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Does Public School Spending Raise Intergenerational Mobility?: Evidence from U.S. School Finance Reforms

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Abstract

It is generally believed that equality of opportunity can be achieved through high quality public schools. This paper examines the causal effect of public school spending on intergenerational mobility by exploiting U.S. court-mandated school finance reforms. I utilize college attendance rate and intergenerational income mobility that Chetty et al. (2014) construct based on administrative tax records. Event study and instrumental variable models show that students are more likely to attend college due to additional resources in public schools. Reform-induced spending increases also improve intergenerational mobility of advantaged children, but have little impact on mobility of disadvantaged children. In fact, the gap in the mean income rank between advantage and disadvantage children widens. The heterogeneity by county characteristics suggests that the school spending effect may be mitigated by negative environments in high poverty area.

JEL Codes: H52, I24, J62

Keywords: Intergenerational mobility, Public school spending, School finance reform

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1 Introduction

It is generally believed that equality of opportunity can be achieved through high-quality public schools. The classical models of intergenerational income mobility (IGM) also suggest that government investment in public education can mitigate the persistence in economic status across generations by helping poor children to form the efficient level of human capital (e.g., Becker and Tomes (1986), Solon (2004)). However, a recent theory shows that government interventions may in fact lower IGM if they complement parents' investments (Becker et al. (2015)). This paper provides quasi-experimental evidence on whether an increase in public school spending can boost IGM.

To identify the causal impact of public school spending, I utilize the plausibly exogenous variation in school spending induced by U.S. court-mandated school finance reforms (SFRs).[1] Since the 1971 California court case, *Serrano v. Priest*, 28 states have found their existing school funding system unconstitutional and implemented SFRs which led to significant changes in school funding formulas. Previous studies find that SFRs increase expenditures and observable school inputs in low-income school districts and reduce spending and resource disparities across districts (e.g., Jackson et al. (2016) and Hyman (2016)). Based on the variation induced by SFRs, I estimate two-stage least-squares difference-in-difference (2SLS DID) regression model. To be specific, following the approach of Jackson et al. (2016), henceforth JJP, I instrument for average per pupil public school spending during school-age years by using the quartile in the state distribution of pre-reform median household income and the number of years of exposure to SFRs.[2] I also estimate flexible reduced-form event study models which map out dynamic treatment effects of SFRs.

For analysis, IGM data set is linked to data on public school spending and other control variables. I utilize county-by-cohort level measures of college attendance and IGM that Chetty et al. (2014), henceforth CHKS, construct based on administrative tax records of more than 40 million children and their parents. School spending data come from the His-

[1]Recent studies using event study models show a structural break around the timing of either court-ordered or legislative reforms which implies the casual relationship between the reforms and school spending (Candelaria and Shores (2015), Jackson et al. (2016), Lafortune et al. (2016)). However, according to Jackson et al. (2014), although both types of reforms reduce the spending gap within a state, legislative reforms in fact decrease the level of expenditures in all districts. So, I focus on court-ordered reforms.

[2]The results are robust to using the quartile in the state distribution of pre-reform spending as JJP.

torical Database on Individual Government Finances (INDFIN) and the Common Core of Data (CCD) School District Finance Survey (F-33). In addition, I use County and City Data Book 1983 and Regional Economic Information System (REIS) for pre-reform demographic characteristics and expenditures on other welfare programs, respectively.

I find that students are more likely to attend college due to additional resources in public schools. Reform-induced spending increases also improve intergenerational mobility of advantaged children, but have little impact on mobility of disadvantaged children. In fact, the gap in the mean income rank between advantage and disadvantage children widens, which is the opposite of what was intended. This implies that *relative mobility* such as intergenerational elasticity of income in a given state can be worse off due to the state finance reform. The event study estimates and results from falsification tests validate my findings.

Then, how do public school spending increases widen the rank gap between advantaged and disadvantaged children? First, children from affluent families and their parents may be better at utilizing public education. In other words, the public investment may complement/crowd in private human capital investment and expand the disparity in investment Becker et al. (2015). Second, funding allocation within school districts may matter. Hyman (2016) suggests that districts target the additional resources toward low-poverty schools. Third, the change in income distribution should be considered. Autor et al. (2008) show that lower-tail inequality in U.S. grew steeply in the 1980s. Thus, it might be more difficult for disadvantaged children born in 1980s to climb the economic ladder.[3] Lastly, it is important to note that, without information on college quality and graduation, college attendance itself is not a great predictor of future income.

I examine heterogeneity by county characteristics including poverty rate and black rate. It shows that the positive spending effects on college attendance and income rank are driven by large increases among low-poverty counties. Importantly, income rank of disadvantaged children in low-poverty counties also significantly increases, which implies that the school spending effect may be mitigated by negative environments in high poverty area. This heterogeneity is consistent with Hyman (2016) and in contrast to Jackson et al. (2016).

This study is related most closely to a small but growing literature that empirically tests

[3]The sample cohorts in this study were born in 1980-88.

the role of government in IGM.[4] While most previous research explores the correlation between government interventions and IGM (e.g., Mayer and Lopoo (2008), Ichino et al. (2011), Chetty et al. (2014) Freeman et al. (2015)), a few recent studies exploit natural experiments to isolate the causal effects (e.g., Biasi (2015) O'Brien (2015), Havnes and Mogstad (2015), Sharkey and Torrats-Espinosa (2016)).[5] Importantly, my findings are not aligned with other evidence showing positive impacts of government interventions. My findings are best explained by a model of Becker et al. (2015) which suggests that public investment can in fact increase persistence in economic status across generation if if it complements private investments.

This study also contributes to the literature on the effect of SFRs on student outcomes (e.g., Downes (1992), Downes et al. (1998), Guryan (2001), Hoxby (2001), Papke (2005), Papke (2008), Chaudhary (2009), Roy (2011), Biasi (2015) Candelaria and Shores (2015), Hyman (2016), Jackson et al. (2016), Lafortune et al. (2016)). My work connects more directly to the recent studies that examine the long-term effect of the reforms.[6] In particular, I look at a less explored but important long-term outcome, IGM. The SFRs are largely motivated by a desire for equal opportunity, but empirical evidence on this fundamental motive is scarce.

The closest study is the ongoing work by Biasi (2015) which investigates the effect of inequality in public school spending on IGM using the commuting zone (CZ) level measures of IGM constructed by CHKS. Contrary to my findings, she documents the positive impact of SFRs on IGM of children from low-income families without any effect on IGM of rich

[4]More generally, I expands the prior literature on the determinants of IGM. The standard models of IGM provide explanations on how the degree of the persistence in economic status across generations can vary in different places and times. It depends on the heritability of endowments, efficacy of human capital investment, the return to human capital, and the progressivity of government investment in children's human capital (e.g., Becker and Tomes (1986), Solon (2004), Black et al. (2011)).

[5]Ichino et al. (2011) provide cross-country evidence that public expenditures on education are associated with higher IGM. Mayer and Lopoo (2008) and Chetty et al. (2014) also find a positive correlation between public school spending and IGM in U.S. O'Brien (2015) utilizes the uneven expansions of Medicaid eligibility in U.S. and finds that early exposure to health insurance can be a lever to achieve equality of opportunity. Exploiting variation in federal grants administered through the Community Oriented Policing office, Sharkey and Torrats-Espinosa (2016) document the negative effect of crime on IGM. Havnes and Mogstad (2015) show the positive association between universal child care programs and IGM using a large scale expansion of subsidized child care in Norway. Lastly, Biasi (2015) exploits U.S. school finance reforms and finds that school finance equalization promotes IGM.

[6]JJP use as outcomes completed years of education, wages, and incidence of adult poverty, and Hyman (2016) looks at college enrollment and postsecondary degree receipt.

children. The differences in findings may be driven by two factors. First, Biasi (2015) focuses on the inequality in school spending rather than absolute spending level. To be specific, she utilizes the slope coefficient of district income level on school expenditure as a CZ level statistic of spending inequality. Importantly, this statistic may not accurately capture available resources for rich children. Second, the empirical strategy is different. She constructs simulated instruments for school spending using the parameters of school funding formulas. Compared to Biasi (2015), I explore additional outcomes, college attendance rates by income rank which shed light on the mechanism of spending effect on IGM.

Another closely related paper is JJP which examine the effect of reform-induced school expenditures on completed years of schooling and earnings. It is important to note that I explicitly use measures of IGM as outcomes and investigate the changes in the children's positions in the national income distribution conditional on their parents' ranks. In addition, measures provided by CHKS are based on a large sample of tax records on income and college attendance, while JJP rely on survey data on 15,353 individuals from Panel Study of Income Dynamics.

The remainder of this paper is organized as follows. In the next section, I briefly describe SFRs. Section 3 describes the data, and Section 4 describes the empirical framework. Section 5 presents the results. The last section concludes.

2 School Finance Reforms in U.S.

The SFRs are triggered by constitutional challenges against local property tax based school funding system. Given high levels of residential segregation by socioeconomic status, the heavy reliance on local financing implies that children from low-income families are likely to go to low quality public schools. Between 1971 and 2009, 28 states have found their existing school funding system unconstitutional and implemented court-ordered reforms which led to significant changes in school funding formulas. Although the details of the reforms are far from uniform, SFRs essentially aim to reduce spending gap across districts ("equity" reforms) and/or provide sufficient funding in low-income districts ("adequacy" reforms).

