INCOME INEQUALITY, THE MEDIAN VOTER, AND THE SUPPORT FOR PUBLIC EDUCATION

Sean P. Corcoran[a] and William N. Evans[b,c]

September 21, 2011

ABSTRACT

Using a panel of U.S. school districts spanning 1970 – 2000, we examine the relationship between income inequality and fiscal support for public education. In contrast with recent theoretical and empirical work suggesting a negative relationship between inequality and public spending, we find results consistent with a median voter model, in which inequality that reduces the median voter's tax share induces higher local spending on public education. We estimate that 11 to 22 percent of the increase in local school spending over this period is attributable to rising inequality.

Acknowledgements:

This research is supported by a grant from the Russell Sage Foundation as part of their special program on the Social Dimensions of Inequality, and by the National Science Foundation. We wish to thank Robert Schwab, Caroline Hoxby, Howard Rosenthal, Jon Sonstelie, Sheila Murray, Amy Harris, Jennifer Godwin, Wally Oates, participants in the NBER Economics of Education program, and two anonymous referees for a number of helpful suggestions. All errors are solely our own.

[a] New York University, Institute for Education and Social Policy, 665 Broadway Suite 805, New York, NY 10012. Email: sean.corcoran@nyu.edu.

[b] University of Notre Dame, Department of Economics and Econometrics, 437 Flanner Hall, Notre Dame, IN 46556. Email: wevans1@nd.edu. Phone: (574) 631-7039, Fax: (574) 631-4783. Contact author.

[c] National Bureau of Economic Research.

1. Introduction

According to the U.S. Census Bureau (2010a), inequality in household income as measured by the Gini coefficient rose nearly 20 percent from 1969 to 2009, an increase driven largely by income growth in the top half of the distribution.[1] This growth in inequality has prompted two important and related strands of research. The first has sought explanations for this trend, focusing primarily on changes in the distribution of wages and earnings.[2] A second has sought to assess the social and economic consequences of inequality, including effects on mortality and health (Kawachi, Kennedy, and Wilkinson, 1999; Deaton, 2001; Mellor and Milyo, 2002) crime (Kelly, 2000; Fajnzylber, Lederman, and Loayza, 2002), civic engagement and trust (Alesina and La Ferrara, 2000, 2002; Costa and Kahn 2003), and economic growth (Benabou, 1996; Forbes, 2000), to name a few.

Related to this second strand is a growing literature on the impact of inequality on the demand for public goods and income redistribution. Recent work has found that inequality and population heterogeneity more broadly defined tend to be associated with a *lower* level of welfare spending and support for public goods, both across nations and within sub-national jurisdictions (Goldin and Katz, 2009; Alesina, Baqir, and Easterly, 1999; Alesina, Glaeser, and Sacerdote, 2001; Luttmer, 2001; Lind, 2007; Fernandez and Levy, 2008).

These findings stand in contrast to the basic predictions of standard voting models. In a classic set of papers, Meltzer and Richard (1981, 1983) proposed that under majority rule, income inequality can result in greater public spending whenever mean income rises relative to that of the

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[1] The Gini coefficients of household income in 1969, 1979, 1989, 1999, and 2009 were 0.391, 0.404, 0.431, 0.458, and 0.468. The U.S. is not unique in this trend—other industrialized nations witnessed a similar rise in inequality over this period—but few experiences match that of the U.S. in terms of magnitude (Gottschalk and Smeeding, 1997; Kenworthy and Pontusson, 2005).

[2] One group of results suggests that skill-biased technological change, and—to a lesser degree—globalization and trade are the primary forces driving earnings inequality (Katz and Autor, 1999; Autor, Katz, and Kearney, 2008) while a competing group of papers argues that institutional factors such as the minimum wage and declining unionization have driven the rise in inequality (Card and DiNardo, 2002; Dinardo, Fortin, and Lemieux, 1996).

median voter. In this model, growing wealth at the top of the income distribution lowers the tax price of raising revenue, allowing the median voter to obtain greater public services at a lower marginal cost. Empirically, this model has met with mixed success over the years (Meltzer and Richard, 1983; Husted and Kenny, 1997; Gouveia and Masia, 1998; Alesina, Glaeser, and Sacerdote, 2001; Borge and Rattsø, 2004; Kenworthy and Pontusson, 2005; Boustan et al., 2010).

