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 public school districts on students' intergenerational mobility. I exploit differences in exposure to equalization across seven cohorts of students in 20 US states, generated by 13 state-level school finance reforms passed between 1980 and 2004. Since these reforms create incentives for households to sort across districts and this sorting affects property values, post-reform revenues are endogenous to an extent that varies across states. I address this issue with a simulated-instruments approach, which uses newly collected data on states' funding formulas to simulate revenues in the absence of sorting. I find that equalization has a large effect on mobility of low-income students, with no significant changes for high-income students. Reductions in the gaps in inputs (such as the number of teachers) and in college attendance between low-income and high-income districts are likely channels behind this effect.

JEL Classification: I22, I24, J62

Keywords: School Finance, Intergenerational Mobility, Simulated Instruments

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1 Introduction

Large differences in intergenerational income mobility (IGM) exist across states and local areas in 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 guaranteeing 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 (as proxied by test scores, 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 levies (such as 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: In 1980, the gap in revenues between the lowest-spending and the highest-spending district was 70 percent in California, but only 40 percent in Maryland. Part of this variation is driven by cross-state differences in funding formulas, which define each district's revenues as a combination of state funds and local levies. In an attempt to equalize spending, over time 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).

To study the causal effects of equalization on children's IGM I exploit changes in the distribution of per pupil revenues generated by 13 state-level school finance reforms, passed in 20 states between 1980 and 2004. 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 β (as in Hoxby, 1998; Card and Payne, 2002; Lafortune et al., 2018). When revenues are perfectly equalized in a state, β equals zero; when wealthier districts receive more, β is positive.

I show that school finance reforms led to sharp declines in β, 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.[1] The model also shows how equalizing revenues across districts is not necessarily akin to raising resources for poor districts in a state: If rich and poor districts compete for scarce inputs (such as good teachers Hanushek et al., 2004), a district's revenues *relative to other districts* will impact children's outcomes, and not just absolute revenue levels. In line with this possibility, my empirical analysis focuses on the effects of revenue equalization, as opposed to revenue increases.

To empirically investigate 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 βs) across states. For example in Wisconsin, which had a reform in 1996, the 1980 cohort (in school between 1986 and 1998) experienced a β 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 β 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 β equal to 0.044 and 0.047, respectively. Assuming that the timing of each reform is independent on 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 mobility was on a flat trend in the years leading to each reform.[2]

To estimate the causal effects of a decline in β on mobility, however, one must deal with an additional endogeneity problem which the existing literature has only partially addressed. The changes in the funding formula that lead to changes in β alter the relationship between the "price" of school spending to taxpayers and the amount of public good they receive in return. This might

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

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

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.[3] 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 mobility (for example through peer effects). Post-reform revenues – and the resulting β – 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. To do this, 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 β, which I use as an instrument for β.

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.[4]

Two-stages least squares (2SLS) estimates of the effects of changes in β indicate that school finance equalization has a sizable positive effect on IGM. A one-standard deviation reduction in β (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

[3] 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.

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

10th percentile by 5.7 percentiles; this estimate correspond to a 16.4 percent increase in income. By comparison, the same reduction in β has a smaller and insignificant effect on children with parents in the 90th income percentile. These results hold when controlling for total school spending in each state. My results also indicate that the average reform would raise the income of children from families in the 10th percentile by 3.6 percentiles, and close approximately 13 percent of the gap between the lowest-mobility and the highest-mobility CZ.[5] Importantly, 2SLS estimates are approximately 50 percent larger than both a) OLS estimates and b) estimates obtained ignoring the differences in the funding formulas across states.

My estimates also show that equalization is most effective when experienced earlier in a child's education career. A one-standard deviation reduction in β raises the income rank of children with parents in the 10th percentile by 7.7 percentiles if the reform that generated this decline was experienced during elementary school, but only 3.6 percentiles if it was experienced during high school. This finding is in line with a large literature highlighting the importance of early childhood investments for long-run outcomes (Cunha and Heckman, 2010).

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

In the last part of the paper I explore some potential channels behind the main estimates. 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) and in intermediate educational outcomes (such as college enrollment) between richer and poorer districts.

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

[5]The average reform reduces β by approximately 0.045 (Figure I), or 0.64 of a standard deviation. The gap in absolute mobility between the highest-mobility CZ (Sioux Center, IA) and the lowest-mobility one (Clarksdale, MS) is equal to 27.6 percentiles for children from families in the 10th percentile.

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

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 mobility, even if it explains a relatively small share of the cross-CZ differences in IGM.[7] My results are also informative of the mechanisms through which equalization in school resources affect children's long-run outcomes, such as equalization in school inputs and in college attendance.

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).[8] A few works have used school finance reforms as a source of variation in school spending to study the effects on student achievement and educational attainment (Hoxby, 2001; Card and Payne, 2002; Hyman, 2017; Lafortune et al., 2018). In the closest paper to mine, Jackson et al. (2015) explore the long-run effects of expenditure increases triggered by these reforms and find that they raised students' incomes and reduced poverty. I extend this literature by studying the effects of the reforms on IGM, a measure of the relationship between children's and parents' outcomes which has received considerable attention in recent years. As such, the focus of my analysis is on the effects of revenue equalization (rather than levels), the key parameter that determines the extent to which public education investments depend on parents' incomes.

Lastly, and perhaps most importantly, 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 changes in spending driven by school finance reforms. 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 ignoring these differences can lead to inconsistent estimates of the effects of equalization. My approach, and the accompanying hand-collected data, can be used in other settings as well.[9]

The rest of the paper is organized as follows. Section 2 describes school finance equalization reforms. Section 3 presents a simple model to illustrate the relationship between school finance

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

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

[9] The data and funding formula details will be made publicly available.

equalization and IGM. Sections 4 and 5 describe the data and the measure of inequality in school revenues. Section 6 outlines the simulated instruments approach. Section 7 presents and discusses the main estimates of the effects of equalization on IGM. Section 8 investigates the mechanisms behind these effects, and Section 9 concludes.

2 School Finance Equalization Reforms

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

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

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

One common aspect of school finance schemes is that the basis for equalization, i.e., the tax

base, is endogenous. A change in the funding formula provides households with incentives to sort across school districts depending on their preference for public schools and their income; these movements affect house prices and district revenues. The failure of policymakers to fully understand and anticipate these responses when designing school finance plans has caused some reforms to *reduce* overall expenditure on public schools.[10]

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

3 A Model of School Finance and Intergenerational Mobility

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

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

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

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

where e_k is public spending per pupil in district k and N_s is the number of districts in s. In words, the effective education investment in district d depends on (a) direct spending in that district

[10] 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).

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 π_s and on spending in other districts. District k's spending is determined as:

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

where K_s is a state-level constant and x_k is average parental income in district k. In this expression, the parameter β_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 β_s, K_s, and π_s as follows:

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

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

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

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

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

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

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

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

This last expression implies that a higher β_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 β_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 β_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 β_s on IGM of children with different parental incomes.

4 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.[11]

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 a given 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 including approximately 62 percent of all students in the country. These 20 states, shown in Figure AII, are similar to all the other states with respect to a range of characteristics of schools, families, and households (Table AI). The elements of the dataset for each state are described in Table DI, and the various formulas are described in detail in Appendix D.[12]

[11] 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.

[12] 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

Table I (Panel A) summarizes the variation in school revenues across districts within each CZ or state. While the difference in revenues between the highest-income and the lowest-income district is small on average, it ranges from -$6,103 to $12,965 across states in 1990.

School Finance Reforms I compile a list of all state-level school finance reforms passed between 1980 and 2004 and covering the time period when the cohorts at study (born 1980-1986) were in grades 1-12 (i.e., 1986-2004). To do so I combine information from Gold et al. (1992) and Sielke et al. (2001) and from Verstegen and Jordan (2009). These publications describe the funding schemes in place at different points in 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 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.[13] 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), and 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.[14]

I combine these mobility measures with state-specific parental income distributions to construct children's income ranks by CZ, cohort, and parental income quantiles *in the state* (note that

(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). The remaining states did not maintain detailed records on historical school finance data.

[13]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.

[14]Children are assigned to CZs based on when they lived at age 16, irrespective of whether they moved when they entered the labor market.

children's ranks are always expressed relative to the national income distribution). The choice to define observation based on state-specific (rather than CZ-specific or national) quantiles is driven by the focus of my empirical exercise 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. In addition, different states could have very different income distributions; using national quantiles would cause observations in the data set to include different numbers of people across different states.[15]

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 mobility, whereas children with parental income above the median experience downward mobility (Table I, Panel B).[16] Wide differences exist across CZs (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. I complement information on income mobility with data on education mobility, also constructed and provided by Chetty et al. (2014) and defined as the probability of being enrolled in college at age 19 for each CZ, birth cohort (1984-1990), and parents' income quantile in the state.[17]

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).[18] I use information from the 1990 Census to link zip codes to school districts, and I 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

[15]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.

[16]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.

[17]Measures of education mobility are available for cohorts 1984 to 1993. Since school finance data are only available until 2004, however, I restrict my attention to cohorts until 1990 to have information on funding schemes for at least nine school years for each cohort.

[18]The construction of this index is explained in detail in Bogin et al. (2016).

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.

