# School Finance Equalization Increases Intergenerational Mobility

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**Abstract**

This paper estimates the causal effect of equalizing revenues across school districts on students' intergenerational mobility. I exploit cohort differences in exposure to equalization generated by state-level reforms. To address endogeneity of post-reform revenues due to household sorting after a reform, I use a simulated-instruments approach that uses newly collected data on states' funding formulas to simulate revenues without sorting. I find that equalization has a large effect on mobility of low-income students. Reductions in input gaps between low-income and high-income districts are likely channels behind this effect.

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Large differences in intergenerational income mobility (IGM) exist across areas of the US. The probability that a child born in a family in the bottom quintile of the national income distribution will reach the top income quintile during adulthood is 14.3 percent on average in Utah, but only 7.3 percent in Tennessee (Chetty et al., 2014). While part of these differences might be due to different people selecting into different places, studies of movers across US counties have highlighted a causal relationship between growing up in certain areas and long-run outcomes (Ludwig et al., 2013; Chetty et al., 2016; Chetty and Hendren, 2018a).

Less is known, however, about what makes a place successful at ensuring equal economic opportunities to all children regardless of their backgrounds. Places with high IGM tend to have lower racial and income segregation, lower inequality, higher social capital, and better schools (Chetty and Hendren, 2018b). While these patterns are suggestive of a role for institutions and public policies, they cannot be interpreted as causal. Yet, understanding the role of public policies is the first step towards raising IGM.

This paper moves beyond descriptive associations and examines the causal role of school finance equalization, i.e., a reduction in the differences in public school revenues and expenditures across school districts within a state. Historically, US schools have been primarily funded with revenues from local property taxes. As a consequence, wealthier districts (with a larger tax base) have received and spent more per pupil than poorer districts. These between-district disparities vary across states due to differences in funding formulas, which define revenues as a combination of state funds and local levies. In 1980, for example, the gap in revenues between the lowest-spending and the highest-spending district was 70 percent in California, but only 40 percent in Maryland. In an attempt to equalize spending, states have reformed their school finance schemes through changes in these formulas. While often sharing a common objective, school finance equalization reforms have taken various forms across states and over time, with some being more successful at equalizing spending than others (Hoxby, 2001).

Using variation in the distribution of per pupil revenues generated by 43 reforms passed across US states between 1980 and 2004, I study the causal effects of equalization on children's IGM. I use the "absolute" measure of IGM of Chetty et al. (2014), defined as children's average percentile in the national income distribution given the income percentile of their parents, and calculated separately for each commuting zone (CZ), cohort (1980-1986), and parents' income quantile. I measure equalization in school revenues as the slope of the relationship between per capita income and per pupil revenues across districts in each state, denoted by $\beta$ (as in Hoxby, 1998; Card and Payne, 2002; Lafortune et al., 2018). When revenues are perfectly equalized in a state, $\beta$ equals zero; when wealthier districts receive more, $\beta$ is positive.

I show that school finance reforms led to a sharp decline in $\beta$, i.e., to a reduction in the difference in school revenues between districts serving children from poorer and wealthier families. A simple model linking education investments to children's incomes predicts that this decline, which breaks the link between public education investments and parental income, should increase the income rank of low-income children relative to heir parents.[1] The model also shows how equalizing revenues across districts is not necessarily akin to raising resources for poor districts in a state: It is possible to have reforms that equalize spending across districts while actually reducing overall spending levels (Hoxby, 2001).[2] Justified by the theoretical model and the level of aggregation of the outcome variable at the CZ level (instead of the district), which prevents me from directly estimating the revenue elasticity of IGM, I chose to focus my empirical analysis on the effects of revenue equalization, as opposed to revenue increases.

To study the effects of equalization on IGM, I exploit the fact that different cohorts were in school during different years and therefore experienced different degrees of equalization (different $\beta$s) across states. For example in Massachusetts, which had a reform in 1994, the 1980 cohort (in school between 1986 and 1998) experienced a $\beta$ equal to 0.041, while the 1986 cohort (in school between 1996 and 2008) experienced a $\beta$ equal to 0.016. In Florida, which had no reform between 1986 and 2004, these two cohorts experienced a $\beta$ equal to 0.014 and 0.021, respectively. Assuming that the timing of each reform is unrelated to other determinants of children's outcomes, one can estimate the effects of equalization by comparing measures of IGM across cohorts within each state. In support of this assumption, I show that IGM was on a flat trend in the years leading to each reform.[3]

OLS estimates indicate that a one-standard deviation reduction in $\beta$ (equivalent to a \$4,500 reduction in the difference in per pupil revenues between the richest and the poorest districts in a state) is associated with a 2.1 percentiles increase in the income rank of children with parental income in the 10th percentile. This is equivalent to a 5.4 percent increase in income.

[1] Existing research has shown that early investments in human capital are among the major determinants of future income (Becker and Tomes, 1979), especially for disadvantaged children (Cunha et al., 2010).

[2] Another possibility is that rich and poor districts compete for scarce inputs (such as good teachers, Hanushek et al., 2004); in this case, a district's revenues *relative to other districts* will impact children's outcomes, and not just absolute revenue levels. I investigate this possibility in Section 5.2.2.

[3] This assumption has also been extensively discussed and argued for by Hoxby (2001); Lafortune et al. (2018); Jackson et al. (2015).

By comparison, the same reduction in $\beta$ has a smaller and insignificant relationship with the rank of children with parental income in the 90th percentile.

Under the assumption of exogeneity of changes in $\beta$, these estimates capture the causal effect of a decline in $\beta$ on IGM. However, changes in the funding formula alter the relationship between the "price" of school spending to taxpayers and the amount of public good they receive in return. This might induce households to "vote with their feet" (Tiebout, 1956) and sort across districts.[4] Because post-reform revenues are a function of funding formula parameters, the tax base, and all other population-specific determinants of funding, which are affected by sorting, the post-reform $\beta$ can be endogenous. I address this issue with a simulated-instruments approach that directly exploits changes in the funding formulas. Combining a variety of administrative and legislative sources, I assemble information on the formulas in place in each of twenty states between 1986 and 2004 and all the district-level variables entering each formula. With this information I simulate district revenues in the absence of sorting, using the post-reform formula but keeping districts' characteristics (e.g. property values, enrollment, and income) fixed at their pre-reform values. I then use these simulated revenues to construct a simulated version of $\beta$, which I use as an instrument for $\beta$.

Two-stages least squares (2SLS) estimates of the effects of changes in $\beta$ indicate that school finance equalization has a sizable positive effect on IGM. A one-standard deviation reduction in $\beta$ raises the income rank of children with parental income in the 10th percentile by 4.4 percentiles. This is equivalent to a 11.8 percent increase in income. The same reduction in $\beta$ has a smaller effect on children with parental income in the 90th percentile. These estimates imply that the average reform (which reduces $\beta$ by approximately one standard deviation) would close approximately 16 percent of the gap between the lowest-IGM and the highest-IGM CZ.[5] Importantly, 2SLS estimates are larger than OLS, highlighting the importance of properly addressing the endogeneity issue.

Because some school finance reforms (especially those passed after 1990) raised spending levels in all districts, it is possible that the estimated effects of a reduction in $\beta$ capture not only revenue equalization, but also any increases in spending levels. To check for this possibility I perform three tests. First, I show that my results hold when controlling for total school

[4] Aaronson (1999); Dee (2000); Figlio and Lucas (2004); Epple and Ferreyra (2008); Chakrabarti and Roy (2015) provide evidence of this type of sorting in various contexts.

[5] The gap in absolute mobility between the highest-IGM CZ (Sioux Center, IA) and the lowest-IGM one (Clarksdale, MS) is equal to 27.6 percentiles for children from families in the 10th percentile.

spending in each state. Second, I re-estimate the effects of a decline in $\beta$ on IGM separately for states that experienced an "equity" reform (passed prior to 1990 and focused on equalizing spending across districts), and those that experienced an "adequacy" reform (designed to raise spending to a minimum acceptable level in all school districts in a state). Lastly, I re-estimate the effects separately for states that experienced a reform that "leveled up" spending and those with a reform that "leveled down." If the effect of $\beta$ mainly captures level effects (as opposed to equalization effects), estimates should be bigger for adequacy reforms and for those that leveled down. Instead, I find that these sets of estimates are indistinguishable from each other.

My estimates also show that equalization is most effective when experienced earlier in a child's education career. A one-standard deviation reduction in $\beta$ raises the income rank of children with parents in the 10th percentile by 5.6 percentiles if the reform that generated this decline was experienced during elementary school, but only 1.8 percentiles if it was experienced during high school. This finding is in line with a large literature highlighting the importance of early childhood investments for long-run outcomes (Cunha and Heckman, 2010).

In line with the prediction of the model, I show that the effects of equalization are considerably larger in states with more inter-district competition. The effects are also larger in CZs with higher income inequality and segregation. A possible explanation is that, when cross-district income inequality is high, the same reduction in $\beta$ might translate into a larger increase in revenues in lower-income districts relative to higher-income ones. Similarly, when segregation is high, a reduction in $\beta$ is more likely to increase revenues for lower-income children.

In the last part of the paper I explore potential channels for the main results. Specifically, I provide suggestive evidence that school finance equalization increases IGM through a reduction in the gaps in basic school inputs (such as the number of teachers) between richer and poorer districts.

This paper makes three main contributions. First, it provides one of the first causal explanations for the differences in IGM across US areas illustrated by Chetty et al. (2014).[6] In a recent paper, Rothstein (2019) shows that differences in school quality explain only a small portion of the observed cross-CZ differences in IGM. While Rothstein's findings are descriptive and focus on cross-sectional variation, this paper shows that school finance equalization *causes* a sizable improvement in long-term outcomes of economically disadvantaged children,

[6]Most of the earlier literature on IGM is descriptive and has focused on estimating the correlation in earnings of parents and children (see Black et al., 2011, for a survey). Another related strand of research has attempted to perform international comparisons of intergenerational income elasticities (Solon, 2002).

in line with Card et al. (2018). This implies that equalization can be an engine for IGM, even if it explains a relatively small share of the cross-CZ differences in IGM.[7] My results are also informative of one of the mechanisms through which equalization in school resources affects children's long-run outcomes, such as equalization in school inputs.

Second, this paper contributes to a large literature on the effects of school resources on students' outcomes, recently summarized by Jackson and Mackevicius (2021) and which includes observational (Hanushek, 1986, 1997, 2003), quasi-experimental (Card and Krueger, 1992; Hyman, 2017), and experimental studies (Krueger, 1999; Dynarski et al., 2013).[8] A few works have used school finance reforms as a source of variation in school spending to study the effects on student achievement and educational attainment (Hoxby, 2001; Card and Payne, 2002; Hyman, 2017; Lafortune et al., 2018). In the closest paper to mine, Jackson et al. (2015) explore the long-run effects of expenditure increases triggered by these reforms and find that they raised students' incomes and reduced poverty. I extend this literature by studying the effects of the reforms on IGM, a measure of the relationship between children's and parents' outcomes which has received considerable attention in recent years.

Lastly, this paper highlights the importance of accounting for the endogeneity in post-reform expenditure and for the differences in funding schemes across states when studying the effects of reform-induced changes in revenues and expenditures. To capture these differences, Jackson et al. (2015) instrument expenditure with the timing and "type" (e.g. foundation plan, or equalized effort) of each reform. This approach, however, cannot fully account for the disparate ways in which different reforms (including those of the same type) affected the distribution of revenues across districts and triggered different household responses. This might affect the estimates of the effects of equalization in an important way. My approach, and the accompanying hand-collected data, can be used in other settings as well.