Previous studies show that SFRs achieved their immediate goals (e.g., Downes (1992), Murray et al. (1998), Card and Payne (2002), Hoxby (2001), Hyman (2016), Jackson et al. (2016)). However, it is less clear how SFRs and subsequent spending changes in fact affect students' outcomes. When it comes to test scores, Downes (1992) find little evidence of

the effect of *Serrano II and Proposition 13* in California on the test scores for 6^{th} grader. Guryan (2001) provides regression discontinuity evidence in Massachusetts that the spending increases improve 4^{th} grade test scores, but not 8^{th} grade test scores. A series of studies which exploit *Proposal A* in Michigan also find mixed evidence (Papke (2005), Papke (2008), Chaudhary (2009) Roy (2011)). The findings from national studies are inconclusive as well. Card and Payne (2002) show that SFRs decrease the gaps in SAT score between rich and poor students, while Downes et al. (1998) find that SFRs have no significant impact on the distribution of test scores. More recently, Lafortune et al. (2016), focusing on "adequacy" reforms, find that SFRs lead to increases in student achievement in low-income school districts. For high school graduation, Hoxby (2001) documents mixed evidence, while Candelaria and Shores (2015) find that "adequacy" reforms raise graduation rates, especially in high poverty districts.

A couple of studies look at the outcomes after high school graduation. A nation wide study, JJP, finds that school spending increases lead to more completed years of education, higher wages, and less incidence of adult poverty. It also shows that the effects are concentrated among children from low-income families. Focusing on Michigan's school finance reform in 1994, Hyman (2016) finds that children exposed to additional spending are more likely to attend college and get a postsecondary degree. In addition, he finds that the spending effects are more pronounced in lower-poverty area.

3 Data

This section describes the data on IGM, SFRs, public school spending, and other control variables combined for analysis. Table 1 shows descriptive statistics.

3.1 College Attendance and Intergenerational Income Mobility

I use county-by-cohort level IGM dataset that CHKS construct based on administrative tax records of more than 40 million children and their parents.[7] CHKS document substantial variation in IGM, which allows one to investigate why some areas have higher IGM than others. Their primary measure of IGM is the *absolute upward mobility*, which is the "*mean rank (in the national child income distribution) of children at age 26 whose parents are at*

[7]Source: Intergenerational Mobility Estimates by County and Birth Cohort (http://www.equality-of-opportunity.org/index.php/data)

the 25^{th} percentile of the national parent income distribution." I also use this as a measure of IGM. CHKS refer to this as the *absolute mobility* under the assumption that incomes in a small area have little impact on the national distribution. It is distinguished from the *relative mobility* (ex. intergenerational elasticity of income) in a sense that it is not affected by the outcomes of the rich.

In addition, I utilize county-by-cohort level college attendance rate constructed by CHKS. Specifically, they estimate what percentage of children whose parents are at the 25^{th} percentile of the national parent income distribution are enrolled in a college at age 19. This measure is also based on administrative tax record. In particular, they use information in 1098-T forms which are directly filed by colleges.

Importantly, CHKS provide the mean income rank and college attendance rate of children whose parents are at the 75^{th} percentile. So, I can test whether an increase in school spending has a heterogeneous effect by family background. For convenience sake, I refer to "*children whose parents are at the 25^{th} (75^{th}) percentile of the national parent income distribution*" as disadvantaged (advantaged) children.

Note that CHKS assign each child to a single county where "*his parent filed their tax return in the first year the child was claimed as a dependent.* So, the single county does not necessarily correspond to the place where the child lives as an adult. Also, CHKS's measures are based on permanent residents defined as "*children whose parents reside in a single county in all years of their sample, 1996-2012.*" Because CHKS can measure parents' location only during this period, there is a measurement error in children's childhood location. But, CHKS argue that "*most families who stay in a given area for several years tend not to have moved in the past either.*" It is also important to note that a large portion of counties in IGM data set were censored based on population size. However, it still has a good coverage in terms of population (more than 88% of U.S. population in 2000).

3.2 U.S. School Finance Reforms, Public School Spending, and Others

I use the timing of SFRs compiled by JJP. JJP compare the information in different sources, and consult state court and legislative records when they are not consistent with each other.[8] Figure 1 shows the number of states with at least one court-mandated reform between

[8] JJP compile the information from prior studies, Public School Finance Programs of the United States and Canada, and the National Access Network.

1970 and 2010.[9] Up to 2009, 28 states have found their existing school funding system unconstitutional and implemented their first SFRs. Note that many states had their first SFRs during the period when my sample cohorts (1980-1988) were in school (1985-2006).

School spending data come from the Historical Database on Individual Government Finances (INDFIN) and the Common Core of Data (CCD) School District Finance Survey (F-33).[10] I utilize County and City Data Books 1983 for pre-reform county and state characteristics: poverty rate, black rate, urban rate, high school graduation rate, and population size. In addition, I use Regional Economic Information System (REIS) for expenditure data of other welfare programs that may affect the sample cohort (1980-1988), including food stamps, Aid to Families with Dependent Children (AFDC), Medicaid, Earned Income Tax Credit (EITC) and unemployment insurance.

4 Empirical Strategy

The goal of my empirical strategy is to estimate the causal impact of public school spending during school-age years on college attendance and income rank in the national distribution. The major challenge here is that school expenditures may be correlated with other factors that can affect student outcomes. For instance, children growing up in a low school spending area may have limited accessibility to other public services as well.

To deal with this concern, I utilize the plausibly exogenous variation in school spending induced by SFRs. Between 1971 and 2009, 28 states have found their existing school funding system unconstitutional and implemented SFRs which led to significant changes in school funding formulas. Previous studies find that SFRs increase expenditures and observable school inputs in low-income districts and reduce spending and resource disparities across districts. Accordingly, children in the same cohort can differently benefit from SFRs depending on the state of residence and the wealth of the community. In addition, children growing up in the same area can be differently exposed to SFRs by their birth cohorts. Based on these variation induced by SFRs, I estimate 2SLS DID regression model. To be specific, following the approach of JJP, I instrument for average per pupil public school spending during school-age years by using the quartile in the state distribution of pre-reform median

[9] Appendix Table A.1 presents a list of first statewide SFRs.

[10] I use compiled data from *The Government Finance Database*. For school spending variable, I utilize the code $E12$, expenditure for Current Operations of Elementary and Secondary Education.

household income and the number of years of exposure to SFRs.

I estimate the following specification for cohort b from county c.

$$\begin{aligned}\ln(\overline{PPE_{5-17}})_{cb} = \pi_1(Exp_{cb} \times Q1_c) + \pi_2(Exp_{cb} \times Q2_c) + \pi 3(Exp_{cb} \times Q3_c) \\ + \pi_4(Exp_{cb}) + \Pi C_{cb} + \rho_c + \rho_b + \xi_{cb}\end{aligned} \tag{1}$$

$$IGM_{cb} = \delta \cdot \ln(\widehat{\overline{PPE_{5-17}}})_{cb} + \Phi C_{cb} + \theta_c + \theta_b + \varepsilon_{cb} \tag{2}$$

Here, the endogenous treatment variable, $\ln(\overline{PPE_{5-17}})_{cb}$ is the natural log of average per pupil public school spending (in real 2005 dollars) during school-age years (ages 5 through 17) for birth cohort b from county c.[11] This variable summarizes the amount of resources available for one's K-12 education. I predict plausibly exogenous changes in school spending using Exp_{cb} and its interaction with $Q1_c$, $Q2_c$, and $Q3_c$.[12] Exp_{cb} denotes the number of school-age years of exposure to SFRs for birth cohort b from county c. It varies from 0 (for those who turned age 17 or older during the year of the state's court order) to 12 (for those who turned age 5 or younger during the year of the state's court order).[13] $Q1_c$, $Q2_c$, and $Q3_c$ are indicators set to one if county c belongs to the bottom 25%, the $25th$ to $50th$ percentile, and the $50th$ to $75th$ percentile in the state distribution of median household income in 1980, respectively.[14] C_{cb} denotes average per capita expenditures during childhood (birth to 17) on other welfare programs such as food stamps, AFDC, Medicaid, EITC and unemployment insurance. IGM_{cb} represents county-by-cohort level measures of IGM. The primary measure is the "*mean rank (in the national child income distribution) of children at age 26 whose parents are at the* $25th$ *percentile of the national parent income distribution.*" I also use as an outcome the percentage of disadvantaged children who are enrolled in a college at age 19. The same measures for advantaged children and the gap between advantaged and disadvantaged children are used to investigate who benefit more from an increase in public school spending.

In all specifications, I include county fixed effects, ρ_c and θ_c, and birth cohort fixed effects,

[11] The natural log is used to account for diminishing marginal return to school spending and make the interpretation of coefficients easier.

[12] I use one of two different pre-reform predictors that JJP propose. They show that estimated spending effects are quite similar using either predictor.

[13] For states with multiple court-mandated reforms, I estimate the impact of the first reform following JJP.

[14] Note that SFRs before 1980 do not affect my estimates because I use county fixed effects in the model. The results are robust to using the quartile in the state distribution of pre-reform spending as JJP. See Appendix Figure A.1, Table A.2, and Table A.3.

ρ_b and θ_b. I also examine the sensitivity to including 1980 state characteristics interacted with linear birth-cohort trends or state-specific trend which are not shown in equation (1) and (2). All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county.

The coefficient of primary interest is δ, the coefficient of the predicted childhood school expenditure, which captures the causal effect of public school spending during school-age years on IGM. The identifying assumption is that the timing of SFRs and reform-induced school spending are not correlated with other county-level changes that could directly affect outcomes. Although I control for county-by-cohort level expenditures on other welfare programs, there might be other unobserved time-varying factors. One way to deal with this concern is estimating the event study model which allows for lags and leads of the treatment effects. If my assumption is valid, there should be no discernible trending in school-age spending, college attendance, and income rank for unexposed cohorts.

