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 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.

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1 Introduction

Large differences in intergenerational income mobility exist across states and local labor markets in the United States. The probability that a child born in a family in the bottom quintile of the national income distribution will reach the top 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 types of people self-selecting into specific places, studies of movers across 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 factors make a place particularly successful at guaranteeing equal opportunities to children regardless of their family background. Places with high intergenerational mobility tend to have lower income and racial 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 in promoting mobility, however, they cannot be interpreted as causal. Yet, understanding the role of public policies is the first step towards improving intergenerational mobility and guaranteeing equal opportunities to all children regardless of their family background.

This paper moves beyond these simple correlations 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 been able to spend more per pupil than poorer districts. These between-district disparities vary across states: In 1980, the gap in expenditure between the lowest-spending and the highest-spending district was 70 percent in California, but only 40 percent in Maryland. This variation is driven by cross-state differences in funding formulas, used to define each district's revenues as a combination of state funds and local levies. In an attempt to equalize expenditures, over the past four decades states have reformed their school finance schemes through changes in these formulas. While often sharing a common objective, however, school finance equalization reforms have taken various forms across states and over time, with some being more successful at equalizing revenues than others (Hoxby, 2001).

To study the causal effects of equalization on children's intergenerational mobility, I exploit

changes in the distribution of per pupil revenues generated by 13 state-level school finance reforms passed in a sample of 20 states between 1980 and 2004. I use the measure of intergenerational mobility of Chetty et al. (2014) (also used by Acciari et al., 2019), defined as a child's percentile in the national income distribution given the income percentile of their parents, and calculated separately for each commuting zone (CZ hereafter), cohort (1980-1986), and income percentile of the parents. 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, β equals zero; when wealthier districts receive and spend more, β is positive.

I show that school finance reforms led to a sharp decline in β, i.e., to a reduction in the difference in school revenues between districts serving children from poorer and wealthier families. A simple theoretical framework predicts that this decline, which breaks the link between parents' economic conditions and children's outcomes, should improve the economic opportunities of lower-income children relative to those of her parents, "leveling the playing field" (Becker and Tomes, 1994).[1]

To empirically investigate the effects of equalization on intergenerational mobility, I exploit the fact that different cohorts were in school during different years and therefore experienced different degrees of equalization (different β) across states. For example in Wisconsin, which had a reform in 1996, the 1980 cohort (in school between 1986 and 1998) had a β equal to 0.064, while the 1986 cohort (in school between 1992 and 2004) had a β equal to 0.005. In Ohio, which had no reform between 1986 and 2004, these two cohorts had a β equal to 0.044 and 0.047 respectively. Assuming that the timing of each reforms is exogenous, comparing intergenerational mobility across cohorts within each state allows me to estimate the effects of equalization. In support of this assumption, I show that mobility was on a flat trend in the years leading to a reform in each state.[2]

The nature of these reforms, however, is such that one cannot simply use post-reform expenditures and revenues as an exogenous variable. Changes to the funding formula alter the relationship between the "price" of school spending to taxpayers and the amount of public

[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).

good they receive in return. This might induce households to "vote with their feet" (Tiebout, 1956), i.e., to move across districts based on their preferences for this public good and their income.[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, sorting changes the composition of each school district and CZ and this could directly affect mobility (for example through peer effects). Post-reform revenues are therefore endogenous.

I address this endogeneity issue with a simulated-instruments approach (similar to Gruber and Saez, 2002), which exploits plausibly random, state-specific changes in 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 a sample of 20 states, 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 district's characteristics (e.g. as property values, enrollment, and income) fixed at their pre-reform values. I then use these simulated revenues to estimate 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 intergenerational mobility. A one-standard

[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. In line with these results, I find that some reforms (such as Massachusetts, 1994 and Wisconsin, 1996) led to larger declines in β compared with others (such as Michigan, 1993). Furthermore, different reforms triggered different changes in house prices: A reform in New Jersey in 1990 led to a decline in overall prices, the one in Massachusetts lead to an increase, and the one in Michigan left prices unchanged.

deviation reduction in β leads to a significant 5.6 percentile increase in the income rank of children with parental income on the 10th percentile; this estimate correspond to a 16.2 percent increase in income. By comparison, the same reduction in β has a smaller and insignificant effect on children with parents on the 90th income percentile. My results also indicate that the average reform would increase mobility of children from families on the 10th percentile by 3.6 percentiles (or 8.6 percent), 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 increases the income rank of children with parents on the 10th percentile by 7.6 percentiles if the reform that generates this decline is experienced during elementary school, but only 4.1 percentiles if it is experienced during high school. This finding is in line with a large literature highlighting the importance of early childhood investments for long-run outcomes (see Cunha and Heckman, 2010, for a review).

The effects of equalization in school revenues might vary depending on the degree of income inequality and segregation within each CZ. When cross-district income inequality is high, the same reduction in β might translate into a much 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. 2SLS estimates confirm that a decline in β has the largest effects on CZs with higher income inequality and higher segregation.

In the last part of the paper I explore the channels behind the main estimates. I provide suggestive evidence that school finance equalization increases intergenerational mobility 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 pieces of evidence on the causal determinants of intergenerational mobility.[6] Recent research has revealed

[5]The average reform reduces β by approximately 0.045 (Figure I), or 0.64 of a standard deviation. The effect of this decline on mobility of children with parents on the 10th percentile is an increase of approximately 3.6 percentiles, which corresponds to 13 percent of the 27.6 percentile gap in mobility between the highest-mobility CZ (Sioux Center, IA) and the lowest-mobility one (Clarksdale, MS).

[6]Most of the earlier literature on intergenerational mobility is descriptive and has focused on comparing vari-

large differences in mobility across US local labor markets and a positive correlation between mobility and school quality (Chetty et al., 2014).[7] At the same time, Rothstein (2019) argues that differences in school quality do not seem to explain much of the observed differences in mobility and suggests that attention should be placed on other types of policies. While these findings are based on correlations, this paper shows that a school-related policy such as school finance equalization *causes* a sizable improvement in long-term outcomes of economically disadvantaged children, in line with Card et al. (2018). This implies that equalization can be an engine for mobility, even if it explains a relatively small share of the cross-sectional variation. My results also shed some light on the mechanisms through which equalization in school resources affect children's long-run outcomes: equalization in school inputs and in college attendance.

Second, this paper contributes to a large literature on the effects of school spending on students' outcomes, initiated decades ago by the Coleman report (Coleman et al., 1966) and encompassing observational (Hanushek, 1986, 1997, 2003), quasi-experimental (Card and Krueger, 1992; Hyman, 2017), and experimental studies (Krueger, 1999; Dynarski et al., 2013). A few studies in this literature have used school finance reforms as a source of variation in the levels of school expenditures to study the effects on short-term absolute outcomes, such as student achievement and educational attainment (Hoxby, 2001; Card and Payne, 2002; Hyman, 2017; Lafortune et al., 2018), and long-term outcomes, such as earnings (Jackson et al., 2015). This paper extends this literature by focusing on intergenerational mobility, a measure of economic opportunity which has received increasing attention in recent years thanks to the availability of administrative tax data (Chetty et al., 2014; Acciari et al., 2019). As mobility is an inherently relative concept, designed to capture the relationship between children's and parents' outcomes, rather than on revenue levels I focus my analysis on the effects of revenue *equalization*, the key parameter that governs the extent to which investments in public education depend on parents' economic status.

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

ous measures across countries and using different samples within each country. Early studies have looked at the correlation in earnings of parents and children at a single point in time (Becker and Tomes, 1994). Subsequent works (surveyed in Solon, 1999) have tried to obtain more precise estimates using panel data and isolating the permanent component of lifetime income. Another related strand of research has attempted to perform international comparisons of intergenerational income elasticities (Solon, 2002).

[7] Using a similar approach and administrative data from Italy, Acciari et al. (2019) find very large differences in intergenerational mobility between the North and the South.

across states when studying the effects of school finance equalization. Trying 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 and with each district's initial income. This approach, however, is unable to fully account for the fact that even reforms of the same type could have affected the distribution of revenues and expenditures in different ways and triggered different household responses. My estimates demonstrate that ignoring these differences can lead to an inconsistent estimation of the effects of equalization. My approach, and the accompanying dataset, can be used in other settings as well.

The rest of the paper proceeds as follows. Section 2 describes the school finance equalization reforms. Section 3 uses a simple theoretical framework to illustrate the relationship between school finance equalization and intergenerational mobility. Section 4 describes the data. Section 5 introduces 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 intergenerational mobility. 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 considerably more compared to low-income districts. Over time, this has created large disparities in per pupil revenues and expenditures 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 instead the outcome 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 motive, 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 over 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 focus on 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 are, however, the combination of two or more of these types. In addition, the parameters of each formula vary considerably across states and over time even within types. As a result, plans passed under the same name have had very different effects on districts' revenues and expenditures.

One common aspect of different 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 (or "level down"; Hoxby, 2001).[8]

Empirical evidence on the effects of school finance equalization on student achievement is mixed. Card and Payne (2002) find that court-mandated reforms reduced gaps in SAT scores between low- and high-income students. More recently, Lafortune et al. (2018) estimate a positive and large effect of adequacy reforms on test scores. Some studies focusing on individual states have also found positive effects of equalization on achievement (Guryan, 2001; Papke, 2005; Roy, 2011) and educational attainment (Hyman, 2017). Downes et al. (1997), on the other hand, find no effects of equalization on the distribution of test scores, and Hoxby (2001) finds mixed evidence on high school dropout. In one of the few studies of the long-run effects of school finance equalization, Jackson et al. (2015) find large effects of increased expenditure on future educational achievement, wages, and poverty incidence among low-income students.

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

3 A Simple Model of School Finance and Intergenerational Mobility

I start with a very simple conceptual framework to illustrate the relationship between school finance equalization and intergenerational mobility. This model yields a testable prediction, which I bring to the data in the remainder of the paper.

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. School districts are responsible for the financing of public schools, and each child attends school in the district she lives in. The income of a child in family i, living in school district d and state s, is determined as follows:

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

where x_{id} is parental income and e_d is public expenditure on the child's education. The parameter θ captures all the possible ways in which parental income is related to children's income (e.g. transmission of ability or private investments in education). By expressing the child's income in this way, I implicitly assume that the returns to public education investments are constant across children.

School spending in district d, located in state s, is defined by the following formula:

$$e_d = (1 - \beta_s)K_s + \beta_s x_d \tag{2}$$

where K_s is a state-level constant and x_d is average parental income in district d. In this expression, the parameter β_s captures the degree of equalization in school expenditure within each state. When $\beta_s = 0$, $e_d = K_s$: expenditure is fully equalized across all districts in state s. When $\beta_s > 0$, on the other hand, e_d depends positively on x_d, which implies that the richer districts in the state have higher expenditures and vice versa. Lastly, when $\beta_s < 0$ the system is redistributive and poorer districts receive and spend more.

The income of the child can be rewritten as a function of K_s and β_s as follows:

$$y_{id} = \theta x_{id} + \gamma K_s + \gamma \beta_s (x_d - K_s) \tag{3}$$

This simple conceptual framework can be used to highlight the relationship between intergenerational income mobility and inequality of school expenditure across districts, captured

by the parameter β_s. Intergenerational income mobility of children born in families in the r percentile of the national parent income distribution can be defined, as in Chetty et al. (2014), as:

$$M_s^r = F_y(y_{id}|F_x(x_{id}) = r/100) \tag{4}$$

where $F_y(\cdot)$ denotes the cumulative distribution function (CDF) of children's income and $F_x(\cdot)$ denotes the CDF of parents' income.[9] I make the simplifying assumption that $x_{id} = x_d$ for every individual i living in district d. I define $Q_t(\cdot)$ as the quantile function of the random variable t, i.e. the function that computes the value of the variable corresponding to a given percentile of its distribution.[10] Substituting the expression for the child's income from equation (1) allows me to express mobility as a function of the parameter β_s:

$$M_s^r = F_y(\theta Q_x(r) + \gamma K_s + \gamma\beta_s(Q_x(r) - K_s)) \tag{5}$$

Being a CDF, the function $F_y(\cdot)$ is non-decreasing. As result, M_s^r is non-increasing in β_s when $Q_x(r) \leq K_s$, i.e. for children with parents below a given percentile in the state's income distribution, and more so for children with a smaller parents' percentile. I empirically test this prediction in the remainder of the paper.

4 Data

To conduct the empirical analysis I combine data from multiple sources. In the final data set, each observation corresponds to a group of children born in a given cohort, CZ, and with parent's income in a given percentile within the CZ. 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 Expenditures and 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.). Both the nature of these elements and the way they are measured vary across states. This information

[9] M_s^r is analogous to the absolute mobility measure of Chetty et al. (2014), presented in Section 4.
[10] Note that $Q_t(a) = F_t^{-1}(a)$.

is therefore not readily available as a unified database.[11]

To address this data limitation I construct a new district-level panel dataset for each state, drawing from states' detailed historical records on school finance 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, highlighted in Figure AII, appear similar to the remaining states with respect to a range of characteristics of schools, families, and households (Table AI).[12] The elements of the dataset for each state are described in Table CI, and the various formulas are described in detail in Appendix D. [13]

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 -$2,306 to $12,965 across states, and from -$10,710 to $14,518 across CZs in 1990.

School Finance Reforms I compile a list of all state-level school finance reforms passed between 1980 and 2004, to encompass the time period when the cohorts at study (born between 1980 and 1986) were in grades one to twelve (i.e., 1986-2004). To do so I combine information from "Public School Finance Programs of the United States and Canada" (1990–1991[14] and 1998–1999[15]) 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 consis-

[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] The only statistically different differences are population size and the divorce rate.

[13] I obtained the data via direct requests or through a FOIA addressed to each state's Department of Education. The request was fulfilled by the states of California (data available for the years 1996-2004), Colorado (1994-2004), Florida (1988-2004), Georgia (1987-2004), Illinois (1987-2004), Kentucky (1991-2004), Louisiana (1993-2004), Massachusetts (1993-2004), Michigan (1990-2004), Minnesota (1991-2004), Montana (1994-2004), Nebraska (1993-2004), New Jersey (1988-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Texas (1986-2004), Utah (1986-2004), and Wisconsin (1986-2004). The remaining states did not maintain detailed records on historical school finance data. California, Illinois, Florida, Georgia, New York, North Dakota, Ohio, Pennsylvania, and Utah did not experience any reform between 1986 and 2004; the remaining states experienced at least one reform, and New Jersey and Texas experienced two reforms. Appendix D describes the formulas in more detail.

[14] Albany, NY : American Education Finance Association and Center for the Study of the States, The Nelson A. Rockefeller Institute of Government, State University of New York, 1992.

[15] Washington, DC: US Dept. of Education, Office of Educational Research and Improvement, National Center for Education Statistics, 2001.

tent across the different sources; when discrepancies are found, priority is given to the "Public School Finance Programs of United States and Canada" 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.

Income I use tabulations of household income at the school district level, taken 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, to calculate median household income in each district.[16] I link these data with information on per pupil school revenues to compute measures of equalization across districts in each state and year.

Intergenerational Mobility I use the intergenerational mobility measure proposed and constructed by Chetty et al. (2014), and defined as children's percentile in the national income distribution for a given CZ, cohort, and income percentile of the parents. To construct this measure, Chetty et al. (2014) use administrative tax records to estimate the intercept and slope of the linear relationship between parents' and children's national income percentiles for 637 out of 722 CZs (including 327 CZs for which simulated revenues are available) and for each cohort of children born between 1980 and 1986.[17] Combined with information on the national and CZ-specific distributions of parental income, these estimates allow me to predict the income percentile of children for each CZ, cohort, and parental income percentile *in the CZ*.[18] Compared to the simple correlation between parents' and children's incomes (used by Solon, 1992; Björklund and Jäntti, 1997; Lee and Solon, 2009, among others) this measure allows me to study the economic performance of children relative to their parents for different percentiles of parental income.[19]

[16]Income tabulations at the school district level are contained in the Census STF3F file for 1980 and published as part of the National Center for Education Statistics' (NCES) School District Demographic System 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 School District Demographic System.

[17]Slope and intercept estimates are published as the Online Data Table 1 of Chetty et al. (2014), available at https://opportunityinsights.org/. Children are assigned to CZs based on when they lived at age 16, irrespective of whether they moved when they entered the labor market.

[18]I make the choice of using parents' percentiles in the CZ, rather than in the national distribution, to account for differences in income distributions across CZs. This allows me to correctly weigh each CZ in the sample; using percentiles in the national distribution instead translates into having observation in the data set that correspond to different counts of people. To see this, consider a CZ with 10 percent of individuals on the 25th national percentile and only 0.1 percent on the 99th percentile. Using national percentiles these two groups would receive equal weight in the estimation, even though the first contains more people than the second. Information on the income distributions within each CZ is published as the Online Data Table 7 of Chetty et al. (2014), available at https://opportunityinsights.org/.

[19]To see this, consider an increase in the parent-child income correlation. Such an increase could be caused by better outcomes for the poor or worse outcomes for the rich. My measure, analogous to Chetty et al. (2014)'s

The final dataset contains children's percentiles for 327 CZs, seven birth cohorts, and six CZ-specific parental income percentiles (10th, 25th, 50th, 75th, 90th, and 99th). 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).[20] Wide differences exist across CZs (Figure AI): The mean percentile of children with parents on the 25th percentile is as low as 32 in Gordon, SD and as high as 61 in Sioux Center, IA, while for children with parents on the 75th percentile it is as low as 51 in Gallup, AZ and as high as 70 in Hiawatha, KS. Mobility appears to increase, albeit slowly, across cohorts. 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 percentile in the CZ.[21]

Cross-County Migration Data on county-level migration flows and on the incomes of migrants are taken 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 Authority's (FHFA).[22] 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 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 dis-

"absolute" mobility, allows me to study these two cases separately.

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

[21] 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.

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

trict and year, available for the years 1988 to 2010.

5 Measuring Inequality in School Expenditure

The first step of the empirical analysis is to build a measure of inequality in per pupil revenues across school districts. 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:[23]

$$e_{dt} = \alpha_{st} + \beta_{st} x_{dt} + \varepsilon_{dt} \tag{6}$$

where e_{dt} is per pupil revenues 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 for 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 (measured at the year level) on intergenerational mobility (measured at the cohort level) I assign each cohort a measure of revenue inequality experienced while in school, constructed as the average β over the calendar years in which the cohort was in grades one to twelve.[24] 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

[23] A similar approach has been used by Hoxby (1998); Card and Payne (2002); Lafortune et al. (2018).
[24] For example, the β_s for the 1980 cohort is the average of the β_{st} for the years 1986-1997.

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 interpolate between the income values of the Census years at the beginning and at the end of the decade.[25] To demonstrate that my results are not driven by this imputation procedure, in robustness checks I use a version of β estimated assigning the 1990 median district income to all years (Table AVIII).

