasons: (a) using observational variation in spending might confound neighborhood disadvantage with increased spending, and (b) districts might allocate endogenously raised funds toward differentially productive school inputs than they do large unexpected exogenous spending increases. Accordingly, our findings provide compelling evidence that money does matter and that better school resources can meaningfully improve the long-run outcomes of recently educated children. At the same time, our results also suggest that money alone might not improve outcomes because the effect of any spending increases will depend on exactly how funds are spent.

The remainder of the paper is organized as follows. Section II describes the school finance reforms and explains how we use these reforms to form our exogenous instrument for school spending. Section III presents the data used. Section IV outlines our empirical strategy for identifying the effects of reform-induced changes in spending on long-run outcomes. Section V presents both event-study and instrumental variables regression results for the effect of school spending on longer-run outcomes. Section VI shows how specific school resource inputs change as a result of reform-induced spending increases, and Section VII presents our conclusions.

II. A Discussion of School Reforms and Constructing the Instrument

We aim to document the relationship between long-run outcomes and exogenous variation in school spending experienced during one's school-age years. To this aim, we isolate exogenous variation in school spending caused by the passage of court-ordered school finance reforms. In most states, prior to the 1970s, most resources spent on K–12 schooling was raised at the local level, through local property taxes (Howell and Miller, 1997; Hoxby, 1996). Because the local property tax base is typically higher in areas with higher home values, and there are persistently high levels of residential segregation by socioeconomic status, heavy reliance on local financing contributed to affluent districts' ability to spend more per student. In response to large within-state differences in per-pupil spending across wealthy/high-income and poor districts, state supreme courts overturned school finance systems in 28 states between 1971 and 2010, and many states implemented legislative reforms that led to important changes in public education funding.[6] Appendix A presents the timing of the court-ordered reforms in each state.

Most SFRs changed the parameters of spending formulas to reduce inequality in school spending by reducing the strength of the relationship between the level of educational spending and the wealth of the district (or at times, the income level of the district). The design of state aid formulas to meet these goals, however, was far from uniform. This variation across states in how they sought to achieve a more equitable distribution of school spending across districts plays a key role in how we isolate exogenous variation in school spending across districts.

a. Isolating Exogenous Variation in School Spending

To document the causal relationship between long-run outcomes and school spending, we isolate variation in spending that can only be attributed to the plausibly exogenous passage of court-ordered SFRs. The basic idea is as follows: If certain kinds of reforms have systematic and predictable effects on certain kinds of school districts (based on observable pre-reform characteristics), then one can predict district-level changes in school spending that are unrelated to potentially confounding changes in unobserved district-level determinants of school spending and student outcomes (e.g., demand for education, demographic characteristics, the local economy). With this clean "predicted" variation in spending, one can then test whether exposed cohorts have better outcomes relative to unexposed cohorts in those districts that are predicted (based on pre-reform characteristics) to experience larger reform-induced spending increases. By

[6] See Jackson, Johnson, and Persico (2014) for an in depth discussion of school finance reforms.

correlating outcomes with only the reform-induced variation in school spending (rather than all variation in spending), one removes the confounding influence of unobserved factors that might both determine actualized school spending and also affect student outcomes.

To document the predictable effects of court-ordered SFRs on school districts, we present a descriptive analysis of the effect of court-ordered reforms on district-level per-pupil spending for districts that vary in their median income levels in 1962 (prior to reforms). For this purpose we employ data on district and state funding from several sources. The Census of Governments has been conducted every five years since 1972 and records administrative data on school spending for every school district in the US. The Historical Database on Individual Government Finances (INDFIN), contains school district finance data annually for a sub-sample of districts from 1967, and 1970 through 1991. After 1991, the CCD School District Finance Survey (F-33) includes data on school spending for every school district in the United States. We combine these data to form a panel of per-pupil spending for US school districts in 1967 and annually from 1970 through 2010.[7] We link school district boundaries to counties and pull county-level median income data from the 1962 Census of Governments and to a database of reforms between 1972 and 2010.[8]

b. Illustrating the Effect of Reforms on the Distribution of Spending

Our proposed shifter of school spending is the passage of court-mandated SFRs. To document the effect of these reforms on the level and distribution of per-pupil spending across district income levels, we employ an event-study Difference-in-Differences (DiD) methodology. Using district-by-year data, we compare spending in districts with low or high median incomes in 1962 *before* implementation of a reform to the spending in the same district *after* implementation. To account for underlying national trends and shocks, we use the difference in spending for low- or high-income districts across the same years in states that did not implement reforms as a comparison.[9] We implement this strategy within a regression framework by estimating [1].

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7 Additional details on the data and the coverage of districts in these data are in Appendix B. We also show that our results are robust to any biases that could be driven by incomplete coverage of districts across years.

8 A detailed description of how this database of reforms was compiled is in Appendix C.

9 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 and region-specific 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.

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

In equation [1], $\$_{dst}$ is per-pupil spending in district d in state s in year t (in real 2012 dollars), Q_d is an indicator for the percentile group of the district's median income in the state distribution in 1962. This is a time-invariant district characteristic that is defined as follows; income percentile group 1 is all districts at or below the 10th percentile of the state distribution of district median income; group 2 are those between the 11th and 25th percentile; group 3 are those between the 26th and 50th percentile; group 4 those between the 51st and 75th percentile; group 5 are those between the 76th and 90th percentile; and income percentile group 6 is districts in the top 10 percent of the state distribution of median income in 1962. θ_d is a district fixed effect (which subsumes a state effect), θ_t is a year fixed effect, and ε_{dt} is a district-year error term. Because some states had multiple reforms, we estimate treatment effects for the first reform of each type (we describe the reform types below).

The treatment variables for the first reform are I_y^{reform}. These are indicator variables equal to 1 if state s will implement its first reform in y years, and 0 otherwise. These variables are interacted with Q_d so that the coefficients $\pi_{q,y}^{reform}$ map out the dynamic treatment effect of the first reform on per-pupil spending for districts in income percentile group q. For example, $\pi_{1,-10}^{reform}$ is the effect today of implementing the first reform 10 years in the future for districts in income percentile group 1 (bottom 10 percent), and $\pi_{1,5}^{reform}$ is the effect today of having implemented the first school finance reform five years ago for districts in the first income percentile group. We plot the estimated treatment effects to illustrate how district per-pupil spending evolves before, during, and after reforms (relative to similar districts in non-reform states and/or non-reform years). All district observations are weighted by the average student enrollment across all years in the sample.

We use the year of the court decision mandating reform as our main exogenous shifter in school spending because the timing is more plausibly exogenous than other policy changes.[10] Figure 1 presents the event-study plots for court-mandated reforms for school districts in the bottom and top 10 percent of the median income distribution in 1962 (before any reforms were implemented). The figure depicts how district-level per-pupil spending evolved annually from five years prior to the first court-mandated reform through 20 years post reform. Each series of event-study estimates

[10] For example, the timing of legislative SFRs are often more likely to pass during more favorable fiscal times which could affect student performance irrespective of whether SFRs occur.

is relative to the effect for the year immediately prior to the first reform. As such, a value of Y in a given year indicates that spending in that year was Y above the year immediately before reforms. Year 0 is the year of the first reform so that if reforms increase/decrease spending relative to the pre-reform year, values for years 1 through 20 should be positive/negative.

Because we aim to exploit the *differential* effect of reforms across districts, we provide the event time plot for the low-income districts (solid grey line) with the 90 percent confidence interval for each event study year (dashed grey lines) along with the event time plot for the high-income districts (solid black line) on the same graph. During the 5 years prior to reforms (years -5 through -1), both high and low income districts in reform states saw similar changes in per-pupil spending as districts of the same income level in non-reform states. This is evidenced by the fact that the 90 percent confidence interval for the lowest income districts includes zero for all these pre-reform years and also includes the event study estimates for the high income districts in reform states.

We find that within the first 7 post-reform years that both high and low income districts in reform states experienced increases in per-pupil spending above and beyond comparison districts in non-reform states, which is consistent with previous findings that court-mandated reforms tend to increase spending levels. However, while this increase is sustained in the low income districts, spending levels in high income districts fall below pre-reform levels by ten years post reform. While the effect of court-ordered reforms in the first 5 years post reform is similar in high and low income districts, the longer-run effects are quite different. After 8 years, the 90 percent confidence interval for the low income district effects tend to be above 0, and tent not to include the effect for high income districts – indicating that court-mandated reforms increase spending in low income districts and reduce spending gaps between low and high income districts in the long run. To directly test the hypothesis that the change in spending post-reform relative to the pre-reform years is equal to zero, we computed the difference between the average outcomes in the 5 years immediately before reforms and the 10 years after reforms using the delta method. These reforms increased per-pupil spending for the bottom income districts over the first 10 years by $582.81 in 2010 dollars (p-value=0.07) and reduced spending in the top income districts by $110.41 (p-value=0.27). Importantly, court-mandated reforms had different effects on high- and low-income districts within the same state such that the difference in the effects across the two groups of districts is statistically significantly different from zero at the 1 percent level.

c. Predicting Reform Induced Exogenous Variation in School Spending

Having described how court-mandated reforms affect school spending in different districts, we detail how we use these patterns to predict reform-induced spending changes for each district that are unrelated to other unobserved changes that might be correlated with both school spending and adult outcomes. To outline the logic, consider the following example. Based on Figure 1, one can predict that 10 years following reforms, on average, spending in the lowest income districts will increase by $582.81 versus a $110.41 decrease for the highest income districts. This prediction relies on the systematic differences in how different kinds of district are affected by court-ordered reforms and does not rely on the particular experiences of any one single district. As pointed out in Hoxby (2001), the effect of a reform on spending depends on the type of school funding formula introduced by the reform. Accordingly, while this is a perfectly valid prediction, one can get an even more precise prediction if one also incorporates information about the kind of funding formula introduced by the court-ordered reform.

Doing an event study analysis for the imposition of school funding formulas that include a spending limit (see Appendix D) we find that in the 10 years after the imposition of a spending limit, on average, spending in the bottom income districts falls by $15.39 (p-value=0.94) and spending for the top income districts falls by $535.91 (p-value<0.01). Assuming these effects are additive, a more precise prediction is that a court-ordered reform that imposes a spending limit will increase spending for the lowest income districts by 582.81-15.39=$576.42 and decrease spending for the highest income districts by 110.41+535.91=$646.32.

This example above illustrates how one can predict the spending increase a district would experience due to the passage of a court-ordered reform based solely on the type of funding formula introduced by the reform and the income level of the district prior to the passage of the reform. Importantly, this prediction is not based on the actual spending increases experienced by a district, but is a prediction based on the experiences of all districts facing the same kind of reform and with the same observable characteristics prior to the passage of reforms. Even though our approach is more complicated than the simple example above, the underlying logic is the same.

There are two key differences between how we construct our predicted reform-induced spending increases and the construction in the example above. The first key difference is that our prediction of the reform-induced effect for districts in any state is based on data from all *other* states (i.e., excluding data for that particular state). This is to ensure that any predicted district-level reform effects are not driven by any unobserved district-level factors that determine actualized spending and are also correlated with student outcomes. The second key difference is that we not only use the imposition of a spending limit as our type of reform, but we use the four key reform types described in Jackson, Johnson and Persico (2014) -- *foundation plans, spending limits, reward for effort plans, and equalization plans.* Foundation plans guarantee a base level of per-pupil school spending. Spending limits prohibit per-pupil spending levels above some predetermined amount. Reward for effort plans match locally raised funds for education with additional state funds (typically in low income districts). Equalization plans typically tax wealthier districts and redistribute funds to low wealth districts. These four key reform types are not mutually exclusive as many states' implemented formula change involved more than one of these types. More detailed descriptions of this typology of reforms and event study estimates for each formula type on spending are presented in Appendix D. We construct out-of-sample district-specific predicted reform-induced spending increases in three steps as described below.

Step 1: For each court-ordered reform we determine the funding formula type that the reform introduced by associating any formula change within three years of a court order to that reform.

Step 2: Omitting data for the state for which the prediction is being created, we estimate the dynamic treatment effect of a court order interacted with the formula type associated with that reform for each district income group in 1962. We estimate [2] below where $\$_{dst}$ is per-pupil spending, I_{F_s} is an indicator for the type of funding formula introduced by the court order (more than one formula type can be introduced by the same court-ordered reform), I_y^{court} is an indicator variable denoting year of the observation relative to the year of the first court-ordered reform for that state (i.e., I_{-5}^{court} is equal to 1 if the observation is five years before the first court-ordered reform and zero otherwise), and Q_d is the quartile group of the district in the state distribution of median income in 1962 (prior to any reforms). All other common variables are defined as in [1].

$$[2] \qquad \$_{dst} = \alpha + \sum^{F} I_{F_s} \cdot \left(Q_d \cdot \sum I_y^{court}\right) \cdot \pi_{F,q,y}^{court} + \sum^{F} I_{F_s} \cdot \left(\$_{1972,d} \cdot \sum I_y^{court}\right) \cdot \pi_{F,\$,y}^{court} + \theta_d + \theta_t + \varepsilon_{dt}$$

The coefficients $\pi_{F,q,y}^{court}$ map out the effect after y years of a court-ordered reform that introduced formula F for a district in income percentile group q. To improve our ability to predict reform-induced spending increases we also use $\$_{1972,d}$, the district's per-pupil spending level relative to the state average in 1972 (prior to the passage of reforms) to predict the marginal effect of any reform. Accordingly, the coefficients $\pi_{F,\$,y}^{court}$ map out the effect after y years of a court-ordered reform that introduced formula F for a district with relative per-pupil spending level \$ in 1972.

Step 3: Because reforms do not take full effect immediately, we use the average effect between 3 and 8 years after reforms. As such, $Spend_d$, our prediction of the spending change a district will experience due to the passage of a court-ordered reform, is *the estimated change in spending between three and eight years after a court-mandated reform for similar districts (same pre-reform relative income level, same pre-reform relative spending level) facing the same kinds of formula changes (foundation, spending limit, reward for effort, equalization) in different states*.

d. Creating an Instrument for School Spending

In predicting adult outcomes, our endogenous treatment variable is the natural log of average school spending over the previous 12 years. This measures average school spending across all school-age years (5 to 17) for expected high school graduates that year. Having predicted the spending change a district will experience with the passage of a court order, we now show how the interaction between this district-specific prediction, $Spend_d$, and the timing of court-ordered reforms isolates plausibly exogenous variation in school spending that is unrelated to potentially confounding district-level determinants of school spending. We estimate equation [3] where $\ln(\overline{\$}_{dst})$ is the natural log of average school spending over the previous 12 years, $Spend_d$ is our scalar district-specific prediction of the reform-induced spending change, I_y^{court} are flexible event time indicators, θ_d is a district fixed effect, θ_t is a year fixed effect, and ε_{dt} is random error.

$$\text{[3]} \qquad \ln(\overline{\$}_{dst}) = \alpha + (Spend_d \sum I_y^{court})\pi_{spend,y}^{court} + Z_{dt} + \theta_d + \theta_t + \varepsilon_{dt}$$

The coefficients $\pi_{\text{Spend},y}^{court}$ map out the spending change associated with a court-mandated reform for a district that is predicted (*based on similar districts in other states*) to double school spending by years 3 through 8 post reform. To show the changes in spending both by duration of exposure and by predicted treatment intensity, Figure 2 plots the estimated flexible event study coefficients for a 5 percent predicted change, a 10 percent change, and a 20 percent predicted change. If our instrument has identified exogenous variation, districts that saw larger versus smaller *predicted* spending increases due to reforms should be on very similar trajectories prior to reforms. Also, if the instrument is valid, after reforms districts with larger versus smaller *predicted* spending increases due to reforms should experience larger versus smaller *actual* spending increases.

To isolate spending changes associated with SFRs, we also include Z_{dt}, controls for an array of other policies that overlap our study period (Johnson, 2013; Chay, Guryan, & Mazumder, 2009; Hoynes, Schanzenbach, & Almond, 2012). These include county-by-year measures of school desegregation, hospital desegregation, community health centers, state funding for kindergarten, per capita Head Start spending, Title I school funding, imposition of tax limit policies, and average childhood spending on food stamps, Aid to Families with Dependent Children, Medicaid, and unemployment insurance. Additionally, to remove any pre-trend differences between reform and non-reform districts we also include linear trends for the poverty rate in the district in 1962, and Census division-specific year fixed effects.

Because we interact each event-study year indicator with the relevant predicted spending increase, the model imposes a linear relationship between predicted spending and actual spending for each event-study year, but imposes no structure on the relationship between predicted spending and actual spending over time. As such, the flexible model does not dictate that effects over time for a predicted 10 percent increase have to be greater than that of a 5 percent increase, and the two lines can even cross. We find districts predicted to experience 5, 10, and 20 percent spending increases were on similar trajectories prior to reforms. One cannot reject the hypothesis of no differential pre-trending at the 10 percent level— consistent with our predicted spending changes being exogenous. Despite similar pre-reform trajectories, these districts were on very different trajectories of spending after reforms. All three groups see increased spending growth post reform with the larger increases for districts with larger predicted effects. The F-statistic on the post-reform year indicators yields an F-statistic over 50 ($p<0.0000$).

Figure 2 is a visual representation of our first stage that isolates exogenous variation in school spending across cohorts within the same district. If there is a causal relationship between school spending and adult outcomes, one should observe (a) no differential pre-reform trends in outcomes for districts with larger or smaller *predicted* reform-induced spending increases, and (b) larger improvements in outcomes for post-reform cohorts in districts with larger *predicted* reform-induced spending increases. These are the patterns we document in Section V.

III. Description of the Longer-Run Outcome Data

Our data on longer-run outcomes come from the Panel Study of Income Dynamics (1968–

2011) that links individuals to their census blocks during childhood.[11] Our sample consists of PSID sample members born between 1955 and 1985 who have been followed from 1968 into adulthood through 2011. This corresponds to cohorts that both straddle the first set of court- mandated SFRs (the first of which was in 1972) and who are also old enough to have completed formal schooling by 2011. Two thirds of those in these cohorts in the PSID grew up in a school district that was subject to a court-mandated school finance reform between 1972 and 2000. We include both the Survey Research Center component and the Survey of Economic Opportunity component, commonly known as the "poverty sample," of the PSID sample. Parental income and education variables are averaged over the ages of 12 and 17, and family structure is measured at birth. Following Ben-Shalom, Moffitt, and Scholz (2011) and Short and Smeeding (2012), a child is defined as "low income" if parental family income falls below two times the poverty line for any year during childhood.[12] This captures both the poor and the near poor. Due to the oversampling of poor families, 59 percent of the sample were low income as children. Henceforth, children from families who were not low income (as defined above) will be referred to as "non-poor".

We match childhood residential location address histories to the school district boundaries that prevailed in 1969 to avoid complications arising from endogenously changing district boundaries over time. We use the earliest available address in childhood to circumvent concerns of endogenous mobility. The algorithm is outlined in Appendix E.[13] We show that our results are robust to using only those who lived in their childhood residence prior to initial court orders. Each record is merged with data on school spending and the aforementioned school finance variables at the school district level that correspond with the prevailing levels during their school-age years. Finally, we merge in county characteristics from the 1962 Census of Governments, and information on the key policy changes (described in Section II) during childhood, allowing for an

[11] 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. Studies have concluded that the PSID sample remains representative of the national sample of adults (Gottschalk et al., 1999; Becketti et al., 1997).

[12] The poverty line is defined by family composition, such that children are defined as "low income" if the family's income-to-needs ratio falls below two for any year during childhood.

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

unusually rich set of controls.[14]

The final sample includes 93,022 adult person-year observations of 15,353 individuals (9,035 low-income children; 6,318 non-poor children) from 1,409 school districts, 1,031 counties, and all 50 states and the District of Columbia.[15] Because we use individuals born between 1955 and 1985, the oldest cohort is observed at age 56, while other cohorts are observed at age 30. To compare individuals from different cohorts at similar ages, we focus on adult observations between the ages of 25 and 45. The mean age is 32.9 years for economic outcomes. The set of adult outcomes examined include (a) educational outcomes—whether graduated from high school, years of completed education (at the most recent survey) – and (b) labor market and economic status outcomes (measured annually and 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 statistics are presented in Table 1.

Average years of education is 13.18, where children from low-income families have about 1 fewer years of schooling than non-poor children. We examine wages (annual earnings/annual work hours) as our main labor market outcome. We use wages only for those who have positive earnings in a given year. However, because we have multiple adult observations for each individual, we have valid wage observations for about 95 percent of the sample. This is more than datasets such as the CPS or the Census that only have earnings at a single point in time (about two-thirds of individuals). This feature of the data allows us to better detect effects for those with low labor market attachment. The average wage in 2000 dollars at age 30 for those from low-income families is \$10.60 and that for those from non-poor families is about 28 percent higher at \$13.60.