The rest of the paper is organized as follows. Section 1 describes school finance equalization reforms. Section 2 presents a simple model to illustrate the relationship between school finance equalization and IGM. Sections 3 and 4 describe the data and the measure of inequality in school revenues. Section 5 presents the empirical strategy and the effects of equalization on IGM. Section 6 investigates the mechanisms behind these effects, and Section 7 concludes.

[7] My findings are also in line with works illustrating the effectiveness of increased education expenditure (Cascio et al., 2013; Cascio and Reber, 2013) and improved access to high-quality education (Meghir and Palme, 2005; Pekkarinen et al., 2009; Havnes and Mogstad, 2015) in closing outcome gaps between advantaged and disadvantaged students.

[8] This literature was initiated decades ago by the Coleman report (Coleman et al., 1966).

# 1 School Finance Equalization Reforms

US school districts have historically drawn a large portion of their revenues from local property taxes (Howell and Miller, 1997; Hoxby, 2001). As a result, wealthier districts (with a larger tax base) have been able to spend more compared to poorer districts. Over time, this has created large disparities in per pupil spending across districts within each state. Capitalization of the quality of public schools into house prices has exacerbated these differences.

To reduce these disparities, states have passed school finance equalization reforms. Some of these reforms followed rulings of unconstitutionality of funding schemes by states' Supreme Courts. Others were the outcomes of legislative processes. Earlier reforms, passed in the 1970s and 1980s, had a predominant equity motive and were designed to weaken the relationship between each district's fiscal capacity and the amount of resources spent on public schools (Card and Payne, 2002; Murray et al., 1998; Jackson et al., 2015). Later reforms have focused more on adequacy, i.e., have sought to guarantee a minimum level of expenditure to children in all districts (Lindseth, 2004; Lafortune et al., 2018).

Regardless of their specific motives, school finance equalization reforms have changed states' funding schemes, typically summarized by a formula. This formula expresses a district's total revenue as a function of a number of variables, including (but not limited to) enrollment, fiscal capacity, and fiscal effort (i.e., local tax rates). The formulas also define the size of state transfers to each school district, and some include limits on total spending or local tax rates. Hoxby (2001) and Jackson et al. (2014) provide a categorization of school finance plans into a number of "types," depending on whether they aim at ensuring a minimum level of expenditure ("foundation" or "equalization" plans), guaranteeing a certain tax base ("guaranteed tax base"), or providing incentives toward fiscal effort ("rewards for effort"). Nearly all funding formulas, however, are the combination of two or more of these types. In addition, the parameters of each formula vary considerably across states and over time. As a result, even plans of the same type have had vastly different effects on districts' revenues across states.

One common aspect of school finance schemes is that the basis for equalization, i.e., the tax base, is endogenous. A change in the funding formula provides households with incentives to sort across school districts depending on their preference for public schools and their income; these movements affect house prices and district revenues. The failure of policymakers to fully understand and anticipate these responses when designing school finance plans has caused some reforms to *reduce* overall expenditure on public schools.[9]

Empirical evidence on the effects of school finance equalization reforms on student achievement is mixed, with some studies finding positive effects on test score gaps (Guryan, 2001; Card and Payne, 2002; Papke, 2005; Roy, 2011; Lafortune et al., 2018) and educational attainment (Hyman, 2017) and others finding no effects (Downes et al., 1997; Hoxby, 2001). Jackson et al. (2015) find large effects of increased expenditure on income and poverty incidence among low-income students. This paper focuses on the effects of equalization in school revenues on students' IGM, with the goal of quantifying the extent to which breaking the link between parental resources and public school spending breaks the link between parents' and children's economic outcomes.

## 2 A Model of School Finance and Intergenerational Mobility

I use a simple framework to illustrate the relationship between school finance equalization and IGM. The world is populated by two generations: parents, with income $x$, and children, with income $y$. Parents and children live in school districts and each district belongs to a state. Districts provide public education and each child goes to school in the district where she lives. The income of a child in family $i$, living in school district $d$ and state $s$, is modeled as:

$$y_{id} = \theta x_{id} + \gamma q_d \tag{1}$$

where $x_{id}$ is parental income and the parameter $\theta$ captures all the possible ways in which parental income affects the income of the child (e.g. transmission of ability or private investments in education). The variable $q_d$ is the public school system's "effective" investment on the child's education, defined as

$$q_d = e_d - \frac{1}{N_s - 1} \sum_{j \neq d, j \in s} \pi_s (e_j - e_d) \tag{2}$$

where $e_k$ is public spending per pupil in district $k$ and $N_s$ is the number of districts in $s$. In words, the effective education investment in district $d$ depends on (a) direct spending in that district and (b) spending in all the other districts in the state. This formulation allows for the

[9] For example, California's 1978 *Serrano* reform was followed by an unprecedented decline in expenditure (Silva and Sonstelie, 1995), or a "leveling down"(Hoxby, 2001). Similarly, Texas's 1993 "Robin Hood" plan is estimated to have destroyed \$27,000 per pupil in property values (Hoxby and Kuziemko, 2004).

education investment in a given district to depend on the competition for scarce resources (e.g. teachers) from all the districts in the state. The extent of this competition depends on the parameter $\pi_s$ and on spending in other districts. District $k$'s spending is determined as:

$$e_k = (1 - \beta_s)K_s + \beta_s x_k \; \forall k \in s \tag{3}$$

where $K_s$ is a state-level constant and $x_k$ is average parental income in district $k$. In this expression, the parameter $\beta_s$ captures the degree of equalization in school expenditure within each state. When $\beta_s = 0$, $e_k = K_s$ and spending is fully equalized across all districts in $s$. When $\beta_s > 0$, $e_k$ increases in $x_k$ and richer districts in the state spend more (and vice versa).

The income of the child can be rewritten as a function of $\beta_s$, $K_s$, and $\pi_s$ as follows:

$$y_{id} = \theta x_{id} + \gamma K_s + \gamma\beta_s \left[(1 + \pi_s)x_d - (K_s + \pi_s \bar{x}_{s,-d})\right] \tag{4}$$

where $\bar{x}_{s,-d}$ is the average parental income in all districts in state $s$, other than $d$.

This framework can be used to highlight the relationship between IGM and equalization, captured by the parameter $\beta_s$. Following Chetty et al. (2014), I define IGM as the child's expected income rank in the national distribution given her state $s$ and the income percentile $r$ of her parents:

$$M_s^r = F_y\left(y_{id} | F_x(x_{id}) = r/100\right) \tag{5}$$

where $F_y(\cdot)$ and $F_x(\cdot)$ denote the CDFs of children's and parents' incomes, respectively. For simplicity, I abstract from within-district income differences and I assume $x_{id} = x_d$ for every $i$ in $d$. Denoting the percentile function of $x$ as $h(r) = F_x^{-1}(r/100)$ (where $r$ denotes a percentile), and substituting the expression for the child's income from equation (1), I can express IGM as:

$$M_s^r = F_y(\theta h(r) + \gamma K_s + \gamma\beta_s \left[(1 + \pi_s)h(r) - (K_s + \pi_s \bar{x}_{s,-d})\right]) \tag{6}$$

Being a CDF, the function $F_y(\cdot)$ is non-decreasing. It follows that

$$\frac{\partial M_s^r}{\partial \beta_s} \leq 0 \text{ when } h(r) \leq \tilde{K}_s = \frac{K_s + \pi_s \bar{x}_{s,-d}}{1 + \pi_s}$$

This last expression implies that a higher $\beta_s$ is associated with lower IGM for all children with parental income below a state-specific threshold $\tilde{K}_s$. Furthermore, this relationship is stronger

(a) the higher is parental income in the other districts, and (b) the strongest is the competition among districts. By the same token, a higher $\beta_s$ is associated with a higher IGM for children with parental income above $\tilde{K}_s$. An important corollary of this result is that, if $\tilde{K}_s$ is large enough that all districts in the state have incomes below this threshold, a given decline in $\beta_s$ would lead to an increase (albeit smaller) in IGM even for children in the richest districts in the state. In the remainder of the paper I test these predictions studying the effects of a decline in $\beta_s$ on IGM of children with different parental incomes.

## 3 Data

To conduct the empirical analysis I combine data from multiple sources. In the final data set each observation corresponds to a given CZ, cohort, and parental income quantile within the state. The components of the final data set are briefly described below; more detail can be found in the Online Appendix B. Expenditures, revenues, and income are converted to 2000 US dollars for easier interpretation.

**School Districts' Finance Data** Information on revenues, expenditures and enrollment comes from the National Center for Education Statistics' School District Finance Survey (F-33), which reports total revenues and expenditures and enrollment figures for each district and year. I calculate per pupil revenues dividing total revenues by district enrollment. Table I (Panel A) summarizes the variation in school revenues across districts within each CZ and state. While the difference in revenues between the highest-income and the lowest-income district is small on average, it ranges from -$2,306 to $12,965 across states in 1990.

**Funding Formula Components** My instrumental variables approach relies on simulating district revenues using states' funding formulas. This procedure requires information not only on total revenues, but also on all the variables entering the formula (such as property values, enrollment, household income, tax rates, etc.). Because the nature of these elements and the way they are measured vary across states, this information is not available from a unified source.

To address this data limitation I constructed a novel district-level panel dataset for each state, drawing from states' historical school finance records accessed through a series of FOIA requests. Each dataset contains all the elements of the funding formula in place in each year in the state, as well as total expenditures and revenues. I was able to construct these datasets for 20 states, comprising 405 CZs and 8,102 school districts enrolling 62 percent of all US students.[10] Appendix DI describes the variables of each state dataset and Appendix D describes the funding formulas in detail.

**School Finance Reforms** I compiled a list of all state-level school finance reforms passed between 1980 and 2004, covering the time period when the cohorts at study (born 1980-1986) were in grades 1 to 12 (i.e., 1986-2004). To do so I draw information from Gold et al. (1992), Sielke et al. (2001), and Verstegen and Jordan (2009), which describe state's funding schemes over time and include details of the timing and content of each reform. I complement these data with information from Manwaring and Sheffrin (1997), Hoxby (2001), Jackson et al. (2015), and Lafortune et al. (2018). Information is largely consistent across the different sources. In case of discrepancies, I gave priority to Gold et al. (1992) and Sielke et al. (2001) for older events and to Lafortune et al. (2018) for more recent ones. Appendix E briefly describes the reforms used in the analysis, and Appendix Figures AIII and AIV summarize the timing of these events.

**Median District Income** I calculate districts' median household income using income tabulations from the US Census of Population and Housing for the years 1980, 1990, and 2000 and from the American Community Survey for the year 2010.[11] I link these data with information on per pupil school revenues to compute measures of equalization in each state and year.

**Intergenerational Mobility** I use the "absolute" measure proposed by Chetty et al. (2014), defined as children's rank in the national income distribution for a given CZ, cohort, and parental income quantile. To construct this measure, Chetty et al. (2014) use administrative tax records and estimate the intercept and slope of the linear relationship between parents' and children's national income ranks, for 637 out of 722 CZs and for each cohort of children born between 1980 and 1986. Children are assigned to CZs based on when they lived at age

[10] I obtained the data via direct or FOIA requests to each state's Department of Education. The requests were fulfilled by the states of California (data available for the years 1996-2004), Colorado (1994-2004), Florida (1988-2004), Georgia (1987-2004), Illinois (1987-2004), Kentucky (1991-2004), Louisiana (1993-2004), Massachusetts (1993-2004), Michigan (1990-2004), Minnesota (1991-2004), Montana (1994-2004), Nebraska (1993-2004), New Jersey (1988-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Texas (1986-2004), Utah (1986-2004), and Wisconsin (1986-2004). These 20 states, shown in Appendix Figure AII, are similar to all the other states with respect to a range of characteristics of schools, families, and households (Appendix Table AI).

