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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, using variation from 13 school finance reforms passed in 20 US states between 1986 and 2004. Since households sort in response to each reform, post-reform revenues are endogenous to an extent that varies across states depending on the funding formula. 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, especially for low-income students. I provide suggestive evidence that this effect acts through a reduction in the gap in inputs (such as the number of teachers) and in college attendance between low-income and high-income districts.

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A data appendix is available at http://www.nber.org/data-appendix/w25600

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 also suggested 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).

Little is known, however, about what factors make a place particularly successful at generating higher income and intergenerational mobility. High-mobility places 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, they cannot be interpreted as causal.

Understanding the role of public policies is the first step towards mitigating these differences and improving mobility in the US. 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, on intergenerational mobility

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 depending on each state's funding scheme: In 1980, the lowest-spending district in California spent 70 percent less than the highest-spending district, whereas the gap between the lowest spending and highest spending districts was only 40 percent in Maryland.

In an attempt to equalize expenditure and guarantee equal opportunities to all children, over the past four decades states have reformed their school finance schemes through changes in their funding formulas. A funding formula expresses each district's revenues as a combination of state funds and local levies, and it allocates state aid to each district. While often sharing a common objective, school finance equalization reforms have taken various forms across states and over time. As a result, reforms implemented under the same name and with the same objective have had very different effects on both the level and the distribution of school expenditure across districts within the same state (Hoxby, 2001).

Using variation in the distribution of per pupil revenues generated by 13 school finance reforms passed in 20 states between 1986 and 2004, I study the causal effects of equalization on intergenerational mobility of children born between 1980 and 1986, who were exposed to these reforms while in school. To get at the causal effect, I use a simulated-instrument approach (similar to Gruber and Saez, 2002) that exploits plausibly random changes in the funding formula, idiosyncratic to each state. This approach allows me to (i) separate the changes in the distribution of school revenues driven by exogenous changes in the funding formula from the changes driven by endogenous household sorting, and (ii) allow for differences in the extent of this endogeneity across states, driven by the fact that different states carried out very different reforms which could have affected revenues and household sorting in heterogeneous ways (Hoxby, 2001).

In theory, equalization of school revenues and expenditures should smooth the differences in economic opportunities among richer and poorer children. 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). Differences in parents' ability to invest in their offspring's education make children's long-run outcomes heavily dependent on their initial conditions, depressing mobility. Closing the gap in education investments can therefore "level the playing field" and reduce the extent to which economic fortune is transmitted across generations (Becker and Tomes, 1994).

I measure equalization in school revenues as the correlation between per capita income and per pupil revenues across districts in each state and year, denoted by β (Hoxby, 1998; Card and Payne, 2002; Lafortune et al., 2018). Estimates of β equal zero when revenues are perfectly equalized, whereas they are positive when wealthier districts receive and spend more.

School finance reforms led to a sharp decline in β. I study the effects of this decline on children's intergenerational mobility, measured, as in Chetty et al. (2014), as children's expected rank on the national income distribution by commuting zone (CZ hereafter), cohort (1980-1986), and income percentile of her parents (relative to the national distribution).[1]

[1] These estimates are available at http://www.equality-of-opportunity.org/data/. The data are described in more detail in Section 4.

The nature of these reforms is such that one cannot simply use post-reform expenditures and revenues as an exogenous variable to explain mobility. First, variables entering the funding formula (such as house prices and income) might change over time and thus affect district revenues, while also having a direct effect on mobility. Second, changes to the formula alter the relationship between the "price" of school spending to taxpayers and the amount of public good they receive in return. This might induce households to "vote with their feet" (Tiebout, 1956), i.e., to move across districts based on their preferences for this public good and their income.[2] This sorting could also affect house prices and, in turn, districts' revenues. Changes in house prices after a school finance reform could therefore cause β to be endogenous.

Importantly, the effects of a school finance reform on revenue equalization and household sorting 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. I show that 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.

To address the issue of endogeneity in post-reform revenues and to account for these heterogeneous responses, I construct a simulated instrument for β which exploits differences in the formulas across states and over time. The intuition behind this strategy is to isolate the (plausibly exogenous) variation in the distribution of revenues and expenditures generated by changes in each state's formula from the endogenous variation driven by households sorting across districts. To implement this strategy, I first codify the formulas in place in each state and year using information from administrative and legislative sources, and I collect data on all the district-level variables entering each formula (these data are available for a sample of 20 states covering 62 percent of student enrollment). I then simulate post-reform revenues for each school district using the post-reform formula, but keeping the district's characteristics (such as property values, enrollment, income, etc.) fixed at their pre-reform values. Lastly, I use these

[2] 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.

simulated revenues to estimate a simulated version of β, which I use as an instrument for β. First-stage results indicate that this instrument is very strong.

Two-stages least squares (2SLS) estimates of the effects of changes in β indicate that school finance equalization (i.e., a reduction in β) has a sizable positive effect on intergenerational mobility. A one-standard deviation reduction in β leads to a 5.6 percentile increase in mobility for children with parental income in the 10th percentile, a 5.2 percentile increase for children from the 25th percentile, and a 3.5 percentile increase for children from the 90th percentile. These estimates correspond to a 16.2 percent, 14.9 percent, and 9.5 percent increase in income, respectively. My results also indicate that the average reform would increase mobility of children from families on the 25th percentile by 3.3 percentiles, and close approximately 10 percent of the gap between the lowest-mobility and the highest-mobility CZ.[3]

Perhaps surprisingly, these estimates reveal positive effects of equalization also on less disadvantaged students; this is, however, consistent with some of these reforms (specifically those passed after 1990) increasing expenditure in all school districts within a state, albeit more in poorer ones ("adequacy reforms," Lafortune et al., 2018). These findings confirm the importance of equalization in school resources across richer and poorer districts for equality of children's economic opportunities, and they are consistent with the literature on the positive effects of increased spending for low-income students' outcomes (Jackson et al., 2015; Lafortune et al., 2018). Importantly, 2SLS estimates are approximately 50 percent larger than OLS, a smaller bias than the one found by Jackson et al. (2015). This highlights the importance of addressing endogeneity in post-reform revenue while accounting for heterogeneity in the effects of different school finance reforms on revenues and household responses.

Grade-specific effects of a decline in β on mobility show that equalization is most effective when experienced during high school, the moment of a student's career that immediately precedes the transition to college. While in partial contrast with the results of Jackson et al. (2015), who find that the positive effects of equalization increase with the length of exposure to each reform, this finding hints at the importance of college attendance for intergenerational mobility, in line with Rothstein (2019).

The effects of equalization in school revenues might vary depending on the degree of

[3] 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 25th percentile is an increase of approximately 3.3 percentiles, which corresponds to 10 percent of the 32.7 percentile gap in mobility between the highest-mobility CZ (Sioux Center, IA) and the lowest-mobility one (Clarksdale, MS).

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 translate into an increase in 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 through which school finance equalization affects intergenerational mobility. Specifically, I show that equalizing revenues and expenditures across districts reduces the gap in basic school inputs (such as the number of teachers) and in intermediate educational outcomes (such as the probability of attending college by age 19) between richer and poorer districts.

This paper makes three main contributions. First, it is one of the first to provide causal evidence of the effects of a given policy on intergenerational mobility.[4] Recent research using administrative data has revealed large differences in mobility across US local labor markets, which appear to be correlated with measures of school quality (Chetty et al., 2014). Using cross-sectional variation among CZs, Rothstein (2019) argues that differences in school quality do not seem to explain much of the observed differences in mobility, which suggests that attention should be placed on other types of policies. My findings indicate that a school-related policy such as school finance equalization *causes* a sizable improvement in long-term outcomes of disadvantaged children within each CZ, 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 in mobility. My results also shed light on the mechanisms through which equalization in school resources affect children's long-run economic outcomes: equalization in school inputs and in college attendance between more and less disadvantaged students.

