

# School Finance Equalization Increases Intergenerational Mobility: Evidence from a Simulated-Instruments Approach\*

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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. These reforms create incentives for households to sort across districts; this sorting affects property values and causes endogeneity of post-reform revenues. I address this issue with a simulated-instruments approach, which 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 13 reforms passed in 20 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 sharp declines 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 the rank of their parents.<sup>1</sup> 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).<sup>2</sup> Justified by the theoretical model and by 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 Wisconsin, which had a reform in 1996, the 1980 cohort (in school between 1986 and 1998) experienced a  $\beta$  equal to 0.016 (with the richest district spending \$15,000 and the poorest \$10,000 in 1990), while the 1990 cohort (in school between 1996 and 2008) experienced a  $\beta$  equal to 0.008 (with the richest district spending \$13,000 and the poorest \$14,000 in 2000). In Ohio, which had no reform between 1986 and 2004, these two cohorts experienced a  $\beta$  equal to 0.044 and 0.047, respectively.<sup>3</sup> 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.<sup>4</sup>

To estimate the causal effects of a decline in  $\beta$  on IGM, however, one must deal with an

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<sup>1</sup>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).

<sup>2</sup>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 6.1.2.

<sup>3</sup>Ohio did have a school finance reform in 1997, but the reform simply increased the foundation amount without affecting the degree of redistribution across districts.

<sup>4</sup>This assumption has also been extensively discussed and argued for by Hoxby (2001); Lafortune et al. (2018); Jackson et al. (2015).

additional endogeneity problem which the existing literature has only partially addressed. The changes in the funding formula that lead to changes in  $\beta$  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), i.e., to move across districts based on their preferences for this public good and their income.<sup>5</sup> On one side, household sorting affects house prices and districts’ revenues (via the funding formula) and the degree of equalization in school funding. On the other side, this sorting changes the composition of each school district and CZ, which could have a direct effect on IGM (for example through peer effects). Post-reform revenues – and the resulting  $\beta$  – are therefore endogenous; ignoring this endogeneity would confound the effects of equalization with the effects of sorting.

I address this endogeneity issue with a simulated-instruments approach (similar to Gruber and Saez, 2002), which exploits plausibly random, state-specific changes in the funding formulas. I combine a variety of administrative and legislative sources to construct an original data set containing information on the formulas in place in each state and year and on all the district-level variables entering each formula. This information is available for 20 states between 1986 and 2004, enrolling 62 percent of all US students. These data allow me to 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  $\beta$ .

This approach is useful for two reasons. First, it allows me to separate the variation in the distribution of school revenues driven by exogenous changes in the funding formula from the variation driven by endogenous household sorting, which is necessary to estimate causal effects. Second, it explicitly takes into account the fact that different reforms had very different effects on the level and distribution of school revenues across districts, because they changed the funding formulas in very different ways (as shown by Hoxby, 2001; Jackson et al., 2015; Hyman, 2017, and also evident in my data). Ignoring this heterogeneity could impact both the precision and the consistency of the estimates.<sup>6</sup>

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<sup>5</sup>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.

<sup>6</sup>Importantly, both the changes in spending inequality and the patterns of household sorting triggered by each reform depend on the pre-reform and post-reform funding formula (Hoxby, 2001). For example, Jackson et al. (2015) find that school finance reforms increase expenditure more in *ex ante* lower-spending districts, whereas

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  $\beta$  (equivalent to a \$4,500 reduction in the difference in per pupil revenues between the richest and the poorest districts in a state) raises the income rank of children with parental income in the 10th percentile by 4.7 percentiles. This is equivalent to a 13.4 percent increase in income. By comparison, the same reduction in  $\beta$  has a smaller and insignificant effect on children with parental income in the 90th percentile. These estimates imply that the average reform would raise the income of children from families in the 10th percentile by 3.2 percentiles, and close approximately 12 percent of the gap between the lowest-IGM and the highest-IGM CZ.<sup>7</sup> Importantly, 2SLS estimates are larger than both a) OLS estimates and b) estimates obtained ignoring the differences in the funding formulas across states.

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 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 which “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 5.3 percentiles if the reform that generated this decline was experienced during elementary school, but only 1.7 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).

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Hyman (2017) finds that a reform passed in Michigan in 1993 increased expenditure more in low-poverty districts.

<sup>7</sup>The average reform reduces  $\beta$  by approximately 0.045 (Figure I), or 0.64 of a standard deviation. 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.

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. \(2014\)](#).<sup>8</sup> 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, 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.<sup>9</sup> 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, which includes observational ([Hanushek, 1986, 1997, 2003](#)), quasi-experimental ([Card and Krueger, 1992; Hyman, 2017](#)), and experimental studies ([Krueger, 1999; Dynarski et al., 2013](#)).<sup>10</sup> 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

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<sup>8</sup>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](#)).

<sup>9</sup>My findings are also in line with works illustrating the effectiveness of increased education expenditure ([Casco 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.

<sup>10</sup>This literature was initiated decades ago by the Coleman report ([Coleman et al., 1966](#)).

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. I show how even this approach is unable to 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. I demonstrate that accounting for these differences affects the estimates of the effects of equalization. My approach, and the accompanying hand-collected data, can be used in other settings as well.<sup>11</sup>

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 outlines the simulated instruments approach. Section 6 presents and discusses the main estimates of the effects of equalization on IGM. Section 7 investigates the mechanisms behind these effects, and Section 8 concludes.

## 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

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<sup>11</sup>The data and funding formula details will be made publicly available.

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.<sup>12</sup>

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

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<sup>12</sup>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).

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:

$$y_{id} = \theta x_{id} + \gamma q_d \quad (1)$$

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

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

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 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:

$$e_k = (1 - \beta_s) K_s + \beta_s x_k \quad \forall k \in s \quad (3)$$

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:

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

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:

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

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:

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

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 [Appendix B](#). Expenditures, revenues, and income are converted to 2000 US dollars.

**School Districts' Revenues and 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.<sup>13</sup>

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 and enrolling 62 percent of all US students.<sup>14</sup> [Appendix Table DI](#) describes the variables of each state dataset and [Appendix D](#) describes the funding formulas in detail.

[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.

**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

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<sup>13</sup>Information on school districts' expenditures and revenues is available from the US Census of Government and the National Center for Education Statistics (NCES) Longitudinal School District Dataset.

<sup>14</sup>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](#)).

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; when discrepancies are found, priority is given 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.<sup>15</sup> 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 16, irrespective of whether they moved when they entered the labor market.<sup>16</sup>

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*.<sup>17</sup> [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

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<sup>15</sup>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.

<sup>16</sup>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 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.

<sup>17</sup>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.

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).<sup>18</sup> 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.

**Cross-County Migration** Data on county-level migration flows and incomes of migrants are from the IRS Statistics of Income (SOI) and cover years 1991 to 2004. I calculate county-level individual migration rates as the ratio between the total number of in-migrants and out-migrants and the county's population.

**House Prices** To calculate changes in property values I use transaction-based annual house price indexes at the 5-digit zip code level for the years 1986 to 2004, published by the Federal Housing Finance Agency (FHFA).<sup>19</sup> I use information from the 1990 Census to link zip codes to school districts and aggregate house prices at the district level based on the population in each zip code. The coverage of this dataset varies across time, with 48 percent of all zip codes in 1986, 70 percent in 1995, and almost 100 percent in 2004. The available information allows me to obtain a measure of house prices for 64 percent of all districts in 1986, 82 percent in 1995, and 100 percent in 2004.

**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.

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<sup>18</sup>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.

<sup>19</sup>The construction of this index is explained in detail in [Bogin et al. \(2016\)](#).

## 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 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:<sup>20</sup>

$$e_{dt} = \alpha_{st} + \beta_{st}x_{dt} + \varepsilon_{dt} \quad (7)$$

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,  $\beta_{st}$  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.<sup>21</sup> 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.<sup>22</sup>

On average, the parameter  $\beta_{st}$  is equal to 0.024 for states without a school finance reform (with a standard deviation of 0.035); for states with a reform it equals 0.041 in the years before the reform (with a standard deviation of 0.035) and -0.003 in the years after the reform (with a standard deviation of 0.035, Table I, Panel C). Event study estimates indicate that  $\beta_{st}$  declines

<sup>20</sup> A similar approach has been used by Hoxby (1998); Card and Payne (2002); Lafortune et al. (2018).