IV. Empirical Strategy for Estimating Effects on Adult Outcomes

We aim to explore how exogenous changes in school spending induced by SFRs affect adult outcomes. Our emphasis on using only exogenous variation is motivated by the observation

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[14] 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).

[15] The average school district has 11 PSID sample children; half of the districts have at least 9 PSID sample children; less than 6% of school districts have only one PSID sample child; and one quarter have at least 25 PSID children.

that 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 if there were other factors that affect both outcomes and school spending simultaneously. For example, a decline in the local economy could depress school spending (through home prices or tax rates) and also have deleterious effects on student outcomes through mechanisms unrelated to school spending such as parental income. This would result in a spurious positive correlation between per-pupil spending and child outcomes. Conversely, an inflow of low-income, special needs, or English language learner students could 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.

To highlight this point, we test the exogeneity of school spending. First, we predict both high school graduation and adult wages (at age 30) using the fitted values of a regression of these outcomes on parental income, race, mother's and father's education and occupational prestige index, mother's marital status at birth, birth weight, childhood county-level average per-capita expenditures on Head Start, AFDC, Medicaid, and food stamps during school-age years—this is an effect-size weighted index of childhood family/community factors. We then examine whether predicted outcomes are related to the average actualized district per-pupil spending during ages 5-17. The results are presented in the top panel of Table 2. Naïve OLS models that rely on variation in school spending both within and across states (Top panel, columns 1 and 2) show a strong positive and statistically significant association between school spending and predicted outcomes. This is consistent with most people's priors that raw correlations between spending and outcomes are likely to be *positively* biased because areas with higher levels of school spending (in the cross section) will tend to comprise children from more advantaged family backgrounds in both observed and unobserved ways. However, when we examine the relationship between *changes* in actualized spending within districts over time and changes in predicted outcomes (columns 3 and 4), there is a statistically significant negative relationship for predicted high school completion and a marginally statistically significant negative relationship for wages at age 30. This is consistent with there being a *negative* bias when using actualized spending changes within districts over time to predict better outcomes. We also look at the relationship between school inputs (student-teacher ratios) and endogenous changes in school spending (Column 5). Surprisingly, while the point estimates show the expected sign, endogenous spending changes are not significantly related to observable school resource inputs.

To provide a basis for comparison, we instrument for school spending with the predicted spending increase interacted with indicators for each year of school-age exposure to reforms (0 to 12). In contrast to the OLS estimates, our exogenous school spending changes (based on the timing and type of SFRs) are not related to changes in predicted outcomes, and the effects go in different directions for the two predicted outcomes (lower panel). Looking to the student-teacher ratio, however, reveals a stark difference between the OLS variation and the 2SLS/IV identifying variation; exogenous spending increases are associated with large statistically significant reductions in the student-teacher ratio. Table 2 illustrates that OLS estimates of the effects of school spending on outcomes may be negatively biased, and may not be associated with improved school inputs. In contrast, the exogenous variation in spending due only to SFRs is likely to uncover the true causal relationship as mediated by improved school inputs. We show evidence of this in Sections V and VI.

a. The Effect of Predicted Spending Increases on Actual Spending Increases

Our treatment measure of exposure to school spending is $\ln(PPE_{5\text{-}17})$, the natural log of average per-pupil spending in an individual's birth district during the years when that individual was ages 5 through 17 (school age years). A 0.1 increase of this average can be interpreted as a 10 percent increase in per-pupil spending for all 12 years of an individual's school career.[16] To show consistency across the CCD universe of school districts and the sample of districts covered in the PSID, we begin by presenting "first-stage" event study model [4] estimates on average per-pupil spending using our PSID sample (Figure 3). The model includes the same set of control variables as that used in the CCD with the addition of controls for individual characteristics.[17]

Figure 3 presents the estimated event-study effects on average *actual* spending during ages 5-17 associated with a *predicted* 5, 10, and 15 percent spending increase between three and eight years after a court-mandated reform. Event study year zero on the x-axis is the year an individual turns 17 minus the initial year of a court-ordered reform (negative values indicate pre-reform cohorts). The average *predicted* spending change is 5 percent, the standard deviation of the

[16] We express spending in logs because school spending likely exhibits diminishing marginal returns. The patterns of results are very similar when spending is expressed in levels (See Appendix F).

[17] The individual level controls include race, race-by-census division of birth year fixed effects, and controls for parental education and occupational status, parental income, mother's marital status at birth, birth weight, child health insurance coverage, and gender. Results are similar without these controls.

predicted spending change is roughly 9 percent, and one-quarter of districts in reform states had predicted spending increases of more than 10 percent. The results mirror the patterns using the full universe of public school districts (Figure 2) -- supporting the generalizability of the PSID results for cohorts born between 1955 and 1985. As in the CCD data, districts with larger versus smaller *predicted* spending increases due to reforms were on similar trajectories in spending prior to reforms; however, after reforms districts with larger *predicted* spending increases due to reforms experienced larger *actual* spending increases. These patterns are the same for both low-income and non-poor children. The right figure presents the event study for a 10 percent predicted spending increase along with the 90 percent confidence interval. We will show this same figure for all long-run outcomes, so it is important to note that a 10 percent *predicted* increase during years 3 and 8 post reform corresponds to about a 15 percent increase in *actual* school-age spending for fully exposed cohorts (all 12 years).

The reform-induced increases in average school spending during ages 5-17 are small in the first three years following reforms, but increase roughly linearly with school-age exposure years 4 through 12, and event-study years 13 through 17, before flattening out. The estimated pattern is insensitive to whether a balanced or unbalanced panel is used. Figure 3 is a visual representation of our first stage that isolates exogenous variation in school spending across cohorts within the same district. We now discuss the patterns one should observe for outcomes if there is a causal effect of these increases in school spending on outcomes.

b. Hypothesized Effects of Reform Induced Spending Increases On Adult Outcomes

Figures 2 and 3 show two distinct sources of variation in school spending during one's school-age years: (a) the staggered timing of court-mandated SFRs across districts to implement a cohort level "event-study" analysis (variation in the timing of reforms across cohorts), and (b) the fact that the same reform led to different changes in spending across districts (variation in intensity among exposed cohorts). Accordingly, there are two natural tests of whether reform-induced spending changes have a causal effect on adult outcomes. The first test is whether exposed cohorts from those districts that experienced increases in per-pupil school spending also had improved outcomes relative to unexposed cohorts from the same district. The second test is whether the improvements observed for exposed cohorts (relative to unexposed cohorts) are larger for those from districts that experienced 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-age 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 school spending on 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 4. The X-axis is years of exposure to the reform for a given cohort, and the y-axis is the cohort-level mean of some outcome for which higher values are better.

For those cohorts who were 18 or older at the time of the passage of reforms (to the left of 0), there should be no systematic increase or decrease in the outcome across cohorts because none of these cohorts was exposed to reforms. As such, an event-study graph of outcomes by cohorts should be relatively flat across cohorts that were too old to be affected by the reforms. Also, outcomes should be similar across the pre-reform cohorts across districts that experienced large and small increases in school spending after reforms. For those cohorts who were of school-going age when reforms were implemented (i.e. between the ages of 17 and 5, 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, for a given increase in spending, relative to unexposed cohorts, cohorts exposed to increased spending for a longer period of time should have larger improvements (variation in timing). Additionally, for a given duration of exposure, relative to unexposed cohorts, individuals exposed to larger spending increases should have larger improvements in outcomes than those from districts with smaller increases in spending (variation in intensity). As such, among those exposed, the relationship between years of exposure and good outcomes should be positive, and this relationship 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.

Among more recent cohorts (i.e., those who were younger than 5 or unborn at the passage of reforms) all 12 of their school-age years were post-reform, and as a result, we hypothesize these cohorts should have better outcomes than the partially exposed cohorts. Spending increases continued beyond 12 years post reform (see Figures 2 and 3); thus, among the fully treated cohorts, recent cohorts may have better outcomes than older cohorts (because more recent cohorts will have experienced more total spending during their school-age years). As with the partially exposed cohorts, one might expect better outcomes (relative to untreated cohorts) for the fully treated cohorts from high-increase spending districts than low-increase spending districts. Note that because those with fewer years of exposure also experience reforms when they are older, we cannot disentangle years of exposure from the effect of age of exposure.

In sum, if there is a causal effect of spending on outcomes and the spending increases due to reforms are exogenous to changes in outcomes, then the event-study plot of outcomes for districts that experience small and large spending increases due to reforms should follow the stylized patterns in Figure 4. That is, outcomes should be improving in years of exposure to reforms (time variation) and the relative improvements should be larger in districts with larger increases in school spending (variation in intensity). We present such patterns below.

c. Analyzing the Effect of Reform Induced Spending Increases on Adult Outcomes

We test for the patterns hypothesized in Figure 4 across several adult outcomes. Our measure of treatment intensity for each district is the predicted reform-induced spending change ($Spend_d$), which is based on the predicted change in spending between three and eight years after a court-mandated reform for similar districts (same pre-reform relative income level, same pre-reform relative spending level) facing the same kinds of formula change (foundation, spending limit, reward for effort, equalization) in different states (as described in Section II). The event-study models used to analyze the effect of predicted school spending changes on the difference in outcomes between treated and untreated cohorts involve estimating equations of the form [4]:[18]

$$\begin{aligned} Y_{idb} = & \sum_{t-T=-20}^{-2} \alpha_{t-T} \cdot 1\left(t_{idb} - T_d^* = t-T\right) \cdot SPEND_d + \sum_{t-T=0}^{12} \theta_{t-T} \cdot 1\left(t_{idb} - T_d^* = t-T\right) \cdot SPEND_d \\ & + \sum_{t-T=13}^{20} \delta_{t-T} \cdot 1\left(t_{idb} - T_d^* = t-T\right) \cdot SPEND_d + X_{idb}\beta + Z_{db}\gamma + (W_{1960d} * b)\phi + \eta_d + \lambda_b^r + \varphi_g^r * b + \varepsilon_{idb} \end{aligned} \tag{4}$$

where i indexes the individual, d the school district, b the year of birth, g the census division of birth, and r the racial group. The term η_d represents a vector of school district fixed effects, and $SPEND_d$ is the predicted SFR-induced change in per-pupil spending in district d. The flexible timing indicators, $1\left(t_{idb} - T_d^* = t-T\right)$, equal 1 if the year the individual from school district d turned

[18] This approach is similar to Johnson (2011) in a study of the long-run effects of court-ordered school desegregation.

age 17 (t_{idb}) minus the year of the initial SFR court order in school district d (T_d^*) equals a value between -20 and 20. For example, values for $\left(t_{idb} - T_d^*\right)$ 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 persons who were younger than 5 when reforms were enacted. 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.

The variables of interest are the coefficients on the interactions between $Spend_d$ and the flexible event-time indicators. These estimates illustrate the exact timing of changes in outcomes in relation to the number of school-age years of exposure to SFR and the predicted changes in spending. These coefficients map out the difference in outcomes between exposed and unexposed cohorts from districts with larger versus smaller predicted reform-induced spending increases. Equation [4] 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.

The validity of our empirical design relies on the assumption that districts that experienced increases in school spending due to reforms were not already on a differential trajectory of improving outcomes. As such, 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 $SPEND_d$, will show if outcomes in districts with larger or smaller spending increases were on a different trajectory than non-reform districts prior to enactment of reforms. This provides a test of exogeneity of the timing and scope of initial court orders.

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). To ensure that it isolates exogenous changes due to reforms we include a variety of additional controls. The model includes school district fixed effects (η_d), race-specific birth year fixed effects (λ_b^r), race-by-region of birth cohort trends ($\varphi_g^r * b$), and controls for an extensive set of child and childhood family characteristics (X_{idb}: parental education and occupational status, parental income, mother's marital status at birth, birth weight, child health insurance coverage, and gender). To control for trends in factors hypothesized to influence the timing of SFR, we also included interactions between 1960 characteristics of the county of birth and linear trends in the year of birth ($W_{1960d} * b$): 1960 county poverty rate, percent black, average education level, percent urban, and population size. Finally, to account for the effect of other policies, we include county-by-birth year level measures of school desegregation, hospital desegregation, community health centers, state funding for kindergarten, imposition of tax limit policies, in addition to per capita expenditures on Head Start (at age four), Title I school funding (average during ages 5-17), and average childhood spending on food stamps, Aid to Families with Dependent Children (AFDC), Medicaid, and unemployment insurance, (Z_{cb}). Few studies simultaneously account for so comprehensive a set of policies. Standard errors are all clustered at the school district level.

One potential parental response to existing school quality differences across public schools is to move to a different location or enroll children in a private school.[19] To avoid bias due to endogenous mobility, we identified the county and school of upbringing based only on the earliest childhood address.[20] This captures the quality of the public schools potentially attended instead of the quality of schools and classes actually attended. As such, one can interpret our results as providing "intent to treat" estimates of the effects of school spending.[21]

To present both the time and intensity variation on the same graph, we display the effects for both a 15 and 20 percent school spending increase. That is, we plot the coefficients for the flexible event time figures interacted with $SPEND_d$ where $SPEND_d$ is set to 0.05 and 0.1, respectively. The event study figures provide a visual depiction of the reduced-form effect of court-ordered reforms on outcomes. To provide point estimates and statistical inference tests for the effect of spending on outcomes, we employ 2SLS/IV regression analysis.

c. Estimating of the Causal Effect of School Spending on Adult Outcomes

To identify effects of school spending solely from the exogenous variation in school spending caused by reforms, we use the number of school-age years of exposure interacted with

[19] 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.
[20] Because we use the earliest address available for children, limiting the analysis sample to those who lived at that childhood residence prior to initial court orders reduces the sample by less than 5 percent. Results are similar when the sample is restricted to individuals who lived in their childhood residence prior to the initial court orders.
[21] We recognize that school spending varies within districts. However, these data are not typically reported.

the district-specific predicted change in spending as our exogenous instrument for school spending. Specifically, we estimate the following system of equations by two-stage least squares (2SLS).

[5] $$\ln(P\hat{P}E_{5-17})_{\mathrm{idb}} = \pi_1\left(t_{idb} - T_d^*\right)\cdot SPEND_d + X_{idb}\pi_2 + Z_{db}\pi_3 + \left(W_{1960d} * b\right)\phi + \eta_{d1} + \lambda_{b1}^r + \varphi_{g1}^r * b$$

[6] $$Y_{idb} = \delta\cdot\ln(P\hat{P}E_{5-17})_{\mathrm{idb}} + X_{idb}\beta + Z_{db}\gamma + (W_{1960d} * b)\varphi + \eta_d + \lambda_b^r + \phi_g^r * b + \varepsilon_{idb}$$

All variables are defined as in [4] and the same controls are included. The difference between [4] and [5] and [6] is that we replace the event time indicators interacted with predicted spending changes in [4] with a single measure of per-pupil spending, $\ln(PPE_{5\text{-}17})_{idb}$, in the second stage regression in [6]. We instrument for $\ln(PPE_{5\text{-}17})_{idb}$, with the event time indicators (i.e., an indicator for each year of exposure between 0 and 12 years) interacted with the district-specific predicted spending change, $\left(t_{idb} - T_d^*\right)\cdot SPEND_d$. Standard errors are clustered at the school district level.

The instrumental variables models exploit both the exogenous variation in timing and intensity of school spending changes due to court-mandated reforms. The coefficient δ from [6] should uncover the causal effect of school spending on 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 predicted increases in school spending due to the reforms. Both the event-study analyses and additional placebo tests suggest that this is the case.

V. Effects on Longer-Run Outcomes

Educational Attainment. Figure 5 presents the semi-parametric event-study model results of the effects of reform-induced changes in per-pupil spending on years of completed education. On the left we present the coefficients on the individual event time indicator variables interacted with the district-specific change in spending for a 5 percent and 10 percent predicted spending increase. Note, these are reduced-form effects of predicted spending increases, not effects of actual spending increases (estimates of actual spending are provided in the 2SLS regression results). Even though [4] imposes a linear relationship between $Spend_d$ and outcomes in any given year, the relationship between $Spend_d$ and outcomes varies across years (e.g., larger spending increases can improve outcomes for those with 8 years of exposure but hurt outcomes for those with 9 years of exposure). As such, the estimated trajectories across time for different spending levels may not be parallel, and can even cross. Insofar as the model shows better outcomes for treated cohorts in districts with larger spending increases, this is an artifact of the data and is indicative of causal effects (rather than an artifact of any modeling assumption). If there are true causal effects, one would expect these lines to possibly cross and be centered on zero during pre-treatment years, but would expect the effect to be increasing with duration of exposure and be consistently larger for a 10 percent increase than a 5 percent increase (with no causal effect, both lines will be centered on zero so that there will be no tendency for one line to be systematically higher than the other). The reference cohort (year 0) are individuals who were 17 years old when the court order was first enacted. The right panel shows the effect of a 10 percent predicted spending increase along with the 90 percent confidence interval for each event study year.

Overall, one can see clear patterns of improved outcomes for exposed cohorts in districts with larger predicted spending increases. Districts that saw increases in school spending exhibit no discernible trending in educational attainment 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 with a predicted 5 and 10 percent spending increase (these lines often cross for the pre-reform cohorts). This indicates that the timing of the reforms was likely exogenous to changes in educational attainment in a given district and that the size of the predicted spending increase was unrelated to the pre-reform trends in outcomes. This lends credibility to our research design and the resulting 2SLS estimates. Looking at treated cohorts, the results are consistent with significant causal effects on exposed cohorts. That is, cohorts with more years of exposure to predicted spending increases have higher completed years of education than unexposed cohorts and cohorts with fewer years of exposure. Also, the increases associated with exposure are larger in districts with the larger predicted increases in spending (indicated by the line for a 10 percent increase being consistently above that of a 5 percent increase for the exposed cohorts). Both the patterns in timing and intensity support the hypothesis that policy-induced increases in school spending led to significant increases in educational attainment. The panel on the right only depicts the event time estimates for a 10 percent predicted increase along with the 90 percent confidence interval for each event study year. About one quarter of districts have predicted spending increases at least this large. The panel on the right shows the same pattern of results as the left, however one can see that each event-study year estimate is noisy. Having said this, after 9 years of exposure, one can reject the null hypothesis that most of the post-reform years is different from that of no exposure at the 10 percent level. Note that testing the difference between individual years of exposure is low powered, and is not a test of the broader hypothesis that variation in school spending is related to variation in outcomes. To test this broader hypothesis we rely on the 2SLS regressions that impose a bit more structure on the data.

Because residential mobility across counties and private school attendance are more common among affluent families than low-income families, one might expect larger effects among low income children.[22] Furthermore, prior research has shown that children from low-income families may be more sensitive to changes in school quality and school-related interventions than children from more advantaged backgrounds.[23] For these reasons, we also conduct all analyses separately by childhood economic status (low income vs non-poor) in Figure 6.[24] Event study figures for a 10 percent predicted increase and the 90 percent confidence intervals are shown separately for low-income (left) and non-poor (right) children. For low income children, the event-study plots follow the same pattern as for the overall sample, but the difference between the exposed and unexposed cohorts are more pronounced. In contrast, the estimates for non-poor children are much weaker. For non-poor children, there is suggestive evidence of an increase in educational attainment after the passage of reforms. Exposed cohorts do appear to have higher years of education (e.g., increases in event study years 4-11) than the pre-reform cohorts, and districts that experienced larger predicted spending increases do tend to have better educational attainment than those with smaller predicted spending increases for the exposed cohorts. The pattern of results suggest small effects for children from non-poor families and large effects for low-income children.

To examine what margin of educational attainment was most affected, we turn to the probability of high school graduation. The results for high school completion reveal very similar patterns to those for years of education. Figure 7 presents the event study plots for the effects of a 10 percent predicted spending increase on the likelihood of high school graduation for children from low-income (left) and non-poor families (right). As with completed years of education, there is no trending in outcomes for the pre-reform cohorts. However, for low income children, the likelihood of high school graduation is increasing in years of exposure for exposed cohorts. For non-poor families, there is no visible systematic effect on high school graduation.

[22] Prior research shows that while residential instability is greater for poor families, poor families are far less likely to move to better neighborhoods, and are less responsive to policy changes due to the greater residential location constraints they face (Johnson, 2008; Kunz, Solon et al., 2008; Mare et al., 2008).

[23] E.g., Krueger and Whitmore (2001).

[24] In pooled models we include childhood economic status indicator interactions with all control variables and test for differential spending effects.

The consistent pattern of these results indicate that these effects reflect the causal effect of school spending particularly for children from low-income families. That is, we see that (a) increases in educational outcomes occur only for children exposed during school-age years, (b) improvements are monotonically increasing with years of exposure, (c) improvements are greater with larger exogenously predicted spending increases, (d) the timing of improved outcomes track the timing of increased spending, and (e) there are no differential pre-reform trends in outcomes for districts with larger or smaller predicted spending increases. These patterns are all more pronounced for children from low-income families.