Second, this paper contributes to a large literature on the effects of public school expenditure on students' outcomes. Due to a scarcity of exogenous variation in school funding, this

[4]Most of the earlier literature on mobility is descriptive and has focused on comparing various 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, reporting an estimate of about 0.2 (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. Similar studies, however, find very different estimates (ranging from 0.3 to more than 0.5) depending on the length of the panel and on the representativeness of the sample. Another related strand of research has attempted to perform international comparisons of intergenerational income elasticities, concluding that countries such as Canada, Sweden and Norway are more mobile than the US (Solon, 2002). Due to differences in the underlying income distribution in each of these countries, however, international comparisons are typically difficult to perform.

literature has often struggled to identify causal effects, and different studies have produced contrasting results.[5] A few studies have used school finance reforms as a quasi-experimental source of variation in school expenditure to study both short-term outcomes, such as student achievement and educational attainment (Hoxby, 2001; Card and Payne, 2002; Lafortune et al., 2018), and long-term outcomes, such as earnings (Jackson et al., 2015). The focus of these studies, however, has been to estimate the effects of increases in the *levels* of revenues and expenditure, as opposed to changes in their distribution across states, which are at the center of this study.

Lastly, and perhaps most importantly, this paper demonstrates that, when studying the effects of school finance reforms, one must take into account not only the endogeneity in post-reform revenues caused by household responses, but also the differences in funding schemes across states. Jackson et al. (2015) have addressed this issue by instrumenting expenditure with the timing of each reform, the initial position of each district in the state distribution of per pupil expenditure, and the type of funding plan (e.g. foundation plan, or equalized effort). This approach, however, is unable to account for the fact that different reforms produce different effects on revenues and expenditure and generate different household responses. My approach builds on Hyman (2017) in that it uses the specific formula parameters as instruments, and it extends this approach to a large sample of US states. This 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 presents 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 of school revenues. Section 6 outlines the empirical strategy and the instrumental variables approach. Section 7 presents and discusses the main estimates of the effects of school finance equalization on intergenerational mobility. Section 8 investigates the mechanisms behind these effects, and Section 9 concludes.

[5]Observational studies dating back a few decades have found small effects from an increase in school expenditure (Coleman et al., 1966; Hanushek, 1986, 1997, 2003). Other works using quasi-experimental (Card and Krueger, 1992) and experimental (Krueger, 1999; Dynarski et al., 2013; Hyman, 2017) variation have instead highlighted the importance of school inputs (such as, but not limited to, smaller classes) on medium- and long-term outcomes, suggesting that greater investments in public schools might be beneficial for students. Burtless (2011) provides a detailed survey of the existing literature on this issue.

2 School Finance Equalization Reforms

Until the early 1970s, the majority of US school districts drew most of their revenue from local property taxes and received state transfers in the form of categorical aid (Howell and Miller, 1997; Hoxby, 2001).[6] Since wealthier areas have a larger tax base, high-income districts have been able to spend considerably more compared to low-income districts. This has created large disparities in per pupil expenditure across districts within each state. Capitalization of the quality of public schools into house prices has exacerbated these differences.

To address 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 involved changes to states' funding schemes, 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 school districts. Some formulas include spending limits in a further attempt to break the relationship between each district's wealth and expenditure on public schools. 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 categories. In addition, the parameters of each formula vary considerably across states and over time even within categories. 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,

[6] Categorical aid is a transfer from the state to the districts based on the students' characteristics and the related average cost of educating them (Hoxby, 2001).

i.e., the tax base, is endogenous. When the funding formula changes, households sort across school districts depending on their preference for public schools and their income, and these movements affect house prices. 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).[7]

Empirical evidence on the effects of school finance equalization on student achievement is mixed. Card and Payne (2002) find that court-mandated reforms reduce gaps in SAT scores between low- and high-income students. More recently, Lafortune et al. (2018) estimate a positive and large effect on test scores scores of an increase in expenditure driven by adequacy reforms. Studies focusing on individual states have also found positive effects of equalization on test scores (Guryan, 2001; Papke, 2005; Roy, 2011) and on 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.

Among the existing studies, Hoxby (2001), Jackson et al. (2015), and Hyman (2017) explicitly address the endogeneity in post-reform expenditure caused by changes in the variables of the funding formula. I build on these works by studying the effects of equalization on intergenerational mobility of students exposed to school finance reform, accounting for endogeneity in post-reform expenditure and heterogeneity in school finance plans across states by means of a simulated instruments approach.

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.

[7] The California 1978 reform, one of the most famous ones, was passed in response to the *Serrano* decision of 1976. The 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).

School districts are responsible for the financing of public schools. Each child attends school in the district he or 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 possible ways through 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 as

$$e_d = \alpha_s \bar{x}_s + \beta_s x_d \tag{2}$$

where $\bar{x}_s$ is average parental income in state s, x_d is average parental income in district d, and α_s and β_s are parameters. The equation can be rewritten as:

$$e_d = \bar{e}_s + \beta_s (x_d - \bar{x}_s) \tag{3}$$

In this expression, the variable $\bar{e}_s$ is average per pupil expenditure in state s. The parameter β_s captures the extent of equalization in school expenditure within each state. When $\beta_s = 0$, $e_d = \bar{e}_s$: 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: richer districts (i.e. those with average income larger than the average income in the state) have larger expenditure, and *vice versa*.

The child's income can be rewritten as a function of $\bar{e}_s$ and β_s as follows:

$$y_{id} = \theta x_{id} + \gamma \bar{e}_s + \gamma \beta_s (x_d - x_s) \tag{4}$$

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

centile of the national parent income distribution can be defined as:

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

where $F_y(\cdot)$ denotes the cumulative income distribution function of the child and $F_x(\cdot)$ denotes the cumulative income distribution function of the parent.[8] 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 quantile of its distribution.[9] Substituting the expression for child's income from equation (1) allows me to express mobility as a function of the parameter β_s:

$$M_s^r = F_y(\theta Q_x(r/100) + \gamma\bar{e}_s + \gamma\beta_s(Q_x(r/100) - x_s)) \tag{6}$$

The function $F_y(\cdot)$ is a cumulative distribution function and is therefore non-decreasing. From this, it follows that M_s^r is non-increasing in β_s when $Q_x(r) - x_s$ is smaller than zero, i.e. for children in families below the mean in the state. In the remainder of the paper I test this theoretical result on the relationship between intergenerational mobility and inequality in school expenditure.

4 Data

To conduct the empirical analysis I combine data from multiple sources. 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 a district's revenues using the funding formula. As such, it 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.).[10] Both the nature of these elements and the way they are measured vary across states. This information is therefore not readily available as a unified database.

[8]This measure is analogous to the absolute mobility measure of Chetty et al. (2014), presented in Section 4.
[9]Note that $Q_t(a) = F_t^{-1}(a)$.
[10]Information on school districts' expenditures and revenues (total and by source) is available through a number of sources, including the US Census of Government and the National Center for Education Statistics (NCES) Longitudinal School District Dataset.

To implement my empirical strategy I assembled a separate district-level dataset for each state, drawing from states' detailed historical records on school finance. 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 twenty states, comprising 405 CZs and 8,102 school districts and covering approximately 62 percent of total student enrollment. The elements of the dataset for each state are described in Table CI, and the various formulas are described in detail in Appendix C.[11]

Table I (Panel A) summarizes the variation in school revenues across districts within each CZ or state, measured as the difference in this variable between the highest-income and the lowest-income district. While this difference is small on average, in 1990 it ranges from -$2,306 to $12,965 across states, and from -$11,045 to $14,518 across CZs.

School Finance Reforms. I compile a list of all state-level school finance reforms passed between 1986 and 2004, the time period when the cohorts at study (born between 1980 and 1986) were in grades 1 to 12. These reforms are defined as court-mandated or legislated changes to the funding scheme. I combine information from "Public School Finance Programs of the United States and Canada" (1990–1991[12] and 1998–1999[13]) and from Verstegen and Jordan (2009). These publications describe the funding schemes in place in each state over time, and they include details of the timing and content of each reform. I complement these sources with information from Manwaring and Sheffrin (1997), Hoxby (2001), Jackson et al. (2015), and Lafortune et al. (2018). Information is largely consistent across the different sources; when discrepancies are found, priority is given to the "Public School Finance Programs of United States and Canada" for older events and to Lafortune et al. (2018) for more recent ones. Appendix D briefly describes the reforms used in the analysis, and Figures AII and AIII summarize the timing of these events.

[11] 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 C describes some of the formulas in more detail.

[12] 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.