<sup>21</sup> For example, the  $\beta_s$  for the 1980 cohort is the average of the  $\beta_{st}$  for the years 1986-1997.

<sup>22</sup> In robustness checks I use a version of  $\beta_{st}$  obtained interpolating income values between Census years (Table VI, column 1).

immediately following the reform and remains stable at this lower level over time (Figure I, top panel).<sup>23</sup> Similarly, a plot of cohort-specific  $\beta$  by years of exposure to a reform (where exposure = birth year + 18 - (reform year - 1) for each cohort) shows a sharp decline in  $\beta$  associated with exposure to a reform (Figure I, bottom panel).<sup>24</sup>

## 5 Addressing The Endogeneity of Post-Reform Revenues

### 5.1 Explaining The Need for An Instrument

Having estimated a measure  $\beta$  for the inequality in school spending, my next goal is to test the theoretical predictions derived in Section 2 and to estimate the effects of a decline in  $\beta$  on IGM. Doing so requires a source of exogenous variation in  $\beta$ . School finance reforms are a natural candidate: Several studies have used these reforms as exogenous shifters of school spending to study a variety of children’s outcomes (Jackson et al., 2015; Lafortune et al., 2018).

If one is simply interested in the 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. If, instead, one wants to estimate the causal effect of the changes in revenues and expenditures triggered by the reform, one must deal with an endogeneity problem.<sup>25</sup> To see this, consider the following simplified version of equation (6), which expresses the income rank of children in CZ  $c$  and state  $s$ , cohort  $b$ , and with parents’ income rank  $r$  as a function of  $\beta$  (for simplicity I focus only on one  $r$ ):

$$M_{cb}^r = \delta\beta_{sb} + \theta_c + \tau_b + \tilde{\varepsilon}_{cb} \quad (8)$$

The variable  $\tilde{\varepsilon}_{cb}$  is a residual component of IGM; it can include the composition of their group of peers or the community, summarized by  $\tilde{X}_{cb}$ :

<sup>23</sup>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) 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.

<sup>24</sup>The figure shows point estimates and 90 percent confidence intervals of the coefficients  $\delta_k$  in the equation  $\hat{\beta}_{sc} = \sum_{k=-6}^{12} \delta_k R_s \mathbb{1}(c + 18 - ryear_s - 1 = k) + \varepsilon_{sc}$ , where  $\hat{\beta}_{sc}$  is the average of  $\beta_{st}$  for cohort  $c$  and it is divided by the standard deviation of  $\beta_{st}$  in 1990.

<sup>25</sup>This is analogous to wanting to estimate a “structural” parameter, whereas Lafortune et al. (2018) estimate the reduced-form effect of equalization reforms.

$$\tilde{\varepsilon}_{cb} = \gamma \tilde{X}_{cb} + \varepsilon_{cb}$$

To simplify matters I express the variable  $\beta_{sb}$  as the product between a vector of parameters of the funding formula of state  $s$ ,  $g_{sb}$ , and a vector of all the variables entering that formula,  $X_{sb}$ :  $\beta_{sb} = X'_{sb}g_{sb}$  (where  $Cov(\tilde{X}_{cb}, X_{sb}) \neq 0$ ). Suppose that, due to a school finance reform, the funding formula for cohort  $b + 1$  changes to  $g_{sb+1}$ . 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 are included in  $X_{sb}$ , will change to  $X_{sb+1}$ . At the same time, this sorting could affect IGM through changes in  $\tilde{X}_{cb}$ .<sup>26</sup>

Due to the inclusion of  $\theta_c$ , OLS estimates of  $\delta$  can also be obtained from a first-differenced version of equation (8). These estimates are only consistent if the following exclusion restriction holds:

$$\mathbb{E}[(X'_{sb+1}g_{sb+1} - X'_{sb}g_{sb})(\gamma \tilde{X}_{cb+1} + \varepsilon_{cb+1} - \gamma \tilde{X}_{cb} - \varepsilon_{cb})] = 0$$

Since  $\mathbb{E}(X_{sb+1}\tilde{X}_{cb+1}) \neq 0$ , the exclusion restriction fails, causing an endogeneity problem.

**How Prevalent Is Household Sorting After A School Finance Reform?** The answer to this question determines the importance of addressing the endogeneity problem. To quantify this, I conduct an event study of county-level migration around an equalization event. I estimate:

$$m_{kt} = \sum_{n=-5}^5 \delta_n R_{s(k)} \mathbb{1}(t - ryear_{s(k)} = n) + \gamma_k + \tau_t + \varepsilon_{kt} \quad (9)$$

where  $m_{kt}$  is the in-migration (out-migration) rate, defined as the total number of households moving (out) of county  $k$  in year  $t$  divided by the total number of households in  $k$ . The variable  $R_{s(k)}$  equals 1 if state  $s$  of county  $k$  experienced a school finance reform in the years 1986-2004, and  $ryear_{s(k)}$  is the year of the earliest reform. The vectors  $\gamma_k$  and  $\tau_t$  are county and year fixed effects, respectively, and  $\varepsilon_{kt}$  is an error term. Estimates of the coefficients  $\delta_n$ , shown in Figure

<sup>26</sup>Note that this is the same justification for the IV strategy of Jackson et al. (2015), who also seek to estimate the causal effect of changes in expenditure levels on student outcomes (as opposed to just the effect of the reform).

II (top panel), capture year-specific changes in migration flows after each reform relative to the year preceding a reform. The differences between in-migration and out-migration rates of counties with and without a reform are indistinguishable from zero in the years leading up to a reform, and then they increase by 17 and 19 percent (or 0.13 and 0.14 percentage points) on average, respectively, in the years following the reform (Appendix Table AIII, panel A).<sup>27</sup>

These migration patterns, however, cause endogeneity in post-reform expenditure only if they are associated with sorting on income and wealth. To characterize these sorting patterns, I re-estimate equation (9) using the absolute value of the percentage difference between the incomes of migrants and stayers as the dependent variable. These estimates, shown in the bottom panel of Figure II, reveal that the absolute difference in the average income of both in-migrants and out-migrants and the average income of stayers is flat in the years leading up to a reform, and then it increases significantly (to a maximum of 9 and 7 percent, or 2.1 and 1.7 percentage points respectively) in the years after the reform.

Taken together, these results indicate that households sort across counties following a school finance reform. This sorting can affect house prices, change the composition of local communities, and in turn lead to the endogeneity of post-reform revenues.

## 5.2 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.<sup>28</sup> The goal of this strategy is to isolate the exogenous variation in funding inequality, driven by the timing of the reform and the funding formula, from the endogenous variation driven by sorting and changes in the tax base.

It is useful to express the post-reform  $\beta_{sb+1}$  as the sum of an exogenous component and an endogenous one:

$$\beta_{st+1} = X'_{st}g_{st+1} + b_{st+1}, \text{ where } b_{st+1} = X'_{st+1}g_{st+1} - X'_{st}g_{st+1}$$

<sup>27</sup>Appendix Table AIII shows estimates of equation (9) obtained pooling together years before and after each reform.

<sup>28</sup>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.

The quantity  $X'_{st}g_{t+1}$  is the  $\beta_{st+1}$  that would have resulted had households not sorted (and house prices not changed). This “simulated” version of  $\beta_{st+1}$ , which I denote as  $\beta_{st+1}^{sim}$ , can be used as an instrument to obtain consistent estimates of  $\delta^r$  in equation (8). The required exclusion restriction becomes:

$$\mathbb{E}[(X'_{sb}g_{sb+1} - X'_{sb}g_{sb})(\gamma\tilde{X}_{cb+1} + \varepsilon_{cb+1} - \gamma\tilde{X}_{cb} - \varepsilon_{cb})] = 0$$

This condition is satisfied if  $g_{sb+1} - g_{sb}$  is unrelated to  $X_{cb+1} - X_{cb}$  or, in other words, if the specific change in the funding formula is unrelated to sorting and the subsequent changes in the tax base. Appendix Table AII (described below) shows evidence in support of this assumption.

The correlation between  $b_{st+1}$  and IGM determines the sign of the bias of OLS estimates. If the effect of  $\beta$  on IGM is negative, a positive correlation implies that OLS will be biased toward zero, whereas a negative correlation implies that OLS will overstate the negative effect of  $\beta$ . The sign of this correlation is uncertain *ex ante* and depends on both  $X_{st}$  and  $g_{st+1}$ .