On average, the parameter β is equal to 0.019 for states without a school finance reform (with a standard deviation of 0.098); for states with a reform it equals 0.041 in the years before the reform (with a standard deviation of 0.027) and to -0.004 in the years after the reform (with a standard deviation of 0.034, Table I, Panel C). Figure I illustrates the changes in β 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:

$$\hat{\beta}_{st} = \sum_{k=-3}^{10} \delta_k R_s \mathbb{1}(t - ryear_s = k) + \varepsilon_{st} \tag{7}$$

where $\hat{\beta}_{st}$ is the estimated β 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.[26] Estimates of β 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 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.

[25] 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.

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

6 Endogeneity of Post-Reform School Expenditure and Simulated Instruments

6.1 Explaining The Need for An Instrument

To test the theoretical predictions derived in Section 3 and to study the effects of school finance equalization (i.e., a reduction in β) on intergenerational mobility, one needs an exogenous source of variation in the distribution of school revenues across richer and poorer districts. School finance reforms have changed the formulas used by states to allocate funds to individual districts, in turn affecting their revenues and expenditures. Assuming that the timing of these events is random, several studies have used them as exogenous shifters of school spending to study a variety of children's outcomes (Jackson et al., 2015; Lafortune et al., 2018).

The particular nature of these reforms, however, is such that post-reform revenues and expenditures can be endogenous even if reforms are random events. Changes to the school funding formula affect the tax price (i.e., the level 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 will respond to this change in the tax price by "voting with their feet" (Tiebout, 1956) and by moving to a different district (Aaronson, 1999; Dee, 2000; Figlio and Lucas, 2004; Epple and Ferreyra, 2008; Chakrabarti and Roy, 2015). On one side, household sorting is likely to affect house prices, property tax revenues and, subsequently, school revenues. On the other side, it could have a direct effect on intergenerational mobility, e.g. through changes in the environment children are exposed to as they grow up.

How Prevalent is Household Sorting After A School Finance Reform? To assess this, I conduct an event study of county-level migration flows around an equalization event. I estimate:

$$m_{kt} = \sum_{n=-5}^{5} \delta_n R_{s(k)} 1(t - ryear_{s(k)} = n) + \gamma_k + \tau_t + \varepsilon_{kt} \tag{8}$$

where m_{kt} is either the in-migration or the out-migration rate, defined respectively as the total number of households moving in or 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 III (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 better characterize the patterns of sorting, I re-estimate equation (8) 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 III, reveal that the absolute difference in incomes of both in-migrants and out-migrants and incomes 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.[27] This sorting can affect house prices, change the composition of local communities, and in turn lead to the endogeneity of post-reform revenues.[28]

6.2 Constructing the Simulated Instrument

I tackle 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.[29] The goal of this strategy is to isolate the exogenous variation in funding inequality (captured by β), driven by the timing of the reform and the type of funding formula, from the endogenous variation driven by changes in the tax base.

Empirical Framework To give a better sense of how simulated instruments work in this context, I illustrate the approach within the empirical model in equation (6). School revenues are a function of a district's characteristics (through the funding formula). By construction, β_{st}

[27] Income and wealth are often strongly positively correlated (Wolff and Zacharias, 2009).

[28] 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.

[29] 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) et al 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.

will be a function of the funding formula type and parameters in place in state s at time t, denoted by $g_{st}(\cdot)$, and the characteristics of the state (including the distribution of property values across districts), denoted by X_{st}: $\beta_{st} = g_{st}(X_{st})$. Suppose a reform takes place between times t and $t+1$, changing the funding formula to $g_{st+1}(\cdot) \neq g_{st}(\cdot)$. The exogeneity of the funding formula parameters and the timing of the reform imply that the change from $g_{st}(\cdot)$ to $g_{st+1}(\cdot)$ is exogenous. Household sorting, however, leads X_{st} to change to X_{st+1}. If this difference has a direct effect on mobility, β_{st+1} will be endogenous and OLS estimates of its effect on mobility will be biased.

It is useful to express β_{st+1} as the sum of an exogenous component and an endogenous one:

$$\beta_{st+1} = g_{st+1}(X_{st}) + b_{st+1} \text{ where } b_{st+1} = g_{st+1}(X_{st+1}) - g_{st+1}(X_{st})$$

The quantity $g_{t+1}(X_{st})$ is the β_{st+1} that would have resulted had households not sorted (and house prices not changed) and it is exogenous. The quantity b_{st+1} captures instead the effect of the endogenous changes in X_{st} on β_{st+1}. To obtain consistent estimates of the effects of changes in β on mobility, I instrument β_{st+1} with $g_{st+1}(X_{st})$, which I denote as β^{sim}_{st+1}.

The correlation between b_{st+1} and intergenerational mobility determines the sign of the bias of the OLS estimates. If the effect of β on mobility is negative, a positive correlation implies that OLS will be biased toward zero, whereas a negative correlation implies that OLS will overstate the negative effect of β on mobility. 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 Formulas Heterogeneity Across States

While earlier studies of school finance reforms (such as Card and Payne, 2002) have not explicitly accounted for post-reform revenues endogeneity, more recent studies (such as Jackson et al., 2015; Hyman, 2017) have recognized and addressed it.Jackson et al. (2015) (JJP hereafter), for example, study the effects of several reforms passed across all US states since the 1970s and instrument expenditure 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 plan, or equalized effort).

While similar to JJP's, my approach bears one important difference. Their strategy relies on the assumption that reforms of the same type have the same effect on expenditure conditional

on a district's relative position in the state's expenditure and income distributions. If I were to use JJP's strategy in my context, the instrument would be specified as $\tilde{\beta}^{sim}_{st+1} = \tilde{g}(\hat{X}_{st})$.[30] In other words, the instrument formula would be the same across all states, and the set of characteristics considered $(\tilde{X}_{st})$ would be a subset of all the ones entering the actual formula.

What happens when one uses $\tilde{\beta}^{sim}_{st+1}$ in lieu of β^{sim}_{st+1} as an instrument? First, the function $\tilde{g}(.)$. can be seen as a "restricted" or simplified version of $g_{st+1}(\cdot)$; as a result, using $\tilde{g}(\cdot)$ implies using fewer instruments than there are available, which could lead to asymptotic inefficiency (Greene, 2008, Chapter 12).[31]

Second, in the presence of large differences in $g_{st}(\cdot)$ across states, the standard IV monotonicity assumption (Angrist and Imbens, 1995; Angrist et al., 1996) is more likely to be violated when using $\tilde{\beta}^{sim}_{st}$ than when using β^{sim}_{st}. To see this, assume I have an endogenous $\beta_{st+1} = g_{st+1}(X_{st+1})$ with a corresponding value of JJP's instrument $\tilde{\beta}^{sim}_{st+1} = \tilde{g}(\tilde{X}_{st})$. Suppose now that all states' formulas change to $g'_{kt+1} \forall k$, such that the resulting instrument for state s would be $\tilde{\beta}'^{sim}_{st+1} = \tilde{g}'(\tilde{X}_{st}) \leq \tilde{\beta}^{sim}_{st+1}$. Monotonicity requires that $\beta'_{st+1} = g'_{st+1}(X_{st+1}) \leq \beta_{st+1}$ 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 $\beta^{sim}_{st+1} = g_{st+1}(X_{st})$, this assumption would be violated 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}, since the function g'_{st+1} is the same in both β_{st+1} and β^{sim}_{st+1}.[32]

Clearly, the extent to which β^{sim}_{st} will be a better instrument than $\tilde{\beta}^{sim}_{st}$ depends on how large the heterogeneity in funding formula changes across states is in reality (Hoxby, 2001). Figure II shows the trend in β around the year of the 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

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

[31] 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.

[32] 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. The reasoning outlined in the text 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 $\tilde{\beta}^{sim}_{st+1}$ they are not.

reduced β from 0.045 to 0.041).[33]

Different reforms also had different effects on house prices. Figure IV shows trends in the difference in house prices between districts with household income above and below the state median in 1990. While some reforms (such as the one of Texas) were followed by a decline in this difference (which implies an increase in house prices in poorer districts relative to richer ones), others (such as Michigan) did not trigger any significant changes, and others (such as Massachusetts) were followed by an increase.[34]

These differences in the effectiveness of each reform and in the house price responses across states suggest that the use of β_{st}^{sim} in lieu of $\tilde{\beta}_{st}^{sim}$ could significantly improve the consistency and efficiency of the estimates. In Section 7, I contrast estimates obtained using β_{st}^{sim} with those obtained using $\tilde{\beta}_{st}^{sim}$, and show that the differences in these two sets of estimates are significant.

Implementation I construct β^{sim} as follows. First, I construct 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 "Public School Finance Programs of United States and Canada" (1990–1991 and 1998–1999), as well as various state legislative bills (see Appendix D for details on the specific formulas). I then use the formulas to simulate each district's post-reform revenues, holding endogenous characteristics (i.e., property values, property tax rates, and income) fixed at their pre-reform values.[35] Lastly, I construct β^{sim} for each state and year, estimating equation (6) with simulated revenues instead of actual revenues.[36]

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 vi-

[33]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.

[34]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.

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

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

olated 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 random.

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, and 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).[37]

On average, the parameter β^{sim} equals 0.040 (with a standard deviation of 0.030) in the years preceding each reform, and it drops to 0.003 (with a standard deviation of 0.031) 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).[38]

7 Effects of Equalization on Intergenerational Mobility

This section studies the effects of equalizing school revenues across districts on children's intergenerational mobility. Identification of these effects exploits variation in exposure to equalization across cohorts and states, given by exogenous differences in the timing and effectiveness of these reforms.

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

[38] The table shows estimates of a regression of β_{st}^{sim} on the average change in house prices, the average in-migration and out-migration rate, and the ratios between the incomes of in-migrants and out-migrants 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_{st}^{sim} over time.

Figure VI shows a simple event study of intergenerational mobility, measured as the income rank of children with parental incomes on the 10th percentile, by exposure to a reform and separately for states with an "effective" school finance 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. In states with an effective reform, mobility is – if anything – on a slightly downward trend for cohorts who were in school before a reform and therefore not exposed to it ("negative" exposure), while it gradually increases with exposure (e.g. it is 4 percentiles higher for cohorts exposed for 7 years compared with non-exposed cohorts). In states with an ineffective reform, on the other hand, mobility does not significantly change with exposure.[39] This figure provides a first piece of evidence that exposure to effective reforms is associated with increased intergenerational mobility. Furthermore, the absence of pre-trends in mobility of non-exposed cohorts supports the assumption of exogenous timing of each reform.

7.1 OLS Estimates

While useful to illustrate the trends in intergenerational mobility 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 intergenerational mobility and to explore its effects on children with different parental incomes, I estimate the following equation:

$$M_{cbx} = \delta_0 \hat{\beta}_{s(c)b} + \delta \hat{\beta}_{s(c)b} \times \theta_{n(cx)} + \kappa_c + \tau_b + \theta_{n(cx)} + \omega_{cbx} \tag{9}$$

where the variable M_{cbx} is the expected income percentile of children in CZ c, cohort b, and with parental income on the x-th percentile in the CZ (either the 10th, 25th, 50th, 70th, 90th, or 99th).[40] 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

[39] The figure shows OLS points estimates and 90 percent confidence intervals of the coefficients δ_n in the equation $m_{cb} = \sum_{n=-4}^{7} \delta_n R_{s(c)} \mathbb{1}(b + 12 - ryear_{s(c)} = n) + \theta_c + \tau_b + \varepsilon_{cv}$, where m_{cb} is the mean rank of children in CZ c, cohort b, and with parents' income on the 10th percentile in the national income distribution, $R_{s(c)}$ equals 1 if state s experienced a school finance reform in the years 1980-2004, $ryear_{s(c)}$ is the year of the first reform, 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 level.

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

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 the parents' rank in the *national* income distribution $n(cx)$, to account for the fact that different CZs might have different income distributions.[41] 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 cluster standard errors 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 (9) are shown in Table II. A one-standard deviation reduction in β is associated with a 3.8 percentile increase in mobility of children with parental income at the bottom of the income distribution, although this coefficient is indistinguishable from zero (estimate of β equal to -3.8397, Table II, column 1, p-value equal to 0.12). An estimate of δ equal to 0.0246 indicates that this positive association is reduced by 0.025 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 3.6 percentile increase in mobility for children with parental income on the 10th percentile (p-value equal to 0.15), a 3.2 percentile increase for children with parental income on the 25th percentile (p-value equal to 0.18), and a smaller 1.6 percentile increase for children with parental income on the 90th percentile (p-value equal to 0.48). These estimates are robust to controlling for state fixed effects (Table II, column 2).

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. To account for these differences, in columns 3 and 4 of Table II I control for average per pupil expenditure in state s on cohort c. Estimates of δ_0 and δ are essentially unchanged.

In Figure VII (solid line) I relax the linearity restriction of equation (9) and I allow the effect of a decline in β to vary flexibly by decile of parental income. These estimates reveal

[41] 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.

that the relationship between the effect of a decline in β and parents' rank in the national income distribution is close to linear. Controlling for CZ fixed effects, a one-standard-deviation reduction in β is associated with a 3.1 percentile increase in mobility for children with parents in the first decile (p-value equal to 0.15), a 3.6 percentile increase for children with parents in the second decile (p-value equal to 0.16), and only a 1.34 percentile increase for children with parents in the top decile (p-value equal to 0.56).

7.2 Two-Stages Least Squares Estimates

Household sorting after each school finance reform directly affects both β and intergenerational mobility: OLS estimates will therefore be biased and cannot be interpreted as causal. To get at the causal effects, 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 regression, 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 Wald F-statistics are all close to 20.[42]

2SLS estimates confirm the positive relationship between equalization and mobility, but yield larger effects. Controlling for CZ fixed effects, a one-standard deviation reduction in β leads to a 5.8 percentile increase in mobility for children with parental income at the bottom of the national distribution (estimate of β equal to -5.8120, Table IV, column 1, significant at 10 percent). A positive estimate for δ ($\beta\times$ *parent centile*, significant at 1 percent) indicates that this effect decreases by 0.026 percentiles with each additional percentile of parental income. These estimates translate into a 5.6 and 5.2 percentile increase for children with parental income on the 10th and 25th percentiles (p-values 0.097 and 0.117) respectively, but only an insignificant 3.5 percentile increase for those with parents on the 90th percentile (p-value 0.256). These results also indicate that the average reform, which decreases β by approximately 0.64 standard deviations, would increase mobility of children from families on 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).

Estimates are slightly smaller when controlling for state fixed effects (Table IV, column 2), and they are largely unchanged when controlling for average per pupil expenditure in the state (Table IV, columns 3 and 4). Importantly, in all these specifications 2SLS estimates

[42] 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 test size equal to 0.10.

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 each decile of parental income in the national distribution. The patterns of the estimates across the distribution of parental income resemble OLS, but the magnitudes are larger. A one-standard deviation reduction in β leads to a 5.4 percentile increase in mobility for children with parental income in the first decile (significant at 10 percent), but only 3.3 percentiles for children with parental income in the top decile (p-value equal to 0.28).

Effects on Income To better characterize the magnitude of these effects in monetary terms, I use the national distribution of children's income to map intergenerational mobility measures by CZ, cohort, and parental income percentile into income *levels*, and I use the logarithm of income as the dependent variable in equation (9). 2SLS estimates, shown in column 3 of Table V, indicate that a one-standard deviation reduction in β leads to a 17 percent increase in income for children of parents at the bottom of the income distribution (with an estimate of β equal to -0.1574, and exp(0.1574)-1=0.17 Table V, column 3, significant at 10 percent). This effect declines by less than 0.1 percent with each additional percentile of parents' income (estimate of $\beta \times$ *parent centile* equal to 0.0007, Table V, column 3, significant at 1 percent). This implies that a one-standard-deviation reduction in β leads to a 16.2 percent increase in income for children with parental income in the 10th percentile (p-value 0.095), a 14.9 percent increase for children with parental income in the 25th percentile (p-value equal to 0.115), and a smaller and insignificant 9.5 percent increase for children with parental income in the 90th percentile (p-value equal to 0.27). 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. To obtain estimates that can more easily inform public policies, in columns 5 and 6 of Table IV I estimate the reduced-form effect of β^{sim} on intergenerational mobility. These estimates indicate that a one-standard deviation decline in β^{sim} leads to a 4.44 percentile increase in the income rank of children with parents on the 10th percentile (significant at 5 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 (9) instrumenting β with the slope coefficient of equation (6) obtained using JJP's instruments for expenditure (Approaches 1 and 2, pages 171-179; I explain the procedure more in depth in Appendix C).[43] These estimates reveal smaller and imprecise effects of equalization on intergenerational mobility. A one-standard deviation decline in β leads to a 2.9 percentile increase in intergenerational mobility for children with parents on the 10th percentile using their Approach 1 (Table AIV, column 1, t-statistic equal to 1.25), and to a 2.0 percentile increase using their Approach 2 (Table AIV, column 3, t-statistic equal to 0.87). 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, a large literature has established that education investments made at earlier ages 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 lower-income children, as college attendance is an important engine for mobility (Rothstein, 2019).

To explore this potential heterogeneity, I estimate the effects of a decline in β separately for cohorts in states with a reform during elementary, middle, and high school. I augment equation (9) as follows:

$$\begin{aligned} M_{cbx} \quad = \quad & \delta_0 \hat{\beta}_{s(c)b} + \delta \hat{\beta}_{s(c)b} \times \theta_{n(xc)} \\ & + \delta_0^e \text{Elem}_{s(c)b} \times \hat{\beta}_{s(c)b} + \delta^e \text{Elem}_{s(c)b} \times \hat{\beta}_{s(c)b} \times \theta_{n(xc)} \\ & + \delta_0^m \text{Middle}_{s(c)b} \times \hat{\beta}_{s(c)b} + \delta^m \text{Middle}_{s(c)b} \times \hat{\beta}_{s(c)b} \times \theta_{n(xc)} \\ & + \delta_0^{hs} \text{High}_{s(c)b} \times \hat{\beta}_{s(c)b} + \delta^{hs} \text{High}_{s(c)b} \times \hat{\beta}_{s(c)b} \times \theta_{n(xc)} \\ & + \eta^m \text{Middle}_{s(c)b} + \eta^{hs} \text{High}_{s(c)b} + \kappa_c + \tau_b + \theta_{n(xc)} + \omega_{cbx} \end{aligned} \tag{10}$$

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

where $\text{Elem}_{s(c)b}$, $\text{Middle}_{s(c)b}$, and $\text{High}_{s(c)b}$ 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 additional effect of a one-standard-deviation 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 (10) indicate that a decline in β is most effective when the reform happens during elementary school, relative to middle and high school. Controlling for CZ fixed effects, a one-standard deviation reduction in β leads to an additional 8.5 percentile increase in the income rank of children with parents at the bottom of the income distribution for cohorts with a reform during elementary school, relative to those with no reform (with an estimate of $\beta\times$ *reform in elementary school* equal to -8.458, Table VI, column 3, significant at 10 percent). This effect declines by 0.09 percentiles with each additional percentile of parents' income (estimate of β $\times$ *parent centile*$\times$ *reform in elementary school* equal to 0.0859, 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 7.6 percentile and 6.3 percentile higher in mobility for children with parental income on the 10th and 25th percentile respectively, with no significant difference for children with parents on the 90th percentile.