A plausible explanation for the mixed evidence for the Meltzer-Richard hypothesis is that most of its empirical tests are applied in settings where the model's assumptions are unlikely to hold. The voting model in these papers presumes direct or representative democracy in which voting is over a single-dimensional policy space and voters have single-peaked preferences over policy options (Borck, 2007). These assumptions are more likely to be approximated in local governments than national ones, yet the Meltzer-Richard hypothesis is most often examined in a national context (Turnbull and Mitias, 1999; Fischel, 2001; Mueller, 2003).

In this paper, we draw upon a balanced panel of more than 10,300 local school districts spanning 1970 – 2000 to explore the relationship between rising income inequality and fiscal support for public elementary and secondary education. In contrast with recent theoretical and empirical work suggesting a negative relationship between inequality and public spending, we find results consistent with the Meltzer-Richard hypothesis—rising income inequality appears to be associated with *higher* per-pupil expenditure, driven primarily by an increase in revenues from local sources. Given the redistributive nature of education, our results suggest that some of the potentially negative consequences of rising social inequality may have been counteracted by local government's ability to raise additional funds from growing incomes at the top of the distribution.

We begin our analysis by examining the relationship between income inequality within school districts and local spending on K-12 education. Estimating this relationship has several challenges. For example, U.S. public education has traditionally been financed through property rather than income taxes. Thus, income inequality in our analysis serves as a proxy for the underlying tax price, and not the tax price itself. This proxy may be imperfect, for reasons we discuss later. Additionally, the Tiebout (1956) model predicts that households sort into communities in line with their preferences for spending and taxes. An implication of sorting is that changes in the local income distribution may be both a cause and effect of local government policies.[3] Finally public education has come to rely increasingly on state aid. The 1970 – 2000 period was characterized by a series of court-ordered school finance reforms that increased the level of state aid, particularly for low-wealth districts (Murray, Evans, and Schwab, 1998; Hoxby, 2001; Corcoran and Evans, 2008).

We address these challenges in a number of ways. First, we exploit the panel nature of our data to address omitted variables bias to a much greater degree than in previous work. Through school district fixed effects, we account for fixed characteristics of localities that would contaminate cross-sectional analyses, such as the presence of non-residential property wealth. Having multiple observations per state per year permits us to control for time-varying state shocks to the level of school spending, and we use variation induced by court-ordered finance reforms to address the potential endogeneity of state aid. Second, two different instruments for inequality are used to address sorting. In the first case, we exploit the specifics of the median voter model and utilize higher-order moments of the income distribution as instruments for inequality. If the median voter model holds, the median voter should only care about the tax price of public services. Thus, the skewness of the income distribution should be correlated with inequality but not enter directly into the spending equation. As a second instrument, we observe that certain types of school districts experienced larger growth in inequality over this period, and group districts into categories based on their 1970 characteristics, including size, location, and initial inequality. We then calculate a "synthetic cohort" inequality measure for 1980 – 2000 which captures income inequality in other,

[3] In fact, we find much more inequality within local school districts than Tiebout might predict; on this, see also Rhode and Strumpf (2003).

observationally similar schools. This measure is highly correlated with a district's inequality, but contains no information about the district itself, and thus is a valid instrument. Finally, we conduct a series of sensitivity checks that re-estimate our models for subsamples of districts (such as rural and suburban districts, or high-income, high-wealth districts) that are less likely to be influenced by confounders such as non-residential property, equalization aid, or sorting.

We next consider the relationship between within-district income inequality and the level of expenditure and state aid per student. In making public spending decisions, local households are concerned with both their local tax burden and the overall level of per-student expenditure. In the U.S., education is a shared responsibility between local, state, and federal governments, and in many cases, the structure of state aid programs influence the tax price of local spending (Hoxby, 2001). Our expenditure model captures the net effect of growing inequality on overall expenditure per student. Finally, we examine the relationship between within-district income inequality and rates of private school enrollment. We hypothesize that private enrollment will respond to income inequality through several channels: families' enrollment response to fiscal changes brought about by changes in income inequality, to changes in the peer composition of schools that accompanies higher inequality, or both. In contrast to de la Croix and Doepke (2009), we find little flight to private schools in districts with rising income inequality.