To show how childhood school spending and outcomes of interest evolve by the duration of exposure and pre-reform income level, I estimate the following event study model.

$$Outcome_{cb} = \sum_{q=1}^{4} \sum_{t=-20}^{20} (I_{T_{cb}=t} \times I_{Q_c=q}) \cdot \alpha_{t,q} + \Pi C_{cb} + \theta_c + \theta_b + \upsilon_{cb} \tag{3}$$

$Outcome_{cb}$ represents average public school spending during school-age years, college attendance, and the income rank in the national distribution. $I_{T_{cb}=t}$ represents a series of indicator variables for the years of exposure to SFRs. They are set to one if T_{cb} equals t which varies between -20 and 20. T_{cb} is the year cohort b from county c turned age 17 minus the year of the state's court order. Thus, T_{cb} has negative value when the court order occurred after the corresponding cohort graduated from high school. Likewise, it can be over 12 if the cohort was age 4 or younger during the year of court order. The reference group is $t = 0$ which denotes those who turned age of 17 during the year of the court order. Each of the time indicator variables is interacted with quartile indicators, $I_{Q_c=q}$. They are set to one if Q_c equals q which varies between 1 and 4. Q_c denotes the quartile of county c in the state distribution of median household income in 1980. Accordingly, $\alpha_{t,q}$ maps out the dynamic treatment effects of SFRs on outcomes by 1980 income quartile q.

It is important to note that the coefficients of interest, δ in equation (2) and $\alpha_{t,q}$ in equation (3), are Intention-To-Treat estimates because my models do not consider residential

mobility and school choice. In fact, as noted in Section 2, measures constructed by CHKS are based on permanent residents.[15] But, one may still expect a smaller impact for advantaged children because they are more likely to attend private school.

5 Results

5.1 The Effect of School Finance Reforms on Childhood Public School Spending

Figure 2 plots the event study estimates, $\alpha_{t,q}$ in equation (3), where the dependent variable is log of average school spending during school-age years.[16] It reveals that those who exposed to SFRs experienced considerable increases in school spending.[17] Childhood expenditures monotonically increase with the years of exposure, and children from poorer counties enjoyed larger spending increases. Importantly, I find little evidence of systematic changes in school-age spending for the unexposed cohorts (those who turned age 18 or older in the year of court order), supporting the identifying assumption that reform timing is exogenous.

Table 2 reports the first stage results in equation (1). Consistent with Figure 2, it shows that children growing up in lower-income counties enjoyed larger spending increases as they were exposed to SFRs for a longer period of time. Here, the coefficients of interest are π_1, π_2, and π_3, the coefficients of interaction between the duration of exposure and $Q1_c$, $Q2_c$, and $Q3_c$. They provide a sort of triple-difference (DDD) estimates of SFRs effect which are relative to those for the top quartile counties. They are robust to arbitrary state-by-cohort shocks if the shocks similarly affect counties regardless of pre-reform income level. In column (3), for instance, as the duration of exposure increases by one year, those from the bottom 25% counties experienced 2.28% more school spending during school-age years than those from the top 25% counties. The additional expenditures compared to the top 25% counties decrease to 1.50% and 0.46% for those from the 25^{th} to 50^{th} percentile and the 50^{th} to 75^{th} percentile counties, respectively. The results are quite robust across specifications. I use the

[15] Permanent residents refer to children whose parents reside in a single county in all years of sample, 1996-2012.

[16] I use the specification which includes birth cohort fixed effects, county fixed effects, 1980 state characteristics interacted with linear birth cohort trend, and other government expenditures.

[17] For instance, children who grew up in the bottom quartile counties and turned age 7 during the year of the court order (10 years of exposure) experienced about 30 percent increases in school-age spending. Note that the event study estimates are relative to the effect for those who turned age 17 during the year of the court order.

last specification with an F-test 14.76 as my preferred specification.

5.2 The Effect of School Spending Increases on College Attendance and Intergenerational Mobility

5.2.1 Reduced-form Event Study Estimates

Figure 3 and 4 present event study estimates, $\alpha_{t,q}$ in equation (3), where the dependent variables are college attendance rate of disadvantaged and advantaged children, respectively. In each figure, the top graph plots the estimates for the 1st quartile counties in the state distribution of pre-reform median household income, and the bottom graph plots the estimates for the 4th quartile counties. In all the graphs, there is a clear pattern of increasing college attendance among exposed cohorts. Consistent with the effects on childhood school spending documented in Section 5.1, I find larger increases across cohorts among the bottom quartile counties. For unexposed cohorts, I find no discernible trends, which supports my causal interpretation.

Figure 5 and 6 show the event study estimates for the mean income rank. In Figure 5, I find that SFRs do not significantly increase the income rank of disadvantaged children (*absolute upward mobility*) growing up in the bottom quartile counties.[18] Interestingly, *absolute upward mobility* of the top quartile counties increase, although they experienced smaller spending increases. This is possible when the school spending effects are mitigated by negative environments in low-income area. Figure 6 shows that SFRs lead to an increase in the income rank of advantaged children. The effects are larger for the bottom quartile counties which experience larger spending increases. Again, for unexposed cohorts, I find no discernible pattern in outcomes.

5.2.2 Two-stage-least-squares Difference-in-difference Estimates

This section answers the primary question of this study, "Does public school spending raise IGM?"[19] The 2SLS DID estimates in Table 3 show that both advantaged and disadvantaged children are more likely to attend college due to additional resources in public schools. For

[18]The estimates are small and not statistically significantly different from zero except the estimate for the 12 years of exposure which is marginally significant.

[19]I focus on 2SLS estimates and present OLS estimates for comparison purposes. The OLS estimates of spending effects tend to be smaller than the 2SLS estimates, which suggests that the OLS estimates can be negatively biased.

instance, the estimates in Column (6) indicate that a 10% increase in per-pupil school spending in all school-age years increases the college attendance rate of disadvantaged children by 2.59% point. The same portion of spending increases leads to 2.14% point increase in college attendance rate of advantaged children. I must admit that the results on college attendance are only suggestive due to the lack of information on college quality and degree completion. For instance, disadvantaged children may go on to low-ranking two-year colleges. It is also possible that they do not complete college education (Stinebrickner and Stinebrickner (2014)).

Table 4 shows that reform-induced spending increases also improve intergenerational mobility of advantaged children, but have little impact on mobility of disadvantaged children. In fact, the gap in the mean income rank between advantage and disadvantage children widens. For instance, the estimate in the column (6) in Panel (C) indicates that a 10% increase in public school spending during childhood widens the income rank gap by 0.55% point.

Importantly, these findings are not aligned with other empirical evidence showing the positive association between government interventions and IGM (Mayer and Lopoo (2008), Ichino et al. (2011), Chetty et al. (2014), Biasi (2015), Freeman et al. (2015), Havnes and Mogstad (2015), O'Brien (2015), Sharkey and Torrats-Espinosa (2016)). The fact that advantaged children eventually benefit more from spending increases than disadvantaged children implies that *relative mobility* such as intergenerational elasticity of income in a given state can be worse off due to the state finance reform. My findings are best explained by a model of Becker et al. (2015) which suggests that public investment can in fact increase persistence in economic status across generation if it complements private investments.

5.3 Heterogeneity

In Table 5 and 6, I examine heterogeneity by county characteristics including poverty rate and black rate. It shows that the positive spending effects on college attendance and income rank are driven by large increases among low-poverty counties. Importantly, income rank of disadvantaged children in low-poverty counties also significantly increases, which implies that the school spending effect may be mitigated by negative environments in high poverty area.

5.4 Comparison to Previous Literature

As mentioned in the introduction of this paper, Biasi (2015) examines the effect of school spending inequality on IGM using the commuting zone level measures of IGM constructed by CHKS. Contrary to my findings, she documents the positive impact of SFRs on IGM of children from low-income families without any effect on IGM of rich children. The differences in findings may be driven by two factors. First, Biasi (2015) focuses on the inequality in school spending rather than absolute spending level. To be specific, she utilizes the slope coefficient of district income level on school expenditure as a CZ level statistic of spending inequality. Importantly, this statistic may not accurately capture available resources for rich children. Second, the empirical strategy is different. She constructs simulated instruments for school spending using the parameters of school funding formulas.

Among a couple of studies that examine the long-run effect of SFRs, my findings are more in line with Hyman (2016) rather than Jackson et al. (2016). Focusing on Michigan's school finance reform in 1994, Hyman (2016) finds that children exposed to 10% spending increases are 3% points more likely to attend college and 2.3 percentage points more likely to get a postsecondary degree. In addition, he finds that the spending effects are more pronounced in lower-poverty area. JJP find that 10% more school spending leads to 0.31 more completed years of education and 7% higher earnings. They find the positive effects on completed years of education and wages are concentrated among children from low-income families.

5.5 Robustness

5.5.1 Placebo falsification test

I implement a placebo falsification test using expenditures during non-school-age years. To be specific, I estimate the marginal effect of school spending after children leave the school.[20] If my findings truly reflect causal impact of school spending, the childhood spending effect should be present without corresponding significant impact for expenditures after graduation. Note that I don't use expenditures before schooling as a placebo because it can have a long-lasting effect on those who enter school after the spending changes. For instance, schools may use the additional resources to purchase computers or experimental tools which can be

[20] In the 2SLS models in equations (1) and (2), I add instrumented public school spending during ages 19 to 24 (in addition to instrumented spending between the ages 5 and 17).

used for all students who enter the schools thereafter.[21] Table 7 presents the falsification test results. It shows that significant spending effects found in Table 3 and 4 still exist without significant impact for expenditure between the ages 19 and 24.