By comparison, a one-standard-deviation decline in β leads to a smaller 4.4 and 4.2 additional percentiles in the income rank of children with parents at the bottom of the income distribution if the reform hits during middle or high school, respectively (with an estimate of $\beta\times$ *reform in middle school* equal to -4.412 and of $\beta\times$ *reform in high school* equal to -4.296, Table VI, column 3, p-values equal to 0.19 and 0.24). These effects decline by 0.02 percentiles with each additional percentile of parents' income (estimate of β $\times$ *parent centile*$\times$ *reform in middle school* equal to 0.024 and of β $\times$ *parent centile*$\times$ *reform in high school* equal to 0.019, Table VI, column 3, significant at 10 and 5 percent respectively). This implies that a reduction in β for these cohorts leads to a 4.1 percentile, 3.8 percentile, and 2.5 percentile increase in mobility for children with parental income in the 10th, 25th, and 90th percentile. All these estimates 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 indicate that equalization in school resources is most effective when experienced earlier in a child's education career. Once more, the differences between OLS and 2SLS estimates highlights the importance of accounting for the endogeneity in post-reform revenues.

7.2.2 Equalization and Income Inequality

The results presented so far indicate that a decline in β has a positive effect on intergenerational mobility, especially for children from low-income families. Intuitively, equalization in school spending closes the gap in investments on the education of low- and high-income students, and this promotes equalization in their later-life outcomes.

The positive average effects of equalization could, however, mask important differences across CZs depending on how income is distributed 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 expenditure equal to \$7,000 and one district with income equal to \$75,000 and expenditure equal to \$9,000. The second has one district with income equal to \$15,000 and expenditure equal to \$5,500 and one district with income equal to \$85,000 and expenditure equal to \$8,300. Both CZs have an estimated β equal to 0.23.[44] Due to a more unequal income distribution, however, children in the lowest-spending district in the second CZ will receive \$2,800 less compared with children in the highest-spending district (or 34 percent). Children in the lowest-spending district in the first CZ, which has a more equal income distribution, will receive only \$2,000 less compared with children in the highest-spending district (or 29 percent). The same reduction in β could therefore have very different implications in these two CZs.

To investigate the heterogeneity in the effects of equalization across CZs with different income inequality, I re-estimate equation (9) 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.[45] Estimates of δ_0 and δ indicate that a decline in β has smaller effects in CZs with income differences in the bottom 25 percent of the cross-CZ distribution ("Low inequality," Table VII, columns 1 and 2) relative to CZs in the top 25 percent ("High inequality," columns 3 and 4). Controlling for CZ fixed effects, a one-standard devi-

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

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

ation decline in β in "Low inequality" CZs leads to a 4.6, 4.2, and 2.4 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -4.8634 and of $\beta\times$ *parent centile* equal to 0.0269, Table VII, column 1, p-values equal to 0.19 and 0.02). These effects are instead larger in "High inequality" CZs: The same decline in β leads to a 6.2, 5.8, and 4.4 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -6.3731 and of $\beta\times$ *parent centile* equal to 0.0221, Table VII, column 3, significant at 10 and 1 percent respectively). Estimates are robust to controlling for state fixed effects (Table VII, column 4).[46]

7.2.3 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 low-income families are more likely to be living and attending school in the same district(s) 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 (9) 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.[47] Estimates of δ_0 and δ indicate that, controlling for CZ fixed effects, a one-standard deviation decline in β in "Low segregation" CZs leads to a 5.2, 4.8, and 3.2 percentile increase for children with parents in the 10th, 25th, and 75th percentile respectively (with an estimate of β equal to -5.4864 and of $\beta\times$ *parent centile* equal to 0.0253, Table VII, column 1, significant at 10 and 1 percent). Equalization is slightly more effective in CZs with high income segregation: The same decline in β leads to a 5.8, 5.5, and 3.9 percentile increase for children with parents in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -6.0725 and of $\beta\times$ *parent centile* equal to 0.0237, Table VIII, column 3, p-values equal to 0.11 and 0.001 respectively). Estimates are robust to controlling for state fixed effects (Table VIII, columns 2 and 4).[48]

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.

[46] OLS estimates are shown in Table AV.

[47] 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.

[48] OLS estimates are shown in Table AVI.

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.

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). 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 sorting 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 districts (but not all) and therefore be driven by a much larger absolute change in expenditure in the affected districts. Table AVII shows 2SLS estimates of the main specifications separately for one-state and multi-state CZs. Estimates are fairly comparable in magnitude across the two groups, indicating that the results are not driven by the presence or absence of borders.

8 Channels: School Inputs and Intermediate Outcomes

The results described so far show that equalizing school funding across richer and poorer districts increases intergenerational mobility for children from low-income families. This section investigates the mechanisms 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 teach-

ers per student is fundamental for the growth in achievement (Krueger and Whitmore, 2001; Bloom and Unterman, 2013). Yet underfunded districts are often forced to cut instructional staff to face budget shortages.[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:

$$TS_{dt} = \delta_1 \hat{\beta}_{s(d)t} q_{dt}^{1q} + \delta_2 \hat{\beta}_{s(d)t} q_{dt}^{2q} + \delta_3 \hat{\beta}_{s(d)t} q_{dt}^{3q} + \delta_4 \hat{\beta}_{s(d)t} q_{dt}^{4q} + \gamma_d + \tau_t + \varepsilon_{dt} \tag{11}$$

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 IX shows OLS and 2SLS estimates of equation (11). 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; these effects, however, are indistinguishable from zero (Table IX, column 2). 2SLS estimates, shown in columns 3 and 4, yield larger and marginally significant positive effects on low-income districts, but no effect on high-income ones. Controlling for district fixed effects, a one-standard deviation reduction in β leads to 0.0076 additional teachers per student in districts in the bottom quartile, or 11 percent more (Table IX, 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 IX, column 3, p-value equal to 0.68).

Although imprecise, these results suggest that equalizing school spending across wealthier and poorer districts promotes intergenerational mobility 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.

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

8.2 Intermediate Outcome: College Enrollment

Rothstein (2019) and Chetty et al. (2017) suggest a positive association between college enrollment and intergenerational income mobility. To understand whether higher education is one of the channels through which school finance equalization affects income mobility, I study the effect of equalization on education mobility. To do this I re-estimate equation (9) using the probability of college enrollment at age 19 (expressed in percentage points and measured separately for each CZ, cohort, and parent percentile within the CZ) as the dependent variable.

Controlling for CZ fixed effects, 2SLS estimates indicate that a one-standard deviation 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 -0.0777, Table X, column 1, p-value equal to 0.45). Compared with an average probability of 55.6 percent, this implies a 14 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 X, 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 on the 10th and 25th percentile, and a 6.1 percentage point increase for children with parents on the 90th percentile. Differently from income mobility, OLS estimates in Table AIX are larger in magnitude.

Estimates of the heterogeneous effects of a decline in β by timing of the reform yields slightly larger effects of equalization in school revenues when experienced during high school, compared to elementary and middle school. A one-standard deviation decline in β during high school leads to a 37 percentage points increase in the probability of college enrollment for children at the bottom of the parental income distribution; the same reduction leads to a 34 percentage point decline if experienced during elementary (Table X, column 7, p-values equal to 0.11 and 0.08 respectively).

These estimates should be interpreted with caution because they are very imprecise. They do, however, suggest that access to college could be one of the channels through which school finance equalization improves low-income children's long-run outcomes. The effects of equalization on college enrollment appear slightly larger when equalization is experienced during high school; this can appear in contrast with the estimates in Section 7. Intuitively, however, high school is the moment of a student's career that immediately precedes college; this might

explain why equalization in school resources at this particular point in time appears to be important for college access.

9 Discussion and Conclusion

This paper studies the effects of equalization in school revenues across public school districts within each state on children's intergenerational income mobility. Using variation in states' funding schemes introduced by school finance reforms and exploiting differences in exposure to equalized schemes across cohorts in different states, I find that equalization increases intergenerational mobility of children from more disadvantaged backgrounds, with insignificant effects on wealthier children. My results also suggest that equalization increases mobility through a reduction in the gap in educational inputs (such as the number of teachers) and in intermediate outcomes (such as college enrollment) between low-income and high-income districts.

While being a useful source of variation in funding, school finance reforms should be used with caution. Funding formulas link property tax revenues to school spending, and tax revenues could be endogenous to mobility. Changes in tax revenues could happen, for example, if households respond to the change in the tax price introduced by each reform by "voting with their feet" and moving across districts. This sorting affects house prices and the property tax base, which in turn affect school districts' revenues. 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 keeping this heterogeneity into account in the empirical analysis to obtain unbiased and precise estimates of the effects.

To account for this source of endogeneity and for the differences in funding formulas across states, I adopt an instrumental-variable approach that directly exploits the change in the formula type and parameters following each reform. Using hand-collected information on each pre-reform and post-reform formula type and parameters, combined with district-level data on the variables entering each formula, I simulate each district's post-reform revenues in the absence of sorting. Simulated revenues can then be used as an instrument for actual expenditure. Compared with OLS, 2SLS estimates are approximately 50 percent larger in magnitude. This shows that failing to account for the endogeneity of post-reform expenditure could lead to misinterpreting the effects of equalization.

At a first glance, these findings might appear in contrast with Rothstein (2019), who performs a correlational analysis and concludes 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 variance in mobility. They also show, however, that equalizing school expenditure has a *causal* positive effect on the educational and labor market outcomes of disadvantaged children. This in turn implies that this type of policy represents an important engine of mobility for low-income children. These results are in line with Jackson et al. (2015), who show that increasing school spending improves long-run outcomes of disadvantaged students. In addition, this paper highlights the importance of accounting for differences across states in the effects of each reform on revenues and in household responses to each reform, and it proposes the direct use of funding formulas as a viable approach to obtain more reliable estimates–an approach that can be used in other studies as well.

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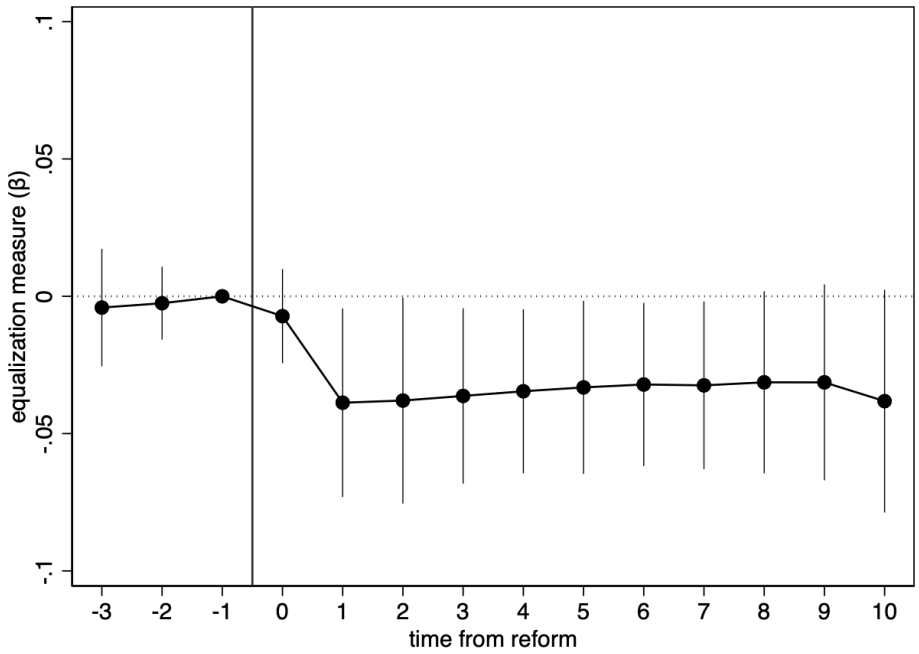
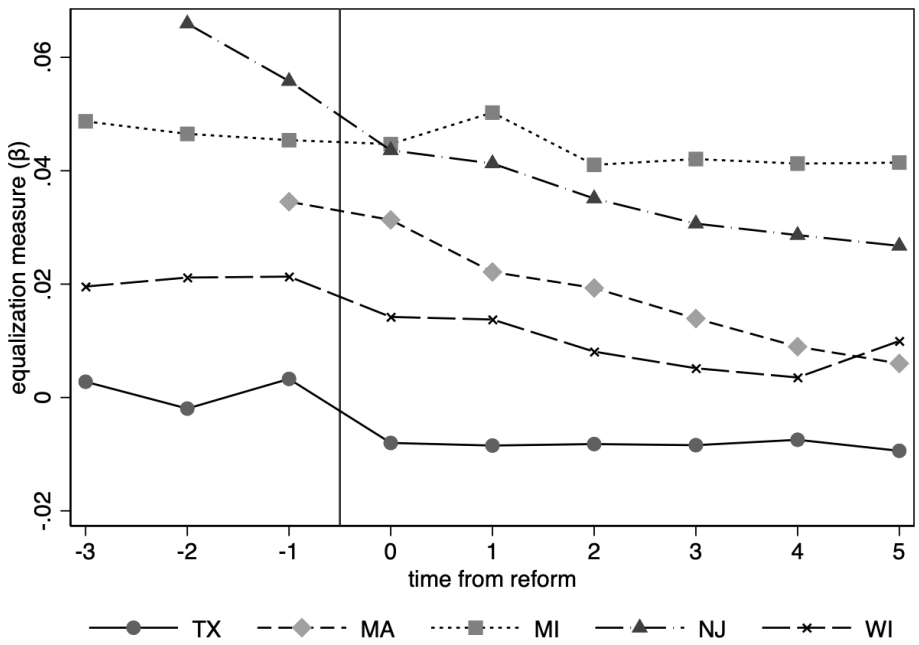
Figure I: Event Study of Equalization Measure β Around A School Finance Reform



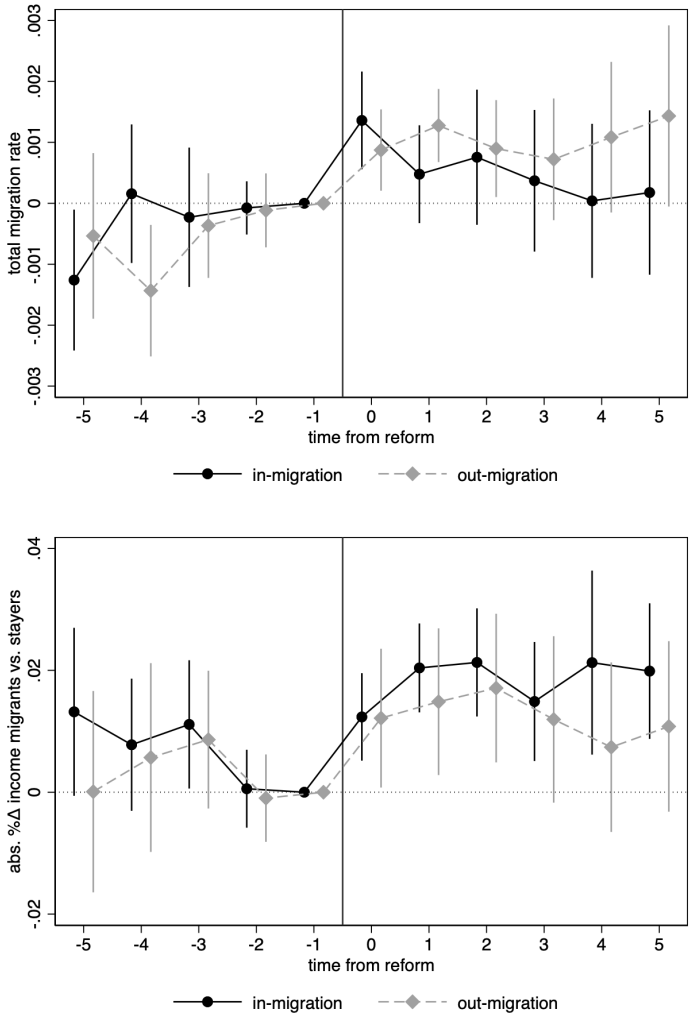
Note: Point estimates and 90 percent confidence intervals for the coefficients δ_k in regression $\beta_{st} = \sum_k \delta_k R_s 1(t - ryear_s = k) + \varepsilon_{st}$,where β_{st} is the slope coefficient in equation (6), estimated separately for each state s and year t from 1986 to 2004, R_s equals 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: Equalization Measure β Around A School Finance Reform - Selected States



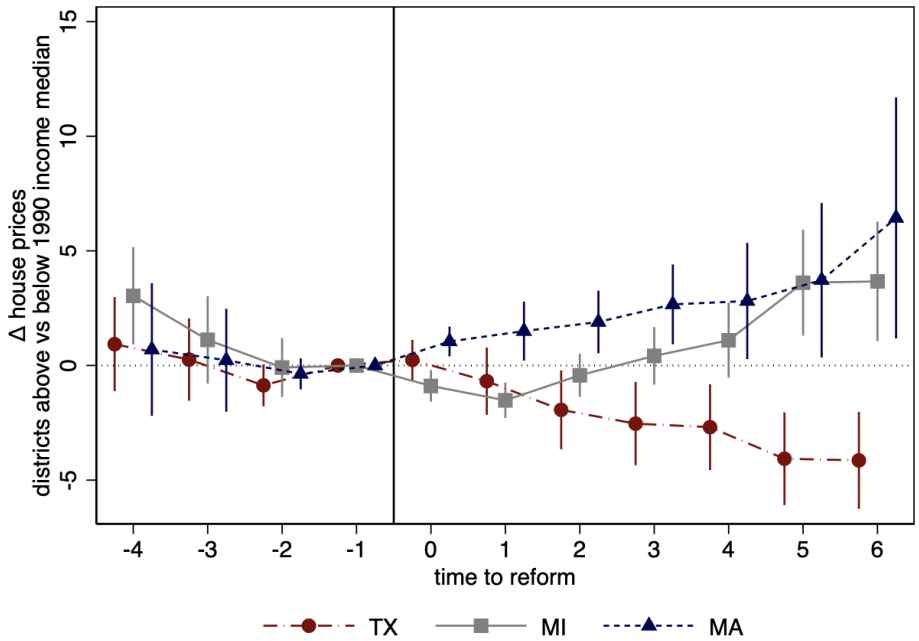
Note: The figure shows estimates of the coefficient β_{st} (defined in equation (6)) for a sample of states in the years surrounding each school finance reform.