[11] School district income tabulations are contained in the Census STF3F file for 1980 and published as part of the NCES School District Demographic System (SDDS) for the years 1990 and 2000. For the year 2010, I use the 2008–2012 district-level tabulations of the American Community Survey provided by the SDDS.

16, irrespective of whether they moved when they entered the labor market.[12]

I combine these IGM measures with CZ-specific parental income distributions to construct children's income ranks by CZ, cohort, and parental income quantiles *in the state*.[13] Chetty et al. (2014) report income levels for the 10th, 25th, 50th, 75th, 90th, and 99th percentiles of the parental income distribution in each CZ. To obtain the income levels of the corresponding state percentiles, I assume a uniform income distribution within each percentile and aggregate the CZ-specific income distributions, using counts of children in each CZ as weights. I then match these income levels to their percentiles in the national distribution and back out the corresponding income ranks of the children.

The final dataset contains children's ranks for 327 CZs, seven birth cohorts, and six state-specific parental income quantiles (the 10th, 25th, 50th, 75th, 90th, and 99th percentiles). On average, children with parental income below the national median experience upward IGM, whereas children with parental income above the median experience downward IGM (Table I, Panel B).[14] Wide differences exist across CZs (Appendix Figure AI): The mean percentile of children with parents on the 25th state percentile is as low as 33 in Milledgeville, GA and as high as 65 in Vernal, UT.

**Other School District Data** Additional district-level information from the NCES's Local Education Agency Universe Survey Data includes the number of teachers employed in each district and year, available for the years 1988 to 2010.

## 4 Measuring Inequality in School Revenues

The first step of my empirical analysis is to build a measure of inequality in per pupil revenues. In keeping with the theoretical framework, I measure inequality as the slope of the relationship

[12] A possible concern is that children's CZ at age 16 might be endogenous to a school finance reform, if – as I show later – household move in response to it. For this type of mobility to be endogenous, however, it would have to be occur across CZs. Studying the case of Michigan, (Chakrabarti and Roy, 2015) argue instead that post-reform sorting occurs within education markets, which typically coincide with either a CZ or a portion of it.

[13] I chose to define observations based on state-specific (rather than CZ-specific or national) quantiles because my empirical exercise focuses on the effects of equalization, a state-level phenomenon. Using CZ-specific quantiles would restrict the effect of equalization to be the same for all children with parental income in a given CZ-specific quantile, even if these quantiles corresponded to different levels of income across CZs. Using national quantiles would cause observations in the data set to include different numbers of people across states with very different income distributions. To see this, consider a state with 10 percent of individuals on the 25th national percentile and only 0.1 percent on the 99th percentile. If one observation corresponded to a national percentile, these two groups would receive equal weight in estimation, even though the first contains more people than the second.

[14] This result is not mechanical: income ranks of parents and children are defined relative to the national income distribution, whereas IGM is estimated at the CZ level.

between per pupil revenues and per capita income across districts within each state and year, captured by the parameter $\beta_{st}$ in the following equation:[15]

$$e_{dt} = \alpha_{st} + \beta_{st} x_{dt} + \varepsilon_{dt} \tag{7}$$

where $e_{dt}$ is per pupil revenue in district $d$ (located in state $s$) and year $t$, $x_{dt}$ is median per capita household income, and $\varepsilon_{dt}$ is an error term. The parameter $\beta_{st}$, estimated separately for each state $s$ and year $t$, represents the degree of inequality in school funding across districts. When revenues are higher (lower) in richer (poorer) districts, $\beta_{st}$ is positive. When instead revenues are higher in low-income districts or vice versa, $\beta_{st}$ is negative. Lastly, when the funding scheme is equalized and revenues are similar across richer and poorer districts, $\beta_{st}$ is close to zero. Appendix Figure AV shows the linear relationship between per-pupil revenues and per capita income across school districts in New Jersey and Georgia in 1990 and 2000. In New Jersey, which experienced a school finance equalization reform in 1991, the slope of the relationship (i.e., $\beta_{st}$) decreased in 2000 relative to 1990. In Georgia, which did not experience any reform, the slope remained constant over this decade.

To study the effects of changes in $\beta_{st}$ (measured at the year level) on IGM (measured at the cohort level), I assign each cohort a measure of revenue inequality experienced while in school, constructed as the average $\beta_{st}$ over the calendar years in which the cohort was in grades 1-12.[16] For cohorts born between 1980 and 1986, this requires estimating $\beta_{st}$ for each state and year between 1986 and 2004. Since income data are only available for Census years, I use the 1990 median district income to estimate $\beta_{st}$ for all years.[17]

On average, the parameter $\beta_{st}$ is equal to -0.002 for states without a school finance reform (with a standard deviation of 0.081); for states with a reform it equals -0.0001 in the years before the reform (with a standard deviation of 0.053) and -0.012 in the years after the reform (with a standard deviation of 0.056, Table I, Panel C). Event study estimates indicate that $\beta_{st}$ declines immediately following the reform and remains stable at this lower level over time (Figure I, top panel).[18]

[15]A similar approach has been used by Hoxby (1998); Card and Payne (2002); Lafortune et al. (2018).

[16]For example, the $\beta_s$ for the 1980 cohort is the average of the $\beta_{st}$ for the years 1986-1997.

[17]In robustness checks I use a version of $\beta_{st}$ obtained interpolating income values between Census years (Table VI, column 1).

[18]The figure shows point estimates and 90 percent confidence intervals of the coefficients $\delta_k$ in the equation $\hat{\beta}_{st} = \sum_{k=-3}^{10} \delta_k R_s \mathbb{1}(t - ryear_s = k) + \varepsilon_{st}$, where $\hat{\beta}_{st}$ is the estimated $\beta_{st}$ coefficient for state $s$ and year $t$, $R_s$ equals 1 if state $s$ experienced a school finance reform between 1986 and 2004, and $ryear_s$ is the year of the first of these reforms. Appendix Figure AVI shows estimates of $\beta_{st}$ separately for "equity" reforms (passed before 1990)

# 5 Effects of School Finance Equalization on IGM

With a measure of inequality in school revenues, I can now test the theoretical predictions derived in Section 2 and estimate the effects of spending equalization on children's IGM. To identify these effects, I exploit variation in exposure to equalization across cohorts and states, given by exogenous differences in the timing and effectiveness of the reforms.

**Identification argument and variation** Figure II shows the average income rank of children with parents in the 25th percentile by exposure to a reform (i.e., the number of post-reform school years of a given a cohort, calculated as birth year + 18 – (reform year + 1)).[19] In states with an "effective" reform (resulting in a negative post-reform $\beta$ or a decline in $\beta$ of at least 20 percent, solid line), IGM gradually increases with exposure relative to states with no reform. In all other states with an ineffective reform, on the other hand, IGM does not change significantly.[20] These estimates provide a first piece of evidence that exposure to effective reforms is associated with increased IGM. However, they are based on an arbitrary definition of the effectiveness of a reform and only informs us on the IGM of children whose parents are at the bottom of the income distribution.

**Empirical model** To more rigorously test the effect of changes in $\beta$ on IGM and explore its effects on children with different parental incomes, I estimate the following equation:

$$M_{cbx} = \delta_0 \hat{\beta}_{s(c)b} + \delta \hat{\beta}_{s(c)b} \theta_{n(cx)} + \kappa_c + \tau_b + \theta_{n(cx)} + \omega_{cbx} \tag{8}$$

where the variable $M_{cbx}$ is the expected income percentile of children in CZ $c$, cohort $b$, and with parental income in the $x$-th state percentile (either the 10th, 25th, 50th, 75th, 90th, or 99th). The variable $\hat{\beta}_{s(c)b}$ is the estimated state and cohort-specific measure of equalization ($s(c)$ denotes the state where CZ $c$ is located). CZ fixed effects $\kappa_c$ control for CZ-specific, time-invariant determinants of IGM, and cohort fixed effects $\tau_b$ control for secular trends in IGM.

and "adequacy" reforms (passed after 1990). The initial drop in $\beta$ after an equity reform is slightly larger than after an adequacy reform. The former, however, tends to revert to its pre-reform values, while the latter remains stable over time.

[19] For example, the 1980 cohort in Massachusetts (where a reform was passed in 1994) was in school from 1986 to 1997; it was therefore exposed to a "post-reform" regime for 1980 + 18 - (1994 + 1) = 3 years: 1995, 1996, and 1997.

[20] The figure shows OLS points estimates and 90 percent confidence intervals of the coefficients $\delta_n$ in the equation $m_{cb} = \sum_{n=-6}^{12} \delta_n E_{n(sb)} + \theta_c + \tau_b + \varepsilon_{cb}$, where $m_{cb}$ is the mean rank of children in CZ $c$, cohort $b$, and with parents' income in the 25th percentile in the national income distribution, $E_n = b + 18 - (ryear_s + 1)$, $ryear_s$ is the year of the first school finance reform in state $s$ between 1980 and 2004), and the vectors $\theta_c$ and $\tau_b$ are CZ and cohort fixed effects. Observations are weighted by the number of children in each CZ and cohort. Standard errors are clustered at the state and birth cohort level.

The vector $\theta_{n(cx)}$ controls for parents' rank in the *national* income distribution $n(cx)$, to account for the fact that different states might have different income distributions.[21] The variable $\omega_{cbx}$ is an error term.

In this model, the parameter $\delta_0$ captures the effect of an increase in $\beta$ (a *decline* in equalization) on the income percentile of children with the lowest-ranked parental income in the national distribution. The parameter $\delta$ measures instead how much this effect changes as the parental income rank increases. I standardize $\hat{\beta}_{sb}$ across all CZs and cohorts and I calculate bootstrapped standard errors clustered at the level of the state and the year using a two-way procedure (Cameron et al., 2011), to account for the fact that $\beta_{s(c)t}$ varies at the state level and allow for spatial correlation in IGM.[22] For ease of interpretation, I describe my estimates in terms of a *reduction* in $\beta$, i.e., an increase in equalization.

### 5.1 OLS Estimates

OLS estimates of $\delta_0$ and $\delta$ in equation (8) are shown in column 1 of Table II. They indicate that a one-standard deviation reduction in $\beta$, equivalent to a \$4,500 reduction in the gap in per pupil revenues between the richest and the poorer districts in a state, is associated with a 2.4 percentile increase in IGM of children with parental income at the bottom of the income distribution (estimate of $\beta$ equal to -2.387, significant at 1 percent). An estimate of $\beta\times$ *parent pctile* equal to 0.024 indicates instead that this positive relationship is reduced by 0.02 percentiles with each additional percentile of parental income (significant at 1 percent). This implies that the same reduction in $\beta$ leads to a 2.1 percentile increase in IGM for children with parental income in the 10th percentile, a 1.8 percentile increase for children with parental income in the 25th percentile, and a smaller and insignificant 0.7 percentile increase for children with parental income in the 90th percentile. These results also indicate that the average reform, which decreases $\beta$ by approximately one standard deviation, would increase IGM of children from families in the 10th percentile by 2.1 percentiles and close approximately 8 percent of the gap between the lowest-IGM CZ (Clarksdale, MS) and the highest-IGM CZ (Sioux Center, IA).

[21] For example, the 25th CZ-specific percentile in Cleveland, MS corresponds to an income of \$15,000 and a 10th percentile in the national distribution; the same CZ-specific percentile in Sheboygan, WI corresponds to an income of \$52,500 and a 45th percentile in the national distribution.

