School Finance Equalization and Intergenerational Mobility *

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Abstract

School finance equalization reforms have been implemented across US states in the past 40 years to reduce differences in per-pupil expenditure across school districts in each state. This paper exploits such changes to examine the effects of equalization on students' intergenerational income mobility, defined as children's income centile for a given parent's centile. I use the correlation between per-capita income and per-pupil expenditure, estimated separately for each US commuting zone and for seven cohorts of students, as a measure of expenditure inequality. Using migration and house prices data, I show that Tiebout sorting of households across districts creates a problem of endogeneity in post-reform expenditure. I address this issue with a simulated instruments approach which relies on the exogeneity of the funding formula, and uses the formula parameters to simulate the revenues each district would have received in the absence of sorting. Two-stages least squares indicate that equalization is beneficial for mobility of both low-income and high-income students. Furthermore, differences between OLS and 2SLS estimates indicate that endogeneity could lead to underestimate the effects of these reforms. An analysis of mechanisms suggests that equalization affects intergenerational mobility by reducing cross-district differences in inputs (such as the number of teachers) and in educational attainment.

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1 Introduction

School finance equalization is one of the the most dramatic waves of state-level public education reforms across the US, and it has affected nearly every school district in the country. The common objective of these reforms is to reduce differences in per-pupil expenditure across school districts. These differences have arisen because, in most states, districts have drawn a considerable share of their revenues from local levies, such as property taxes. When this is the case, wealthier districts - which have a larger tax base - are able to spend more compared with poorer districts.

To equalize expenditure, starting from the early 1970s states have changed their school finance plans, i.e. the way in which state funds are redistributed to districts. These plans are summarized by funding formulas, used to allocate state aid to each district and to determine district revenues as a combination of state funds and local levies. While sharing a common equalization objective, school finance equalization reforms have been implemented in different ways, and in the form of different plans, across states and over time. As a result, reforms implemented under the same name and purpose have had very different effects on both the levels and the distribution of school expenditure across districts within the same state (Hoxby, 2001).

In theory, equalization of school expenditure also equalize outcomes between wealthier and poorer students. Investments in human capital are among the major determinants of future income (Becker and Tomes, 1979). Differences in the ability of households to invest in their offspring's education make income prospects of young generations heavily dependent on the economic status of their families. As a result, policies which affect public expenditure on education, both in its levels and in its distribution across children, have the potential to greatly influence the extent to which economic fortune is transmitted across generations (Becker and Tomes, 1994).

Empirical evidence on the effects of increased funding for public schools on children's short- and long-term outcomes, however, is mixed. Observational studies dating back a few decades have found zero or small effects of an increase in school expenditure (Coleman et al., 1966; Hanushek, 1986, 1997, 2003). Other works, using quasi-experimental (Card and Krueger, 1992) and experimental variation (Krueger, 1999; Dynarski et al., 2013; Hyman, 2013), 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 investment in public schools might be beneficial for students.[1]

In general, the literature on the effects of increases in school expenditure on student outcomes has struggled to identify causal effects, due to a lack of exogenous variation in school funding. To address this issue, a few recent studies have exploited school finance equalization 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., 2016), and long-term outcomes, such as earnings (Jackson et al., 2015). The focus of these studies, however, has been on the effects of increases in the levels of expenditure, as opposed to changes in its distribution across states and and local labor markets. If resources spent on public schools matter for students' outcomes, a more equal distribution of resources can equalize students' opportunities by "leveling the playing field", and in turn improve the relative performance of disadvantaged students compared with students of higher socioeconomic status.

I test this hypothesis by studying the effects of equalization in school expenditure, driven by school finance reforms, on intergenerational mobility of 14 cohorts of children born between 1980 and 1986. The intrinsic nature of school finance reforms calls for some caution in the use of post-reform expenditure as an exogenous variable, for at least two reasons. First, these reforms almost always involve an increase in state expenditure on public schools, and are therefore more likely to happen during good economic times. Second, school finance equalization reforms change the relationship between the "price" taxpayers face for a public good, and the public good that they receive in return. This might induce households to "vote with their feet" (as suggested by Tiebout, 1956), i.e. to move across districts, based on their preferences and on their budget constraint. This sorting affects each district's tax base, and in turn its revenues, in an endogenous way.

This source of endogeneity can in principle cause a serious bias in the estimates of an increase in expenditure on student outcomes. A simple analysis of migration and house prices data indicates a significant increase in migration to and from each county, as well as an increase in the change in house prices at the zipcode level, following a school finance reform. This evidence suggests that households do responds to these reforms by moving. In spite of it, existing studies have largely ignored the issue.

[1] Burtless (2011) provides a detailed survey of the existing literature on the issue.

I explicitly address this endogeneity concern using simulated instruments to estimate the causal effects of school finance equalization on students' intergenerational income mobility. My empirical strategy can be described as follows. First, I measure inequality in school expenditure with an approach similar to Hoxby (1998) and Card and Payne (2002), and I estimate the correlation between districts' per-pupil expenditure and per-capita income, separately for each US commuting zone (CZ thereafter) and grade 1-12 school year of 14 cohorts of children, born between 1980 and 1986. School finance equalization reforms provide variation in this measure of inequality of expenditure, which I refer to as β, both across CZs and over time. I then study the effects of equalization, i.e. a decline in β, on absolute intergenerational mobility of children.

I measure mobility with the expected rank of children in the national children's income distribution, given their parents' rank on the national parents' income distribution. This measure of "absolute mobility" was first introduced by Chetty et al. (2014), who have calculated it for 7 cohorts of children and 722 commuting zones (CZs) using the population of US tax records. OLS results indicate that a one-standard deviation reduction in β is associated with a 1.4 centiles increase in mobility for children of parents on the 10th centile, a 0.875 centiles increase for children from the 25th percentile, and with a decline in mobility for children of parents with income above the national mean.

To address endogeneity in the level and the distribution of post-reform school expenditure, I exploit the exogeneity of the specific funding formula in place in each state and year in a simulated instruments framework (Currie and Gruber, 1996; Gruber and Saez, 2002). Specifically, I first construct the formulas in place in each school district and year, using information from different sources. I then use these formulas to simulate each district's post-reform revenue, keeping the characteristics of each district entering the formula (such as property values, enrollment, income, etc.) fixed at their pre-reform values. This allows me to isolate the variation in the distribution of districts' revenues and expenditures generated by changes in the formula, and to control for endogenous sorting of households across districts. As a last step, I estimate a simulated version of β, defined as the CZ-year specific correlation between simulated revenues per-pupil and per-capita income, and I use it as an instrument for β to estimate its effects on intergenerational mobility.

Two-stages least squares estimates confirm a significant role of school finance equalization in promoting intergenerational mobility, but yield much larger effects: A one-standard devi-

ation reduction in β leads to a 4.6 centiles increase in mobility for children of parents on the 10th centile, a 3.3 centiles increase for children from the 25th centile, and with a decline in mobility for children of parents with income above the national mean. These findings confirm the importance of equalization in school resources across richer and poorer districts for long-run economic opportunities of children, and are consistent with the literature on the positive effects of increased spending for outcomes of low-income students. In addition, they highlight the importance of addressing the endogeneity in post-reform expenditure caused by Tiebout sorting across districts, when studying the effects of changes in school resources driven by changes in the funding scheme, and suggest that the existing evidence on the effects of school finance equalization should be reassessed taking this issue into account.

Grade-specific effects of equalization on mobility indicate that equalization is most effective when experienced during primary school, as well as during high school. This highlights the importance of early childhood investments in education, but it also indicates that equalization might improve mobility through an effect on educational attainment (Rothstein, 2016). I formally study the mechanisms through which school finance equalization affects intergenerational mobility. First, I show that equalizing revenues and expenditures reduces disparities in basic inputs, such as the number of teachers per student, between richer and poorer districts. Next, I study the effects of equalization on two medium-run outcomes such as high school completion and college enrollment, and show that a reduction in β leads to a reduction in the differences in these two outcomes across districts. Once more, not accounting for the endogeneity of β leads to severely underestimate its effects.

Other than demonstrating the importance of accounting for Tiebout sorting when studying the effects of changes in public school funding schemes, this paper contributes to the literature on intergenerational mobility by being one of the few to focus on its causes. In fact, most of the literature has focused on estimating various measures of mobility using different samples.[2] Recent research on the population of US taxpayers has estimated measures of mobility at a more disaggregated geographic level, and it has unveiled large differences across small geo-

[2]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 U.S (Solon, 2002). Due to differences in the underlying income distribution in each of these countries, however, international comparisons are typically difficult to perform.

graphic labor markets (Chetty et al., 2014).[3] These differences are correlated with a range of socioeconomic characteristics of each community, including school quality (Chetty et al., 2014), suggesting a role for education policies in boosting mobility. Empirical evidence on the *causes* of intergenerational mobility, however, is still very scarce, despite being fundamental for policy design. Results from this paper demonstrate the causal effects of a reduction in spending inequalities across public school districts on intergenerational income and education mobility, even at a fine geographic level.

The rest of the paper proceeds as follows. Section 2 provides details on school finance equalization reforms. Section 3 introduces a simple theoretical framework of the relationship between school finance equalization and intergenerational mobility. Section 4 describes the datasets used in the empirical analysis. Section 5 describes the measure of inequality of school expenditure. Section 6 outlines the empirical strategy and the instrumental variables approach. Section 7 presents the main estimates of the effects of school finance equalization on intergenerational mobility. Section 8 investigates the mechanisms through which school finance equalization affects intergenerational mobility. Section 9 concludes.

2 School Finance Equalization Reforms

In virtually all US states and until the early 1970s, school districts have drawn most of their revenues from local property taxes, and received state transfers under the form of categorical aid (Howell and Miller, 1997; Hoxby, 2001).[4] As the local tax base is higher in wealthier areas, high-income districts have been able to spend considerably more than low-income districts. Furthermore, capitalization of the quality of public schools into house prices has exacerbated these differences.

To correct this situation, states have implemented school finance equalization reforms. Some of these reforms have been passed in response to the overturning of financing schemes by state supreme courts; others, instead, have been 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

[3] Research based on the same data has identified neighborhood-specific "causal" effects of intergenerational mobility (Chetty and Hendren, 2016a,b). While these neighborhood effects appear correlated with neighborhood characteristics, their determinants remain unstudied to date.

[4] Categorical aid redistributes resources from high-income districts to districts serving a larger share of students who are expensive to educate (Hoxby, 2001)

spent on public schools (Card and Payne, 2002; 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., 2016).

Regardless of their specific motive, school finance equalization reforms change the financing formulas, which express 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). These formulas also define the size of the transfers from the state to the district, and they may specify spending limits, in an attempt to break the relationship between each district's income and wealth and public school expenditure. Different states have designed their own formula in a specific way. Hoxby (2001) and Jackson et al. (2014) provide a categorization of school finance plans, depending on whether they focus on ensuring a minimum level of expenditure ("foundation" or "equalization" plans), on guaranteeing a certain tax base ("guaranteed tax base"), or on providing incentives towards fiscal effort ("rewards for effort"). Almost all funding formulas, however, are a combination of two or more of these categories, and the parameters of each formula vary considerably across states and over time.

One characteristic that different school finance schemes have in common is that the basis for redistribution (or equalization), i.e. the tax base, is endogenous: Households sort across school districts depending on their preferences for public schools and their income, and these movements influence house prices. Failure of policymakers to fully understand and internalize this source of endogeneity when designing school finance plans has caused some reforms to reduce overall expenditure on public schools (or "level down", to use the language of Hoxby, 2001).[5]

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. (2016) estimate a positive and large effect of an increase in expenditure driven by adequacy reforms on NAEP scores. Works on the experience of individual states have also found positive effects of equalization on test scores (Guryan, 2001; Papke, 2005; Roy, 2011). On the other hand, Downes et al. (1997) found no effects of equalization on the distribution of test scores, and Hoxby (2001) finds

[5]One of the most famous school finance reforms was the one of California. Passed in response to the *Serrano* decision in 1976, it was followed by an unprecedented decline in expenditure (Silva and Sonstelie, 1995). More recently, Texas's "Robin Hood" plan of 1993 is estimated to have destroyed $27,000 per pupil in property values (Hoxby and Kuziemko, 2004).

mixed evidence on effects on high school dropout.

In what is possibly the only comprehensive study on 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. With the exception of Hoxby (2001), however, all these existing studies have ignored the endogeneity in post-reform expenditure caused by Tiebout sorting of households across districts. In this paper, I complement the existing evidence by studying the effects of equalization on intergenerational income and education mobility of students exposed to school finance reform, and by explicitly accounting for endogeneity using a simulated instruments approach.

3 A Simple Model of School Finance and Intergenerational Mobility

I introduce a very simple conceptual framework to guide the reader's thinking through the relationship between school finance equalization and intergenerational mobility. This simple model yields two testable predictions.

The world is populated by two generations: parents, with income x, and children, with income y. Parents and children live in school districts, which are grouped into commuting zones. School districts are responsible for the financing of local public schools. Commuting zones represent local labor markets, i.e. areas in which individuals live and work. Each child can only go to school in the district he or she lives in.

Income of a child in family i, living in school district d and commuting zone c, is determined as follows:

$$y_{idc} = \theta x_{idc} + \gamma e_{dc} \tag{1}$$

where x_{idc} is the income of the parent and e_{dc} is monetary investment in child i's education. By expressing the child's income in this way, I implicitly assume that the returns to education do not depend on the income of the parent. I also assume that investment in a child's education only depends on the school district the child lives in, i.e. I rule out the possibility of private investments in education.

School spending in district d and commuting zone c is defined as

$$e_{dc} = \alpha_c x_c + \beta_c x_{dc} \tag{2}$$

where x_c is average parental income in commuting zone c, x_{dc} is average parental income in district d, and α_c and β_c are parameters. The equation can be rewritten as:

$$e_{dc} = e_c + \beta_c(x_{dc} - \bar{x_c}) \tag{3}$$

In this expression, the variable e_c captures the level of average per-pupil expenditure in commuting zone c. The parameter β_c, on the other hand, describes the extent of equalization in school expenditure within each commuting zone. When $\beta_c = 0$, $e_{dc} = e_c$: expenditure is fully equalized across all districts in commuting zone c. When $\beta_c > 0$, on the other hand, e_{dc} depends positively on x_{dc}: richer districts (i.e. those with average income larger than the average income in the CZ) have larger expenditure, and viceversa.

The child's income can be rewritten as a function of e_c and β_c as follows:

$$y_{idc} = \theta x_{idc} + \gamma e_c + \gamma\beta_c(x_{dc} - x_c) \tag{4}$$

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_c. Absolute intergenerational income mobility of children born in families on the r centile of the national parent income distribution can be defined as:

$$M_c^r = F_y(y_{idc})|_{F_x(x_{idc})=r/100} \tag{5}$$

where $F_y(.)$ denotes the cumulative income distribution function of the child, and $F_x(.)$ denotes the cumulative income distribution function of the parent.[6] Without loss of generality I make the simplifying assumption that $x_{idc} = x_{dc}$ for every individual i living in district d. I define $Q_t(.)$ as the quantile function of the random variable t, i.e. the function that computes the quantile of a given realization of the random variable.[7] Substituting the expression for child's income from equation (1) allows me to express absolute income mobility a function of the parameter β_c:

$$M_c^r = F_y(\theta Q_x(r) + \gamma e_c + \gamma\beta_c(Q_x(r) - x_c)) \tag{6}$$

The function $F_y(.)$ is increasing, being it a cumulative distribution function. From this, it fol-

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

lows that M_c^y is increasing in β_c when $Q_x(r) - x_c$ is smaller than zero, i.e. for children in families below the mean. In the remainder of the paper I test this simple theoretical result on the relationship between intergenerational mobility and inequality in school expenditure.

4 Data

To study the effects of equalization on intergenerational mobility I use information on per-pupil expenditures and revenues, school finance reforms, per-capita household income, school inputs (number of teachers), and high school completion rates at the school district-year level; migration rates at the county-year level; house price indexes at the zip code-year level; and intergenerational mobility measures at the CZ-cohort level. Per-pupil expenditures and revenues and per-capita income are converted in 2000 dollars using the CPI. The data is briefly described below. Details on how the final dataset is constructed are included in Appendix B.