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



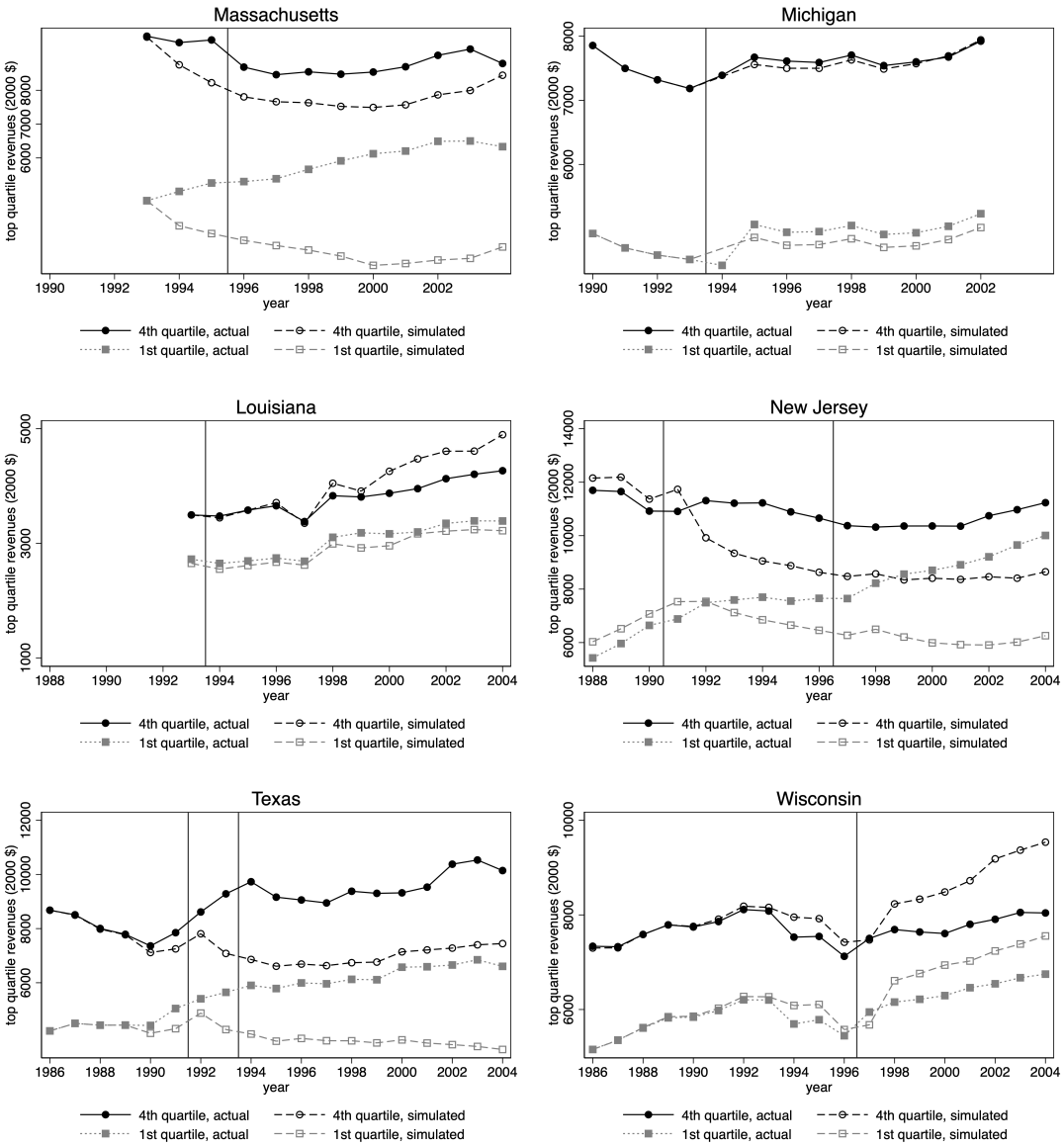
Note: Changes in total migration rates and incomes of migrants in a 10-years window around each school finance reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients δ_n in the regression $y_{kt} = \sum_{n=-5}^{5} \delta_n R_{s(k)} 1(t - ryear_k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



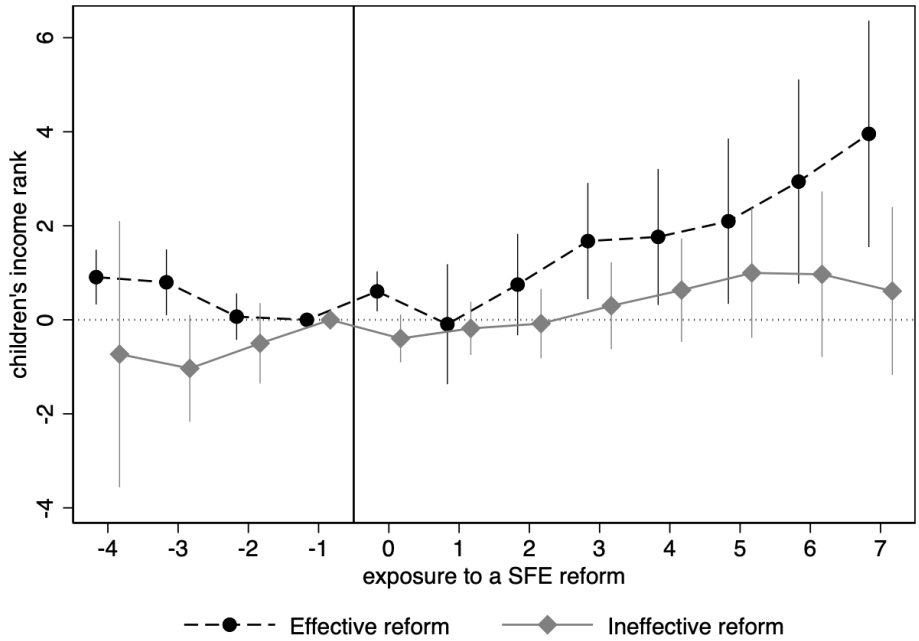
Note: Changes in the difference in house prices between households with incomes above and below the median in 1990, in a 10-years window around each reform and relative to the year before the reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients 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. 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



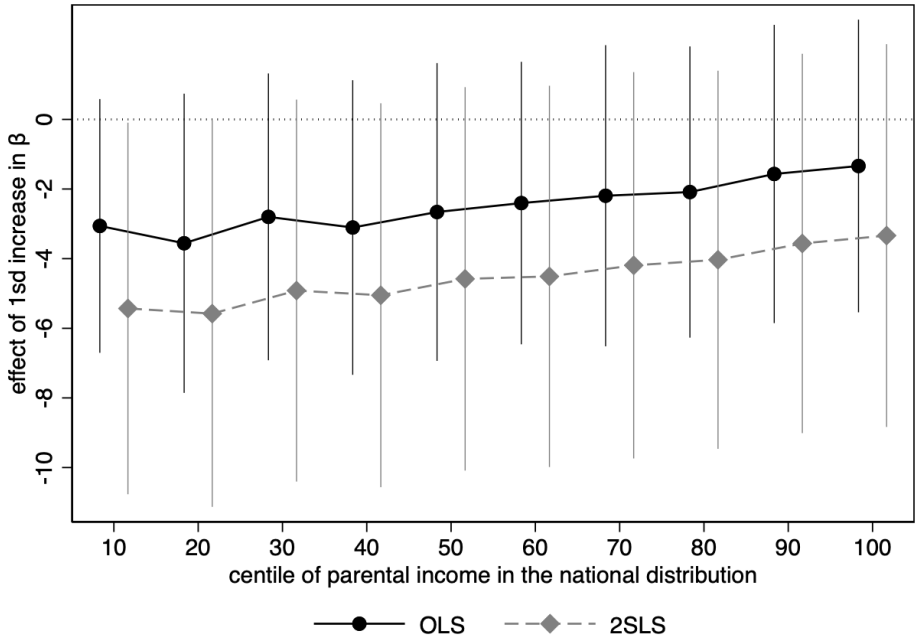
Note: Trends in average simulated and actual per pupil revenues, for districts in the top and bottom quartiles of the state's distribution of per pupil expenditure at the beginning of each sample. Vertical lines denote reform years. Simulated expenditures are calculated using the funding formula in place 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 Successful vs. Unsuccessful Reforms



Note: The figure shows OLS points estimates and 90 percent confidence intervals of the coefficients δ_n in the equation $m_{cb} = \sum_{n=-4}^{7} \delta_n R_{s(c)} \mathbb{1}(b+12-ryear_{s(c)} = n) + \theta_c + \tau_b + \varepsilon_{cv}$, where m_{cb} is the mean rank of children in CZ c, cohort b, and with parents' income on the 10th percentile in the national income distribution, $R_{s(c)}$ equals 1 if state s experienced a school finance reform in the years 1980-2004, $ryear_{s(c)}$ is the year of the first reform, 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 all the three groups. Standard errors are clustered at the state and cohort level.

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



Note: OLS (solid line) and 2SLS (dashed line) estimates and 90-percent confidence intervals for the coefficients δ_d in the regression $M_{cxb} = \sum_{d=1}^{10} \delta_d D_{d(cx)} \hat{\beta}_{s(c)b} + \kappa_c + \theta_{n(xc)} + \sigma_b + \omega_{cxb}$, where M_{cxb} is the average national income percentile of children with parents on the x percentile of the CZ 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 percentile x falls in decile d of the national distribution, $\theta_{n(xc)}$ are fixed effects for the parent percentile on 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 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
Median income					
1980	36417	11041	33961	18286	67924
1990	46552	17916	41249	18149	115499
2000	44018	15891	37500	17500	87500
2010	42974	16444	46250	14800	92500
Δ *exp, richest vs poorest district within state ($)*					
1986	3150	5855	2154	-1914	14162
1990	2501	4630	-593	-2306	12965
2000	-239	8696	-678	-16959	15415
2004	3140	7667	52	-7316	20334
Δ *exp, richest vs poorest district within CZ ($)*					
1986	1416	4695	541	-13816	13890
1990	1392	3570	673	-10710	14518
2000	102	5162	-274	-18060	19620
2004	2	5566	-236	-18794	19453

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)

Panel C: Measures of School Finance Equalization (β)

	All	No reform	Pre-Reform	Post-Reform	Difference
β	0.009	0.019	0.041	-0.004	-0.044***
	(0.063)	(0.098)	(0.027)	(0.034)	(0.006)
β^{sim}	0.017	0.026	0.040	0.003	-0.037***
	(0.058)	(0.090)	(0.030)	(0.031)	(0.007)

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 (6), estimated separately for each state and year using actual revenues (β) and simulated revenues (β^{sim}).

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

	(1)	(2)	(3)	(4)
β	-3.8397	-3.7174	-3.7667	-3.6370
	(2.1546)	(2.1259)	(2.2392)	(2.2195)
$\beta\times$ parent centile	0.0246***	0.0239***	0.0246***	0.0239***
	(0.0044)	(0.0044)	(0.0044)	(0.0044)
e_s			0.2472	0.2729
			(0.8907)	(0.8886)
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	3.593	3.478	3.520	3.398
10th [p-value]	[0.146]	[0.153]	[0.166]	[0.177]
25th	3.224	3.119	3.151	3.038
25th [p-value]	[0.184]	[0.194]	[0.207]	[0.220]
90th	1.622	1.562	1.549	1.482
90th [p-value]	[0.480]	[0.499]	[0.512]	[0.534]

Note: The table shows OLS estimates of the parameters δ_0 and δ in equation (9). The dependent variable is children's income percentile in the national distribution for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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	0.7526***	-12.6055*	0.7525***	-12.5859*
	(0.1204)	(6.2276)	(0.1203)	(6.2186)
β simulated $\times$ parent centile	0.0000	0.9908***	0.0000	0.9907***
	(0.0000)	(0.0163)	(0.0000)	(0.0166)
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	19.56		19.58	
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 (9). The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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	-5.8120*	-5.6920*	-5.6430*	-5.4974*		
	(2.8364)	(2.8014)	(2.6623)	(2.6271)		
$\beta\times$ parent centile	0.0253***	0.0244***	0.0253***	0.0244***		
	(0.0044)	(0.0044)	(0.0044)	(0.0044)		
e_s			0.1731	0.1993		
			(0.9230)	(0.9195)		
β simulated					-4.6927*	-4.5900*
					(1.9486)	(1.9184)
β simulated $\times$ parent centile					0.0250***	0.0241***
					(0.0044)	(0.0043)
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	5.559	5.448	5.390	5.253	4.443	4.349
10th [p-value]	[0.097]	[0.100]	[0.088]	[0.092]	[0.063]	[0.064]
25th	5.181	5.082	5.012	4.888	4.068	3.987
25th [p-value]	[0.117]	[0.120]	[0.106]	[0.111]	[0.082]	[0.084]
90th	3.539	3.497	3.370	3.302	2.443	2.418
90th [p-value]	[0.256]	[0.262]	[0.242]	[0.252]	[0.263]	[0.270]

Note: The table shows 2SLS second-stage estimates of the parameters δ_0 and δ in equation (9). The dependent variable is children's income percentile in the national distribution for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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	-0.1035	-0.1004	-0.1574*	-0.1541*
	(0.0566)	(0.0557)	(0.0761)	(0.0752)
$\beta\times$ 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	0.101	0.098	0.162	0.158
10th [p-value]	[0.138]	[0.144]	[0.095]	[0.098]
25th	0.089	0.086	0.149	0.146
25th [p-value]	[0.178]	[0.187]	[0.115]	[0.119]
90th	0.039	0.038	0.095	0.094
90th [p-value]	[0.515]	[0.532]	[0.269]	[0.275]

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 (9). The dependent variable is the natural logarithm of children's income for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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	-3.5210	-3.1433	-8.4582*	-8.0583*
	(2.6070)	(2.5024)	(4.0107)	(3.9468)
$\beta\times$ parent centile $\times$ reform in elementary school	0.0704**	0.0676**	0.0859***	0.0810***
	(0.0269)	(0.0235)	(0.0193)	(0.0169)
$\beta\times$ reform in middle school	-1.4628	-1.3803	-4.4117	-4.3311
	(1.7470)	(1.7809)	(2.9891)	(2.9831)
$\beta\times$ parent centile $\times$ reform in middle school	0.0237**	0.0240**	0.0241*	0.0241*
	(0.0087)	(0.0098)	(0.0105)	(0.0113)
$\beta\times$ reform in high school	-1.5429	-1.4843	-4.2956	-4.2679
	(1.7644)	(1.7677)	(3.3262)	(3.2903)
$\beta\times$ parent centile $\times$ reform in high school	0.0201**	0.0198**	0.0195**	0.0193**
	(0.0074)	(0.0076)	(0.0074)	(0.0076)
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	2.817	2.467	7.599	7.248
25th, elem	1.761	1.452	6.311	6.033
90th, elem	-2.816	-2.945	0.727	0.766
10th, high	1.342	1.286	4.101	4.075
25th, high	1.041	0.989	3.809	3.785
90th, high	-0.262	-0.298	2.545	2.529

Note: The table shows OLS (columns 1 and 2) and 2SLS second-stage estimates (columns 3 and 4) of the parameters in equation (10). The dependent variable is children's income percentile in the national distribution for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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 CZs' Income Inequality. 2SLS, Dependent Variable is Children's Income Percentile

	Low Inequality		High Inequality	
	(1)	(2)	(3)	(4)
β	-4.8634	-4.6557	-6.3731*	-6.4087*
	(3.2733)	(3.2213)	(3.1138)	(3.0938)
$\beta\times$ parent centile	0.0269**	0.0237**	0.0221***	0.0235***
	(0.0077)	(0.0072)	(0.0025)	(0.0031)
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	4.595	4.418	6.152	6.174
10th [p-value]	[0.208]	[0.218]	[0.096]	[0.093]
25th	4.191	4.062	5.821	5.822
25th [p-value]	[0.242]	[0.251]	[0.111]	[0.109]
90th	2.444	2.520	4.385	4.295
90th [p-value]	[0.470]	[0.458]	[0.210]	[0.215]

Note: The dependent variable is children's income percentile in the national distribution for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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 CZs' Income Segregation. 2SLS, Dependent Variable is Children's Income Percentile

	Low Segregation		High Segregation	
	(1)	(2)	(3)	(4)
β	-5.4864*	-5.4230*	-6.0725	-6.0227
	(2.6903)	(2.6721)	(3.2122)	(3.2019)
$\beta\times$ parent centile	0.0253***	0.0242***	0.0237***	0.0244***
	(0.0067)	(0.0065)	(0.0034)	(0.0037)
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	5.233	5.181	5.835	5.778
10th [p-value]	[0.098]	[0.099]	[0.120]	[0.122]
25th	4.853	4.819	5.479	5.411
25th [p-value]	[0.117]	[0.118]	[0.140]	[0.144]
90th	3.207	3.249	3.935	3.822
90th [p-value]	[0.265]	[0.262]	[0.274]	[0.287]

Note: The dependent variable is children's income percentile in the national distribution for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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: 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 (6), 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 X: School Finance Equalization and College Enrollment. 2SLS, Dependent Variable is Children's Probability of College Enrollment at Age 19

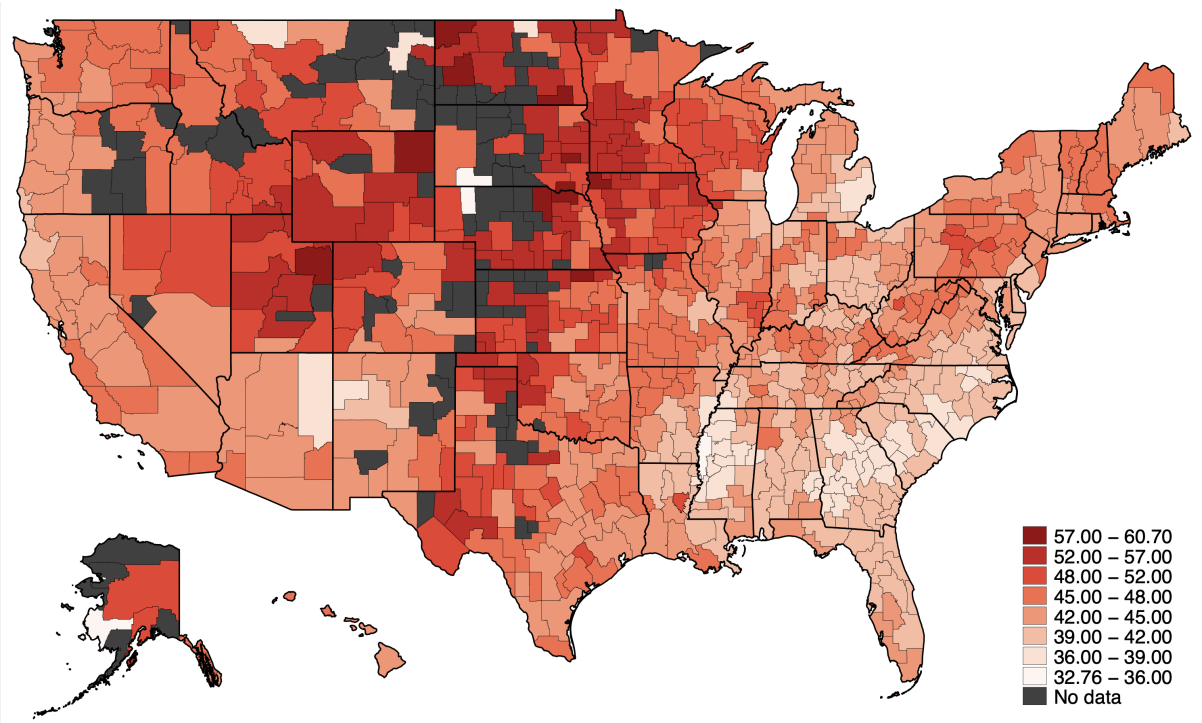
	(1)	(2)	(3)	(4)
β	-0.0777	-0.0746		
	(0.0971)	(0.0959)		
$\beta\times$ parent centile	0.0002**	0.0001		
	(0.0001)	(0.0001)		
$\beta\times$ reform in elementary school			-0.3359	-0.3286
			(0.1780)	(0.1757)
$\beta\times$ parent centile $\times$ reform in elementary school			-0.0000	-0.0001
			(0.0002)	(0.0002)
$\beta\times$ reform in middle school			-0.3402	-0.3333
			(0.1757)	(0.1740)
$\beta\times$ parent centile $\times$ reform in middle school			-0.0000	-0.0000
			(0.0001)	(0.0001)
$\beta\times$ reform in high school			-0.3677*	-0.3644*
			(0.1718)	(0.1691)
$\beta\times$ parent centile $\times$ reform in high school			0.0008***	0.0008***
			(0.0002)	(0.0002)
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.	0.556	0.556	0.556	0.556
10th	0.076	0.073		
25th	0.073	0.071		
90th	0.061	0.061		
10th, High School			0.360	0.356
25th, High School			0.347	0.344
90th, High School			0.294	0.289

Note: The dependent variable is the probability of college enrollment by age 19 for each parental income percentile in the distribution of each CZ, for cohorts 1984 to 1990. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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.