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

The regression estimates are presented in Table 3. 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. The interpretation of a 0.10 and 0.2 change in this variable is the effect of increasing school spending by 10 and 20 percent throughout all 12 of an individual's school-age years, respectively. The excluded instruments for this spending variable are indicators for each number of school-age-years of exposure to reforms (0 to 12) interacted with the school district's reform-induced change in school spending. The first stage F-statistic is greater than 10 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 2 of Table 3 presents the 2SLS/IV regression results based on variation presented in Figures 12 for all children. The 2SLS estimates indicate that increasing per-pupil spending by 10 percent in all 12 school-age years increases educational attainment by 0.27 years on average among all children. Results from column 3 show even larger effects for low-income children. For children from low-income families, increasing per-pupil spending by 10 percent in all 12 school-age years increases educational attainment by 0.44 years. This estimate is statistically significant at the 5 percent level. In contrast, for non-poor children, a 10 percent increase in per-pupil spending throughout one's school-age years increases educational attainment by only 0.075 years and this estimate is not statistically significant. To put these educational attainment estimates in perspective, the education gap between children from low-income and non-poor families is one full year. Thus, the estimated effect of a 22.7 percent increase in per-pupil spending throughout all 12 school-age years for low-income children is large enough to eliminate the education gap between children from low-income and non-poor families. In relation to current spending levels (the average for 2012 was $12,600 per pupil), this would correspond to increasing per-pupil spending permanently by roughly $2,863 per student.

Columns 5 and 6 present the 2SLS regression estimates for the likelihood of high school graduation. Overall, the 2SLS estimate indicates that increasing per-pupil spending by 10 percent in all 12 school-age years increases the probability of high school graduation by 9.5 percentage points. The 2SLS estimate indicates that increasing per-pupil spending by 10 percent in all 12 school-age years increases the probability of high school graduation by 11.6 percentage points (p-value<0.01) for low income children and 6 percentage points (p-value<0.10) for non-poor children. The 95 percent confidence interval for the effect of a 10 percent increase for low-income children is between 3.3 and 20.5 percentage points. The high school graduation rates for low-income and non-poor children were 79 and 92 percent, respectively. Accordingly, increasing per-pupil school spending by 10 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 4 and 26 percent. These results suggest some weak positive effects for more affluent children and large positive effects for low income children.

To put these estimates in perspective, attending Head Start or the Perry Pre-School Program increased high school completion by 8.5 and 14 percentage points, respectively (Carneiro and Heckman 2003; Deming 2009). Also, Barrow, Claessens, and Schanzenbach (2013) and Schwartz, Stiefel and Wiswall (2013) find that attending small schools increases graduation rates for low-income children by between 16 and 18 percentage points. Accordingly, our effects on educational attainment are in line with those of other successful interventions.

In sum, both the event study and 2SLS/IV models reveal that exogenous increases in school spending (caused by SFRs) led to substantial improvements in educational outcomes of affected children. Both analyses suggest that there are much larger effects of school spending on educational attainment of children from low-income families.

Labor Market Outcomes, *Adult Family Income, and Poverty Status*. The next series of results reveal meaningful effects of school spending on low-income children's subsequent adult economic status and labor market outcomes, using the same model specifications. It is important to note that our models that analyze economic outcomes (such as wages and annual family income) use all available person-year observations for ages 20-45 and control for a cubic in age to avoid confounding life cycle and birth cohort effects. Figures 9, 10, and 11 present event study school spending effects on adult economic outcomes (ages 25–45), including wages (Figures 9), annual family income (Figure 10), and the annual incidence of poverty (Figure 11), by childhood economic status. 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 4 for low-income children and are indicative of the causal effects of increases in school spending induced by reforms.

We first discuss the effects on adult wages for the full sample (Figure 8). Overall, one can see clear patterns of improved outcomes for exposed cohorts in districts with larger predicted spending increases. Districts that saw increases in school spending exhibit no discernible trending in wages for the pre-treatment cohorts and the pre-reform year effects are very similar for districts with a predicted 5 and 10 percent spending increase. As before, these are reduced-form effects of predicted spending increases, not effects of actual spending increases (estimates of actual spending are provided in the 2SLS regression results). Looking at treated cohorts, cohorts with more years of exposure to predicted spending increases have higher wages than unexposed cohorts and cohorts with fewer years of exposure. While not as pronounced as in the education outcomes, the increases associated with exposure are larger in districts with the larger predicted increases in spending (indicated by the line for a 10 percent predicted increase being consistently above that of a 5 percent increase for the exposed cohorts). The panel on the right depicts the event time estimates for a 10 percent predicted increase along with the 90 percent confidence interval for each event study year. The panel on the right shows the same pattern of results as the left. After 9 years of exposure, one can reject the null hypothesis that most of the post-reform years is different from that of no exposure at the 10 percent level. We now look at patterns for low-income and non-poor families separately.

For both the log of wages and family income (Figures 9 and 11), 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 low-income families, both wages and family income exhibit substantial improvements across cohorts associated with more years of exposure to a predicted spending increase. For children from low-income 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 for children from low-income families. The results on wages and adult family income for children from low-income families mirror those of the education results. For children from non-poor families, there is suggestive evidence of small positive effects on wages and annual family income in adulthood.

The 2SLS/IV estimates for adult economic outcomes are presented in Table 4. As shown in Column 3, the 2SLS/IV estimates reveal that, for children from low-income families, increasing per-pupil spending by 10 percent in all 12 school-age years increases adult wages by 9.5 percent. This implies an elasticity of wages with respect to per-pupil spending close to 1. However, the 95 percent confidence interval of this estimate supports a range of elasticities between a modest 0.08 and a sizable 1.8. In contrast, the 2SLS estimate for children from non-poor families is much smaller and statistically insignificant. It is worth noting that the point estimate implies that for non-poor children, increasing per-pupil spending by 10 percent in all 12 school-age years increases adult wages by 4.3 percent. While this effect is not statistically significant, the effect is economically important and is suggestive of benefits for all children, with larger effects for those from low income families.

While some of these wage effects will be due to increased years of schooling (for those induced to stay in school longer), the effect of improved school quality on those who do not change their school-going behaviors will be reflected in their wages but not their years of schooling.[25] To put our estimates in context, it is helpful to determine how much of the estimated effect on wages can plausibly be attributed only to increases in years of schooling. Tables 3 and 4 indicate that increasing school spending by 10 percent for all of a low-income child's school-age years will increase their years of schooling by 0.43 years and their adult wages by 9.5 percent. Recent

[25] Recent studies find that improvement in instruction are reflected in improved outcomes above and beyond their effects on years of schooling. Goodman (2012) who finds that an addition year of math coursework in high school increases the earnings of Black males by 5 to 9 percent, even when one conditions on the overall years of schooling. Also, Vestman et al (2013) find that the effects of class size on earnings are much larger than imputed effects based on increases in years of education.

credible estimates of the returns to an additional year of schooling indicate returns between 9 and 28 percent[26], so that wage effects between 3.87 and 12.04 percent can be expected *only* through a years of schooling effect. The actual increase in wages of 9.5 percent is well within this range. Because years of education may only capture some of the effect on wages, our estimates are consistent with effect sizes suggested by the existing literature.

Note that while we look at individuals in their 40s, recent studies of interventions on earnings tend to look at individuals in their 20s (e.g., Chetty et al, 2013). The estimated effects on wages for those in their twenties likely understates the effect on permanent income. To assess the importance of this, we estimate the effect of school spending interacted with a cubic in an individual's age. The implied age profile of the school spending effects on wages are presented in Figure 10. One can reject the null of no age profile at the 5 percent level. The results imply that the increase in wages that result from a 10% increase in school spending throughout the school-age years is 2.8% at age 20, about 8% during one's 30s, and 13.4% at age 45. Another important aspect of our data is that we observe the same individuals in multiple years rather than at one point in time (as in the CPS or Census). As such, individuals with low labor market attachment (who might be highly responsive to improvements in school quality) who might not have earnings in any given year can be observed with earnings in the PSID at some point over the panel, which minimizes potential sample selection bias. To show the importance of this, we estimate wage effects using only wage outcomes from the 2001 or 2011 waves (Table 5). Using a single year of wage data yields point estimates between half and two-thirds as large as those using all years, and yields standard errors four times as large. Haider and Solon (2006) show that using a single year of data, and data at young ages, will lead one to understate effects on earnings. This appears to be an important reason why our wage estimates are somewhat larger than others in the literature.

The 2SLS/IV estimates for the adult family income are similar to those of other outcomes. As shown in the middle panel of Column 4, the results indicate that for children from low-income families, increasing per-pupil spending by 10 percent in all 12 school-age years increases family income by 16.4 percent and this estimate is significant at the one percent level. The 95 percent

[26] Older estimates range from between 7.2 percent (Angrist and Krueger (1991) using only males in the 1980 census) to 16 percent (Ashenfelter and Krueger (1994) using both males and females in 1990 CPS). From Katz and Autor (1999), we know that the wage premium has been increasing over time, such that estimates from the 1980s and 1990s are likely to provide a lower bound to what one might expect for a cohort of workers in 2010. Jepsen, Troske and Cooms (2014) use very recent data and find wage returns due to an additional year of community college enrollment as high as 28 percent for women and 14 percent for men.

confidence interval for a 10 percent spending increase is between 4.3 and 28.5 percent. For children from non-poor families, the estimated effect is small and not statistically significant at the 10 percent level. The effects on family income reflect (a) increases in own income, (b) increases in other income due to increases in the likelihood of being married (i.e., there are more potential earners), and (c) increase in the income of one's family members (which is likely if persons marry individuals who were also affected by spending increases). Consistent with the effects on family income reflecting, in part, a family composition effect, we find that, among low-income children, a 10 percent spending increase is associated with a 10 percentage-point increased likelihood of currently being married and never previously divorced (not shown). There is no effect on the probability of ever being married, so this appears to reflect a marital stability effect.

Our final measure of overall economic well-being is the annual incidence of 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 12. As with the other outcomes, there is evidence of a causal effect of school spending on outcomes for children from low-income families and little to no effect for children from non-poor families. There is no pre-reform trending in outcomes across unexposed cohorts. However, the exposed cohorts from low-income families have steady declines in the annual incidence of adult poverty that become more pronounced in years of exposure. In stark contrast to that for low-income children, the event study for children from non-poor families (right) shows no systematic change in outcomes across cohorts. The 2SLS/IV results (Column 9 of Table 4) mirror the findings from the event study models. The 2SLS/IV estimate for children from low-income families indicates that increasing per-pupil spending by 10 percent in all 12 school-age years reduces the annual incidence of poverty in adulthood by 6.8 percentage points. This estimated effect is statistically significant at the one percent level and the 95 percent confidence interval is between 2.7 and 10.9 percentage points. The effect for children from non-poor families is small and not statistically significantly different from zero.

In summary, for individuals from low-income families, increases in school spending led to increases in adult economic attainment that rose in line with their educational improvements (likely reflecting a combination of improvements in both the quantity and quality of education received). Taken together, the event study graphs and the instrumental variables regression estimates based on exogenous changes in school spending show that increased school spending caused by SFRs had meaningful causal effects on adult wages, family income, and poverty status. We now present a series of robustness tests and discuss the findings in the context of prior studies 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 equations [5] and [6] where in addition to including instrumented school spending between the ages 5 and 17, we also include instrumented school spending between the ages 0 and 4 (when there should be no effect) and between the ages of 20 and 24 (when there should also be no effect) in the same model. 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 5 and 17 (school-age years) with the number of years of exposure between ages 5 and 17 interacted with the district-specific increase in spending. We instrument for school spending between ages 0 and 4 (pre-school-age years) with the number of years of exposure between ages 0 and 4 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 yields F-statistics above 10 for each of the three endogenous regressors. If the effects documented for low-income children are truly reflective of the causal effects of school spending, significant effects should be present during school-age years with no corresponding significant effects for non-school-age years. While the placebo estimates are somewhat noisy, the results of the placebo tests presented in Table 6 support a causal interpretation. 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 falsification tests support a causal interpretation of our main school spending estimates.

Address Endogenous Residential Mobility: We explored whether our results suffer from potential bias due to endogenous residential mobility. To address this concern, we re-estimated all models limiting the analysis sample to those who lived at their (earliest) childhood residence prior to the enactment of initial court orders in their respective state. The results are presented in

Appendix G and demonstrate the robustness of the findings. We find nearly identical results as those reported above across all of the outcomes considered and conclude, based on this evidence, that endogenous residential mobility is not an important source of bias in our analysis.

Addressing bias due to recent education reforms: Even though we are careful to control for a variety of potentially confounding policies, one might worry that we have not accounted for some recent education reforms. For example, test based accountability, charter schools and increase graduation requirements were all introduced in the late 1980s and early 1990s. While unlikely, if these recent reforms are correlated with the timing of court-mandated reforms, it could confound our estimates. To test for this, we estimate our educational attainment models interacting school spending with a dummy variable for those born between 1970 and 1985 (where earlier cohorts born between 1955-1969 serve as the comparison group). We focus on the education outcomes because differences in the effect on labor market outcomes across cohorts might be confounded with differences in economic conditions, returns to skills, and life cycle effects. If our effects are driven by the recent reforms, there should be no effect for the early cohorts, and the effect for more recent cohorts should be statistically significantly different from that of the older cohorts. We find that this is not the case. Instead, we find large, statistically significant effects for the older cohorts that are similar in magnitude to those for the full sample, and there is no statistically significant difference between the marginal effects for the older versus more recent cohorts (see Appendix H). This evidence suggests that these other recent educational reforms are not driving our results and suggest little to no bias due to more recent other reforms.

Validating Using Other Data: While the tests thus far show that our estimates are internally valid, readers might wonder how these patterns generalize to districts that are not included in the PSID. To address this, 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. The model is the same as that employed for the PSID sample. However, it is important to note that 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, 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. Third, because outcomes in the CCD data are aggregated at the district level, the CCD results may suffer from aggregation bias. Fourth, while we have actual high school graduation data on the PSID, we only have proxies for the graduation rate in the CCD (the number of graduates divided by the number of 8th graders four years prior). 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.

Figure 13 presents the event study graph for the number of high school graduates per 8th grader (four year prior). The pattern of results using the district level CCD data for 1987 through 2010 are similar to those in Figure 5 using the individual level PSID data for 1976 through 2000. The 2SLS regression results in Table 7 using the CCD yield a point estimate of 29.37 (p-value<0.01) – indicating that increasing per pupil funding by 10 percent over all 12 of a graduating cohort's school-age years would increase the number of graduates per 8th grader by 2.93 percentage points. We find it reassuring that the PSID and CCD point estimates are on a similar order of magnitude. However, we emphasize that the PSID estimates *are not* directly comparable to those from the CCD for all the reasons above. The important take-away is that the results from both datasets are qualitatively similar and point in the same direction – that money matters.

Because the CCD only has education outcomes, we also employ Census and American Community Survey (ACS) data for the same cohorts as those covered in the PSID. Unfortunately, these data do not link individuals to their school district during childhood, but only to their state of birth. While we cannot test for differences in outcomes using district-level per-pupil spending, we can test whether cohorts in states that saw increases in average school spending due to the passage of reforms have better outcomes than unexposed cohorts from the same state. It is important to note that using state-level increases in school spending as opposed to district level variation will likely lead to aggregation bias if the relationship between school spending and outcomes is non-linear. Accordingly, the relationship between state measures of school spending and outcomes will not be the same as the relationship between district measures of school spending and outcomes. It is also important to note that the Census data only include earnings for a single year. As shown previously, this will yield wage or earnings effects that are between one half and two-thirds the size found in the PSID. However, if there is a positive relationship between state measures of school spending and outcomes, it will lend further credibility to our results.

To mimic the PSID sample restriction, using a 3 percent sample from each decennial census and the ACS from 2000-2010, we only keep observations for individuals between the ages of 25 and 45 who were born between 1955 and 1985. This results in roughly 1.5 million observations. Because we do not have individuals linked to their childhood school districts we rely only on the variation in timing across cohorts within states. To illustrate that this variation is valid, we present event study plots in Figure 14 of the effects of court-mandated school finance reforms on school-age per-pupil spending, the likelihood of not entering 12th grade (a proxy for dropout), the likelihood of high school graduation or GED receipt (not the same as high school graduation), and personal income. These figures only use variation in exposure to reforms (not variation in intensity). Because all variation is at the state level, the estimates for each event-study year is imprecise. However we present a linear fit for the pre-reform years and the post-reform years along with the 95 percent confidence intervals.

For all outcomes, the event study plots show (a) minimal pre-reform differences in cohort trends between reform and non-reform states, (b) clear improvement in all outcomes after the passage of a court-ordered school finance reform, and (c) improved outcomes with increased years of exposure. Given that this is only using state-level variation in the timing, these patterns are reassuring. To quantify these patterns, we estimate 2SLS models where we predict outcomes as a function of school spending, conditional on state fixed effects, census region trends, gender, and race-specific cohort trends, and age. We instrument for school spending with indicator variables for the number of years of school-age exposure to reforms. The results are presented in Table 8.

Columns 4 through 6 show that a 10 percent increase in median state per-pupil spending increases the likelihood of graduating from high school or earning a GED by 0.8 percentage points (p-value<0.05), reduced the likelihood of not entering 12th grade (a proxy for dropout) by 0.89 percentage points (p-value<0.05), and increases individual income by \$1,249 (p-value$=0.012$). To improve statistical precision we also exploit some variation in intensity across states. For each state, we compute the median district's predicted spending increase. We then use the interaction between this state median and years of exposure as our excluded instruments. Columns 7 through 9 show that a 10 percent increase in median state per-pupil spending increases the likelihood of graduating high school or having a GED by 0.489 percentage points (p-value<0.1), reduced the likelihood of not entering 12th grade (a proxy for dropout) by 0.498 percentage points (p-value<0.1), and increases individual income by 1,186 dollars (p-value<0.01). Relative to base income levels, this represents a 3 percent earnings increase. Recall that the Census results are based on a single observation per person, so that the estimates will be between one-half and two-thirds of the effect on permanent earnings. As such, the Census estimates imply real earnings effects between 5 and 6 percent. Even though, one might expect very different results using state level spending changes and district level spending changes, the implied earnings effects using the Census data are quite well aligned with the PSID estimate of 7.5 percent.

We have replicated patterns of economically meaningful effects of school spending on adult outcomes across different datasets, for different cohorts, using different sources of variation, and using data at different levels of aggregation. Given the inherent differences between the various checks, unsurprisingly, the point estimates are not identical. However, across all datasets, cohorts, sources of variation, and levels of aggregation we are able to rule out small effects and zero effects—providing compelling evidence that exogenous spending increases improve children's long-run outcomes.

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 compare our estimated naïve OLS regression to the 2SLS regression estimates for all our models.

For all outcomes and subsamples, the OLS estimates are orders of magnitudes smaller than the 2SLS/IV estimates and most of the OLS estimates are not statistically significantly different from zero. Looking at the education outcomes in the PSID sample (Table 3 columns 1 and 4), OLS estimates show no statistically significant relationship between school spending and outcomes. As further evidence of no effects using observational variation, both OLS point estimates are very small and the point estimates for high school graduation and years of education have opposite signs. The OLS estimates for the economic outcomes show a similar pattern in Table 4. The naively estimated school spending effects are close to zero and go in opposite directions – indicating no relationship between potentially endogenous variation in school spending and adult outcomes despite large effects of exogenous spending increases on adult outcomes. To ensure that this is not an artifact of the PSID data we replicate this pattern using the CCD data and the Census data. Using the CCD, the OLS estimates on graduating and dropout are both small, and go in opposite directions. However, the OLS estimate on dropout, while about 20 times smaller than the 2SLS estimate, is statistically significant at the 1 percent level. Finally, the results using the Census data (Table 8) reinforce the difference between using actualized spending and exogenous variation. The OLS effect on graduating from high school is between 4 and 6 times smaller than the 2SLS estimate, and OLS effect on personal income is about 6 times smaller than the 2SLS estimate. Even though the Census analysis uses state level changes in spending, the basic pattern of results holds up across all the datasets and sources of variation.