4.1 School Expenditures and Revenues

The main source of data on school district per-pupil expenditure, used to compute measures of equalization at the CZ level, is the U.S. Census Data Files on Historical Finances of Individual Governments for the years 1980, 1990, 2000, and 2010, which contain administrative data on revenues and expenditures of individual local governments (counties, municipalities, townships, school districts).[8] Per-pupil expenditure is calculated as total expenditure on education, divided by the school district population counts. To compute the simulated instruments I combine this data with information from the Finance Survey of Local Education Agencies (F-33 files) from the National Center for Education Statistics (NCES), which include revenues by source (federal, state, and local), as well as enrollment for the academic years 1989-90, 1991-92, and 1994-95 to 2009-10.

Panel A of Table 1 shows summary statistics of per-pupil expenditure. Average per-pupil expenditure increased over time, from $4,589 in 1980 to $6,350 in 1990, $7,418 in 2000, and $9,595 in 2010. The panel also shows a summary of the degree of variation in school expenditure across richer and poorer districts within each state and CZ, measured as the difference in per-pupil expenditure between the richest and the poorer district in each jurisdiction. While this difference is small on average, there appears to be a considerable degree of variation in

[8] U.S. Census Bureau, Annual Survey of State and Local Government Finances and Census of Governments (years 1980, 1990, 2000 and 2010).

disparity across CZs and states: the within-CZ richest-poorest difference ranges from -$5,435 for the 1st percentile to $8,265 for the 99th percentile in 1980, from -$11,029 to $8,006 in 1990, from -$15,386 to $8,451 in 2000, and from -$13,380 to $9,827 in 2010.

4.2 School Finance Reforms

I compile a list of school finance reforms, defined as court-mandated or legislated changes to the funding scheme of each state between 1986 and 2004, combining information from "Public School Finance Programs of United States and Canada" (1990-1991[9] and 1998–1999[10]), and from Verstegen and Jordan (2009). These publications provide a description of school funding schemes in place in each state at different points in time, and include details of the timing and content of each school finance reform. I complement these two sources with information from Manwaring and Sheffrin (1997), Hoxby (2001), as well as Jackson et al. (2015) and Lafortune et al. (2016). In general, information is consistent across the different sources; when discrepancies are found, priority is given to the "Public School Finance Programs of United States and Canada," and to Lafortune et al. (2016) for the latest reforms. A list and description of the reforms is contained in Appendix D, and the timing of such reforms is summarized in Figures A1 and A2.

4.3 Income

Information on household income at the school district level is taken from the Census of Population and Housing (1980, 1990, and 2000) and from the American Community Survey (2010).[11] I match average household income in each school district with per-pupil school expenditures to compute measures of equalization in expenditure across districts in each CZ. The final sample includes 12,132 districts in 1980, 13,055 in 1990, 12,347 in 2000, and 12,909 in 2010.

[9] 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.

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

[11] Income tabulations at the school district level are contained in the Census STF3F file for 1980, and published as part of the School District Demographic System for the years 1990 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.

4.4 Intergenerational Mobility

To measure intergenerational mobility I adopt the "absolute mobility" metric described in Chetty et al. (2014). This metric is defined as the expected income rank of children in the national distribution of children's income, given their parents' rank in the national distribution of parents' income.[12] 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 to separately analyze mobility outcomes of children in different parts of the income distribution.[13]

For each of 722 CZ and 7 cohort born between 1980 and 1986, Chetty et al. (2014) and Chetty et al. (2014) provide estimates of the intercept and slope of the linear relationship between parents' and children's income ranks, where the ranks are measured on the respective national distributions. In addition, they provide summary statistics of the income distributions of parents within each CZ.[14] I combine this information to construct a dataset that contains the expected national income centile of children by national income centile of the parent, for each CZ and cohort. Appendix B contains details on the construction of this dataset.

Summary statistics of mobility measures are shown in Panel B of Table 1. On average, children in families below the median experience upward mobility, whereas children in families above the median experience downward mobility. Wide differences in mobility exist across the country. The expected income rank of children born in 1980-86 with parents on the 25th percentile, for example, is as low as 32 in Gordon, SD, and as high as 61 in Sioux Center, IA. The expected income rank of children born in 1980-86 with parents on the 75th percentile is as low as 51 in Gallup, AZ, and as high as 70 in Hiawatha, KS. Mobility appears to increase, albeit slowly, over cohorts (Table 1, Panel B).

[12] Intergenerational mobility measures are available at www.equality-of-opportunity.org.

[13] 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. Absolute mobility allows to separately study these two cases (Chetty et al., 2014).

[14] This data is available as part of the "Equality of Opportunity Project", at www.equality-of-opportunity.org. Slope and intercepts of the linear relationship between parents' and children's ranks are reported in Online Table 1 of Chetty et al. (2014); summary statistics of CZ-level income distributions are reported in Online Table 7 of Chetty et al. (2014).

4.5 Cross-County Migration and House Prices

To study household sorting following a school finance reform I use data on county-level migration flows and income of migrants from the IRS Statistics of Income (SOI) for the years 1991 to 2010. I calculate county-level individual migration rates for each year as the ratio between the total number of migrants (both in and out of the county) and the total population. To study changes in home values I use annual house price indexes at the 5-digit zip code level from the Federal Housing Finance Agency (FHFA), constructed using a repeat-sale methodology with data from FHFA's "all-transactions" sample (Bogin et al., 2016).[15]

4.6 Other School District Data

Additional information used in the empirical analysis includes the number of teachers employed in each district and year, taken from the NCES Local Education Agency Survey Data and available for the years 1988 to 2010, and the high school completion rate, defined as the share of students who complete high school, taken from the NCES Local Education Agency Universe Survey Dropout and Completion Data and available for the years 1998 to 2010.

5 Measuring Inequality in School Expenditure

I start my empirical analysis by estimating a measure of inequality in per-pupil expenditure across districts within each CZ. In line with the theoretical framework, I use the parameter β in equation 3 as a sufficient statistic.[16] I estimate β separately for each CZ and year, using the following model:

$$e_{dct} = \alpha_{ct} + \beta_{ct} x_{dct} + \varepsilon_{dct} \tag{7}$$

where e_{dct} is the natural logarithm of time-t per-pupil expenditure in district d located in CZ c, x_{dtc} is the natural logarithm of median per-capita household income in the same district, and ε_{dct} is an error term.

The parameter β_{ct} is the elasticity of per-pupil expenditure to per-capita income, which summarizes the nature of the school financing scheme in each CZ at a single point in time. Higher positive values of β_{ct} denote higher per-pupil expenditure in richer districts (and vicev-

[15] Data available at https://www.fhfa.gov/DataTools/Downloads/Pages/House-Price-Index-Datasets.aspx.
[16] A similar approach has been used by Hoxby (1998); Card and Payne (2002); Lafortune et al. (2016).

ersa), and therefore a more unequal (or regressive) financing scheme. Negative values of β_{ct}, on the other hand, denote higher per-pupil expenditure in lower-income districts, and thus a redistributive financing scheme. Lastly, values of β_{ct} close to zero characterize an equalized financing scheme, with similar levels of expenditure across richer and poorer districts. Appendix Figure A3 shows the linear relationship captured by the parameter β for two CZs in 1990 and 2000: Detroit, MI, which experienced a school finance equalization reform in 1994, and Columbus, OH, which did not experience any reform between 1990 and 2000. Changes in the parameter β summarize changes in the slope of these relationships over time, both across and within commuting zones, and represent the variation I use to study the effects of school finance equalization on intergenerational income mobility.

I assign a β to each cohort in each CZ by taking the average over the calendar years in which each cohort was in grades K-12. Intergenerational mobility measures are available for cohorts 1980 to 1986, in school during years 1986 to 2004. This implies that the β parameters must be estimated separately for each of these calendar years and for each CZ. Data on district averages of per-capita income, however, are only available for Census years. To overcome this issue I exploit the timing of school finance equalization reforms in each state, and I back out one β for each CZ and schooling year as follows. First, I estimate one β for each CZ for Census years 1980, 1990, 2000, and 2010. Second, I impute β to years between two Censuses, depending on whether the CZ experienced a school finance reform during that decade or not. If a reform took place, I impute the β of the Census year at the start of the decade to the years preceding the reform (including the year of the reform), and the β 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 values of β of the Census years at the start and at the end of the decade.[17] Once I have obtained a β_{ct} for each CZ and year, I assign one parameter to each cohort of children in each CZ by averaging out the β_{ct} across the years in which each cohort was enrolled in K-12 education.[18]

On average, the parameter β is equal to 0.023 for CZs without a school finance reform in the years 1986 to 2004 (with a standard deviation of 0.268), to 0.019 for CZs with a reform

[17] If two reforms take place in one decade (as it is the case for Montana, New Jersey, New York, and Oregon), I assign the β of the Census year at the start of the decade to the years preceding the first reform, the β of the Census year at the end of the decade to the years following the last reform, and I interpolate between these two values of β for the years between the two reforms. A graphical representation of this assignment procedure is shown in Appendix Figure A4.

[18] For example, the β_c for the 1980 cohort is the average of the β_{ct}'s for the years 1986-1997.

in the years preceding the reform (with a standard deviation of 0.352), and to -0.048 (with a standard deviation of 0.301) in the years after the reform (Table 1, Panel C). Figure 1 shows the geographic distribution of the absolute change in β in each state, between the start and the end of the decade. Combined with Figure A1, the figure shows heterogeneity in the effectiveness of different school finance laws in equalizing resources.

I formally estimate the within-CZ change in β following a school finance reform with an event-study approach, estimating the following equation:

$$\hat{\beta}_{ct} = \sum_{s=-7}^{7} \delta_s 1(t - ryear_c)_t + \kappa_c + \varepsilon_{ct} \tag{8}$$

where $\hat{\beta}_{ct}$ is the estimated β coefficient for CZ c and year t, R_c equals 1 if CZ c experienced any school finance reform between 1986 and 2004, $ryear_c$ is the year of the first of such reforms, and κ_c are CZ fixed effects.[19]

Figure 2 (black line) shows point estimates and 90 percent confidence intervals of the coefficients δ_s. Estimates of β decline immediately following a school finance reform; they do, however, steadily revert to their pre-reform levels over time. In the next sections I discuss how this phenomenon can be caused by endogenous sorting of households following each reform, and show how simulated instruments can address this endogeneity issue.

6 Endogeneity of Post-Reform School Expenditure and Simulated Instruments

To test the theoretical predictions derived in Section 3, and to empirically investigate the effects of school finance equalization on intergenerational mobility, one must estimate the following model:

$$M_{cb} = \delta^x \hat{\beta_{cb}} + \kappa_c + \theta_x + \tau_b + \omega_{cb} \tag{9}$$

where M_{cxb} is the average national income centile of children with parents on the x centile of the national parents' income distribution, born in cohort b in CZ c, and $\hat{\beta_{cb}}$ is the estimated, cohort-specific measure of school finance equalization described in the previous section. CZ

[19] The estimation includes years 1986 to 2006: observations are weighted by the number of children in each CZ, and standard errors are clustered at the CZ level.

fixed effects κ_c control for CZ-specific, time-invariant determinants of mobility, and cohort fixed effects τ_b control for time trends in intergenerational mobility. The vector θ_x controls for parents' centile, and the variable ω_{jcg} is an error term. The parameter of interest is δ^x, which captures the effect of within-CZ changes in school finance equalization across time (and thus cohort) on intergenerational mobility of children from parents on the x centile.

A study of the causal effects of school finance equalization (i.e. a reduction in β) on intergenerational mobility requires an exogenous source of variation in the distribution of school expenditure across richer and poorer districts. School finance equalization reforms have changed in the funding formulas used by states to allocate funds to individual districts, in turn affecting their revenue and expenditure. For this reason, these reforms have been widely used in the literature as an exogenous shifter of school expenditure in studies of various outcomes (see Jackson et al., 2015; Lafortune et al., 2016, for an example). The particular nature of school finance reforms, however, casts doubts on the assumption of exogeneity of the post-reform level and distribution of school expenditure for two main reasons.

First, school finance reforms almost always involve an increase in total state aid to school districts, which implies an increase in government expenditure; they are therefore more likely to be passed during good economic times.[20] The state of the economy can affect children's outcomes in many other ways. Using these reforms as shifters to identify causal effects of changes in the *level* of expenditure can therefore lead to biased estimates. In this paper I focus on reform-induced changes in the *distribution* of school expenditure across districts, which is a function of the pre-reform and post-reform funding formula type and parameters.

Second, school finance equalization reforms can induce households to endogenously sort across school districts, depending on their income and/or wealth and on their preferences for public schools, and this in turn affects the level and distribution of expenditure. By changing the funding formula, school finance reforms change the tax price of school expenditure, i.e. the level of revenues raised through local property taxes necessary for a district to increase expenditure by one dollar.[21] The tax price can be seen as the "price" of public schools to taxpayers. An increase in the tax price raises districts' revenues if the tax base does not change. The same increase, however, can induce some households to migrate to a different district

[20] Most reforms passed after 1990 were "adequacy" reforms, aimed at increasing overall expenditure across all school districts to a level deemed adequate (Jackson et al., 2015; Lafortune et al., 2016).

[21] 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).

based on their preferences and their budget constraint (and thus their income), in line with a Tiebout model. These movements have a direct effect on house values and, subsequently, on the tax base and on districts' revenues and expenditures, and give rise to an endogeneity problem in any study of the effects of expenditure on districts' economic outcomes.

Most existing studies have given little to no importance to this source of endogeneity.[22] Jackson et al. (2015) recognize that expenditure can in general be endogenous, and instrument it using the timing of the passage of each school finance reform, the initial position of each district in the state distribution of per-pupil expenditure, and the macro-type of reform implemented (e.g. foundation plan, or equalized effort). This strategy, however, implicitly assumes that each type of reform has the same effect on expenditure of poorer and richer districts, and it fails to account for the fact that it is the specific formula type and parameters that determine households' response to the policy change and, in turn, revenue and expenditure for each district. As explained in detail by Hoxby (2001), reforms similar in timing and type can have different effects on the level and distribution of expenditure across districts, and these effects are impossible to predict without taking into account how the different formulas interact with preferences of high- and low-income households.

6.1 School finance equalization, migration, and house prices

In order to gauge the importance of household sorting in response to a school finance reform, I perform event studies on county-level migration flows and zip code-level house prices around a school finance reform year. I estimate models of the form:

$$y_{it} = \sum_{s \neq 0} \delta_s 1(t - Ryear_i = s) + \gamma_i + \tau_t + \varepsilon_{it} \tag{10}$$

where y_{it} is either total migration rates of county i in year t, the absolute difference between the income of migrants and the income of incumbents for county i in year t, or the absolute change in house prices in zip code i and year t. The indicator variable $1(t - Ryear_i = s)$ equals one if s years have elapsed between year t and the year $Ryear_i$ in which a school finance reform was passed in county (or zip code) i; γ_i and τ_t are county (or zip code) and year fixed effects, respectively. Estimates of the coefficients δ_s capture the change in the dependent variable by

[22] Card and Payne (2002) do not explicitly address this source of endogeneity. Lafortune et al. (2016) 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 fail to reject the hypothesis of no changes in these variables.

time-to-reform, relative to the reform year.

The top panel of Figure 3 shows OLS estimates and 90 percent confidence intervals of the coefficients δ_s on total migration rates, defined as the number of individuals migrating in and out of the country, divided by the county's population. These estimates are indistinguishable from zero in the years preceding a reform, and they increase in its aftermath, reaching 0.5 percentage points after 4 years. Compared with an average migration rate of 11 percent preceding a reform, this corresponds to a 5 percent increase in county-level migration following a school finance reform. It should be noted that, in most US states, counties are much larger than school districts: as a result, these county-level estimates of migration flows following a reform should be taken as a lower bound of the true estimates, because they do not capture cross-district, within-county movements.