Online Appendix

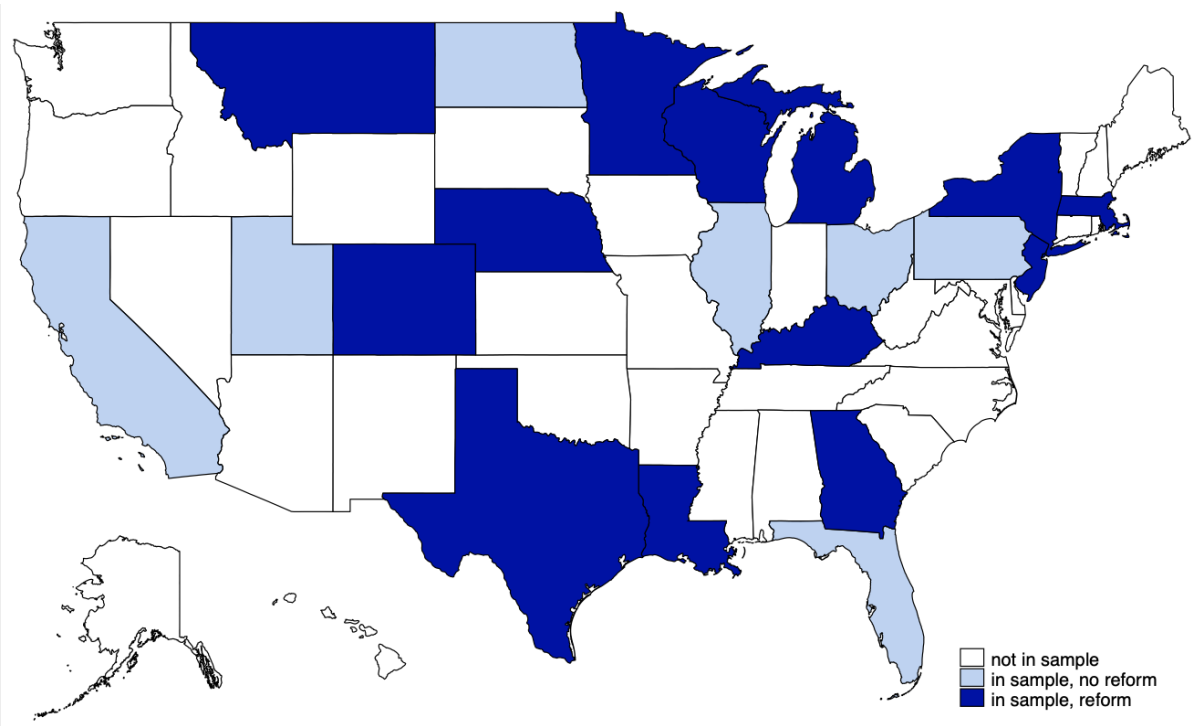
Appendix A Additional Figures and Tables

Figure AI: Intergenerational Mobility Across US Commuting Zones: Expected Income Percentile for Children with Parents on the 25th Percentile, Cohorts 1980-1986



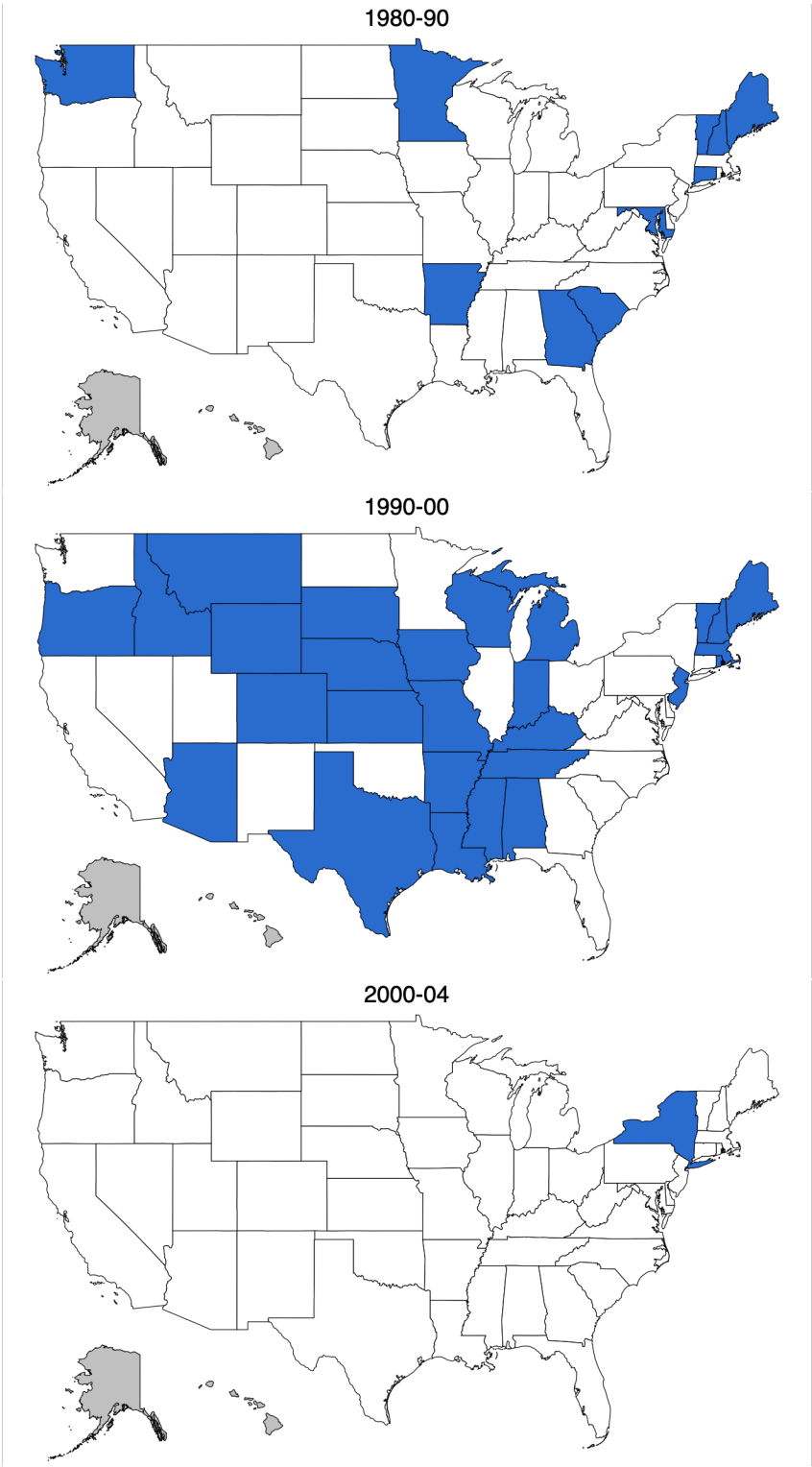
Note: Mean income percentile of children born between 1980 and 1986 with parents on the 25th percentile. Each shaded area corresponds to a CZ. Weighted average across cohorts with number of children used as weights.

Figure AII: US States Included In The Estimation Sample With And Without A Reform, And States Not Included



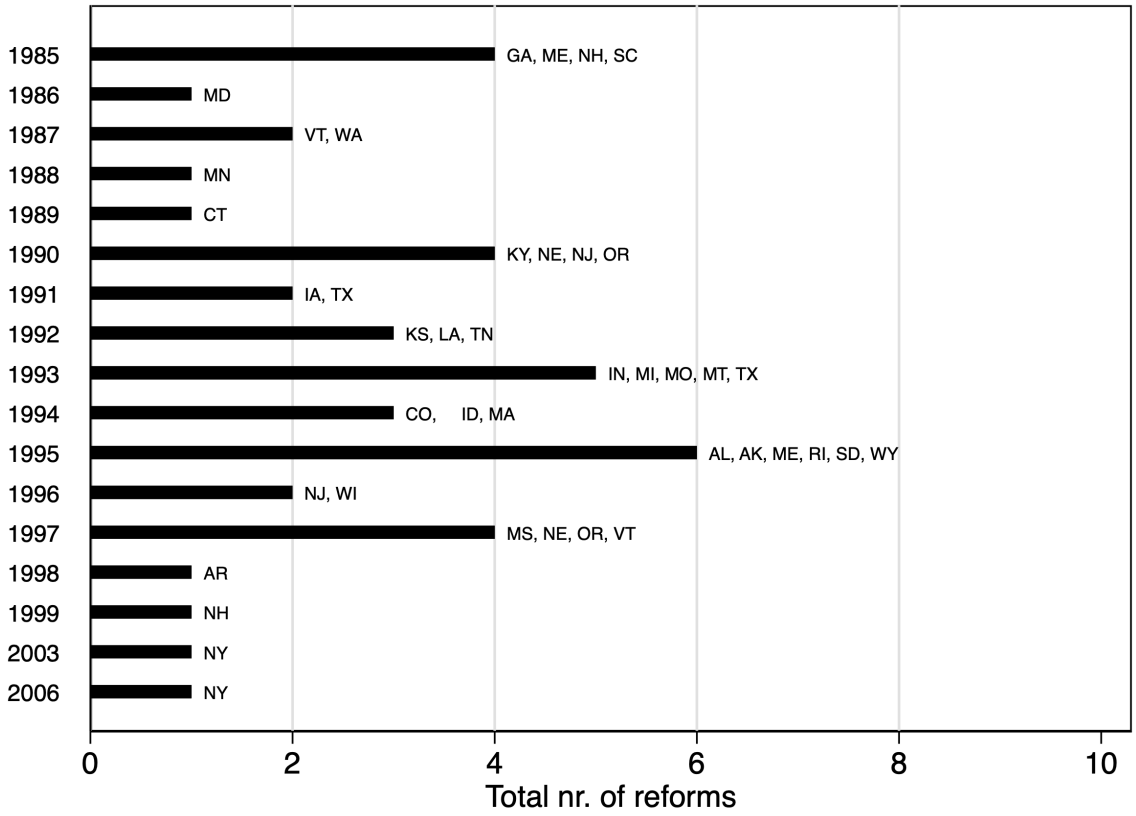
Note: The figure shows states included in the analysis sample with a reform and without a reform, as well as states not included in the analysis. The first group includes Colorado, Florida, Georgia, Kentucky, Louisiana, Massachusetts, Michigan, Minnesota, Montana, Nebraska, New Jersey, New York, Pennsylvania, Texas, and Wisconsin. The second includes California, Florida, Illinois, North Dakota, Ohio, Pennsylvania, and Utah. The third includes all remaining states.

Figure AIII: US States with School Finance Equalization Reforms, 1980-2010



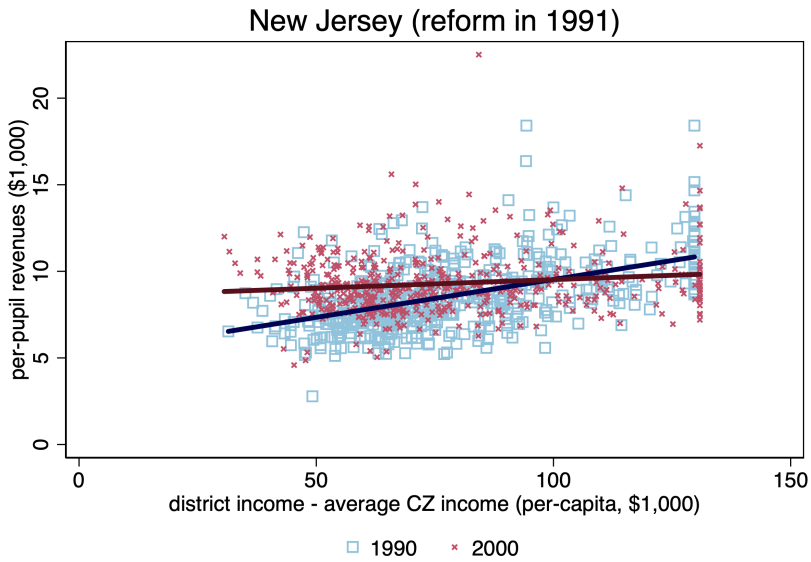
Note: Shaded areas denote states which passed a school finance equalization reform during each time period. Alaska and Hawaii (excluded from the estimation sample) had one (1987, a revision of its school finance foundation program) and zero reforms in this time period, respectively. Source: "Public School Finance Programs of United States and Canada" (1990-1991 and 1998–1999), Verstegen and Jordan (2009), Jackson et al. (2015), and Lafortune et al. (2018).

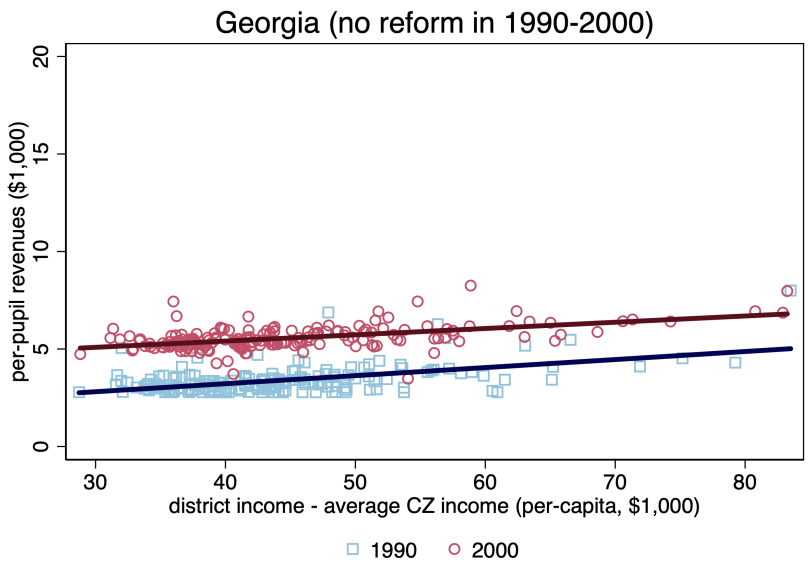
Figure AIV: School Finance Reforms Over Time



Note: Number of school finance reforms by year.

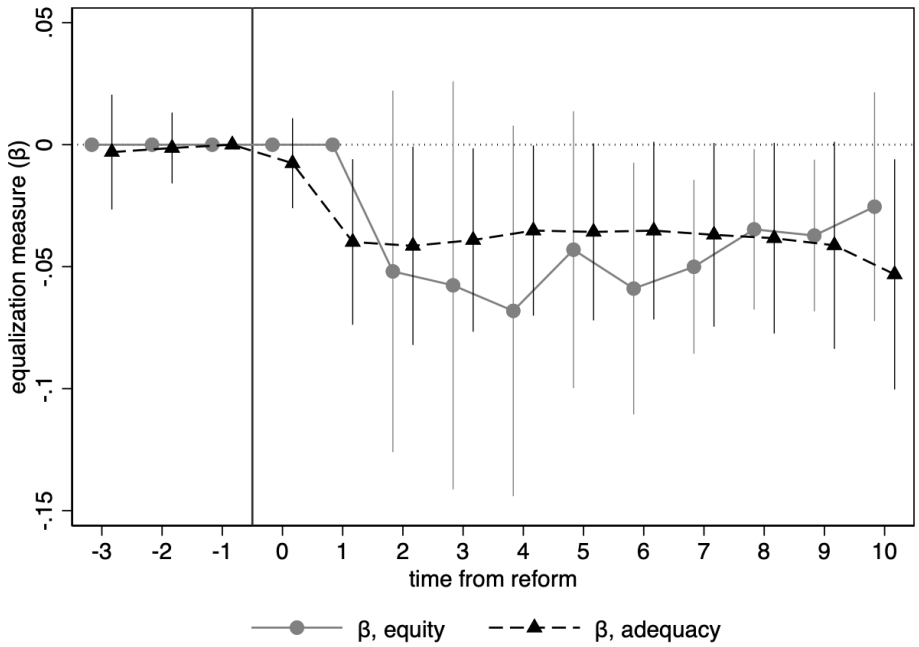
Figure AV: Per-pupil Revenues and Per-capita Income in New Jersey and Georgia, 1990 and 2000





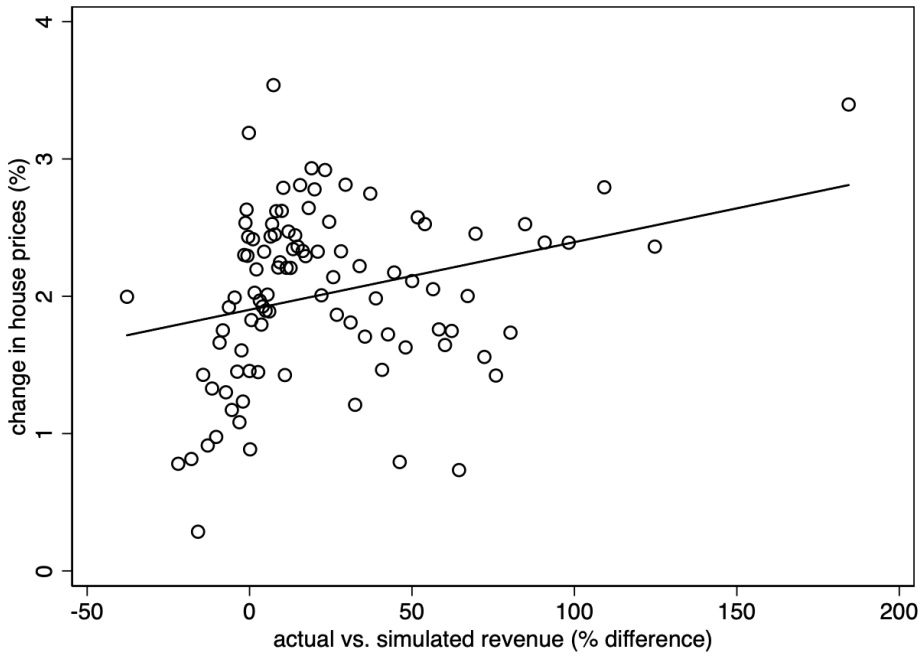
Note: Per-pupil revenues (y-axis) and per-capita income (x-axis) in 1990 and 2000, in New Jersey (which had a reform in 1991) and Georgia (which did not have a reform between 1990 and 2000). Each observation is a school district.