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

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 obtain information on average and median household income in each district.[14] I match 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 children's expected rank in the national income distribution as a measure of intergenerational income mobility. This measure varies at the level of the CZ $\times$ birth cohort $\times$ parents' income rank in the CZ. I construct this variable using Chetty et al. (2014)'s estimates of the intercept and slope of the linear relationship between parents' and children's national income ranks, available separately for 637 out of 722 CZs (including 327 CZs for which simulated revenues are available) and for children born between 1980 and 1986.[15] Combined with data on the national income distribution, these estimates allow me to calculate a child's expected rank given the income of her parent. I further combine this information with data on the incomes of parents in the 10th, 25th, 50th, 75th, 90th, and 99th centile of the income distribution in each CZ.[16] The final dataset contains children's income ranks for 327 CZs, 7 birth cohorts, and 6 parental income centiles. 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 intergenerational mobility of children in different parts of the parental income distribution.[17]

Summary statistics of mobility are shown in Panel B of Table I. On average, children with parental income below the median experience upward mobility, whereas children with parental income above the median experience downward mobility.[18] Wide differences exist across CZs (Figure AI): The expected income rank of children with parental income in the 25th percentile is as low as 32 in Gordon, SD and as high as 61 in Sioux Center, IA, while for children with

[14]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.

[15]Slope and intercept estimates are published as the Online Data Table 1 of Chetty et al. (2014), available at www.equality-of-opportunity.org.

[16]Information on the income distributions within each CZ is published as the Online Data Table 7 of Chetty et al. (2014), available at www.equality-of-opportunity.org.

[17]To see this, consider an increase in the measure of mobility that Chetty et al. (2014) refer to as "relative" (i.e., a lower elasticity between parents' and children's incomes or income ranks). 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 "absolute" mobility, allows me to study these two cases separately.

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

parental income in 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 measures of education mobility, defined as the probability of being enrolled in college by age 19 for each CZ, birth cohort, and parents' income rank in the CZ. I use this measure for cohorts 1984 to 1990 to estimate the effects of equalization on educational attainment.[19]

House Prices. To capture 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).[20] 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 district and year (available for the years 1988-2010).

5 Measuring Inequality in School Expenditure

I start my analysis by constructing 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 districts' per pupil revenues and per capita income, captured by the parameter β_{st} in the following equation:[21]

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

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.

[19] 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 years for each cohort.

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

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

The parameter β_{st} represents the degree of inequality in school funding across districts in state s and year t. Larger positive values of β_{st} indicate higher (lower) per pupil revenues in richer (poorer) districts and a more unequal funding scheme. Negative values of β_{st}, on the other hand, denote higher per pupil revenues in lower-income districts and a redistributive funding scheme. Lastly, values of β_{st} close to zero characterize an equalized funding scheme, with similar levels of revenues across richer and poorer districts. Appendix Figure AIV 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 on intergenerational mobility measured at the birth cohort level, I assign each cohort a measure of revenue inequality based on the β experienced while in school. I calculate this measure as the average over the calendar years in which each cohort was in grades 1–12.[22] 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 the reforms in each state and I impute income values to each district depending on whether the state where the district is located experienced a school finance reform during that decade. If a reform took place, I impute the income of the Census year at the beginning of the decade to the years preceding the reform (including the year of the reform) and the income of the Census year at the end of the decade to the years following the reform. If no reform took place in the CZ during that decade, I interpolate between the income values of the Census years at the beginning and at the end of the decade.[23] To demonstrate that my results are not driven by this imputation method, in robustness checks I use a version of β estimated assigning the 1990 median district income to all years.

On average, the parameter β is equal to 0.019 for states without a school finance reform (with a standard deviation of 0.098), to 0.041 for states with a reform in the years preceding the event (with a standard deviation of 0.027) and to -0.004 in the years after the event (with a

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

[23]If two reforms take place in one decade (as is the case for Montana, New Jersey, New York, and Oregon), I assign the income of the Census year at the start 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.

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:

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

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.[24] Estimates of β decline immediately following a school finance reform and remain stable at this lower level 10 years after the reform. Appendix Figure AV 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.

6 Endogeneity of Post-Reform School Expenditure and Simulated Instruments

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 these reforms as exogenous shifters of school spending to study its effects on a variety of children's outcomes (Jackson et al., 2015; Lafortune et al., 2018).

The particular nature of these reforms, however, creates a problem of endogeneity of post-reform revenues and expenditures even if reforms are random events. School revenues directly depend on district-specific characteristics entering the funding formula, such as house prices and income. These variables could vary over time and have a direct effect on mobility. This would make the post-reform $\hat{\beta}_{sb}$ endogenous. In addition, school finance equalization reforms

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

could lead households to sort across school districts based on their income, wealth, and preferences for school spending (Aaronson, 1999; Dee, 2000; Figlio and Lucas, 2004; Epple and Ferreyra, 2008; Chakrabarti and Roy, 2015). This happens because changes to the 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.[25] A change in the tax price affects households' budget constraints; the Tiebout model predicts that some households will respond by "voting with their feet" and moving to a different district. These movements affect house prices, the property tax base, and districts' revenues and expenditures in an endogenous way.

While earlier studies of school finance reforms (such as Card and Payne, 2002) have not explicitly accounted for this issue, more recent studies have recognized and addressed it. Hyman (2017), for example, studies the effect of Michigan's Proposal A of 1993 and instruments expenditure with the amount of the foundation grant, determined by the law. Jackson et al. (2015) study the effects of several reforms passed across all US states since the 1970s and instrument expenditure using the timing of each reform, the initial position of each district in the state distribution of per pupil expenditure, and the type of funding plan (e.g. foundation plan, or equalized effort).[26]

6.1 Endogeneity in the Presence of Heterogeneity in Reform Effectiveness

The approach of Jackson et al. (2015) 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 spending distribution. As explained in detail by Hoxby (2001), however, reforms that are similar in timing and involve similar funding plans can have different effects on the level and distribution of expenditure across districts. In fact, while Jackson et al. (2015) find that school finance reforms on average increase expenditure more in *ex ante* lower-spending districts, Hyman (2017) finds that Michigan's Proposal A increased expenditure more in low-poverty districts. The contrast between these two sets of findings suggests that different reforms could yield different effects on districts' finances.

[25] The effect of an equalization reform on the tax price can be either positive, negative, or zero, depending on the specific formula adopted. Reforms of the three types have been implemented across US states in the past 40 years (Hoxby, 2001).

[26] Lafortune et al. (2018) 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 they fail to reject the hypothesis of no changes in these variables.

The heterogeneity in the effects of different reforms is also evident in my data. 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 reduced β from 0.045 to 0.041).

Different Reforms Led to Different Changes in House Prices. The contrast between the findings of Jackson et al. (2015) and Hyman (2017) and the evidence in Figure II suggest that the effects of a reform on revenues, expenditures, and households' incentives to sort across districts can differ even among reforms of the same type, and they are idiosyncratic to the specific formula type and parameters adopted by each state. This is supported by Figure III, which shows trends in average house prices across school districts in each state for a sample of four states in the years surrounding a reform (house prices are normalized to zero in each school district).[27] While some reforms (such as the ones of Texas and New Jersey) were followed by a decline in house prices, others (such as Michigan) do not appear to have triggered any significant changes, and others (such as Massachusetts) were followed by an increase in house prices.

6.2 Constructing the Simulated Instrument

Figure III suggests that even reforms that are similar in type can have very different effects on revenues and trigger different endogenous household responses. The extent of the endogeneity in post-reform expenditure can thus vary across states. To account for this heterogeneity, I use a simulated-instruments approach (Currie and Gruber, 1996; Gruber and Saez, 2002) which, similarly to Hyman (2017), directly exploits changes in each state's formula type and parameters driven by a reform.[28] The goal of this strategy is to isolate the exogenous varia-

[27] Each point and spike in Figure III represent the estimate and the 90 percent confidence interval of the coefficients δ_n in the regression $HP_{dt} = \sum_{s=-4}^{6} \delta_n R_{s(d)} 1(t - Ryear_{s(d)} = n) + \varepsilon_{dt}$, where HP_{dt} is the house price index of district d in year t, $R_{s(d)}$ equals 1 if state s where the district is located experienced a school finance reform in the years 1986-2004, and $Ryear_{s(d)}$ is the year of the earliest school finance reform. The parameters are estimated separately for each state. Observations are weighted by population. Annual House Price Indexes data are taken from the Federal Housing Finance Agency, aggregated at the district level using population weights, and cover years from 1986 to 2004.