Such an increase in migration rates, however, causes endogeneity in post-reform expenditure only if it is associated with *sorting* of households based on income and/or wealth, which affects home values and, in turn, the property tax base. To study the evolution of house prices around a school finance reform, I estimate equation 10 using the absolute yearly change in house prices indexes at the 5-digit zip code level as the dependent variable. Estimates of δ_s, in Figure 4, show no significant change in house prices in the 4 years preceding a reform, and a significant increase following a reform, which reaches 5 percentage points after 6 years. Together with the evidence on migration rates, results from this test suggest that household respond to this change by sorting across counties, and that this sorting affects house prices. As a consequence, failing to account for this source of endogeneity in post-reform expenditure can lead to substantial bias in the estimates of the effects of equalization on children's economic outcomes.

6.2 Constructing the Simulated Instrument

To account for this source of bias, I use a simulated instruments approach (Currie and Gruber, 1996; Gruber and Saez, 2002). The rationale behind it lies in isolating the exogenous source of variation in the distribution in post-reform expenditure (i.e. in the estimate of β), coming from the timing of the reform and from the type of post-reform funding formula, from the variation coming from endogenous changes in property tax bases and revenues caused by household sorting. To do so, I calculate the revenues that each district would have had in the absence of sorting after each school finance reform, and I use them to estimate a simulated

version of β, denoted by β^s. I then estimate the effect of changes in β on children's intergenerational income mobility with two-stages least squares, using β^s as an instrument for β.

I implement this procedure as follows. I start by constructing the funding formulas in place in each school district and year, which 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 of parameters set by state laws. I then use these formulas to simulate each district's post-reform revenues, holding endogenous characteristics such as property values and income fixed at their pre-reform values.[23] To construct each formula I use 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 more details on the specific formulas). Lastly, I compute a simulated version of the parameter β for each CZ and cohort, denoted by β^s, and obtained estimating equation 7 using simulated revenues in place of actual expenditure.

This approach is rather demanding in terms of data, because it requires information on all the variables entering each state's formula, which vary from state to state. To the best of my knowledge, this information is not available in a unified format for all US districts. I hence requested data on the variables of each formula directly from each state, and I successfully obtained it for a subsample of 20 states, enrolling enrolling approximately 63 percent of US children.[24]

The exclusion restriction of this instrumental variable approach relies on the exogeneity of the timing of the reform and of the type and parameters of the funding formula. This identification assumption could be threatened if, for example, formula types and parameters are correlated with state's socioeconomic and/or political characteristics. Hoxby (2001), however, explains that

> "[...] the [school finance] schemes are manifestations of the legal rhetoric of equal-

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

[24] 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 (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004). The remaining states do not maintain detailed records on historical school finance. 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 more in detail.

ization rather than economic logic about taxation and redistribution. In practice, schemes often have effects that are smaller or different than what was intended. As a result, equalization debates and court cases recur, and states seem to have abandoned trying to understand school finance in favor of trial-and-error methods of finding suitable schemes."

This suggests that specific formula types and parameters are adopted by states almost in a random way at each point in time.[25] Further from this, I leverage on the quasi-randomness of the formulas to estimate the causal effect of school finance equalization on intergenerational mobility.[26]

Figure 6 shows trends in simulated revenues (obtained using the first method) and actual expenditures in some of the largest states, separately for districts in the top and bottom quartile of the state's per-pupil expenditure distribution at the beginning of each sample. The figure shows that the extent to which actual revenues differ from simulated simulated revenues differs across states. In Texas, where school finance reforms were implemented in 1989 and 1991, simulated revenues understate actual revenues in both high-spending and low-spending districts; Wisconsin, on the other hand, simulated revenues are higher than actual revenues in both types of districts after a 1996 school finance reform, while in Michigan (which passed a reform in 1993) simulated revenues are higher than actual revenues for high-spending districts, but lower for low-spending districts. This, once more, highlights the importance of addressing the endogeneity problem explained above by directly exploiting each state's formula.

On average, the parameter β^s equals 0.054 (with a standard deviation of 0.285) in the years preceding each reform, and it drops to -0.078 (with a standard deviation of 0.340) in the years after the reform (Table 1, Panel C, and Figure 2, grey line). The parameters β and β^s are strongly correlated (Appendix Figure A6). Estimates from the first stage of the IV estimation reveal that β^s is a rather strong predictor for β; the F-statistics of the first stage, shown in Table 4, are all

[25] Figure A5 shows the correlation between estimates of β, and socioeconomic characteristics of each CZ, such as a measure of racial segregation, the share of single mothers, the share of individuals who identify themselves as religious, and the crime rate. These characteristics appear not correlated (or only weakly correlated) with β.

[26] In the case of simulated instruments, another important threat to identification is endogenous variables following time processes which vary across observations. When a tax reform is used to estimate the elasticity of labor supply to income, for example, this issue arises if income follows a mean-reverting process. Since the tax rate experienced by each individual (and, in turn, the size of the variation induced by the policy change) depends on his ex-ante income, individuals experiencing different changes in the tax rate have incomes which are inherently growing at different rates, which affects identification of the relevant elasticities. This issue is less compelling in the context of this paper for two reasons. First, property values and local tax rates, the main ingredients of the formulas, follow much more stable processes over time compared to income. Second, the parameters of each formula are chosen in a quasi-random way. For this reason they are not systematically related to ex-ante levels of property values, tax rates, or incomes.

above 10.

7 Effects of School Finance Equalization on Intergenerational Income Mobility

7.1 OLS estimates

I start by estimating equation 9 via OLS. For comparability, I present estimates on the subsample of states and years for which data required for the instrumental variable estimation is available. I standardize $\hat{\beta_{cb}}$ across all CZs and cohorts, and I use a bootstrap procedure to calculate CZ-clustered standard errors, to account for the fact that $\hat{\beta_{cb}}$ is a generated regressor (Freedman, 1984; Murphy and Topel, 2002). In what follows, I will interpret the opposite of each estimate of δ^x from equation 9 as the effect of a one-standard deviation reduction in β - or, in other words, of equalizing per-pupil expenditure across districts - on children's mobility.

OLS estimates of different versions of equation (9) are shown in Table 3. Column 1 restricts β to have the same effect across children of parent at different centiles of the national income distribution ($\delta x = \delta \forall x$). An imprecise estimate of δ equal to 0.4143 suggest, if anything, a *negative* effect of school finance equalization on children's intergenerational income mobility across the board (Table 3, column 7, p-value equal to 0.27).

While school finance equalization reforms are typically intended to reduce differences in expenditure between high-income and low-income districts, they might have a (sometimes unintended) effect on the total amount of resources spent on education in the state. In other words, some reforms might "level up" total expenditure and some others might "level down", depending on the particular type of formula adopted (Hoxby, 2001). An increase in equalization might therefore be associated with an overall, state-level change in expenditure, which could have an additional effect on children's outcomes (as highlighted by equations 1 and 3 of the theoretical framework).

To parse out the effects of increases in overall expenditure from the effects of equalization, in column 2 of Table 3 I augment equation 9 to include interactions between state and cohort indicators. Controlling for cohort-specific state effects, which proxy for state-level increases or decreases in total expenditure over time, yields a negative and imprecise estimate of δ on intergenerational mobility of children across the board. Specifically, a one-standard deviation reduction in β is associated with a 0.09 centiles increase in children's mobility on average (Table

3, column 2, p-value equal to 0.68). The change of sign of the estimated coefficient indicates that controlling for changes in overall expenditure is important. I therefore include state-by-cohort fixed effects in all the remaining specifications.

The small estimate of δ in column 2 could mask important heterogeneity in the effects of school expenditure equalization across children of parents ranked differently in the national income distribution. To investigate this heterogeneity, column 3 of Table 3 estimates a specification which allows δ to vary linearly with parents' centile. At the baseline (i.e. for children of parents at the very bottom of the income distribution) a one-standard deviation decline in β is associated with a 1.75 higher centile. The estimate for the interaction between β and parents' centile is positive and significant. Its magnitude indicates that the positive correlation between a reduction in β is reduced by 0.035 centiles with an extra centile of parental income. For example, the same reduction in β is associated with a 1.4 centiles increase in mobility for children of parents on the 10th centile, a 0.875 centiles increase for children from the 25th percentile, and with a decline in mobility for children of parents with income above the national mean.[27]

In Figure 5 (black series), I relax the linearity assumption and I allow δ to vary depending on parents' income decile. A one-standard deviation reduction in β is associated with a 1.29 centiles increase in mobility for children with parents on the first decile, and with a 0.96 centiles increase for children with parents on the second decile. Equalization is instead negatively associated with mobility for children with parents above the income median. Interestingly, the positive mobility effects on low-income children almost mirror, in magnitude, the negative effects on high-income children.

Taken together, OLS results seem to suggest a positive effect on equalization (conditional on overall expenditure) on intergenerational mobility of children of parents below the income median, and a corresponding negative effect for children above the income median.[28]

7.2 Two-stages least squares estimates

In Table 4 I re-estimate the specifications of Table 3 using two-stages least squares, to account for endogeneity in post-reform expenditure caused by household sorting. Column 1 estimates a constant effect of β on mobility of children of parents at different centiles on the na-

[27] The effects of equalization change sign (from positive to negative) for children of parents on the income median: $\beta + \beta \times$ *parent centile* equals zero when *parent centile* equals 50.

[28] Table A1 shows OLS estimates on the full sample of CZs. Estimates are consistent in sign and precision, but smaller in magnitude.

tional income distribution, not accounting for overall changes in expenditure. While standard errors are rather large, the point estimates indicate that a one-standard deviation reduction in β leads to a large 25 centiles increase in mobility for all children. As explained above, however, it is important to control for overall changes in expenditure. To this purpose, in column 2 I controls for state-by-cohort fixed effects. This estimate indicates that a similar reduction in β leads to a considerably smaller, and perhaps more credible, 0.95 centiles increase in mobility for all children (Table 4, column 2, p-value equal to 0.50).

It is useful to explore the heterogeneity in the effect of equalization across children of parents on different parts of the income distribution. Column 3 of Table 4 shows 2SLS estimates of a specification which allows δ to vary linearly with parents' centile. At the baseline, a one-standard deviation decline in β leads to a 5.59 centiles higher mobility. The estimate for the interaction between β and parents' centile is positive and significant, and it indicates that the positive effect of a reduction in β is reduced by 0.093 centiles with an extra centile of parental income. As an illustration, the same reduction in β is associated with a 4.6 centiles increase in mobility for children of parents on the 10th centile, a 3.3 centiles increase for children from the 25th percentile, and with a decline in mobility for children of parents with income above the national mean.[29]

To assess the magnitudes of these results in income terms, I match each child's income centile with the corresponding income level on the national income distribution, and use this measure of income as the dependent variable in my estimates. Results are shown in Table A2. They indicate that a one-standard deviation reduction in β leads to a 17 percent increase in income for children of parents with income on the first centile (estimate of β + $\beta \times$ *parent centile*, Table A2, column 3, significant at 5 percent), and with a 10 percent increase in income for children of parents on the 25th centile (estimate of β + $\beta \times$ *parent centile* * 25, Table A2, column 3, significant at 10 percent).

In Figure 5 (grey series), I estimate δ separately for each parents' income decile. The patterns of the estimates resemble OLS: a reduction in β has positive effects on children of lower-income parents and negative effects on children of high-income parents. The magnitudes are, however, much larger, and especially so for children below the median. A one-standard deviation reduction in β leads to a 4.22 centiles increase in mobility for children with parents on the first decile (p-value equal to 0.11), and to a 3.49 centiles increase for children with parents on

[29] 2SLS estimates indicate that $\beta + \beta \times$ *parent centile* equals zero when *parent centile* equals 60.

the second decile (significant at 10 percent). The negative effects of equalization on mobility of children with parents above the income median is, however, smaller in magnitude than the positive effect on low-income children. For example, a one-standard deviation reduction in β leads to a 2.88 centiles decline in mobility for children of parents with income on the 9th centile (p-value equal to 0.21), and to a 3.44 centiles decline for children of parents on the 10th decile (p-value equal to 0.33).

Compared with OLS, two-stages least squares reveal a much a larger effect of school finance equalization on intergenerational mobility of children. This confirms that failing to account for household sorting following a school finance reform can lead to underestimate the effects of equalization on children's outcomes.

7.3 Heterogeneous Effects of Spending Distribution Across Grades

A sizable literature has shown that education investments made at earlier ages yield higher returns (see Cunha and Heckman, 1994, for a review). Further from this, the effects of school finance equalization could differ depending on whether it affects students during primary, middle, or high school. If this is the case, estimates of an average effect of a decline in β might confound stronger effects during elementary school with smaller or zero effects during later school years.

Figure 7 provides a first piece of evidence in favor of the hypothesis that longer exposure to equalized school financing systems improves intergenerational mobility of lower income children. The figure shows estimates and 90 percent confidence intervals of the difference between the average income rank of children exposed to school finance reforms and the rank of children in the same CZ who were not exposed, separately for children of parents by length of exposure, and controlling for state-by-cohort fixed effects to account for changes in overall expenditure. These estimates indicate, for example, that children from families on the 10th income percentile who were exposed to a reform for 1 to 4 years have a 4.8 centiles higher income rank compared with children in the same cohort who were not exposed; children exposed for 5 to 8 years have a 7.5 centiles higher rank; and children exposed for 9 to 12 years have a 11.7 centiles income rank (Figure 7, black continuous line). A similar pattern can be observed for children from families on the 25th income percentile (Figure 7, black dashed line), whereas no change in mobility is associated with exposure to school finance reforms for children from families on the 75th and 90th percentile (Figure 7, grey lines).

To formally test for heterogeneous effects of school finance equalization across school grades, I adopt a more flexible empirical specification which allows changes in β to have differential effects on mobility, depending on the school grades in which a given cohort experiences them. I estimate:

$$M_{cb}^{x} = \delta^{x,p}\hat{\beta}_{cb}^{p} + \delta^{x,m}\hat{\beta}_{cb}^{m} + \delta^{x,hs}\hat{\beta}_{cb}^{hs} + \kappa_c + \tau_b + \omega_{cb} \tag{11}$$

where $\hat{\beta}_{cb}^{p}$, $\hat{\beta}_{cb}^{m}$, and $\hat{\beta}_{cb}^{hs}$ are average β parameters for cohort b in CZ c during primary (grades 1-4), middle (grades 5-8) and high school (grades 9-12) respectively.

Two-stages least squares estimates of equation (11), shown in Table 5, indicate that school finance equalization has larger effects on mobility of all children when experienced during primary and high school, compared with middle school. Controlling for state-by-cohort fixed effects, a one-standard deviation reduction in β leads to a 0.92 centiles increase in mobility if experienced during primary school (estimate of β, *grades 1-4* equal to 0.9169, Table 5, column 2, p-value equal to 0.14), and to a 1.17 centiles increase if experienced during high school (estimate of β, *grades 9-12* equal to -1.1688, Table 5, column 2, p-value equal to 0.50). By comparison, the same reduction in β has a small and imprecise negative effect on mobility if experienced during middle school (estimate of β, *grades 5-8* equal to 0.1837, Table 5, column 1, p-value equal to 0.54).

For the sake of comparison, OLS estimates of equation (11) are shown in Table A3. They indicate that, while a reduction in β is negatively associated with mobility of all children (column 2), it has a positive correlation with mobility of lower-income children when experienced during primary school and high-school (but not middle school), which declines as parents' income centile grows (Table A3, column 3). Again, the magnitudes of the OLS estimates are much smaller than 2SLS.

As above, I test whether the grade-specific effects of school finance equalization are larger for lower-income children, by interacting the grade-specific β with parents' income centile. These results indicate that, at the baseline (i.e. for children of parents with income on the first centile), a one-standard deviation reduction in β leads to a 11.5 centiles increase in income if experienced during primary school (estimate of β, *grades 1-4* equal to -11.472, Table 5, column 3, significant at 10 percent), and to a large, but imprecisely estimated decline if experienced

during middle school or high school. The positive effect of a decline in β, *grades 1-4* is reduced by 0.21 centiles with any additional centile of parents' income (estimate of β, *grades 1-4* $\times$ parent centile, Table 5, column 3, significant at 10 percent).