### 5.2.1 The Importance of Accounting for Differences in Funding Formulas Across States

While earlier studies of school finance reforms (such as Card and Payne, 2002) have not explicitly accounted for the endogeneity in post-reform revenues and expenditure, more recent studies (such as Jackson et al., 2015; Hyman, 2017) have recognized and addressed it. Jackson et al. (2015, JJP hereafter), for example, 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).

Although similar to JJP’s, my approach bears one important difference. 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 position in the state’s expenditure and income distributions. If one were to apply JJP’s strategy in my context, the instrument would be specified as  $\hat{\beta}_{st+1}^{sim} = \hat{X}'_{st}\hat{g}$ .<sup>29</sup> In other words, the instrument formula would be the same across all states, and the set of characteristics considered ( $\hat{X}_{st}$ ) would be a subset of all the ones entering the actual formula.

What happens when one uses  $\hat{\beta}_{st+1}^{sim}$  instead of  $\beta_{st+1}^{sim}$  as an instrument? First, the formula  $\hat{g}$  can be seen as a “restricted” or simplified version of  $g_{st+1}$ ; as a result, using  $\hat{g}$  implies using fewer instruments than there are available, which could lead to asymptotic inefficiency.

<sup>29</sup>Note that Jackson et al. (2015) instrument expenditure and not  $\beta$ .

efficiency.<sup>30</sup> Second, in the presence of large differences in  $g_{st}$  across states, the standard IV monotonicity assumption (Angrist and Imbens, 1995; Angrist et al., 1996) is more likely to be violated when using  $\hat{\beta}_{st}^{sim}$  than when using  $\beta_{st}^{sim}$ . To see this, consider an endogenous  $\beta_{st+1} = X'_{st+1}g_{st+1}$  with a corresponding value of JJP's instrument  $\hat{\beta}_{st+1}^{sim} = \hat{X}'_{st}\hat{g}$ . Suppose now that all states' formulas change to  $g_{kt+1}^0 \forall k$ , such that the resulting instrument for state  $s$  would be  $\hat{\beta}'_{st+1}^{sim} = \hat{X}'_{st}\hat{g}^0 \leq \hat{\beta}_{st+1}^{sim}$ . Monotonicity requires that  $\beta'_{st+1} = X'_{st+1}g_{st+1}^0 \leq \beta_{st+1}$  for all  $s$ ; this condition would be violated if there is a state where the instrument predicts an increase in equalization, but the actual changes in the formula and in  $X_{st}$  lead to a decline in equalization (or vice versa). If instead one uses an instrument  $\beta_{st+1}^{sim} = X'_{st}g_{st+1}$ , this assumption would fail only if the endogenous change in  $X_{st}$  alone were so dramatic to yield a change in  $\beta_{st+1}$  of the opposite sign as the the change in  $\beta_{st+1}^{sim}$ , since the function  $g_{st+1}^0$  is the same in  $\beta_{st+1}$  and  $\beta_{st+1}^{sim}$ .<sup>31</sup>

Clearly, the extent to which  $\beta_{st}^{sim}$  will be a better instrument than  $\hat{\beta}_{st}^{sim}$  depends on the actual heterogeneity in funding formulas across states (Hoxby, 2001). Figure III shows the trend in  $\beta$  around a reform in five states with reforms between 1989 and 1996. While some reforms were effective in reducing  $\beta$  (such as the one in Wisconsin in 1996, which reduced it from 0.021 in the year before the reform to 0.003 four years after the reform), some others were considerably less effective (such as the one in Michigan, which only reduced  $\beta$  from 0.045 to 0.041).<sup>32</sup>

Different reforms also had different effects on house prices. Figure IV shows trends in the house price difference between districts with average incomes above and below the state median in 1990. While some reforms (e.g. in Texas) were followed by a decline in this difference (i.e., an increase in house prices in poorer relative to richer districts), others did not trigger any changes (e.g. Michigan) or were followed by an increase (e.g. Massachusetts).<sup>33</sup>

<sup>30</sup>See Greene (2008, Chapter 12). Goldsmith-Pinkham et al. (2018) explain how, in a simulated instrument context, the parameters of the formula used to construct the instrument represent the actual instruments. Therefore, using a simplified version of the formula implies using fewer-than-available parameters.

<sup>31</sup>Mogstad et al. (2019) explain how, in the context of 2SLS with many instruments, the validity of the strategy is guaranteed by a (milder) "partial" monotonicity assumption, which essentially requires the standard monotonicity assumption to apply individually to each instrument. My argument still applies: when using  $\beta_{st+1}^{sim}$ , the instruments are the actual parameters of the funding formula entering  $\beta_{st+1}$ , whereas when using  $\hat{\beta}_{st+1}^{sim}$  they are not.

<sup>32</sup>These differences are consistent with the fact that Jackson et al. (2015) find that, on average, school finance reforms increase expenditure more in *ex ante* lower-spending districts, Hyman (2017) finds that Michigan's Proposal A increased expenditure more in low-poverty districts.

<sup>33</sup>Each point and spike in Figure IV represent the estimate and the 90 percent confidence interval of the coefficients  $\delta_n$  in the regression  $HP_{dt} = \sum_{n=-4}^6 \delta_n 1(Income_{d,1990} > Median_s) R_{s(d)} 1(t - Ryear_{s(d)} = n) + \theta_d + \tau_t + \varepsilon_{dt}$ , where  $HP_{dt}$  is the house price index of district  $d$  in year  $t$ ,  $Income_{d,1990}$  is average household income of district  $d$  in 1990,  $Median_s$  is median household income in state  $s$  in 1990,  $R_{s(d)}$  equals 1 if state  $s$  where the district is located experienced a school finance reform in the years 1986-2004,  $Ryear_{s(d)}$  is the year of the earliest reform, and  $\theta_d$  and  $\tau_t$  are district and year fixed effects. The parameters are estimated separately for each state. Observations are weighted by population. Standard errors are clustered at the state level.

Differences in the effectiveness of each reform and in the house price responses across states suggest that the use of  $\beta_{st}^{sim}$  instead of  $\hat{\beta}_{st}^{sim}$  could improve both the consistency and the efficiency of the estimates. In Section 6, I show that the differences between the estimates obtained using  $\beta_{st}^{sim}$  and those obtained using  $\hat{\beta}_{st}^{sim}$  are significant.

**Implementation** I construct  $\beta^{sim}$  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.<sup>34</sup> Lastly, I construct  $\beta^{sim}$  for each state and year, re-estimating equation (7) using simulated instead of actual revenues.<sup>35</sup>

**Assumptions** The validity of this approach relies on the exogeneity of the timing of each reform and of the type and parameters of the funding formula. This assumption could be violated if the funding formula chosen by each state or the timing of the reform were related to the state’s socioeconomic or political conditions. Hoxby (2001), however, explains that equalization schemes are more likely to be a reflection of a particular legal rhetoric rather than of specific objectives in terms of school spending and redistribution. This 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.

Appendix Figure AIX shows trends in simulated and actual revenues in some of the largest states, separately for districts in the top and bottom quartile of the state’s initial distribution of per pupil expenditure. The extent to which actual revenues differ from simulated revenues varies across states; it depends on the changes in property values in each district following a reform, driven by the *ex ante* characteristics of the district and by the change in the funding formulas. Districts where a reform triggered an increase in house prices experienced higher

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<sup>34</sup>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.

<sup>35</sup>For states with no reform between 1986 and 2004, I simply set  $\beta = \beta^{sim}$  for all years and cohorts.

revenues than they would have had house prices not changed, and vice versa (Appendix Figure AVII).<sup>36</sup>

On average, the parameter  $\beta^{sim}$  equals 0.042 (with a standard deviation of 0.036) in the years preceding each reform, and it drops to 0.005 (with a standard deviation of 0.035) in the years after the reform (Table I, Panel C). Estimates from the first stage of the IV estimation reveal that  $\beta^{sim}$  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 around 60. 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).<sup>37</sup>

## 6 Effects of Equalization on Intergenerational Mobility

Armed with a measure of inequality in school revenues and an instrument for it, I now estimate the effects of spending equalization on children’s IGM. To identify them, I exploit variation in exposure to equalization across cohorts and states, given by exogenous differences in the timing and effectiveness of the reforms.