6 Conclusion

It is generally believed that equality of opportunity can be achieved through high-quality public schools. I test this assumption exploiting the plausibly exogenous variation in school spending induced by SFRs. Using college attendance rate and intergenerational income mobility constructed based on administrative tax records. I find that students are more likely to attend college due to additional resources in public schools. Reform-induced spending increases also improve intergenerational mobility of advantaged children, but have no impact on mobility of disadvantaged children. In fact, the gap in the mean income rank between advantage and disadvantage children widens. The heterogeneity by county characteristics suggests that the school spending effect may be mitigated by negative environments in high poverty area.

These findings are not aligned with other empirical evidence showing the positive association between government interventions and IGM. The fact that advantaged children eventually benefit more from spending increases than disadvantaged children implies that *relative mobility* such as intergenerational elasticity of income in a given state can be worse off due to the state finance reform. My findings are best explained by a model of Becker et al. (2015) which suggests that public investment can in fact increase persistence in economic status across generation if it complements private investments.

[21] However, I include as a control the log of spending during ages 0 to 4.

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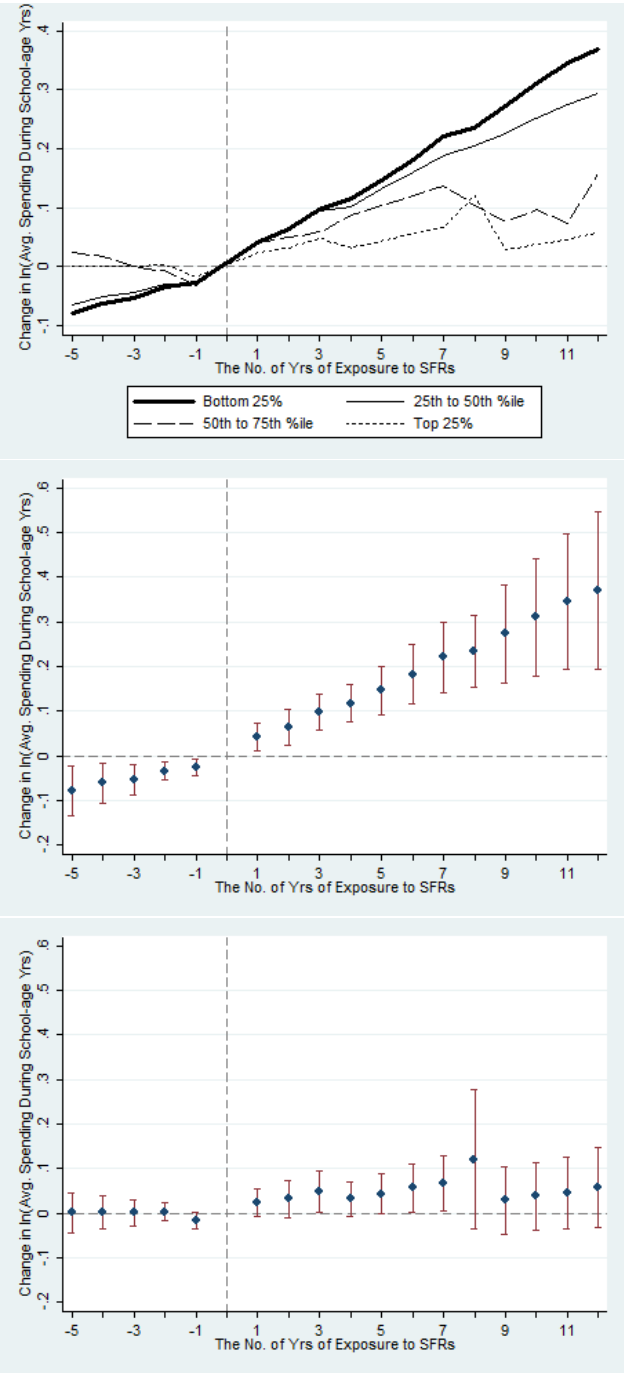
7 Figures and Tables

Figure 1: The Number of States with Court-mandated School Finance Reform over Time

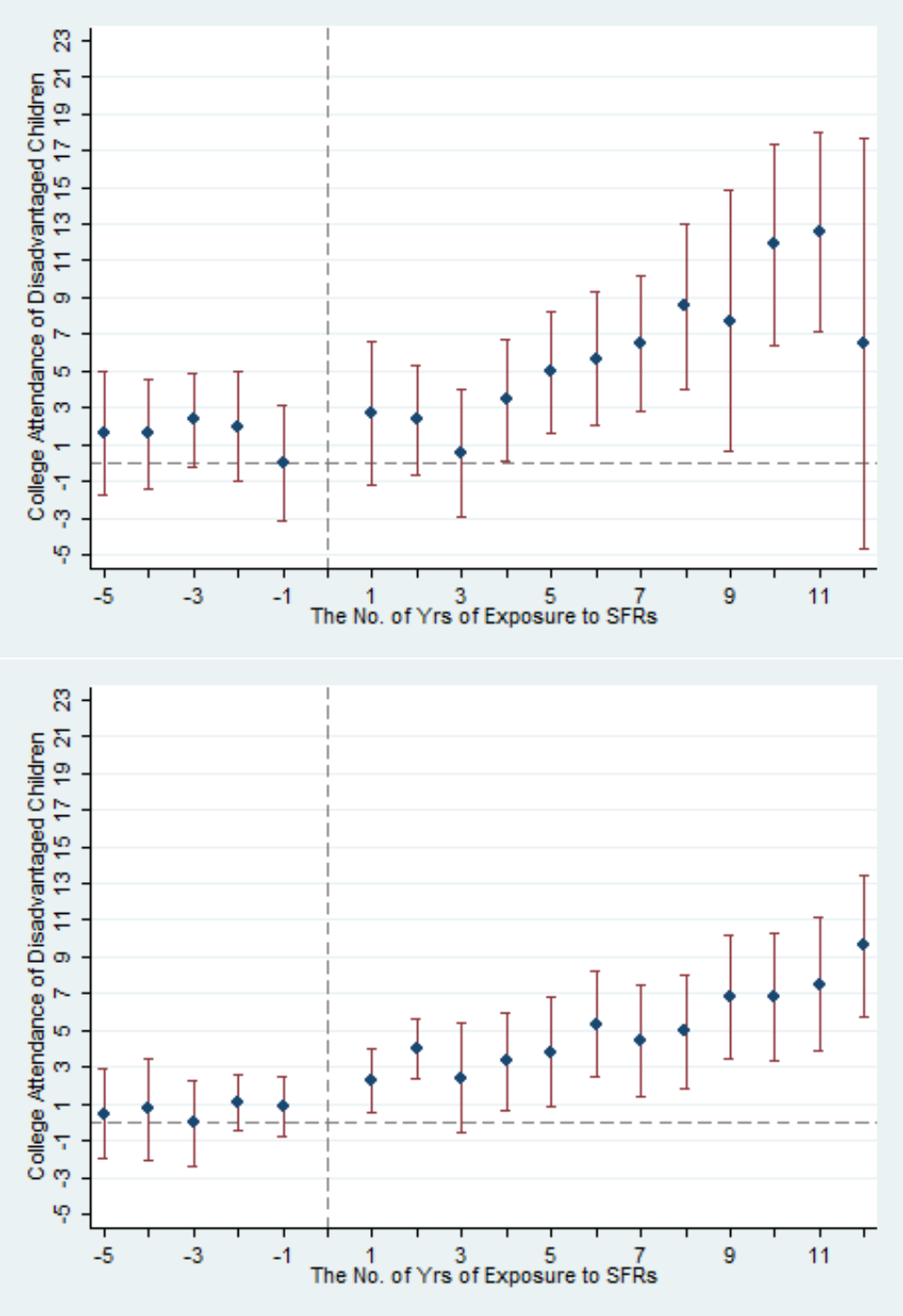
NOTES: Figure 1 shows the number of states with at least one court-mandated SFR between 1970 and 2010. Up to 2009, 28 states has found their existing school funding system unconstitutional and implemented court-ordered SFRs which led to significant changes in school funding formulas. Two vertical lines indicate the period when my sample cohorts (1980-1988) were in school (1985-2006).