Overall, these estimates indicate that the effects of equalization are largest if experienced during primary and high school. This finding is consistent with the empirical evidence on the importance of early childhood investments in education on long-run outcomes, but it also highlights an important role for investments in education in the years leading to the choice of whether to graduate from high school, and possibly to enroll in college, or whether to drop out. This finding is consistent with the existing evidence of a strong association between children's educational attainment and income mobility (Rothstein, 2016). In line with this story, in the next section I show that equalization of school expenditure affects children's educational attainment.

8 Mechanisms

The empirical evidence presented so far confirms the importance of equalization of school finance across richer and poorer districts for long-run outcomes of students. To shed light on the channels behind these effects, in this section I investigate the relationship between equalization in expenditure and equalization of school inputs, as proxied by the teacher-student ratio, and between equalization of expenditure and an intermediate educational outcome, such as the rate of high school completion and the probability of enrolling in college,

8.1 Equalization in Expenditure and Equalization in School Resources: Teacher-Student Ratio

Equalization of school expenditure promotes intergenerational mobility by "leveling the playing field" between children born in more and less affluent school districts, i.e. by reducing the disparities in the educational inputs experienced by these two groups. Teachers represent one of the most important input in the educational production function (Chetty et al., 2014). Having an adequate number of teachers for each student is fundamental for increasing educational outcomes (Krueger and Whitmore, 2001; Bloom and Unterman, 2013); yet, underfunded districts have resorted to a reduction in staff to face budget shortages.[30]

[30] From an analysis of the Center on Budget and Policy Priorities elaboration of Bureau of Labor Statistics data.

I test whether equalization of school expenditure leads to an increase in the number of teachers available to each student in lower-income districts compared with higher-income ones. I estimate the following equation:

$$y_{dt} = \delta_1 \hat{\beta}_{ct} \times q^1_{dt} + \delta_2 \hat{\beta}_{ct} \times q^2_{dt} + \delta_3 \hat{\beta}_{ct} \times q^3_{dt} + \delta_4 \hat{\beta}_{ct} \times q^4_{dt} + \gamma_d + \sigma_{st} + \varepsilon_{dt} \tag{12}$$

where y_{dt} is the teacher-student ratio of district d, located in CZ c and state s, in year t. The variable q^n_{dt} equals 1 for districts in the n-th quartile of the 1990 distribution of median income in each state, and the vectors γ_d and σ_{st} control for district and state-by-year fixed effects, respectively.

Table 6 shows OLS and 2SLS estimates of equation 12. OLS results indicate a small and positive effect of equalization on lower-income districts, and a small and negative effect on higher-income ones. Specifically, a one-standard deviaton reduction in β is associated with 0.002 extra teachers per student in districts in the bottom income quartile (or 3 percent compared with an average of 0.065 teachers per student), with no significant change for districts in the second and third quartiles, and with 0.005 fewer teachers per student in districts in the top income quartile (Table 6, column 2). 2SLS results indicate larger effects on low-income districts, and small and insignificant effects on higher-income districts. A similar reduction in β brings to 0.0126 additional teachers per student in districts in the bottom income quartile, or 19 percent, and has a negative, although insignificant, effect on districts in higher quartiles.

Taken together, these results suggest that equalizing expenditures across wealthier and poorer districts promotes intergenerational mobility by equalizing educational inputs across children with different socioeconomic backgrounds. Interestingly, while OLS show small positive effects on low-income districts and small negative effects on high-income ones, 2SLS reveals much larger effects on low-income districts, but no change in high-income ones. Endogeneity in school expenditure driven by household sorting appears to, once more, bias the results towards zero.

8.2 Equalization in Expenditure, High-School Completion Rates, and College Enrollment

High school dropout is still a relevant issue in US compulsory education, and it affects some categories of students in some geographic areas more than others. On average, in 2012

6.6 percent of students dropped out of school.[31] The percentage of dropouts, however, is higher for black (7.5) and hispanic students (12.7). In 2010, less than 2 percent of students dropped out of high school in the Palo Alto Unified School District (CA), but almost 15 percent dropped out in Chicago Public Schools (IL).

Large variation in dropout rates across districts can translate in substantial differences in adult-life opportunities between more and less disadvantaged children and in turn undermine intergenerational mobility. To assess the extent to which equalization in school expenditure is associated to a reduction in disparities in high school completion rates across districts, I estimate a version of equation 12 with completion rates at the district-year level as the dependent variable. The results of this exercise are shown in Table 7. OLS results indicate a small and positive effect of equalization on completion rates in lower-income districts, and a small and negative effect on higher-income ones. Specifically, a one-standard deviaton reduction in β is associated with a 0.54 percentage points higher completion rates in districts in the bottom income quartile, with no significant change for districts in the second and third quartiles, and with a 0.16 percentage points lower completion rate in districts in the top income quartile (Table 6, column 2). 2SLS results are very imprecise, but are suggestive of larger positive effects of equalization on completion rates in low-income districts.

8.3 Education Mobility

Lastly, I study whether school finance equalization leads to an increase in the probability that children from lower-income families enroll in college. I use data on education mobility from the Equality of Opportunity Projeect, which includes the probability that children from families on different percentiles of the income distribution are enrolled in college by age 19. This data covers cohorts born between 1984 and 1993.

I re-estimate equation (9) using the probability of college enrollment by parents' income percentile (expressed in percentage points) as the dependent variable.[32] OLS results indicate that a one-standard deviation reduction in β is associated with a 0.09 percentage points higher probability of college enrollment for all children (Table 8, column 1, p-value equal to 0.13). Allowing the effect of β to vary by parents' income centile reveals that, while a one-standard deviation decline in β is positively associated with a 0.53 percentage points higher probability

[31] 2013 Digest of Education Statistics, National Center for Education Statistics.
[32] College enrollment is measured at age 19 (Chetty et al., 2014).

of college enrollment at the baseline, this effect declines with parental income (estimate of $\beta\times$ *parent centile* equal to 0.0093, Table 8, column 2, significant at 1 percent). For example, the same reduction in β is associated with a 0.433 percentage points higher probability of college enrollment for children of parents on the 10th centile, a 0.21 percentage points higher probability for children from the 25th centile. The same increase in β is instead associated with a lower probability for children of parents with income above the median.

2SLS reveal larger effects. A one-standard deviation reduction in β increases all children's probability of enrolling in college by 5.21 percentage points (Table 8, column 2, significant at 1 percent). Allowing the effect of β to vary by parents' income centile indicates that a one-standard deviation decline in β increases this probability by 6.89 percentage points at the baseline (i.e. for children of parents from the 1st centile), and that this effect declines with parental income. The same reduction in β increases college enrollment by 6.55 percentage points for children of parents on the 10th centile, by 6.04 percentiles for children from the 25th centile. The effect is positive, although smaller, also for children of parents above the median, and equal to 4.34 percentage points for children of parents on the 75th centile, and to 3.83 percentage points for children of parents on the 90th centile.

Together with the results on completion rates, these findings indicate that equalization of school expenditure improves long-run economic outcomes of children by improving their educational attainment.

9 Conclusion

Existing empirical evidence on the effects of increases in school expenditure due to school finance reforms has highlighted a positive impact on children's outcomes both in the short run (i.e. on achievement), and in the long run (i.e. on labor market outcomes). These reforms, however, change not only the level of resources, but also their distribution across poorer and richer districts in each state, and therefore across more and less disadvantaged students. If money spent on schools matters for students' outcomes, a more equal distribution can promote equality of opportunities across children from different socioeconomic backgrounds.

In an attempt to test this hypothesis, in this paper I have studied the effects of equalization in school expenditure across districts within local labor markets (CZs) on intergenerational income and education mobility, defined as the predicted economic and educational outcomes

of children born in families in different parts of the income distribution. I have summarized the degree of inequality in the school financing scheme each cohort of children is exposed to, with the correlation between per-pupil expenditure and per-capita income across districts in each US CZ, for each year in which the cohort was in school. I have then exploited changes in this measure of inequality across cohorts within each CZ, driven by school finance equalization reforms, on intergenerational mobility of children.

Apparently a useful source of exogenous variation in the level and distribution of school expenditure, school finance reforms should be used with caution. Households can respond to changes to the quality of schools and to local prooperty tax rates by "voting with their feet." These sorting patterns have an effect on house prices, which form the basis of redistribution in revenues and expenditure: as a result, post-reform expenditure is endogenous. A simple analysis of migration data shows an increase in cross-county movements following a reform. This suggests that the resulting endogeneity bias could be important.

In this paper I have addressed this issue using simulated instruments. Using each state's pre-reform and post-reform funding formula, I have simulated the revenues that each district would have received if the composition of the district and, in particular, house prices had remained to their pre-reform levels. This approach allows to separate the (exogenous) variation in the inequality in the distribution of school expenditure generated by changes in the funding formula, from the (endogenous) variation caused by households sorting.

Results from an instrumental variables estimation strategy indicate that equalizing school expenditure across districts improves intergenerational income mobility of low-income students, while it has a negative effect on high-income students. A one standard deviation reduction in inequality in school expenditure increases children's income rank by 4.6 centiles for children of parents on the 10th centile, and by 3.3 centiles for children of parents on the 25th centile. Importantly, instrumental variables estimates are considerably larger than OLS. This indicates that failing to account for endogeneity in post-reform expenditure can lead to underestimate the effects of equalization.

My findings indicate that school finance equalization can be beneficial not only for low-income, but also for high-income students. A possible explanation for this is that school finance reforms sometimes "level up" expenditure, and therefore increase the total size of education investments in a given state. Controlling for state-cohort fixed effects allows to account for changes in total expenditure, and leads to estimates of the effects of equalization which are

smaller in magnitude and less precise, but still positive, for income mobility of low-income students, but have the opposite sign on income mobility of high-income students.

To shed light on the mechanisms through which school finance equalization improves intergenerational mobility, I have studied its effects on school inputs, such as the number of teachers per student, and on intermediate outputs, such as the rate of high-school completion. I show that equalization of expenditure leads to equalization in both inputs and intermediate outputs across low- and high-income districts.

Overall, the findings of this paper indicate a positive impact of equalization in school expenditure on income and education mobility. These results are qualitatively in line with the existing quasi-experimental evidence on the effects of increased school expenditure on long-run outcomes of disadvantaged students (Jackson et al., 2015). They do, however, reveal that accounting for endogeneity in post-reform expenditure caused by household sorting can lead to underestimate the effects of equalization, and call for caution in the interpretation of the magnitudes of the causal effects of school expenditure on children's outcomes proposed by the previous literature. In addition, my findings provide one of the very first causal explanations for the large differences in mobility across US local geographic areas documented by (Chetty et al., 2014) and, as such, depict equalization in school expenditure as an important tool to promote equal opportunities for all children in the country.

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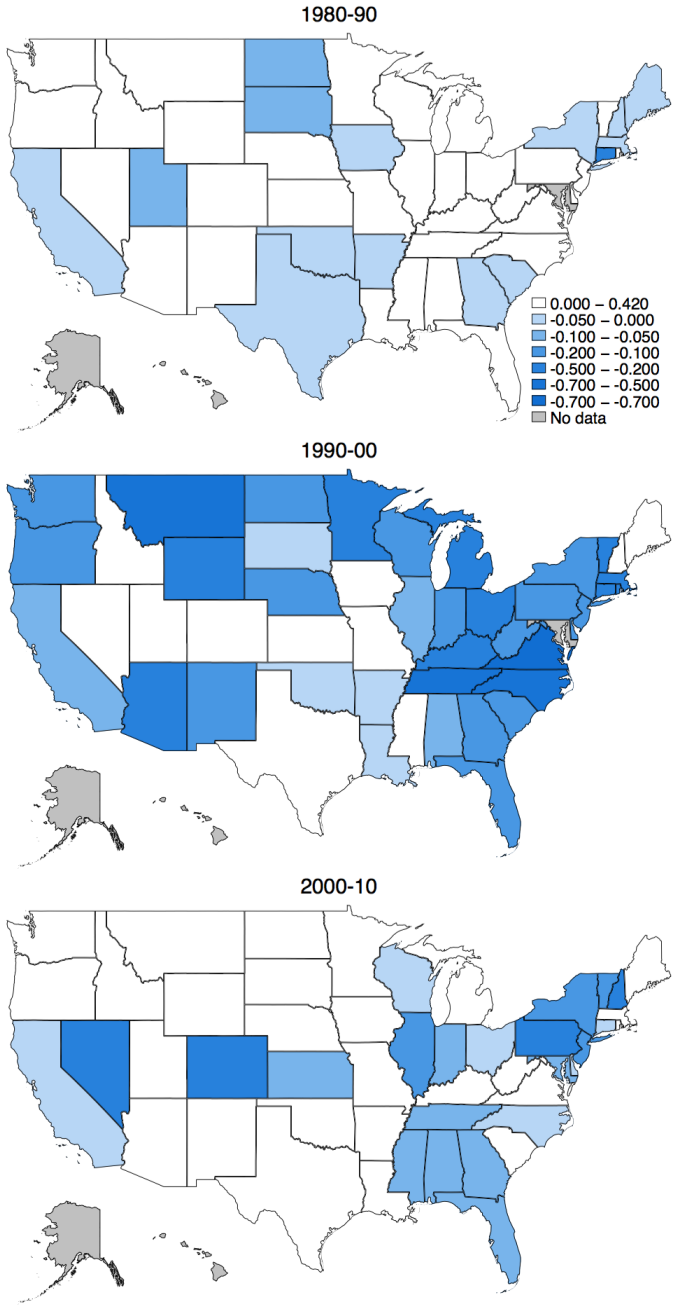
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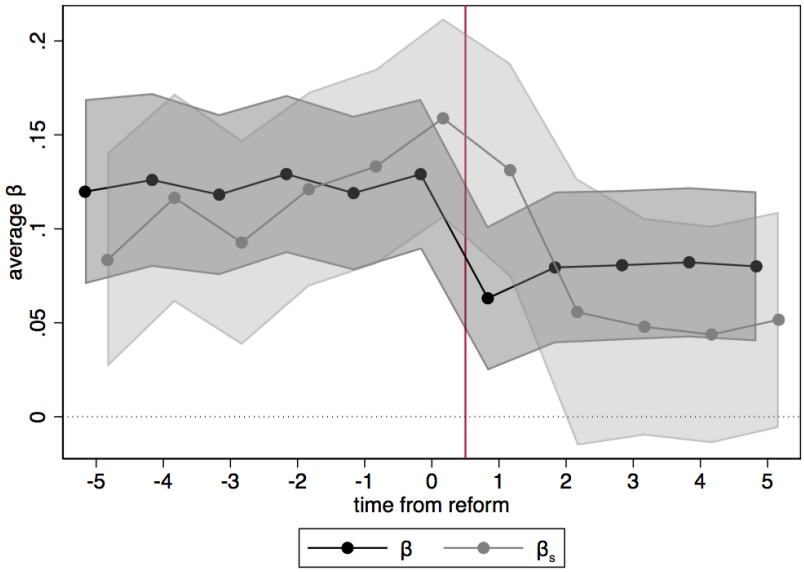
Figures

Figure 1: Equalization of School Expenditure: Changes in β Coefficient, 1980-2010



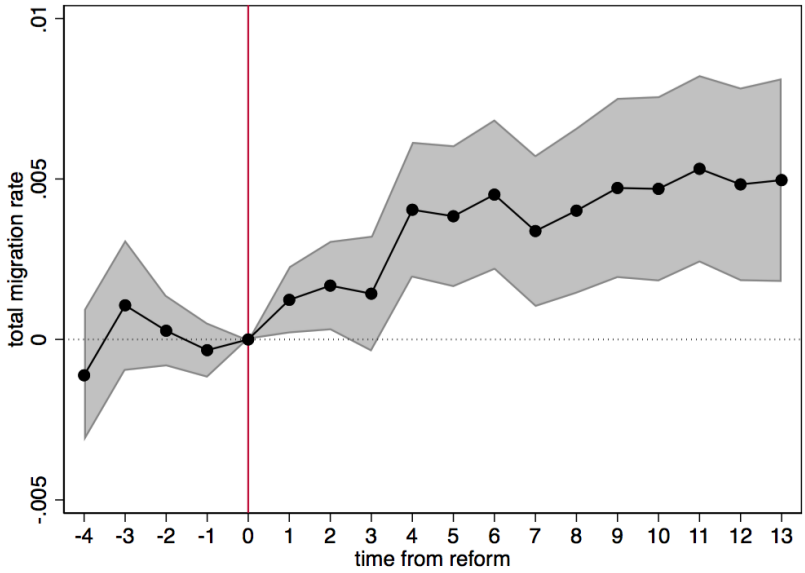
Note: Average change in the coefficient β from equation (7) during each decade. Averages are taken across CZs in each states, weighting each CZ by the number of children in cohorts born 1980-86.