[28] Hyman (2017) does not construct simulated instruments and instead directly uses the foundation grant as an instrument for expenditures. The foundation grant, however, can be seen as the relevant formula parameter of Michigan's school finance plan. 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 essentially the same as the one of Hyman (2017). I expand Hyman (2017)'s analysis to a large sample of US states.

tion 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 and in revenues.

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 (7). School revenues are a function of a district's characteristics (through the funding formula). By construction, β_{st} 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+1} to differ from X_{st}. If this difference has a direct effect on mobility, β_{st+1} will be endogenous and estimates of the effect of the change in β_{st} 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/or house prices not changed, and it is exogenous. The quantity b_{st+1} instead captures 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. Assuming that 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}.

Implementation. I obtain the simulated 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 C 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.[29] Lastly, I compute a simulated version of the parameter β for each CZ and cohort, denoted by β^{sim}, by estimating equation 7 with simulated revenues instead of actual revenues.[30]

Assumptions. The validity of this approach relies on the exogeneity of the timing of each reform and of the type and parameters of the funding formula. This assumption could be violated, for example, if the funding formula chosen by each state is 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 effects than what was intended and appear to have been adopted in a trial-and-error fashion. In addition, the precise time in which a reform is passed 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.

The simulated instruments approach would also be problematic if the reform-induced household sorting directly affected mobility, for example through changes in the composition of children in a CZ or through peer effects. Chetty et al. (2014), however, assign each child to the CZ of her parents when they first claimed her as a dependent. In addition, given that CZs represent local labor markets, most of the sorting is likely to happen *within* as opposed to *between* CZs. This partially mitigates these concerns.

Figure IV 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. In Texas, where school finance reforms were implemented in 1991 and 1993, simulated revenues understate actual revenues in both high-spending and low-spending districts. In Wisconsin, which had a reform in 1996, simulated revenues are higher than actual revenues in both types of districts. In Michigan, which passed a reform in 1993, simulated revenues are

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

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

higher than actual revenues for high-spending districts, but lower for low-spending districts.

The difference between simulated and actual revenues 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. Figure AVI shows the relationship between the percentage change in house prices after a reform and the difference between actual and simulated revenues. A positive correlation confirms that 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.

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 F-statistic of the first stage, shown in column 3 of Table II, is equal to 39.16.[31]

7 Effects of Equalization on Intergenerational Mobility

The goal of my empirical analysis is to study the effect of equalization in school revenues across districts within each state, captured by a decline in β in equation (7) and generated by school finance reforms, on intergenerational mobility of children exposed to these reforms while in school. Identification of this effect leverages the heterogeneity in exposure to an equalized funding system across cohorts within each state, given by differences in the timing and the effectiveness of these reforms in equalizing revenues.

Figure V illustrates the variation in mobility across cohorts in states which experienced an "effective" school finance reform (i.e. one which resulted in a negative post-reform β or a decline in β of at least 50 percent), an "ineffective" reform, and no reform at all. Mobility is measured as the expected income rank of children with parents on the 25th percentile of the national distribution. This rank increases by almost two percentiles between the cohorts of 1980 and 1986 for children in CZs with an effective reform; it does not vary across cohorts for children exposed to ineffective reforms; and it declines by one percentile for children in CZs without a reform.[32]

[31] Appendix Figure AVII shows a binned scatterplot of β and β^s and reveals a strong positive correlation between these two variables.

[32] Figure V shows point estimates and confidence intervals of the coefficients $\delta_{1980} - \delta_{1986}$ in the regression $m_{cb} = \sum_{t=1980}^{1986} \delta_t \mathbb{1}(b = t) + \varepsilon_{cb}$, where m_{cb} is mobility of CZ c and cohort b. The coefficients are estimated

7.1 OLS Estimates

While suggestive of an increase in mobility across cohorts in states with effective reforms, Figure V does not directly exploit the timing of the reform nor the exact change in β. To more formally study the effect of equalization on mobility of children with parents in different percentiles of the income distribution, I estimate the following equation:

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

where the variable M_{cbx} is the expected percentile of children in CZ c, cohort b, and with parental income in the x-th percentile within the CZ. One observation corresponds to a birth cohort, CZ, and percentile of parental income in the CZ (either the 10th, 25th, 50th, 70th, 90th, or 99th). The variable $\hat{\beta}_{s(c)b}$ is the estimated state and cohort-specific measure of equalization described in the previous section ($s(c)$ denotes the state where CZ c is located). CZ fixed effects κ_c control for CZ-specific, time-invariant determinants of mobility, and cohort fixed effects τ_b control for time 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.[33] The variable ω_{cbx} is an error term.

In this specification, the parameter δ_0 captures the effect of an increase in β, i.e., a *decline* in equalization, on the expected income percentile of children with the lowest-ranked parental income in the national distribution. The parameter δ measures instead how much this effect varies as the parental income rank increases. I standardize $\hat{\beta}_{sb}$ across all CZs and cohorts, and I cluster standard errors at the state level and at the year level (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

separately for the three groups, and observations are at the CZ $\times$ cohort level. The coefficient δ_{1980} is normalized to equal zero for all the three groups. Standard errors are clustered at the CZ level.

[33] 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.

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 in the 10th percentile, a 3.2 percentile increase for children with parental income in the 25th percentile, and a smaller 1.6 percentile increase for children with parental income in the 90th percentile. These estimates are robust to controlling for state fixed effects (Table II, column 2).

In Figure VI (solid line) I relax the linearity restriction of equation (9) and I allow the effect of a decline in β to vary by decile of parental income in a flexible way. These estimates reveal that the relationship between the effect of a decline in β and parents' rank in the national income distribution is close to linear; furthermore, the effect is positive across the whole distribution of parental income. Controlling for CZ fixed effects, a one-standard-deviation reduction in β is associated with a 3.3 percentile increase in mobility for children with parents in the first decile (p-value equal to 0.16), a 3.6 percentile increase for children with parents in the second decile (p-value equal to 0.15), and a 1.4 percentile increase for children with parents in the top decile (p-value equal to 0.53).

7.2 Two-Stages Least Squares Estimates

OLS estimates of the effects of β on mobility are likely to suffer from endogeneity bias generated by changes in districts' tax bases after a school finance reform. These estimates cannot therefore be interpreted as causal. To address endogeneity, in columns 4 and 5 of Table II I re-estimate the specifications in columns 1 and 2 via 2SLS, using β^{sim} as an instrument for β. Estimates of the first-stage regression, shown in column 3 of Table II, indicate that β^{sim} is a strong instrument for β, with a F-statistic equal to 39.16.

2SLS estimates confirm the positive relationship between equalization and mobility, but yield larger effects. Controlling for state 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 II, column 4, significant at 10 percent). A positive estimate for δ indicates that this effect decreases by 0.025 percentiles with each additional percentile of parental income (estimate of $\beta\times$ *parent centile*, Table II, column 4, significant at 1 percent). This implies that the same reduction in β leads to a 5.6 percentile increase for children with parental income in the 10th percentile, a 5.2 percentile increase for children with parental income in the 25th percentile, and a 3.5 percentile increase for children with parental income in the 90th percentile. Estimates are slightly smaller when controlling for state fixed effects (Table II, column 4). Importantly, 2SLS estimates are approximately 50 percent larger than OLS.

In Figure VI (Panel B), 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) and to a 5.6 percentile increase for children with parental income in the second decile (significant at 10 percent). The same estimate is equal to 3.3 percentiles for children with parental income in the top decile (p-value equal to 0.28).