The stark 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, 1966; Betts, 1995; Hanushek, 1996; and Grogger, 1996). We suspect prior studies that relied on actualized variation in spending may have produced modest estimated effects of school spending due to unresolved endogeneity biases. Indeed, in Table 2, we show that non-instrumented within-district increases in school spending is negatively related to (effect-size weighted index of) childhood family/community SES factors, while instrumented school spending is not. This suggests that OLS estimates are likely biased against finding a positive school spending effect and makes clear the need for exogenous variation in school spending. However, Table 2 also provides another possible reason for the difference in findings: non-instrumented school spending is unrelated to better school inputs while instrumented school spending is. As such, another possible explanation for our findings of large school spending effects is that how the money is spent matters a lot and that exogenous increases in school spending are more closely tied to productive inputs than endogenous increases in school spending.[27] Given that money *per se* will not improve student outcomes (for example, using the funds to pay for lavish faculty retreats will likely not have a positive effect on student outcomes), understanding *how* the increased funding was spent is key to understanding why we find large spending effects where others do not. We

[27] This finding begs the question of why school districts are more likely to reduce class sizes and improve other inputs with an exogenous windfall of school spending than endogenous changes in school spending. While investigating this is outside the scope of this project, one possible explanation with anecdotal support is that teachers' unions may be much more likely to demand higher salaries and smaller class sizes when they know that the district has recently received additional state funding. Indeed, teachers unions in NJ and NY explicitly advise that members use information about state funding to gain leverage for smaller classes and higher salaries. http://finance.tc-library.org/Content.asp?uid=1305, https://www.njsba.org/mo/labor_relations/home.php

explore these issues below.

VI. Exploring Mechanisms.

To shed light on the causal pathways through which various types of education spending affects subsequent adult outcomes, we examine the effects of exogenous spending increases on spending for school support services, physical capital spending, and instructional spending. We also estimate effects on student-to-teacher ratios, student-to-guidance-counselors ratios, teacher salaries, and the length of the school year (key measures employed in the seminal literature on school quality). We employ data on the types of school spending (available for years 1992 through 2010 from the CCD), student staff ratios (available for years 1987 through 2010 from the CCD and Office of Civil Rights), and information on teacher salaries and length of the school year (available approximately every 3 years for years from 1987 through 2010) from the School and Staffing Survey housed at Institute of Education Sciences. The earliest CCD data start in 1987 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 may be instructive.

To determine how each additional dollar associated with reforms was spent, we employ instrumental variables models similar to equation [5] and [6] where the main outcomes are the various school inputs. For ease of interpretation we present effects on the type of expenditure in levels. The interpretation of the estimate is the marginal propensity to spend (i.e., the increase in a particular type of spending associated with a 1 dollar increase in total spending). For all other outcomes we use logs as in the rest of the paper. The endogenous regressor is per-pupil spending or log 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 9.

When a district increases school spending by $1,000 due to reforms, spending on capital increases by $148, spending on instruction increases by $808, and spending on support services increases by $259 on average. While instructional spending makes up about two-thirds of all spending, it accounts for about 80 percent of the marginal increase. This suggests that, on the margin, exogenous increases in school spending are more likely to go to instruction than other spending increases. To account for this increase, districts that experience increases in total spending tend to see declines in other spending (non-instructional, non-support services, non-capital spending). The increases for instruction and support services (which includes expenditures to hire more teachers and/or increase teacher salary along with funds to hire more guidance counselors and social workers) are consistent with the large, positive effects for those from low-income families.

Prior research has emphasized that an important determinant of how much students learn is teacher quality; and, teachers' salaries represent the largest single cost in K-12 education and may exert a direct effect on the ability to attract and retain a high-quality teaching workforce. The largest share of school districts' spending (annual operating budgets--instructional expenditures) is comprised of two components: 1) the number of teachers hired which governs the teacher-student ratio, and 2) the salary schedule (by qualifications--experience and educational background credentials). Accordingly, we next separately estimate effects on average teacher salaries and student-staff ratios. For these models, the endogenous regressor is the natural log of school spending. Districts that increased spending due to reforms see reductions in student-to-teacher ratios. This 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). To show that our effects on student-teacher ratios track the increases in school spending, we linked the school spending data to the PSID sample and augmented these data with student-teacher data at the district level during 1968 through 1977 from the Office of Civil Rights. We then estimated our event-study models on student-teacher ratios. The results are presented in Figure 15. The results clearly show that there were no pre-existing time trends in student teacher ratios, that the decreases in student teacher ratios coincide with the passage of school finance reforms, and the reduction in class size closely track the exogenous reform-induced increases in spending.

We also find that schools in these districts have fewer students per counselor and fewer students per administrator, which have also been found to improve student outcomes (e.g., Reback, 2010; Carell and Carell, 2006). In addition to using student teacher ratios, Card and Krueger (1992) and Loeb and Bound (1996) proxy for school quality with the length of the school year and teacher salaries. We also analyze effects on these measures. The 2SLS estimates indicate that a 10 percent increase in school spending is associated with a 5.3 percent reduction in the student-to-teacher ratio (p-value<0.01), 1.14 more school days (p-value<0.01), and a 2 percent increase in base teacher salaries (p-value<0.01). Insofar as these mechanisms are responsible, at least in part, for the improved student outcomes, these findings stand in stark contrast to studies finding little effect of these measures on student outcomes for cohorts educated after 1950 (Betts, 1995; Betts, 1996; Hanushek, 2001) and are more in line with studies on recent cohorts that use randomized and quasi-random variation in school inputs (e.g. Chetty *et al*, 2013; Fredriksson, *et al*, 2012) – further underscoring the limitations of using observational variation for these important questions.

While there may be other mechanisms through which increased school spending improves student outcomes, the results suggest that the positive effects are driven, at least in part, by some combination of reductions in class size[28], having more adults per student in schools, increases in instructional time, and increases in teacher salary that may have helped to attract and retain a more highly qualified teaching workforce.

VII. Summary Discussion and Conclusion

Previous national studies correlated observed school resources with student outcomes and found little association for those born after 1950 (e.g., Coleman, 1966; Hanushek, 1986; Betts, 1995; Grogger, 1996). This study builds and improves upon previous work by using nationally-representative, individual-level panel data from birth to adulthood (matched with school spending and reform data) and quasi-experimental methods to estimate credible causal relationships. We investigate the causal effect of exogenous school spending increases (induced by the passage of SFRs) on educational attainment, and (eventual) labor market success. For children from low-income families, increasing per-pupil spending yields large improvements in educational attainment, wages, family income, and reductions in the annual incidence of adult poverty. All of these effects are statistically significant and are robust to a rich set of controls for confounding policies and trends. For children from non-poor families, we find smaller effects of increased school spending on subsequent educational attainment and family income in adulthood. 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.

To explore the potential mechanisms from which these spending effects arise, we documented that reform-induced school spending increases were associated with sizable improvements in measured school quality, including reductions in student-to-teacher ratios,

[28] Class sizes are roughly 1.4 times larger than student-teacher ratios, so that our estimates imply a class size reduction of 0.91 students for a 10% spending increase. Fredriksson et al (2012) find that wages are 0.0063 higher for three years with one fewer student in class. If we multiply this by our reduction of 0.91 students for 12 years, this implies a wage increase of 2.3 percent. This implies that just under one-third of our wage effects can be attributed to class size while about two-thirds may be attributed to improvements in other domains.

increases in teacher salaries, and longer school years.[29] These finding parallel those of Card and Krueger's influential 1992 study of males born between 1920 and 1949, and recent studies that link adult outcomes to quasi-experimental variation in school inputs (Fredriksson et al, 2012). The similarities suggest that money still matters and so do school resources.

A suggestive benefit-cost analysis reveals that investments in school spending are worthwhile. Increasing spending by 10 percent for all school-age years increased wages by 7.25 percent each year (Table 4). Someone born in 1975 would start school around 1980 when average per-pupil spending was 5,459 in 2013 dollars. A ten percent increase for 12 years starting in 1980 is equal to $4,850 in present value (assuming a 6 percent discount rate). The median worker in 2013 earned $28,031 so that a 7.2 percent increase in earnings for such a worker between ages 25 and 60 is worth $9,753 in present value. This implies a benefit-cost ratio of 2.01 and an internal rate of return of 8.9 percent. This internal rate of return is similar to those estimated for pre-school programs (Deming, 2009), smaller than estimates of the internal rates of return for class size reductions (Fredriksson et al, 2012), and larger than long-term returns to stocks. In sum, the estimated benefits to increased school spending (that go toward productive inputs) are large enough to justify the increased spending under most reasonable benefit-cost calculations.

Given that school spending levels have risen significantly since the 1970s, our results might lead one to expect to have seen improved outcomes for children from low-income families, and indeed other research suggests this occurred over the relevant time period. For example, Krueger (1998) documents test score increases over time, with large improvements for disadvantaged children from poor urban areas[30]; the Current Population Survey shows declining dropout rates since 1975 for those from the lowest income quartile (Digest of Education Statistics, NCES 2012). Murnane (2013) finds that high school completion rates have been increasing since 1970 with larger increases for black and Hispanic students; Baum, Ma and Pavea (2013) find that postsecondary enrollment rates have been increasing since the 1980s, particularly for those from poor families.[31] Our results suggest increased school spending may have played a key role.

[29] These improvements also likely led to improvements in unobserved teacher quality (Jackson, 2009; 2013).
[30] Note, however, that Reardon (2013) finds that the gap between those at the 90th and 10th percentile of the income distribution (one of many measures of inequality) has been growing over time. He attributes this growth to improvement at the top of the income distribution rather than deterioration at the bottom. Also, his measure does not capture changes at other points in the income distribution. As such, the patterns documented in Reardon (2013) are not inconsistent with improved outcomes for the poor documented in Krueger (1998).
[31] Note all research supports this conclusion. Bailey and Dynarski (2013) find that the gaps in college completion rates

However, because our evidence suggests that exogenous spending increases went toward more productive inputs than endogenous spending increases, the effect of endogenous aggregate increases in school spending may differ from those implied by our estimates.

After Coleman (1966), many have questioned whether money matters, and whether increased school spending can improve the lifetime outcomes of children from disadvantaged backgrounds. Our findings indicate that state school finance reform policies can improve student outcomes and help reduce the intergenerational transmission of poverty. Money alone may not be sufficient, but our findings indicate that provision of adequate funding may be a necessary condition. Importantly, we find that how the money is spent may be important. As such, to be most effective it is likely that spending increases should be coupled with systems that help ensure spending is allocated toward the most productive uses.

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between poor and non-poor students have grown over time. Similar to Reardon (2013), this appears to be driven by more rapid improvement in college completion (as distinct from college entry) for the most affluent. As such, the patterns documented in Bailey and Dynarski (2013) are not inconsistent with improved high school completion and college-going outcomes for poor children documented in Baum, Ma and Pavea (2013) and in aggregate data.

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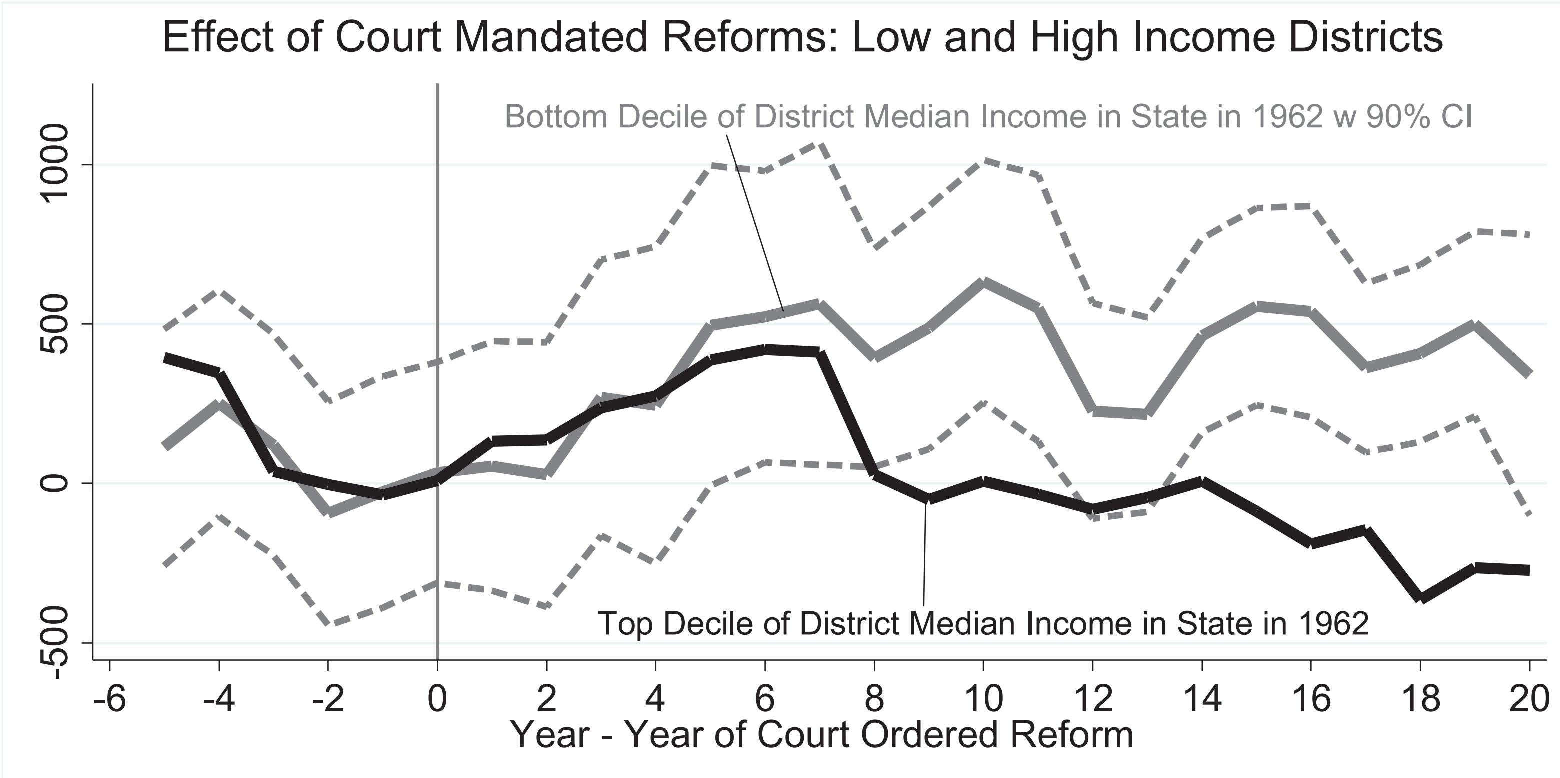
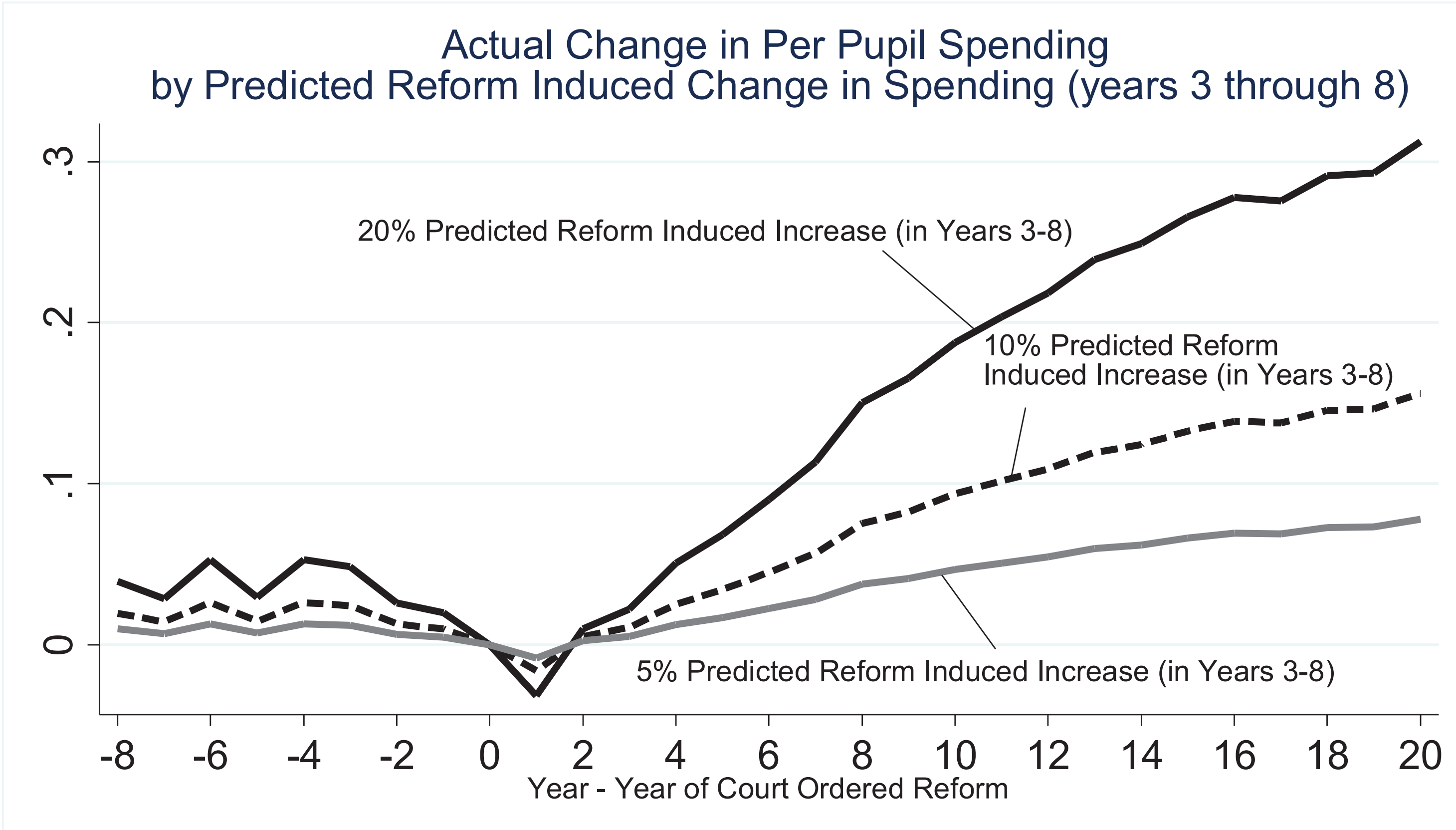
Tables and Figures

Figure 1 *Event Study for Court-Mandated Reforms for High and Low Income Districts (+/- 1 SE)*



Data: The sample includes all school districts in the United States between the years of 1967 and 2010. The sample is made up of 483,047 district-year observations. Each district is weighted by average enrollment for the full sample.
Model: These plots present the estimated event time 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 the first court mandated reform. The event study plots are shown for the top and bottom 10 percent of districts in the state distribution of median district income. The event time plot has been re-centered at zero for the 10 pre-reform years so that the estimated coefficients represent the change in spending relative to the levels that persisted in the 10 years prior to the first reform. Standard errors are adjusted for clustering at the state level.

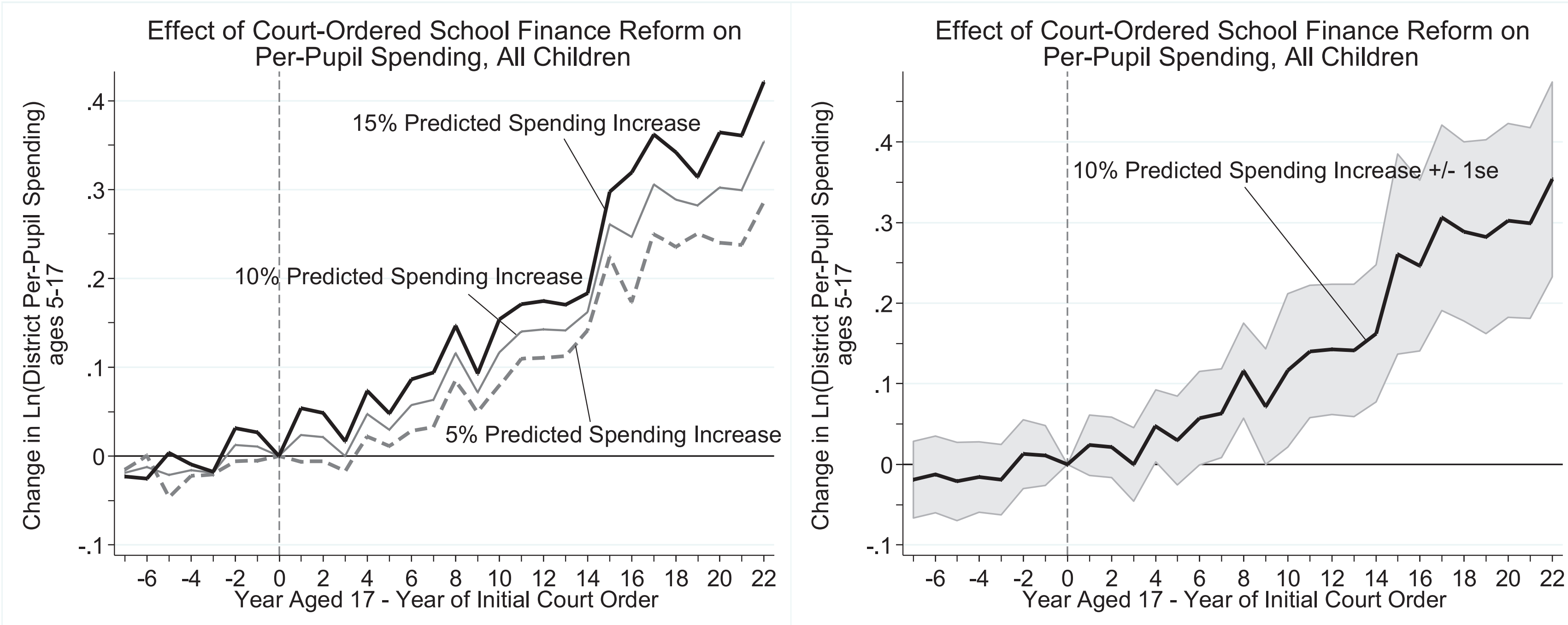
Figure 2: *Effect of Predicted Reform Induced Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Actual Spending Over Time: CCD Population*



Data: The sample includes all school districts in the United States between the years of 1967 and 2010. The sample is made up of 483,047 district-year observations. Each district is weighted by average enrollment for the full sample.