5 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, captured by the parameter β_{st} in the following equation:[19]

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

where e_{dt} is per pupil revenue in district d (located in state s) and year t, x_{dt} is median per capita household income, and ε_{dt} is an error term.

The parameter β_{st}, estimated separately for each state s and year t, represents the degree of inequality in school funding across districts. When the funding scheme is unequal and revenues are higher (lower) in richer (poorer) districts, β_{st} will be positive. When the funding scheme is redistributive and revenues are higher in low-income districts, β_{st} will instead be negative. Lastly, when the funding scheme is equalized and revenues are similar across richer and poorer districts, β_{st} will be 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., β_{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 β_{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 β_{st} over the calendar years in which the cohort was in grades one

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

to twelve.[20] For cohorts born between 1980 and 1986, this requires estimating β_{st} for each state and year between 1986 and 2004. Income data, however, are only available for Census years. To back out median district incomes for intercensal years, I directly exploit the timing of each reform and I impute income values to each district depending on whether the corresponding state had a school finance reform during that decade. If a reform took place, I impute the income of the Census year at the beginning of the decade to the years preceding the reform (including the year of the reform) and the income of the Census year at the end of the decade to the years following the reform. If no reform took place, I assign income values to intercensal years by interpolating between the incomes of the Census years at the beginning and at the end of the decade; I then use the interpolated income values to estimate β.[21] To demonstrate that my results are not driven by this imputation procedure, in robustness checks I use a version of β_{st} estimated assigning the 1990 median district income to all years (Table AVIII).

On average, the parameter β_{st} is equal to 0.011 for states without a school finance reform (with a standard deviation of 0.068); for states with a reform it equals 0.044 in the years before the reform (with a standard deviation of 0.038) and -0.009 in the years after the reform (with a standard deviation of 0.048, Table I, Panel C). Figure I illustrates the change in β_{st} in the years surrounding a reform. The figure shows point estimates and 90 percent confidence intervals of the coefficients δ_k in the following equation:

$$\hat{\beta}_{st} = \sum_{k=-3}^{10} \delta_k R_s \mathbb{1}(t - ryear_s = k) + \varepsilon_{st} \tag{8}$$

where $\hat{\beta}_{st}$ is the estimated β_{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.[22] Estimates of β_{st} decline immediately following a school finance reform and remain stable at this lower level ten years after the reform. Appendix Figure AVI shows estimates of β_{st} separately for "equity" reforms (passed before 1990) and "adequacy" reforms (passed after 1990). The initial drop in β 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.

[20] For example, the β_s for the 1980 cohort is the average of the β_{st} for the years 1986-1997.

[21] If two reforms take place in one decade (which is the case for Montana, New Jersey, New York, and Oregon), I assign the income of the Census year at the beginning of the decade to the years preceding the first reform, the income of the Census year at the end of the decade to the years following the last reform, and I interpolate between these two values for the years between the two reforms.

[22] The estimation includes years 1986 to 2004, and standard errors are clustered at the state level.

6 Addressing The Endogeneity of Post-Reform Revenues

6.1 Explaining The Need for An Instrument

Having estimated a measure β for the inequality in school spending, my next goal is to test the theoretical predictions derived in Section 3 and to estimate the effects of a decline in β on IGM. Doing so requires a source of exogenous variation in β. School finance reforms are a natural candidate, and 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.[23] 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 β (for simplicity I focus only on one r):

$$M^r_{cb} = \delta\beta_{sb} + \theta_c + \tau_b + \tilde{\varepsilon}_{cb} \tag{9}$$

The variable $\tilde{\varepsilon}_{cb}$ is a residual component of mobility, which can include the composition of their group of peers and, more generally, of their community, summarized by $\tilde{X}_{bc}$:

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

To simplify matters I express the variable β_{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 by moving to a different

[23] This is analogous to wanting to estimate a "structural" parameter, whereas Lafortune et al. (2018) estimate the reduced-form effect of equalization reforms.

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 $\tilde{X}_{cb}$.[24]

Due to the inclusion of θ_c, OLS estimates of δ can also be obtained from a first-differenced version of equation (9). 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, giving rise to 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:

$$m_{kt} = \sum_{n=-5}^{5} \delta_n R_{s(k)} \mathbb{1}(t - ryear_{s(k)} = n) + \gamma_k + \tau_t + \varepsilon_{kt} \tag{10}$$

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 γ_k and τ_t are county and year fixed effects, respectively, and ε_{kt} is an error term. Estimates of the coefficients δ_n, shown in Figure 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 to a reform, and they increase by 17 and 19 percent (or 0.13 and 0.14 percentage points) respectively in the years following the reform.

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 (10), 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 the average income of both in-migrants and out-

[24]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).

migrants and the average income of stayers is flat in the years leading to a reform, and 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 provide evidence of significant household sorting 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.[25]

6.2 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.[26] 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 β_{sb+1} as the sum of an exogenous component and an endogenous one:

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

The quantity $X'_{st} g_{t+1}$ is the β_{st+1} that would have resulted had households not sorted (and house prices not changed). This "simulated" version of β_{st+1}, which I denote as β^{sim}_{st+1}, can be used as an instrument to obtain consistent estimates of δ^r in equation (9). 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 β on IGM is negative, a positive correlation implies that OLS will be biased toward zero,

[25] This finding is in partial contrast with Lafortune et al. (2018), who analyze changes in the income gap between *ex ante* richer and poorer districts, as well as changes in the demographic composition of students across districts after each reform, and find no evidence of changes in these variables.

[26] 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.

whereas a negative correlation implies that OLS will overstate the negative effect of β. The sign of this correlation is uncertain *ex ante* and depends on both X_{st} and g_{st+1}.

6.2.1 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).

While 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 $\hat{\beta}^{sim}_{st+1} = \hat{X}'_{st}\hat{g}$.[27] 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 $\hat{\beta}^{sim}_{st+1}$ in lieu of β^{sim}_{st+1} 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.[28]

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 $\hat{\beta}^{sim}_{st}$ than when using β^{sim}_{st}. To see this, consider an endogenous $\beta_{st+1} = X'_{st+1}g_{st+1}$ with a corresponding value of JJP's instrument $\hat{\beta}^{sim}_{st+1} = \hat{X}'_{st}\hat{g}$. Suppose now that all states' formulas change to $g^0_{kt+1}\forall k$, such that the resulting instrument for state s would be $\hat{\beta}'^{sim}_{st+1} = \hat{X}'_{st}\hat{g}^0 \leq \hat{\beta}^{sim}_{st+1}$. Monotonicity requires that $\beta'_{st+1} = X'_{st+1}g^0_{st+1} \leq \beta_{st+1}$ 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 $\beta^{sim}_{st+1} = X'_{st}g_{st+1}$, this assumption would fail only if the endogenous change in X_{st} *alone* were so dramatic to yield a change in β_{st+1} of the opposite sign as the the change in β^{sim}_{st+1},

[27] Note that Jackson et al. (2015) instrument expenditure and not β.

[28] 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.

since the function g^0_{st+1} is the same in β_{st+1} and β^{sim}_{st+1}.[29]

Clearly, the extent to which β^{sim}_{st} will be a better instrument than $\hat{\beta}^{sim}_{st}$ depends on the actual heterogeneity in funding formulas across states (Hoxby, 2001). Figure III shows the trend in β around a reform in five states with reforms between 1989 and 1996. While some reforms were effective in reducing β (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 β from 0.045 to 0.041).[30]

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 (e.g. Michigan) did not trigger any changes, and others (e.g. Massachusetts) were followed by an increase.[31]

Differences in the effectiveness of each reform and in the house price responses across states suggest that the use of β^{sim}_{st} in lieu of $\hat{\beta}^{sim}_{st}$ could improve both the consistency and the efficiency of the estimates. In Section 7 I show that the differences between the estimates obtained using β^{sim}_{st} and those obtained using $\hat{\beta}^{sim}_{st}$ are significant.

Implementation I construct β^{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

[29] 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 β^{sim}_{st+1}, the instruments are the actual parameters of the funding formula entering β_{st+1}, whereas when using $\hat{\beta}^{sim}_{st+1}$ they are not.

[30] 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.

[31] Each point and spike in Figure IV represent the estimate and the 90 percent confidence interval of the coefficients δ_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 θ_d and τ_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.

values, property tax rates, and income) fixed at their pre-reform values.[32] Lastly, I construct β^{sim} for each state and year, re-estimating equation (7) using simulated instead of actual revenues.[33]

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.

Figure V 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 revenues than they would have had house prices not changed, and vice versa (Figure AVII).[34]

On average, the parameter β^{sim} equals 0.044 (with a standard deviation of 0.039) in the years preceding each reform, and it drops to -0.0003 (with a standard deviation of 0.048) in the years after the reform (Table I, Panel C). Estimates from the first stage of the IV estimation reveal that β^{sim} is a strong predictor for β; the Kleibergen-Paap Wald F-statistic of the first stage (Stock and Yogo, 2002), shown in Table III, is around 20. 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 (Table AII).[35]

[32] 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 nation-wide changes in house prices, and I correct for inflation using the CPI.

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

[34] Figure AVII shows the relationship between the percentage change in house prices after a reform and the difference between actual and simulated revenues.

[35] The table shows estimates of a regression of β^{sim}_{st} 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 β^{sim}_{st} over time.