These results also indicate that the average reform, which decreases β by approximately 0.045 (or 0.64 of a standard deviation), would increase mobility of children from families on the 25th percentile by 3.3 percentiles, and close approximately 10 percent of the gap between the lowest-mobility CZ (Clarksdale, MS) and the highest-mobility CZ (Sioux Center, IA). Perhaps surprisingly, these results show no evidence of a negative effect of equalization on students from families in the top percentiles of the income distribution. This finding might seem at odds with the prediction of the model that equalization should lower mobility for children from families above the income median. It should be noted, however, that this prediction refers to the median income in the state, whereas the results above are expressed in terms of parents' position in the national distribution. Furthermore, one should keep in mind that some of the reforms (and most of those passed after 1990) had an adequacy motive and ended up increasing expenditure in all school districts within a state (albeit more in poorer ones), which implies that wealthier districts did not necessarily lose resources as a consequence (Hoxby, 2001).

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 III, 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=0.1704, Table

III, 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 III, 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, a 14.9 percent increase for children with parental income in the 25th percentile, and a 9.5 percent increase for children with parental income in the 90th percentile. The average reform, which leads to a decline in β of approximately 0.07 standard deviations, leads to a 1.13 percent increase in income for children with parental income in the 10th percentile. Estimates are robust to controlling for state fixed effects (column 4).

OLS estimates, shown in columns 1 and 2 of Table III, are smaller than 2SLS and less precise. The change in income associated with a one-standard-deviation reduction in β is 10.1 percent for children with parental income in the 10th percentile, 8.9 percent for children with parental income in the 25th percentile, and 3.9 percent for children with parental income in the 90th percentile. The differences between OLS and 2SLS, once more, reveal how failing to account for the endogeneity of post-reform expenditure can lead to severely underestimating the effects of school finance equalization on children's outcomes.

7.3 Heterogeneous Effects of Equalization by Length of Exposure to a Reform

The effects of equalization in school revenues and expenditures could differ depending on whether equalization happens earlier or later during a child's education path. On the 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 in high school if it facilitates the transition to college for lower-income children and if college attendance is an important engine of mobility.

To explore this potential heterogeneity, I separately estimate the effects of the decline in β experienced while in elementary, middle, or high school. 2SLS estimates of δ_0 and δ, shown in Table IV, indicate that the effects of equalization are largest when experienced during high school.

A one-standard-deviation reduction in β experienced during elementary school (grades 1 to 5) leads to a 2.7 percentile increase in the income rank of children with parents at the bottom of the income distribution (with an estimate of β equal to -2.6676, Table IV, column 1, significant at 5 percent). This effect declines by 0.020 percentiles with each additional percentile of parents' income (estimate of $\beta \times$ *parent centile* equal to 0.0204, Table IV, column 1, significant at 1 percent). These estimates imply that this reduction in β leads to a 2.5 percentile, 2.2 percentile, and 0.8 percentile increase in mobility for children with parental income in the 10th, 25th, and 90th percentile respectively.

By comparison, a one-standard-deviation decline in β experienced between grades 5 and 8 leads to a larger 3.9, 3.5, and 2.0 percentile increase in mobility for children with parental income in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -4.1323 and of $\beta \times$ *parent centile* equal to 0.0238, Table IV, column 3). Estimates are largest for high school: The same reduction in β leads to a 5.4, 5.0, and 3.4 percentile increase in income ranks for children with parental income in the 10th, 25th, and 90th percentile respectively (with an estimate of β equal to -5.6131 and of $\beta \times$ *parent centile* equal to 0.0246, Table IV, column 5, significant at 10 and 1 percent respectively). Estimates are only slightly smaller when controlling for state fixed effects (Table IV, columns 2, 4, and 6).[34]

Overall, these estimates indicate that the positive effects of equalization on low-income children are largest if experienced in the moment that immediately precedes the transition between K–12 education and college. While this finding partially contrasts with the literature on early-childhood investments, it hints at the importance of college attendance for intergenerational mobility, already suggested by Rothstein (2019), which I directly explore in the next section. Once more, the difference between OLS and 2SLS estimates highlights the importance of accounting for the endogeneity in post-reform revenues in this context.

7.4 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 effect 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 CZ has one district with per capita income equal to \$25,000 and per pupil expenditure equal to \$7,000 and one district with per capita income equal to \$75,000 and per pupil expenditure equal to \$9,000. The second

[34] OLS estimates are shown in Appendix Table AI.

CZ has one district with per capita income equal to \$15,000 and per pupil expenditure equal to \$5,500 and one district with per capita income equal to \$85,000 and per pupil expenditure equal to \$8,200. Both CZs have an estimated β equal to 0.23.[35] Due to a more unequal income distribution, however, children in the lowest-spending district in the second CZ will receive \$2,700 less compared with children in the highest-spending district in the same CZ (or 49 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 effects of equalization across CZs with different income inequality, I re-estimate equation 9 separately for CZs above and below the national median of the percentage difference in per capita income between the richest and the poorest district.[36]

Table V shows the results of this exercise. 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," 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 deviation decline in β in "Low inequality" CZs leads to a 4.9 percentile increase in mobility for children with parents at the bottom of the income distribution and to a 4.6, 4.2, and 2.8 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 V, column 1, p-values equal to 0.19 and 0.02).

These effects are instead much larger in "High inequality" CZs. The same decline in β leads to a 6.4 percentile increase in mobility for children with parents at the bottom of the income distribution and to a 6.2, 5.8, and 4.7 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 V, column 3, significant at 10 and 1 percent respectively). Estimates are robust to controlling for state fixed effects (Table V, column 4).[37]

[35] $\beta = \frac{\log(9{,}000)-\log(7{,}000)}{\log(75{,}000)-\log(25{,}000)} = \frac{\log(8{,}200)-\log(5{,}500)}{\log(85{,}000)-\log(15{,}000)} = 0.23.$

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

[37] OLS estimates are shown in Table AII.

7.5 Equalization and Income Segregation

The effects of a decline in β could also depend on 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 in these districts 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. I measure segregation using the Theil index of districts' 1990 income within each CZ.[38].

Estimates of δ_0 and δ for CZs with "Low segregation" (i.e., in the bottom quartile) and with "High segregation" (in the top quartile) are shown in Table VI. Controlling for CZ fixed effects, a one-standard deviation decline in β in "Low segregation" CZs leads to a 5.49 percentile increase in mobility for children with parents at the bottom of the income distribution and to a 5.2, 4.8, and 3.6 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 V, column 1, significant at 10 and 1 percent).

Equalization is more effective in CZs with high income segregation. The same decline in β leads to a 6.07 percentile increase in mobility for children with parents at the bottom of the income distribution and to a 5.8, 5.5, and 4.3 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 VI, column 3, p-values equal to 0.11 and 0.001 respectively). Estimates are robust to controlling for state fixed effects (Table VI, column 4).[39]

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

7.6 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

[38]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.

[39]OLS estimates are shown in Table AIII.

not dependent on this imputation, in Table AV I re-estimate the main specification with a version of β estimated using income data from 1990 for all years. These estimates are essentially identical to those in Table II, 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 expenditure only in some districts (but not all) and therefore involve a much larger absolute change in expenditure in the affected districts. Table AIV shows 2SLS estimates of the main specifications, separately for one-state and multi-state CZs. Estimates are fairly comparable across the two groups, indicating that the results are not driven by either type of CZs.

8 Channels: School Inputs and Intermediate Outcomes

The results described so far show that equalizing school funding across richer and poorer districts boosts intergenerational mobility, and especially so for children from low-income families. I now investigate the mechanisms behind these effects, focusing on the role of school inputs and on the effects 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 the most important input for student learning (Chetty et al., 2014), and an adequate number of teachers per student is fundamental for 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.[40]

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

I investigate the effects of a reduction in β on districts' 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:

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

where TS_{dt} is the teacher-student ratio of district d, located in state s, in year t; the variable q_{dt}^{nq} equals 1 for districts with per-capita income in the n-th quartile of the within-state distribution, and the vectors γ_s and τ_{st} control for state 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 VII shows OLS and 2SLS estimates of equation 10. OLS results indicate a positive relationship between equalization and the number of teachers per student in low-income districts and a negative relationship in high-income ones; these effects, however, are indistinguishable from zero (Table VII, column 1). 2SLS estimates, shown in columns 3 and 4, yield larger and marginally significant positive effects on low-income districts and negligible effects on high-income ones. Controlling for state fixed effects, a one-standard-deviation reduction in β leads to 0.0061 additional teachers per student in districts in the bottom quartile, or 8.7 percent more (Table VII, column 3, significant at 10 percent) and to 0.0015 additional teachers per student in districts in the top quartile (Table VII, column 3, p-value equal to 0.66).