To better illustrate the identification argument, Figure V shows a plot of IGM (measured as the income rank of children with parental incomes in the 25th percentile) by exposure to a reform, separately for states with an “effective” reform (i.e. one that resulted in a negative post-reform  $\beta$  or a decline in  $\beta$  of at least 50 percent, solid line) and for those with an “ineffective” reform (dashed line), using states with no reform as a control group. Exposure counts the number of school years a cohort was exposed to a “post-reform” regime while in school, equal to birth year + 18 – (reform year + 1), and it is negative for non-exposed cohorts.<sup>38</sup> In states with an effective reform, IGM gradually increases with exposure (e.g. it is 3.3 percentiles higher for cohorts exposed for twelve years compared with non-exposed cohorts). In states with an ineffective reform, on the other hand, IGM declines slightly.<sup>39</sup> Importantly, no pre-trends

<sup>36</sup> Appendix Figure AVII shows the relationship between the percentage change in house prices after a reform and the difference between actual and simulated revenues.

<sup>37</sup> Appendix Table AII shows estimates of a regression of  $\beta_{st}^{sim}$  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  $\beta_{st}^{sim}$  over time.

<sup>38</sup> 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.

<sup>39</sup> The figure shows OLS points estimates and 90 percent confidence intervals of the coefficients  $\delta_n$  in the equation  $m_{cb} = \sum_{n=-6}^{12} \delta_n E_n(s_b) + \theta_c + \tau_b + \varepsilon_{cb}$ , where  $m_{cb}$  is the mean rank of children in CZ  $c$ , cohort  $b$ , and with parents’

in IGM exist for non-exposed cohorts, which supports the assumption of exogenous timing. These estimates provide a first piece of evidence that exposure to effective reforms is associated with increased IGM.

## 6.1 2SLS and OLS Estimates

While useful to illustrate the trends in IGM across cohorts and states, the evidence in Figure V is based on an arbitrary definition of the effectiveness of a reform; in addition, it only informs us on the IGM of children whose parents are at the bottom of the income distribution. 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:

$$M_{cbx} = \delta_0 \hat{\beta}_{s(c)b} + \delta \hat{\beta}_{s(c)b} \theta_{n(cx)} + \kappa_c + \tau_b + \theta_{n(cx)} + \omega_{cbx} \quad (10)$$

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. 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.<sup>40</sup> 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 and Miller, 2015; Abadie et al., 2017), to account for the fact that  $\beta_{s(c)t}$  varies at the state level and allow for spatial correlation in IGM. For ease of interpretation, I

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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.

<sup>40</sup>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.

describe my estimates in terms of a *reduction* in  $\beta$ , i.e., an increase in equalization.

I begin by describing 2SLS estimates of equation (10), obtained using  $\hat{\beta}_{s(c)b}^{sim}$  and  $\hat{\beta}_{s(c)b}^{sim} \times \theta_{n(xc)}$  (the simulated versions of  $\hat{\beta}_{s(c)b}$  and  $\hat{\beta}_{s(c)b} \times \theta_{n(xc)}$ ) as instruments. First-stage estimates show that  $\hat{\beta}_{s(c)b}^{sim}$  is a strong instrument for  $\hat{\beta}_{s(c)b}$ , with Kleibergen-Paap F-statistics above 60 (Table II, columns 1-2).<sup>41</sup> Second-stage estimates indicate that a one-standard deviation (SD) 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, leads to a 5 percentile increase in IGM of children with parental income at the bottom of the income distribution (estimate of  $\beta$  equal to -4.998, Table II, column 3, significant at 1 percent). An estimate of  $\delta$  equal to 0.025 indicates that 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 3, significant at 1 percent). This implies that the same reduction in  $\beta$  leads to a 4.7 percentile increase in IGM for children with parental income in the 10th percentile, a 4.4 percentile increase for children with parental income in the 25th percentile, and a smaller 2.7 percentile increase for children with parental income in the 90th percentile. These results also indicate that the average reform, which decreases  $\beta$  by approximately 0.64 SD, would increase IGM of children from families in the 10th percentile by 3.2 percentiles and close 12 percent of the gap between the lowest-IGM CZ (Clarksdale, MS) and the highest-IGM CZ (Sioux Center, IA). Estimates are robust to controlling for state fixed effects (Appendix Table AIV).

OLS estimates of of equation (10), shown in column 5 of Table II, are consistent in sign with 2SLS estimates, but approximately 20 percent smaller in magnitude. They imply that a one-SD reduction in  $\beta$  leads to a 4.0 and 3.6 percentile increase for children with parental income in the 10th and 25th percentiles respectively, and an insignificant 2 percentile increase for those with parents in the 90th percentile (p-value 0.16). The difference between OLS and 2SLS suggests that not accounting for the endogeneity of  $\beta$  might yield biased estimates of the effects of equalization.

In Appendix Figure AX (lighter line) I estimate the effects of a decline in  $\beta$  separately for various quantiles of parental income in the national distribution. A one-SD reduction in  $\beta$  leads to a 4.8 percentile increase in IGM for children with parental income in the first decile (significant at 1 percent), but only 2.5 percentiles for children with parental income in the top one percent of the distribution (p-value equal to 0.11). Again, OLS estimates are smaller in

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<sup>41</sup>These estimates are above the critical values reported in Kleibergen and Paap (2006).

magnitude than 2SLS (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 (10). 2SLS estimates indicate that a one-SD reduction in  $\beta$  leads to a 13.4 percent increase in income for children with parental income in the 10th percentile, a 12.2 percent increase for children with parental income in the 25th percentile, and a smaller 7.2 percent increase for children with parental income in the 90th percentile (with an estimate of  $\beta$  equal to -0.133 and of  $\beta \times \text{parent centile}$  equal to 0.0007, Table III, column 1, significant 1 percent).

**Effects of Revenue Equalization vs Revenue Increases** So far I have interpreted the effects of a decline in  $\beta_s$  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  $\beta_s$  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  $\delta_0$  and  $\delta$  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).<sup>42</sup> Similarly, estimates of the effects of a decline in  $\beta_s$  for reforms that “leveled down” are only slightly smaller than those for reforms that “leveled up” (Appendix

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<sup>42</sup>For states that experienced both an equalization and an adequacy reform, I refer to the equalization reform. The results hold using adequacy reforms.

Table AVII).<sup>43</sup> Taken together, these tests suggest that a decline in  $\beta$  primarily captures the effects of equalization, rather than revenue levels.

**Reduced-Form Estimates** While useful to capture the causal effects of equalization on IGM, 2SLS 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  $\beta^{sim}$  through changes in the formula type and parameters. In column 4 of Table II I estimate the “reduced-form” effect of  $\beta^{sim}$  on IGM. These estimates indicate that a one-SD decline in  $\beta^{sim}$  leads to a 4.2 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.

**Estimates Using Jackson, Johnson, and Persico’s (2015) IV Approaches** In Appendix Table AIX I re-estimate equation (10) instrumenting  $\beta$  with the slope coefficient of equation (7) obtained using JJP’s instruments for expenditure (Jackson et al., 2015, approaches 1 and 2, pages 171-179; the top panel is estimated on the subsample of states included in the main analysis and the bottom panel is estimated on all states except Hawaii, which has only one school district.<sup>44</sup> I explain the procedure in more depth in Appendix C). These estimates reveal smaller effects of equalization on IGM.<sup>45</sup> The differences with my 2SLS estimates suggest that accounting for heterogeneity in funding formulas across states might matter when estimating the effects of equalization.

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<sup>43</sup>“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.

<sup>44</sup>The first stage estimates are shown in Appendix Table AVIII.

<sup>45</sup>To check that my replication exercise produces estimates of the effects of revenue increases on incomes of poorer children in the ballpark of those of JJP, in Appendix Table AX I estimate the effects of  $\beta_s$  on the *income*, rather than the income percentile, of children with parental incomes at different points in the distribution. Estimates from the full sample indicate that a one-standard deviation decline in  $\beta$  leads to a 4 percent increase in income for children with parental income on the 10th percentile (column 2). This estimate implies that the average reform, which reduces  $\beta$  by 0.64 standard deviations and produces a 10 percent (or \$660 dollars) increase in revenues in districts in the bottom decile of each state’s spending distribution in 1990, leads to a 3 percent increase in income for children with parents on the 10th income percentile.