Model: These plots present the estimated event time 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 the first equalization plan. The event study plots are shown for the top and bottom 10 percent of districts in the state distribution of median district income. The event time plot has been re-centered at zero for the 10 pre-reform years so that the estimated coefficients represent the change in spending relative to the levels that persisted in the 10 years prior to the first equalization plan. Standard errors are adjusted for clustering at the state level.

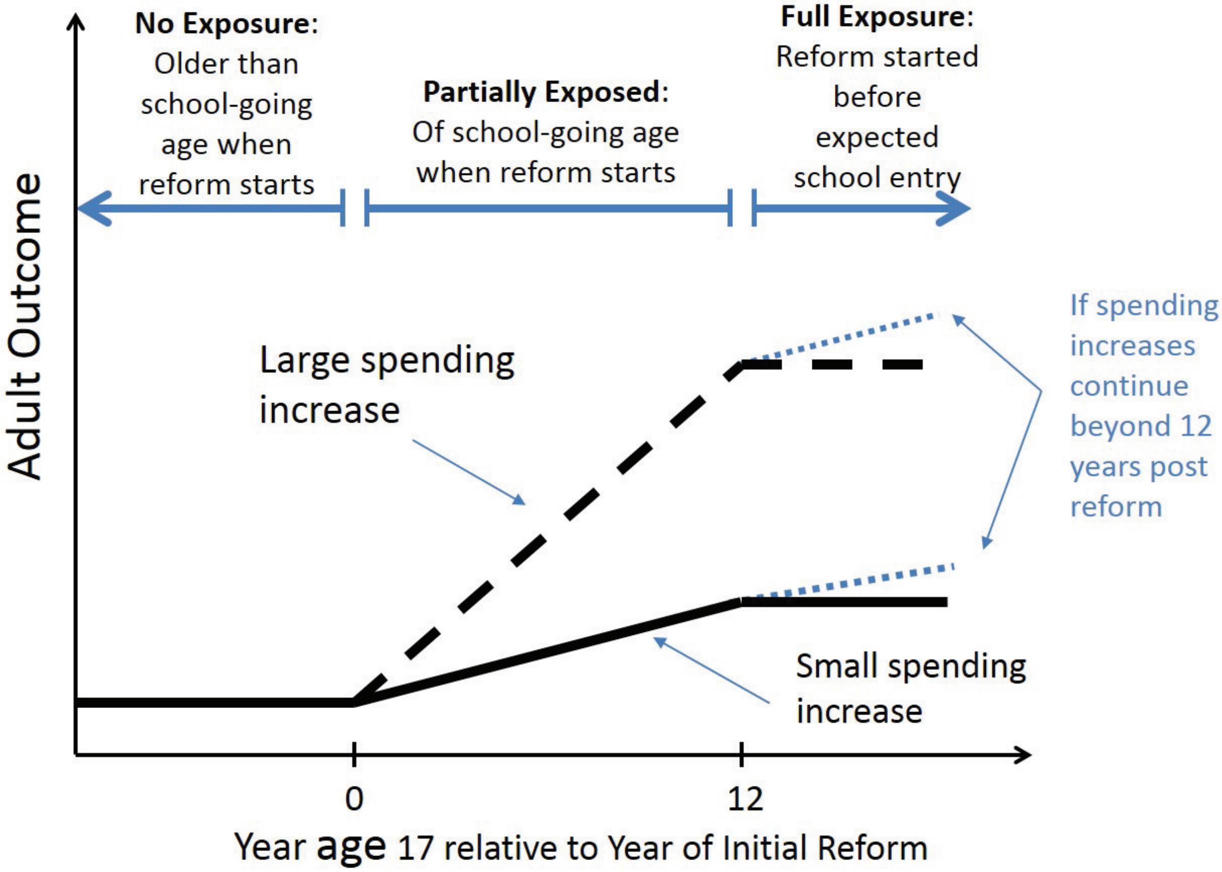
Figure 3: *Effect of Predicted Reform Induced Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Actual Spending Over Time: PSID Sample*



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

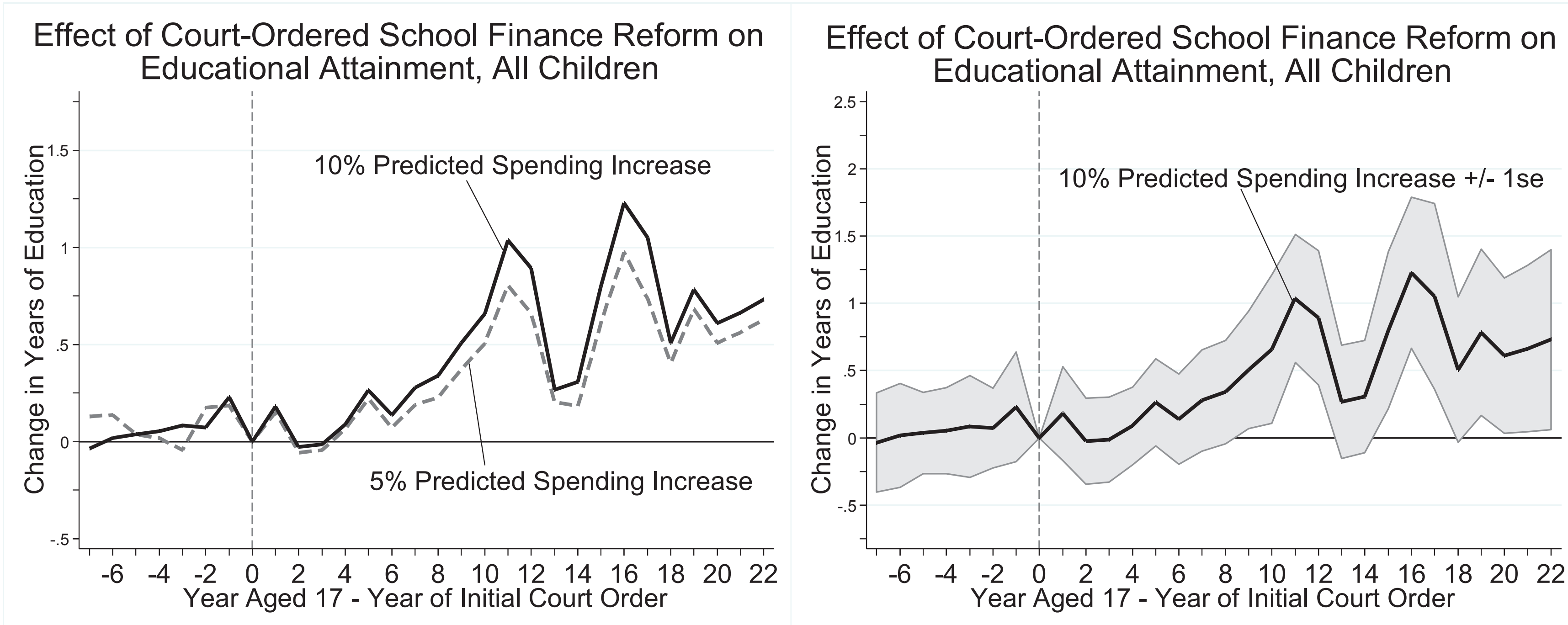
Models: Results are based on non-parametric event-study models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends, controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "War on Poverty" & related safety-net programs, controls for 1960 county characteristics, and controls for childhood family characteristics. Standard errors are clustered at the childhood county level. the event study plot is based on the duration of school-age years of exposure to reform-induced spending changes interacted with the districts predicted spending increase.

Figure 4: *Hypothesized Patterns with a Causal Effect of an Exogenous Increase in Spending*



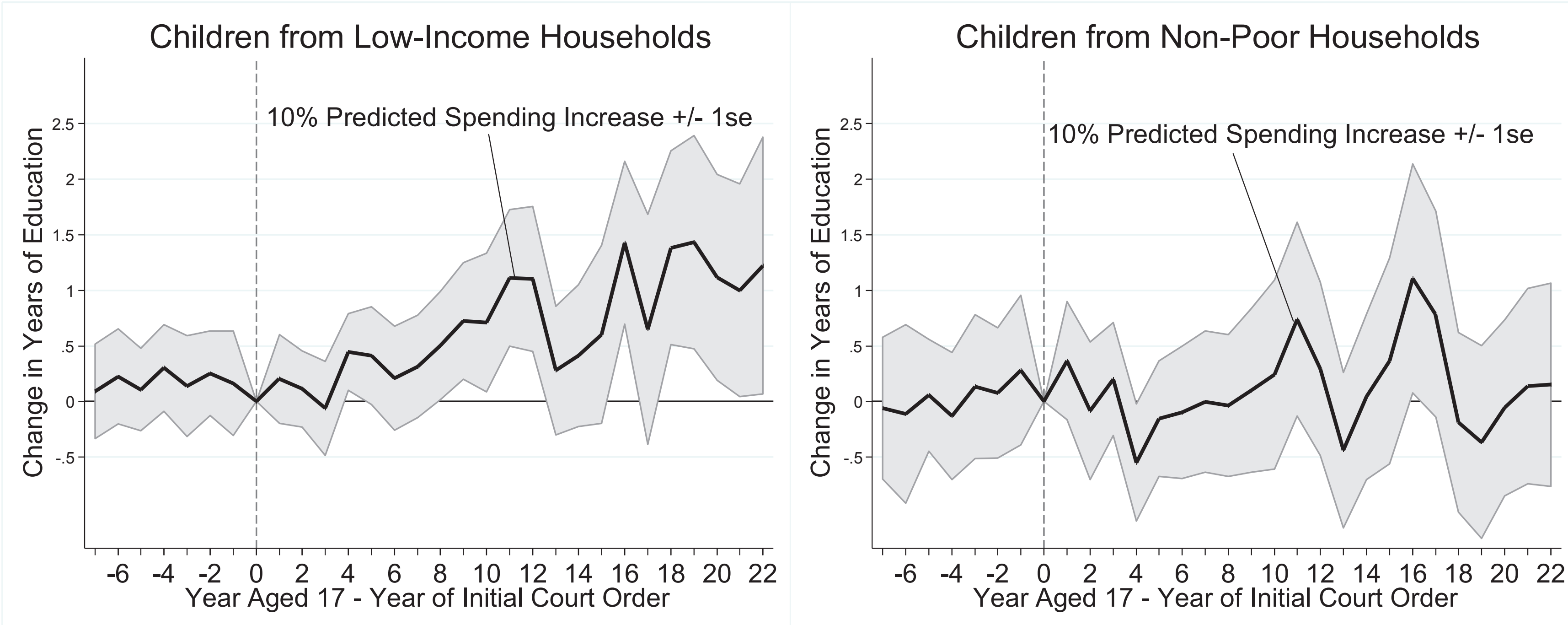
Note: While the stylized figure depicts a linear relationship between years of expose and outcomes this is only for illustrative purposes. We expect that outcome will be weakly monotonically increasing in years of exposure.

Figure 5: *Effect of Predicted Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Years of Completed Schooling*



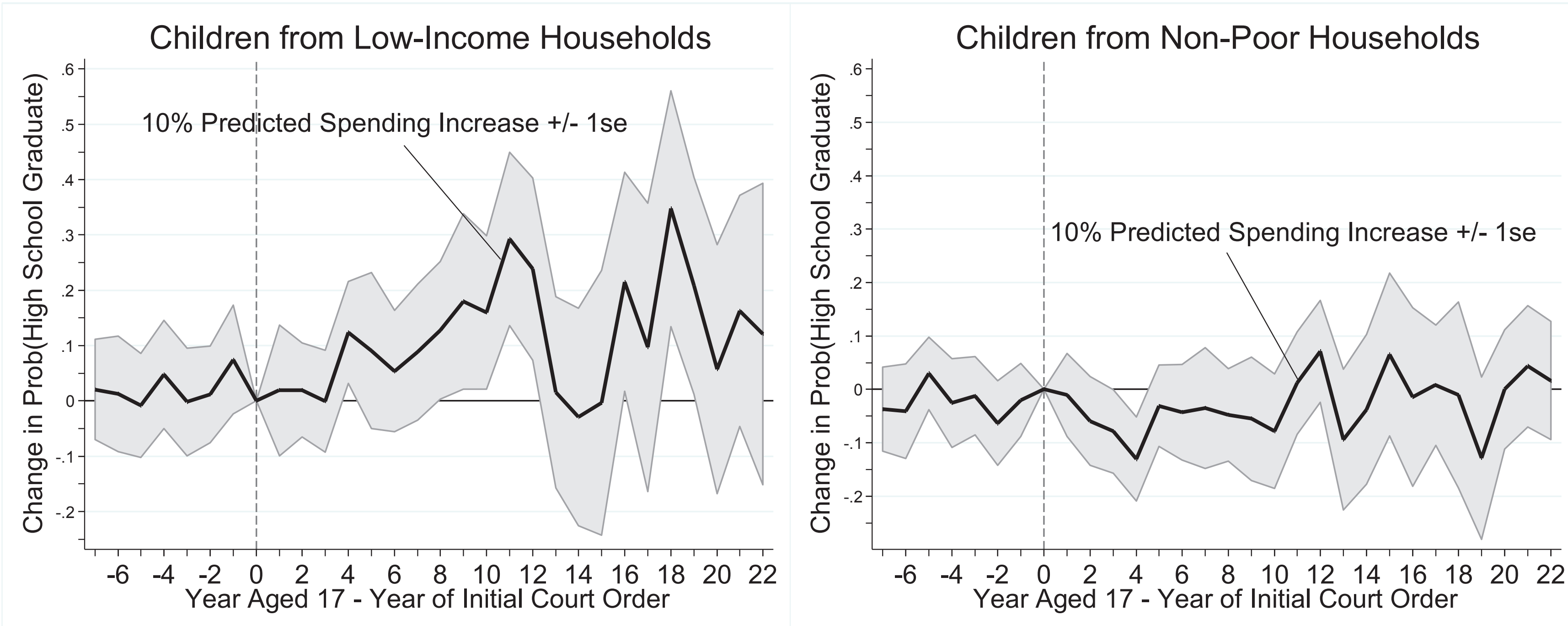
Data: PSID geocode Data (1968-2011), matched with childhood school and district characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011, (N=15,353 individuals from 1,409 school districts (1,031 child counties, 50 states).
Models: Results are based on 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).

Figure 6: *Effect of Predicted Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Years of Completed Schooling: By Childhood Poverty Status*



<u>Data</u>: PSID geocode Data (1968-2011), matched with childhood school and district characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011, (N=15,353 individuals from 1,409 school districts (1,031 child counties, 50 states).
<u>Models</u>: Results are based on 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).

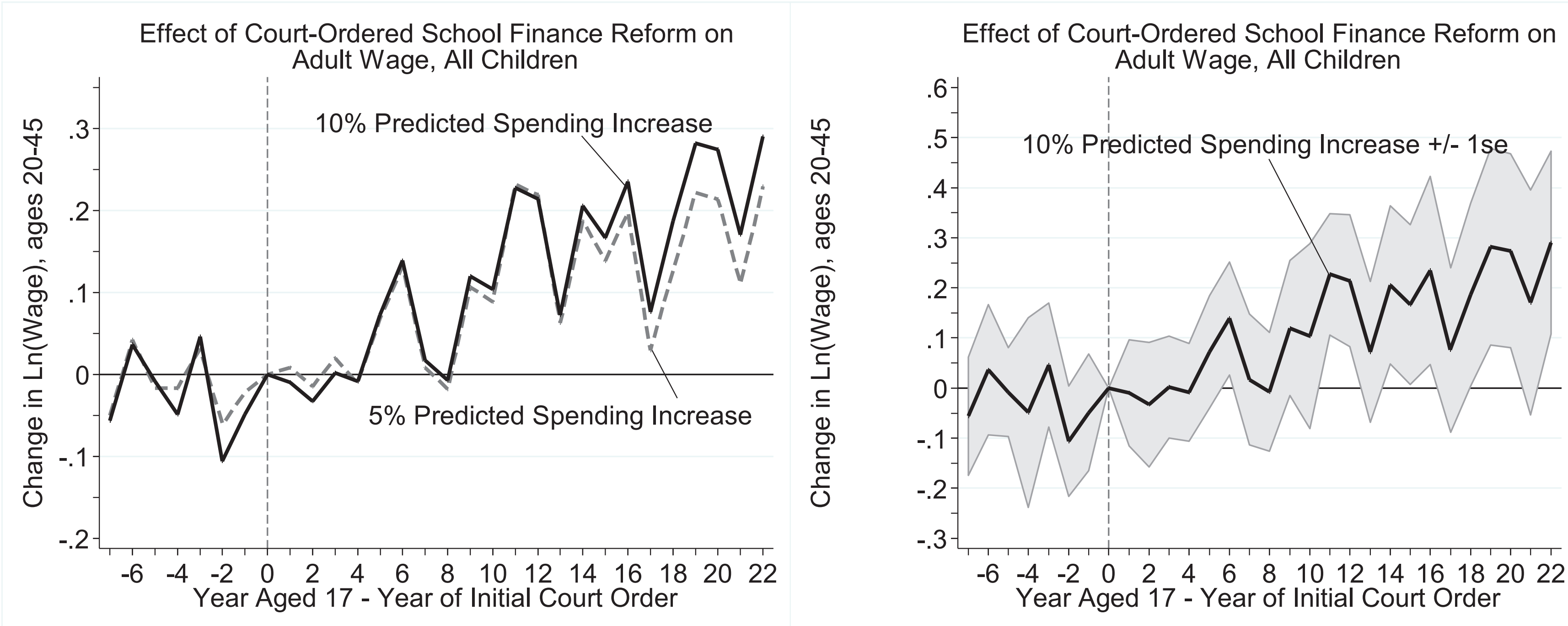
Figure 7: *Effect of Predicted Spending Changes Interacted with Time Relative to the First Court Ordered Reform on High School Graduation: By Childhood Poverty Status*



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

Models: Results are based on 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).