7 Effects of Equalization on Intergenerational Mobility

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

To better illustrate identification, Figure VI 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 β or a decline in β 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. Similarly to JJP, exposure counts the number of school years a cohort was exposed to a "post-reform" regime while in school: Exposure = birth year + 18 – (reform year + 1) (exposure is negative for non-exposed cohorts).[36]

The figure shows that, 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.[37] Importantly, the figure shows the absence of any trends in IGM 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 mobility.

7.1 OLS Estimates

While useful to illustrate the trends in IGM across cohorts and states, the evidence in Figure VI is based on an arbitrary definition of the effectiveness of a reform; in addition, it only informs us on the mobility of children whose parents are at the bottom of the income distribution. To more rigorously test the effect of a change in β on IGM and to explore its effects on children with

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

[37] The figure shows OLS points estimates and 90 percent confidence intervals of the coefficients δ_n in the equation $m_{cb} = \sum_{n=-6}^{12} \delta_n E_{n(sb)} + \theta_c + \tau_b + \varepsilon_{cb}$, where m_{cb} is the mean rank of children in CZ c, cohort b, and with parents' income in the 25th percentile in the national income distribution, E_n equals one if cohort b in state s was exposed to a post-school finance reform regime for n years ($E_n = b + 18 - (ryear_s + 1)$, and $ryear_s$ is the year of the first school finance reform in state s between 1980 and 2004), and the vectors θ_c and τ_b contain CZ and cohort fixed effects. The coefficients are estimated separately for states with effective and ineffective reforms, using states with no reform as a control group. Observations are weighted by the number of children in each CZ and cohort. Standard errors are clustered at the state and birth cohort level.

different parental incomes, I estimate the following equation:

$$M_{cbx} = \delta_0 \hat{\beta}_{s(c)b} + \delta \hat{\beta}_{s(c)b} * \theta_{n(cx)} + \kappa_c + \tau_b + \theta_{n(cx)} + \omega_{cbx} \tag{11}$$

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 quantile (either the 10th, 25th, 50th, 75th, 90th, or 99th percentile).[38] The variable $\hat{\beta}_{s(c)b}$ is the estimated state and cohort-specific measure of equalization described in the previous section ($s(c)$ denotes the state where CZ c is located). CZ fixed effects κ_c control for CZ-specific, time-invariant determinants of mobility, and cohort fixed effects τ_b control for secular trends in mobility. 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.[39] The variable ω_{cbx} is an error term.

In this model the parameter δ_0 captures the effect of an increase in β, i.e., a *decline* in equalization, on the income percentile of children with the lowest-ranked parental income in the national distribution. The parameter δ 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 to allow for spatial correlation in mobility. For ease of interpretation, I describe my estimates in terms of a *reduction* in β, i.e., an increase in equalization.

OLS estimates of equation (11) are shown in Table II. A one-standard deviation (SD) reduction in β, equivalent to a \$4,500 reduction in the gap in per pupil revenues between the richest and the poorer districts in a state, is associated with a 4.1 percentile increase in mobility of children with parental income at the bottom of the income distribution (estimate of β equal to -4.127, Table II, column 1, significant at 10 percent). An estimate of δ equal to 0.0256 indicates that this positive association is reduced by 0.026 percentiles with each additional percentile of parental income (estimate of $\beta\times$ *parent centile*, Table II, column 1, significant at 1 percent). This implies that the same reduction in β is associated with a 3.9 percentile increase in mobility for children with parental income in the 10th percentile (p-value equal to 0.03), a 3.5 percentile increase for children with parental income in the 25th percentile (p-value equal to 0.05), and a smaller 1.8 percentile

[38] One observation corresponds to a birth cohort, CZ, and percentile of parental income in the CZ.

[39] 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.

increase for children with parental income in the 90th percentile (p-value equal to 0.31). These estimates are robust to controlling for state fixed effects (column 2).

7.2 Two-Stages Least Squares Estimates

Household sorting after each school finance reform directly affects both β and IGM: OLS estimates will therefore be inconsistent. To address this issue, in Table IV I re-estimate the specifications in Table II via 2SLS, using $\hat{\beta}^{sim}_{s(c)b}$ (as defined in Section 6) and $\hat{\beta}^{sim}_{s(c)b} \times \theta_{n(xc)}$ as instruments for $\hat{\beta}_{s(c)b}$ and $\hat{\beta}_{s(c)b} \times \theta_{n(xc)}$. Estimates of the first-stage equations, shown in Table III, indicate that $\hat{\beta}^{sim}_{s(c)b}$ is a strong instrument for $\hat{\beta}_{s(c)b}$: the Kleibergen-Paap F-statistics are all close to 20.[40]

2SLS estimates confirm the positive relationship between equalization and mobility, but yield larger effects. Controlling for CZ fixed effects, a one-SD reduction in β leads to a 5.9 percentile increase in mobility for children with parental income at the bottom of the national distribution (estimate of β equal to -5.904, Table IV, column 1, significant at 5 percent). A positive estimate for δ ($\beta\times$ *parent centile*, significant at 1 percent) indicates that this effect decreases by 0.025 percentiles with each additional percentile of parental income. These estimates translate into a 5.7 and 5.3 percentile increase for children with parental income in the 10th and 25th percentiles (p-values 0.02 and 0.03) respectively, but only an insignificant 3.6 percentile increase for those with parents in the 90th percentile (p-value 0.13). These results also indicate that the average reform, which decreases β by approximately 0.64 SD, would increase mobility of children from families in the 10th percentile by 3.6 percentiles and close 13 percent of the gap between the lowest-mobility CZ (Clarksdale, MS) and the highest-mobility CZ (Sioux Center, IA).

As explained by Hoxby (2001), different reforms had different effects on overall school spending in each state: some ended up increasing it, some reduced it. These changes could have a direct effect on IGM, above and beyond the change in spending inequality captured by β (this effect can be seen in equation (6) through the variable K_s). To account for these changes, in columns 3 and 4 of Table IV I control for average per pupil expenditure in state s on cohort c (e_{sb}). These estimates reveal that an increase in e_{sb} has a positive, yet small and statistically insignificant effect on mobility. Importantly, the estimates of δ_0 and δ are robust to controlling for e_{sb}. These estimates highlight the importance of studying changes in revenue equalization, as opposed to levels, when the outcome of interest is children's IGM.

Estimates are slightly smaller when controlling for state fixed effects (Table IV, column 2). Im-

[40] This value is above the critical threshold of 7.03 proposed by Stock and Yogo (2002) for a test with two endogenous variables, two instruments, and a test size equal to 0.10.

portantly, in all these specifications 2SLS estimates are approximately 50 percent larger than OLS. This indicates that failure to account for the endogeneity of β would lead to severely underestimating the effects of equalization.

In Figure VII (dashed line) I estimate the effects of a decline in β separately for various quantiles of parental income in the national distribution. A one-SD reduction in β leads to a 5.7 percentile increase in mobility for children with parental income in the first quartile (significant at 5 percent), but only 3.3 percentiles for children with parental income in the top one percent of the distribution (p-value equal to 0.15). Again, OLS estimates are smaller in magnitude (solid line).

Effects on Income To quantify the magnitude of these effects in monetary terms, I use the national distribution of children's income to map IGM measures by CZ, cohort, and parental income quantile to income levels, and I use the logarithm of children's income as the dependent variable in equation (11). 2SLS estimates indicate that a one-SD reduction in β leads to a 16.4 percent increase in income for children with parental income in the 10th percentile (p-value 0.017), a 15.2 percent increase for children with parental income in the 25th percentile (p-value equal to 0.025), and a smaller and insignificant 10 percent increase for children with parental income in the 90th percentile (p-value equal to 0.13, with an estimate of β equal to -0.1593 and of $\beta \times$ *parent centile* equal to 0.0007, Table V, column 3, significant at 5 and 1 percent respectively). Estimates are robust to controlling for state fixed effects (Table V, column 4).

Reduced-Form Estimates While useful to capture the causal effects of equalization on mobility, 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 β, but only on β^{sim} though changes in the formula type and parameters. In columns 5 and 6 of Table IV I estimate the "reduced-form" effect of β^{sim} on IGM. These estimates indicate that a one-SD decline in β^{sim} leads to a 4.6 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 mobility.

Estimates Using Jackson, Johnson, and Persico's (2015) IV Approaches In Table AIV I re-estimate equation (11) instrumenting β 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; I explain the procedure in more depth in Appendix C).[41] These estimates reveal smaller and impre-

[41] The first stage estimates are shown in Table AIII.

cise effects of equalization on IGM. The significant differences with my 2SLS estimates indicate that failing to account for heterogeneity in funding formulas across states could generate biased estimates of the effects of equalization.

7.2.1 Heterogeneous Effects of Equalization by School Grade

The effects of school finance equalization could differ depending on whether equalization happens earlier or later during a child's education path. On one hand, education investments made at earlier ages have been shown to yield higher returns (see Cunha and Heckman, 2010, for a review). On the other hand, equalization could be beneficial during high school if it facilitates the transition to college for low-income children, as college attendance is an important engine for mobility (Rothstein, 2019).