### 6.1.1 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 a reform during elementary, middle, and high school.<sup>46</sup> I augment equation (10) as follows:

$$M_{cbx} = \delta_0^e \text{El}_{s(c)b} \hat{\beta}_{s(c)b} + \delta^e \text{El}_{s(c)b} \hat{\beta}_{s(c)b} \theta_{n(xc)} + \eta^m \text{Md}_{s(c)b} + \delta_0^m \text{Md}_{s(c)b} \hat{\beta}_{s(c)b} + \delta^m \text{Md}_{s(c)b} \hat{\beta}_{s(c)b} \theta_{n(xc)} \\ + \eta^{hs} \text{HS}_{s(c)b} + \delta_0^{hs} \text{HS}_{s(c)b} \hat{\beta}_{s(c)b} + \delta^{hs} \text{HS}_{s(c)b} \hat{\beta}_{s(c)b} \theta_{n(xc)} + \kappa_c + \tau_b + \theta_{n(xc)} + \omega_{cbx} \quad (11)$$

where  $\text{El}_{s(c)b}$ ,  $\text{Md}_{s(c)b}$ , and  $\text{HS}_{s(c)b}$  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-SD 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.

2SLS estimates of equation (11) indicate that a decline in  $\beta$  is most effective when the reform hits during elementary school, relative to middle and high school. A one-SD reduction in  $\beta$  leads to an additional 6.2 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  $\beta \times \text{reform in elementary school}$  equal to -6.236, Table IV, column 1, significant at 1 percent). This effect declines by 0.09 percentiles with each additional percentile of parents' income (estimate of  $\beta \times \text{parent centile} \times \text{reform in elementary school}$  equal to 0.0947, Table IV, column 1, significant at 1 percent). These estimates imply that, when a reform hits during elementary school, a reduction in  $\beta$  leads to an additional 5.3

<sup>46</sup>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.

and 3.9 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-SD decline in  $\beta$  leads to a smaller 1.7 and 1.4 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  $\beta \times \text{reform in high school}$  equal to -1.8915 and of  $\beta \times \text{parent centile} \times \text{reform in high school}$  equal to 0.0215, Table IV, column 1). 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 elementary and high school ones. OLS estimates, shown in columns 3 and 4 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 2SLS estimates confirms the importance of accounting for the endogeneity in post-reform revenues.

### 6.1.2 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 (10) 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).

These estimates, shown in columns 1 and 2 of Table V, indicate that a decline in  $\beta$  has a much larger effect in states with more competition across school districts. Controlling for CZ fixed effects, a one-SD decline in  $\beta$  in "High competition" states leads to a 6.8, 6.3, and 3.9 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of  $\beta$  equal to -7.170 and of  $\beta \times \text{parent centile}$  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 3.9 and 2.8 percentile increase for children with parents in the 10th and 25th percentile, respectively, and to an insignificant 2.0 percentile decline for children with parents on the 90th percentile (with an estimate of  $\beta$  equal to -4.610 and of  $\beta \times \text{parent centile}$  equal to 0.074, Table V, column 1, significant at 1 percent). These estimates confirm the prediction of

Section 2 and suggest that district competition might explain part of the effects of equalization on IGM.

### 6.1.3 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 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.<sup>47</sup> 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 income inequality, I re-estimate equation (10) 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.<sup>48</sup> These 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-SD decline in  $\beta$  in “Low inequality” CZs leads to a 3.1, 2.7, and 0.9 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of  $\beta$  equal to -3.3868 and of  $\beta \times \text{parent centile}$  equal to 0.0277, Table V, column 3). These effects are larger in “High inequality” CZs: The same decline in  $\beta$  leads to a 5.2, 4.9, and 3.5 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of  $\beta$  equal to -5.4077 and of  $\beta \times \text{parent centile}$  equal to 0.021, Table V, column 4, significant at 1 percent).

<sup>47</sup> $\beta = \frac{9,000 - 7,000}{75,000 - 25,000} = \frac{8,300 - 5,500}{85,000 - 15,000} = 0.04.$

<sup>48</sup>I calculate this difference using incomes from 1990.

### 6.1.4 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 (10) 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.<sup>49</sup> Estimates of  $\delta_0$  and  $\delta$  indicate that, controlling for CZ fixed effects, a one-SD decline in  $\beta$  in "Low segregation" CZs leads to a 4.1, 3.7, and 1.8 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of  $\beta$  equal to -4.4046 and of  $\beta \times \text{parent centile}$  equal to 0.029, 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 5.2, 4.9, and 3.4 percentile increase for children with parents in the 10th, 25th, and 90th percentiles, respectively (with an estimate of  $\beta$  equal to -5.4730 and of  $\beta \times \text{parent centile}$  equal to 0.023, 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.

## 6.2 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 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.

**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).<sup>50</sup> The same decline in  $\beta$  might have different effects in one-state and multi-state CZs. On one

<sup>49</sup>The Theil index is calculated as  $T_c = \frac{1}{N} \sum_{i \in c} \frac{y_i}{\bar{y}_c} \ln \frac{y_i}{\bar{y}_c}$ , 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.

<sup>50</sup>In the baseline estimates I follow Chetty et al. (2014) and assign each of these CZs the state with the largest population share.

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 2SLS estimates of the main specifications separately for one-state and multi-state CZs. Estimates are comparable across the two groups, indicating that the results are not driven by the presence or absence of borders.

**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 corresponds 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.

## 7 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.

### 7.1 Inputs: Teacher-Student Ratio

School finance equalization is often described as a way of “leveling the playing field,” i.e., reducing the gap in educational inputs between more and less disadvantaged children. To test 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 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.<sup>51</sup>

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

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<sup>51</sup>From an analysis of the Center on Budget and Policy Priorities using data from the Bureau of Labor Statistics.

estimate the following equation:

$$TS_{dt} = \delta_1 \hat{\beta}_{s(d)t} q_{dt}^{1q} + \delta_2 \hat{\beta}_{s(d)t} q_{dt}^{2q} + \delta_3 \hat{\beta}_{s(d)t} q_{dt}^{3q} + \delta_4 \hat{\beta}_{s(d)t} q_{dt}^{4q} + \gamma_d + \tau_t + \varepsilon_{dt} \quad (12)$$

where  $TS_{dt}$  is the teacher-student ratio of district  $d$  in year  $t$ ; the variable  $q_{dt}^{nq}$  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 2SLS estimates of equation (12). 2SLS estimates, shown in columns 1 and 2, yield positive effects on low-income districts, but no effect on high-income ones. Controlling for district fixed effects, a one-SD reduction in  $\beta$  leads to 0.0094 additional teachers per student in districts in the bottom quartile, or 14 percent more (Table VII, column 4, significant at 1 percent). The same estimate is -0.001 for districts in the top quartile and it is indistinguishable from zero (Table VII, column 4).

Taken together, these estimates indicate that equalizing school spending across richer and poorer districts raises IGM by reducing the gap in educational inputs between low-income and high-income districts. This reduction is achieved through an improvement in the teacher-student ratio in low-income districts, with no significant change in high-income ones.

## 8 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.

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." Importantly, I show that both household incentives to sort across districts and the ultimate effects on equalization are idiosyncratic to each reform;

this stresses the importance of accounting for this heterogeneity in the empirical analysis to obtain consistent and efficient estimates of the effects.

To account for this source of endogeneity and for the differences in funding formulas across states, I propose 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. My approach and the accompanying dataset can be used in other studies as well.

At a first glance, these findings might appear in contrast with Rothstein (2019), who uses a decomposition analysis to conclude that differences in school quality across the US play a minor role in explaining the observed cross-sectional variation in intergenerational mobility. My results, however, do not necessarily disprove Rothstein's argument. In fact, they confirm that school quality explains a small share (approximately 10 percent) of the total variation in mobility. In spite of this, they also show 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.