Figure 2: Event-Study Estimates of the Effect of the Court-ordered School Finance Reform on Average Per-pupil Public School Spending during School-age Years



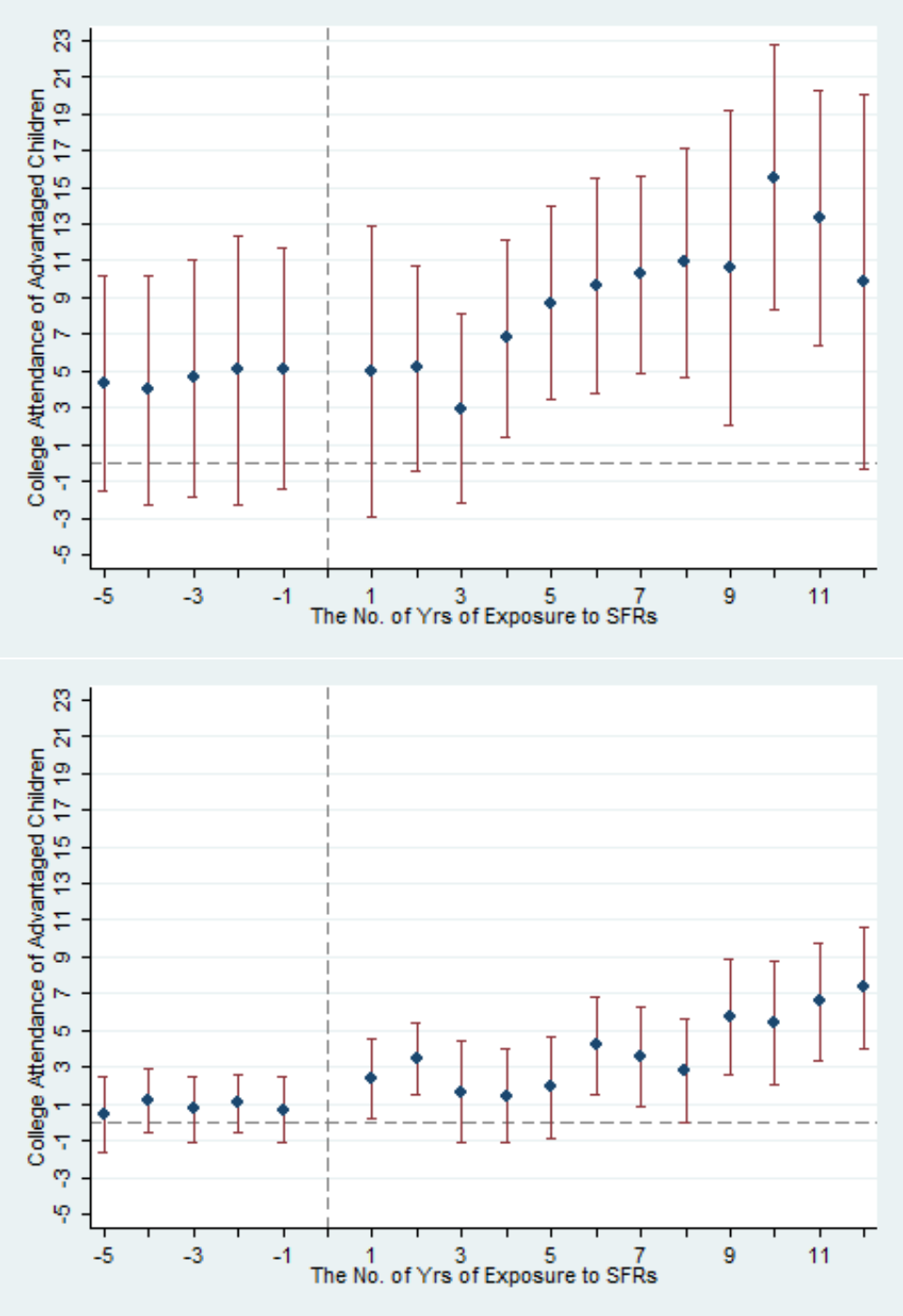
NOTES: Figure 2 plots the event study estimates, $\alpha_{t,q}$ in equation (3), where the dependent variable is log of average per-pupil public school spending during school-age years (ages 5-17). The top graph shows the estimates for all the quartile in the state distribution of pre-reform household income. The middle and bottom graphs show estimates for the 1st and 4th quartile, respectively. The bars extending from each point represent the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the county level. I use the specification including birth cohort fixed effects, county fixed effects, and 1980 state characteristics interacted with linear birth cohort trend. I also control for other government expenditures that may affect the sample cohort (1980-1988), including food stamps, AFDC, Medicaid, EITC and unemployment insurance.

Figure 3: Event-Study Estimates of the Effect of the Court-ordered School Finance Reform on College Attendance Rate of Disadvantaged Children



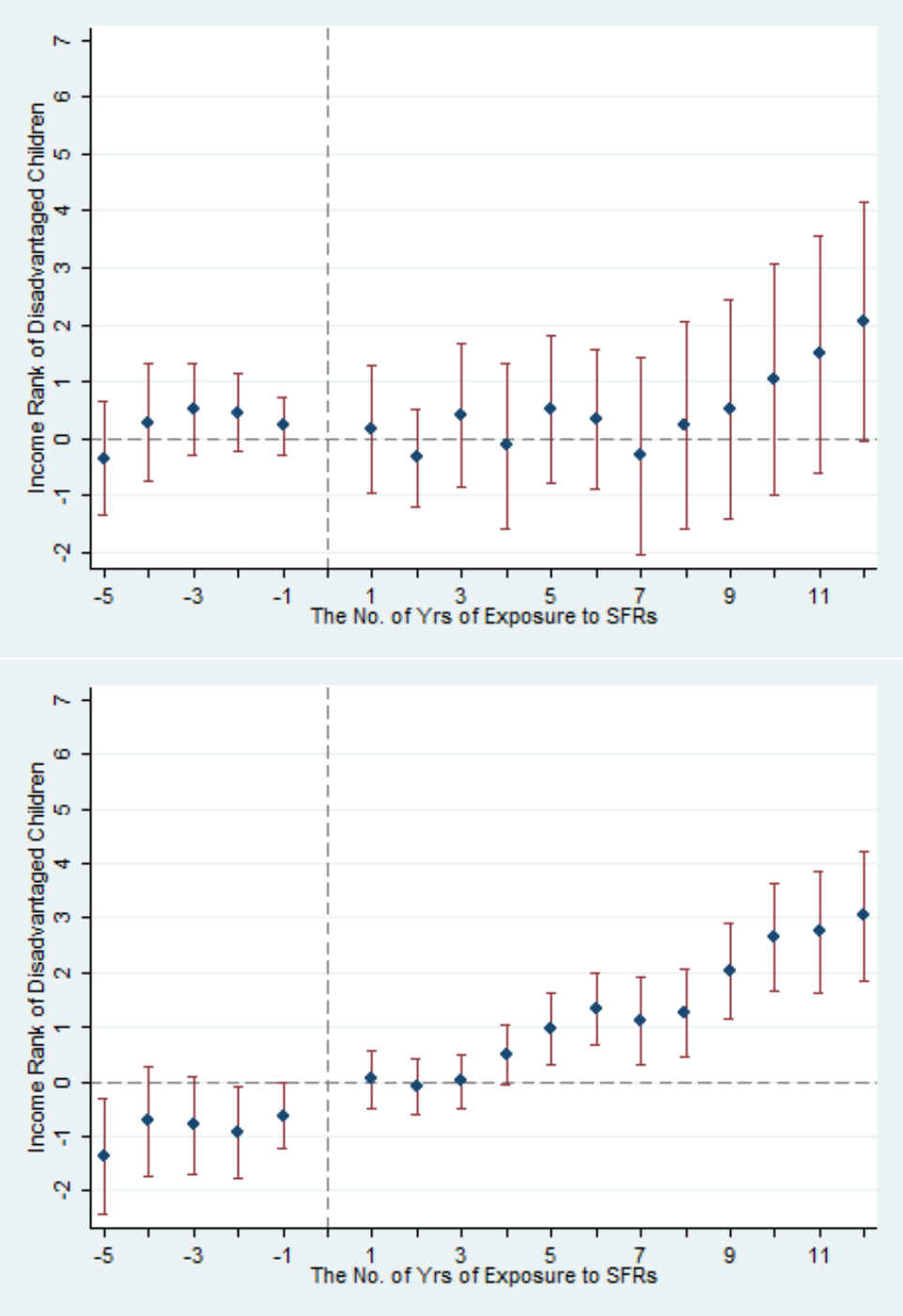
NOTES: Figure 3 plots event study estimates, $\alpha_{t,q}$ in equation (3), where the dependent variable is college attendance rate of disadvantaged children. The top and bottom graphs show estimates for the 1st and 4th quartile in the state distribution of pre-reform median household income, respectively. The bars extending from each point represent the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the county level. I use the specification including birth cohort fixed effects, county fixed effects, and 1980 state characteristics interacted with linear birth cohort trend. I also control for other government expenditures that may affect the sample cohort (1980-1988), including food stamps, AFDC, Medicaid, EITC and unemployment insurance.

Figure 4: Event-Study Estimates of the Effect of the Court-ordered School Finance Reform on College Attendance Rate of Advantaged Children