To test for this heterogeneity I allow the effects of a decline in β to differ for cohorts which experienced a reform during elementary, middle, and high school.[42] I augment (11) as follows:

$$\begin{aligned} M_{cbx} \quad = \quad & \delta_0^e \text{El}_{s(c)b} * \hat{\beta}_{s(c)b} + \delta^e \text{El}_{s(c)b} * \hat{\beta}_{s(c)b} * \theta_{n(xc)} \\ & + \eta^m \text{Mid}_{s(c)b} + \delta_0^m \text{Mid}_{s(c)b} * \hat{\beta}_{s(c)b} + \delta^m \text{Mid}_{s(c)b} * \hat{\beta}_{s(c)b} * \theta_{n(xc)} \\ & + \eta^{hs} \text{HS}_{s(c)b} + \delta_0^{hs} \text{HS}_{s(c)b} * \hat{\beta}_{s(c)b} + \delta^{hs} \text{HS}_{s(c)b} * \hat{\beta}_{s(c)b} * \theta_{n(xc)} + \kappa_c + \tau_b + \theta_{n(xc)} + \omega_{cbx} \end{aligned} \tag{12}$$

where $\text{El}_{s(c)b}$, $\text{Mid}_{s(c)b}$, 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 δ_0^e, δ_0^m, and δ_0^{hs} represent the effect of a one-SD increase in β on the income percentile of children with the lowest-ranked parental income, for cohorts in states where a reform hit during elementary, middle, and high school, respectively, and relative to cohorts and states without a reform. The parameters δ^e, δ^m, and δ^{hs} measure instead how much these effects vary as parents' income ranks increase.

2SLS estimates of equation (12) indicate that a decline in β is most effective when the reform hits during elementary school, relative to middle and high school. Controlling for CZ fixed effects, a one-SD reduction in β leads to an additional 8.7 percentile increase in the income rank of children with parents at the bottom of the income distribution for cohorts hit by a reform during elementary school, relative to those with no reform (with an estimate of $\beta\times$ *reform in elementary*

[42] 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 β, the older one will be exposed to a lower average β. Two cohorts in two different states, however, could be exposed to the same average β but experience a reform at different points in the 12 years.

school equal to -8.733, Table VI, column 3, significant at 1 percent). This effect declines by 0.11 percentiles with each additional percentile of parents' income (estimate of $\beta \times$ *parent centile*$\times$ *reform in elementary school* equal to 0.107, Table VI, column 3, significant at 1 percent). These estimates imply that, when a reform hits during elementary school, a reduction in β leads to an additional 7.7 and 6.1 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 β leads to a smaller 3.6 and 3.2 percentile increase in the income rank of children with parental income in the 10th and 25th income percentile if the reform hits during high school (with an estimate of $\beta\times$ *reform in high school* equal to -3.785 and of $\beta \times$ *parent centile*$\times$ *reform in high school* equal to 0.023, Table VI, column 3). The effect is indistinguishable from zero for children with parents on the 90th percentile. All these estimates are similar when the reform hits during middle school, and they are robust to controlling for state fixed effects (Table VI, column 4). OLS estimates, shown in columns 1 and 3 of Table VI, 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.

7.2.2 Equalization and Competition Across Districts

The theoretical framework in Section 3 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 π_s). I test this prediction by re-estimating equation (11) 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 Table VII, indicate that a decline in β has a much larger effect in states with more competition across school districts. Controlling for CZ fixed effects, a one-SD decline in β in "High competition" states leads to a 11.0, 10.6, and 9.2 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -11.200 and of $\beta\times$ *parent centile* equal to 0.022, Table VII, column 1, significant at 5 and 1 percent respectively). The effect is smaller in "Low competition" states: The same decline in β

leads to a 5.7 and 4.4 percentile increase for children with parents in the 10th and 25th percentile, respectively, and to an insignificant 1.3 percentile decline for children with parents on the 90th percentile (with an estimate of β equal to -6.519 and of $\beta\times$ *parent centile* equal to 0.087, Table VII, column 3, significant at 1 percent). These estimates confirm the prediction of Section 3 and suggest a role for district competition in explaining the effects of revenue equalization on IGM.

7.2.3 Equalization and Income Inequality

The results presented so far indicate that a decline in β has a positive effect on IGM, especially for children from low-income families. Equalization in school spending closes the gap in the public investments in education for low- and high-income students, which in turn closes the gaps in later-life outcomes.

These estimates could, however, 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 β equal to 0.23.[43] 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 β 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 (11) 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.[44] These estimates, shown in Table VIII, indicate that a decline in β has a smaller effect in CZs with income differences in the bottom 50 percent of the cross-CZ distribution ("Low inequality," columns 1 and 2) relative to CZs in the top 50 percent ("High inequality," columns 3 and 4). Controlling for CZ fixed effects, a one-SD decline in β in "Low inequality" CZs leads to a 4.4, 4.0, and 2.2 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -4.668 and of $\beta\times$ *parent centile* equal to 0.0274, Table VIII, column 1). These effects are larger in "High inequality" CZs: The same decline

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

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

in β leads to a 6.4, 6.1, and 4.6 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -6.594 and of $\beta\times$ *parent centile* equal to 0.022, Table VIII, column 3, significant at 10 and 1 percent respectively).[45]

7.2.4 Equalization and Income Segregation

The effects of a decline in β could also vary according to 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 (11) 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.[46] Estimates of δ_0 and δ indicate that, controlling for CZ fixed effects, a one-SD decline in β in "Low segregation" CZs leads to a 5.4, 5.0, and 2.9 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -5.740 and of $\beta\times$ *parent centile* equal to 0.031, Table VIII, column 1, significant at 5 and 1 percent). Equalization is more effective in CZs with high income segregation: The same decline in β leads to a 6.0, 5.7, and 4.1 percentile increase for children with parents in the 10th, 25th, and 90th percentiles, respectively (with an estimate of β equal to -6.246 and of $\beta\times$ *parent centile* equal to 0.023, Table IX, column 3, significant at 5 and 1 percent, respectively).[47]

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.

7.3 Robustness

Estimating β Without Income Interpolation The above estimates are obtained imputing income for intercensal years, using the procedure outlined in Section 5. To check that results are not dependent on this imputation procedure, in Table AVIII I re-estimate the main specifications with a version of β estimated using income data from 1990 for all years. These estimates are similar to those in Table IV, indicating that the main results are not driven by this imputation procedure.

[45] OLS estimates are shown in Table AVI.

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

[47] OLS estimates are shown in Table AVII.

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).[48] The same decline in β might have different effects in one-state and multi-state CZs. On one hand, if sorting across state borders is more costly than within states, the endogeneity problem might be more pressing in one-state CZs. On the other hand, a decline in β 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. Table AIX shows 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 Appendix Tables AX and AXI shows estimates based on 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 IV, which suggests that the main results are not driven by this normalization choice.

8 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.

8.1 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

[48] In the baseline estimates I follow Chetty et al. (2014) and assign each of these CZs the state with the largest population share.

shortages.[49]

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

$$TS_{dt} = \delta_1 \hat{\beta}_{s(d)t} q_{dt}^{1q} + \delta_2 \hat{\beta}_{s(d)t} q_{dt}^{2q} + \delta_3 \hat{\beta}_{s(d)t} q_{dt}^{3q} + \delta_4 \hat{\beta}_{s(d)t} q_{dt}^{4q} + \gamma_d + \tau_t + \varepsilon_{dt} \tag{13}$$

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 γ_d and τ_{st} control for district and year fixed effects. The parameters δ_1, δ_2, δ_3, and δ_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 X shows OLS and 2SLS estimates of equation (13). OLS results indicate a positive relationship between equalization and the number of teachers per student in low-income districts and a negative (but imprecise) relationship in high-income ones (Table X, column 2). 2SLS estimates, shown in columns 3 and 4, yield instead larger and marginally significant positive effects on low-income districts, but no effect on high-income ones. Controlling for district fixed effects, a one-SD reduction in β leads to 0.0076 additional teachers per student in districts in the bottom quartile, or 11 percent more (Table X, column 4, significant at 5 percent). The same estimate is 0.0017 for districts in the top quartile and it is indistinguishable from zero (Table X, column 3, p-value equal to 0.68).

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.

8.2 Intermediate Outcome: College Enrollment

Rothstein (2019) and Chetty et al. (2017) suggest a positive association between college enrollment and IGM. To explore the role of college enrollment as one of the channels behind my main estimates, I study the effect of equalization on education mobility. To do this, I re-estimate equation (11) using the probability of college enrollment at age 19 (expressed in percentage points and measured separately for each CZ, cohort, and parent quantile within the state) as the dependent

[49] From an analysis of the Center on Budget and Policy Priorities using data from the Bureau of Labor Statistics.

variable.

Controlling for CZ fixed effects, 2SLS estimates indicate that a one-SD reduction in β leads to a 7.8 percentage point increase in the probability of college enrollment for children from families at the bottom of the income distribution, although this estimate is imprecise (estimate of β equal to -0.0782, Table XI, column 1, p-value equal to 0.40). Compared with an average probability of 59.3 percent, this implies a 13 percent increase. This effect is reduced by 0.02 percentage points for each additional percentile of parental income (estimate of $\beta\times$ *parent centile*, Table XI, column 1, significant at 5 percent). These estimates imply that the same reduction in β leads to a 7.6 and 7.3 percentage point increase in the probability of college enrollment for children with parents in the 10th and 25th percentile, and a 6.1 percentage point increase for children with parents in the 90th percentile. Estimates of the heterogeneous effects of a decline in β by timing of the reform indicate that the effects of equalization on college enrollment are similar when experienced during elementary, middle, and high school (Table XI, column 4).