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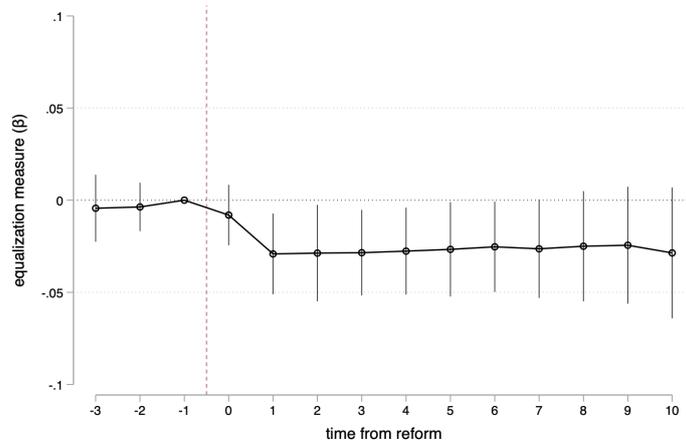
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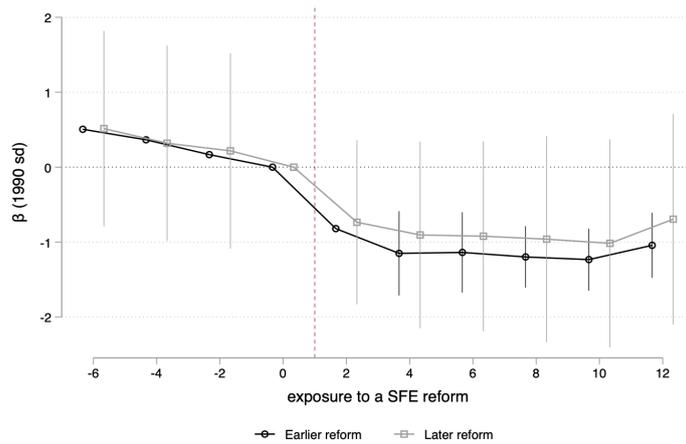
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Figure I: School Finance Reforms and Changes in  $\beta$

Panel A) Event Study of Equalization Measure  $\beta$  Around A School Finance Reform

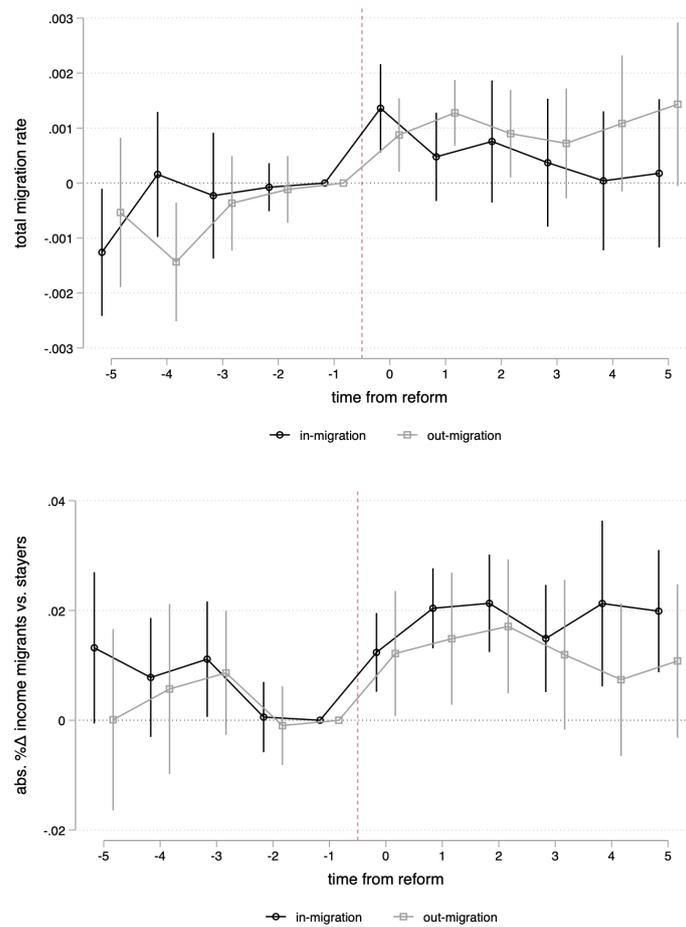


Panel B)  $\beta$  By Length of Exposure to a Reform



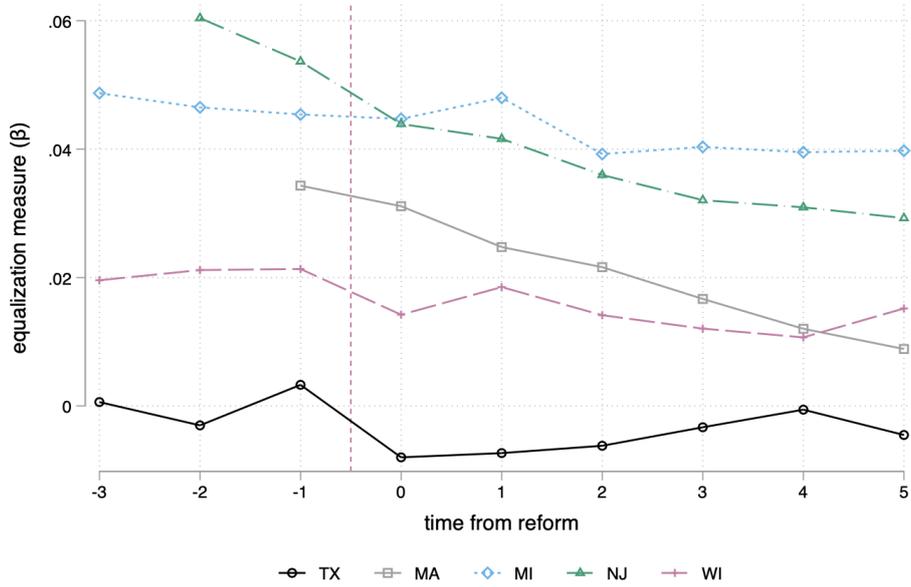
*Note:* Panel A) shows OLS estimates and 90 percent confidence intervals for the coefficients  $\delta_k$  in regression  $\beta_{st} = \sum_k \delta_k R_s \mathbb{1}(t - ryear_s = k) + \varepsilon_{st}$ , where  $\beta_{st}$  is the slope coefficient in equation (7), estimated separately for each state  $s$  and year  $t$  from 1986 to 2004,  $R_s$  equals 1 if state  $s$  had a school finance reform in the years 1980-2004, and  $ryear_s$  is the year of the first reform in this time period. The coefficient  $\delta_{-1}$  is normalized to equal zero. Panel B) shows OLS estimates and 90 percent confidence intervals of the parameters  $\delta_n$  in the equation  $\beta_{sc} = \sum_{-6}^{12} \delta_n \mathbb{1}(c + 18 - R_s + 1) + \varepsilon_{sc}$ , where  $s$  denotes the state,  $c$  the birth cohort and  $\beta_{sc}$  is the OLS coefficient from equation (7), averaged across the years in which each cohort is in school. The parameter  $\delta_0$  is normalized to equal zero. Standard errors are clustered at the state level. The sample is restricted to California, Colorado, Florida, Georgia, Illinois, Kentucky, Louisiana, Massachusetts, Michigan, Minnesota, Montana, Nebraska, New Jersey, New York, North Dakota, Ohio, Pennsylvania, Utah, Texas, and Wisconsin.

Figure II: Event Studies of Migration Rates (Top Panel) and Incomes of Migrants vs Incumbents (Bottom Panel) Around A School Finance Reform



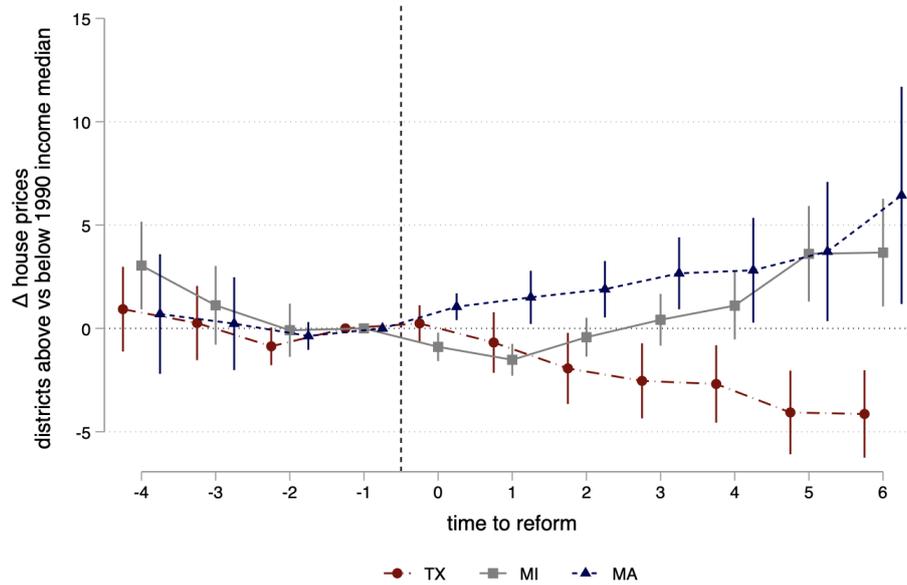
*Note:* Changes in total migration rates and incomes of migrants in a 10-year window around each school finance reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients  $\delta_n$  in the regression  $y_{kt} = \sum_{n=-5}^5 \delta_n R_{s(k)} 1(t - ryear_{s(k)} = n) + \gamma_k + \tau_t + \varepsilon_{kt}$ , where  $R_{s(k)}$  equals 1 if state  $s$  of county  $k$  experienced a school finance reform in the years 1980-2004,  $ryear_{s(k)}$  is the year of the earliest reform,  $\gamma_k$  are county fixed effects, and  $\tau_t$  are year fixed effects. In the top panel,  $y_{kt}$  is the total in-migration or out-migration rate in county  $k$  and year  $t$  (the ratio between the sum of in-migrants or out-migrants and the total population in each county). In the bottom panel,  $y_{kt}$  is the absolute percentage difference between incomes of in-migrants or out-migrants and incomes of stayers in county  $k$  and year  $t$ , divided by 100. Standard errors are clustered at the county level. County-year level observations are weighted by population. Data on migration are from the Statistics of Income of the Internal Revenue Service and cover years from 1991 to 2004.