[22] I calculate standard errors following Cameron et al. (2011). Specifically, for each regression I obtain 300 subsamples of the data (with replacement) in three separate steps. In the first I draw clusters of states. In the second I draw clusters of cohorts. In the third I draw clusters of states-by-cohorts. In each step, I estimate the parameters of interest for each subsample using the relevant method (OLS or IV) and use the distribution of the estimates across subsamples to construct the variance-covariance matrix of the parameters. This results in three variance-covariance matrices, which are then combined by adding the first two and subtracting the third.

Estimates are robust to controlling for state fixed effects (Appendix Table AIV).

### 5.2 Endogeneity Problem and Simulated Instrument

OLS estimates of $\delta_0$ and $\delta$ capture the causal effect of a decline in $\beta$ on IGM under the assumption of exogeneity of changes in $\beta$. While school finance reforms provide plausible exogenous shifts in spending across districts (Jackson et al., 2015; Lafortune et al., 2018), an endogeneity problem might still arise when using $\beta$ as an explanatory variable (as in equation (8)).[23] This issue, described in more detail in Appendix C, can be best seen by recognizing that post-reform revenues, expenditures, and $\beta$ are a function not only of the funding formula parameters, but also of the tax base and all other population-specific determinants of funding. Changes to the funding formula affect the tax price (i.e., the dollars of tax revenues required to increase spending by one dollar), which represents the "price" of public schools to taxpayers, and – in turn – households' budget constraints. Households could respond to this change in the tax price by "voting with their feet" (Tiebout, 1956) and moving to a different district (Aaronson, 1999; Dee, 2000; Figlio and Lucas, 2004; Epple and Ferreyra, 2008; Chakrabarti and Roy, 2015). Due to this sorting, variables such as house prices and property tax revenues (which enter the funding formula) could also change, while at the same time being directly related to and IGM and leading to the endogeneity of $\beta$.

The degree of bias in OLS estimates caused by this issue depends on the extent of this sorting. In Appendix C I use event studies of cross-county migration rates around a school finance equalization reform to show that these episodes were followed by an increase in migration (Appendix Figure AVII, panel a, and Table AIII). In addition, the absolute difference between the incomes of migrants and stayers also increased post-reform, indicating that households sort across counties based on income (Appendix Figure AVII, panel b).

**Constructing the Simulated Instrument** I address this endogeneity issue with a simulated-instruments approach (as in Gruber and Saez, 2002) which, similarly to Hyman (2017), directly exploits changes in *each state's* formula type and parameters generated by a reform.[24] The goal of this strategy is to isolate the exogenous variation in funding inequality, driven by the timing

[23] If instead one is interested in the *direct* effect of the passage of any school finance reform on students' outcomes (as in Lafortune et al., 2018), the exogeneity in the timing is the only required identifying assumption.

[24] Hyman (2017) focuses on Michigan's 1994 school finance reform and directly uses changes in the foundation grant (the relevant policy parameter for this reform) as an instrument for expenditures. Goldsmith-Pinkham et al. (2018) illustrate how, in a simulated-instruments context, identification leverages variation in the change in the parameters of a given policy. The source of exogenous variation used in my analysis is thus the same as in Hyman (2017), which I expand to include a large sample of US states.

of the reform and the funding formula, from the endogenous variation driven by sorting and changes in the tax base.

I construct $\beta^s$, instrument for $\beta$, as follows. First, I obtain the funding formulas in place in each school district and year. These formulas express total and per pupil revenues as a function of district-specific characteristics (such as enrollment, property tax rates, property values, and average gross income) and parameters set by state laws. I construct each formula using information from Gold et al. (1992), Sielke et al. (2001), Verstegen and Jordan (2009), and various state legislative bills (see Appendix D for details on each specific formula). I then use the formulas to simulate each district's post-reform revenues, holding endogenous characteristics (i.e., property values, property tax rates, and income) fixed at their pre-reform values.[25] Lastly, I construct $\beta^s$ for each state and year, re-estimating equation (7) using simulated instead of actual revenues.[26]

This simulated instrument is similar in spirit to that of Jackson et al. (2015, JJP hereafter), who instrument spending using the timing of each reform, districts' initial position in the state's expenditure and income distributions, and the type of funding plan (e.g. foundation). However, while their strategy relies on the assumption that all reforms of the same type had the same effect on expenditure conditional on a district's initial expenditure and income, my approach allows for different impacts of reforms of a similar type, but with different formulas and parameters. As I show in Appendix C, ignoring differences in funding formulas across states can be problematic because it can lead to a violation of the monotonicity assumption of IV. The presence of important heterogeneity in funding formulas and reforms is confirmed by evidence from house prices in Appendix Figure AIX: While some reforms (e.g. in Texas) were followed by a decline in the difference in prices between rich and poor districts, others did not trigger any changes (e.g. Michigan) or were followed by an increase in this difference (e.g. Massachusetts).

**Assumptions** The validity of my IV approach relies on the exogeneity of the timing of each reform and of the type and parameters of the funding formula. This assumption is supported by previous analyses of school finance reforms (Hoxby, 2001) that argue that equalization schemes are more likely to be a reflection of a particular legal rhetoric rather than of specific

[25] I adjust property values using the FHFA's US All Transactions Index (quarterly data, available at https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx) to account for nationwide changes in house prices, and I correct for inflation using the CPI.

[26] For states with no reform between 1986 and 2004, I simply set $\beta = \beta^{sim}$ for all years and cohorts.

objectives in terms of school spending and redistribution (which would explain why some of these reforms have had smaller-than-intended effects). In addition, the timing of a reform often depends on the length of a legislative process or on the timing of a court ruling. This suggests that both the timing and the type of reforms can be plausibly considered exogenous.

On average, the parameter $\beta^s$ equals 0.037 (with a standard deviation of 0.029) in the years preceding each reform, and it drops to -0.001(with a standard deviation of 0.049) in the years after the reform (Table I, Panel C; the difference is significant at 1 percent). Estimates from the first stage of the IV estimation reveal that $\beta^s$ is a strong predictor for $\beta$; the Kleibergen-Paap Wald F-statistic of the first stage (Stock and Yogo, 2002), shown in Table II, is equal to 18 (Table II, columns 3 and 4). The instrument is also uncorrelated with changes in house prices, migration rates, and differences in the incomes of migrants and incumbents, which are precisely the sources of the endogeneity that the instrument is supposed to address (Appendix Table AII).[27]

**IV Estimates** Before presenting the IV estimates of $\delta_0$ and $\delta$, I describe OLS estimates obtained on the IV subsample. These estimates, shown in column 2 of Table II, are very similar to the ones obtained on the full sample. They indicate that a one-standard deviation reduction in $\beta$ is associated to a 2.9 percentile increase in IGM of children with parental income at the bottom of the income distribution (estimate of $\beta$ equal to -2.918, significant at 1 percent), and that this positive relationship is reduced by 0.024 percentiles with each additional percentile of parental income (estimate of $\beta\times$ *parent centile*, significant at 1 percent).

Column 5 of Table II shows IV estimates, obtained using $\hat{\beta}^s_{s(c)b}$ and $\hat{\beta}^s_{s(c)b} \times \theta_{n(xc)}$ (the simulated versions of $\hat{\beta}_{s(c)b}$ and $\hat{\beta}_{s(c)b} \times \theta_{n(xc)}$) as instruments. First-stage estimates are shown in columns 3 and 4. Second-stage estimates indicate that a one-standard deviation reduction in $\beta$ leads to a 4.7 percentile increase in IGM of children with parental income at the bottom of the income distribution (estimate of $\beta$ equal to -4.681, Table II, column 5, significant at 1 percent). This positive relationship is reduced by 0.025 percentiles with each additional percentile of parental income (estimate of $\beta\times$ *parent centile*, Table II, column 5, significant at 1 percent). This implies that the same reduction in $\beta$ leads to a 4.4 percentile increase in IGM for children with parental income in the 10th percentile, a 4.1 percentile increase for children with parental

[27] Appendix Table AII shows estimates of a regression of $\beta^s$ on the average change in house prices, the average in-migration and out-migration rate, and the ratios between the incomes of in-migrants and out-migrants and the incomes of stayers, as well as state and year fixed effects. Observations are at the state and year level. These estimates indicate that none of these variables predict the change in $\beta^s$ over time.

income in the 25th percentile, and a smaller 2.5 percentile increase for children with parental income in the 90th percentile.

In Figure AXII (lighter line) I estimate the effects of a decline in $\beta$ separately for various quantiles of parental income in the national distribution. A one-standard deviation reduction in $\beta$ leads to a 4.5 percentile increase in IGM for children with parental income in the first decile (significant at 1 percent), but only 2.3 percentiles for children with parental income in the top one percent of the distribution (p-value equal to 0.10). Again, OLS estimates are smaller in magnitude than IV (darker line).

**Effects on Income** To quantify the magnitude of the effects of equalization in monetary terms, I use the national distribution of children's income to construct income levels for each CZ, cohort, and parental income quantile, and I use the logarithm of children's income as the dependent variable in equation (8). IV estimates indicate that a one-standard deviation reduction in $\beta$ leads to a 11.8 percent increase in income for children with parental income in the 10th percentile, a 10.7 percent increase for children with parental income in the 25th percentile, and a smaller 6.2 percent increase for children with parental income in the 90th percentile (with an estimate of $\beta$ equal to -0.125 and of $\beta \times$ *parent centile* equal to 0.001, Table III, column 1, both significant at 1 percent).

**Reduced-Form Estimates** While useful to capture the causal effects of equalization on IGM, IV estimates might be difficult to use for policy purposes: Since households can sort after a reform, policy-makers do not have direct control on $\beta$, but only on $\beta^s$ through changes in the formula type and parameters. In column 6 of Table II I estimate the "reduced-form" effect of $\beta^s$ on IGM. These estimates indicate that a one-standard deviation decline in $\beta^s$ leads to a 3.9 percentile increase in the income rank of children with parents in the 10th percentile (significant at 1 percent). This positive and large estimate implies that a reform which – absent household responses – is effective at equalizing revenues across districts can have significant effects on children's IGM.

**Effects of Revenue Equalization vs Revenue Increases** So far I have interpreted the effects of a decline in $\beta$ as those of equalization on IGM. As explained by Hoxby (2001), different reforms had different effects on spending *levels* in each state: Some raised it, some reduced it. Estimates of the effect of a decline in $\beta$ on IGM will therefore also capture the effects of changes in the level of resources in some districts. To quantify the extent to which the estimated effect of a change in $\beta$ on IGM captures the effect of equalization, rather than changes in levels, I proceed in two ways. First, in Appendix Table AV I control for average per pupil expenditure in state $s$ on cohort $b$ ($e_{sb}$). An increase in $e_{sb}$ has a positive but small and statistically insignificant effect on IGM and estimates of $\beta$ and $\beta\times$ *parent pctile* are unchanged.

Second, I leverage the fact that the time period of analysis encompasses both "equalization" and "adequacy" reforms. The extent to which a decline in $\beta$ conflates equalization and level effects should be larger for adequacy reforms, which focused more on raising revenue levels, compared with equalization reforms. In fact, even among equalization reforms, it should be bigger for reforms that "leveled up" (i.e., raised) expenditure compared with those that "leveled down." Separately estimating the main specification on states that experienced an equalization reform and states that experienced an adequacy reform, using states with no reform are used as a control group, yields estimates that are comparable across the two sets of reforms (Appendix Table AVI).[28] Similarly, estimates of the effects of a decline in $\beta_s$ for reforms that "leveled down" are comparable to those for reforms that "leveled up" (Appendix Table AVII).[29] Taken together, these tests suggest that a decline in $\beta$ primarily captures the effects of equalization, rather than revenue levels.