While these estimates should be interpreted with caution because they are imprecise, they suggest that access to college is one of the channels through which school finance equalization improves low-income children's long-run outcomes.

9 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) and in intermediate outcomes (such as college enrollment) 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 approximately 50 percent 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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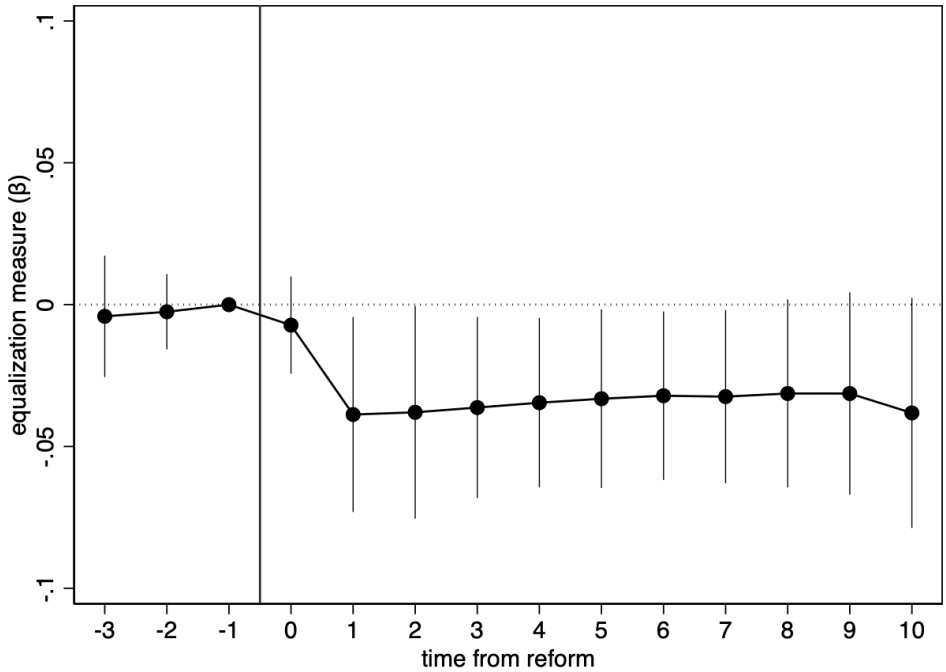
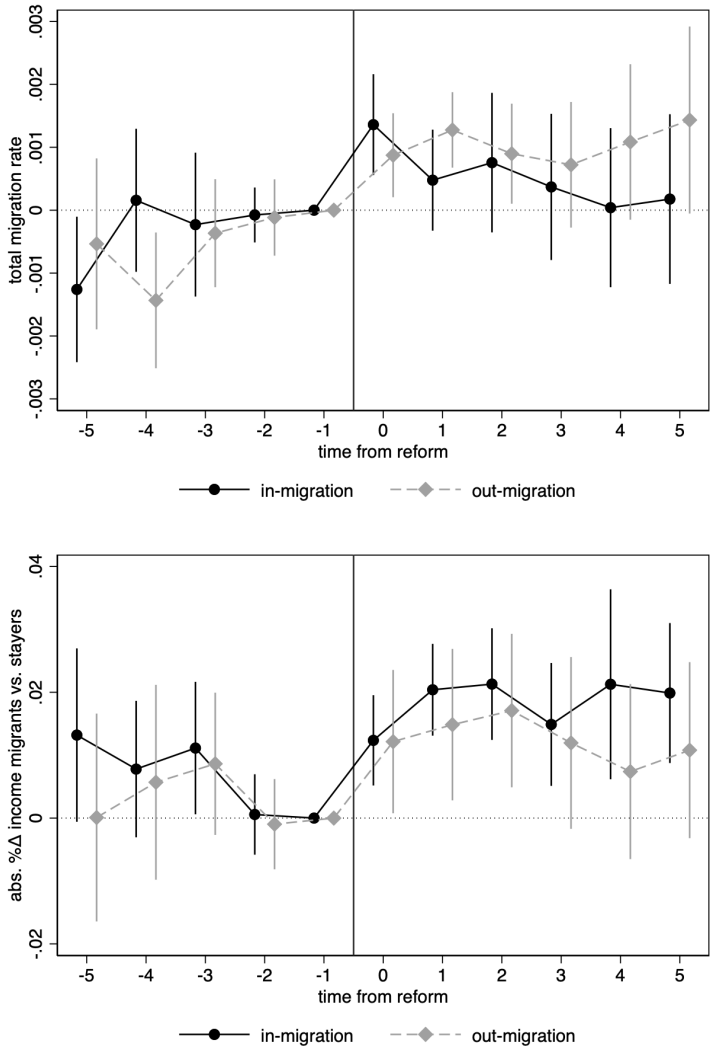
Figure I: Event Study of Equalization Measure β Around A School Finance Reform



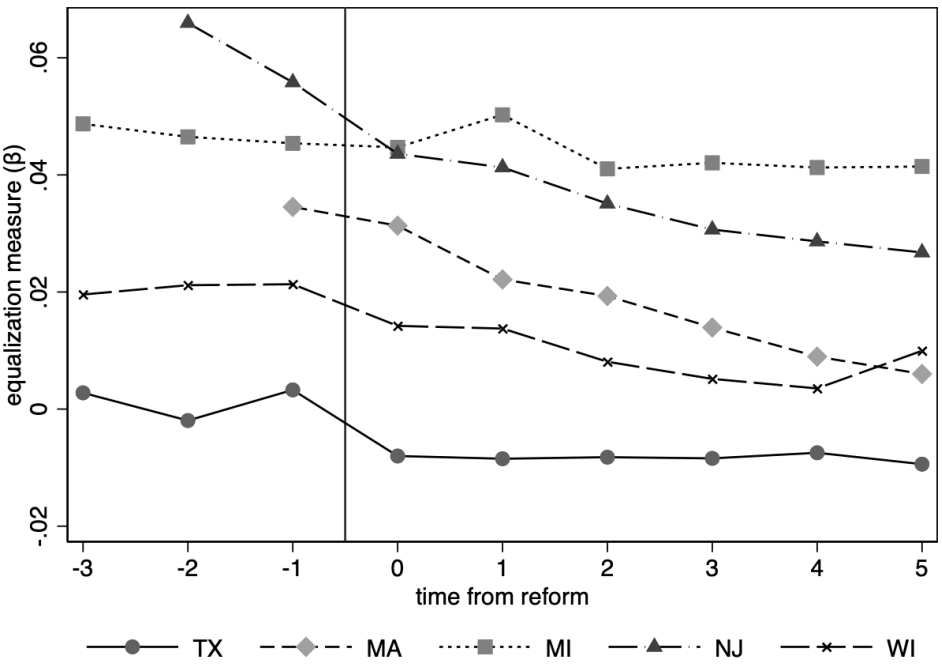
Note: Point estimates and 90 percent confidence intervals for the coefficients δ_k in regression $\beta_{st} = \sum_k \delta_k R_s 1(t - ryear_s = k) + \varepsilon_{st}$, where β_{st} is the slope coefficient in equation (7), estimated separately for each state s and year t from 1986 to 2004, R_s equals 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 δ_{-1} 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



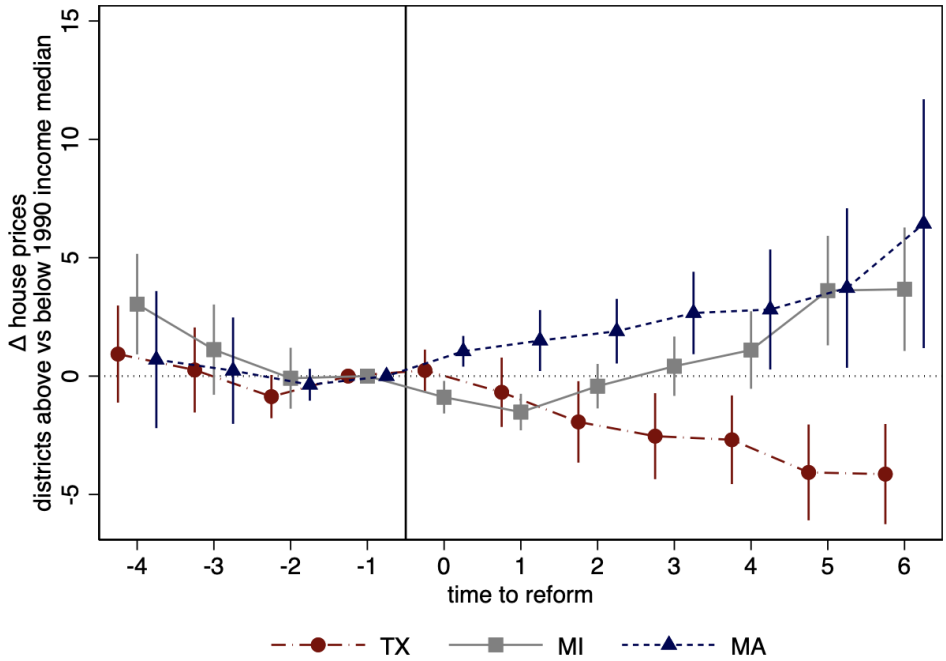
Note: Changes in total migration rates and incomes of migrants in a 10-year window around each school finance reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients δ_n in the regression $y_{kt} = \sum_{n=-5}^{5} \delta_n R_{s(k)} 1(t - ryear_k = n) + \gamma_k + \tau_t + \varepsilon_{kt}$, 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, γ_k are county fixed effects, and τ_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 β Around A School Finance Reform - Selected States