Figure 2: Measures of Equalization Around Reform Years



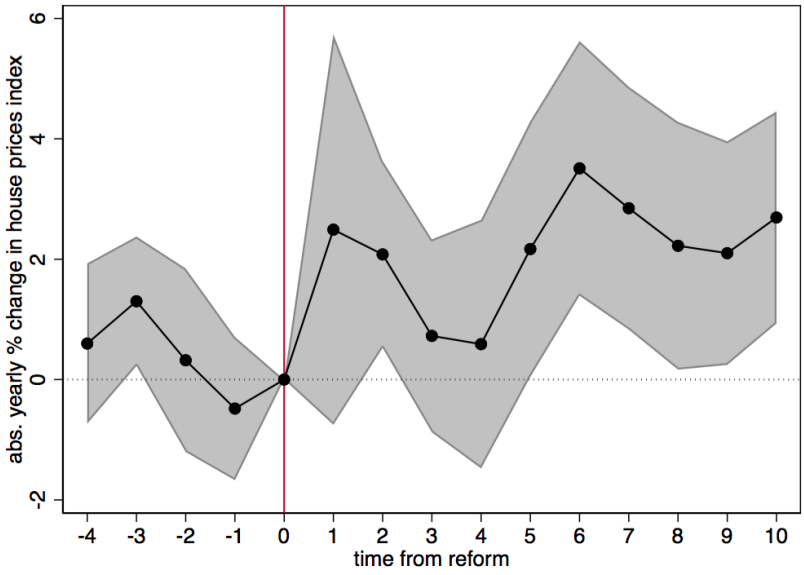
Note: Point estimates and 90 percent confidence intervals for the coefficients δ_s in regression $y_{ct} = \sum_s \delta_s R_c 1(t - ryear_c)_t + \kappa_c + \varepsilon_{ct}$, where y_{ct} is either the actual or the simulated β coefficient, defined in equation 7 and estimated separately for each CZ c and year t from 1986 to 2010, R_c equals 1 if CZ c had a reform between 1986 and 2004, $ryear_c$ is the year of the first of such reforms, and κ_c are CZ fixed effects. Observations are weighted by the number of children in each CZ, born in 1980-86. Standard errors are clustered at the CZ level.

Figure 3: School finance reforms and migration: Migration rates (top panel) and Difference between income of migrants and incumbents (bottom panel



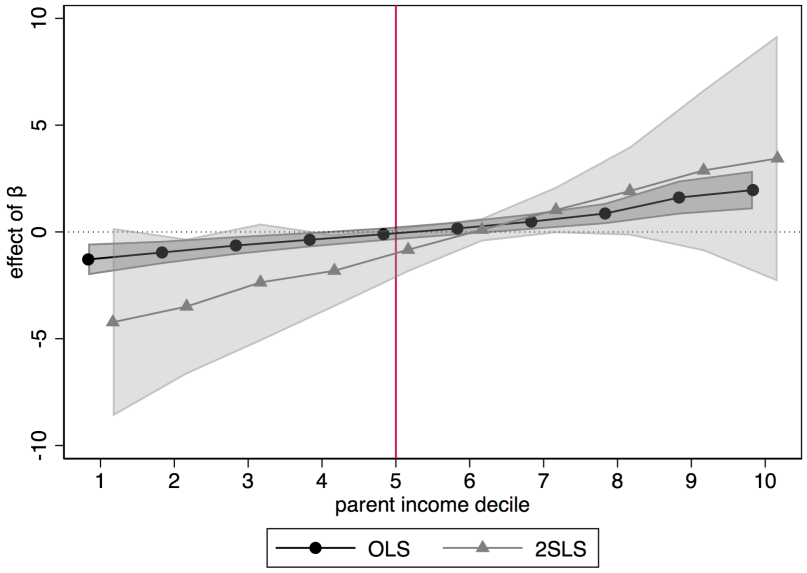
Note: Event-study of total migration rates in a 15-years window around each reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients δ_m in the regression $mig_{at} = \sum_{m=-7}^{7} \delta_m 1(t - y_a = m) * R_a + \alpha_a + \tau_t + \varepsilon_{at}$, where mig_{at} is the total migration rate of county a in time t (the ratio between the sum of in-migrants and out-migrants and the total population in each county), R_a is a dummy equal to 1 if county a experienced a reform between 1990 and 2011, y_a is the reform year of county a, α_a are county fixed effects, and τ_t are year fixed effects. Standard errors are clustered at the county level. County-year level observations are weighted by population. Data on migration are taken from the IRS Statistics of Income, and cover years from 1990-91 to 2010-2011.

Figure 4: School finance reforms and house prices



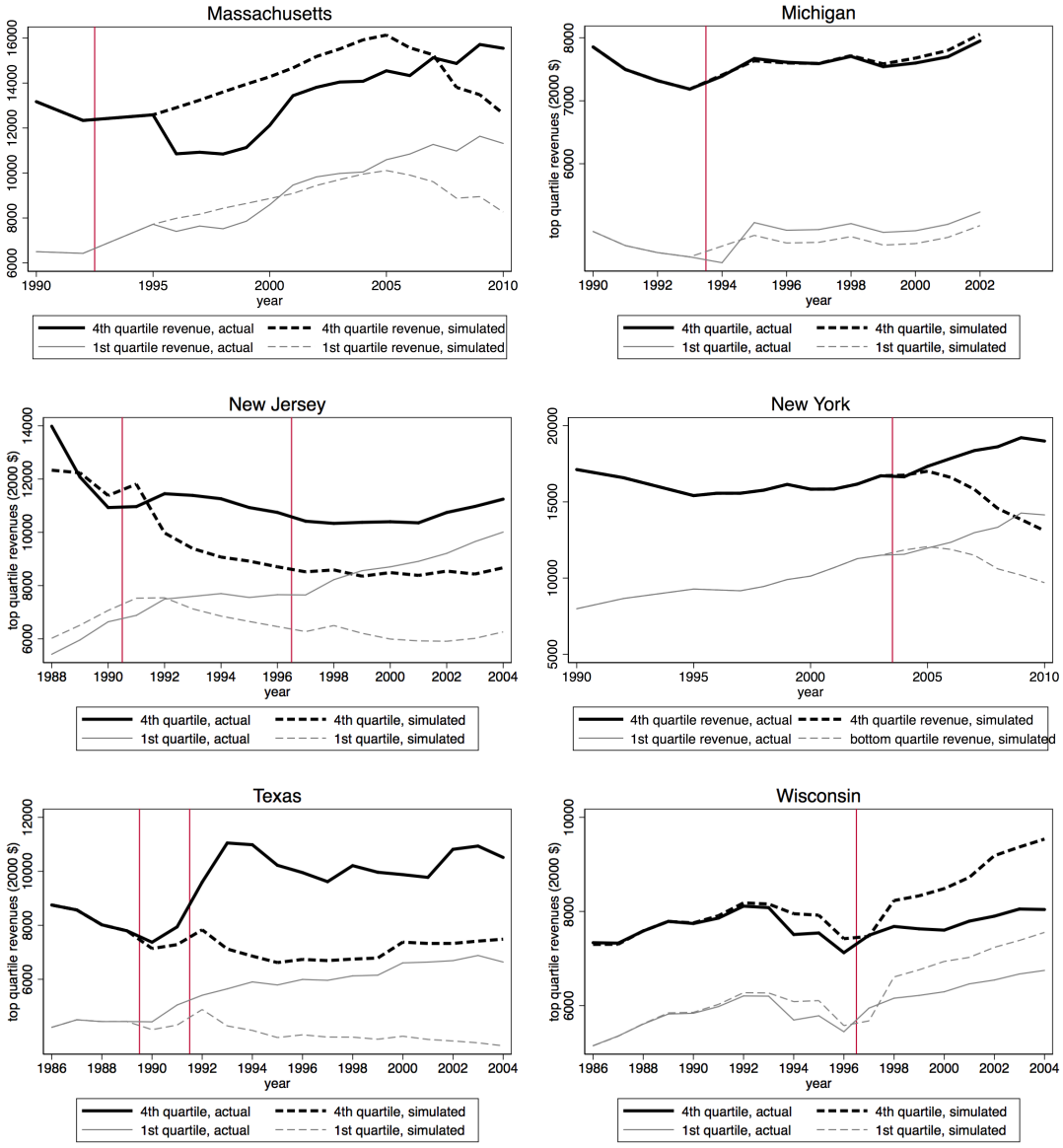
Note: Event-study of the change in house prices in a 15-years window around each reform. Each point and spike represent the estimate and the 90 percent confidence interval of the coefficients δ_m in the regression $hp_{zt} = \sum_{m=-7}^{7} \delta_m 1(t - y_z = m) * R_z + \zeta_z + \tau_t + \varepsilon_{at}$, where mig_{zt} is the absolute percentage change in house prices from year $t-1$ to year t in 5-digits zip code z, R_z is a dummy equal to 1 if zip code z experienced a reform between 1990 and 2011, y_z is the reform year of zip code z, α_z are zip code fixed effects, and τ_t are year fixed effects. Standard errors are clustered at the county level. Zip code-year level observations are weighted by population. House prices indexes are provided by the Federal Housing Finance Agency (FHFA), and are constructed using a repeat-sale methodology with data from FHFA's "all-transactions" sample for the years 1986 to 2004.

Figure 5: Effect of a decline in β, by parents' income centile



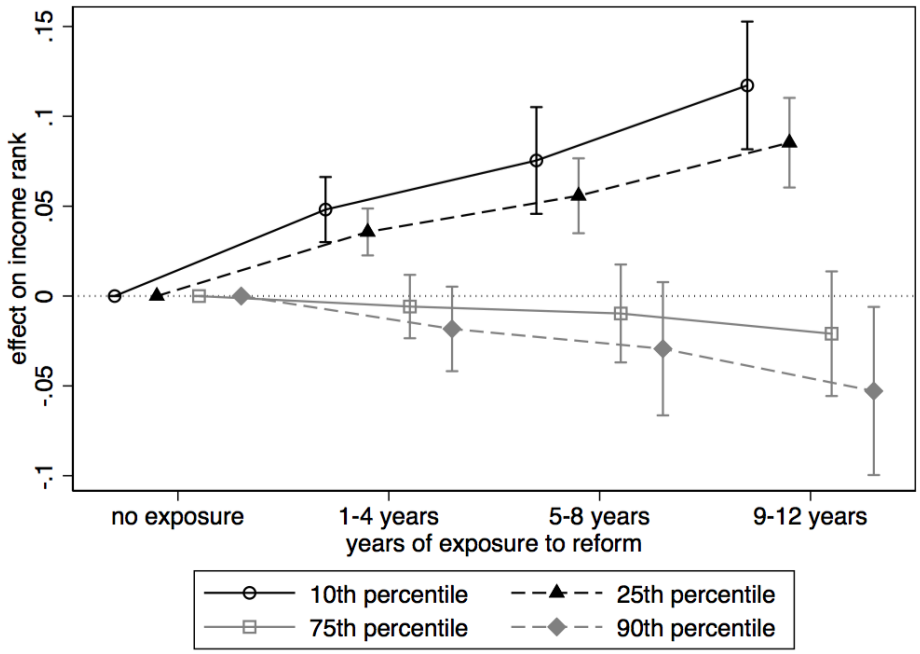
Note: OLS and 2SLS estimates and 90-percent confidence intervals for the coefficients δ^x in the regression $M_{cxb} = \sum_{d=1}^{10} \delta_d D_d \hat{\beta_{cb}} + \kappa_c + \theta_x + \sigma_{sb} + \omega_{cxb}$, where M_{cxb} is the average national income centile of children with parents on the x centile of the national parents' income distribution, born in cohort b in CZ c, $\hat{\beta_{cb}}$ is the estimated, cohort-specific measure of school finance equalization, D_d equals 1 if parents' income falls in decile d, κ_c are CZ fixed effects, and σ_{sb} are state-by-cohort fixed effects. Observations are weighted by the number of children in each CZ, in cohorts 1980-82. Standard errors in parentheses are clustered at the CZ level.

Figure 6: Simulated and actual expenditures by per-pupil expenditure (above and below state median), selected states



Note: Trends in simulated and actual per-pupil expenditure at the district level, for districts above and below the state median per-pupil 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, applied to pre-reform district characteristics. District-level observations are weighted by the number of students.

Figure 7: Exposure to a Reform and Changes in Intergenerational Income Mobility



Note: OLS estimates and 90 percent confidence intervals of the coefficients δ_{1-4}, δ_{5-8}, and δ_{9-12} in the regression $M_{cb} = \delta_{1-4}e_{cb}^{1-4} + \delta_{5-8}e_{cb}^{5-8} + \delta_{9-12}e_{cb}^{9-12} + \kappa_c + \sigma_{sb} + \varepsilon_{cb}$, where M_{cb}^{x} is the expected income rank of cohort b in CZ c born in families on the x centile, e_{cb}^{x} equals to 1 if cohort b in CZ c is exposed to x years to a school finance reform (where x equalz 0, 1-4, 5-8, or 9-12), and κ_c and σ_{cb} are, respectively, CZ and state-by-cohort fixed effects. Observations are weighted by the number of children in each CZ and cohort. Bootstrapped standard errors in parentheses are clustered at the CZ level.

Tables

Table 1: Summary Statistics

Panel A: Per-pupil Expenditure and Income

	1980	1990	2000	2010
Per-pupil expenditure ($)				
mean	4,589	6,350	7,418	9,595
s.d.	1,220	2,031	2,019	3,397
1st percentile	2,688	3,363	2,536	5,400
99th percentile	8,773	13,885	14,708	20,353
Δ expenditure, richest vs poorest district, within states ($)				
mean	19	747	803	1,163
s.d.	2,465	3,519	4,912	4,771
1st percentile	-3,758	-6,323	-11,475	-10,677
99th percentile	8,860	7,989	10,641	9,896
Δ expenditure, richest vs poorest district, within CZs ($)				
mean	288	525	178	-570
s.d.	2,479	3625	3,471	4,141
1st percentile	-5,435	-11,029	-15,386	-13,380
99th percentile	8,265	8,006	8,451	9,827

Panel B: Intergenerational Income Mobility Measures

cohort	10th	25th	75th	90th
1980-82	0.415	0.455	0.589	0.629
	(0.058)	(0.050)	(0.036)	(0.038)
1983-86	0.419	0.460	0.596	0.637
	(0.055)	(0.049)	(0.042)	(0.045)

Panel C: Measures of School Finance Equalization

	no reform	pre-reform	post-reform	diff. (post-pre)
β	0.020	0.019	-0.037	-0.057***
	(0.620)	(0.374)	(0.462)	(0.009)
β^s	-0.166	0.054	-0.078	-0.061***
	(0.891)	(0.285)	(0.340)	(0.054)

Note: Panel A: Summary statistics of per-pupil expenditure (measured in 2000 dollars), and difference in per-pupil expenditure 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, from www.equality-of-opportunity.org. Panel C: means and standard deviations of slope coefficient in equation (7), estimated separately for each CZ and year using actual expenditure (β) and simulated revenues (β^s). Observations are weighted by the number of children in each cell.