Figure AVI: Event Study of Equalization Measure β Around A School Finance Reform: "Equity" Reforms (passed before 1990) and "Adequacy" Reforms (passed after 1990)



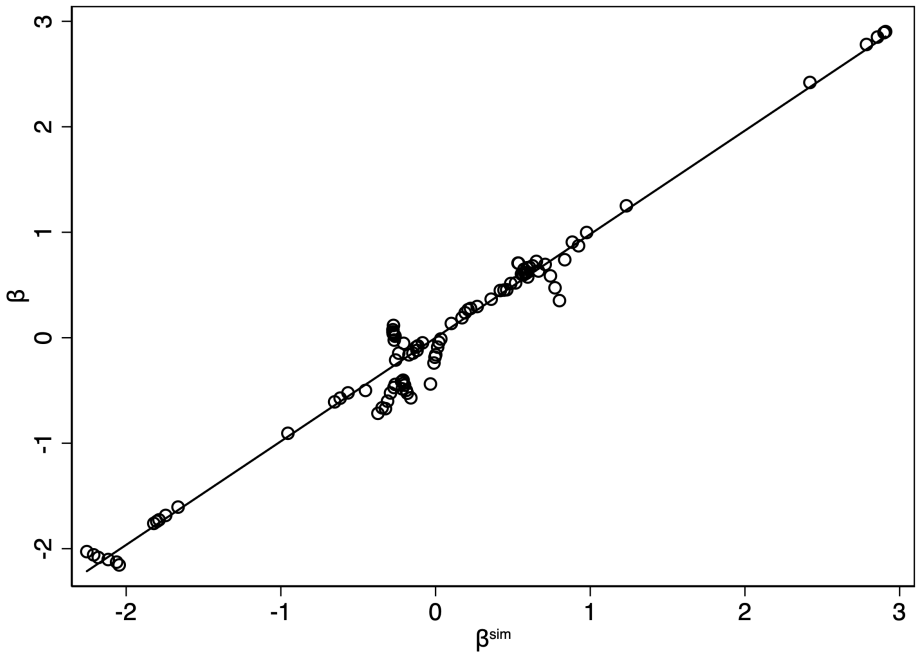
Note: Point estimates and 90 percent confidence intervals for the coefficients δ_k in regression $\beta_{st} = \sum_k \delta_k R_s 1(t - ryear_s = k) + \varepsilon_{st}$,where β_{st} is the slope coefficient in equation (6), estimated separately for each state s and year t from 1986 to 2004, R_s equals 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. Estimates are obtained and shown separately for reforms passed before or in 1990 ("equity", solid line) and for reforms passed after 1990 ("adequacy", dashed line). 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 AVII: Change in House Prices and Difference Between Actual vs. Simulated Revenues



Note: Binned scatterplot of the annual percentage change in zip code-level annual house price indexes (y-axis) and the percentage difference between actual and simulated revenues in the corresponding school district (x-axis). Each dot corresponds to a percentile in the distribution of the percentage difference between actual and simulated revenues. Annual house price index data are taken from the Federal Housing Finance Agency, and cover years 1986 to 2004.

Figure AVIII: First Stage: Correlation Between β and β_s



Note: Binned scatterplot of β (vertical axis) and β^{sim} (horizontal axis).

Table AI: Differences Between US States Included in The Sample and Other States

	(1) In sample	Not in sample	Difference
population (2000 Census)	3287992.9	9375981.8	-6087988.8***
			(1642271.0)
in urban area	0.52	0.53	-0.014
			(0.080)
racial segregation	0.13	0.15	-0.017
			(0.018)
income segregation	0.045	0.047	-0.0023
			(0.0065)
school expenditure in 1996 ($1,000)	6.16	6.33	-0.16
			(0.33)
Gini coefficient	0.42	0.41	0.012
			(0.016)
crime rate	0.0017	0.0014	0.00034
			(0.00027)
share single mothers	0.21	0.20	0.014
			(0.011)
share divorced	0.10	0.093	0.0087**
			(0.0037)

Note: The table shows means and differences in means in a set of state-level variables between US states included in the analysis sample (California, Colorado, Florida, Georgia, Illinois, Kentucky, Louisiana, Massachusetts, Michigan, Minnesota, Montana, Nebraska, New Jersey, New York, North Dakota, Ohio, Pennsylvania, Utah, Texas, and Wisconsin) and all the other states. The variables are defined as in Figure VII of Chetty et al. (2014).

Table AII: Simulated Instrument, House Prices, and Migration: OLS, Dependent Variable is β Simulated

	β simulated		
	(1)	(2)	(3)
avg change in house prices	-0.0514	0.0036	0.0210
	(0.0613)	(0.0752)	(0.0664)
in-migration rate		0.0283	-0.1949
		(0.8690)	(1.0835)
out-migration rate		-0.1727	-1.4894
		(0.8781)	(1.0416)
income in-migrants/ income incumbents			-0.0997
			(0.7614)
income out-migrants/ income incumbents			1.1085
			(0.8922)
State FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N (state $\times$ year)	289	247	223
F-stat of joint significance	0.703	0.018	0.513

Note: The dependent variable is β *simulated*, estimated as β in equation (6) using simulated revenues instead of actual revenues. The variable *avg change in house prices* represents the average change in the house price index in each state and year. The variables *in-migration rate* and *out-migration rate* are ratios of the number of in-migrants and out-migrants in a county, respectively, and the county's population; these rates are averaged across all counties in a state and year using population weights. The variables *income in-migrants/ income incumbents* and *income out-migrants/ income incumbents* are ratios of incomes of in-migrants and out-migrants of a county and the incomes of the county's incumbent residents, also averaged across all counties in a state and year using population weights. All specifications include state and year fixed effects. Standard errors in parentheses are clustered at the state level. *** p<0.01, ** p<0.05, * p<0.1.

Table AIII: 2SLS Estimates of Equalization On Intergenerational Mobility Using Jackson, Johnson, Persico (2015) IV Approach: First Stage

	Approach 1		Approach 2	
	(1)	(2)	(3)	(4)
	β	$\beta\times$ parent centile	β	$\beta\times$ parent centile
β (IV, approach 1)	0.8969***	-4.1167		
	(0.0374)	(3.5634)		
β (IV, approach 1) $\times$ parent centile	0.0000	0.9722***		
	(0.0000)	(0.0505)		
β (IV, approach 2)			1.0759***	-4.8650
			(0.0565)	(4.6855)
β (IV, approach 2) $\times$ parent centile			-0.0000	1.1621***
			(0.0000)	(0.0367)
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	287.15		286.70	
N (CZ $\times$ parent cent. $\times$ cohort)	12924	12924	5886	5886

Note: The table shows the first stage of the 2SLS estimation of the parameters δ_0 and δ in equation (9). The variable β is the OLS estimate of the slope coefficient in equation (6), 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 variables *β (IV, approach 1)* and *β (IV, approach 2)* are estimated as β using the instruments for revenues developed by Jackson, Johnson, and Persico (2015). In this first stage, the variables *β (IV, approach 1 or 2)* and *β (IV, approach 1 or 2) $\times$ 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 AIV: 2SLS Estimates of Equalization On Intergenerational Mobility Using Jackson, Johnson, Persico (2015) IV Approach: Second Stage

	Approach 1		Approach 2	
	(1)	(2)	(3)	(4)
β	-3.1625	-3.1648	-2.2985	-2.2859
	(2.3397)	(2.3291)	(2.3482)	(2.3205)
betacent	0.0247***	0.0236***	0.0256***	0.0249***
	(0.0052)	(0.0050)	(0.0068)	(0.0069)
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)	12924	12924	5886	5886
10th	2.916	2.928	2.043	2.037
10th [p-value]	[0.228]	[0.224]	[0.410]	[0.407]
25th	2.545	2.574	1.660	1.662
25th [p-value]	[0.290]	[0.282]	[0.501]	[0.497]
90th	0.940	1.038	-0.002	0.041
90th [p-value]	[0.694]	[0.662]	[0.999]	[0.987]

Note: The dependent variable is children's income percentile for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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 using a version of the same β calculated using the instrument for school revenues developed by Jackson, Johnson, and Persico (2015) (their Approach 1 is shown in columns 1 and 2, their Approach 2 is shown in columns 3 and 4). 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 AV: Heterogeneous Effects of School Finance Equalization by CZs' Income Inequality. OLS, Dependent Variable is Children's Income Percentile

	Low Inequality		High Inequality	
	(1)	(2)	(3)	(4)
β	-3.2779	-3.1455	-4.0853	-3.8953
	(2.9337)	(2.9293)	(2.3074)	(2.2531)
$\beta\times$ parent centile	0.0273**	0.0249**	0.0219***	0.0220***
	(0.0089)	(0.0083)	(0.0026)	(0.0034)
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	3.005	2.897	3.867	3.676
10th [p-value]	[0.344]	[0.359]	[0.144]	[0.155]
25th	2.595	2.524	3.539	3.346
25th [p-value]	[0.407]	[0.419]	[0.174]	[0.190]
90th	0.821	0.909	2.117	1.919
90th [p-value]	[0.789]	[0.766]	[0.388]	[0.439]

Note: The dependent variable is children's income percentile in the national distribution for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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. 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 AVI: Heterogeneous Effects of School Finance Equalization by CZs' Income Segregation. OLS, Dependent Variable is Children's Income Percentile

	Low Segregation		High Segregation	
	(1)	(2)	(3)	(4)
β	-3.7674	-3.7683	-3.8799	-3.6842
	(2.3266)	(2.3165)	(2.2818)	(2.2140)
$\beta\times$ parent centile	0.0255**	0.0256***	0.0235***	0.0228***
	(0.0073)	(0.0068)	(0.0037)	(0.0046)
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	3.512	3.513	3.644	3.456
10th [p-value]	[0.180]	[0.179]	[0.161]	[0.171]
25th	3.129	3.129	3.291	3.114
25th [p-value]	[0.224]	[0.223]	[0.199]	[0.215]
90th	1.469	1.468	1.761	1.631
90th [p-value]	[0.551]	[0.552]	[0.472]	[0.513]

Note: The dependent variable is children's income percentile in the national distribution for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), 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. 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. 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 AVII: School Finance Equalization and Intergenerational Mobility. 2SLS, Dependent Variable is Children's Income Percentile. CZs With and Without A State Border

	Without border		With border	
	(1)	(2)	(3)	(4)
β	-5.7181	-5.4664	-5.4032**	-5.5626**
	(3.1547)	(3.1143)	(1.8942)	(2.0034)
$\beta\times$ parent centile	0.0270***	0.0239***	0.0185***	0.0217***
	(0.0051)	(0.0051)	(0.0048)	(0.0050)
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)	187506	187506	36531	36531
10th	5.448	5.228	5.218	5.346
10th [p-value]	[0.134]	[0.144]	[0.033]	[0.036]
25th	5.043	4.870	4.941	5.020
25th [p-value]	[0.160]	[0.170]	[0.039]	[0.043]
90th	3.289	3.318	3.741	3.611
90th [p-value]	[0.336]	[0.334]	[0.093]	[0.108]

Note: The dependent variable is children's income percentile for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), computed separately for each state and cohort, and standardized across all states and cohorts. The variable *parent centile* is the percentile of parents on the national income distribution. All the 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. "Without border" refers to CZs entirely belonging to one state, and "With border" refers to CZs belonging to two or more states. 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 AVIII: School Finance Equalization and Intergenerational Mobility. OLS and 2SLS, Dependent Variable is is Children's Income Percentile. No Imputation of Income for Intercensal Years

	OLS	First stage		2SLS
	(1)	(2)	(3)	(4)
β	-5.3940**			-6.4071**
	(2.0738)			(2.0071)
$\beta\times$ parent centile	0.0233***			0.0240***
	(0.0048)			(0.0049)
β simulated		0.8078***	-9.7646	
		(0.0981)	(5.2412)	
β simulated $\times$ parent centile		0.0000	0.9899***	
		(0.0000)	(0.0190)	
Parent centile FE	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes	Yes
N (CZ $\times$ parent cent. $\times$ cohort)	13578	13578	13578	13578
Kleibergen-Paap Wald F-stat		33.980		
10th	5.161			6.167
10th [p-value]	[0.046]			[0.021]
25th	4.811			5.808
25th [p-value]	[0.057]			[0.027]
90th	3.294			4.251
90th [p-value]	[0.153]			[0.075]

Note: The table shows OLS estimates (columns 1 and 2) as well as the 2SLS first stage (column 3) and second stage (columns 4 and 5) estimates of the parameters δ_0 and δ in equation (9). The dependent variable is children's income percentile for each parental income percentile in the distribution of each CZ, for cohorts 1980 to 1986. The variable β is the OLS estimate of the slope coefficient in equation (6), computed separately for each state and cohort using income values from 1990 for all years (instead of the imputation procedure described in the text), and standardized across all states and cohorts. The variable *parent centile* is the percentile of parents in the national income distribution. In columns 2-4, the variable β is instrumented using β *simulated*, estimated as β using simulated revenues instead of actual revenues and income values from 1990 for all years; columns 2 and 3 show the 2SLS first stage, and column 4 shows the second stage. All specifications include CZ, parent percentile, and cohort fixed effects. 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 AIX: School Finance Equalization and College Enrollment. OLS, Dependent Variable is Children's Probability of College Enrollment at Age 19

	(1)	(2)	(3)	(4)
β	-0.0949**	-0.0884*		
	(0.0372)	(0.0366)		
$\beta\times$ parent centile	0.0002**	0.0002*		
	(0.0001)	(0.0001)		
$\beta\times$ reform in elementary school			-0.1300***	-0.1218***
			(0.0115)	(0.0127)
$\beta\times$ parent centile $\times$ reform in elementary school			0.0001	0.0000
			(0.0002)	(0.0002)
$\beta\times$ reform in middle school			-0.1387***	-0.1307***
			(0.0054)	(0.0073)
$\beta\times$ parent centile $\times$ reform in middle school			0.0001	0.0001
			(0.0001)	(0.0001)
$\beta\times$ reform in high school			-0.1849***	-0.1802***
			(0.0207)	(0.0203)
$\beta\times$ parent centile $\times$ reform in high school			0.0008***	0.0009***
			(0.0002)	(0.0002)
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.	0.556	0.556	0.556	0.556
10th	0.093	0.087		
25th	0.090	0.084		
90th	0.076	0.073		
10th, High School			0.176	0.172
25th, High School			0.164	0.159
90th, High School			0.109	0.103

Note: The dependent variable is the probability of college enrollment by age 19 for each parental income percentile in the distribution of each CZ, for cohorts 1984 to 1990. The variable β is the OLS estimate of the slope coefficient in equation (6), 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 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. 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.

Appendix B Construction of the dataset

Income

I use tabulations of household income at the school district level from the US Census of Population and Housing for the years 1980, 1990 and 2000, and from the American Community Survey 5-year estimates (2008-2012) for the year 2010. Income tabulations at the school district level are contained in the Census STF3F file for 1980, and published as part of the School District Demographic System for the years 1990, 2000, and 2010. Income data at the district level is reported in the form of tabulations of the counts of households in 17, 25, 16 and 16 income bins in each school district in 1980, 1990, 2000 and 2010 respectively. To calculate median income from these tabulations, I assume a uniform distribution of households in each bin, and I assign each district the level of income of the class containing the median household. I winsorize the top and bottom 1 percent of observations in the distribution of each year. In the final sample, median income is available for 15,960, 15,272, 14,373, and 13,576 districts in 1980, 1990, 2000, and 2010 respectively.

Actual and Simulated School Revenues

To obtain actual and simulated revenues for each district, I have collected data from each state's Department of Education on the variables entering the school funding formula in each available year between 1986 and 2004. These variables include, but are not limited to, assessments of property values, property tax rates, income, measures of enrollment (such as full-time-equivalents or average daily membership/attendance, weighted by type of students or unweighted). Detailed information on the variables used in each formula is contained in Table CI. I successfully obtained this information for the following states (years): California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Montana (1994-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004), and for a total of 8,102 school districts.

After collecting data on the formula variables from each state, I constructed the funding

formula preceding and following each school finance reform, which allows to calculate total revenue as a function of the various variables and of formula parameters set by each state. These formulas are described in Appendix D.

Equalization measures

To estimate state-year specific β as in equation 6, I match per pupil revenues data with median district income data. I assign state-year estimates to states and cohorts using the timing of each reform, with the procedure described in the paper. I also match districts to CZs using the 1990 county-CZ crosswalk provided by Chetty et al. (2014) and Chetty et al. (2014) and information on districts' counties provided by the NCES Common Core of Data.

To construct the simulated β^{sim}, instrument for β, I simulate post-reform revenues keeping endogenous variables, such as property values and income, at their levels at the time of passage of each reform. I adjust property values using the US Annual Price Index (calculated by the Federal Housing Finance Authority using a repeated-sales method). Third, I estimate the simulated β^s using simulated revenues and median income at the district level, for each Census year as well as for the first and last year for which simulated revenues are available. I then impute β^{sim} to each school year using the same procedure described in the text for the imputation of β. To maximize the size of the sample, I set $\beta^{sim} = \beta$ for all states without a school finance reform in the years 1986-2004, which include California, Florida, Georgia, Illinois, New York, North Dakota, Ohio, Pennsylvania, and Utah. β^s is available for a total of 327 CZs with non-missing mobility information.