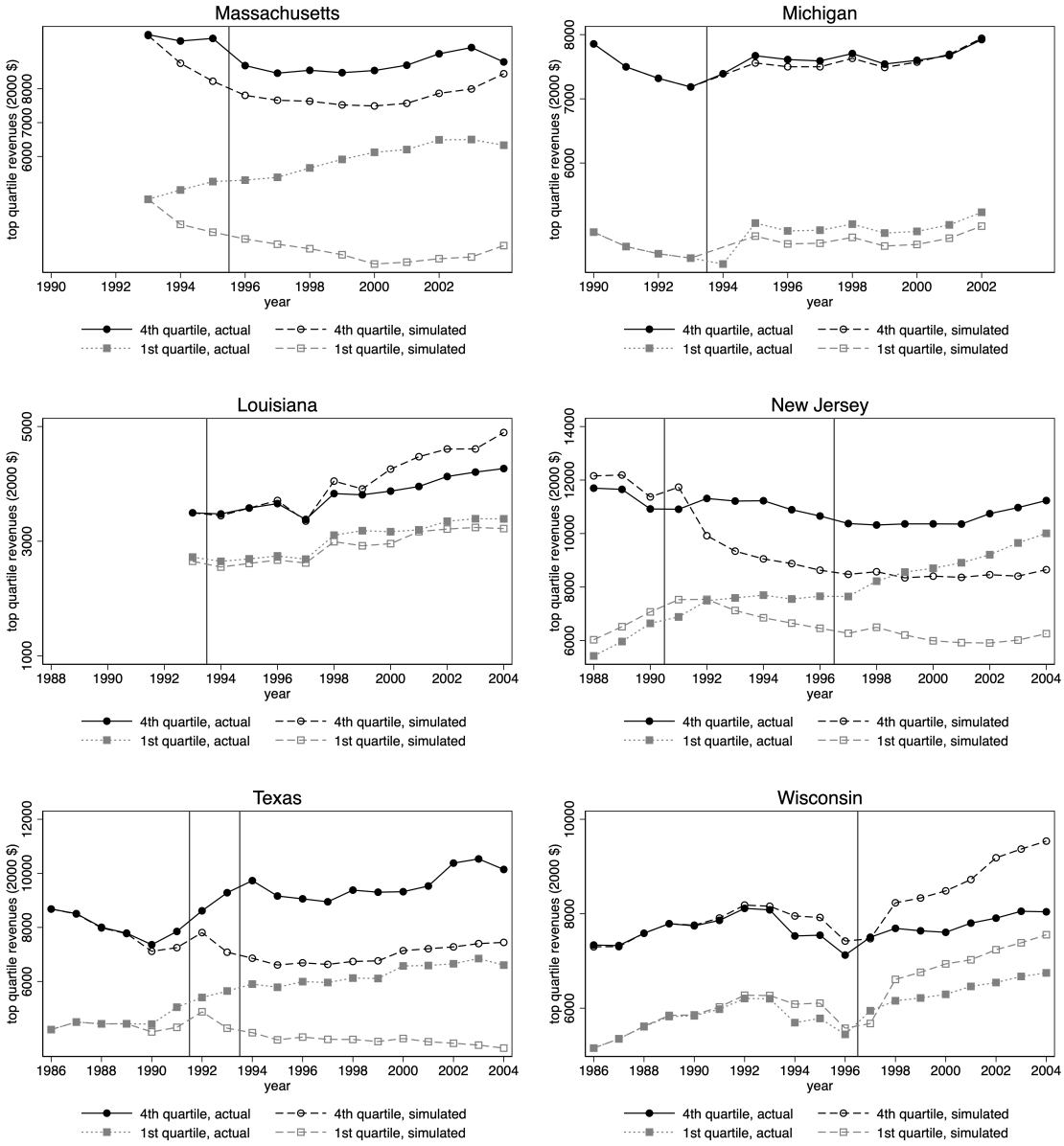
Note: The figure shows estimates of the coefficient β_{st} (defined in equation (7)) for a sample of states in the years surrounding each school finance reform.

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



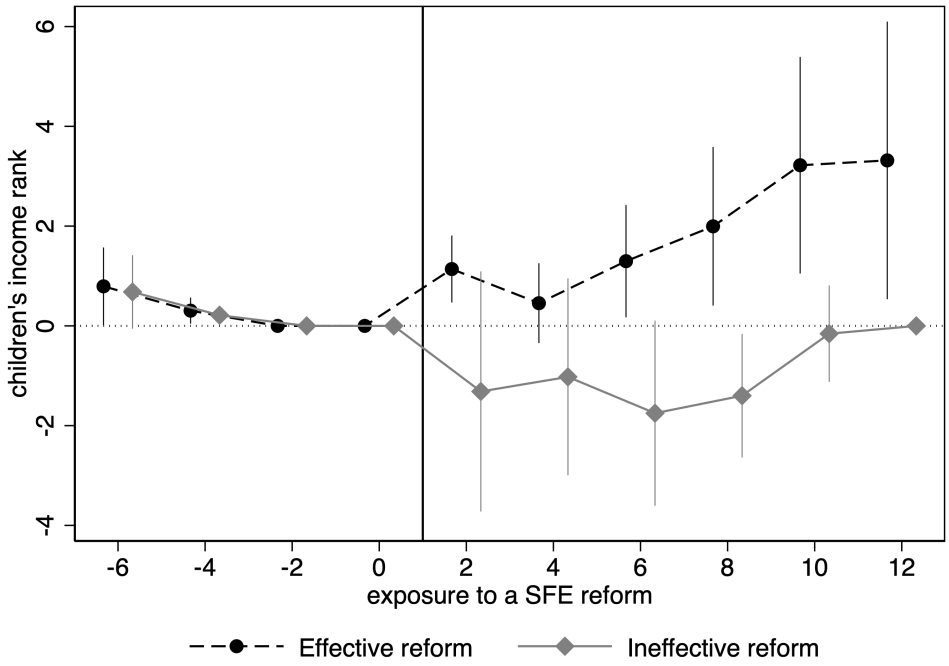
Note: Changes in the difference in house prices between households with incomes above and below the median in 1990, in a 10-year window around each reform and relative to the year before the reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients δ_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 θ_d and τ_t are district and year fixed effects. The coefficient δ_{-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: Simulated and Actual Revenues, Districts In The Top And Bottom Quartiles of Expenditure - Selected States



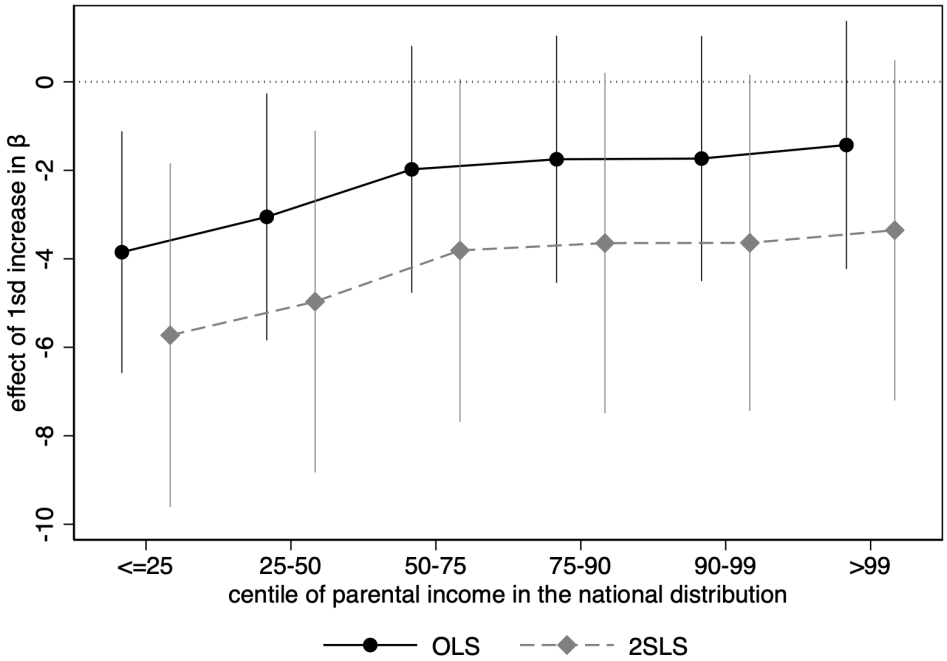
Note: Trends in average simulated and actual per pupil revenues, for districts in the top and bottom quartiles of the state's distribution of per pupil expenditure at the beginning of each sample. Vertical lines denote reform years. Simulated expenditures are calculated using the funding formula in place in every state and year and pre-reform district variables, using the procedure described in the text.

Figure VI: 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 δ_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 θ_c and τ_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 β or a decline in β 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 δ_0 is normalized to equal zero for both groups. Standard errors are clustered at the state and cohort level.