Figure III: Equalization Measure  $\beta$  Around A School Finance Reform - Selected States



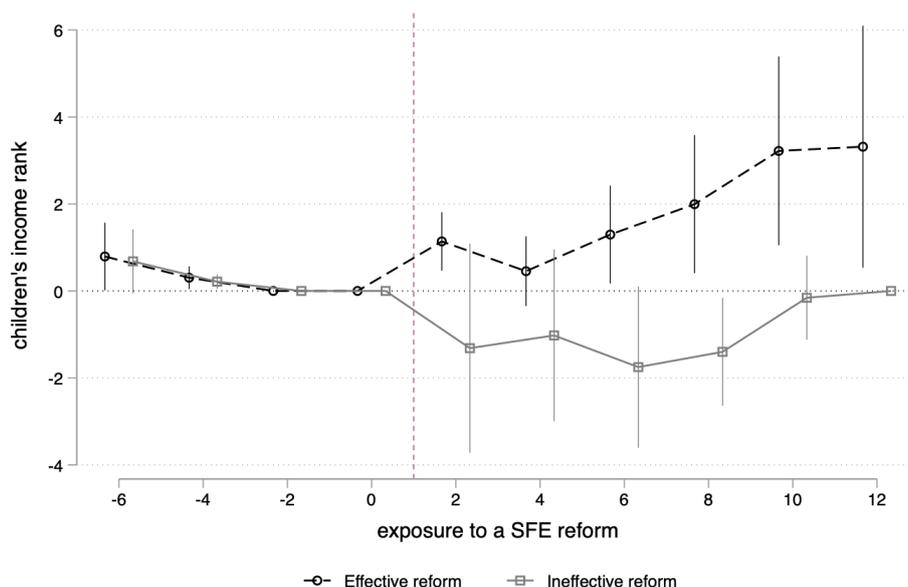
Note: The figure shows estimates of the coefficient  $\beta_{st}$  (defined in equation (7)) for a sample of states in the years surrounding each school finance reform.

Figure IV: Variation in House Prices Around a School Finance Reform - Selected States



*Note:* Changes in the difference in house prices between households with incomes above and below the median in 1990, in a 10-year window around each reform and relative to the year before the reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients  $\delta_n$  in the regression  $HP_{dt} = \sum_{n=-4}^6 \delta_n 1(Income_{d,1990} > Median_s) R_{s(d)} 1(t - ryear_{s(d)} = n) + \theta_d + \tau_t + \varepsilon_{dt}$ , where  $HP_{dt}$  is the house price index of district  $d$  in year  $t$ ,  $Income_{d,1990}$  is average household income of district  $d$  in 1990,  $Median_s$  is median household income across districts in state  $s$  in 1990,  $R_{s(d)}$  equals 1 if state  $s$  where the district is located experienced a school finance reform in the years 1980-2004,  $ryear_{s(d)}$  is the year of the earliest reform, and  $\theta_d$  and  $\tau_t$  are district and year fixed effects. The coefficient  $\delta_{-1}$  is normalized to zero. The parameters are estimated separately for each state. Standard errors are clustered at the district level. Observations are weighted by population. Annual House Price Indexes data are taken from the FHFA, aggregated at the district level using population weights, and cover years from 1986 to 2004.

Figure V: 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  $m_{cb} = \sum_n \delta_n E_{n(sb)} + \theta_c + \tau_b + \varepsilon_{cv}$ , 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 vectors  $\theta_c$  and  $\tau_b$  contain CZ and cohort 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 50 percent, including Colorado, Kentucky, Montana, Nebraska, Texas, and Wisconsin, solid line) and ineffective reforms (including Louisiana, Massachusetts, Michigan, Minnesota, and New Jersey, dashed line), using states with no reform (including California, Florida, Georgia, Illinois, New York, North Dakota, Ohio, Pennsylvania, and Utah) as a control group. Observations are at the CZ  $\times$  birth cohort level, and they are weighted by the number of children in each CZ and cohort. 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
<i>Median income</i>						
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
<i><math>\Delta</math> revenue, richest vs poorest district within state (\$)</i>						
1986	2907	6318	2717	-7052	14162	2788
1990	2987	4474	2476	-2306	12965	4895
2000	2666	5449	1860	-7024	15415	7146
2004	2336	7119	52	-8346	18120	6653
<i><math>\Delta</math> revenue, richest vs poorest district within CZ (\$)</i>						
1986	1261	4860	201	-13816	13890	2788
1990	1468	3347	408	-11045	14518	4895
2000	96	5255	-51	-15841	17195	7146
2004	278	5406	-344	-17741	19453	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 (0.040)	0.433 (0.033)	0.569 (0.024)	0.609 (0.027)
1983-86	0.396 (0.034)	0.435 (0.030)	0.567 (0.031)	0.607 (0.035)
N (CZs)	589	589	589	589

Panel C: Measures of School Finance Equalization ( $\beta$ )					
	All	No reform	Pre-Reform	Post-Reform	Difference
$\beta$	0.013 (0.064)	0.024 (0.096)	0.041 (0.035)	-0.003 (0.035)	-0.044*** (0.006)
$\beta^{sim}$	0.017 (0.063)	0.024 (0.096)	0.042 (0.036)	0.005 (0.035)	-0.036*** (0.006)

*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 ( $\beta^{sim}$ ).

Table II: School Finance Equalization and Intergenerational Mobility. OLS and 2SLS, Dependent Variable is Children's Income Percentile

	2SLS			OLS	
	1st stage		2nd stage	Red.form	Main
	(1)	(2)	(3)	(4)	(5)
	$\beta$	$\beta \times \text{par. pctile}$	IGM	IGM	IGM
$\beta \text{ sim.}$	0.8433*** (0.0505)	-8.2195** (3.2204)		-4.4216*** (1.3596)	
$\beta \text{ sim.} \times \text{parent pctile}$	0.0000 (0.0000)	0.9780*** (0.0072)		0.0245*** (0.0021)	
$\beta$			-4.9984*** (1.6627)		-4.2663*** (1.4170)
$\beta \times \text{parent pctile}$			0.0251*** (0.0021)		0.0254*** (0.0022)
Parent pctile FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes	Yes
KP Wald F-stat	61.39				
N	13578	13578	13578	13578	13578
10th			4.747	4.176	4.012
10th [p-value]			0.004	0.002	0.005
25th			4.371	3.808	3.630
25th [p-value]			0.008	0.005	0.010
90th			2.740	2.213	1.976
90th [p-value]			0.097	0.104	0.157

Note: The table shows OLS and 2SLS estimates of the parameters  $\delta_0$  and  $\delta$  in equation (10). Columns 1-2 show the 2SLS first stage, column 3 shows the second stage, column 4 shows the reduced-form, and column 5 shows simple OLS. In columns 3-5, 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 *parent pctile* is the percentile of parents in the national income distribution. 2SLS estimates are obtained using  $\beta \text{ sim}$  and  $\beta \text{ sim} \times \text{parent pctile}$  as instruments for  $\beta$  and  $\beta \times \text{parent pctile}$ ; the variable  $\beta \text{ sim}$  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 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 III: School Finance Equalization and Intergenerational Mobility. OLS and 2SLS, Dependent Variable is Children's log(Income)