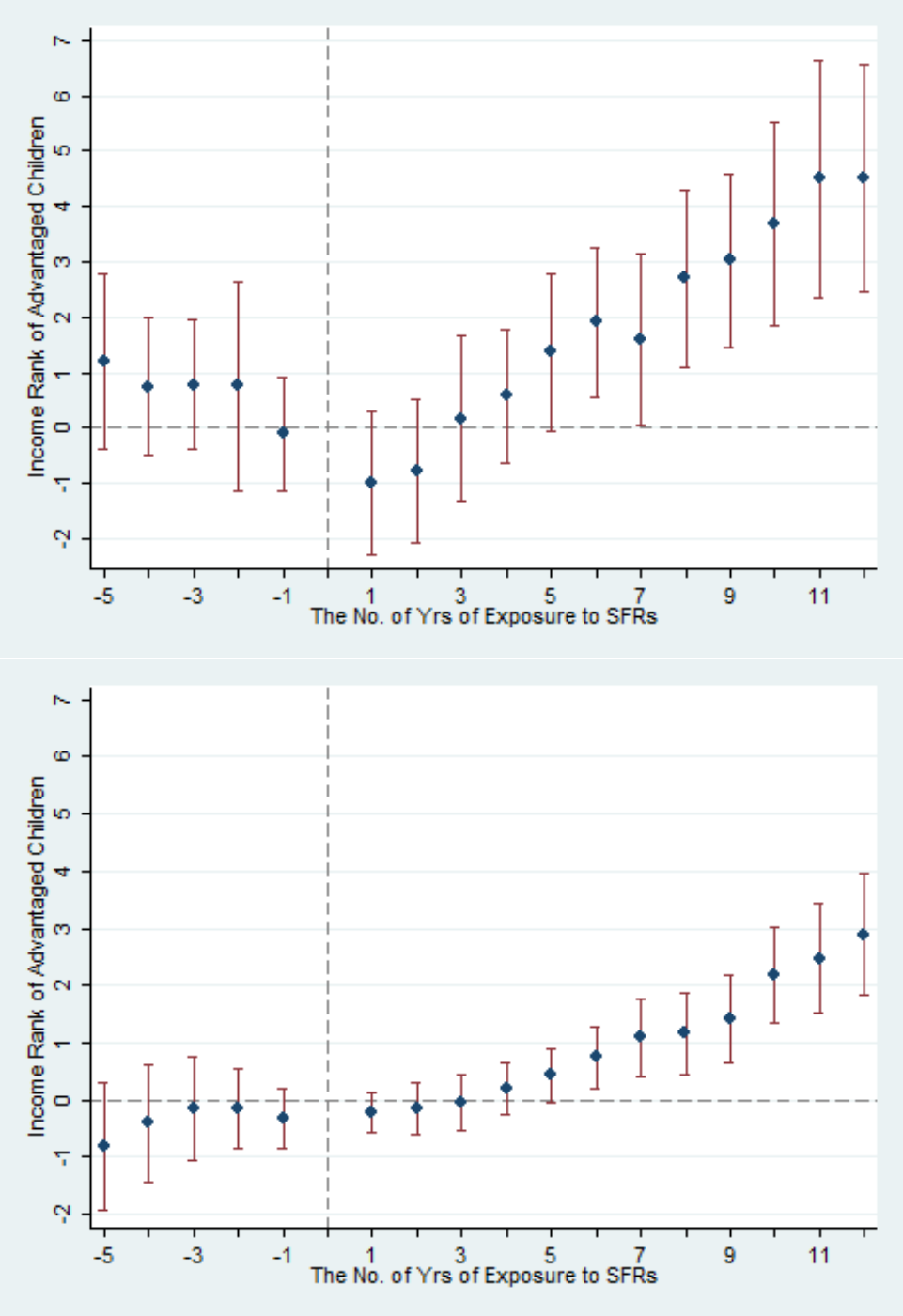
NOTES: Figure 4 plots event study estimates, $\alpha_{t,q}$ in equation (3), where the dependent variable is college attendance rate of advantaged children. The top and bottom graphs show estimates for the 1st and 4th quartile in the state distribution of pre-reform median household income, respectively. The bars extending from each point represent the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the county level. I use the specification including birth cohort fixed effects, county fixed effects, and 1980 state characteristics interacted with linear birth cohort trend. I also control for other government expenditures that may affect the sample cohort (1980-1988), including food stamps, AFDC, Medicaid, EITC and unemployment insurance.

Figure 5: Event-Study Estimates of the Effect of the Court-ordered School Finance Reform on Intergenerational Income Mobility of Disadvantaged Children



NOTES: Figure 5 plots event study estimates, $\alpha_{t,q}$ in equation (3), where the dependent variable is the mean income rank of disadvantaged children. The top and bottom graphs show estimates for the 1st and 4th quartile in the state distribution of pre-reform median household income, respectively. The bars extending from each point represent the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the county level. I use the specification including birth cohort fixed effects, county fixed effects, and 1980 state characteristics interacted with linear birth cohort trend. I also control for other government expenditures that may affect the sample cohort (1980-1988), including food stamps, AFDC, Medicaid, EITC and unemployment insurance.

Figure 6: Event-Study Estimates of the Effect of the Court-ordered School Finance Reform on Intergenerational Income Mobility of Advantaged Children



NOTES: Figure 6 plots event study estimates, $\alpha_{t,q}$ in equation (3), where the dependent variable is the mean income rank of advantaged children. The top and bottom graphs show estimates for the 1st and 4th quartile in the state distribution of pre-reform median household income, respectively. The bars extending from each point represent the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the county level. I use the specification including birth cohort fixed effects, county fixed effects, and 1980 state characteristics interacted with linear birth cohort trend. I also control for other government expenditures that may affect the sample cohort (1980-1988), including food stamps, AFDC, Medicaid, EITC and unemployment insurance.

Table 1: Descriptive Statistics

Variable	Mean	SD	Min	Max	N
Income Rank of Dis. Children	47.66	5.11	30.78	71.09	9,481
Income Rank of Adv. Children	58.90	3.77	42.60	75.80	9,481
College Attendance of Dis. Children	32.79	9.59	0.87	76.15	12,065
College Attendance of Adv. Children	68.30	9.56	11.70	92.72	12,065
Childhood Public School Spending	6.61	2.65	0.15	227.3	24,552
Poverty Rate	0.15	0.07	0.00	0.53	28,026
Black Rate	0.08	0.14	0.00	0.84	27,999
High School Graduates Rate	0.59	0.12	0.25	0.95	28,026
Urban Rate	0.36	0.29	0.00	1.00	28,053
Population	61,076	129,853	91	1,367,000	28,053
Welfare Expenditure Per-capita	1.04	0.44	0.14	5.02	27,738

NOTES: The sample is at the county-cohort level. Income Rank of Dis. (Adv.) Children is the mean income rank in the national child income distribution of children at age 26 whose parents are at the 25th (or 75th) percentile of the national parent income distribution. College Attendance is the percentage of children who are enrolled in a college at age 19. Note that a large portion of counties in IGM data set were censored based on population size although it still has good coverage (more than 88% of U.S. population in 2000) in terms of population. Childhood Public School Spending is the average per-pupil public school spending between age 5 and 17. Poverty Rate, Black Rate, High School Graduates Rate, Urban Rate, and Population are county characteristics in 1980. Welfare Expenditures Per-capita is the average per-capita expenditures on other welfare programs (food stamps, AFDC, Medicaid, EITC and unemployment insurance) during childhood (ages 0 to 17)

Table 2: The Effect of the Court-ordered School Finance Reforms on Average Per-pupil Public School Spending during School-age Years

Dependent variable:	Log of Spending during School-age Years		
	(1)	(2)	(3)
Years of Exposure to SFRs	0.0034	0.0048	0.0052
	(0.0054)	(0.0039)	(0.0040)
Years of Exposure to SFRs*Q1	0.0232***	0.0247***	0.0228***
	(0.0056)	(0.0064)	(0.0068)
Years of Exposure to SFRs*Q2	0.0155***	0.0164***	0.0150***
	(0.0068)	(0.0070)	(0.0070)
Years of Exposure to SFRs*Q3	0.0054	0.0052	0.0046
	(0.0105)	(0.0103)	(0.0098)
F-statistic	17.85	12.34	14.76
Cohort FE	x	x	x
County FE	x	x	x
1980 State*Trend		x	x
Other Policies			x
N	24,400	24,400	24,391

NOTES: This table reports the coefficients corresponding to equation (1). Each column is a separate regression. The dependent variable is the natural log of average per pupil public school spending (in real 2005 dollars) during school-age years (ages 5 through 17). Years of Exposure to SFRs denotes the number of school-age years of exposure to SFRs. Q1, Q2, and Q3 are the indicators set to one if the county belongs to the bottom 25%, the 25^{th} to 50^{th} percentile, and the 50^{th} to 75^{th} percentile in the state distribution of median household income in 1980, respectively. F-statistic denotes the F-test statistic which tests whether all the excluded instruments jointly have no effect. All regressions account for birth cohort fixed effects and county fixed effects. 1980 State*Trend refers to 1980 state characteristics (poverty rate, black rate, urban rate, high school graduates rate, and population size) interacted with linear birth cohort trend. Other Policies refers to the average expenditures on other welfare programs (food stamps, AFDC, Medicaid, EITC and unemployment insurance) during childhood (ages 0 to 17). All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county. *** 1%, ** 5%, * 10%.

Table 3: OLS and 2SLS Estimates of the Effect of Public School Spending on College Attendance Rate

Dep variable:	**College Attendance Rate at Age 19**					
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Disadvantaged Children					
$\ln(\overline{PPE_{5-17}})_{cb}$	1.20*	1.01	1.12	15.92**	19.99***	25.92***
	(0.70)	(0.70)	(0.71)	(10.07)	(8.50)	(8.90)
N	10,200	10,200	10,200	10,173	10,173	10,173
Dep var mean				32.79		
	Panel B: Advantaged Children					
$\ln(\overline{PPE_{5-17}})_{cb}$	2.03**	1.99**	2.04***	18.24**	18.47***	21.35****
	(0.97)	(0.96)	(0.96)	(8.81)	(7.14)	(7.15)
N	10,200	10,200	10,200	10,173	10,173	10,173
Dep var mean				68.30		
	Panel C: Gap between Adv. and Dis.					
$\ln(\overline{PPE_{5-17}})_{cb}$	0.83*	0.98**	0.92**	2.32	-1.52	-4.57
	(0.48)	(0.45)	(0.47)	(5.95)	(5.01)	(5.20)
N	10,200	10,200	10,200	10,173	10,173	10,173
Dep var mean				31.30		
Cohort FE	x	x	x	x	x	x
County FE	x	x	x	x	x	x
1980 State*Trend		x	x		x	x
Other Policies			x			x

NOTES: This table reports the OLS and 2SLS DID estimates of the effect of average per-pupil public school spending during school-age years on college attendance. Each column is a separate regression. In Panel A/B/C, the dependent variable is the college attendance of disadvantaged children / college attendance of advantaged children / the gap in college attendance between advantaged and disadvantaged children. The first(latter) three columns show the estimates of OLS (2SLS). $\ln(\overline{PPE_{5-17}})_{cb}$ is the natural log of average per pupil school spending (in real 2005 dollars) during school-age years (ages 5 through 17) for birth cohort b from county c. Dep var mean is the mean of the dependent variable. All regressions account for birth cohort fixed effects and county fixed effects. 1980 State*Trend refers to 1980 state characteristics (poverty rate, black rate, urban rate, high school graduates rate, and population size) interacted with linear birth cohort trend. Other Policies refers to the average expenditures on other welfare programs (food stamps, AFDC, Medicaid, EITC and unemployment insurance) during childhood (ages 0 to 17). All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county. *** 1%, ** 5%, * 10%.