Figure VII: Effect of an Increase in β, by Parents' Income Quantile



Note: OLS (solid line) and 2SLS (dashed line) estimates and 90-percent confidence intervals for the coefficients δ_d in the regression $M_{cxb} = \sum_d \delta_d D_{d(cx)} \hat{\beta}_{s(c)b} + \kappa_c + \theta_{n(xc)} + \sigma_b + \omega_{cxb}$, where M_{cxb} is the average national income percentile of children with parents on the x quantile of the state income distribution, born in cohort b in CZ c, $\hat{\beta}_{s(c)b}$ is the estimated, cohort-specific measure of school finance equalization, $D_{d(cx)}$ equals 1 if the income of the parents of children in cohort c and quantile x falls in decile d of the national distribution, $\theta_{n(xc)}$ are fixed effects for the parent percentile in the national income distribution, κ_c are CZ fixed effects, and σ_b are cohort fixed effects. Standard errors are clustered at the state and birth cohort 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.

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

Panel A: Per Pupil Revenues and Income

	mean	sd	median	min	max	N
Median income						
1980	36417	11041	33961	18286	67924	7578
1990	46552	17916	41249	18149	115499	7621
2000	44018	15891	37500	17500	87500	7936
2010	42974	16444	46250	14800	92500	7942
Δ *exp, richest vs poorest district within state ($)*						
1986	1484	6516	-1814	-2840	14162	2788
1990	3131	4455	1614	-6103	12965	4895
2000	2346	5586	635	-7519	15415	7146
2004	1390	7385	784	-11020	18120	6653
Δ *exp, richest vs poorest district within CZ ($)*						
1986	1072	4337	249	-13816	13890	2788
1990	1210	3807	569	-11045	14518	4895
2000	261	5208	92	-18060	19620	7146
2004	97	5704	-482	-18327	22584	6653

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

	10th	25th	75th	90th
1980-82	0.394	0.435	0.569	0.609
	(0.040)	(0.033)	(0.024)	(0.028)
1983-86	0.398	0.437	0.567	0.607
	(0.034)	(0.030)	(0.031)	(0.036)
N (CZs)	589	589	589	589

Panel C: Measures of School Finance Equalization (β)

	All	No reform	Pre-Reform	Post-Reform	Difference
β	0.011	0.028	0.044	-0.009	-0.053***
	(0.068)	(0.091)	(0.038)	(0.048)	(0.008)
β_s	0.016	0.028	0.044	-0.000	-0.045***
	(0.067)	(0.091)	(0.039)	(0.048)	(0.008)

Note: Panel A: Summary statistics of income and per-pupil revenues (measured in 2000 dollars), and difference in per-pupil revenues between the highest-income district and the lowest-income district within each state and CZ. Panel B: Means and standard deviations of CZ-cohort level intergenerational mobility measures for cohorts 1980 to 1986, published as part of the Opportunity Insights Project (https://opportunityinsights.org/). Panel C: means and standard deviations of the slope coefficient in equation (7), estimated separately for each state and year using actual revenues (β) and simulated revenues (β^{sim}). In Panels A and C, the sample is restricted to school districts and CZs in 20 states with available data to construct the simulated instruments.

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

	(1)	(2)	(3)	(4)
β	-4.1270**	-4.0240**	-4.1012**	-3.9921**
	(1.7924)	(1.8121)	(1.8123)	(1.9279)
$\beta\times$ parent centile	0.0256***	0.0256***	0.0256***	0.0256***
	(0.0020)	(0.0022)	(0.0021)	(0.0022)
e_s			0.0873	0.1081
			(0.6396)	(0.6063)
Parent centile FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
CZ FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
N (CZ $\times$ parent cent. $\times$ cohort)	13578	13578	13578	13578
10th	3.871	3.768	3.845	3.736
10th [p-value]	0.031	0.037	0.034	0.053
25th	3.487	3.384	3.461	3.352
25th [p-value]	0.051	0.061	0.055	0.082
90th	1.822	1.719	1.796	1.687
90th [p-value]	0.307	0.337	0.318	0.383

Note: The table shows OLS estimates of the parameters δ_0 and δ 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 β 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 centile* is the percentile of parents in the national income distribution. The variable e_s is average per-pupil expenditure for each state and cohort. 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table III: School Finance Equalization and Intergenerational Mobility. 2SLS, First Stage

	β	$\beta\times$ parent centile	β	$\beta\times$ parent centile
	(1)	(2)	(3)	(4)
β simulated	0.7529***	-14.2380***	0.7528***	-14.2443***
	(0.0768)	(5.0147)	(0.0734)	(4.3563)
β simulated $\times$ parent centile	-0.0000	0.9846***	-0.0000	0.9846***
	(0.0000)	(0.0062)	(0.0000)	(0.0062)
Parent centile FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	No	No
State FE	No	No	Yes	Yes
Kleibergen-Paap Wald F-stat	19.61		19.64	
N (CZ $\times$ parent cent. $\times$ cohort)	13578	13578	13578	13578

Note: The table shows the first stage of the 2SLS estimation of the parameters δ_0 and δ in equation (11). The variable β 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 centile* is the percentile of parents in the national income distribution. The variable β *simulated* is estimated as β using simulated revenues instead of actual revenues. In this first stage, the variables β *simulated* and β *simulated* $\times$ *parent centile* are used as instruments for β and $\beta\times$ *parent centile*. All specifications include parent percentile and cohort fixed effects; columns 1 and 2 include CZ fixed effects, and columns 3 and 4 include state fixed effects. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort 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. *** p<0.01, ** p<0.05, * p<0.1.

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

	(1)	(2)	(3)	(4)	(5)	(6)
β	-5.9035**	-5.8499**	-5.8890***	-5.8163***		
	(2.4206)	(2.3721)	(2.2269)	(2.1304)		
$\beta\times$ parent centile	0.0253***	0.0253***	0.0253***	0.0253***		
	(0.0020)	(0.0022)	(0.0021)	(0.0021)		
e_s			0.0148	0.0343		
			(0.6542)	(0.6280)		
β simulated					-4.8049***	-4.7642***
					(1.5979)	(1.6740)
β simulated $\times$ parent centile					0.0249***	0.0249***
					(0.0020)	(0.0023)
Parent centile FE	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes
CZ FE	Yes	No	Yes	No	Yes	No
State FE	No	Yes	No	Yes	No	Yes
N (CZ $\times$ parent cent. $\times$ cohort)	13578	13578	13578	13578	13578	13578
10th	5.651	5.597	5.636	5.563	4.556	4.515
10th [p-value]	0.019	0.018	0.011	0.009	0.004	0.007
25th	5.271	5.217	5.257	5.184	4.182	4.141
25th [p-value]	0.029	0.027	0.018	0.015	0.008	0.013
90th	3.627	3.573	3.612	3.540	2.563	2.522
90th [p-value]	0.129	0.126	0.100	0.093	0.103	0.124

Note: The table shows 2SLS second-stage estimates of the parameters δ_0 and δ 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 β 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 centile* is the percentile of parents in the national income distribution. The variable e_s is average per-pupil expenditure for each state and cohort. The variables β and $\beta\times$ *parent centile* are instrumented using β *simulated* and β *simulated* $\times$ *parent centile*; the variable β *simulated* is estimated as β using simulated revenues instead of actual revenues. All specifications include parent percentile and cohort fixed effects; columns 1, 3, and 5 include CZ fixed effects, and columns 2, 4, and 6 include state fixed effects. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort 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. *** p<0.01, ** p<0.05, * p<0.1.

Table V: School Finance Equalization and Intergenerational Mobility. OLS and 2SLS, Dependent Variable is Children's log(Income)

	OLS		2SLS, Second stage	
	(1)	(2)	(3)	(4)
β	-0.1104**	-0.1079**	-0.1593**	-0.1579***
	(0.0434)	(0.0514)	(0.0638)	(0.0574)
$\beta\times$ parent centile	0.0007***	0.0007***	0.0007***	0.0007***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Parent centile FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
CZ FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
N (CZ $\times$ parent cent. $\times$ cohort)	13578	13578	13578	13578
10th	0.109	0.106	0.164	0.163
10th [p-value]	0.017	0.050	0.017	0.008
25th	0.097	0.094	0.152	0.151
25th [p-value]	0.031	0.078	0.025	0.014
90th	0.047	0.045	0.100	0.099
90th [p-value]	0.272	0.390	0.127	0.094

Note: The table shows OLS (columns 1 and 2) and 2SLS second-stage estimates (columns 3 and 4) of the parameters δ_0 and δ in equation (11). 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 β 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 centile* is the percentile of parents in the national income distribution. In columns 3 and 4, the variables β and $\beta\times$ *parent centile* are instrumented using β *simulated* and β *simulated* $\times$ *parent centile*; the variable β *simulated* is estimated as β 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table VI: Heterogeneous Effects of School Finance Equalization Across School Grades. OLS and 2SLS, Dependent Variable is Children's Income Percentile