Table 2: 2SLS, First stage - Dependent variable is β

	(1)	(2)
β simulated	0.0446***	0.1192***
	(0.0015)	(0.0019)
Cohort FE	Yes	No
Parent centile FE	Yes	Yes
CZ FE	Yes	Yes
State * cohort FE	No	Yes
Fstat	904.0400	4101.8336

Note: The dependent variable is β, defined as the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable β^s is defined as the OLS estimate of the coefficient in equation (7), using simulated revenues instead of actual expenditure. All the specifications include CZ, parent centile, and cohort fixed effects; column 2 includes state-by-cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the CZ level. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

Table 3: OLS, Dependent variable is children's income centile

	(1)	(2)	(3)
β	0.4143	-0.0899	-1.7464***
	(0.3766)	(0.2225)	(0.5023)
$\beta\times$ parent centile			0.0350***
			(0.0097)
Cohort FE	Yes	No	No
Parent centile FE	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes
State * cohort FE	No	Yes	Yes
Observations	180279	180279	180279

Note: The dependent variable is the income centile (on the national children's income distribution) of children for each parents' income centile (on the national parents' income distribution), for each CZ and for cohorts 1980-1986. The variable β is the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable *parent centile* is the centile of parents on the national income distribution. All the specifications include CZ, parents' centile, and cohort fixed effects; columns 2 and 3 include state-by-cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the CZ level. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

Table 4: 2SLS, Dependent variable is children's income centile

	(1)	(2)	(3)
β	-25.1152	-0.9594	-5.5905*
	(19.0745)	(1.4118)	(3.0522)
$\beta\times$ parent centile			0.0926
			(0.0660)
Cohort FE	Yes	No	No
Parent centile FE	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes
State * cohort FE	No	Yes	Yes
Observations	180279	180279	180279

Note: The dependent variable is the income centile (on the national children's income distribution) of children for each parents' income centile (on the national parents' income distribution), for each CZ and for cohorts 1980-1986. The variable β is the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable *parent centile* is the centile of parents on the national income distribution. All the specifications include CZ, parents' centile, and cohort fixed effects; columns 2 and 3 include state-by-cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the CZ level. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

Table 5: 2SLS, Dependent variable is children's income centile

	(1)	(2)	(3)
β, grades 1-4	365.6443*	-0.9169	-11.4720*
	(207.9148)	(0.6209)	(6.1611)
β, grades 5-8	90.0246	0.1837	4.4824
	(64.9316)	(0.2952)	(5.2064)
β, grades 9-12	766.5924**	-1.1688	10.5682
	(383.2604)	(1.7513)	(8.9222)
β, grades 1-4 $\times$ parent centile			0.2088*
			(0.1243)
β, grades 5-8 $\times$ parent centile			-0.0800
			(0.1050)
β, grades 9-12 $\times$ parent centile			-0.2720
			(0.1917)
Cohort FE	Yes	No	No
Parent centile FE	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes
State * cohort FE	No	Yes	Yes
Observations	156222	156222	156222

Note: The dependent variable is the income centile (on the national children's income distribution) of children for each parents' income centile (on the national parents' income distribution), for each CZ and for for cohorts 1980-1986. The variable β is the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable *parent centile* is the centile of parents on the national income distribution. All the specifications include CZ, parents' centile, and cohort fixed effects; columns 2 and 3 include state-by-cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the CZ level. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

Table 6: OLS and 2SLS, Dependent variable is district-level share of teachers per student

	OLS		2SLS	
	(1)	(2)	(3)	(4)
$\beta\times$ q1	-0.0023**	-0.0021**	-0.0145**	-0.0126**
	(0.0010)	(0.0010)	(0.0071)	(0.0055)
$\beta\times$ q2	-0.0003	-0.0001	-0.0034	0.0002
	(0.0004)	(0.0004)	(0.0046)	(0.0039)
$\beta\times$ q3	0.0010	0.0012	0.0038	0.0080*
	(0.0012)	(0.0013)	(0.0046)	(0.0048)
$\beta\times$ q4	0.0048***	0.0055***	0.0050	0.0112
	(0.0016)	(0.0016)	(0.0087)	(0.0089)
Year FE	Yes	No	Yes	No
CZ FE	Yes	Yes	No	No
State * year FE	No	Yes	No	No
Observations	115917	115917	115917	115917

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 years 1986-2004. The variable β is defined as the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable q X 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^s as an instrument for β. All specifications include CZ and year fixed effects; columns 2 and 4 include state-by-year fixed effects. Bootstrapped standard errors are shown in parentheses. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004) .

Table 7: OLS and 2SLS, Dependent variable is rate of high school completion

	OLS		2SLS	
	(1)	(2)	(3)	(4)
$\beta\times$ q1	-0.0027	-0.0054***	-0.0420	-0.0661
	(0.0027)	(0.0016)	(0.8975)	(1.0050)
$\beta\times$ q2	0.0018	-0.0014	-0.0651	-0.1034
	(0.0028)	(0.0015)	(1.4877)	(4.4319)
$\beta\times$ q3	0.0050	0.0019	0.2036	0.2394
	(0.0033)	(0.0023)	(7.0484)	(9.0834)
$\beta\times$ q4	0.0190***	0.0157***	-0.3224	-0.4508
	(0.0043)	(0.0037)	(11.8295)	(20.8542)
Cohort FE	Yes	No	Yes	No
CZ FE	Yes	Yes	No	No
State * cohort FE	No	Yes	No	No
Observations	28386	28386	28386	28386

Note: The dependent variable is the probability of college enrollment by age 19 for each parents' income centile (on the national parents' income distribution), for each CZ and for cohorts 1980-1986. The variable β is the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable *parent centile* is the centile of parents on the national income distribution. All the specifications include CZ, parents' centile, and cohort fixed effects; columns 2 and 3 include state-by-cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the CZ level. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

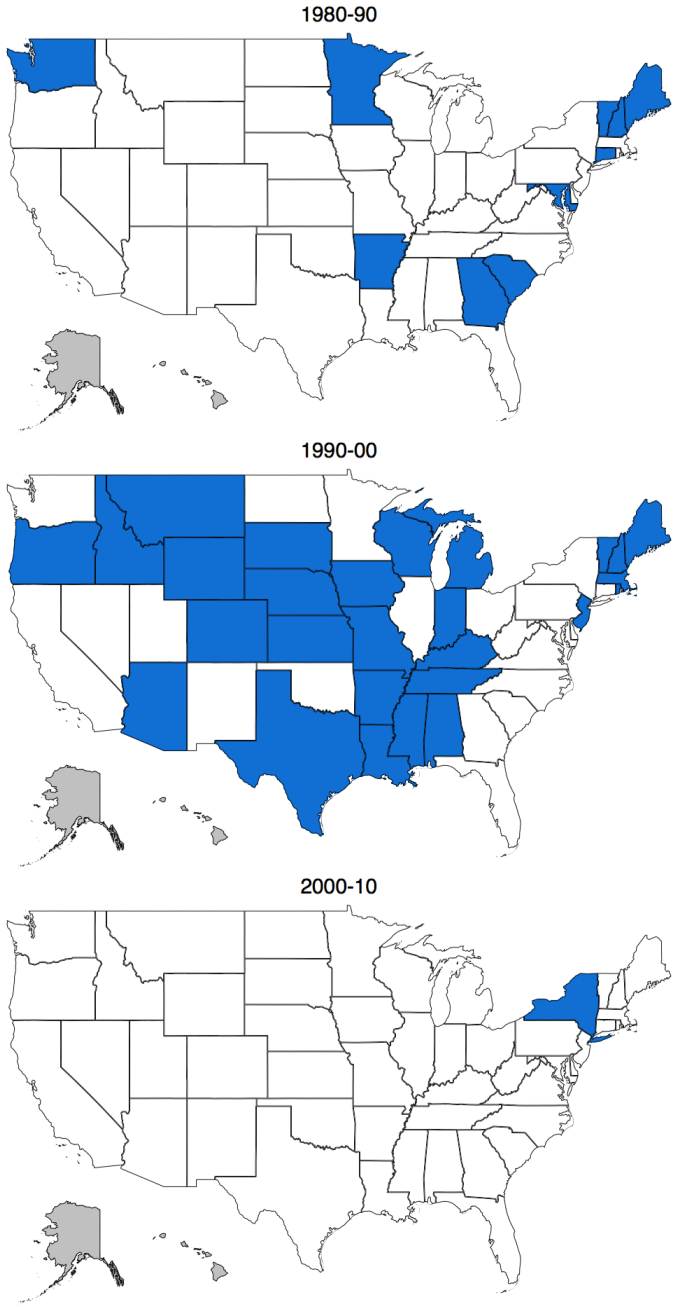
Table 8: OLS and 2SLS, Dependent variable is probability of college enrollment by age 19

	OLS		2SLS	
	(1)	(2)	(3)	(4)
β	-0.0958	-0.5260***	-5.2173***	-6.8876***
	(0.0617)	(0.0685)	(0.4297)	(0.4724)
$\beta\times$ parent centile		0.0093***		0.0340***
		(0.0007)		(0.0039)
Parent centile FE	Yes	Yes	No	No
CZ FE	No	No	Yes	Yes
State * cohort FE	Yes	Yes	Yes	Yes
Observations	253440	253440	253440	253440

Note: The dependent variable is expected probability that a child is enrolled in college by age 19, by parents' income percentile; observations are at the CZ-cohort level, for cohorts 1984-1993. The variable β is defined as the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The vairable *income in 1st quintile* equals 1 for districts with median household income in the bottom 25 percent of the national distribution in 1990. Columns 1-4 estimate OLS; columns 5-8 estimate 2SLS, with β^s as an instrument for β. All specifications include CZ and cohort fixed effects; columns 2 and 4 include state-by-cohort fixed effects. Bootstrapped standard errors are shown in parentheses. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

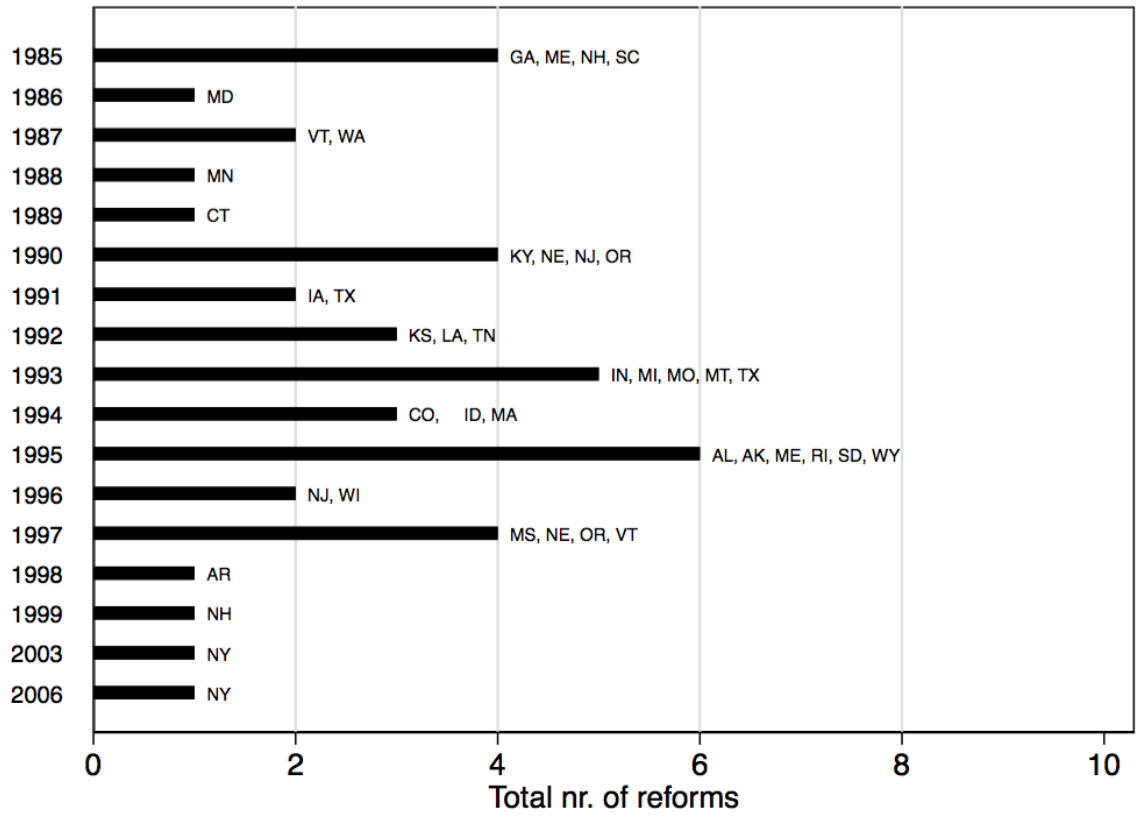
Appendix A Additional Figures and Tables

Figure A1: US States with School Finance Equalization Reforms, 1980-2010



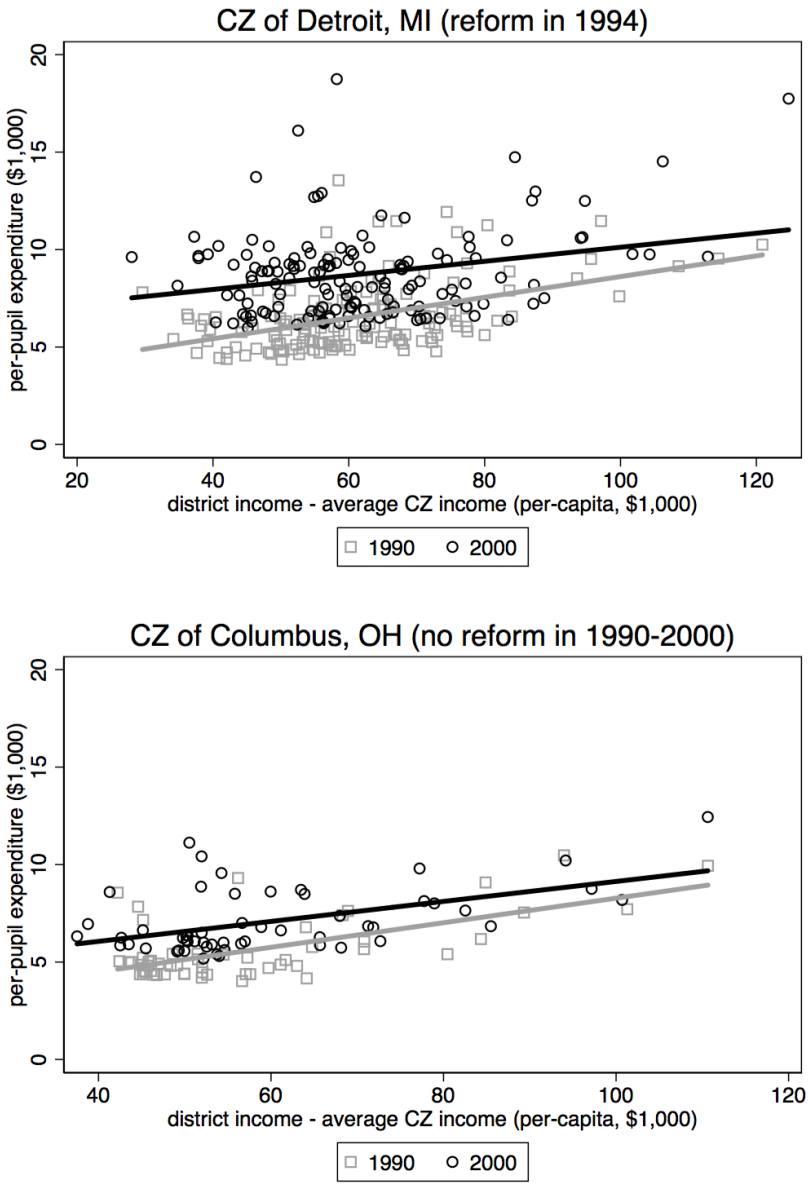
Note: Shaded areas denote states which passed a school finance equalization reform during each decade. Alaska passed a reform in 1987 (a revision of its school finance foundation program), Hawaii did not pass any reform in this time period. Source: "Public School Finance Programs of United States and Canada" (1990-1991 and 1998–1999), Verstegen and Jordan (2009), Jackson et al. (2015), and Lafortune et al. (2016).