### 5.2.1 Heterogeneous Effects of Equalization by School Grade

School finance equalization could differently impact IGM depending on whether it happens earlier or later during a child's education path. On one hand, education investments made at earlier ages tend to yield higher returns (Cunha and Heckman, 2010). On the other hand, equalization during high school could be beneficial if it facilitates the transition to college for low-income children, since college attendance is an important engine for IGM (Rothstein, 2019).

To test for this heterogeneity I allow the effect of $\beta$ to differ for cohorts which experienced

[28] For states that experienced both an equalization and an adequacy reform, I refer to the equalization reform. The results hold using adequacy reforms.

[29] "Level Down" ("Level Up") specifications are obtained using only states with reforms which leveled down (up) revenues, using states with no reforms as a control group; a reform is defined as leveling down if it generated a decline in revenues two years after the reform, relative to the year prior to the reform, in at least 20 percent of the state's districts. Again, states with no reforms are used as a control in all columns of Appendix Table AVII. The decline in spending is measured two years after a reform relative to the year before the reform. Results are robust to measuring the decline one and three years after the reform.

a reform during elementary, middle, and high school.[30] I augment equation (8) as follows:

$$\begin{aligned} M_{cbx} &= \delta_0^e \mathrm{El}_{s(c)b}\hat{\beta}_{s(c)b} + \delta^e \mathrm{El}_{s(c)b}\hat{\beta}_{s(c)b}\theta_{n(xc)} + \eta^m \mathrm{Md}_{s(c)b} + \delta_0^m \mathrm{Md}_{s(c)b}\hat{\beta}_{s(c)b} + \delta^m \mathrm{Md}_{s(c)b}\hat{\beta}_{s(c)b}\theta_{n(xc)} \\ &+ \eta^{hs}\mathrm{HS}_{s(c)b} + \delta_0^{hs}\mathrm{HS}_{s(c)b}\hat{\beta}_{s(c)b} + \delta^{hs}\mathrm{HS}_{s(c)b}\hat{\beta}_{s(c)b}\theta_{n(xc)} + \kappa_c + \tau_b + \theta_{n(xc)} + \omega_{cbx} \end{aligned} \tag{9}$$

where $\mathrm{El}_{s(c)b}$, $\mathrm{Md}_{s(c)b}$, and $\mathrm{HS}_{s(c)b}$ equal one if cohort $b$ in state $s$ experienced a reform during elementary school (grades 1-5), middle school (grades 6-8), or high school (grades 9-12) respectively. In this specification, the parameters $\delta_0^e$, $\delta_0^m$, and $\delta_0^{hs}$ represent the effect of a one-standard deviation increase in $\beta$ on the income percentile of children with the lowest-ranked parental income, for cohorts in states where a reform hit during elementary, middle, or high school and relative to cohorts and states without a reform. The parameters $\delta^e$, $\delta^m$, and $\delta^{hs}$ measure instead how much these effects vary as parents' income rank increases.

IV estimates of equation (9) indicate that a decline in $\beta$ is most effective when the reform hits during elementary school, relative to middle and high school. A one-standard deviation reduction in $\beta$ leads to an additional 6.6 percentile increase in the income rank of children with parents at the bottom of the income distribution for cohorts hit by a reform during elementary school, relative to those with no reform (with an estimate of $\beta\times$ *reform in elementary school* equal to -6.556, Table IV, column 3, significant at 10 percent). This effect declines by 0.095 percentiles with each additional percentile of parents' income (estimate of $\beta$ $\times$ *parent centile*$\times$ *reform in elementary school* equal to 0.095, Table IV, column 3, significant at 1 percent). These estimates imply that, when a reform hits during elementary school, a reduction in $\beta$ leads to an additional 5.6 and 4.2 percentiles in the income rank of children with parental income in the 10th and 25th percentiles, respectively, with no significant difference for children with parents in the 90th percentile.

By comparison, a one-standard deviation decline in $\beta$ leads to a smaller 1.8 and 1.5 percentile increase in the income rank of children with parental income in the 10th and 25th income percentile if the reform hits during high school (with an estimate of $\beta\times$ *reform in high school* equal to -1.989 and of $\beta$ $\times$ *parent centile*$\times$ *reform in high school* equal to 0.018, Table IV, column 3). The effect is indistinguishable from zero for children with parents on the 90th percentile. Estimates of the effects of a reform that hits during middle school fall in between the

[30]Note that $\hat{\beta}_{s(c)b}$ is already calculated as an average over the 12 school years. It follows that, if two cohorts experience the same reform in the same state (and if the reform is effective in lowering $\beta$), the older one will be exposed to a lower average $\beta$. Two cohorts in two different states, however, could be exposed to the same average $\beta$ but experience a reform at different points in the 12 years.

elementary and high school ones. OLS estimates, shown in columns 1 and 2 of Table IV, are smaller in magnitude but indicate similar patterns.

Consistently with the literature on early childhood investments, these estimates suggest that equalization in school resources is most effective when experienced earlier in a child's education career. The differences between OLS and IV estimates confirms the importance of accounting for the endogeneity in post-reform revenues.

#### 5.2.2 Equalization and Competition Across Districts

The theoretical framework in Section 2 predicts that the effects of equalization on IGM of low-income children should be larger in states with more competition across districts for scarce resources (i.e., a larger $\pi_s$). I test this prediction by re-estimating equation (8) separately for CZs above and below the national median level of cross-district competition, measured as the average number of districts per student in the state in 1980 (as in Hoxby, 2000).

IV estimates, shown in columns 1 and 2 of Table V, indicate that a decline in $\beta$ has a larger effect in states with more competition across school districts. Controlling for CZ fixed effects, a one-standard deviation decline in $\beta$ in "High competition" states leads to a 5.5, 5.0, and 2.6 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of $\beta$ equal to -5.876 and of $\beta \times$ *parent centile* equal to 0.036, Table V, column 2, significant at 1 percent). The effect is smaller in "Low competition" states: The same decline in $\beta$ leads to a 3.7, 3.3, and 1.4 percentile increase for children with parents in the 10th 25th, and 90th percentile, respectively (with an estimate of $\beta$ equal to -4.004 and of $\beta \times$ *parent centile* equal to 0.029, Table V, column 1, p-values equal to 0.29 and 0.02). These estimates confirm the prediction of Section 2 and suggest that district competition might explain part of the effects of equalization on IGM.[31]

#### 5.2.3 Equalization and Income Inequality

The estimates presented so far could mask important differences across CZs depending on the income distribution across school districts. To see this, consider two CZs in the same state, each containing only two districts. The first has one district with per capita income equal to \$25,000 and per pupil revenues equal to \$7,000 and one district with income equal to \$75,000 and revenues equal to \$9,000. The second has one district with income equal to \$15,000 and

[31]OLS estimates on the IV sample are shown in Appendix Table AVIII (column 1).

revenues equal to \$5,500 and one district with income equal to \$85,000 and revenues equal to \$8,300. Both CZs have a $\beta$ equal to 0.04.[32] Due to a more unequal income distribution, however, the revenue difference between the poorest and richest district in the second CZ is \$2,800 (or 34 percent), compared with only \$2,000 (29 percent) in the first CZ. The same reduction in $\beta$ could therefore have different implications in these two CZs.

To test for heterogeneity in the effects of equalization across CZs with different degrees of income inequality, I re-estimate equation (8) separately for CZs above and below the national median level of inequality, measured as the percentage difference in per capita income between the richest and the poorest district.[33] IV estimates, shown in columns 3 and 4 of Table V, indicate that a decline in $\beta$ has a smaller effect in CZs with income differences in the bottom 50 percent of the cross-CZ distribution ("Low inequality," column 3) relative to CZs in the top 50 percent ("High inequality," column 4). Controlling for CZ fixed effects, a one-standard deviation decline in $\beta$ in "Low inequality" CZs leads to a 3.7, 3.0, and 1.2 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of $\beta$ equal to -3.964 and of $\beta\times$ *parent centile* equal to 0.030, Table V, column 3). These effects are larger in "High inequality" CZs: The same decline in $\beta$ leads to a 4.5, 4.2, and 2.9 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of $\beta$ equal to -4.688 and of $\beta\times$ *parent centile* equal to 0.020, Table V, column 4, significant at 1 percent).

#### 5.2.4 Equalization and Income Segregation

The effects of a decline in $\beta$ could also vary depending on the degree of income segregation across districts within each CZ. When segregation is high, children from all low-income families are more likely to live and attend school in the same district and, in turn, more likely to benefit from the relative increase in school expenditure following a school finance reform.

To test this hypothesis, I re-estimate equation (8) separately for CZs above and below the national median level of income segregation, measured using the Theil index of districts' 1990 income within each CZ.[34] Estimates of $\delta_0$ and $\delta$ indicate that, controlling for CZ fixed effects, a one-standard deviation decline in $\beta$ in "Low segregation" CZs leads to a 4.0, 3.6, and 1.6

[32] $\beta = \frac{9{,}000-7{,}000}{75{,}000-25{,}000} = \frac{8{,}300-5{,}500}{85{,}000-15{,}000} = 0.04$.

[33] I calculate this difference using incomes from 1990.

[34] The Theil index is calculated as $T_c = \frac{1}{N}\sum_{i\in c}\frac{y_i}{\bar{y}}\ln\frac{y_i}{\bar{y}_c}$, where $i$ denotes a district, $c$ denotes a CZ, $y_i$ is a district's income, and $\bar{y}_c$ is median income in the CZ.

percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of $\beta$ equal to -4.291 and of $\beta\times$ *parent centile* equal to 0.030, Table V, column 5, significant at 5 and 1 percent). Equalization is more effective in CZs with high income segregation: The same decline in $\beta$ leads to a 4.8, 4.5, and 3.0 percentile increase for children with parents in the 10th, 25th, and 90th percentiles, respectively (with an estimate of $\beta$ equal to -5.042 and of $\beta\times$ *parent centile* equal to 0.022, Table V, column 6, significant at 1 percent).

Taken together, these results suggest that the effectiveness of an equalization reform depends on the geographic distribution of income. This heterogeneity could have important implications for the design of school finance plans.

### 5.3 Robustness

**Estimating $\beta$ With Interpolated Income** The above estimates are obtained using income figures from the 1990 Census to estimate $\beta$ for all years. To check that my results are not driven by this choice or by the impossibility to observe income for all years, in Table VI (column 1) I re-estimate the main IV specifications with a version of $\beta$ obtained interpolating income figures between Census years. These estimates are similar to those in Table II, indicating that the main results are not driven by this choice.[35]

**CZs Without a State Border** Out of 327 CZs included in the analysis, 53 are crossed by one or more state borders (for example, the CZ of New York City, NY also includes Newark, NJ).[36] The same decline in $\beta$ might have different effects in one-state and multi-state CZs. On one hand, if sorting across state borders is more costly than within states, the endogeneity problem might be more pressing in one-state CZs. On the other hand, a decline in $\beta$ in a multi-state CZ might be driven by a change in revenues and expenditures only in some (but not all) districts, but a much larger one in absolute terms. Columns 2 and 3 of Table VI show IV estimates of the main specifications separately for one-state and multi-state CZs. Estimates are slightly smaller for multi-state CZs but still significant, indicating that the results are not driven by the presence or absence of borders.[37]

**Alternative Definitions of Parental Income Quantiles** Columns 4 and 5 of Tables VI show estimates of the main specification obtained using a dataset in which each observation corre-

[35]OLS estimates on the IV sample are shown in Appendix Table AIX (column 1).
[36]In the baseline estimates I follow Chetty et al. (2014) and assign each of these CZs the state with the largest population share.
[37]OLS estimates on the IV sample are shown in Appendix Table AIX (columns 2 and 3).

sponds to a CZ, birth cohort, and parental quintile in the national and CZ-specific distributions, respectively. The magnitude of these estimates is very similar to those in Table II, which suggests that the main results are not driven by this normalization choice.[38]

## 6 Channels: School Inputs and Intermediate Outcomes

The results described so far show that equalizing school funding across richer and poorer districts increases IGM for children from low-income families. This section investigates some potential channels behind these effects, focusing on the role of school inputs and on the effects of equalization on intermediate educational outcomes.