Although imprecise, these results suggest that equalizing school spending across wealthier and poorer districts promotes intergenerational mobility by closing the gap in educational inputs between low-income and high-income districts. This gap is reduced through an improvement in the teacher-student ratio in low-income districts, with no effect on high-income ones.

8.2 Intermediate Outcome: College Enrollment

College enrollment is associated with mobility (Rothstein, 2019; Chetty et al., 2017). Equalization of school resources can therefore promote mobility through an equalization in college attendance across students with low-income and high-income parents. To test this hypothesis, I study whether school finance equalization leads to an increase in the probability of college enrollment for children with parents in different points of the national income distribution. To do so I re-estimate equation (9) using the probability of college enrollment at age 19 as the dependent variable, expressed in percentage points and measured separately for each CZ, cohort, and parent percentile in the CZ.

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 VIII, 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 point for each additional percentile of parental income (estimate of $\beta\times$ *parent centile*, Table VIII, column 1, significant at 5 percent). These estimates imply that the same reduction in β leads to a 7.6, 7.3, and 6.1 percentage point increase in the probability of college enrollment for children with parents in the 10th, 25th, and 90th percentile. Estimates are robust to controlling for state fixed effects (Table VIII, column 2). OLS estimates are shown in Table AVI.

Estimating the effect of a decline in β at different points between grades 1 and 12 confirm that equalization in school revenues is most effective when experienced during middle and high school. A one-standard deviation decline in β during middle school leads to a 9.5, 9.3, and 8.2 percentage point increase in the probability of college enrollment for children with parental income in the 10th, 25th, and 90th income percentile, which correspond to a 17, 16, and 14 percent increase (Table VIII, column 7). By comparison, the same decline leads, if anything, to a 3.3, 3.6, and 4.9 percentage point decline when experienced during elementary school (although indistinguishable from zero, Table VIII, column 3), and to a 7.4, 7.2, and 6.3 percentage point increase when experienced during high school (Table VIII, column 5).

These findings suggest that equalization of school expenditure improves long-run economic outcomes of children by improving their educational attainment. Notably, equalization appears to have positive effects for all children across the distribution of parental income, although the effects are larger for lower-income students. Once more, failing to account for endogeneity in β leads to underestimating these effects (Table AVI).

9 Conclusion

This paper has studied 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, I find that exposure to a more equalized scheme increases mobility of all children, especially those from low-income families. My results suggest that equalization boosts 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 household incentives to sort across districts are idiosyncratic to each reform, which implies that each reform leads to different changes in house prices (i.e., some lead to an increase, some to a decrease, some to no change). This implies that the extent of this endogeneity varies across states and over time.

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. This procedure allows me to separate the (exogenous) change in expenditure levels and distribution driven by changes to the funding formula from the (endogenous) change driven by household sorting, while allowing for heterogeneity in the effects of each reform on house prices. 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, my results might appear to contrast with Rothstein (2019), who uses 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 findings, however, do not necessarily disprove Rothstein's argument. In fact, my findings 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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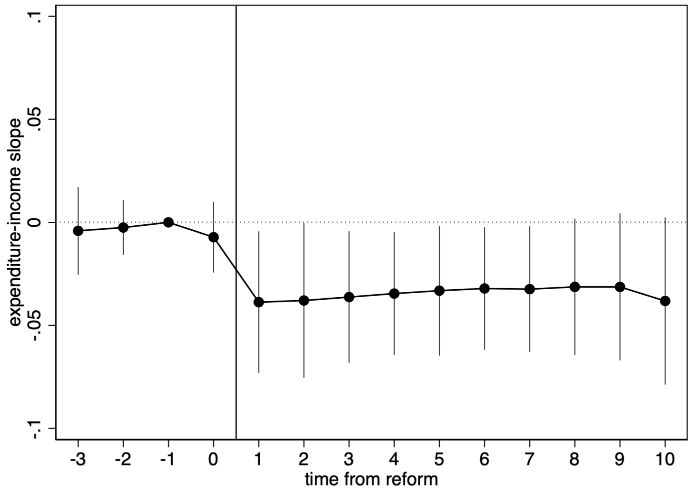
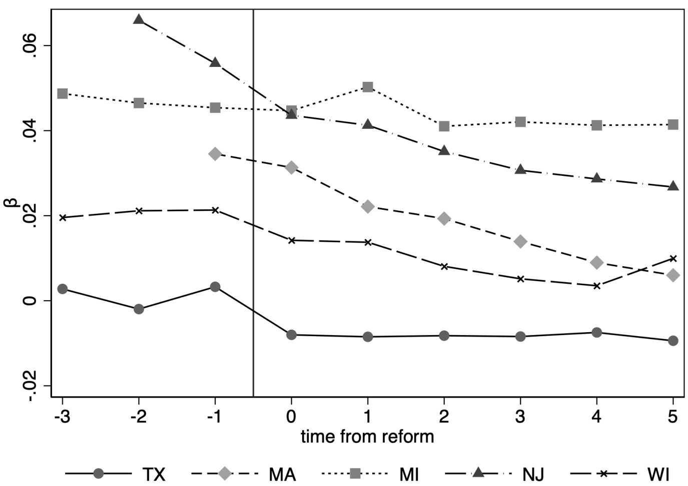
Figure I: Measures of Equalization Around Reform Years



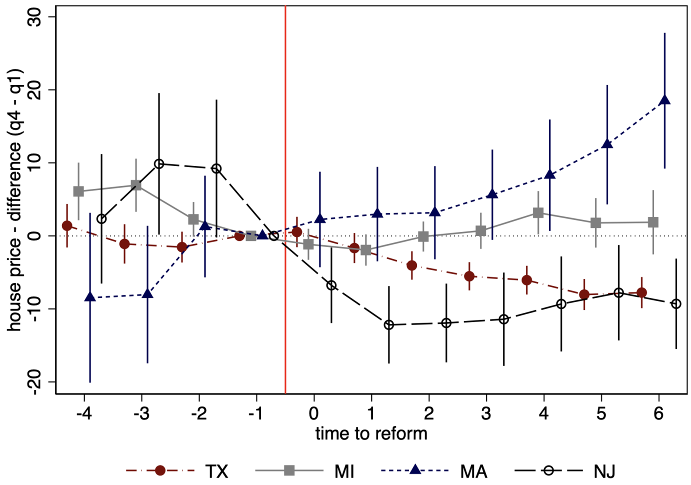
Note: Point estimates and 90 percent confidence intervals for the coefficients δ_k in regression $\beta_{st} = \sum_k \delta_k R_s 1(t - ryear_s = k) + \varepsilon_{st}$,where β_{st} is the slope coefficient in equation (7), estimated separately for each state s and year t from 1986 to 2004, R_s equals 1 if state s had a reform between 1986 and 2004, and $ryear_s$ is the year of the first reform in this time period. 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: Measures of Equalization Around Reform Years, By State