Table 4: OLS and 2SLS Estimates of the Effect of Public School Spending on Intergenerational Income Mobility

Dep variable:	**Mean Income Rank at Age 26**					
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Disadvantaged Children					
$\ln(\overline{PPE_{5-17}})_{cb}$	0.20	0.53**	0.51**	5.49	2.79	0.41
	(0.31)	(0.25)	(0.24)	(4.71)	(2.35)	(2.01)
N	8,029	8,029	8,029	8,008	8,008	8,008
Dep var mean	45.66					
	Panel B: Advantaged Children					
$\ln(\overline{PPE_{5-17}})_{cb}$	0.62	0.81**	0.80**	13.88***	8.06***	5.86***
	(0.48)	(0.36)	(0.39)	(5.39)	(2.70)	(2.17)
N	8,029	8,029	8,029	8,008	8,008	8,008
Dep var mean	58.90					
	Panel C: Gap between Adv. and Dis.					
$\ln(\overline{PPE_{5-17}})_{cb}$	0.42	0.28	0.29	8.39**	5.27**	5.45**
	(0.34)	(0.29)	(0.29)	(3.51)	(2.41)	(2.35)
N	8,029	8,029	8,029	8,008	8,008	8,008
Dep var mean	13.24					
Cohort FE	x	x	x	x	x	x
County FE	x	x	x	x	x	x
1980 State*Trend		x	x		x	x
Other Policies			x			x

NOTES: This table reports the OLS and 2SLS DID estimates of the effect of average per-pupil public school spending during school-age years on income rank. Each column is a separate regression. In Panel A/B/C, the dependent variable is the income rank of disadvantaged children / income rank of advantaged children / the gap in income rank between advantaged and disadvantaged children. The first(latter) three columns show the estimates of OLS (2SLS). $\ln(\overline{PPE_{5-17}})_{cb}$ is the natural log of average per pupil school spending (in real 2005 dollars) during school-age years (ages 5 through 17) for birth cohort b from county c. Dep var mean is the mean of the dependent variable. All regressions account for birth cohort fixed effects and county fixed effects. 1980 State*Trend refers to 1980 state characteristics (poverty rate, black rate, urban rate, high school graduates rate, and population size) interacted with linear birth cohort trend. Other Policies refers to the average expenditures on other welfare programs (food stamps, AFDC, Medicaid, EITC and unemployment insurance) during childhood (ages 0 to 17). All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county. *** 1%, ** 5%, * 10%.

Table 5: Heterogeneous Effect on College Attendance Rate by County Characteristics

Dep variable:	**College Attendance Rate at Age 19**			
	Poverty Rate		Black Rate	
	>Median	<Median	>Median	<Median
	(1)	(2)	(3)	(4)
	Panel A: Disadvantaged Children			
$\ln(\overline{PPE_{5-17}})_{cb}$	0.16	26.27***	27.77*	17.76*
	(9.69)	(9.36)	(15.55)	(9.21)
N	4,896	5,253	4,515	5,631
Dep var mean	30.60	34.91	30.98	34.53
	Panel B: Advantaged Children			
$\ln(\overline{PPE_{5-17}})_{cb}$	0.19	12.60**	21.51*	13.75*
	(9.37)	(6.31)	(12.49)	(7.61)
N	4,896	5,253	4,515	5,631
Dep var mean	66.78	69.79	67.72	68.83
Cohort FE	x	x	x	x
County FE	x	x	x	x
1980 State*Trend	x	x	x	x
Other Policies	x	x	x	x

NOTES: This table reports the 2SLS DID estimates of the effect of childhood public school spending on college attendance by county characteristics in 1980. Each column is a separate regression. In Panel A(B), the dependent variable is the college attendance of disadvantaged(advantaged) children. $\ln(\overline{PPE_{5-17}})_{cb}$ is the natural log of average per pupil school spending (in real 2005 dollars) during school-age years (ages 5 through 17) for birth cohort b from county c. Dep var mean is the mean of the dependent variable. All regressions account for birth cohort fixed effects, county fixed effects, 1980 state characteristics (poverty rate, black rate, urban rate, high school graduates rate, and population size) interacted with linear birth cohort trend, and the average expenditures on other welfare programs (food stamps, AFDC, Medicaid, EITC and unemployment insurance) during childhood (ages 0 to 17). All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county. *** 1%, ** 5%, * 10%.

Table 6: Heterogeneous Effect on Intergenerational Income Mobility by County Characteristics

Dep variable:	**Mean Income Rank at Age 26**			
	Poverty Rate		Black Rate	
	>Median	<Median	>Median	<Median
	(1)	(2)	(3)	(4)
	Panel A: Disadvantaged Children			
$\ln(\overline{PPE_{5-17}})_{cb}$	-0.65	5.53*	6.55*	-2.82
	(1.71)	(3.16)	(3.74)	(3.36)
N	3,872	4,118	3,544	4,443
Dep var mean	43.90	47.53	42.88	48.56
	Panel B: Advantaged Children			
$\ln(\overline{PPE_{5-17}})_{cb}$	3.17	8.85***	4.51	5.73*
	(2.10)	(3.13)	(3.00)	(2.96)
N	3,872	4,118	3,544	4,443
Dep var mean	58.37	59.56	58.12	59.84
Cohort FE	x	x	x	x
County FE	x	x	x	x
1980 State*Trend	x	x	x	x
Other Policies	x	x	x	x

NOTES: This table reports the 2SLS DID estimates of the effect of childhood public school spending on income rank by county characteristics in 1980. Each column is a separate regression. In Panel A(B), the dependent variable is the income rank of disadvantaged(advantaged) children. $\ln(\overline{PPE_{5-17}})_{cb}$ is the natural log of average per pupil school spending (in real 2005 dollars) during school-age years (ages 5 through 17) for birth cohort b from county c. Dep var mean is the mean of the dependent variable. All regressions account for birth cohort fixed effects, county fixed effects, 1980 state characteristics (poverty rate, black rate, urban rate, high school graduates rate, and population size) interacted with linear birth cohort trend, and the average expenditures on other welfare programs (food stamps, AFDC, Medicaid, EITC and unemployment insurance) during childhood (ages 0 to 17). All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county. *** 1%, ** 5%, * 10%.

Table 7: The Effect of Public School Spending during Non-school Ages (Falsification Test)

	College Attendance		Income Rank	
	(1)	(2)	(3)	(4)
	Dis.	Adv.	Dis.	Adv.
$\ln(\overline{PPE_{5-17}})_{cb}$	16.23**	16.05***	0.61	5.83***
	(6.75)	(5.87)	(2.52)	(2.08)
$\ln(\overline{PPE_{19-24}})_{cb}$	8.38	-0.01	6.11*	1.21
	(9.61)	(8.60)	(3.35)	(2.47)
N	9,995	9,995	7,888	7,888
Dep var mean	32.79	68.30	45.66	58.90