### 6.1 Inputs: Spending Categories

To understand how equalization in school revenues translated into equalization of spending on different types of educational inputs, I use data from the NCES fiscal school district universe survey (F33), which reports district-level expenditures in various spending categories for the years 1990, 1992, and 1995 to 2004. I re-calculate $\beta$ using per pupil spending as the dependent variable in equation (7), separately for each of four categories: current expenditures, capital expenditures, instructional salaries, and other non-instructional current expenditures. Event studies of $\beta$, shown in Appendix Figure AXIII, indicate that school finance equalization reforms were followed by an equalization of both current and capital expenditures, with a decline in the corresponding $\beta$ (although the latter is imprecisely estimated). Among current expenditures, reforms are most effective in equalizing spending on salaries (including instructional salaries) compared with other current expenditures.

### 6.2 Inputs: Teacher-Student Ratio

Equalization in instructional spending following each reform suggests that these reforms might be effective to "level the playing field," i.e., to reduce the gap in educational inputs between more and less disadvantaged children. For a more direct test of this hypothesis, I study the effects of equalization on the gap in inputs between low-income and high-income districts. I focus on the teacher-student ratio: Teachers are among the most important factors for student learning (Chetty et al., 2014), and an adequate number of teachers per student is fundamental

[38] OLS estimates on the IV sample are shown in Table Appendix AIX (columns 4 and 5).

for the growth in achievement (Krueger and Whitmore, 2001; Bloom and Unterman, 2013). Yet underfunded districts are often forced to cut instructional staff to face budget shortages.[39]

I investigate the effects of a reduction in $\beta$ on the teacher-student ratio, measured at the district-year level, allowing this effect to vary across low-income and high-income districts. I estimate the following equation:

$$TS_{dt} = \delta_1 \hat{\beta}_{s(d)t} q_d^1 + \delta_2 \hat{\beta}_{s(d)t} q_d^2 + \delta_3 \hat{\beta}_{s(d)t} q_d^3 + \delta_4 \hat{\beta}_{s(d)t} q_d^4 + \gamma_d + \tau_t + \varepsilon_{dt} \tag{10}$$

where $TS_{dt}$ is the teacher-student ratio of district $d$ in year $t$; the variable $q_d^{nq}$ equals 1 for districts in the n-th quartile of the state income distribution in 1990, and the vectors $\gamma_d$ and $\tau_t$ control for district and year fixed effects. The parameters $\delta_1$, $\delta_2$, $\delta_3$, and $\delta_4$ capture the effects of equalization on the teacher-student ratio in districts in the first, second, third and fourth quartile of the income distribution.

Table VII shows OLS and IV estimates of equation (10). IV estimates, shown in column 3, yield positive effects on low-income districts, but no effect on high-income ones. Controlling for district fixed effects, a one-standard deviation reduction in $\beta$ leads to 0.01 additional teachers per student in districts in the bottom quartile, or 14 percent more (Table VII, column 3, significant at 1 percent). The same estimate is -0.006 for districts in the top quartile and it is indistinguishable from zero.

The unavailability of data on the composition of the teaching body prevents me from studying whether changes in teacher-student ratios also changed the composition of the teaching body across districts. In fact it is possible that, in lower-income districts, the teaching workforce grew in size with the addition of less experienced teachers (who are often less effective, Rockoff, 2004).[40] Yet, the estimates presented above are suggestive of a mediating role for the equalization in teacher-student ratios across districts in the relationship between spending equalization and intergenerational mobility.

### 6.3 Intermediate Outcomes: High School Completion Rates

High school dropout is still a pressing issue for US education. In 2012 6.6 percent of enrolled students did not finish high school; this rate is even higher for black (7.5) and Hispanic stu-

[39] From an analysis of the Center on Budget and Policy Priorities using data from the Bureau of Labor Statistics.
[40] Goldhaber et al. (2015) show that less effective teachers are disproportionately more likely to teach in districts serving larger shares of economically disadvantaged students.

dents (12.7).[41] Large differences exist across districts: In 2010, less than 2 percent of students dropped out of high school in the Palo Alto Unified School District (Palo Alto, CA), but almost 15 percent dropped out in the City of Chicago Public Schools (Chicago, IL).

A gap in rates of high school completion between students in low-income and high-income districts can translate into a persistent gap in adult-life economic opportunities, which reduces mobility. To assess the extent to which the positive effects of equalization on intergenerational mobility act through a reduction in cross-district differences in completion rates, I estimate the following equation:

$$d_{dc} = \delta_1 \beta_{s(d)c} q_d^1 + \delta_2 \beta_{s(d)c} q_d^2 + \delta_3 \beta_{s(d)c} q_d^3 + \delta_4 \beta_{s(d)c} q_d^4 + \gamma_d + \tau_c + \varepsilon_{dc} \tag{11}$$

where $d_{dc}$ is the average dropout rate for cohort $c$ in district $d$, and the remaining variables are as above. Estimates of $\delta_1$ to $\delta_4$ capture the effect of a decline in $\beta$ on the dropout rate in districts in the 1st to 4th income quartile.

OLS and IV estimates from this specification are imprecise. Nevertheless, IV point estimates suggest that a one-standard deviation reduction in $\beta$ leads to a 1.4 percentage points decline in dropout rates in school districts in the bottom quartile of the income distribution within each state in 1990 (a 31 percent decline relative to an average rate of 4.5 percent, Table VIII, column 3, p-value equal to 0.38). In contrast, the same decline in $\beta$ leads to a 0.4 percentage points increase in dropout rates in districts in the fourth income quartile (8 percent, p-value equal to 0.77). These results suggest that equalization in school expenditure might close the gap in completion rates between low- and high-income districts and that high dropout rates in low-income districts might represent an important obstacle to upward mobility.

## 7 Discussion and Conclusion

Using variation in states' funding schemes introduced by school finance reforms and exploiting differences in exposure to equalized schemes across cohorts in different states, this paper shows that equalization in school revenues across districts increases IGM of children from low-income families, with insignificant effects on wealthier children. These effects work through a reduction in the gap in educational inputs (such as the number of teachers) between wealthier and poorer districts.

[41] 2013 Digest of Education Statistics, National Center for Education Statistics.

My results also indicate how, while being a useful source of variation in funding, school finance reforms should be used by researchers with caution. Funding formulas link school revenues to property taxes, whose tax base could be endogenous to IGM if households respond to a reform by "voting with their feet." To account for this source of endogeneity and for the differences in funding formulas across states, I use a simulated-instruments approach which directly exploits the change in the formula type and parameters following each reform. The approach requires detailed information on each pre-reform and post-reform formula type and parameters, which I hand-collected and combined with district-level data on the variables entering each formula. Estimates obtained using this approach are larger in magnitude than OLS. This shows that not properly accounting for the endogeneity of post-reform expenditure could lead to misleading interpretations of the effects of equalization.

Taken together, my findings indicate that equalizing school expenditure has a *causal* positive effect on future outcomes of disadvantaged children. This in turn implies that this type of policy represents an important engine of mobility for low-income children. My approach and the accompanying dataset can be used in other studies as well.

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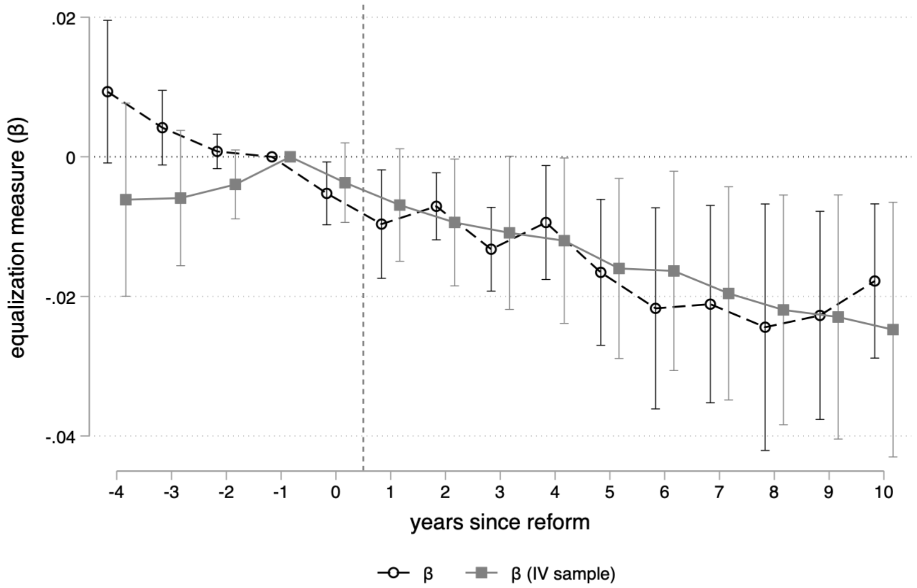
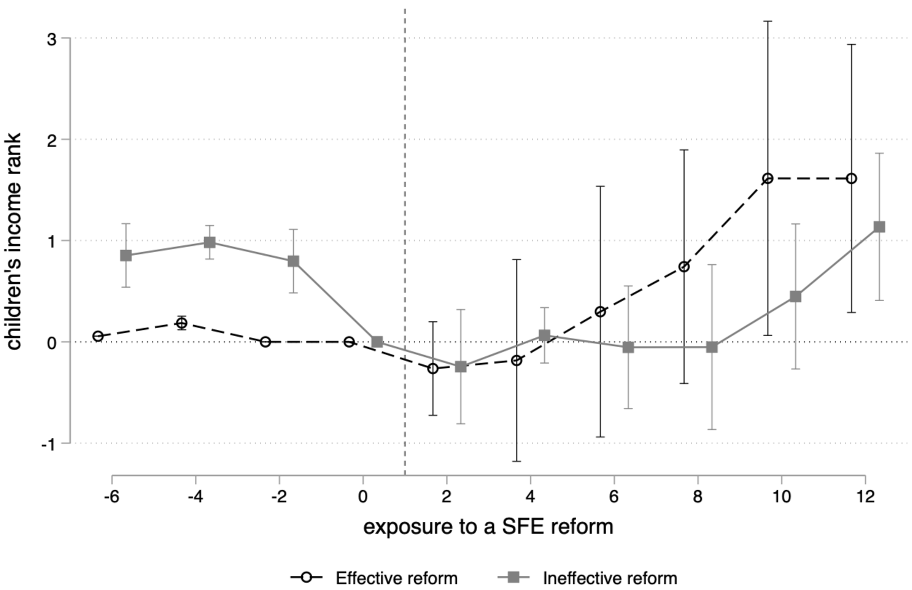
Figure I: Event Study of Equalization Measure $\beta$ Around A School Finance Reform




*Note*:OLS estimates and 90 percent confidence intervals for the coefficients $\delta_k$ in regression $\beta_{st} = \sum_k \delta_k R_s 1(t - ryear_s = k) + \theta_s + \varepsilon_{st}$, where $\beta_{st}$ is the slope coefficient in equation (7), estimated separately for each state $s$ and year $t$ from 1986 to 2004, $R_s$ equals 1 if state $s$ had a school finance reform in the years 1980-2004, $ryear_s$ is the year of the first reform in this time period, and $\theta_s$ are state fixed effects. The coefficient $\delta_{-1}$ is normalized to equal zero. Standard errors are clustered at the state level. The IV sample is restricted to CA, CO, FL, GA, IL, KY, LA, MA, MI, MN, MT, NE, NJ, NY, ND, OH, PA, UT, TX, and WI. The full sample excludes HI.