Intergenerational Mobility

Chetty et al. (2014) (Online Table 1) calculate and report intergenerational mobility measures, separately for each cohort of children and for each of the 637 out of 722 CZs in the US, using individual-level data from IRS tax records (estimates are not available for CZs with a very low number of children). These measures include the intercept and the slope of the linear relationship between parents' and children's income ranks (on the national income distribution of parents' income and children's income, respectively), separately for each CZ and for cohorts 1980-86.[50] Parents' income is calculated as the average yearly income in years 1996-2000, mea-

[50] A commuting zone is defined by the Census Bureau and the United States Department of Agriculture as "[...] a geographic unit that better captures the economic and social diversity of non-metro areas." For confidentiality issues, mobility measures are not disclosed for 13 CZs with less than 250 children.

sured in 2010 dollars. Children's income is calculated as the average yearly income in years 2010, 2011 and 2012, measured in 2010 dollars.[51] Each child is matched to his or her parent (or parents), i.e. the taxpayer who claimed him or her as dependent when he or she was age 25 or younger in IRS tax records covering the period 1996-2011.[52] Matched parent-child pairs are assigned to a CZ based on the earliest non-missing zip code reported on the tax form of the parent. The sample is restricted to children of parents with non-missing zip codes and non-negative income. The final sample of children includes nearly 24 million US citizens born in the period 1980-1986.[53]

My measure of mobility is children's national income rank by percentile of parental income in the distribution of each CZ income distribution of parents, separately for each CZ and cohort. I construct these measures as follows. Chetty et al. (2014) (Online Data Table 7) report the parents' income distribution for each CZ. Specifically, they report income levels corresponding to the 10th, 25th, 50th, 75th, 90th, and 99th percentile in each CZ. I match income levels for each of these CZ percentiles with the corresponding percentiles in the national distribution. I then use the slope and intercept of the linear relationship between parents' and children's national income ranks to back out the national income rank of the child, for each of these parental income percentiles in each CZ, for each CZ, and for each cohort born between 1980 and 1986. Assuming that the income distributions did not change over time (and across cohorts) in each CZ, this procedure allows to approximate the distribution of income for children in each CZ and birth cohort, given each parent's income percentile on the national distribution.

[51] See Chetty et al. (2014) for a detailed description of the income definitions used to compute intergenerational mobility measures.

[52] If an individual was claimed as dependent by more than one taxpayer, he or she is considered as the dependent of the taxpayer who claimed him or her in the earliest year.

[53] Differently from Chetty et al. (2014) who base their analysis of income mobility on a "core sample" of children born in 1980 and 1981, my sample also includes younger children. As explained by Chetty et al. (2014), measuring children's income at early ages can overestimate mobility with respect to lifetime income, because children with high lifetime incomes have steeper earnings profiles when young (which stabilize around age 30). Children in the younger cohort in my income mobility sample (born in 1986) are 26 in 2012. The measurement error generated by the inclusion of the younger cohorts, however, should be quite limited (see Chetty et al., 2014, Figure IIIA). In addition, younger cohorts are more likely to be correctly matched to the the zip code where they grew up.

Appendix C Using The Instrumental-Variables Approach of Jackson, Johnson, and Persico (2015)

To compare my simulated instruments estimation strategy with the instrumental variables approach of Jackson et al. (2015), I estimate a version of my main equation of interest (equation (9)) using their approach. To do this, I obtain instruments for actual school district revenues following their approaches 1 and 2 (described in Jackson et al., 2015, pages 171-179). The two approaches are described below.

1. Approach 1 consists in estimating the parameters of the following equation via OLS:

$$e_{dt} = \sum_{q=1}^{4} \sum_{k} \delta_{qk} \mathbb{1}(Q_d^e = q) \mathbb{1}(t - ryear_d = k) + \theta_d + \tau_t + \omega_{dt}$$

   where e_{dt} are actual revenues of school district d in year t, Q_d^e is the quartile of district d in the state distribution of district revenues in 1980, $ryear_d$ is the year in which the first school finance reform was passed between 1986 and 2004, θ_d are district fixed effects, and τ_t are year fixed effects. This equation is analogous to equation (3) of Jackson et al. (2015) (page 172). Using OLS estimates of this equation, I predict $\tilde{e}_{dt}$ for each d and t, and I construct an instrument $\tilde{\beta}_{st}$ as the estimate of the slope coefficient in the equation $\tilde{e}_{dt} = \alpha_{st} + \tilde{\beta}_{st} y_{dt} + \varepsilon_{dt}$. I then assign $\tilde{\beta}_{st}$ to each cohort depending on the state and the year in which the cohort was in grades 1 to 12, and I use $\tilde{\beta}$ as an instrument for β in equation (9). The first stage estimates are shown in Table AIII (columns 1 and 2); the second stage estimates are shown in Table AIV (columns 1 and 2).

2. Approach 2 consists in estimating the parameters of the following equation via OLS:

$$\begin{aligned} e_{dt} &= \sum_{q=1}^{4} \sum_{k} \delta_{qk} \mathbb{1}(Q_d^e = q) \mathbb{1}(t - ryear_d = k) \\ &+ \sum_{q=1}^{4} \sum_{k} \sum_{r} \delta_{qkr} \mathbb{1}(Q_d^y = q) \mathbb{1}(t - ryear_d = k) \mathbb{1}(Type_d = r) + \theta_d + \tau_t + \omega_{dt} \end{aligned}$$

   where Q_d^y is the quartile of district d in the state distribution of district median income in 1980, $ryear_d$ is the year in which the first school finance reform was passed between 1986 and 2004, $Type_d$ is a vector of indicators for the type of reform (I use the same classification as Jackson, Johnson and Persico and classify reforms into foundation, equalization,

revenue limit, adequacy, and reward for effort; a reform can be of more than one type), θ_d are district fixed effects, and τ_t are year fixed effects. This equation is analogous to equation (4) of Jackson et al. (2015) (page 178). For each district d, I estimate this equation via OLS using data for all the other states, and I then use these estimates to predict revenues for district d, which I define as $\tilde{\tilde{e}}_{dt}$. I then construct the instrument $\tilde{\tilde{\beta}}_{st}$ as the estimate of the slope coefficient in the equation $\tilde{\tilde{e}}_{dt} = \alpha_{st} + \tilde{\tilde{\beta}}_{st} y_{dt} + \varepsilon_{dt}$. Lastly, I assign $\tilde{\tilde{\beta}}_{st}$ to each cohort depending on the state and the year in which the cohort was in grades 1 to 12, and I use $\tilde{\tilde{\beta}}$ as an instrument for β in equation (9). The first stage estimates are shown in Table AIII (columns 3 and 4); the second stage estimates are shown in Table AIV (columns 3 and 4).

Appendix D School Finance Equalization Reforms

California

The school finance plan in place in 1986 in California is the product of the *Serrano* vs. *Priest* lawsuit, and the passage of Proposition 13 (1978), which limited property tax rates to 1% of assessed property value. The passage of Proposition 98 in 1988 slightly modified the funding scheme, by earmarking a fixed minimum percentage of the state budget to education. After these changes, control of school finance has been shifted more and more to the state. State aid is distributed through a foundation plan. The foundation base, called Revenue Limit, is based on historical revenues adjusted by the cost of living, with increases inversely related to the level of revenues. The formula, although very complicated, can be summarized as follows:

$$\begin{aligned} R &= \max\{\max\{2,400,400 \times n\}, \max\{\text{RL} - 0.01p\}\} + 0.01p \\ RL &= \bar{RL}_{-1} \times CODB \end{aligned}$$

where RL is the revenue limit, $\bar{RL}_{-1}$ is the average of previous year's revenue limit, $CODB$ is the cost of doing business, proxy for the cost of living, and p is property value.

Colorado

Until 1993, Colorado had a Guaranteed Tax Base formula with a fixed tax rate. Local revenues came from property taxes as well as from appropriations of revenues from an ownership tax on all registered vehicles. The formula was as follows:

$$\begin{aligned} R &= \min\{t * max\{p, B\} + t * 10, ARB\} \\ \text{where } t &= \text{tax rate in district} = 1\% \text{ fixed (collected and redistributed at county/city level)} \\ R^o &= \text{per-pupil revenues from ownership tax base} \\ B &= \text{minimum guaranteed tax base, comes from the state} \\ ARB &= \text{authorized revenue base} \end{aligned}$$

The Public School Finance Act of 1994 changed the formula to a foundation plan. The foundation amount is determined by the Per-Pupil-Revenue and it is district-specific, to account for differences in the cost of living in the number of at-risk children. The formula in place between 1994 and 2004 is as follows:

$$
\begin{aligned}
R &= t * p + \max\{0, PPR - t * p - R^o\} \\
\text{where } t &= \text{tax rate in district } = 1\% \text{ (fixed)} \\
R^o &= \text{per-pupil revenues of ownership tax base} \\
p^o &= \text{per-pupil ownership tax base} \\
PPR &= \text{per-pupil revenue, function of "base" and cost of living, as well as number of "at risk" childre}
\end{aligned}
$$

Florida

Florida's school funding scheme in the years 1988-2004 involved a combination of a Foundation Grant and a Guaranteed Tax Base. The formula was as follows:

$$
\begin{aligned}
R &= f * cost\_diff + \max\{t - \bar{t}, 0\} * p \\
\text{where } t &= \text{tax rate in district} \\
t - \bar{t} &\leq 0.0005 \\
\bar{t} &= \text{required tax rate (decided by the state)} \\
\bar{t} * p &\leq 0.9 * f \\
p &= \text{per-pupil property value} \\
f &= \text{foundation grant (\$3,223.06 in 1998-1999)} \\
cost\_diff &= \text{cost of living adjustment}
\end{aligned}
$$

Georgia

Georgia's school finance plan for the years 1987-2004 was introduced in 1985 as part of the Quality Basic Education program. It involves a Foundation grant and a Required Local Effort

component. The formula is as follows:

$$
\begin{aligned}
R &= f + t1 * p + t2 * \max\{p90, p\} + t3 * p \\
& \quad \text{such that } t1 + t2 + t3 \leq 0.02 \\
\text{where } f &= \text{foundation grant base amount (\$2038.74 in 1998-99)} \\
t1 &= \text{compulsory local effort (5 mills)} \\
t2 &= \text{optional additional effort subject to equalization (max. 3.25 mills)} \\
t3 &= \text{optional additional effort in addition to 5 + 3.25 mills}
\end{aligned}
$$

Illinois

The school finance plan in place in 1986 had been implemented in 1980. The funding formula has three tiers: Foundation, Alternate Method and Flat Grant. Per pupil property wealth in each district determines which formula must be used to compute the funding. The state aid formula compares the district valuation to a guaranteed wealth per ADA. The guaranteed level varies by the type of school district: in 1999 it was equal to $188,478 for elementary districts, $361,250 for secondary districts, and $144,500 for unit districts. Districts qualifying under the Foundation formula have per pupil valuation less than 93% of the foundation level. Districts qualifying under the Alternate Method formula have per pupil valuation of at least 93% but less than 175% of the foundation level. Districts qualifying under the Flat Grant formula have per pupil valuation greater than 175% of the foundation level. The foundation level was $4,225 in 1999, the flat grant was $218. The formula can be summarized as follows:

$$
\begin{aligned}
R &= \text{Aid} + \tau * p * n \\
\text{Aid} &= \text{Foundation or AM or FG} \\
\text{Foundation} &= n(f - \text{Local Resources}) \\
\text{AM} &= nf[0.07 - ((\text{Local Percentage} - 0.93)/0.82)0.02] \\
\text{FG} &= n * 218 \\
\text{Local Resources} &= np_i\hat{\tau} + \text{CPPRT}/n \\
\text{Local Percentage} &= 100 \times \text{Local Resources}/f
\end{aligned}
$$

where τ is the property tax rate, p is per pupil property valuation, n is the weighted count of pupils, f is the foundation level, $\hat{\tau}$ is equal to 2.3% for elementary districts, 1.2% for secondary districts and 3.0% for unit districts, and CPPRT denotes the Corporate Personal Property Replacement Taxes.

Kentucky

Kentucky changed its school finance plan in 1990, with the Kentucky Education Reform Act (KERA). The post-reform plan is a mix between a Foundation plan and a Power Equalization. The formula is as follows:

$$
\begin{aligned}
R &= t * p + t2 * \max\{\bar{p}, p\} + \max\{f - t * p - t2 * \max\{\bar{p}, p\}, 0\} + t3 * p \\
\text{where } t &= \text{tax rate, compulsory effort and fixed at 0.003} \\
t2 * \max\{\bar{p}, p\} &\leq 0.15 * f \\
p &= \text{property valuation per pupil} \\
t2 &* \max\{\bar{p}, p\} \leq 0.3 * f \\
\bar{p} &= \text{level of guaranteed tax base, 1.5 * average state} \\
t2 &= \text{discretionary additional fiscal effort (tier 1, power equalization)} \\
t3 &= \text{discretionary additional fiscal effort (tier 2, no equalization)} \\
f &= \text{foundation base: \$2,839 in 1998-99}
\end{aligned}
$$

Louisiana

Louisiana had a school finance reform in 1992; this reform introduced the Minimum Foundation Plan. The post-reform formula involves two tiers: a foundation plan and a required local

effort plan. Tier 1 is as follows:

$$\begin{aligned} R_1 &= t * pi + (p/P * n * f/N * 0.65) \\ \text{where } t &= t = 0.005 \text{ (but can be bigger)} \\ \text{Local + State} &= f * n \\ \text{Local share} &= p/P * n * f/N * 0.35 \\ \text{State share} &= p/P * n * f/N * 0.65 \end{aligned}$$

and where f is the foundation amount, n is district enrollment, N is state enrollment, p is local revenue capacity (encompassing both property and sales tax base) per pupil, and P is the state revenue capacity per pupil.

Tier 2 funding is only awarded to districts with $p/P \leq 1.66$ and $t * p > p/P * n * f/N * 0.35$:

$$R_2 = t * p * (1 - 0.6 * p/P)$$

Total revenues are therefore $R = R_1 + R_2$.

Massachusetts

Massachusetts' school finance plan was implemented in 1994, with a reform that introduced the so-called Chapter 70 state aid. The formula involves a foundation plan with required local spending. The state establishes a foundation budget (F) as the sum of per pupil cost categories, which are a function of student enrollment in different grades and student categories (e.g. special education students), and a net school spending (NS), which is a function of the foundation amount in the previous year. If $NS \geq F$, districts receive the same aid as the previous year, plus a minimum \$100 increase per pupil. If $NS < F$, districts receive $F - NS + \$100$ per pupil. Districts and the state then share the burden of this required spending: specifically, districts are required to contribute a local share, which is a function of property values and income. The formula is therefore as follows:

$$R = \min NS, F + \$100$$

Michigan

The school finance plan in place in 1986 dates back to 1974. Under this finance scheme, district revenues came from local property taxes (constitutionally capped at 50 mills) and from state aid, distributed to districts using a Guaranteed Tax Base plan and a foundation allowance. The formula worked as follows:

$$
\begin{aligned}
R &= \tau p + \max\{f + \tau(\bar{p} - p), 0\} \\
f &= \text{Foundation allowance (\$400)} \\
\tau &= \text{actual property tax rate} \\
p &= \text{value of property per pupil} \\
\bar{p} &= \text{guaranteed tax base (\$102,500)}
\end{aligned}
$$

By 1993-94, however, only approximately 60 percent of districts were receiving any aid, and differences in per pupil expenditure spending between the highest- and lowest-spending districts had increased considerably. Further, school property tax rates were very close to the constitutional limit for most districts. For this reason, in 1993 governor John Engler signed P.A. 145. The Act reduced the share of operating revenue for public schools coming from local property taxes, and increased the importance of state aid.

The nature of the new funding scheme is a foundation plan. The state guarantees each district a basic level of funding per pupil, provided that the district levies a minimum local voter-approved property tax at a millage rate set by the Legislature (equal to 18 mills). Districts' foundation allowances each year have been based upon their foundation allowances of the immediately preceding year. In the first year of the reform (1994-95), the foundation allowance was set at $5,000; however, districts whose revenues were above and below this level the preceding year were assigned an allowance between $4,200 and $6,500, and gradu-

ally moved towards $5,000. The formula can be summarized as follows:

$$
\begin{aligned}
R &= f + \tau p - \bar{\tau} p \\
f &= \text{Foundation allowance} \\
\tau &= \text{actual property tax rate} \\
\bar{\tau} &= 0.018 \\
p &= \text{non-homestead property per pupil}
\end{aligned}
$$

Minnesota

The funding plan in place in Minnesota was implemented in 1988 and it is a simple foundation amount. The cost of the foundation is split between the state and the school districts based on the ratio between a district's adjusted net total capacity per pupil (ANTC, proxy for property tax base) and a guaranteed ANTC (GANTC) set by the state. Districts raise their share of the foundation through local property taxes. The formula is as follows:

$$
\begin{aligned}
R &= \text{Basic Revenue (foundation amount - \$3,530 in 1998-99)} \\
\text{Local Share} &= \text{Basic Revenue} * \min\{1, ANTC/GANTC\} \\
\text{State Share} &= \text{Basic Revenue - Local Share}
\end{aligned}
$$

Montana

Montana's school funding formula was introduced in 1993. It involves a foundation amount and a guaranteed tax base; the foundation amount must cover 80 percent of the total budget. The formula is as follows:

$$R = f + tp + t * \max\{1.74 * P/F - p, 0\} - t^F * p$$

where t is the tax rate chosen by the district, t^F is the state tax rate intended to finance the foundation aid (equal to 0.095), p is per pupil property value, f is the foundation amount, F is the sum of all foundation grants in the state, and P is the total property value in the state.

Nebraska

The school plan in Nebraska was implemented in 1990. The formula consists in a foundation plan with incentives for local effort. The formula is as follows:

$$R = \max\{f - LC, 0\} + t * p$$

where f is the foundation amount, p is the property value per pupil, t is the district's property tax rate, and LC is the local capacity, defined as the local tax revenue a district could raise at a "normal" tax rate.

New Jersey

In 1986 school finance in New Jersey followed the provisions of Chapter 212, as mandated by the *Public School Education Act* of 1975. State aid was distributed to districts under the form of an equalization grant. The formula is as follows:

$$R = \tau p + \max\{0.1\bar{S}, \max\{0, (1 - \frac{p}{1.35\bar{p}}) \min\{e, \bar{S}\}\}\}$$

where τ is the property tax rate chosen by the district, p is property value, $\bar{S}$ is the state aid limit, $\bar{p}$ is the average property value, and e is previous year's current expenditures.

Following a court declaration of unconstitutionality of the funding scheme resulting from the *Abbott* vs. *Burke* lawsuit started in 1981, in 1990 Governor Florio signed the *Quality Education Act* (QEA) into law. Among other provisions, the QEA substantially changed the financing formula, which became a foundation program. The local share had to be determined considering a district's property valuation and average income. The new formula, in place from 1992, is as follows:

$$R = \tau p + \max\{0, f - 0.5(Pp + Yy)\}$$

where f is the foundation amount ($6,640 in 1992); P and Y are, respectively, the property and the income multipliers, used to compute a district's fiscal capacity; p is property valuation and y is average income.