	2SLS, Second stage		OLS	
	(1)	(2)	(3)	(4)
$\beta$	-0.1328*** (0.0404)	-0.1309*** (0.0414)	-0.1127*** (0.0347)	-0.1100*** (0.0364)
$\beta \times$ parent centile	0.0007*** (0.0001)	0.0007*** (0.0001)	0.0007*** (0.0001)	0.0007*** (0.0001)
Parent pctile FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
CZ FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
N	13578	13578	13578	13578
10th	0.134	0.132	0.111	0.108
10th [p-value]	0.002	0.003	0.002	0.005
25th	0.122	0.120	0.099	0.096
25th [p-value]	0.004	0.006	0.006	0.011
90th	0.072	0.069	0.049	0.047
90th [p-value]	0.083	0.097	0.161	0.201

*Note:* The table shows 2SLS second-stage (columns 1 and 2) and OLS estimates (columns 3 and 4) of the parameters  $\delta_0$  and  $\delta$  in equation (10). 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 *parent pctile* is the percentile of parents in the national income distribution. In columns 3 and 4, the variables  $\beta$  and  $\beta \times$  *parent pctile* are instrumented using  $\beta$  *sim* and  $\beta$  *sim*  $\times$  *parent pctile*; the variable  $\beta$  *sim* is estimated as  $\beta$  using simulated revenues instead of actual revenues. All specifications include parent percentile and cohort fixed effects; columns 1 and 3 include CZ fixed effects, and columns 2 and 4 include state 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 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 IV: Heterogeneous Effects of School Finance Equalization Across School Grades. OLS and 2SLS, Dependent Variable is Children's Income Percentile

	2SLS, Second stage		OLS	
	(1)	(2)	(3)	(4)
$\beta \times$ reform in elementary school	-6.2355*** (0.8725)	-6.1336* (2.9812)	-3.3543* (1.9475)	-3.1335* (1.8829)
$\beta \times$ parent pctile $\times$ reform in elem school	0.0947*** (0.0039)	0.0949** (0.0269)	0.0742*** (0.0168)	0.0744*** (0.0176)
$\beta \times$ reform in middle school	-2.6918*** (0.7784)	-2.6304 (2.0931)	-1.5624 (1.7875)	-1.4326 (1.7425)
$\beta \times$ parent pctile $\times$ reform in middle school	0.0362*** (0.0029)	0.0362*** (0.0075)	0.0376*** (0.0079)	0.0375*** (0.0073)
$\beta \times$ reform in high school	-1.8915** (0.7464)	-1.8640 (2.2420)	-1.0279 (1.6769)	-0.9250 (1.6954)
$\beta \times$ parent pctile $\times$ reform in HS	0.0215*** (0.0020)	0.0215 (0.0140)	0.0246*** (0.0084)	0.0246*** (0.0086)
Parent pctile FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
CZ FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
Reform in elem, middle, high	Yes	Yes	Yes	Yes
N	13578	13578	13578	13578
10th, elem	5.288	5.185	2.612	2.390
25th, elem	3.867	3.762	1.498	1.274
90th, elem	-2.292	-2.404	-3.327	-3.560
10th, high	1.677	1.649	0.782	0.679
25th, high	1.355	1.327	0.412	0.309
90th, high	-0.040	-0.068	-1.189	-1.291

Note: The table shows OLS (columns 1 and 2) and 2SLS second-stage estimates (columns 3 and 4) of the parameters in equation (11). 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 *parent pctile* is the percentile of parents in the national income distribution. In columns 3 and 4, the variable  $\beta$  is instrumented with  $\beta sim$ , estimated as  $\beta$  using simulated revenues instead of actual revenues. The variables *reform in elem school*, *reform in middle school*, and *reform in HS* equal one for cohorts and states for which a reform hit during elementary, middle, and high school, respectively. All specifications include parent percentile and cohort fixed effects; columns 1 and 3 include CZ fixed effects, and columns 2 and 4 include state 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 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 V: Heterogeneous Effects of School Finance Equalization. 2SLS, Dependent Variable is Children's Income Percentile

	Competition		Inequality		Segregation	
	(1) Low	(2) High	(3) Low	(4) High	(5) Low	(6) High
$\beta$	-4.6097*** (1.6712)	-7.1699*** (2.2842)	-3.3868 (2.1181)	-5.4077*** (1.6155)	-4.4046** (1.7184)	-5.4730*** (1.5662)
$\beta \times$ parent pctile	0.0735*** (0.0080)	0.0363*** (0.0046)	0.0277*** (0.0053)	0.0210*** (0.0019)	0.0286*** (0.0038)	0.0232*** (0.0022)
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	6510	7068	5586	7950	5880	7698
10th	3.874	6.807	3.110	5.198	4.118	5.241
10th [p-value]	0.019	0.003	0.140	0.001	0.016	0.001
25th	2.771	6.263	2.695	4.883	3.689	4.893
25th [p-value]	0.088	0.006	0.198	0.003	0.029	0.002
90th	-2.009	3.904	0.896	3.518	1.828	3.384
90th [p-value]	0.212	0.099	0.664	0.033	0.264	0.034

*Note:* 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 *parent pctile* is the percentile of parents in the national income distribution. The variable  $\beta$  is instrumented by  $\beta_{sim}$ , estimated as  $\beta$  using simulated revenues instead of actual revenues. All specifications include parent percentile, CZ, 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 *X*th 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. 2SLS, Dependent Variable is Children's Income Percentile

	Intpl income	Border		Alt. pctl def.	
	(1)	(2) Without	(3) With	(4) National	(5) Cz
$\beta$	-5.9568** (2.3708)	-5.5086*** (1.9901)	-3.7527*** (1.2334)	-4.6355*** (1.5575)	-4.7162*** (1.3600)
$\beta \times$ parent pctl	0.0253*** (0.0022)	0.0266*** (0.0024)	0.0212*** (0.0029)	0.0228*** (0.0022)	0.0259*** (0.0025)
Parent pctl 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	5.704	5.242	3.541	4.408	4.457
10th [p-value]	0.016	0.008	0.004	0.005	0.001
25th	5.324	4.842	3.224	4.066	4.069
25th [p-value]	0.024	0.015	0.008	0.009	0.003
90th	3.678	3.111	1.849	2.585	2.385
90th [p-value]	0.115	0.116	0.128	0.094	0.074

*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 *parent pctl* is the percentile of parents in the national income distribution. The variable  $\beta$  is instrumented by  $\beta$  *sim*, estimated as  $\beta$  using simulated revenues instead of actual revenues. All specifications include parent percentile, CZ, 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 2SLS, Dependent Variable is the Number of Teachers per Student

	2SLS, Second stage		OLS	
	(1)	(2)	(3)	(4)
$\beta \times$ income in the 1 <sup>st</sup> quartile	-0.0069*** (0.0019)	-0.0094*** (0.0023)	-0.0053*** (0.0014)	-0.0086*** (0.0016)
$\beta \times$ income in the 2 <sup>nd</sup> quartile	-0.0046** (0.0018)	-0.0045** (0.0020)	-0.0027** (0.0013)	-0.0014 (0.0013)
$\beta \times$ income in the 3 <sup>rd</sup> quartile	-0.0031* (0.0019)	-0.0045* (0.0025)	-0.0012 (0.0014)	-0.0028 (0.0018)
$\beta \times$ income in the 4 <sup>th</sup> quartile	-0.0019 (0.0019)	-0.0010 (0.0023)	-0.0003 (0.0014)	-0.0004 (0.0019)
Year FE	Yes	Yes	Yes	Yes
State FE	Yes	No	Yes	No
District FE	No	Yes	No	Yes
Quartile FE	Yes	Yes	Yes	Yes
N (district $\times$ year)	110833	110773	110833	110773
Y-mean	0.072	0.072	0.072	0.072

*Note:* 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<sup>th</sup> quartile* equals 1 for districts with median household income in the X<sup>th</sup> quartile of the national distribution in 1990. Columns 1 and 2 estimate OLS; columns 3 and 4 estimate 2SLS, with  $\beta^{sim}$  (obtained using simulated revenues instead of actual revenues) as an instrument for  $\beta$ . All specifications include year fixed effects; columns 1 and 3 include state fixed effects, and columns 2 and 4 include district fixed effects. Standard errors in parentheses are clustered at the state and year level. 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.