Figure II: Changes in Intergenerational Income Mobility by Exposure to a School Finance Reform, in States with Effective vs. Ineffective Reforms




*Note*: The figure shows OLS points estimates and 90 percent confidence intervals of the coefficients $\delta_n$ in the equation $m_{cb} = \sum_n \delta_n E_{n(sb)} + \theta_c + \varepsilon_{cv}$, where $m_{cb}$ is the mean rank of children in CZ $c$, cohort $b$, and with parents' income on the 25th percentile in the national income distribution, $E_n$ equals one if cohort $b$ in state $s$ was exposed to a post-school finance reform regime for $n$ years (and $E_n = b + 17 - ryear_s$, where $ryear_s$ is the year of the first school finance reform in state $s$ between 1980 and 2004), and the vector $\theta_c$ contains CZ fixed effects. Estimates are obtained and shown separately for states with effective reforms (i.e. those which resulted in a negative post-reform $\beta$ or a decline in $\beta$ of at least 20 percent, including AK, AZ, AR, CO, IA, KT, LA, MN, MA, MI, MS, MO, NE, OR, RI, TX, WA, WI, and WY, solid line) and ineffective reforms (including AL, CT, ID, IN, KS, MD, MI, MO, NH, NJ, SC, SD, TN, and VT, dashed line), using states with no reform (including CA, DE, FL, GA, IL, NE, NM, NY, NC, ND, OH, OK, PA, UT, VA, and WV) as a control group. Observations are at the CZ $\times$ birth cohort level. The coefficient $\delta_0$ is normalized to equal zero for both groups. Standard errors are clustered at the state and cohort level.

Table I: Summary Statistics: School District Revenues, Intergenerational Mobility, and Measures of Equalization ($\beta$)

Panel A: Per Pupil Revenues and Income

| | mean | sd | median | min | max | N |
|---|---|---|---|---|---|---|
| *Median income* | | | | | | |
| 1980 | 36417 | 11041 | 33961 | 18286 | 67924 | 7578 |
| 1990 | 46552 | 17916 | 41249 | 18149 | 115499 | 7621 |
| 2000 | 44018 | 15891 | 37500 | 17500 | 87500 | 7936 |
| 2010 | 42974 | 16444 | 46250 | 14800 | 92500 | 7942 |
| $\Delta$ *revenues, richest vs poorest district within state ($)* | | | | | | |
| 1986 | 2143 | 6173 | -581 | -2179 | 14162 | 2788 |
| 1990 | 1950 | 4466 | 539 | -2306 | 12965 | 4895 |
| 2000 | 1137 | 5037 | -411 | -7195 | 15415 | 7146 |
| 2004 | 2743 | 6714 | 342 | -8090 | 18120 | 6653 |
| $\Delta$ *revenues, richest vs poorest district within CZ ($)* | | | | | | |
| 1986 | 1273 | 4210 | 506 | -13816 | 13782 | 2788 |
| 1990 | 1207 | 3570 | 451 | -10502 | 14518 | 4895 |
| 2000 | 139 | 5135 | 73 | -14991 | 17197 | 7146 |
| 2004 | -61 | 5582 | -480 | -21618 | 18332 | 6653 |

Panel B: Intergenerational Income Mobility Measures
Expected Income Percentile of Children by Percentile of the Parents

| | 10th | 25th | 75th | 90th |
|---|---|---|---|---|
| 1980-82 | 0.392 | 0.433 | 0.569 | 0.609 |
| | (0.040) | (0.033) | (0.024) | (0.027) |
| 1983-86 | 0.396 | 0.435 | 0.567 | 0.607 |
| | (0.035) | (0.030) | (0.031) | (0.035) |
| N (CZs) | 634 | 634 | 634 | 634 |

Panel C: Measures of School Finance Equalization ($\beta$)

| | All | No reform | Pre-Reform | Post-Reform | Difference |
|---|---|---|---|---|---|
| $\beta$ | -0.006 | -0.002 | -0.000 | -0.012 | -0.011* |
| | (0.064) | (0.081) | (0.053) | (0.056) | (0.005) |
| $\beta$ (IV sample) | 0.011 | 0.028 | 0.044 | -0.009 | -0.053*** |
| | (0.068) | (0.091) | (0.038) | (0.048) | (0.008) |
| $\beta^s$ | 0.017 | 0.034 | 0.037 | -0.001 | -0.038*** |
| | (0.065) | (0.087) | (0.029) | (0.049) | (0.008) |

*Note*: Income and per-pupil revenues (measured in 2000 dollars), and difference in per-pupil revenues between the highest and the lowest-income district within each state and CZ (panel A); CZ-cohort level intergenerational mobility measures for cohorts 1980 to 1986 (from the Opportunity Insights Project, panel B); slope coefficient in equation (7), estimated separately for each state and year using actual revenues ($\beta$) and simulated revenues ($\beta^s$, panel C).

Table II: School Finance Equalization and Intergenerational Mobility. OLS and IV, Dependent Variable is Children's Income Percentile

| | OLS | OLS, IV sample | IV, 1st stage | | IV, 2nd stage | OLS |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Dep. var. | IGM | IGM | $\beta$ | $\beta\times$ par. pctile | IGM | IGM |
| $\beta$ | -2.387*** | -2.918*** | | | -4.681*** | |
| | (0.646) | (0.905) | | | (1.403) | |
| $\beta\times$ par. pctile | 0.024*** | 0.024*** | | | 0.025*** | |
| | (0.002) | (0.002) | | | (0.002) | |
| $\beta^s$ | | | 0.853*** | -3.630 | | -4.084*** |
| | | | (0.100) | (6.526) | | (1.233) |
| $\beta^s\times$ par. pctile | | | 0.000 | 0.925*** | | 0.023*** |
| | | | (0.000) | (0.012) | | (0.002) |
| Parent pctile FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| CZ FE | Yes | Yes | Yes | Yes | Yes | Yes |
| KP Wald F-stat | | | 18.10 | | | |
| N | 26688 | 13578 | 13578 | 13578 | 13578 | 13578 |
| 10th | 2.145 | 2.681 | | | 4.436 | 3.857 |
| 10th [p-value] | 0.001 | 0.003 | | | 0.002 | 0.002 |
| 25th | 1.782 | 2.325 | | | 4.068 | 3.517 |
| 25th [p-value] | 0.006 | 0.010 | | | 0.003 | 0.004 |
| 90th | 0.210 | 0.783 | | | 2.473 | 2.044 |
| 90th [p-value] | 0.746 | 0.390 | | | 0.072 | 0.093 |

*Note*: The table shows OLS and IV estimates of the parameters $\delta_0$ and $\delta$ in equation (8). Column 1 shows OLS estimates on the full sample of US states. Column 2 shows OLS estimated on the IV sample. Columns 3 and 4 show the IV first stage, column 5 shows the second stage, and column 6 shows the reduced form. In columns 1, 2, 5, and 6 the dependent variable is children's income percentile in the national distribution for each parental income quantile in the state distribution, for cohorts 1980 to 1986. The variable $\beta$ is the OLS estimate of the slope coefficient in equation (7), computed separately for each state and cohort and standardized across all states and cohorts. The variable *par. pctile* is the percentile of parents in the national income distribution. IV estimates are obtained using $\beta^s$ and $\beta^s\times$ *par. pctile* as instruments for $\beta$ and $\beta\times$ *par. pctile*; the variable $\beta^s$ is estimated as $\beta$ using simulated revenues instead of actual revenues. All specifications include parent percentile, CZ, and cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort level. *KP Wald F-stat* refers to the Kleibergen-Paap Wald F-statistic as a test of weak instruments. *Xth* refers to the effects on children with parents in the Xth centile of the national distribution. The IV sample is restricted to CA, CO, FL, GA, IL, KY, LA, MA, MI, MN, MT, NE, NJ, NY, ND, OH, PA, UT, TX, and WI. The full sample excludes HI. *** p<0.01, ** p<0.05, * p<0.1.

Table III: School Finance Equalization and Intergenerational Mobility. OLS and IV, Dependent Variable is Children's log(Income)

| | OLS | OLS, IV sample | IV |
|---|---|---|---|
| | (1) | (2) | (3) |
| $\beta$ | -0.061*** | -0.076*** | -0.125*** |
| | (0.018) | (0.024) | (0.035) |
| $\beta\times$ par. pctile | 0.001*** | 0.001*** | 0.001*** |
| | (0.000) | (0.000) | (0.000) |
| Parent pctile FE | Yes | Yes | Yes |
| Cohort FE | Yes | Yes | Yes |
| CZ FE | Yes | Yes | Yes |
| N | 26688 | 13578 | 13578 |
| 10th | 0.054 | 0.069 | 0.118 |
| 10th [p-value] | 0.002 | 0.004 | 0.001 |
| 25th | 0.044 | 0.059 | 0.107 |
| 25th [p-value] | 0.011 | 0.012 | 0.002 |
| 90th | 0.001 | 0.016 | 0.062 |
| 90th [p-value] | 0.971 | 0.490 | 0.066 |

*Note*: OLS and IV estimates of the parameters $\delta_0$ and $\delta$ in equation (8). Column 1 shows OLS estimates on the full sample of US states, column 2 shows OLS estimated on the IV sample, and column 3 shows IV. The dependent variable is the natural logarithm of children's income for each parental income quantile in the state distribution, for cohorts 1980 to 1986. The variable $\beta$ is the OLS estimate of the slope coefficient in equation (7), computed separately for each state and cohort and standardized across all states and cohorts. The variable *par. pctile* is the percentile of parents in the national income distribution. IV estimates are obtained using $\beta^s$ and $\beta^s\times$ *par. pctile* as instruments for $\beta$ and $\beta\times$ *par. pctile*; the variable $\beta^s$ is estimated as $\beta$ using simulated revenues instead of actual revenues. All specifications include parent percentile, CZ, and cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort level. *Xth* refers to the effects on children with parents in the Xth centile of the national distribution. The IV sample is restricted to CA, CO, FL, GA, IL, KY, LA, MA, MI, MN, MT, NE, NJ, NY, ND, OH, PA, UT, TX, and WI. The full sample excludes HI. *** p<0.01, ** p<0.05, * p<0.1.