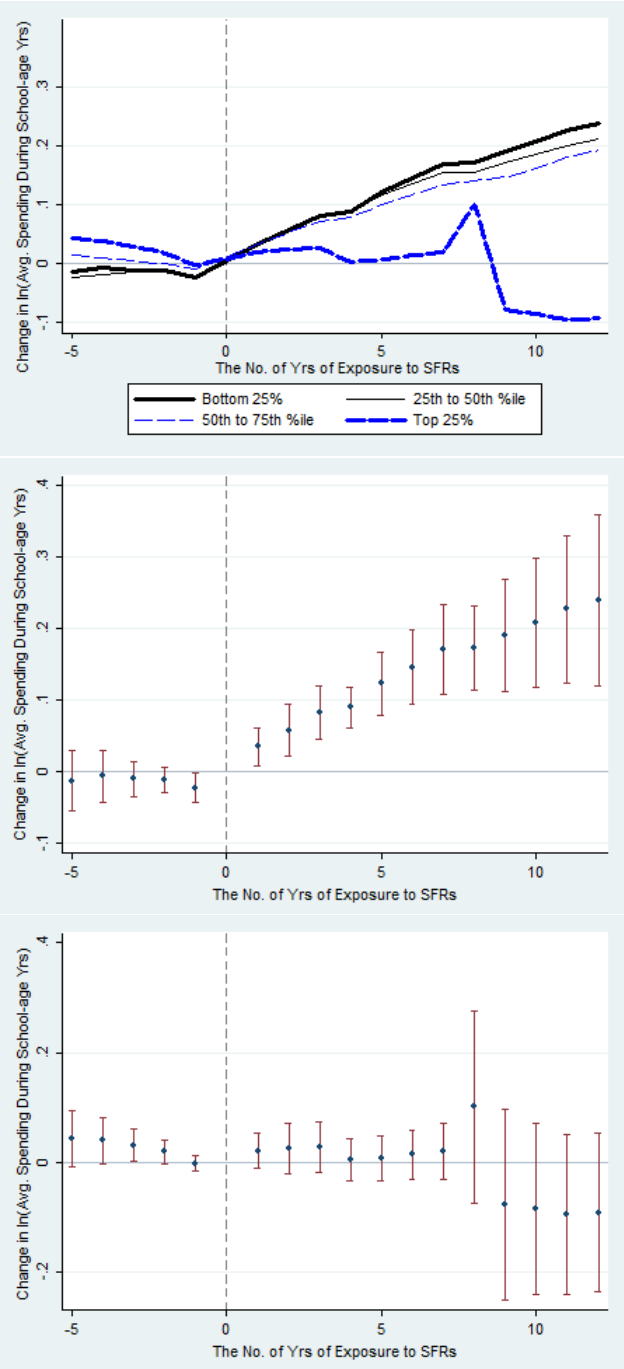
NOTES: This table reports the results of falsification tests described in Section 5.5.1. Each column is a separate regression with a different dependent variable. In the first (latter) two columns, the dependent variables are college attendance rate (mean income rank) of disadvantaged and advantaged children, respectively. $\ln(\overline{PPE_{5-17}})_{cb}$ is the natural log of average per pupil school spending (in real 2005 dollars) during school-age years (ages 5 through 17) for birth cohort b from county c. $\ln(\overline{PPE_{19-24}})_{cb}$ is the natural log of average per pupil school spending (in real 2005 dollars) during non-school ages (ages 19 through 24) for birth cohort b from county c. Dep var mean is the mean of the dependent variable. I use the specification including birth cohort fixed effects, county fixed effects, and 1980 state characteristics interacted with linear birth cohort trend. I also control for other government expenditures that may affect the sample cohort (1980-1988), including food stamps, AFDC, Medicaid, EITC and unemployment insurance. All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county. *** 1%, ** 5%, * 10%.

A Appendix

Table A.1: List of First Court-mandated School Finance Reforms, 1967-2010

State	Case name, year
Alabama	Alabama Coalition for Equity v. Hunt; Harr v. Hunt, 1993
Alaska	Kasayulie v. Alaska, 1999
Arizona	Roosevelt v. Bishop, 1994
Arkansas	Dupree v. Alma School District No. 30, 1983
California	Serrano v. Priest, 1971
Connecticut	Horton v. Meskill, 1978
Idaho	Idaho Schools for Equal Educational Opportunity v. State, 1998
Kansas	Knowles v. State Board of Education, 1972
Kentucky	Rose v. The Council for Better Education, Inc., 1989
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
Missouri	Committee for Educational Equality v. Missouri, 1993
Montana	Helena Elementary School District No. 1 v. State of Montana, 1989
New Hampshire	Claremont New Hampshire v. Gregg, 1993
New Jersey	Robinson v. Cahill, 1973
New Mexico	Zuni School District v. State, 1998
New York	CFE v. State, 2003
North Carolina	Leandro v. State, 1997
Ohio	DeRolph v. Ohio, 1997
Oregon	Pendleton School District v. State of Oregon, 2009
South Carolina	Abbeville County School District v. State, 2005
Tennessee	Tennessee Small School Systems v. McWheter, 1993
Texas	Edgewood Independent School District v. Kirby, 1989
Vermont	Brigham v. State, 1997
Washington	Seattle School District No. 1 of King County v. State, 1977
West Virginia	Pauley v. Kelly, 1979
Wisconsin	Buse v. Smith, 1976
Wyoming	Washakie v. Herschler, 1980

Figure A.1: Event-Study Estimates of the Effect of the Court-ordered School Finance Reform on Average Per-pupil Public School Spending during School-age Years (Using 1980 Pre-reform Spending Instead of Median Household Income)



NOTES: This figure plots the event study estimates, $\alpha_{t,q}$ in equation (3), where the dependent variable is log of average per-pupil public school spending during school-age years (ages 5-17). The top graph shows the estimates for all the quartile in the state distribution of pre-reform spending. The middle and bottom graph show estimates for the 1st and 4th quartile, respectively. The bars extending from each point represent the bounds of the 95 percent confidence interval calculated from standard errors that are clustered at the county level. I use the specification including birth cohort fixed effects, county fixed effects, and 1980 state characteristics interacted with linear birth cohort trend. I also control for other government expenditures that may affect the sample cohort (1980-1988), including food stamps, AFDC, Medicaid, EITC and unemployment insurance.

Table A.2: OLS and 2SLS Estimates of the Effect of Public School Spending on College Attendance Rate (Using 1980 Pre-reform Spending Instead of Median Household Income)

Dep variable:	**College Attendance Rate at Age 19**					
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Disadvantaged Children					
$\ln(\overline{PPE_{5-17}})_{cb}$	1.20*	1.01	1.12	13.05**	16.50***	17.22***
	(0.70)	(0.70)	(0.71)	(5.70)	(5.59)	(5.69)
N	10,200	10,200	10,200	10,168	10,168	10,168
Dep var mean				32.79		
	Panel B: Advantaged Children					
$\ln(\overline{PPE_{5-17}})_{cb}$	2.03**	1.99**	2.04***	21.75***	22.56***	23.11****
	(0.97)	(0.96)	(0.96)	(5.16)	(5.67)	(5.81)
N	10,200	10,200	10,200	10,168	10,168	10,168
Dep var mean				68.30		
	Panel C: Gap between Adv. and Dis.					
$\ln(\overline{PPE_{5-17}})_{cb}$	0.83*	0.98**	0.92**	8.70** 7	6.06**	5.89*
	(0.48)	(0.45)	(0.47)	(3.63)	(3.07)	(3.03)
N	10,200	10,200	10,200	10,168	10,168	10,168
Dep var mean				31.30		
Cohort FE	x	x	x	x	x	x
County FE	x	x	x	x	x	x
1980 State*Trend		x	x		x	x
Other Policies			x			x

NOTES: This table reports the OLS and 2SLS DID estimates of the effect of average per-pupil public school spending during school-age years on college attendance. Each column is a separate regression. In Panel A/B/C, the dependent variable is the college attendance of disadvantaged children / college attendance of advantaged children / the gap in college attendance between advantaged and disadvantaged children. The first(latter) three columns show the estimates of OLS (2SLS). $\ln(\overline{PPE_{5-17}})_{cb}$ is the natural log of average per pupil school spending (in real 2005 dollars) during school-age years (ages 5 through 17) for birth cohort b from county c. Dep var mean is the mean of the dependent variable. All regressions account for birth cohort fixed effects and county fixed effects. 1980 State*Trend refers to 1980 state characteristics (poverty rate, black rate, urban rate, high school graduates rate, and population size) interacted with linear birth cohort trend. Other Policies refers to the average expenditures on other welfare programs (food stamps, AFDC, Medicaid, EITC and unemployment insurance) during childhood (ages 0 to 17). All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county. *** 1%, ** 5%, * 10%.

Table A.3: OLS and 2SLS Estimates of the Effect of Public School Spending on Intergenerational Income Mobility (Using 1980 Pre-reform Spending Instead of Median Household Income)

Dep variable:	**Mean Income Rank at Age 26**					
	OLS			2SLS		
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Disadvantaged Children					
$\ln(\overline{PPE_{5-17}})_{cb}$	0.20	0.53**	0.51**	-0.63	-0.29	-0.34
	(0.31)	(0.25)	(0.24)	(1.81)	(1.20)	(1.15)
N	8,029	8,029	8,029	8,004	8,004	8,004
Dep var mean	45.66					
	Panel B: Advantaged Children					
$\ln(\overline{PPE_{5-17}})_{cb}$	0.62	0.81**	0.80**	3.69**	3.68**	3.64**
	(0.48)	(0.36)	(0.39)	(1.73)	(1.44)	(1.43)
N	8,029	8,029	8,029	8,004	8,004	8,004
Dep var mean	58.90					
	Panel C: Gap between Adv. and Dis.					
$\ln(\overline{PPE_{5-17}})_{cb}$	0.42	0.28	0.29	4.32**	3.98**	3.98**
	(0.34)	(0.29)	(0.29)	(1.48)	(1.23)	(1.25)
N	8,029	8,029	8,029	8,004	8,004	8,004
Dep var mean	13.24					
Cohort FE	x	x	x	x	x	x
County FE	x	x	x	x	x	x
1980 State*Trend		x	x		x	x
Other Policies			x			x

NOTES: This table reports the OLS and 2SLS DID estimates of the effect of average per-pupil public school spending during school-age years on income rank. Each column is a separate regression. In Panel A/B/C, the dependent variable is the income rank of disadvantaged children / income rank of advantaged children / the gap in income rank between advantaged and disadvantaged children. The first(latter) three columns show the estimates of OLS (2SLS). $\ln(\overline{PPE_{5-17}})_{cb}$ is the natural log of average per pupil school spending (in real 2005 dollars) during school-age years (ages 5 through 17) for birth cohort b from county c. Dep var mean is the mean of the dependent variable. All regressions account for birth cohort fixed effects and county fixed effects. 1980 State*Trend refers to 1980 state characteristics (poverty rate, black rate, urban rate, high school graduates rate, and population size) interacted with linear birth cohort trend. Other Policies refers to the average expenditures on other welfare programs (food stamps, AFDC, Medicaid, EITC and unemployment insurance) during childhood (ages 0 to 17). All estimates are weighted using the population size for the corresponding county and cohort. The standard errors are clustered by county. *** 1%, ** 5%, * 10%.