	OLS		2SLS	
	(1)	(2)	(3)	(4)
$\beta\times$ reform in elementary school	-4.4631**	-4.2367*	-8.7333***	-8.6311*
	(2.0137)	(2.2228)	(0.9291)	(3.7392)
$\beta\times$ parent centile $\times$ reform in elementary school	0.0866***	0.0867***	0.1069***	0.1070***
	(0.0154)	(0.0170)	(0.0041)	(0.0240)
$\beta\times$ reform in middle school	-1.5813	-1.4562	-3.9573***	-3.9077
	(1.7978)	(1.9235)	(0.7984)	(2.9054)
$\beta\times$ parent centile $\times$ reform in middle school	0.0293***	0.0293***	0.0275***	0.0275***
	(0.0053)	(0.0047)	(0.0021)	(0.0045)
$\beta\times$ reform in high school	-1.5393	-1.4367	-3.7847***	-3.7592
	(1.7468)	(1.8755)	(0.7731)	(3.0245)
$\beta\times$ parent centile $\times$ reform in high school	0.0244***	0.0244***	0.0225***	0.0225**
	(0.0063)	(0.0066)	(0.0016)	(0.0089)
Parent centile 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 (CZ $\times$ parent cent. $\times$ cohort)	13578	13578	13578	13578
10th, elem	3.597	3.369	7.665	7.561
25th, elem	2.298	2.068	6.062	5.957
90th, elem	-3.331	-3.570	-0.883	-0.996
10th, high	1.296	1.193	3.560	3.535
25th, high	0.930	0.828	3.223	3.198
90th, high	-0.654	-0.756	1.763	1.738

Note: The table shows OLS (columns 1 and 2) and 2SLS second-stage estimates (columns 3 and 4) of the parameters in equation (12). 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 β 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 centile* is the percentile of parents in the national income distribution. In columns 3 and 4, the variable β is instrumented with β *simulated*, estimated as β using simulated revenues instead of actual revenues. The variables *reform in elementary school*, *reform in middle school*, and *reform in high school* equal one for cohorts and states for which a reform hit during elementary, middle, and high school, respectively. 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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table VII: Heterogeneous Effects of School Finance Equalization by Competition Among Districts. 2SLS, Dependent Variable is Children's Income Percentile

	High Competition		Low Competition	
	(1)	(2)	(3)	(4)
β	-11.1998*	-11.0473	-6.5190***	-6.5190***
	(6.5759)	(8.1185)	(2.0356)	(2.1545)
$\beta\times$ parent centile	0.0223***	0.0223***	0.0867***	0.0867***
	(0.0019)	(0.0018)	(0.0159)	(0.0169)
Parent centile FE	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes
CZ FE	Yes	No	Yes	No
Cohort FE	Yes	Yes	Yes	Yes
N (CZ $\times$ parent cent. $\times$ cohort)	8790	8790	4788	4788
10th	10.977	10.825	5.652	5.652
10th [p-value]	0.095	0.183	0.005	0.008
25th	10.643	10.491	4.352	4.352
25th [p-value]	0.106	0.197	0.026	0.039
90th	9.197	9.045	-1.281	-1.281
90th [p-value]	0.162	0.267	0.548	0.589

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 β 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 centile* is the percentile of parents in the national income distribution. The variable β is instrumented by β *simulated*, estimated as β 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. "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. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort 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. *** p<0.01, ** p<0.05, * p<0.1.

Table VIII: Heterogeneous Effects of School Finance Equalization by Income Inequality. 2SLS, Dependent Variable is Children's Income Percentile

	Low Inequality		High Inequality	
	(1)	(2)	(3)	(4)
β	-4.6680	-4.6243*	-6.5942**	-6.5510**
	(2.9321)	(2.7704)	(2.6800)	(2.6456)
$\beta\times$ parent centile	0.0274***	0.0274***	0.0217***	0.0217***
	(0.0066)	(0.0062)	(0.0016)	(0.0017)
Parent centile FE	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes
CZ FE	Yes	No	Yes	No
Cohort FE	Yes	Yes	Yes	Yes
N (CZ $\times$ parent cent. $\times$ cohort)	5586	5586	7950	7950
10th	4.394	4.350	6.378	6.334
10th [p-value]	0.131	0.115	0.017	0.017
25th	3.983	3.939	6.053	6.010
25th [p-value]	0.168	0.150	0.024	0.023
90th	2.202	2.159	4.645	4.602
90th [p-value]	0.434	0.424	0.082	0.083

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 β 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 centile* is the percentile of parents in the national income distribution. The variable β is instrumented by β *simulated*, estimated as β 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. "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. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort 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. *** p<0.01, ** p<0.05, * p<0.1.

Table IX: Heterogeneous Effects of School Finance Equalization by Income Segregation. 2SLS, Dependent Variable is Children's Income Percentile

	Low Segregation		High Segregation	
	(1)	(2)	(3)	(4)
β	-5.7397**	-5.7397**	-6.2459**	-6.1846**
	(2.3990)	(2.3302)	(2.7334)	(2.5457)
$\beta\times$ parent centile	0.0309***	0.0309***	0.0234***	0.0234***
	(0.0045)	(0.0046)	(0.0019)	(0.0019)
Parent centile FE	Yes	Yes	Yes	Yes
State FE	No	Yes	No	Yes
CZ FE	Yes	No	Yes	No
Cohort FE	Yes	Yes	Yes	Yes
N (CZ $\times$ parent cent. $\times$ cohort)	5880	5880	7698	7698
10th	5.431	5.431	6.012	5.951
10th [p-value]	0.023	0.019	0.028	0.019
25th	4.967	4.967	5.662	5.600
25th [p-value]	0.036	0.031	0.038	0.027
90th	2.958	2.958	4.143	4.082
90th [p-value]	0.205	0.194	0.128	0.106

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 β 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 centile* is the percentile of parents in the national income distribution. The variable β is instrumented by β *simulated*, estimated as β 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. "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. 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. *** p<0.01, ** p<0.05, * p<0.1.

Table X: School Finance Equalization and School Inputs. OLS and 2SLS, Dependent Variable is the Number of Teachers per Student

	OLS		2SLS	
	(1)	(2)	(3)	(4)
$\beta\times$ income in the 1st quartile	-0.0029*	-0.0060**	-0.0051*	-0.0076**
	(0.0015)	(0.0024)	(0.0028)	(0.0032)
$\beta\times$ income in the 2nd quartile	-0.0002	0.0010	-0.0027	-0.0035
	(0.0015)	(0.0012)	(0.0026)	(0.0029)
$\beta\times$ income in the 3rd quartile	0.0017	0.0018	-0.0009	0.0003
	(0.0018)	(0.0023)	(0.0025)	(0.0027)
$\beta\times$ income in the 4th quartile	0.0021	0.0021	-0.0000	0.0017
	(0.0017)	(0.0036)	(0.0031)	(0.0040)
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 β 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 Xth quartile* equals 1 for districts with median household income in the Xth quartile of the national distribution in 1990. Columns 1 and 2 estimate OLS; columns 3 and 4 estimate 2SLS, with β^{sim} (obtained using simulated revenues instead of actual revenues) as an instrument for β. 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 California, Colorado, Florida, Georgia, Illinois, Kentucky, Louisiana, Massachusetts, Michigan, Minnesota, Montana, Nebraska, New Jersey, New York, North Dakota, Ohio, Pennsylvania, Utah, Texas, and Wisconsin. *** p<0.01, ** p<0.05, * p<0.1.

Table XI: School Finance Equalization and College Enrollment. 2SLS, Dependent Variable is Children's Probability of College Enrollment at Age 19

	(1)	(2)	(3)	(4)
β	-0.0782	-0.0773		
	(0.0937)	(0.0983)		
$\beta\times$ parent centile	0.0002***	0.0002***		
	(0.0000)	(0.0000)		
$\beta\times$ reform in elementary school			-0.3322	-0.3281
			(0.1731)	(0.1701)
$\beta\times$ parent centile $\times$ reform in elementary school			0.0003**	0.0003**
			(0.0001)	(0.0001)
$\beta\times$ reform in middle school			-0.3256*	-0.3209*
			(0.1673)	(0.1648)
$\beta\times$ parent centile $\times$ reform in middle school			0.0001	0.0001
			(0.0001)	(0.0001)
$\beta\times$ reform in high school			-0.3291*	-0.3255*
			(0.1588)	(0.1561)
$\beta\times$ parent centile $\times$ reform in high school			0.0005***	0.0005***
			(0.0001)	(0.0001)
Parent centile FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
CZ FE	Yes	No	Yes	No
State FE	No	Yes	No	Yes
N (CZ $\times$ parent cent. $\times$ cohort)	13296	13296	13296	13296
Mean of dep. var.	0.593	0.593	0.593	0.593
10th	0.076	0.075		
25th	0.073	0.073		
90th	0.061	0.060		
10th, High School			0.325	0.321
25th, High School			0.318	0.314
90th, High School			0.288	0.284

Note: The dependent variable is the probability of college enrollment by age 19 for each parental income quantile in the state distribution, for cohorts 1984 to 1990. The variable β 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 centile* is the percentile of parents in the national income distribution. The variable β is instrumented with β *simulated*, estimated using simulated revenues instead of actual revenues. The variables *reform in elementary school*, *reform in middle school*, and *reform in high school* equal one for cohorts and states for which a reform hit during elementary, middle, and high school, respectively. All specifications include parent percentile and cohort fixed effects; columns 1 and 3 include CZ fixed effects, while columns 2 and 4 include state fixed effects. Bootstrapped standard errors in parentheses are clustered at the state and birth cohort 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. *** p<0.01, ** p<0.05, * p<0.1.