Table IV: Heterogeneous Effects of School Finance Equalization Across School Grades. OLS and IV, Dependent Variable is Children's Income Percentile

| | OLS | OLS, IV sample | IV |
|---|---|---|---|
| | (1) | (2) | (3) |
| $\beta\times$ reform in elementary school | -2.051** | -3.101* | -6.556 |
| | (0.808) | (1.635) | (4.208) |
| $\beta\times$ par. pctile $\times$ reform in elem school | 0.047*** | 0.077*** | 0.095*** |
| | (0.008) | (0.015) | (0.017) |
| $\beta\times$ reform in middle school | -1.467** | -0.518 | -2.666 |
| | (0.709) | (1.419) | (3.505) |
| $\beta\times$ par. pctile $\times$ reform in middle school | 0.033*** | 0.031*** | 0.031** |
| | (0.007) | (0.009) | (0.009) |
| $\beta\times$ reform in high school | -1.056* | 0.276 | -1.989 |
| | (0.564) | (1.465) | (3.739) |
| $\beta\times$ par. pctile $\times$ reform in high school | 0.028*** | 0.017** | 0.018 |
| | (0.003) | (0.007) | (0.013) |
| Parent pctile FE | Yes | Yes | Yes |
| Cohort FE | Yes | Yes | Yes |
| CZ FE | Yes | Yes | Yes |
| Reform in elem, middle, high | Yes | Yes | Yes |
| N | 26688 | 13578 | 13578 |
| 10th, elem | 1.577 | 2.335 | 5.603 |
| 25th, elem | 0.865 | 1.185 | 4.173 |
| 90th, elem | -2.217 | -3.796 | -2.021 |
| 10th, high | 0.776 | -0.444 | 1.809 |
| 25th, high | 0.356 | -0.696 | 1.539 |
| 90th, high | -1.465 | -1.788 | 0.371 |

*Note*: OLS and IV estimates of the parameters in equation (9). Column 1 shows OLS estimates on the full sample of US states, column 2 shows OLS estimated on the IV sample, and column 3 shows IV. The dependent variable is children's income percentile in the national distribution for each parental income quantile in the state distribution, for cohorts 1980 to 1986. The variable $\beta$ is the OLS estimate of the slope coefficient in equation (7), computed separately for each state and cohort and standardized across all states and cohorts. The variable *par. pctile* is the percentile of parents in the national income distribution. The variables *reform in elem school*, *reform in middle school*, and *reform in high school* equal one for cohorts and states for which a reform hit during elementary, middle, and high school, respectively. IV estimates are obtained using $\beta^s$and $\beta^s\times$ *par. pctile* as instruments for $\beta$ and $\beta\times$ *par. pctile*; the variable $\beta^s$ is estimated as $\beta$ using simulated revenues instead of actual revenues. All specifications include parent percentile, CZ, and cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort level. *KP Wald F-stat* refers to the Kleibergen-Paap Wald F-statistic as a test of weak instruments. *Xth* refers to the effects on children with parents in the Xth centile of the national distribution. The IV sample is restricted to CA, CO, FL, GA, IL, KY, LA, MA, MI, MN, MT, NE, NJ, NY, ND, OH, PA, UT, TX, and WI. The full sample excludes HI. *** p<0.01, ** p<0.05, * p<0.1.

Table V: Heterogeneous Effects of School Finance Equalization. IV, Dependent Variable is Children's Income Percentile

| | By competition | | By inequality | | By segregation | |
|---|---|---|---|---|---|---|
| | (1) Low | (2) High | (3) Low | (4) High | (5) Low | (6) High |
| $\beta$ | -4.004 | -5.876*** | -3.964 | -4.688*** | -4.291** | -5.042*** |
| | (2.471) | (1.932) | (2.572) | (1.451) | (1.761) | (1.610) |
| $\beta\times$ parent pctile | 0.029** | 0.036*** | 0.030*** | 0.020*** | 0.030*** | 0.022*** |
| | (0.011) | (0.004) | (0.006) | (0.002) | (0.004) | (0.002) |
| Parent pctile FE | Yes | Yes | Yes | Yes | Yes | Yes |
| CZ FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Cohort FE | Yes | Yes | Yes | Yes | Yes | Yes |
| N | 8784 | 4794 | 5460 | 8076 | 5922 | 7656 |
| 10th | 3.716 | 5.513 | 3.661 | 4.486 | 3.995 | 4.818 |
| 10th [p-value] | 0.126 | 0.004 | 0.152 | 0.002 | 0.022 | 0.003 |
| 25th | 3.285 | 4.968 | 3.207 | 4.182 | 3.550 | 4.483 |
| 25th [p-value] | 0.167 | 0.011 | 0.206 | 0.004 | 0.040 | 0.006 |
| 90th | 1.415 | 2.608 | 1.238 | 2.868 | 1.624 | 3.032 |
| 90th [p-value] | 0.536 | 0.192 | 0.619 | 0.051 | 0.335 | 0.065 |

*Note*: IV estimates of the parameters $\delta_0$ and $\delta$ in equation (8). The dependent variable is children's income percentile in the national distribution for each parental income quantile in the state distribution, for cohorts 1980 to 1986. The variable $\beta$ is the OLS estimate of the slope coefficient in equation (7), computed separately for each state and cohort and standardized across all states and cohorts. The variable *par. pctile* is the percentile of parents in the national income distribution. IV estimates are obtained using $\beta^s$ and $\beta^s\times$ *par. pctile* as instruments for $\beta$ and $\beta\times$ *par. pctile*; the variable $\beta^s$ is estimated as $\beta$ using simulated revenues instead of actual revenues. All specifications include parent percentile, CZ, and and cohort fixed effects. "Low Competition" ("High Competition") refers to states below (above) the median level of cross-district competition, measured as the number of districts per student in the state in 1980. "Low Inequality" ("High Inequality") refers to CZs below (above) the median level of income inequality, measured as the percentage difference in average income between the richest and poorest district in each CZ in 1990. "Low Segregation" ("High Segregation") refers to CZs below (above) the median level of income segregation across all CZs, where income segregation is measured with a Theil index calculated across districts within each CZ using data from 1990. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort level. *Xth* refers to the effects on children with parents in the Xth centile of the national distribution. The sample is restricted to CA, CO, FL, GA, IL, KY, LA, MA, MI, MN, MT, NE, NJ, NY, ND, OH, PA, UT, TX, and WI.*** p<0.01, ** p<0.05, * p<0.1.

Table VI: Robustness Checks. IV, Dependent Variable is Children's Income Percentile

| | Interpolated income | By border | | Alt. centile def. | |
|---|---|---|---|---|---|
| | (1) | (2) Without | (3) With | (4) National | (5) Cz |
| $\beta$ | -5.713** | -5.239*** | -3.412*** | -4.307*** | -4.418*** |
| | (2.471) | (1.633) | (1.149) | (1.438) | (1.323) |
| $\beta\times$ parent pctile | 0.023*** | 0.026*** | 0.021*** | 0.022*** | 0.026*** |
| | (0.002) | (0.002) | (0.003) | (0.002) | (0.003) |
| Parent pctile FE | Yes | Yes | Yes | Yes | Yes |
| CZ FE | Yes | Yes | Yes | Yes | Yes |
| Cohort FE | Yes | Yes | Yes | Yes | Yes |
| N | 13578 | 11364 | 2214 | 13578 | 14458 |
| 10th | 5.485 | 4.978 | 3.203 | 4.085 | 4.162 |
| 10th [p-value] | 0.026 | 0.002 | 0.005 | 0.004 | 0.002 |
| 25th | 5.142 | 4.588 | 2.889 | 3.751 | 3.779 |
| 25th [p-value] | 0.037 | 0.005 | 0.011 | 0.009 | 0.004 |
| 90th | 3.656 | 2.894 | 1.530 | 2.305 | 2.118 |
| 90th [p-value] | 0.133 | 0.074 | 0.182 | 0.105 | 0.088 |

*Note*: The dependent variable is children's income percentile in the national distribution for each parental income quantile in the state distribution (columns 1-3), national distribution (column 4), or CZ distribution (column 5), for cohorts 1980 to 1986. The variable $\beta$ is the OLS estimate of the slope coefficient in equation (7), computed separately for each state and cohort and standardized across all states and cohorts. The variable *par. pctile* is the percentile of parents in the national income distribution. The variable $\beta$ is instrumented with $\beta^s$, estimated as $\beta$ using simulated revenues instead of actual revenues. All specifications include parent percentile, CZ, and and cohort fixed effects. In column 1, $\beta$ is calculated using income figures that are interpolated between Census years. In column 2 the sample is restricted to CZs entirely belonging to one state, and in column 3 the same includes only CZs belonging to two or more states. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort level. *Xth* refers to the effects on children with parents in the Xth centile of the national distribution. The sample is restricted to CA, CO, FL, GA, IL, KY, LA, MA, MI, MN, MT, NE, NJ, NY, ND, OH, PA, UT, TX, and WI. *** p<0.01, ** p<0.05, * p<0.1.

Table VII: School Finance Equalization and School Inputs. OLS and IV, Dependent Variable is the Number of Teachers per Student

| | OLS | OLS, IV sample | IV |
|---|---|---|---|
| | (1) | (2) | (3) |
| $\beta\times$ income in the $1^{st}$ quartile | -0.004*** | -0.005*** | -0.010*** |
| | (0.001) | (0.001) | (0.003) |
| $\beta\times$ income in the $2^{nd}$ quartile | -0.002 | -0.002** | -0.008** |
| | (0.002) | (0.001) | (0.003) |
| $\beta\times$ income in the $3^{rd}$ quartile | -0.000 | -0.001 | -0.007** |
| | (0.001) | (0.001) | (0.003) |
| $\beta\times$ income in the $4^{th}$ quartile | 0.001 | -0.000 | -0.006* |
| | (0.001) | (0.001) | (0.003) |
| Year FE | Yes | Yes | Yes |
| State FE | Yes | Yes | Yes |
| Quartile FE | Yes | Yes | Yes |
| N | 232750 | 110833 | 110833 |
| Y-mean | 0.070 | 0.072 | 0.072 |

*Note*: OLS and IV estimates of equation (10). Column 1 shows OLS estimates on the full sample of US states, column 2 shows OLS estimated on the IV sample, and column 3 shows IV. The dependent variable is the total number of teachers employed in a district, divided by the total number of students; observations are at the district-year level and cover years 1988-2004. The variable $\beta$ is defined as the OLS estimate of the slope coefficient in equation (7), computed separately for each state and year and standardized across all states and years. The variable *income in the* $X^{th}$ *quartile* equals 1 for districts with median household income in the $X^{th}$ quartile of the national distribution in 1990. Columns 1 and 2 estimate OLS; column 3 estimates IV, with $\beta^s$ (obtained using simulated revenues instead of actual revenues) as an instrument for $\beta$. All specifications include state and year fixed effects. Standard errors in parentheses are clustered at the state and year level. The IV sample is restricted to CA, CO, FL, GA, IL, KY, LA, MA, MI, MN, MT, NE, NJ, NY, ND, OH, PA, UT, TX, and WI. The full sample excludes HI. *** p<0.01, ** p<0.05, * p<0.1.

Table VIII: School Finance Equalization and Dropout Rates. OLS and 2SLS, Dependent Variable Are Dropout Rates

| | OLS | OLS, IV sample | IV |
|---|---|---|---|
| | (1) | (2) | (3) |
| $\beta\times$ income in the $1^{st}$ quartile | 0.003 | 0.005 | 0.011 |
| | (0.006) | (0.006) | (0.012) |
| $\beta\times$ income in the $2^{nd}$ quartile | 0.002 | 0.005 | 0.004 |
| | (0.005) | (0.006) | (0.013) |
| $\beta\times$ income in the $3^{rd}$ quartile | -0.004 | -0.003 | -0.007 |
| | (0.005) | (0.006) | (0.012) |
| $\beta\times$ income in the $4^{th}$ quartile | -0.001 | -0.001 | -0.003 |
| | (0.005) | (0.006) | (0.009) |
| Year FE | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes |
| N | 49678 | 28745 | 28745 |
| Y-mean | 0.047 | 0.045 | 0.045 |

*Note*: The dependent variable is the average dropout rate for each school district and cohort, i.e., the average of grade-specific ratios between the number of students who drop out at the start of the grade and the number of students enrolled in the previous grade in the previous year; observations are at the district-cohort level. The variable $\beta$ is defined as the OLS estimate of the slope coefficient in equation (7), computed separately for each state and year and standardized across all states and years. The variable *income in the* $X^{th}$ *quartile* equals 1 for districts with median household income in the $X^{th}$ quartile of the national distribution in 1990. Columns 1 and 2 estimate OLS; column estimates IV, with $\beta^s$ (obtained using simulated revenues instead of actual revenues) as an instrument for $\beta$. All specifications include district and year fixed effects. The IV sample is restricted to CA, CO, FL, GA, IL, KY, LA, MA, MI, MN, MT, NE, NJ, NY, ND, OH, PA, UT, TX, and WI. The full sample excludes HI. *** p<0.01, ** p<0.05, * p<0.1.