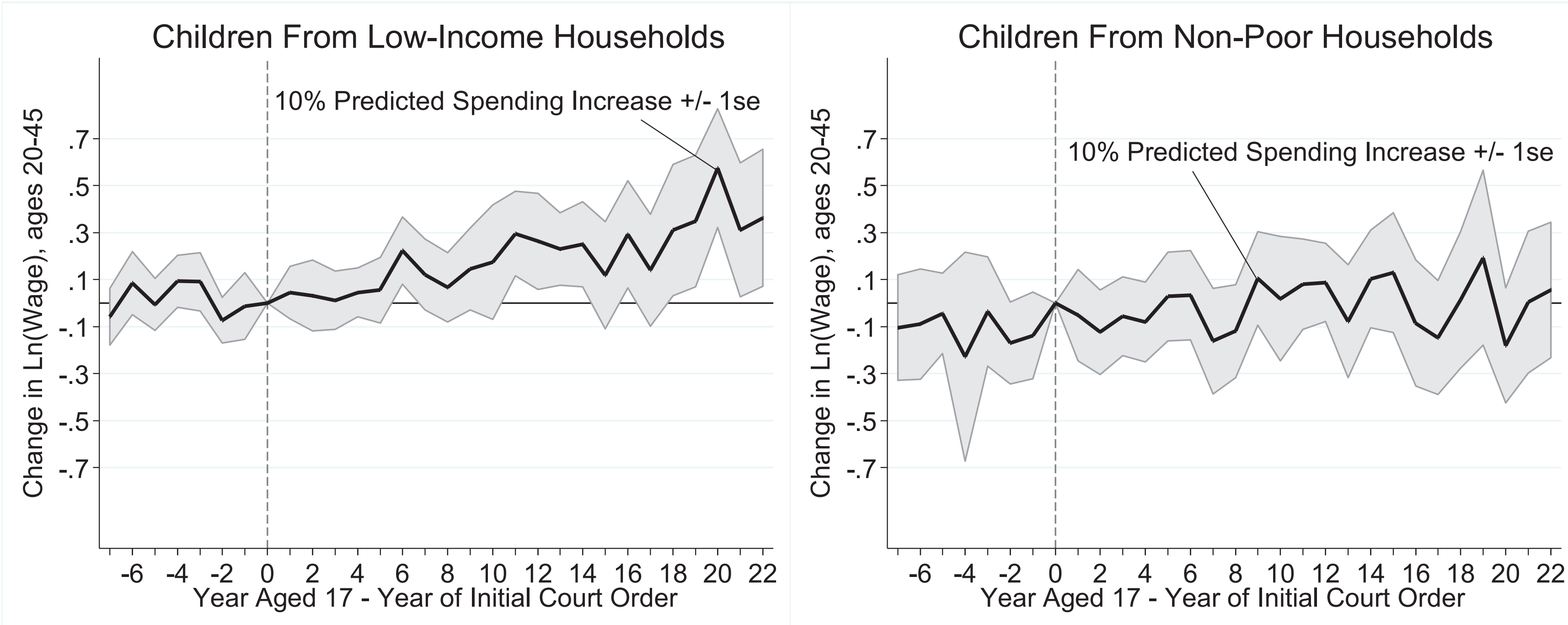
Figure 8: *Effect of Predicted Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Wage*



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

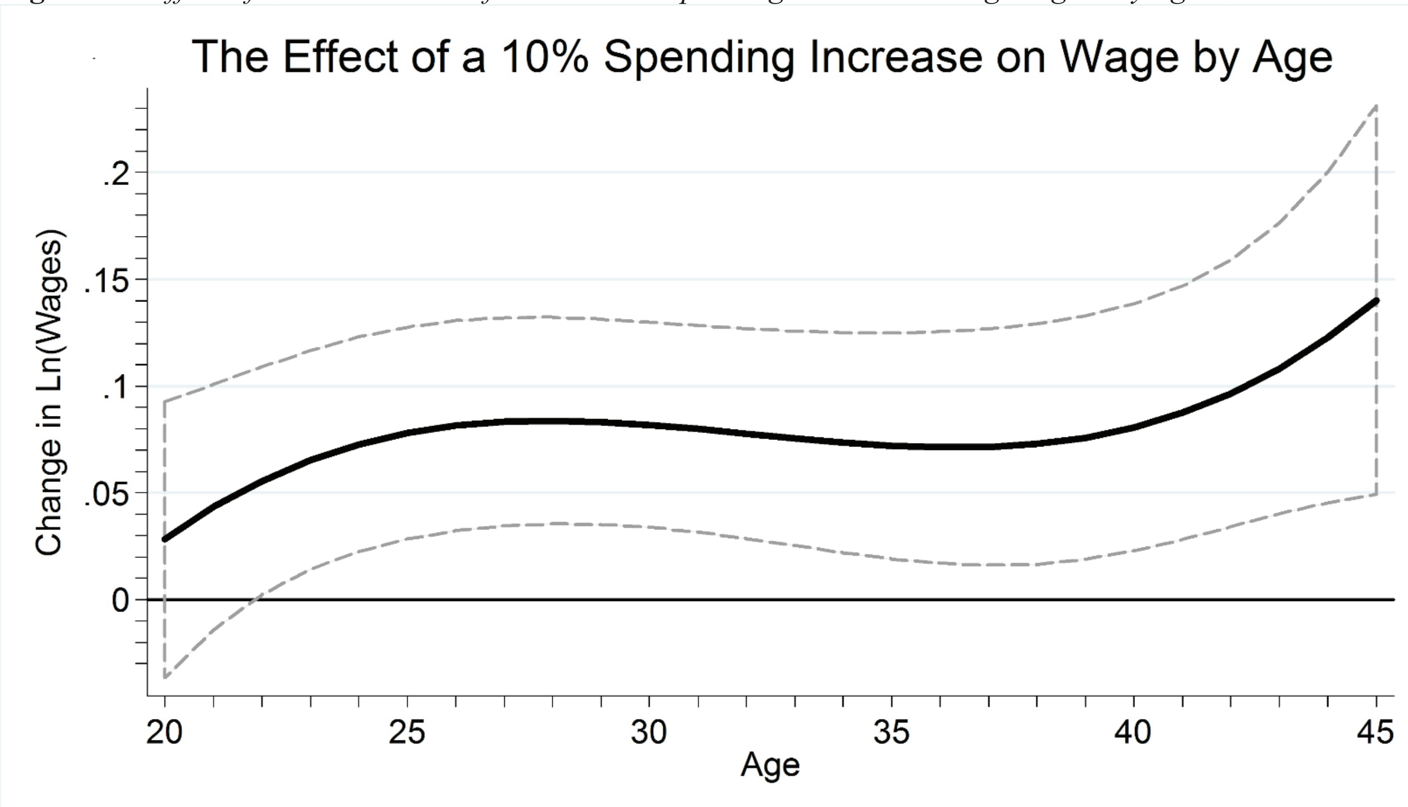
Models: Results are based on event-study models that include: school district fixed effects, race-specific year of birth fixed effects, race*census division-specific linear cohort trends, controls at the county-level for the timing of school desegregation*race, hospital desegregation*race, roll-out of "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).

Figure 9: *Effect of Predicted Reform Induced Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Wage: By Childhood Poverty Status*



Data: PSID geocode Data (1968-2011), matched with childhood school and district characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011, (N=15,353 individuals from 1,409 school districts (1,031 child counties, 50 states).
Models: Results are based on 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).

Figure 10: *Effect of a Ten Percent Reform Induced Spending Increase on Log Wages: By age*

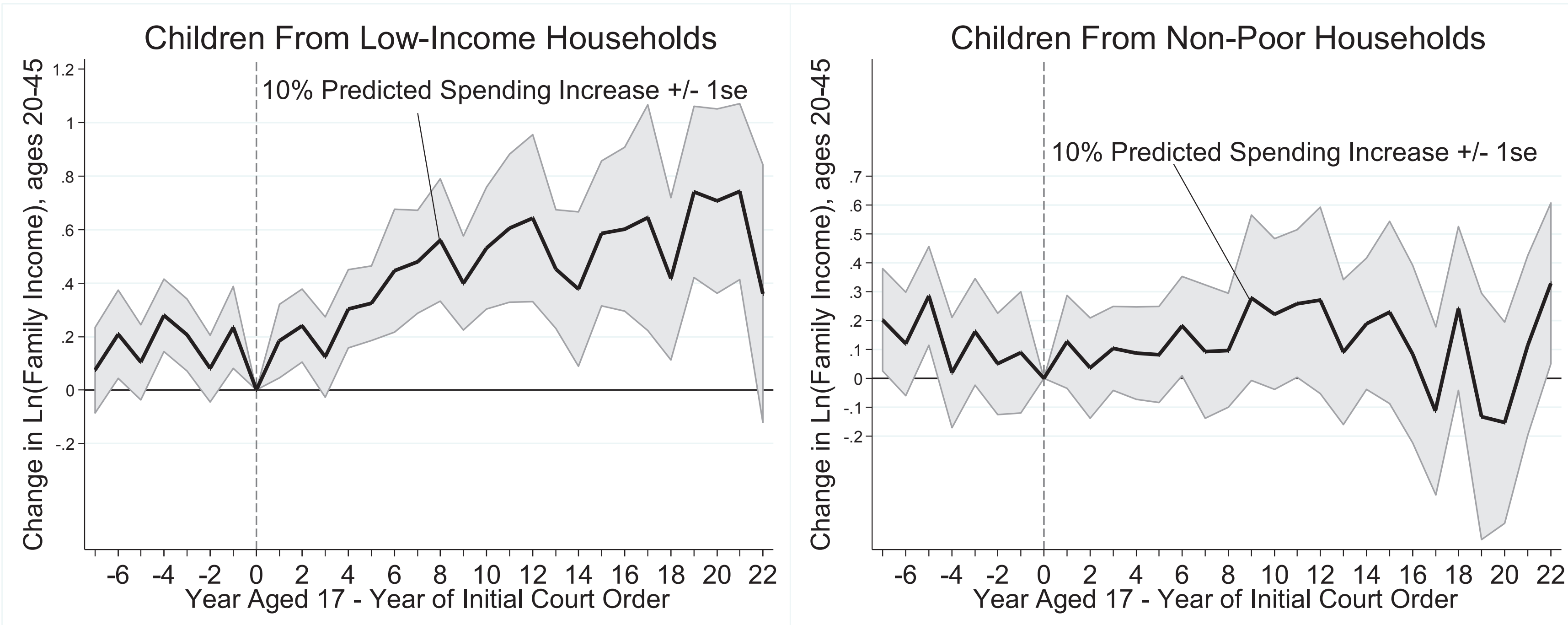


Note: This figure plots the marginal effect of a 10 percent spending increase based on the interaction of per-pupil district spending and a cubic in age. One rejects a linear model at the 5 percent level.

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

Models: Results are based on non-parametric event-study models that include all controls. 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).

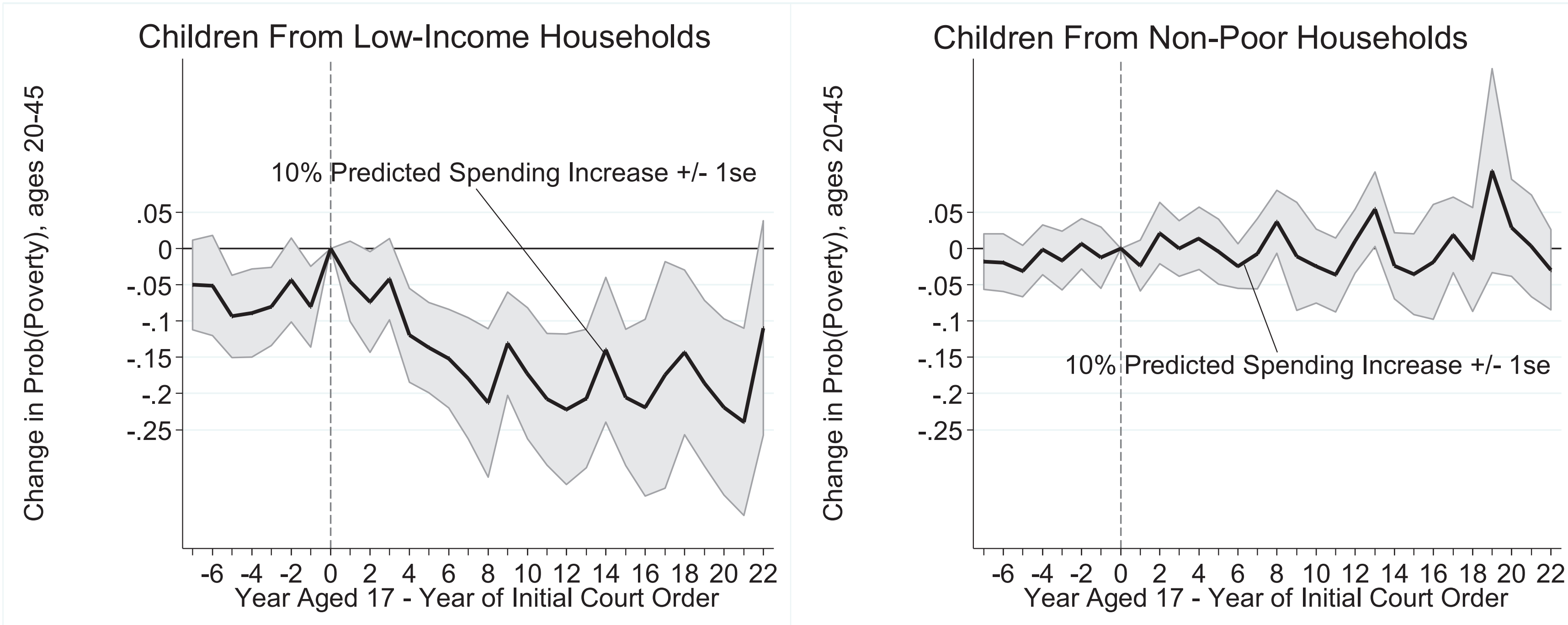
Figure 11: *Effect of Predicted Reform Induced Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Annual Family Income: By Childhood Poverty Status*



Data: PSID geocode Data (1968-2011), matched with childhood school and district characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011, (N=15,353 individuals 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).

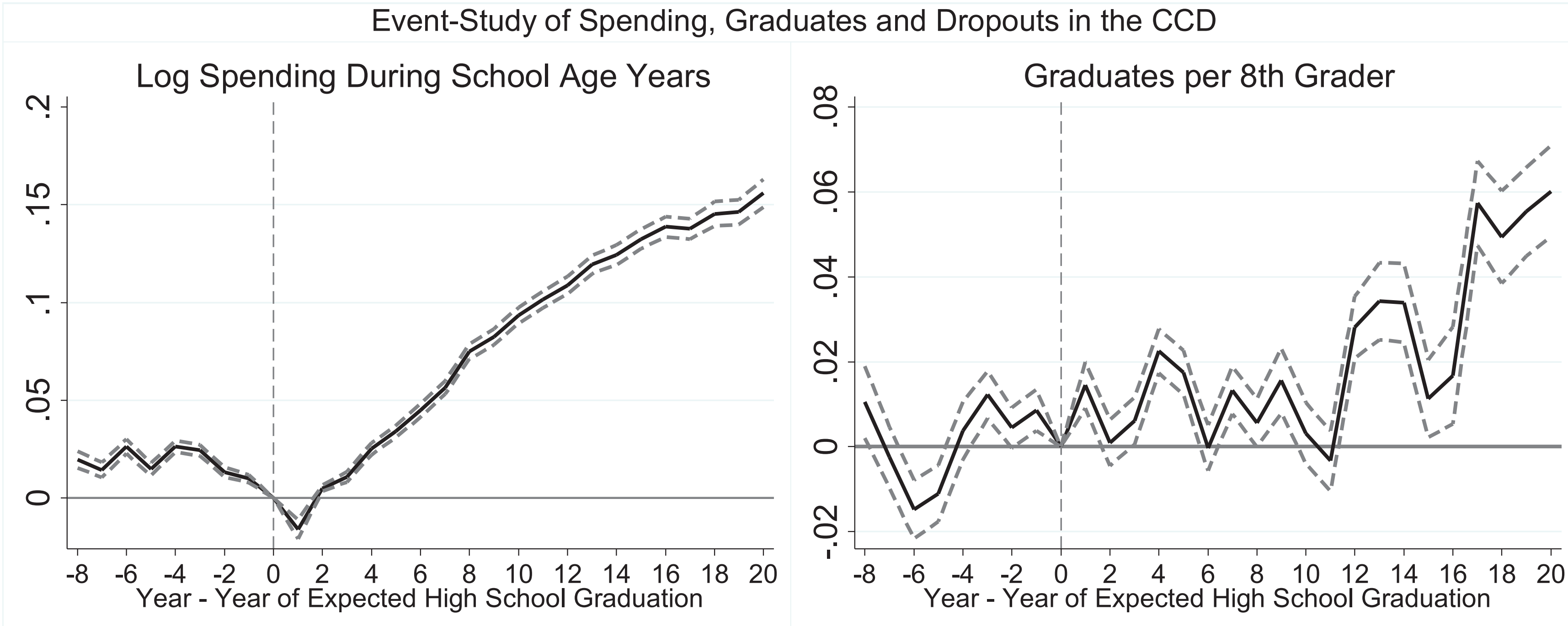
Figure 12: *Effect of Predicted Reform Induced Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Annual Incidence of Adult Poverty: By Childhood Poverty Status*



Data: PSID geocode Data (1968-2011), matched with childhood school and district characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011, (N=15,353 individuals 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).

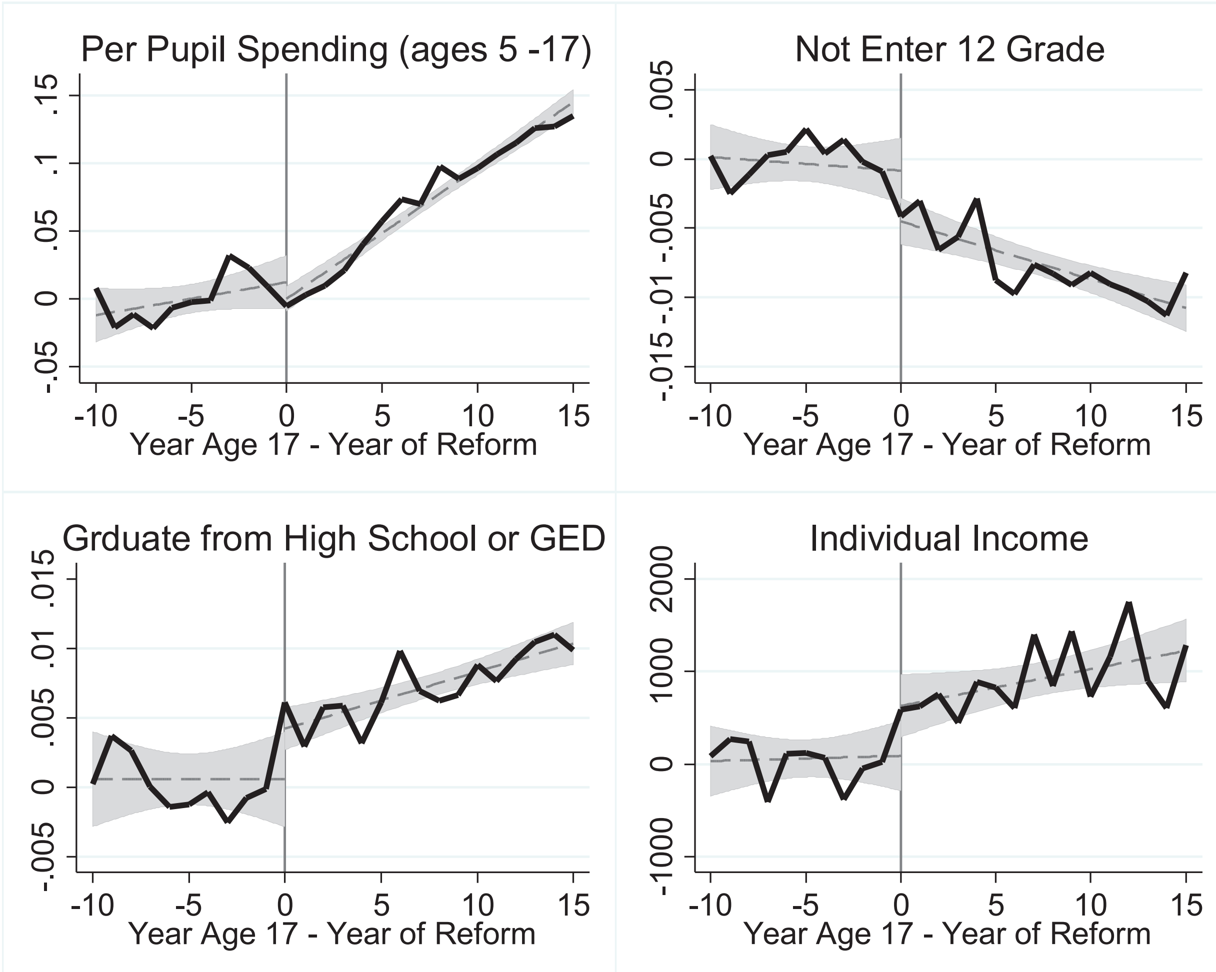
Figure 13: *Effect of Predicted Reform Induced Spending Changes Interacted with Time Relative to the First Court Ordered Reform on High School Graduation Rates: Using the CCD and Full Population of Districts from 1987 – 2010.*



Data: CCD Data from 1987-2010 for 15,353 districts.

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 for 1960 county characteristics (poverty rate, percent black, education, percent urban, population size, each interacted with linear cohort trends. Standard errors are clustered at the state 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).