Figure A2: School Finance Reforms Over Time



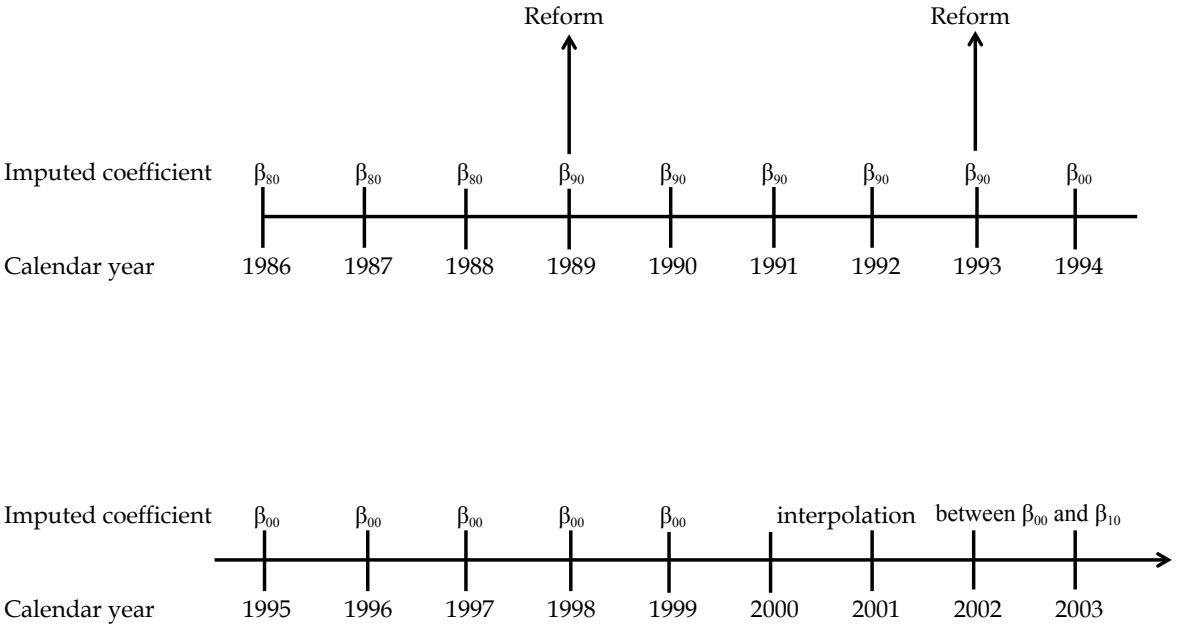
Note: Number of school finance reforms in each two-year interval.

Figure A3: Per-pupil Expenditure and Per-capita Income in Two Commuting Zones, 1990 and 2000



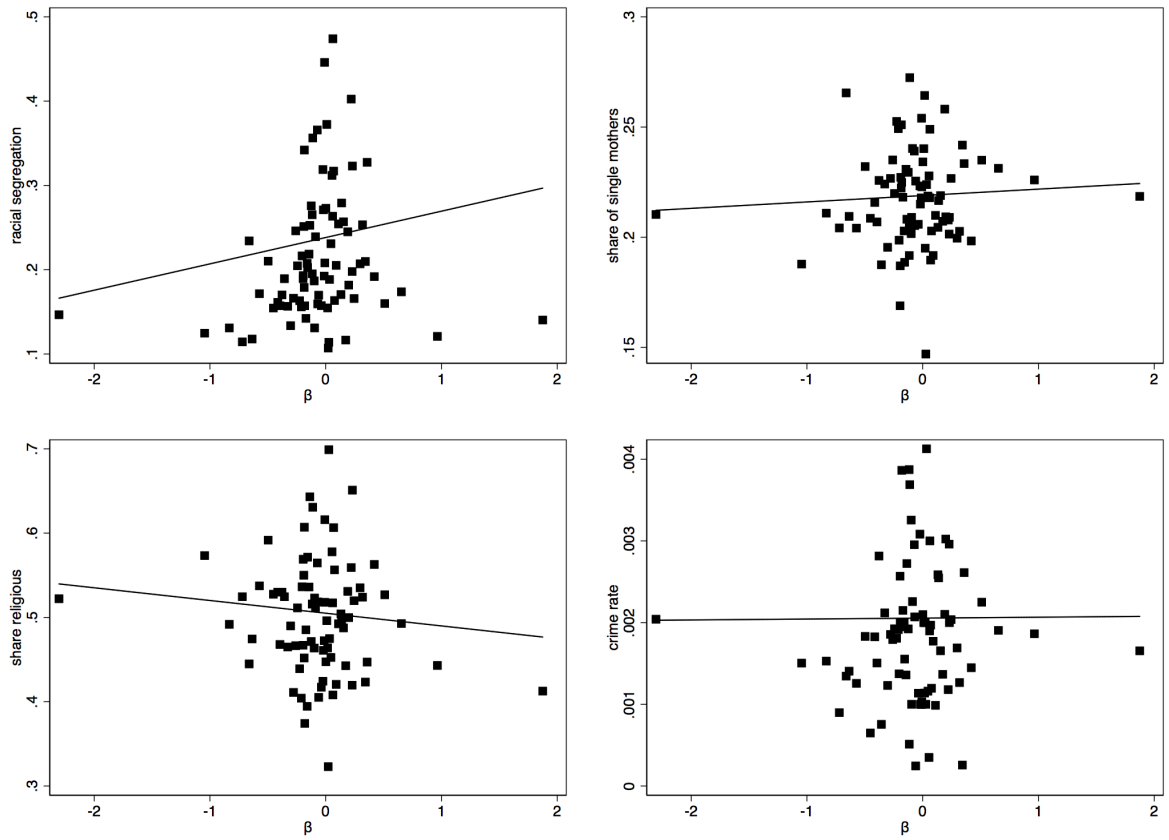
Note: Per-pupil expenditure (y-axis) and per-capita income (x-axis) in 1990 and 2000, in the CZs of Detroit, MI, which had a reform in 1994, and in Columbus, OH, which did not have a reform between 1990 and 2000. Expenditure and income are measured in 2000 thousand dollars.

Figure A4: Imputation of Equalization Measures



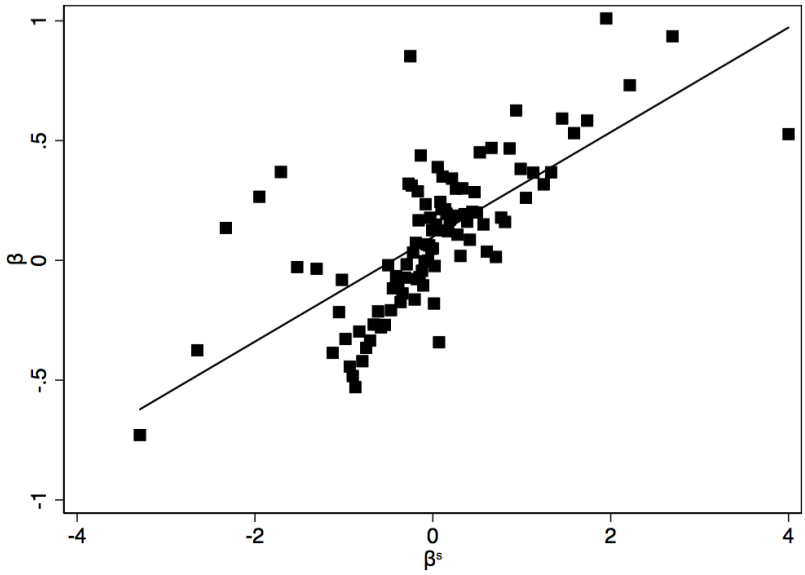
Note: The figure displays the procedure of imputation of measures of equality in expenditure across school districts to each calendar year. The example shown refers to Texas, which experienced school finance equalization reforms in 1989 (*Edgewood vs. Kirby*) and 1993 (Senate Bill 351).

Figure A5: Correlation between β and CZ-level characteristics



textitNote: Binned scatterplot of β measured in 2000 (vertical axis) and CZ characteristics. *Racial segregation* is defined as the multi-group Theil Index calculated at the census-tract level, using the 2000 Census over four groups: White alone, Black alone, Hispanic, and Other, and aggregated at the CZ level. The variable *Share of single mothers* is defined as the number of single female households with children divided by total number of households with children, in the 2000 Census. The variable *Share religious* is defined as the share of religious adherents in the 2000 Census. The *crime rate* is defined as the number of arrests for serious violent crimes per capita, and computed using data from the Uniform Crime Reports. The variable β is defined as the OLS estimate of the coefficient in equation (7), computed separately for each CZ and year. Observations are weighted by the total number of children in each CZ.

Figure A6: First stage: Correlation between β and β_s



textitNote: Binned scatterplot of β (vertical axis) and β_s (horizontal axis). Observations are at the level of the CZ and cohort, and are weighted by the total number of children.

Table A1: OLS, Dependent variable is children's income centile - Full sample

	(1)	(2)	(3)
β	0.3262	0.0154	-0.7511***
	(0.2389)	(0.1313)	(0.2857)
$\beta\times$ parent centile			0.0160***
			(0.0053)
Cohort FE	Yes	No	No
Parent centile FE	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes
State * cohort FE	No	Yes	Yes
Observations	373626	373626	373626

Note: The dependent variable is the income centile (on the national children's income distribution) of children for each parents' income centile (on the national parents' income distribution), for each CZ and for cohorts 1980-1986. The variable β is the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable *parent centile* is the centile of parents on the national income distribution. All the specifications include CZ, parents' centile, and cohort fixed effects; columns 2 and 3 include state-by-cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the CZ level.The sample covers 598 CZs with non-missing information on mobility measures and β.

Table A2: OLS, Dependent variable is children's log(income)

	OLS		2SLS	
	(1)	(2)	(3)	(4)
β	0.0113	-0.0508***	-0.0275	-0.1784**
	(0.0103)	(0.0144)	(0.0386)	(0.0875)
$\beta\times$ parent centile		0.0010***		0.0030
		(0.0003)		(0.0019)
Cohort FE	Yes	No	No	No
Parent centile FE	Yes	Yes	Yes	Yes
CZ FE	No	No	Yes	Yes
State * cohort FE	No	Yes	Yes	Yes
Observations	180279	180279	180279	180279

Note: The dependent variable is the natural logarithm of children's income for each parents' income centile (on the national parents' income distribution), for each CZ and for cohorts 1980-1986. The variable β is the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable *parent centile* is the centile of parents on the national income distribution. All the specifications include CZ, parents' centile, and cohort fixed effects; columns 2 and 3 include state-by-cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the CZ level. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

Table A3: OLS, Dependent variables are measures of intergenerational income mobility

	(1)	(2)	(3)
β, grades 1-4	0.1205	0.1056	-1.5003***
	(0.2798)	(0.1225)	(0.3889)
β, grades 5-8	0.3186	0.0580	0.2711
	(0.1967)	(0.0631)	(0.2249)
β, grades 9-12	0.3200	0.2624**	-0.6584*
	(0.2781)	(0.1261)	(0.3354)
β, grades 1-4 $\times$ parent centile			0.0320***
			(0.0075)
β, grades 5-8 $\times$ parent centile			-0.0040
			(0.0044)
β, grades 9-12 $\times$ parent centile			0.0185***
			(0.0069)
Cohort FE	Yes	No	No
Parent centile FE	Yes	Yes	Yes
CZ FE	Yes	Yes	Yes
State * cohort FE	No	Yes	Yes
Observations	171171	171171	171171

Note: The dependent variable is the income centile (on the national children's income distribution) of children for each parents' income centile (on the national parents' income distribution), for each CZ and for cohorts 1980-1986. The variable β is the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable *parent centile* is the centile of parents on the national income distribution. All the specifications include CZ, parents' centile, and cohort fixed effects; columns 2 and 3 include state-by-cohort fixed effects. Bootstrapped standard errors in parentheses are clustered at the CZ level. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

Table A4: OLS and 2SLS, Dependent variable is children's income centile - including CZs with less than 5 districts

	(1)	(2)	(3)	(4)
β	0.2091	-1.1351**	-33.2483*	-48.2622
	(0.3675)	(0.4866)	(17.5159)	(292.3631)
$\beta\times$ parent centile		0.0232**		-8.4996
		(0.0115)		(29.1111)
Cohort FE	Yes	No	No	No
Parent centile FE	Yes	Yes	No	No
CZ FE	Yes	Yes	No	No
State * cohort FE	No	Yes	Yes	Yes
Observations	222354	222354	222354	222354

Note: The dependent variable is the income centile (on the national children's income distribution) of children for each parents' income centile (on the national parents' income distribution), for each CZ and for cohorts 1980-1986. The variable β is the OLS estimate of the coefficient in equation (7), computed separately for each CZ and cohort, and standardized across all CZs and cohorts. The variable *parent centile* is the centile of parents on the national income distribution. All the specifications include CZ, parents' centile, and cohort fixed effects; columns 2 and 3 include state-by-cohort fixed effects. The variables β and β^s are calculated for all CZs and years (as opposed to only for those with at least 5 districts, as in the main specifications). Bootstrapped standard errors in parentheses are clustered at the CZ level. The sample is restricted to states with available detailed school finance information, which include California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004).

Appendix B Construction of the dataset

School expenditure

I use data on per-pupil expenditure at the school district level from the US Census Data Files on Historical Finances of Individual Governments. These files contain administrative data on revenues and expenditures of individual local governments (counties, municipalities, townships, school districts). Revenue data are classified by source, while expenditure data are reported by function. Districts classified as independent jurisdictions appear as separate entries in these data files. Information on revenues and expenditures of districts classified as dependent jurisdictions are instead included in the entries of the counties or municipalities they depend on. For all school districts, regardless of their nature, I use total expenditure on education as my main variable to compute per-pupil school expenditure. To obtain per-pupil expenditure, I divide total expenditure by the measure of enrollment reported in the annual census of school districts conducted by the National Center for Education Statistics (NCES Common Core of Data, or CCD, district finance files, also known as the"F-33" survey). The NCES data covers years from 1989-90 onwards; I impute the 1990 measure of enrollment to early years as well. I consider both independent and dependent school districts, and I exclude special entities such as vocational, technical, educational services districts. The final sample includes 13,464 , 14,395, 13,202, and 13,306 districts in 1980, 1990, 2000, and 2010 respectively.

Income

I use tabulations of household income at the school district level from the US Census of Population and Housing for the years 1980, 1990 and 2000, and from the American Community Survey 5-years estimates (2008-2012) for the year 2010. Income tabulations at the school district level are contained in the Census STF3F file for 1980, and published as part of the School District Demographic System for the years 1990, 2000, and 2010. Income data at the district level is however reported under the form of tabulations of the counts of households in 17, 25, 16 and 16 income bins in each school district in 1980, 1990, 2000 and 2010 respectively. To calculate median income from these tabulations, I assign to each class a level of income equal to $0.5(y_u^c - y_l^c)$, where y_u^c and y_l^c are, respectively, the upper and lower income bounds of class

$c \in [1, C]$.[33] I then calculate median income as the income of the class containing the median household. Information on income is available for 15,861, 15,037, 14,222, and 13,429 districts in 1980, 1990, 2000, and 2010 respectively.

Equalization measures

To estimate CZ-year specific β as in equation 7, I match per-pupil expenditure data with median district income data. I use the 1990 county-CZ crosswalk provided by Chetty et al. (2014) and Chetty et al. (2014) to assign school districts to CZs using information on each district's county provided by the NCES Common Core of Data. I successfully match 12,997, 13,879, 12,905, and 12,954 districts in 1980, 1990, 2000, and 2010 respectively. The main estimates reported in the paper refer to β estimates based on CZs containing at least 5 districts with non-missing income and expenditure information in each year. Appendix Table A4 shows estimates of the main specification using β estimated on all CZs. I assign CZ-year estimates to CZ-cohorts using the timing of each reform, with the procedure described in the paper.