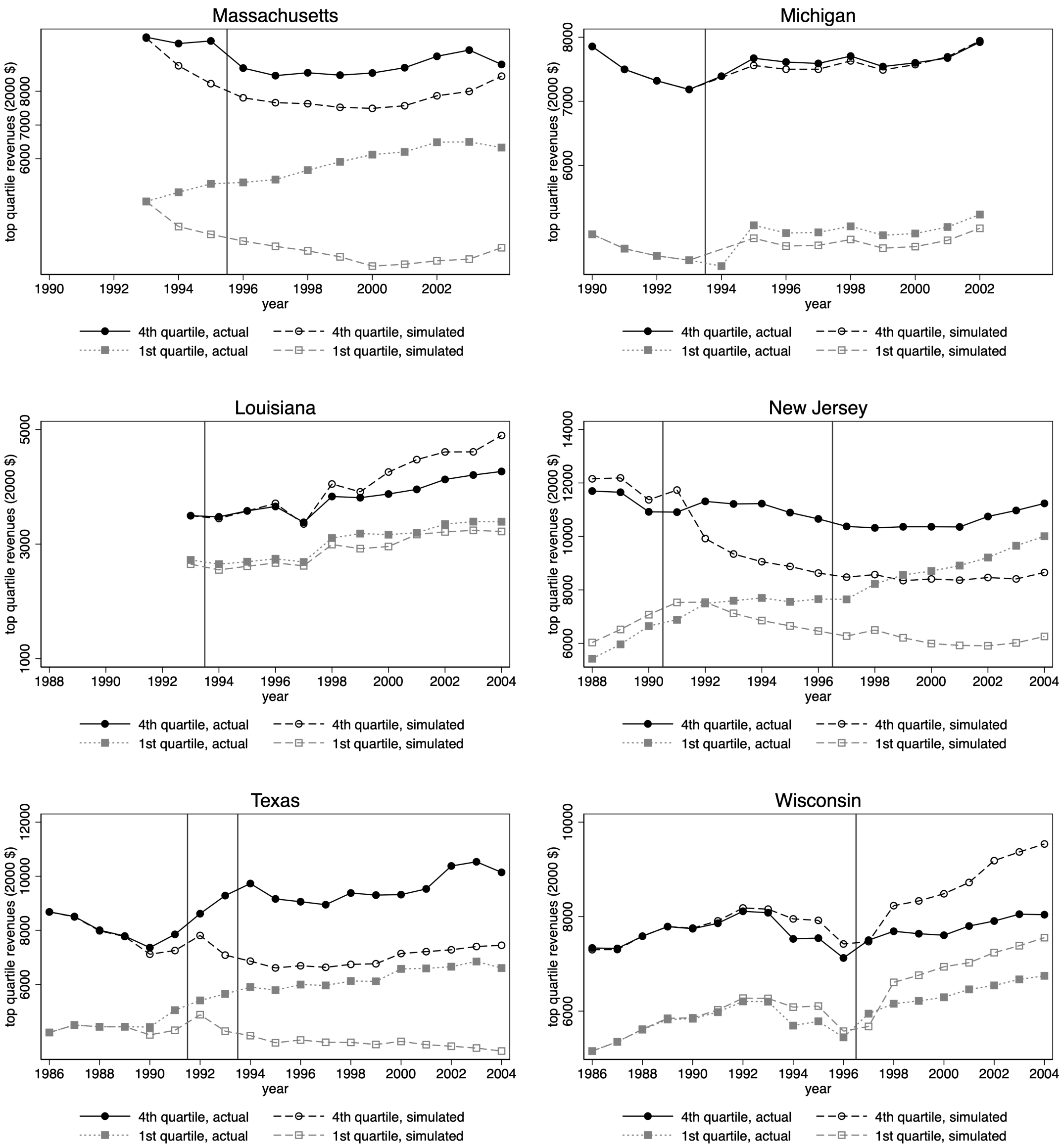
Note: The figure shows estimates of the coefficient β_{st} in the years surrounding a reform, defined in equation (7) and estimated separately for each state.

Figure III: Change in House Prices Around a School Finance Reform - Selected States



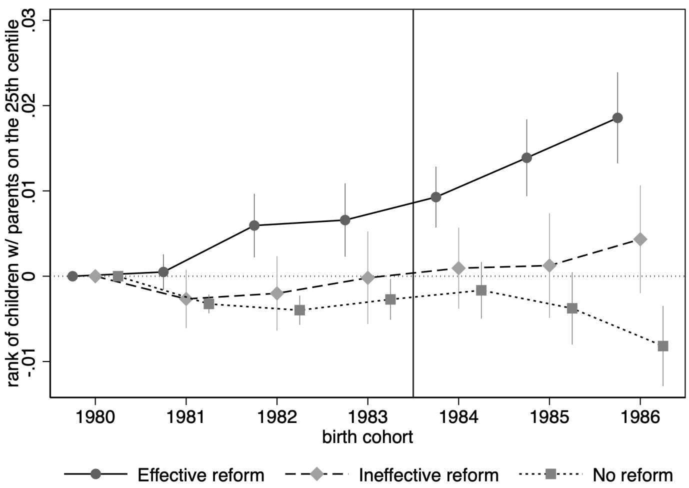
Note: Changes in average house price indexes in a 10-years window around each reform, relative to the year before the reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients δ_n in the regression $HP_{dt} = \sum_{n=-4}^{6} \delta_n R_{s(d)} 1(t - Ryear_{s(d)} = n) + \varepsilon_{dt}$, where HP_{dt} is the house price index of district d in year t, $R_{s(d)}$ equals 1 if state s where the district is located experienced a school finance reform in the years 1986-2004, and $Ryear_{s(d)}$ is the year of the earliest school finance reform. The parameters are estimated separately for each state. Observations are weighted by population. Annual House Price Indexes data are taken from the Federal Housing Finance Agency, aggregated at the district level using population weights, and cover years from 1986 to 2004.

Figure IV: Simulated and Actual Revenues, Districts Above and Below the State Median - Selected States



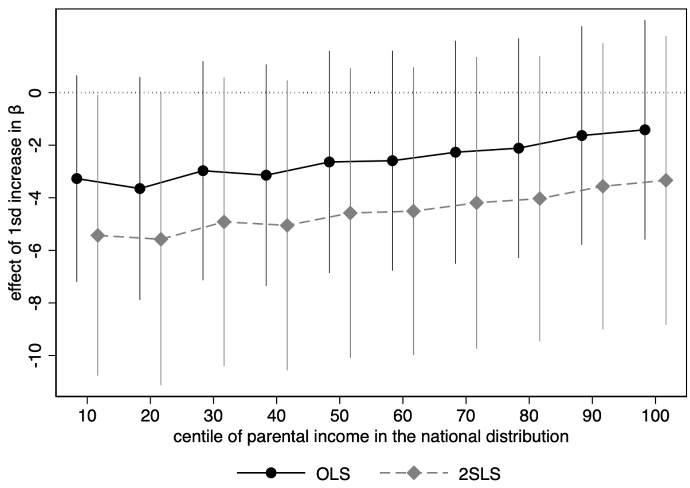
Note: Trends in simulated and actual per pupil revenues at the district level, for districts above and below the state median expenditure at the beginning of each sample. Vertical red lines denote reform years. Simulated expenditures are calculated using the funding formula in place in every state and year and pre-reform district variables.

Figure V: Changes in Intergenerational Income Mobility in States with Successful Reforms, Unsuccessful Reforms, and No Reform



Note: The figure shows the trend in intergenerational mobility (measured as the expected income rank of children with parents on the 25th percentile and relative to 1980) across cohorts, separately for states with a successful school finance reform between 1986 and 2004 (defined as producing either a negative β or a decline in β of at least fifty percent after the reform), states with an unsuccessful reform (defined as producing either a positive β or a decline in β smaller than fifty percent after the reform), and states with no reform. The first group includes Colorado, Kentucky, Montana, Nebraska, Texas, and Wisconsin; the second group includes Louisiana, Massachusetts, Michigan, Minnesota, and New Jersey; and the third group includes California, Florida, Georgia, Illinois, New York, North Dakota, Ohio, Pennsylvania, and Utah. Point estimates and confidence intervals correspond to the coefficients $\delta_{1980} - \delta_{1986}$ in the regression $m_{cb} = \sum_{t=1980}^{1986} \delta_t \mathbb{1}(b = t) + \varepsilon_{cb}$, where m_{cb} is mobility of CZ c and cohort b. The coefficients are estimated separately for the three groups. 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_{1980} is normalized to equal zero for all the three groups. Standard errors are clustered at the CZ level.

Figure VI: Effect of a Decline 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 $M_{cxb} = \sum_{d=1}^{10} \delta_d D_{d(cx)} \hat{\beta}_{s(c)b} + \kappa_c + \theta_{n(cx)} + \sigma_b + \omega_{cxb}$, 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, $\theta_{n(cx)}$ 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

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 ($)					
1986	2602	5992	644	-1914	14162
1990	2818	4565	1553	-2306	12965
2000	1615	5690	297	-8717	15415
2004	1889	7168	52	-9405	18120
Δ exp, richest vs poorest district within CZ ($)					
1986	1236	4360	563	-13816	13890
1990	1636	3355	850	-11045	14518
2000	15	5083	-387	-14780	17197
2004	331	5833	-313	-21618	20638

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

	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 Equality of Opportunity Project (www.equality-of-opportunity.org). Panel C: means and standard deviations of the slope coefficient in equation (7), estimated separately for each state and year using actual revenues (β) and simulated revenues (β^{sim}).