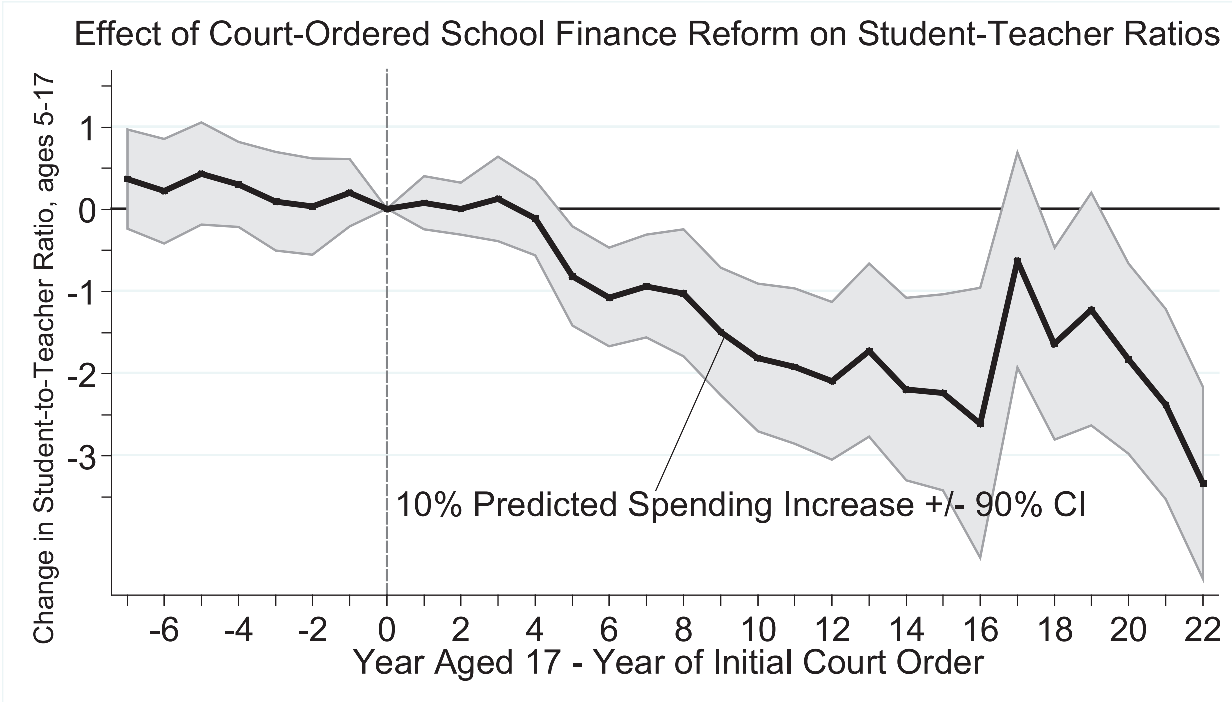
Figure 14: *Effect of Court-Mandated Reforms on Spending and Outcomes: IPUMS Census Data*



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).

Figure 15: *Effect of Predicted Reform Induced Spending Changes Interacted with Time Relative to the First Court Ordered Reform on Student Teacher Ratios*



Data: PSID geocode Data (1968-2011), matched with childhood school and district characteristics. Analysis sample includes all PSID individuals born 1955-1985, followed into adulthood through 2011, (N=15,353 individuals 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).

Table 1: *Descriptive Statistics by Childhood Poverty Status*

	All (N=15,353)	Low-income 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 SFR, age 5-17	0.53	0.53	0.53
Years of Exposure to SFR, 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:			
County 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 dollars.

Table 2: *Effect of Endogenous and Exogenous Spending increases on Predicted Outcomes and School Resources*

	1	2	3	4	5
	Predicted Prob(High School Grad)	Predicted Ln(wage) at age 30	Predicted Prob(High School Grad)	Predicted Ln(wage) at age 30	Pupil-Teacher Ratio, age 5-17
Model: OLS	Birth cohorts and gender controls		Birth cohorts and gender controls with District Fixed Effects		Birth cohorts and gender controls with District Fixed Effects
Spending$_{(age\ 5\text{-}17)}$	**0.0164*** **	**0.0358*** **	**-0.0044** **	**-0.0069+**	-0.0331
	(0.0022)	**(0.0043)**	**(0.0019)**	**(0.0048)**	(0.0482)
Ln(Spending)$_{(age\ 5\text{-}17)}$	**0.0959*** **	**0.1844*** **	**-0.0152+**	-0.0223	-0.4052
	(0.0083)	**(0.0174)**	**(0.0111)**	(0.0205)	(0.2929)
Model: 2SLS/IV					
Spending$_{(age\ 5\text{-}17)}$			-0.0006	0.0177	**-0.8266** **
			(0.0091)	(0.0180)	**(0.3313)**
ln(Spending)$_{(age\ 5\text{-}17)}$			0.0040	0.0834	**-3.6331** **
			(0.0561)	(0.0812)	**(1.5166)**

*** 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 county characteristics. Analysis sample includes all PSID low-income children born 1955-1985, followed into adulthood through 2011 (N=9,035 individuals, 2,945 childhood families, 985 school districts). We estimated models that predict adult earnings using only childhood family/county SES characteristics (including parental income, race, mother's and father's education and occupational prestige index, mother's marital status at birth, birth weight, childhood county-level average per-capita expenditures on Head Start, AFDC, Medicaid, food stamps during school-age years), holding constant school district fixed effects and birth year fixed effects—this is intended to capture an effect-size weighted index of childhood family/community SES factors. We then examined whether individuals' predicted earnings at age 30 based only on childhood family/county characteristics (i.e., the effect-size weighted index of childhood family/community SES factors) is related to the a) non-instrumented average per-pupil school spending during ages 5-17; and b) instrumented average school spending during school-age years, respectively, holding constant school district fixed effects and year of birth fixed effects. The results presented in this Table show that naïve OLS models rely on identifying variation in school spending increases that a) are negatively related to childhood family/county SES characteristics (row 1, column 2); and b) are not significantly related to student-to-teacher ratio (row 1, column 1). In contrast, our instrumented school spending changes (based on the timing and type of SFRs) do not appear to be significantly related to (changes in) childhood family/county SES (rows 3-4, column 2) and are significantly related to student-to-teacher ratios (row 3, column 1).School district per-pupil spending is in thousands of dollars (real 2000 dollars), so that a one-unit change represents a $1,000 change in spending.

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

	Dependent variable:					
	Years of Education			Prob(High School Graduate)		
	OLS	2SLS	2SLS	OLS	2SLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(Spending)$_{(age\ 5\text{-}17)}$	-0.0763	**2.7289****	-	0.0216	**0.9523*****	-
	(0.1474)	**(1.2155)**	-	(0.0278)	**(0.2898)**	-
Ln(Spending)$_{(age\ 5\text{-}17)}$*Low-income	-	-	**4.3548****	-	-	**1.1948*****
	-	-	**(1.8165)**	-	-	**(0.4408)**
Ln(Spending)$_{(age\ 5\text{-}17)}$*Non-Poor	-	-	0.7564	-	-	**0.6208***
	-	-	(1.9200)	-	-	**(0.3538)**
Number of Individuals	15,353	15,353	15,353	15,353	15,353	15,353
Number of Childhood Families	4,586	4,586	4,586	4,586	4,586	4,586
Number of School Districts	1,409	1,409	1,409	1,409	1,409	1,409

Robust standard errors in parentheses (clustered at school district

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

Data: PSID geocode Data (1968-2011), matched with childhood school and county 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 birth year fixed effects; 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). All of these controls are interacted with childhood poverty status (including school district FE). PSID sample weights are used to account for oversampling of low-income families to produce nationally-representative estimates. The first-stage model include as predictors the school-age years of exposure to school finance reform (non-parametric specification) interacted with the respective school district's predicted reform-induced change in school spending based on the timing and type of court-ordered reform interacted with 1967 (within-state) district income percentile category. All event study years that correspond with non-school ages (whether pre-K or ages>=18) are included in 2SLS models as controls and not for identification, using a non-parametric specification and interacted with the respective school district's predicted reform-induced change in school spending. There exists a significant first-stage for all kids (both low-income & Non-Poor kids). The main school spending coefficient in model specifications shown in columns (3) and (6) applies to low-income kids (due to the inclusion of the spending interaction term with Non-Poor kid).

Table 4: *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 adult outcomes are measured between ages 20-45)

	Dependent variable:								
	Ln(Wage)			Ln(Family Income)			Prob(Poverty)		
	OLS	2SLS	2SLS	OLS	2SLS	2SLS	OLS	2SLS	2SLS
	1	2	3	4	5	6	7	8	9
Ln(Spending)$_{(age\ 5\text{-}17)}$	-0.0504	**0.7257*****		-0.0012	**0.9678****		-0.0046	**-0.3671*****	
	(0.0511)	**(0.2637)**		(0.0457)	**(0.3993)**		(0.0105)	**(0.1281)**	
Ln(Spending)$_{(age\ 5\text{-}17)}$*Low-income			**0.9535****			**1.6408*****			**-0.6856*****
			(0.4434)			**(0.6186)**			**(0.2103)**
Ln(Spending)$_{(age\ 5\text{-}17)}$*Non-Poor			0.4333			0.1981			0.0549
			(0.3575)			(0.5612)			(0.1237)
Number of person-year observations	106,545	106,545	106,545	151,349	151,349	151,349	151,756	151,756	151,756
Number of Individuals	13,183	13,183	13,183	14,730	14,730	14,730	14,737	14,737	14,737
Number of Childhood Families	4,454	4,454	4,454	4,588	4,588	4,588	4,588	4,588	4,588
Number of School Districts	1,395	1,395	1,395	1,414	1,414	1,414	1,414	1,414	1,414

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 county 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 birth year fixed effects; 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) and age (quadratic). All of these controls are interacted with childhood poverty status (including school district FE). PSID sample weights are used to account for oversampling of low-income families to produce nationally-representative estimates. The first-stage model include as predictors the school-age years of exposure to school finance reform (non-parametric specification) interacted with the respective school district's predicted reform-induced change in school spending based on the timing and type of court-ordered reform interacted with 1967 (within-state) district income percentile category. All event study years that correspond with non-school ages (whether pre-K or ages>=18) are included in 2SLS models as controls and not for identification, using a non-parametric specification and interacted with the respective school district's predicted reform-induced change in school spending. There exists a significant first-stage for all kids (both low-income & Non-Poor kids). The school spending coefficient shown in columns (3),(6), and (9) applies to low-income kids (due to the inclusion of the spending interaction term with Non-Poor kid).

Table 5: *Effect using only a Single Year of Earnings Data*

	Point in Time Wage Effects Versus Wages over Multiple Time Periods		
	Using 2001 survey Data Only	Using 2010 survey Data Only	All Years
	1	2	3
Ln(Spending)(age 5-17)	0.3699	0.5014	**0.7257*** **
	(1.001)	(1.1368)	**(0.2637)**

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

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

<u>Data</u>: PSID geocode Data (1968-2011), matched with childhood school and county characteristics. Analysis sample includes all PSID children

<u>Models</u>: 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 birth year fixed effects; 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, age (quadratic)). All of these controls are interacted with childhood poverty status (including school district FE). PSID sample weights are used to account for oversampling of low-income families to produce nationally-representative estimates. The first-stage model include as predictors the number of school-age years of exposure to school finance reform interacted with the respective school district's predicted reform-induced change in school spending. All event study years that correspond with non-school ages (whether pre-K or ages>=18) are included in 2SLS models as controls and not for identification. All first stage regressions yield F-statistics above 100.

Table 6: *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* (All children. All adult outcomes are measured between ages 20-45)

	Years of Education	Prob(High School Grad)	Ln(Wage)	Ln(Family Income)	Prob(Poverty)
	1	2	3	4	5
Ln(Spending)(age 5-17)	**2.7222***	**1.0227*****	**0.8254*****	**1.0431****	**-0.2739*****
	(1.4126)	**(0.3567)**	**(0.2623)**	**(0.4559)**	**(0.1044)**
Ln(Spending)(age 20-24)	-2.4950	0.0648	0.4580	0.5105	-0.0543
	(3.1584)	(0.6989)	(0.5200)	(0.6863)	(0.1479)
Ln(Spending)(age 0-4)	0.1527	-0.1131	-0.0810	-0.0591	-0.0761
	(0.7678)	(0.2073)	(0.2164)	(0.2446)	(0.0883)
Number of Individuals	15353	15353	13183	14730	14737
Number of Districts	1409	1409	1395	1414	1414

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 county 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 low-income and Non-Poor kids.

Table 7: *Effects on high School Graduates per 100 8th Graders and Dropout Rate in the CCD and High School Equivalent Holder and Personal Income in the IPUMS Census Data*

	1	2	3	4
	CCD Data			
	Graduates Per 100 8th Graders		Dropout Rate (0-100)	
Mean of Dependent Variable	82.93		3.55	
	OLS	2SLS	OLS	2SLS
Log Spending (age 4-17)	-0.719	**29.37****	**-0.845****	**-16.31****
	(0.784)	**(6.410)**	**(0.294)**	**(1.785)**
Observations	115,369		99,838	

Robust standard errors in parenthesis adjusted for clustering at the school district level.

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

Data: CCD Data (1987-2013).

CCD Models: Results are based on OLS and 2SLS/IV models that include: school district fixed effects; 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. The first-stage model include as predictors the number of school-age years of exposure to school finance reform interacted with the respective school district's predicted reform-induced change in school spending. There exists a significant first-stage.

Table 8: *Effects on High School Equivalent Holder and Personal Income in the IPUMS Census Data*

	1	2	3	4	5	6	7	8	9
	OLS			2SLS[a]: Variation in Timing Only			2SLS[b]: Variation in Timing and Intensity		
	Graduate High School or GED	Less Than 12th Grade	Total Individual Income	Graduate High School or GED	Less Than 12th Grade	Total Individual Income	Graduate High School or GED	Less Than 12th Grade	Total Individual Income
Log Spending (age 4-17)	**0.0127***	**-0.0109+**	1,966	**0.0805***	**-0.0891***	12,495	**0.0489+**	**-0.0498+**	**11,862****
	(0.00645)	**(0.00594)**	(1,5240)	**(0.0389)**	**(0.0379)**	(8,119)	**(0.0287)**	**(0.0266)**	**(2,846)**
Mean of Dep. Variable	0.91	0.07	36,076	0.91	0.07	36,076	0.91	0.07	36,076
First Stage F-statistic	-	-	-	31	31	31	146	146	146
Observations	1,574,299	1,574,299	1,572,377	1,574,299	1,574,299	1,572,377	1,553,838	1,553,838	1,551,936

Robust standard errors in parenthesis adjusted for clustering at the state level.

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

Census Data (1970, 1980, 1990, 2000 – 2011).

Census Models: Results are based on OLS and 2SLS/IV models that include: state of birth fixed effects, year of birth by race fixed effects, race-by-year linear trends, gender fixed effects, and age indicators. In 2SLS[a] the first-stage model include as predictors the number of school-age years of exposure to school finance reform. In 2SLS[b] the first-stage model include as predictors the number of school-age years of exposure to school finance reform interacted with the median district-predicted reform-induced change in school spending in the state.

Table 9: *Evidence on Mechanisms (Using Data from Multiple Sources)*

	1	2	3	4	5	6	7	8	9
Dependent Variable	Capital Spending Per Pupil	Instructional Spending Per Pupil	Services Spending Per Pupil	Other Spending Per Pupil	Students per teacher	Students per guidance counselor	Students per admin	Length of school year	Teacher base salary
Source of data	INDFIN	Common Core Data			Common Core Data			School and Staffing Survey	
Years outcome available	Annually 1972 - 2010	Annually 1987 - 2010			Annually 1987 - 2010			1987, 1990, 1993, 1999, 2003, 2007, 2011	
Spending	**0.148****	**0.808****	**0.259****	**-0.0880****					
	(0.0240)	**(0.0704)**	**(0.0449)**	**(0.0163)**					
Ln(Per Pupil Spending)					**-6.449****	**-422.3****	**-261.0***	**11.47****	**5,308****
					(0.544)	**(76.96)**	**(121.4)**	**(4.050)**	**(1,472)**
Observations	457,820	234,882	234,882	234,882	269,332	180,684	201,125	21,945	21,754
Number of did	13,472	13,306	13,306	13,306	13,398	11,539	12,503	6,356	6,291
Mean of Dep var	838.8	6175	3473	428.5	12.23	414.2	245.5	179.3	26272

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

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

Models: Results are based on 2SLS/IV models that include: school district fixed effects; 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. The first-stage model include as predictors the number of school-age years of exposure to school finance reform interacted with the respective school district's predicted reform-induced change in school spending. There exists a significant first-stage.

Appendix A

Table A1: Supreme Court Rulings on the Constitutionality of School Finance Systems from 1967-2010

State	First Case Name, Year	Second Case Name, Year	Third Case Name, Year	Fourth Case Name, Year	Fifth Case Name, Year
Alabama	Alabama Coalition for Equity v. Hunt; Harr v. Hunt, 1993				
Alaska	Kasayulie v. Alaska, 1999				
Arizona	Roosevelt v. Bishop, 1994	Roosevelt v. Bishop, 1997	Roosevelt v. Bishop, 1998	Flores v. Arizona, 2007	
Arkansas	Dupree v. Alma School District No. 30, 1983	Lake View v. Arkansas, 1994	Lake View School District, No. 25 v. Huckabee, 2002	Lake View School District, No. 25 v. Huckabee, 2005	
California	Serrano v. Priest, 1971	Serrano v. Priest, 1977	Eliezer Williams, et al., vs. State of California, et al, 2004		
Colorado	None				
Connecticut	Horton v. Meskill, 1978	Horton v. Meskill, 1982	Sheff v. O'Neill, 1995	Coalition for Justice in Education Funding, Inc v. Rell, 2010	
Delaware	None				
Florida	None				
Georgia	None				
Hawaii	None				
Idaho	Idaho Schools for Equal Educational Opportunity v. State, 1998	Idaho Schools for Equal Educational Opportunity v. State, 2005			
Illinois	None				
Indiana	None				
Iowa	None				
Kansas	Knowles v. State Board of Education, 1972	Montoy v. State, 2005			

Kentucky	Rose v. The Council for Better Education, Inc., 1989				
Louisiana	None				
Maine	None				
Maryland	Bradford v. Maryland State Board of Education, 2005				
Massachusetts	Mc Duffy v. Secretary of the Executive Office of Education, 1993				
Michigan	Durant vs State of Michigan, 1997				
Minnesota	None				
Mississippi	None				
Missouri	Committee for Educational Equality v. Missouri, 1993				
Montana	Helena Elementary School District No. 1 v. State of Montana, 1989	Montana Rural Ed. Association v. Montana, 1993	Columbia Falls Public Schools v. State, 2005	Montana Quality Education Coalition v Montana, 2008	
Nebraska	None				
Nevada	None				
New Hampshire	Claremont New Hampshire v. Gregg, 1993	Claremont v. Governor, 1997	Claremont v. Governor, 1999	Claremont v. Governor, 2002	Londonderry School District v. New Hampshire, 2006
New Jersey	Robinson v. Cahill, 1973	Robinson v. Cahill, 1976	Abbott v. Burke, 1990	Abbott v. Burke, 1991	Abbott v. Burke, 1994
New Mexico	Zuni School District v. State, 1998				
New York	CFE v. State, 2003	CFE v. State, 2006			
North Carolina	Leandro v. State, 1997	Leandro v. State, 2004			
North Dakota	None				
Ohio	DeRolph v. Ohio, 1997	DeRolph v. Ohio, 2000	DeRolph v. Ohio, 2002		
Oklahoma	None				

Oregon	Pendleton School District v. State of Oregon, 2009				
Pennsylvania	None				
Rhode Island	None				
South Carolina	Abbeville County School District v. State, 2005				
South Dakota	None				
Tennessee	Tennessee Small School Systems v. McWheter, 1993	Tennessee Small School Systems v. McWheter, 1995	Tennessee Small School Systems v. McWheter, 2002		
Texas	Edgewood Independent School District v. Kirby, 1989	Edgewood Independent School District v. Kirby, 1991	Carrollton-Farmers v. Edgewood, 1992	West Orange-Cove Consolidated ISD v. Nelson, 2004	
Utah	None				
Vermont	Brigham v. State, 1997				
Virginia	None				
Washington	Seattle School District No. 1 of King County v. State, 1977	Seattle II, 1991	Federal Way School District v. State of Washington, 2007		
West Virginia	Pauley v. Kelly, 1979	Pauley v. Bailey, 1984	Pauley v. Gainer, 1995		
Wisconsin	Buse v. Smith, 1976				
Wyoming	Washakie v. Herschler, 1980	Campbell v. State, 1995	Campbell II, 2001		

Appendix B: *Coverage of School Districts in our 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.

Figure A1 below shows the number of district observations in our data for each year. The bars highlighted in red are the census of government years employed in previous national studies of school finance reforms (e.g. Card and Payne 2002, Hoxby 2001, Murray Evans and Schwab 1998). While the coverage of the data we use is arguably better that that used previously, it is not perfect. As shown in Appendix figure A1, for years, 1967, 1970, 1971, 1972, 1972, 1975, 1976, and 1978 only about 40 percent of districts are present (often larger districts). After 1979 almost all districts are included. To assuage concern that our results are affected by the composition of districts for these few years with incomplete coverage, we estimate the main event study models using only data after 1979. As one can see in figure A2, the pattern of results is similar when the sample is restricted to only observations after 1979.