Simulated instrument

To construct the simulated instrument, I proceed in three steps. First, I have collected data from each state's Department of Education on the variables entering the school funding formula in each available year between 1986 and 2004. These variables include, but are not limited to, assessments of property values, property tax rates, income, measures of enrollment (such as full-time-equivalents or average daily membership/attendance, weighted by type of students or unweighted). I successfully obtained this information for the following states (years): California (data available for the years 1996-2004), Colorado (1993-2004), Florida (1986-2004), Georgia (1988-2004), Kentucky (1991-2004), Illinois (1987-2004), Louisiana (1993-2004), Massachusetts (1995-2004), Michigan (1993-2004), Minnesota (1991-2004), Nebraska (1993-2004), New Jersey (1998-2004), New York (1986-2004), North Dakota (1986-2004), Ohio (1986-2004), Pennsylvania (1995-2004), Utah (1986-2004), Texas (1986-2004), and Wisconsin (1986-2004), and for a total of 7,687 school districts. Second, for each state I construct the funding formula preceding and following each school finance reform, which allows to calculate total revenue as a function of the various variables and of formula parameters set by each state. I then simulate

[33] For the top income class only a lower bound (y_l) is specified. I assume a Pareto distribution and calculate the midpoint as $y_u^{C-1} \times 2^{1/\alpha}$, where $\alpha = |\log(n_C + n_{C-1}) - \log(n_C)| / \log(y_l^C - y_l^{C-1})$, C denotes the top income class, $C-1$ denotes the class immediately below the top, and n_c is the counts of households in class c.

post-reform revenues keeping endogenous variables, such as property values and income, at their levels at the time of passage of each reform. I have adjusted property values using the US Annual Price Index (calculated by the Federal Housing Finance Authority using a repeated-sales method). Third, I estimate the simulated β using simulated revenues, matched with information on median income at the district level. For each CZ, I have estimated this parameter for the first available year of revenue data, for 1990, and for 2004. Since income information is only available for Census years, I impute income to the first year of data and to 2004 using a simple linear interpolation method. Once I estimate β for these three points in time, I assign CZ-year estimates to CZ-cohorts using the timing of each reform. I successfully estimate the simulated β for 272 CZs.

Intergenerational mobility

Chetty et al. (2014) (Online Table 1) calculate and report intergenerational mobility measures, separately for each cohort of children and for each of the 722 CZs in the US, using individual-level data from IRS tax records. These measures include the intercept and the slope of the linear relationship between parents' and children's income ranks (on the national income distribution of parents' income and children's income, respectively), separately for each CZ and for cohorts 1980-86.[34] Parents' income is calculated as the average yearly income in years 1996-2000, measured in 2010 dollars. Children's income is calculated as the average yearly income in years 2010, 2011 and 2012, measured in 2010 dollars.[35] Each child is matched to his or her parent (or parents), i.e. the taxpayer who claimed him or her as dependent when he or she was age 25 or younger in IRS tax records covering the period 1996-2011.[36] Matched parent-child pairs are assigned to a CZ based on the earliest non-missing ZIP code reported on the tax form of the parent. The sample is restricted to children of parents with non-missing ZIP codes and non-negative income. The final sample of children includes 47 million U.S. citizens born in the period 1980-1993.[37]

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

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

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

[37] Differently from Chetty et al. (2014) who base their analysis of income mobility on a "core sample" of children born in 1980 and 1981, my sample also includes younger children. As explained by Chetty et al. (2014), measuring children's income at early ages can overestimate mobility with respect to lifetime income, because children with high lifetime incomes have steeper earnings profiles when young (which stabilize around age 30). Children in the

The measure of mobility I use in my tests is the expected income centile of children (in the national children's income distribution) for each centile of the CZ income distribution of parents, for each CZ and cohort. I construct these measures as follows. Chetty et al. (2014) (Table 7) reports the parents' income distribution for each CZ. Specifically, they report income levels of the 10th, 25th, 50th, 75th, 90th, and 99th centiles of the distribution of parents' income within each CZ. I use this information to reconstruct the entire parental income distribution (i.e. for each centile), by simply interpolating incomes between these centiles. I then match income levels for each CZ centile with the corresponding centiles on the national distribution. I then use the slope and intercept of the linear relationship between parents' and children's income centiles on their respective national distributions to back out the national income centile of the child, for each CZ income centile of the parent, separately for each CZ and birth cohort between 1980 and 1986. Assuming that the income distributions did not change over time (and across cohorts) in each CZ, and assuming that each centile of the CZ distribution of parents' income contains 1/100 of all children in the CZ, this procedure allows to approximate the distribution of income for children in each CZ and birth cohort, given each parent's income centile on the national distribution. In each regression, I weight observations by the number of children in cohorts 1980-82 in each CZ.

younger cohort in my income mobility sample (born in 1986) are 26 in 2012. The measurement error generated by the inclusion of the younger cohorts, however, should be quite limited (see Chetty et al., 2014, Figure IIIA). In addition, younger cohorts are more likely to be correctly matched to the the ZIP code where they grew up.

Appendix C School Finance Equalization Reforms

California

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

$$\begin{aligned} R_i &= \max\{\max\{2,400,400 \times n_i\}, \max\{\text{RL}_i - 0.01p_i\}\} + 0.01p_i \\ \text{RL}_i &= \bar{\text{RL}}_{-1} \times \text{CODB}_i \end{aligned}$$

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

Illinois

The school finance plan in place in 1986 had been implemented in 1980. District revenues came from state transfers and from a local property tax. General State Aid (GSA) was distributed using a three-tiers formula: property value determined which tier applied to each district. The three tiers were: Special Equalization (SE), Alternate Method, and Flat Grant. The formula can be summarized as follows:

$$
\begin{aligned}
R_i &= \text{Aid}_i + \tau_i p_i n_i \\
\text{Aid} &= \text{SE or AM or FG} \\
\text{SE} &= n_i(f/\hat{\tau} - p_i)\hat{\tau} \\
\text{AM} &= n_i f \times 0.13 \times 0.87 \times f/p_i \\
\text{FG} &= n_i 321.21
\end{aligned}
$$

where τ_i is the property tax rate in district i, p_i is per-pupil property valuation, n_i is the weighted count of pupils, f is the foundation level, and $\hat{\tau}$ is equal to 1.9% for elementary districts, 1.1% for secondary districts and 2.76% for unit districts.

The legislation changed the financing formula in 1998. The three-tier structure was maintained (Foundation, Alternate Method and Flat Grant). Per-pupil property wealth in each district determines which formula must be used to compute the funding. The state aid formula compares the district valuation to a guaranteed wealth per ADA. The guaranteed level varies by the type of school district: in 1999 it was equal to $188,478 for elementary districts, $361,250 for secondary districts, and $144,500 for unit districts. Districts qualifying under the Foundation formula have per-pupil valuation less than 93% of the foundation level. Districts qualifying under the Alternate Method formula have per-pupil valuation of at least 93% but less than 175% of the foundation level. Districts qualifying under the Flat Grant formula have per-pupil valuation greater than 175% of the foundation level. The foundation level was $4,225 in 1999, the flat grant was $218. The formula can be summarized as follows:

$$
\begin{aligned}
R_i &= \text{Aid}_i + \tau_i p_i n_i \\
\text{Aid} &= \text{Foundation or AM or FG} \\
\text{Foundation} &= n_i(f - \text{Local Resources}) \\
\text{AM} &= n_i f[0.07 - ((\text{Local Percentage} - 0.93)/0.82)0.02] \\
\text{FG} &= n_i * 218 \\
\text{Local Resources} &= n_i p_i \hat{\tau} + \text{CPPRT}/n_i \\
\text{Local Percentage} &= 100 \times \text{Local Resources}/f
\end{aligned}
$$

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

Michigan

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

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

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

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

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

$$\begin{aligned} R_i &= f_i + \tau_i p_i - \bar{\tau}_i p_i \\ f &= \text{Foundation allowance} \\ \tau_i &= \text{actual property tax rate} \\ \bar{\tau}_i &= 0.018 \\ p_i &= \text{non-homestead property per-pupil} \end{aligned}$$

New Jersey

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

$$R_i = \tau_i p_i + \max\{0.1\bar{S}, \max\{0, (1 - \frac{p_i}{1.35\bar{p}}) \min\{e_i, \bar{S}\}\}\}$$

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

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

$$R_i = \tau_i p_i + \max\{0, f - 0.5(P_i p_i + Y_i y_i)\}$$

where f is the foundation amount (%6,640 in 1992), P_i and Y_i are, respectively, the property and the income multipliers, used to compute a district's fiscal capacity; p_i is property valuation and y_i is average income.

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

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

New York

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

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

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

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

centration of at-risk pupils:

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

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

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

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

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

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

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

Ohio

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

$$R_i = \tau_i p_i + \max\{n_i f(C_i) - \bar{\tau} p_i\} + e(\tau^e_{1i}, \bar{p}_1, p_i) + g n_i$$

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

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

Texas

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

$$R_i = \max\{n_i f(X_i) - \bar{\tau}_1 p_i\} + \tau_i p_i + e(\tau^e_{1i}, \bar{p}_1, p_i) + g n_i$$

where R_i is total revenues for district i, f is the foundation amount, function of n_i (weighted count of pupils) and X_i (characteristics of the district, such as price index, small size, etc.), $\bar{\tau}_1$ is the mandatory share of local effort (\$0.33 per \$100), p_i is local property valuation, τ_i is the property tax rate chosen by the district, e is the Enrichment Equalization Allotment, which depends on the district's property valuation, the average property valuation in the state, and local effort as summarized by a reference tax rate τ^e_{1i}, and g is a flat grant.

The formula changed in October 1989, when the Texas Supreme Court declared the state school finance system to be unconstitutional, as part of the *Edgewood* vs. *Kirby* lawsuit. The legislature responded with Senate Bill 1019, which modified the formula as follows. First,

it modified some parameters of the original formula. Second, it eliminated the Equalization Allotment, substituting it with a Guaranteed Tax Yield, which provides a specified amount per weighted pupil per penny of tax effort ($\bar{p}_2$), for up to 36 cents above the local fund assignment tax rate ($\bar{\tau}_2$). The flat grant was eliminated. The resulting formula is as follows:

$$R_i = \max\{n_i f(X_i) - \bar{\tau}_2 p_i\} + \tau_i p_i + \tau_{2i}^e \max\{\bar{p}_2 - p_i\}$$

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

$$R_i = \max\{n_i f(X_i) - \bar{\tau}_3 p_i\} + \tau_i p_i + \tau_{3i}^e \max\{\bar{p}_3 - p_i\}$$

Wisconsin

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

$$\begin{aligned} R_i &= T_i^1 + \max\{T_i^2 + T_i^3, 0\} + \tau_i p_i \\ T_i^1 &= (1 - p_i/\bar{p^1}) * \min\{C_i, \bar{C}^1\} \\ T_i^2 &= (1 - p_i/\bar{p^2}) * \min\{C_i - \bar{C}^1, \bar{C}^2\} \\ T_i^2 &= (1 - p_i/\bar{p^3}) * \max\{C_i - \bar{C}^2, 0\} \end{aligned}$$

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

decrease first-tier aid. In addition, districts are subject to a limit on the annual increase in their revenue per-pupil derived from state aid and property taxes. In 1999, this increase could not exceed $208.88. A school district that exceeds its revenue limit is subject to a penalty, in the form of reduced state aid, in the amount of the excess revenue.

Appendix D List of School Finance Reforms

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

Iowa	Yes	Foundation + equalization	1991	Code of Iowa, Chapter 257	Legislated	Foundation + equalization
Kansas	Yes	Guaranteed tax base	1992	School District Finance and Quality Performance Act (SDFQPA, 1992)	Legislated	Foundation + recapture
Kentucky	Yes	Foundation with power equalization	1990	*Rose v. Council for Better Education*, 790 S.W.2d 186 (Ky. 1989), followed by Kentucky Education Reform Act (1990)	Court-ruled	Minimum foundation with power equalization
Louisiana	Yes	Foundation	1992	Legislature	Legislated	Foundation
Maine	Yes		1985	School Finance Act of 1985	Legislated	Foundation
			1995	School Finance Act of 1995	Legislated	Foundation (minimum change in how state aid is
Maryland	Yes	Foundation	1986	Action Plan for Education Excellence (APEX),	Legislated	Foundation with required local effort
Massachusetts	Yes	Foundation	1994	*Mc Duffy v. Secretary of the Executive Office of Education*, 1993; Chapter 70	Court-ruled	Foundation
Michigan	Yes	Foundation + Guaranteed Tax Base	1993	P.A. 145 2 of 1993	Legislated	Foundation
Minnesota	Yes	Foundation	1988	General Education Revenue Program	Legislated	Foundation
Mississippi	Yes	Foundation	1997	Mississippi Adequate Education Program	Legislated	Foundation with required local effort
Missouri	Yes	Foundation + Guaranteed Tax Base	1993	Committee for Educational Equality v. Missouri; Outstanding Schools Act (OSA)	Court-ruled	Foundation with required local effort
Montana	Yes	Foundation	1993	*Montana Rural Ed. Association v. Montana*; House Bill 667	Court-ruled	Foundation + Guaranteed Tax Base

Nebraska	Yes	Foundation	1990	Tax Equity and Educational Opportunities Support Act (LB1059)	Legislated	Foundation
			1997	LB 806 (minor changes)	Legislated	Foundation
Nevada		Foundation				
New Hampshire	Yes	Foundation	1985	Statute	Legislated	Flat grant +
			1999	*Claremont v. Governor*	Court-ruled	Flat grant + equalization
New Jersey	Yes	Guaranteed tax base	1990	*Abbott v. Burke* 575 A.2d 359 (N.J. 1990)	Court-ruled	Foundation
			1996	"Comprehensive Educational Improvement and Financing Act of 1996	Legislated	Foundation
New Mexico		Foundation				
New York	Yes	Percentage equalization + flat grant	2003	*Campaign for Fiscal Equity, Inc. v. State*	Court-ruled	Percentage equalization + flat grant
			2006	*Campaign for Fiscal Equity, Inc. v. State*	Court-ruled	Percentage equalization + flat grant
North Carolina		Flat grant				
North Dakota		Equalized foundation				
Ohio		Foundation with local effort				
Oklahoma		Foundation + Guaranteed Tax Base				
Oregon	Yes	Foundation	1990	Measure 5; Chapter 780, Oregon Laws 1991	Legislated	Foundation (caps on local tax rates)
			1997	Measure 50	Legislated	Foundation (caps on local tax rates)

Pennsylvania		Foundation + percentage equalization				
Rhode Island	Yes	Foundation	1995	Legislation	Legislated	Flat grant
South Carolina	Yes	Foundation	1985	Education Improvement Act (EIA)	Legislated	Foundation + categorical (with
South Dakota	Yes	Expenditure-driven formula	1995	Legislation	Legislated	Foundation
Tennessee	Yes	Foundation	1992	Education Improvement Act	Legislated	
Texas	Yes	Foundation	1991	Edgewood Independent School District v. Kirby	Court-ruled	Foundation
			1993	Senate Bill 7	Court-ruled	Foundation (tier 1) + Guaranteed Tax Yield (tier 2) +
Utah		Foundation + required local effort				
Vermont	Yes	Percentage equalization	1987	Legislation	Legislated	Foundation
			1997	*Brigham v. State* , followed by Act 60	Court-ruled	Flat grant + guaranteed tax yield
Virginia		Foundation				
Washington	Yes	Foundation	1987	Legislation	Legislated	Foundation + Guaranteed Tax
West Virginia		Foundation				
Wisconsin	Yes	Guaranteed tax base - 2 tiers	1996	Legislation	Legislated	Guaranteed tax base - 3 tiers
Wyoming	Yes	Foundation	1995	*Campbell County v. State*	Court-ruled	Foundation