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

	OLS		2SLS, First stage	2SLS, Second stage	
	(1)	(2)	(3)	(4)	(5)
β	-3.8397	-3.7174		-5.8120*	-5.6920*
	(2.1545)	(2.1257)		(2.8362)	(2.8013)
$\beta \times$ parent centile	0.0246***	0.0239***		0.0253***	0.0244***
	(0.0044)	(0.0044)		(0.0044)	(0.0044)
β simulated			0.7527***		
			(0.1203)		
Parent centile FE	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes
CZ FE	Yes	No	No	Yes	No
State FE	No	Yes	Yes	No	Yes
F-stat			39.16		
N (CZ * parent centile * cohort)	13578	13578	13578	13578	13578
	Effect of 1sd decline in β, by parents' centile				
10th	3.593	3.478		5.559	5.448
25th	3.224	3.119		5.181	5.082
90th	1.622	1.562		3.539	3.497

Note: The table shows OLS estimates (columns 1 and 2) as well as 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 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 coefficient in equation (7), computed separately for each state and cohort, and standardized across all states and cohorts. The variable *parent centile* is the percentile of parents in the national income distribution. The variable β *simulated* is estimated as β using simulated revenues instead of actual revenues. All specifications include parent percentile and cohort fixed effects; columns 1, 3, an 4 include CZ fixed effects, and columns 2 and 5 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.

Table III: School Finance Equalization and Intergenerational Mobility. 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*
	(0.0565)	(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
F-stat				
N (CZ * parent centile * cohort)	13578	13578	13578	13578
	Effect of 1sd decline in β, by parents' centile			
10th	0.101	0.098	0.162	0.158
25th	0.089	0.086	0.149	0.146
90th	0.039	0.038	0.095	0.094

Note: 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 coefficient in equation (7), computed separately for each state and cohort, and standardized across all states and cohorts. The variable *parent centile* is the percentile of parents in the national income distribution. The variable β is instrumented with β *simulated*, estimated 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.

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

	Elementary school		Middle school		High school	
	(1)	(2)	(3)	(4)	(5)	(6)
β	-2.6676**	-2.6431**	-4.1323	-4.0606	-5.6131*	-5.3347*
	(1.0858)	(1.0461)	(2.1851)	(2.1254)	(2.4776)	(2.4460)
$\beta\times$ parent centile	0.0204***	0.0196***	0.0238***	0.0232***	0.0246***	0.0237***
	(0.0039)	(0.0038)	(0.0039)	(0.0039)	(0.0043)	(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
F-stat						
N (CZ * parent centile * cohort)	10362	10362	12756	12756	12930	12930
	Effect of 1sd decline in β, by parents' centile					
10th	2.464	2.447	3.895	3.829	5.368	5.097
25th	2.158	2.152	3.538	3.481	4.999	4.742
90th	0.834	0.877	1.993	1.975	3.403	3.199

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 coefficient in equation (7), computed separately for each state and cohort, and standardized across all states and cohorts. The variable *parent centile* is the percentile of parents in the national income distribution. The variable β is instrumented with β *simulated*, estimated as β using simulated revenues instead of actual revenues. In columns 1 and 2, β is the average over elementary school years (grades 1-5); in columns 3 and 4 it is the average over middle school years (grades 6-8); and in columns 5 and 6 it is the average over high school years (grades 9 to 12). 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.

Table V: 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*
	(3.2730)	(3.2210)	(3.1136)	(3.0937)
$\beta \times$ 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
F-stat				
N (CZ * parent centile * cohort)	5586	5586	7950	7950
	Effect of 1sd decline in β, by parents' centile			
10th	4.595	4.418	6.152	6.174
25th	4.191	4.062	5.821	5.822
75th	2.847	2.876	4.717	4.647

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 coefficient in equation (7), computed separately for each state and cohort, and standardized across all states and cohorts. The variable *parent centile* is the percentile of parents in the national income distribution. The variable β is instrumented by β *simulated*, estimated as β using simulated revenues instead of actual revenues. All specifications include parent percentile and cohort fixed effects; columns 1 and 3 include CZ fixed effects, and columns 2 and 4 include state fixed effects. "Low Inequality" ("High Inequality") refers to CZs in the bottom (top) quartile percent of the distribution 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.

Table VI: 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
	(2.6900)	(2.6719)	(3.2120)	(3.2016)
$\beta \times$ 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
F-stat				
N (CZ * parent centile * cohort)	5880	5880	7698	7698
	Effect of 1sd decline in β, by parents' centile			
10th	5.233	5.181	5.835	5.778
25th	4.853	4.819	5.479	5.411
75th	3.587	3.611	4.291	4.189

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 coefficient in equation (7), computed separately for each state and cohort, and standardized across all states and cohorts. The variable *parent centile* is the percentile of parents in the national income distribution. The variable β is instrumented by β *simulated*, estimated as β using simulated revenues instead of actual revenues. All specifications include parent percentile and cohort fixed effects; columns 1 and 3 include CZ fixed effects, and columns 2 and 4 include state fixed effects. "Low Segregation" ("High Segregation") refers to CZs in the bottom (top) quartile of the distribution 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.

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

	OLS		2SLS	
	(1)	(2)	(3)	(4)
$\beta\times$ q1	-0.0040	-0.0044	-0.0061*	-0.0065
	(0.0024)	(0.0030)	(0.0029)	(0.0039)
$\beta\times$ q2	-0.0017	-0.0003	-0.0040	-0.0031
	(0.0023)	(0.0025)	(0.0026)	(0.0019)
$\beta\times$ q3	-0.0004	-0.0013	-0.0028	-0.0029
	(0.0024)	(0.0032)	(0.0026)	(0.0020)
$\beta\times$ q4	0.0004	-0.0018	-0.0015	-0.0034
	(0.0030)	(0.0047)	(0.0034)	(0.0052)
Year FE	Yes	No	Yes	No
State FE	Yes	No	Yes	No
District FE	No	Yes	No	Yes
Quartile FE	Yes	Yes	Yes	Yes
N (district * year)	64214	64140	64214	64140
Y-mean	0	0	0	0

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, for the years 1988-2004. The variable β is defined as the OLS estimate of the coefficient in equation (7), computed separately for each state and year, and standardized across all states and years. The variable qX equals 1 for districts with median household income in the X 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.

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

	All		Elementary		Middle		High	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
β	-0.0777	-0.0746	0.0306	0.0324	-0.0968*	-0.0955*	-0.0755	-0.0710
	(0.0971)	(0.0959)	(0.0373)	(0.0377)	(0.0401)	(0.0396)	(0.0395)	(0.0385)
$\beta\times$ parent centile	0.0002**	0.0001	0.0002**	0.0002*	0.0002*	0.0001	0.0001	0.0001
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Parent centile FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Cohort FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CZ FE	Yes	No	Yes	No	Yes	No	Yes	No
State FE	No	Yes	No	Yes	No	Yes	No	Yes
N (CZ * parent centile * cohort)	13296	13296	12690	12690	12864	12864	12540	12540
Mean of dep. var.	0.556	0.556	0.556	0.556	0.554	0.554	0.552	0.552
	Effect of 1sd decline in β, by parents' centile							
10th	0.076	0.073	-0.033	-0.034	0.095	0.094	0.074	0.070
25th	0.073	0.071	-0.036	-0.037	0.093	0.092	0.072	0.068
90th	0.061	0.061	-0.049	-0.048	0.082	0.084	0.063	0.062

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 coefficient in equation (7), computed separately for each state and cohort, and standardized across all states and cohorts. The variable *parent centile* is the percentile of parents in the national income distribution. The variable β is instrumented with β *simulated*, estimated using simulated revenues instead of actual revenues. In columns 3 and 4, β is the average over elementary school years (grades 1-5); in columns 5 and 6 it is the average over middle school years (grades 6-8); and in columns 7 and 8 it is the average over high school years (grades 9 to 12). All specifications include parent percentile and cohort fixed effects; columns 1, 3, 5, and 7 include CZ fixed effects, while columns 2, 4, 6, and 8 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.