Figure B1: *The number of district observations for each year.*

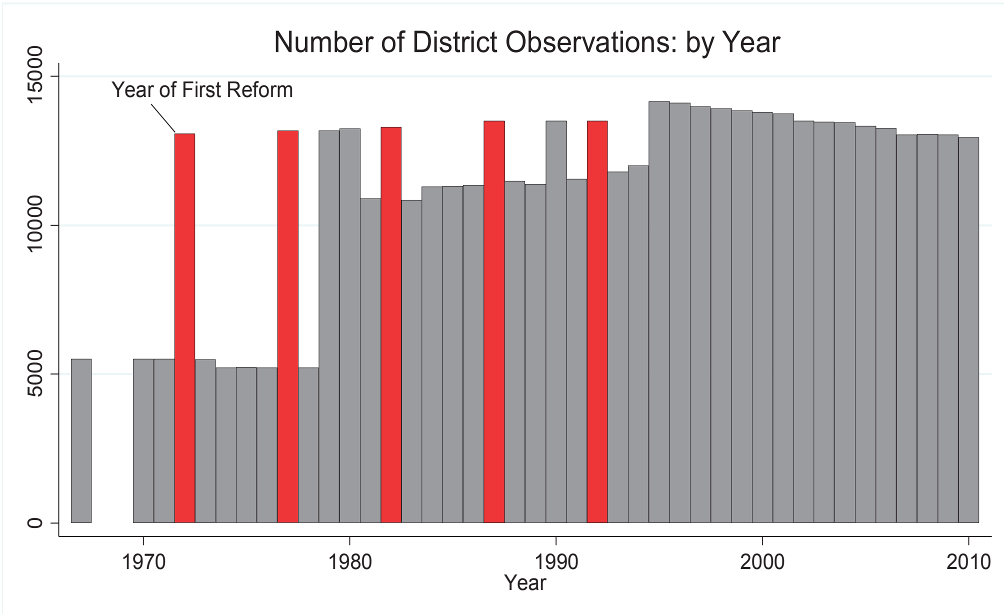
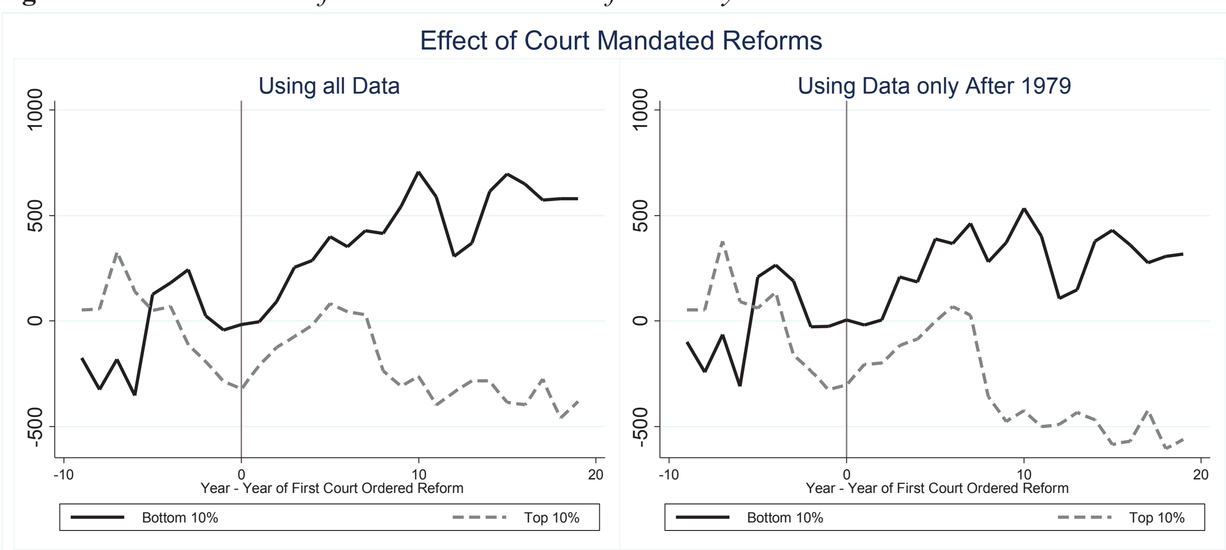


Figure B2: *The number of district observations for each year*



Appendix C: *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 the timing of spending and tax limits came from Downes and Figlio (1998). We also supplemented this with data from *PSFP* for years after those covered in Downes and Figlio (1998).

Appendix D: *The Effect of Introducing Different Types of Funding Formulas by Income in 1963*

As pointed out in Hoxby (2001), the effect of a reform on school spending depends on the type of school funding formula introduced by the reform. We follow the typology outlined by Jackson, Johnson, and Persico (2014b) and categorize funding formulas into four main types; foundation, equalization, reward for local effort, and spending limits. To show how these formula types affect school spending, we estimate the same event study model as described in [1] where timing of the specific formula change is used (e.g. year relative to the first introduction of a foundation plan). Even though we discus each reform type in turn, the event-study figures for each reform type are based on a model that includes the effect of all four reform types simultaneously.

First we consider formulas that impose **spending limits**. Under such plans, the state imposes a limit on how much a district may spend on education. In addition, some plans take away all tax revenues raised above a certain amount. A key feature of such formulas is that at the limit districts face a zero inverted tax price – that is, a district receives zero additional dollars from raising one dollar in local revenue. Figure D1.a shows the event study for state plans that impose spending limits for districts in the top and bottom of the median income distribution in 1963. For the poorest districts, the spending limit reduces spending by \$15.39 (p-value=0.946) in the 10 years after reforms and by \$910.63 (p-value=0.01) after 20 years. For high-income districts, spending limits reduce spending by \$535.91 (p-value<0.01) in the 10 years after reforms and by \$1,494.96 (p-value<0.01) after 20 years. Spending limits reduce per-pupil spending for all districts in the long run with the most pronounced effect in the more affluent districts.

On the other side of the policy spectrum are "**reward for local effort**" formulas that encourage local districts to increase per-pupil spending with matching funds. These formulas affect tax prices directly. These plans provide greater incentives for lower-income and low-wealth districts to increase taxes by allowing such districts to have more than one dollar in spending for each dollar raised in taxes.[32] Figure D1.b provides the event study for this kind of formula change. There is evidence of a downward pre-trend. However, relative to trend, there is a clear spending increase for all districts. Because of the pre-trend, a simple pre-post comparison of levels is not meaningful. However, because both high- and low-income districts were on a similar pre-reform trend, one can test for differential effects by testing for a change in the spending gap. Consistent with the aims of these plans, spending does increase relative to trend and these plans reduce the spending gap between low- and high-income districts in the long run by \$295.83 (p-value=0.11).

The third kind of formula we consider are **foundation formulas**. 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, but do redistribute resources to provide extra funding to low-income/low-wealth districts. Figure D1.c presents the event study for foundation plans. Foundation plans tended to be introduced in states that saw increased school spending. While the effect on spending levels is difficult to discern given the pre-existing trends, 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.

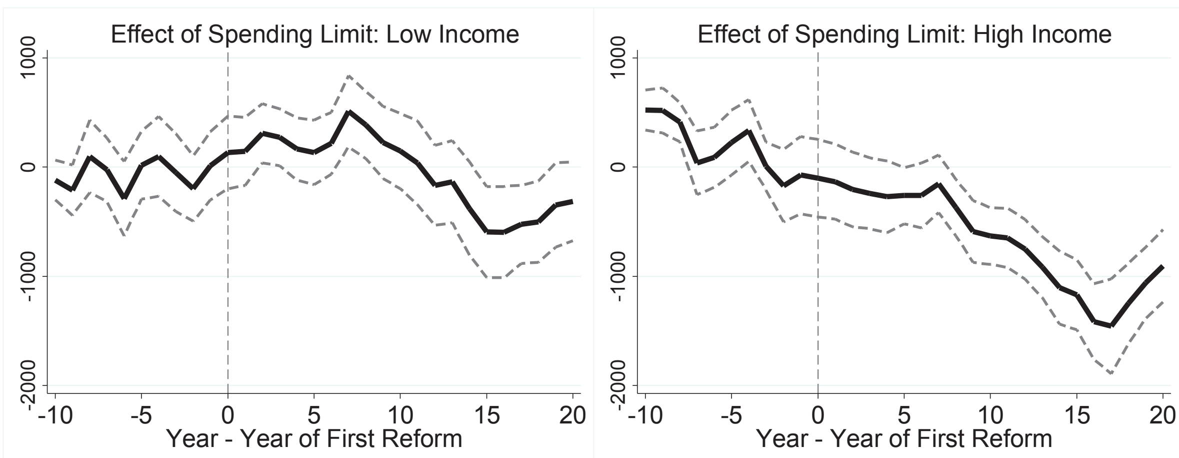
Finally, we turn to **equalization formulas**. Equalization plans redistribute locally raised and state funds. They provide extra funding to low- income or wealth districts while possibly taking money away from high-income or wealthy districts. These plans do not affect tax prices directly

[32] 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.

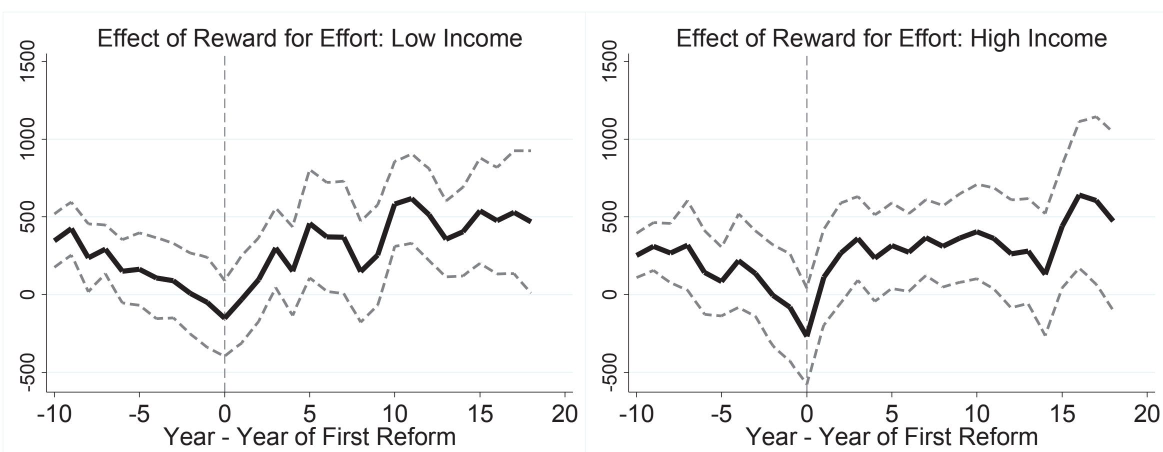
although they may provide incentives to alter the tax base. Figure D1.d presents the event study for equalization plans. There is evidence of a positive pre-trend for both district types. However, the gaps in spending between low- and high-income districts was stable prior to reforms. Focusing on the differential effect by district income, the gap in spending associated with these reforms between the low- and high-income districts was reduced by \$342.22 (p-value<0.01) in the 10 years after reforms. However, as evidenced by the figure, this effect may not persist over time.

Figure D1: *Event Study for Different Kinds of Formula Changes*

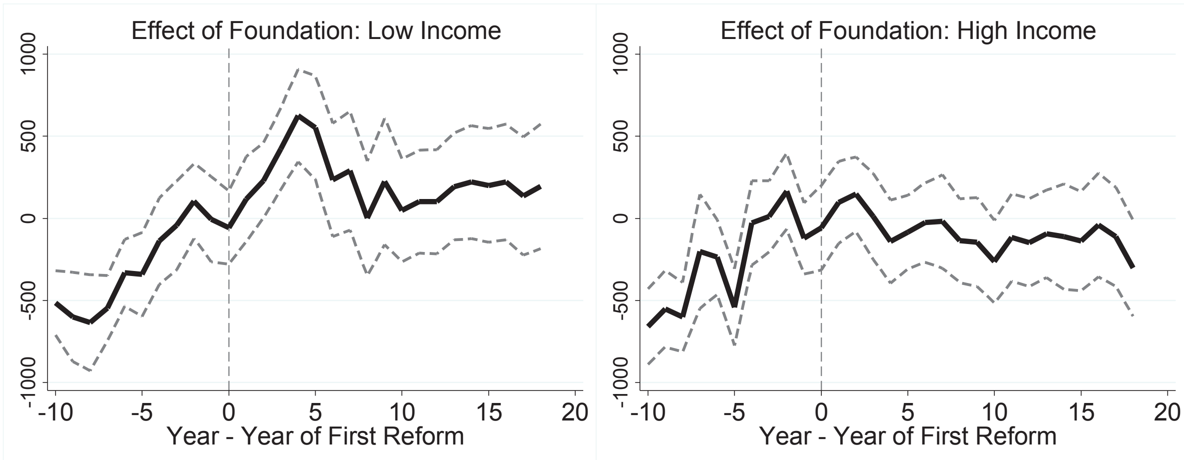
a. *Event Study for Spending Limits for High and Low Income Districts (+/- 1 SE)*



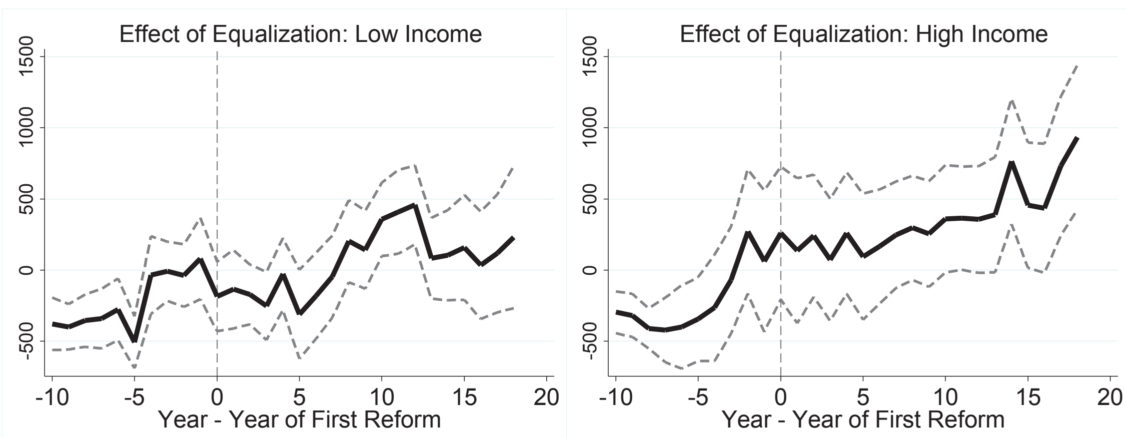
b. *Event Study for Reward for Effort for High and Low Income Districts (+/- 1 SE)*



c. *Event Study for Foundation Plans for High and Low Income Districts (+/- 1 SE)*



d. *Event Study for Equalization Plans for High and Low Income Districts (+/- 1 SE)*



<u>Data:</u> The sample includes all school districts in the United States between the years of 1967 and 2010. The sample is made up of 483,047 district-year observations. Each district is weighted by average enrollment for the full sample.
<u>Model:</u> These plots present the estimated event time 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 the first court mandated reform. The event study plots are shown for the top and bottom 10 percent of districts in the state distribution of median district income. The event time plot has been re-centered at zero for the 10 pre-reform years so that the estimated coefficients represent the change in spending relative to the levels that persisted in the 10 years prior to the first reform. Standard errors are adjusted for clustering at the state level.

Appendix E: *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 1969 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 1969-1970-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 F: 2SLS Effects Using the Absolute Level of Per Pupil Spending (as opposed to logs)

Table F1: *2SLS Effects of Spending Levels on Outcomes*

	Years of Education		Prob(High School Grad)		Ln(Wage), age 20-45		Ln(annual Family Income), age 20-45		Prob(Adult Poverty), age 20-45	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Spending$_{(age\ 5\text{-}17)}$	**0.4905****		**0.1764*****		**0.1451*****		**0.1826****		**-0.0811*****	
	(0.2473)		**(0.0670)**		**(0.0547)**		**(0.0799)**		**(0.0267)**	
Spending (age 5-17)*Low Income		**0.7853****		**0.1900***		**0.1932****		**0.3239****		**-0.1458*****
		(0.3737)		**(0.1032)**		**(0.0886)**		**(0.1280)**		**(0.0451)**
Spending (age 5-17)*Non Poor		0.1212		**0.1503***		0.0875		0.0214		0.0039
		(0.4146)		**(0.0775)**		(0.0767)		(0.1241)		(0.0254)
Number of person-year observations	--	--	--	--	106,545	106,545	151,349	151,349	151,756	151,756
Number of Individuals	15,353	15,353	15,353	15,353	13,183	13,183	14,730	14,730	14,737	14,737
Number of Childhood Families	4,586	4,586	4,586	4,586	4,454	4,454	4,588	4,588	4,588	4,588
Number of School Districts	1,409	1,409	1,409	1,409	1,395	1,395	1,414	1,414	1,414	1,414

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 children 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 birth year fixed effects; 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 and timing of tax limit policies; 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). All of these controls are interacted with childhood poverty status (including school district FE). PSID sample weights are used to account for oversampling of poor families to produce nationally-representative estimates. The first-stage model include as predictors the school-age years of exposure to school finance reform (non-parametric specification) interacted with the respective school district's predicted reform-induced change in school spending based on the timing and type of court-ordered reform interacted with 1967 (within-state) district income percentile category. There exists a significant first-stage for all kids (both low-income and non-poor kids). The (instrumented) school spending variable is in $000s (CPI-deflated real 2000 dollars), so that a one-unit change represents a $1,000 school spending increase experienced in each school-age year between ages 5-17 (which corresponds to roughly a 20% spending increase throughout K-12 years for this sample period).

Appendix G: *Checking Robustness to Migration after Reforms*

Table G1: *2SLS/IV Estimates of Court-Ordered School Finance Reform Induced Effects of Per-Pupil Spending on Long-Run Outcomes: Using only Observations with Addresses Before the First Reform* (All children. All adult outcomes are measured between ages 20-45)

	Years of Education	Prob(High School Grad)	Ln(Wage)	Ln(Family Income)	Prob(Poverty)
	1	2	3	4	5
Ln(Spending)(age 5-17) * Low-income	**4.7175****	**0.9315****	**1.0493****	**1.4740*****	**-0.6041*****
	(1.9336)	**(0.4117)**	**(0.4561)**	**(0.5526)**	**(0.1972)**
Ln(Spending)(age 5-17) * Non Poor	-0.6205	0.2453	0.4356	0.5405	-0.0576
	(2.0622)	(0.3546)	(0.4336)	(0.5849)	(0.5849)
Number of Individuals	15353	15353	13183	14730	14737
Number of School Districts	1409	1409	1395	1414	1414

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 county 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 low-income and Non-Poor kids.

Appendix H: *Showing Similar Results for More Recent Versus Less Recently Educated Cohorts*

Table H1: *2SLS/IV Estimates of School Spending on Education Outcomes for cohorts born 1955-69 vs 1970-85*

	Dependent variable:	
	Years of Education	Prob(High School Grad)
	1	2
Ln(School District Per-pupil Spending)$_{(age\ 5\text{-}17)}$	**2.6973+**	**0.6760+**
	(1.7925)	**(0.4648)**
Ln(School District Per-pupil Spending)$_{(age\ 5\text{-}17)}$*Born '70-85	0.0500	0.4377
	(2.0739)	(0.5332)
F-test for differential spending effects by birth cohort (prob > F)	0.57	0.66
Number of Individuals	15,353	15,353
Number of Childhood Families	4,586	4,586
Number of School Districts	1,409	1,409

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 children 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 birth year fixed effects; 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 and timing of tax limit policies; 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). All of these controls are interacted with childhood poverty status (including school district FE). PSID sample weights are used to account for oversampling of poor families to produce nationally-representative estimates. The first-stage model include as predictors the school-age years of exposure to school finance reform (non-parametric specification) interacted with the respective school district's predicted reform-induced change in school spending based on the timing and type of court-ordered reform interacted with 1967 (within-state) district income percentile category. All event study years that correspond with non-school ages (whether pre-K or ages>=18) are included in 2SLS models as controls and not for identification, using a non-parametric specification and interacted with the respective school district's predicted reform-induced change in school spending. There exists a significant first-stage for all kids (both low-income and non-poor kids). The main school spending coefficient in model specifications applies to kids born '55-69 (due to the inclusion of the spending interaction term the born '70-85 indicator).