The formula introduced with the QEA was declared unconstitutional by the NJ Supreme Court in 1994 (*Abbott* vs. *Burke* III), because it did not equalize funding or guarantee needed supplemental programs. In 1996, Governor Whitman signs into law the *Comprehensive Ed-*

ucation Improvement and Financing Act (CEIFA). The act leaves the formula substantially unchanged, but it allocates $246 million ("parity aid") to 28 designated poor urban districts, denominated "Abbott districts". The funding scheme designed with CEIFA was ruled unconstitutional already in 1997, but the formula remained unchanged through 2004.

New York

The school finance plan in place in New York from 1986 to 2004 consisted in a combination of state and local funds. The largest part of local revenues came from property taxes. State aid was distributed through a variety of programs. The largest of them were:

- Basic Operating Aid (BOA), proportional to a district's Approved Operating Expenses (AOE, including salaries of administrators, teachers and non- professionals, fringe benefits, utilities, and maintenance of school facilities), and inversely proportional to its wealth:

$$
\begin{aligned}
\text{BOA} &= \max\{\text{Formula Aid}, 400\} \\
\text{Formula Aid} &= \text{OAR} \times \text{Ceiling} \\
\text{OAR} &= \min\{\max\{0, [1.37 - (1.23 \times \text{CWR})], \\
&\quad [1.00 - (0.64 \times \text{CWR})], \\
&\quad [0.80 - (0.39 \times \text{CWR})], \\
&\quad [0.51 - (0.22 \times \text{CWR})]\}, 0.9\} \\
\text{CWR} &= 0.5[(p/\bar{p}) + (y/\bar{y})] \\
\text{Ceiling} &= 3,900 + [\min\{8,000, \text{AOE}/n\} - 3,900] \times [\max\{0.075, 0.075/\text{CWR}\}] \\
n &= \text{weighted pupil count (TAPU)} \\
p &= \text{property value per pupil} \\
\bar{p} &= \text{mean property value per pupil} \\
y &= \text{average gross income per pupil} \\
\bar{y} &= \text{mean average gross income value per pupil}
\end{aligned}
$$

- Extraordinary Needs Aid (ENA), which provides extra funds to districts with high con-

centration of at-risk pupils:

$$\begin{aligned}
\text{ENA} &= (3,900 + \text{Ceiling}) \times \text{ENA Ratio} \times \text{ENC} \times 0.11 \times \text{Concentration Factor} \\
\text{ENA Ratio} &= (1 - (p/\bar{p}) \times 0.40) \\
\text{Concentration Factor} &= \max\{1 + [(\text{ENC}/\text{Enrollment}) - 0.745]/0.387, 1\} \\
\text{ENC} &= \text{Free \& Reduced Price Lunch Students} \\
&\quad + \text{Limited English Proficiency Students} \\
&\quad + \text{Sparsity Count} \\
\text{Sparsity Count} &= 25 - (\text{Enrollment}/\text{Square Mile})/58
\end{aligned}$$

- Growth Aid, which supplements operating aid for districts experiencing enrollment growth:

$$\begin{aligned}
\text{Growth Aid} &= (\text{Growth Index} - 1.004) \times \text{BOA} \\
\text{Growth Index} &= \text{Enrollment}/\text{Enrollment}_{-1}
\end{aligned}$$

- Tax Effort Aid (TEffA), for districts with particularly low levels of property valuation per pupil:

$$\begin{aligned}
\text{TEffA} &= 912.48 \times \text{Tax Effort Factor} \times n \\
\text{Tax Effort Factor} &= [\min\{(\text{Tax levy}/yn) \times 100, 7\} - 3]/4
\end{aligned}$$

- Tax Equalization Adjustment (TEqA), for districts with exceptionally high tax rates:

$$\begin{aligned}
\text{TEqA} &= (\text{Expense per pupil} - \text{Tax levy per pupil}) \times n \\
\text{Expense per pupil} &= \min\{8,000, \text{AOE}_{-1}/n_{-1} - \text{BOA}/n\}
\end{aligned}$$

North Dakota

The school finance plan in place North Dakota between 1986 and 2004 consisted in an equalized foundation formula:

$$R = t * p + \max\{f + T + tr - 0.0032t * p\}$$

where t is the property tax rate (capped at 0.185 and with some restrictions on its increase from one year to the other), p is the property valuation per pupil, f is the foundation base ($2,032 per pupil in 1998-99), T is a tuition apportionment ($223 per child aged 6-17 living in the school district and not necessarily enrolled in public schools), and tr is transportation aid, determined on a per district basis.

Ohio

The school finance plan in place in Ohio in 1986 was implemented in 1982. The formula in place is based on a foundation plan with a required minimum local effort. The formula is as follows:

$$R = \tau p + \max\{nf(C) - \bar{\tau}p\} + e(\tau_1^e, \bar{p}_1, p) + gn$$

where R is total revenues, f is the per pupil foundation amount, C is the cost of doing business, n is the weighted count of pupils, $\bar{\tau}$ is the required local effort (or "charge-off mileage", 0.23 percent in 1998-99), p is local property valuation, and τ is the property tax rate chosen by the district. In order for the districts to receive state aid, τ must be at least 20 mills.

The lawsuit *DeRolph vs. Ohio*, started in 1991, has led to a series of court rulings (including in 1997 and 2002) which have found the funding scheme unconstitutional and have led to an overall increase in state aid (i.e. a gradual increase in f over time). The funding formula, however, has remained the same.

Pennsylvania

In the period 1986-2004, Pennsylvania did not have a school finance reform. Its funding formula involved a percentage-equalized foundation plan as follows:

$$R = t_1 p + t_2 y + f(0.6(1 - p/\bar{p}) + 0.4 * (1 - y/\bar{y})) * 1(0.6 * (1 - p/\bar{p}) + 0.4 * (1 - y/\bar{y}) \geq 0.4)$$

where t_1 is the property tax rate (capped at 25 mills), p is per pupil property valuation, t_2 is the income income tax rate, y is per pupil taxable income, $\bar{p}$ is a statewide average of per pupil property valuation, $\bar{y}$ is a statewide average of income, and f is the foundation base.

Texas

In 1986, school district revenues in Texas stemmed mainly from state aid and local revenues. State aid was provided through a Foundation Program. The foundation amount was calculated as the sum of a Basic per pupil Allotment, a supplemental Experienced Teacher Allotment (which provided extra funds to districts employing more experienced, and therefore more costly, teachers), an Education Improvement Allotment, and an Enrichment Equalization Allotment, which provided districts with a matching transfer based on district fiscal effort and wealth. Districts were required to cover a share of the total cost of the Foundation Program with local revenues, raising at least \$0.33 for every \$100 of property valuation (Stevens, 1989). The resulting revenues formula is the following:

$$R = \max\{nf(X) - \bar{\tau}_1 p\} + \tau p + e(\tau^e_{1i}, \bar{p}_1, p) + gn$$

where R is total revenues, f is the foundation amount, function of n (weighted count of pupils) and X (characteristics of the district, such as price index, small size, etc.), $\bar{\tau}_1$ is the mandatory share of local effort (\$0.33 per \$100), p is local property valuation, τ is the property tax rate chosen by the district, e is the Enrichment Equalization Allotment, which depends on the district's property valuation, the average property valuation in the state, and local effort as summarized by a reference tax rate τ^e_1, and g is a flat grant.

The formula changed in October 1989, when the Texas Supreme Court declared the state school finance system to be unconstitutional, as part of the *Edgewood* vs. *Kirby* lawsuit. The legislature responded with Senate Bill 1019, which modified the formula as follows. First, it modified some parameters of the original formula. Second, it eliminated the Equalization Allotment, substituting it with a Guaranteed Tax Yield, which provides a specified amount per weighted pupil per penny of tax effort ($\bar{p}_2$), for up to 36 cents above the local fund assignment tax rate ($\bar{\tau}_2$). The flat grant was eliminated. The resulting formula, implemented in 1991, is as follows:

$$R = \max\{nf(X) - \bar{\tau}_2 p\} + \tau p + \tau^e_2 \max\{\bar{p}_2 - p\}$$

Senate Bill 1019 was declared unconstitutional in 1992 (Picus and Hertert, 1993). In 1994, a new bill (Senate Bill 351) was enacted to design a new school finance scheme. The 1989 formula

was preserved, but its parameters changed:

$$R = \max\{nf(X) - \bar{\tau}_3 p\} + \tau p + \tau_3^e \max\{\bar{p}_3 - p\}$$

Utah

The funding plan in place in Utah between 1986 and 2004 was a foundation plan. The formula was as follows:

$$R = t * p + \max\{f - t_l * p\}$$

where t is a district's property tax rate, p are property values, f is the foundation amount, and t_l is a "required" local effort.

Wisconsin

Until 1996, Wisconsin used a two-tiered Guaranteed Tax Base (GTB) formula to allocate state aid to the districts. A third tier has been added in 1996. With this formula, the state shares part of the costs (such as operating expenses, capital outlays, and debt service) with the districts, by guaranteeing districts with a certain amount of local revenues per mill of tax levied. The formula can be summarized as follows:

$$\begin{aligned} R &= T^1 + \max\{T^2 + T^3, 0\} + \tau p \\ T^1 &= (1 - p/\bar{p^1}) * \min\{C, \bar{C}^1\} \\ T^2 &= (1 - p/\bar{p^2}) * \min\{C - \bar{C}^1, \bar{C}^2\} \\ T^2 &= (1 - p/\bar{p^3}) * \max\{C - \bar{C}^2, 0\} \end{aligned}$$

where R is per pupil revenue, τ is its local property tax rate, p is the district's per pupil equalized expenditure, and T^1, T^2, and T^3 are the three tiers of state aid. The variables p^1, p^2, p^3 represent per pupil guaranteed tax base in each tier, whereas $\bar{C}^1$ and $\bar{C}^2$ are the cost ceilings for the first two tiers of expenditure. In words, the state guarantees a certain level of tax revenue for different portions of the total shared costs. In addition, while a negative third-tier aid can decrease second-tier aid, a negative sum of second- and third-tier aid cannot decrease first-tier aid. In addition, districts are subject to a limit on the annual increase in their revenue per pupil

derived from state aid and property taxes. In 1999, this increase could not exceed $208.88. A school district that exceeds its revenue limit is subject to a penalty, in the form of reduced state aid, in the amount of the excess revenue.

Table DI: Details on the elements of the funding formula

state	data starts	data ends	reform in	variables of the formula (kept constant in simulation)	parameters of the formula
California	1996	2004		property values, enrollment	property tax rate (1 percent); revenue limit
Colorado	1994	2004	1994	assessed property value (tax base for property tax); specific ownership tax revenue (tax on registered vehicles); enrollment	per-pupil revenue formula (function of cost-of-living and enrollment)
Florida	1988	2004		property values, property tax rates, enrollment	foundation amount, limits on property tax rate, "required" property tax rate, cost-of-living adjustment
Georgia	1987	2004	1985	property values, property tax rates, enrollment	foundation amount, upper bound on equalization mills, minimum tax rate to receive guaranteed tax base aid
Illinois	1987	2004		equalized property valuation, property tax rate, enrollment	foundation amount, flat grant amount, thresholds for property values to assign tiers
Kentucky	1991	2004	1990	property values, property tax rates, enrollment	foundation amount, thresholds between tiers
Louisiana	1993	2004	1992	local revenue capacity, district enrollment, tax rates	foundation amount, state revenue capacity, state enrollment
Massachusetts	1993	2004	1994	property values, income, enrollent	foundation amount, net spending, tax rates
Michigan	1990	2004	1993	non-homestead property values, enrollment, property tax rates	foundation amount, threshold tax base
Minnesota	1991	2004	1988	enrollment, property tax rates, adjusted net total capacity (measure of property tax base)	foundation amount (basic revenue), guaranteed adjusted net total capacity
Montana	1994	2004	1993	enrollment, property values, tax rates	foundation amount, tax rate to finance the foundation amount
Nebraska	1993	2004	1990	enrollment, property values, tax rates	foundation amount
New Jersey	1988	2004	1990	property values, enrollment, property tax rates, average district income	foundation amount, property and income multipliers
New York	1986	2004		enrollment, property values, income	maximum amount of Basic Operation Amount, threshold to Ceiling for Formula aid,
North Dakota	1986	2004		enrollment, property values, income, number of children aged 6-17 living in the district	foundation amount, transportation aid, tuition apportionment
Ohio	1986	2004		property values, property tax rates, enrollment	foundation amount, cost-of-doing-business, required local effort tax rate, lower bound for tax rate
Pennsylvania	1995	2004		property values, property tax rates, income, income tax rate, enrollment	foundation amount, cap on local property tax rate
Texas	1986	2004	1989, 1993	property values, property tax rates, enrollment	foundation amount, local fund assignment tax rate, parameters of guaranteed tax yield
Utah	1986	2004		property values, property tax rates, enrollment	foundation amount, required local effort
Wisconsin	1986	2004	1996	property values, property tax rates, enrollment	guaranteed tax base in each tier, ceilings of expenditure in each tier, revenue limit

Appendix E List of School Finance Reforms

State	Reform?	Pre-Reform Formula	Reform Year	Reform Name	Reform Type	Reform Formula
Alabama	Yes	Foundation w/equalization	1995	*Ace v. Hunt*, 624 So.2d 107 (Ala. 1993)	Court-ruled	Foundation w/equalization
Arizona	Yes	Foundation w/equalization + maximum spending	1998	*Roosevelt vs. Bishop*	Court-ruled	Foundation w/equalization + maximum spending + extra aid for minimum infrastructure
Arkansas	Yes		1983	*Dupree v. Alma School District No. 30* (Ark. 1983)	Court-ruled	
			1995	Equitable School Finance Plan (Acts 917, 916, and 1194)	Legislated	Foundation w/equalization
California		Foundation + flat grant				
Colorado	Yes	Guaranteed tax base	1994	Public School Finance Act of 1994	Legislated	Foundation
Connecticut	Yes	Guaranteed tax base	1989	Education Cost Sharing	Legislated	Foundation w/ equalization
Delaware		Guaranteed tax base				
Florida		Foundaton + guaranteed tax base				
Georgia		Foundation + required local effort + equalization	1985	Quality Basic Education (QBE)	Legislated	Foundation + required local effort + equalization
Idaho	Yes	Foundation + equalization	1994	Senate Bill 1560	Legislation	Foundation (allocation based on salaries) + equalization
Illinois		Hybrid: foundation, alternate, flat grant				
Indiana	Yes	Foundation	1993	*Lake Central v. State of Indiana*	Court-ruled	Guaranteed tax base
Iowa	Yes	Foundation + equalization	1991	Code of Iowa, Chapter 257	Legislated	Foundation + equalization
Kansas	Yes	Guaranteed tax base	1992	School District Finance and Quality Performance Act (SDFQPA, 1992)	Legislated	Foundation + recapture
Kentucky	Yes	Foundation with power equalization	1990	*Rose v. Council for Better Education*, 790 S.W.2d 186 (Ky. 1989), followed by Kentucky Education Reform Act (1990)	Court-ruled	Minimum foundation with power equalization

Louisiana	Yes	Foundation	1992	Legislature	Legislated	Foundation
Maine	Yes		1985	School Finance Act of 1985	Legislated	Foundation
			1995	School Finance Act of 1995	Legislated	Foundation (minimum change in how state aid is calculated)
Maryland	Yes	Foundation	1986	Action Plan for Education Excellence (APEX),	Legislated	Foundation with required local effort
Massachusetts	Yes	Foundation	1994	*Mc Duffy v. Secretary of the Executive Office of Education*, 1993; Chapter 70	Court-ruled	Foundation
Michigan	Yes	Foundation + Guaranteed Tax Base	1993	P.A. 145 2 of 1993	Legislated	Foundation
Minnesota	Yes	Foundation	1988	General Education Revenue Program	Legislated	Foundation
Mississippi	Yes	Foundation	1997	Mississippi Adequate Education Program	Legislated	Foundation with required local effort
Missouri	Yes	Foundation + Guaranteed Tax Base	1993	Committee for Educational Equality v. Missouri; Outstanding Schools Act (OSA)	Court-ruled	Foundation with required local effort
Montana	Yes	Foundation	1993	*Montana Rural Ed. Association v. Montana*; House Bill 667	Court-ruled	Foundation + Guaranteed Tax Base
Nebraska	Yes	Foundation	1990	Tax Equity and Educational Opportunities Support Act (LB1059)	Legislated	Foundation
			1997	LB 806 (minor changes)	Legislated	Foundation
Nevada		Foundation				
New Hampshire	Yes	Foundation	1985	Statute	Legislated	Flat grant + equalization
			1999	*Claremont v. Governor*	Court-ruled	Flat grant + equalization
New Jersey	Yes	Guaranteed tax base	1990	*Abbott v. Burke* 575 A.2d 359 (N.J. 1990)	Court-ruled	Foundation
			1996	"Comprehensive Educational Improvement and Financing Act of 1996	Legislated	Foundation
New Mexico		Foundation				
New York	Yes	Percentage equalization + flat grant	2003	*Campaign for Fiscal Equity, Inc. v. State*	Court-ruled	Percentage equalization + flat grant
			2006	*Campaign for Fiscal Equity, Inc. v. State*	Court-ruled	Percentage equalization + flat grant
North Carolina		Flat grant				

North Dakota		Equalized foundation				
Ohio		Foundation with local effort				
Oklahoma		Foundation + Guaranteed Tax Base				
Oregon	Yes	Foundation	1990	Measure 5; Chapter 780, Oregon Laws 1991	Legislated	Foundation (caps on local tax rates)
			1997	Measure 50	Legislated	Foundation (caps on local tax rates)
Pennsylvania		Foundation + percentage equalization				
Rhode Island	Yes	Foundation	1995	Legislation	Legislated	Flat grant
South Carolina	Yes	Foundation	1985	Education Improvement Act (EIA)	Legislated	Foundation + categorical (with required local effort)
South Dakota	Yes	Expenditure-driven formula	1995	Legislation	Legislated	Foundation
Tennessee	Yes	Foundation	1992	Education Improvement Act	Legislated	
Texas	Yes	Foundation	1989	Edgewood Independent School District v. Kirby	Court-ruled	Foundation
			1993	Senate Bill 7	Court-ruled	Foundation (tier 1) + Guaranteed Tax Yield (tier 2) + Recapture component
Utah		Foundation + required local effort				
Vermont	Yes	Percentage equalization	1987	Legislation	Legislated	Foundation
			1997	*Brigham v. State*, followed by Act 60	Court-ruled	Flat grant + guaranteed tax yield
Virginia		Foundation				
Washington	Yes	Foundation	1987	Legislation	Legislated	Foundation + Guaranteed Tax Yield
West Virginia		Foundation				
Wisconsin	Yes	Guaranteed tax base - 2 tiers	1996	Legislation	Legislated	Guaranteed tax base - 3 tiers
Wyoming	Yes	Foundation	1995	*Campbell County v. State*	Court-ruled	Foundation