2. Related Literature and Theoretical Framework

2.1. Inequality and Public Finance

In recent years, the public finance literature has examined how inequality and population heterogeneity broadly defined impact the demand for public goods. Much of this literature attempts to explain differences in government size and income redistribution across nations and within countries over time. Meltzer and Richard (1981, 1983) proposed a simple model where an electorate votes via majority rule on a system of income redistribution funded by a proportional income tax. They showed that changes in the relative position of the decisive (e.g., median) voter in the income distribution affects the level of redistribution and thus the size of government. Specifically, they showed that growth in mean income relative to the median lowers the "tax price" of redistribution to the median voter, who rationally votes for greater government spending.[4] This model has been tested empirically in multiple contexts, with mixed results. In the U.S., Husted and Kenny (1997) found that the extension of the voting franchise led to greater state welfare spending as the income of the median voter fell relative to statewide income.[5] In contrast, cross-national comparisons of public spending in developed countries have been less supportive of the Meltzer-Richard hypothesis (Perotti, 1996; Benabou, 1996). Comparing U.S. and European welfare policies, Alesina, Glaeser, and Sacerdote (2001) noted that pre-tax income inequality is considerably higher in the U.S. than in European countries, while European nations tend to have larger welfare systems.[6]

Variation in social spending has been linked to other forms of population heterogeneity, including racial and ethnic diversity. Alesina, Glaeser, and Sacerdote (2001) argued that population heterogeneity explains much of the variation in redistributive spending across developed countries. Within the U.S., Luttmer (2001) modeled individual preferences for welfare spending as a function of the share of beneficiaries from the same socioeconomic or ethnic group. Using the General Social Survey, he showed that while individual support for redistribution decreased in the number of local welfare recipients, support *increased* with the fraction of area recipients of the same race (see also Lind, 2007). Alesina, Baqir, and Easterly (1999) similarly showed that public expenditure in U.S. metropolitan areas on education, roads, libraries, sewers, and trash pickup is negatively related to

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[4] This assumes a non-symmetric, positively skewed income distribution.
[5] See Gouveia and Masia (1998), Borge and Rattsø (2004), and Perotti (1993) for contrasting evidence. Borck (2007) provides a review of voting models of redistribution and empirical evidence on these models.
[6] More recently, however, Kenworthy and Pontusson (2005) show that nations that experienced the largest *increases* in inequality in the 1980s and 1990s experienced the greatest increases in redistribution.

within-MSA ethnic fragmentation.

2.2. Inequality and the Support for Public Education

With its hybrid of local property tax funding and state and federal aid, public education in the U.S. is not an obvious archetype for the Meltzer-Richard model. However, the tight link between household income, property wealth, and school spending suggests that income inequality may have similar tax price effects in local school districts, something we discuss in greater depth in section 3.2.

How income inequality relates to spending on public education is important for several reasons. First, K-12 education comprised 29 percent of aggregate state and local government expenditure in 2007, a larger share than any other general expenditure category (U.S. Census Bureau, 2011). Any impact of inequality on public spending is thus likely to affect education. Second, not all households directly benefit from the quality and quantity of public schools. Households without school aged children, the elderly, and families with children in private school only indirectly benefit from investments in public education. To the extent interpersonal preferences of the type exemplified in Luttmer (2001) exist, they may be particularly important in education. Third, the level and distribution of school spending has historically been tightly linked with income (Goldin and Katz, 2009; Hoxby, 1998; Fernandez and Rogerson, 2001). Much has been written on the effects of income inequality on spending disparities *across* jurisdictions, but less is known about the consequences of rising inequalities *within* jurisdictions over time. Finally, the availability of private alternatives may alter the balance of support for public schooling in more unequal populations (de la Croix and Doepke, 2009).

Epple and Romano (1996) provided an intriguing example of the latter. They argued that for public goods like education where private options exist, the likely majority voting equilibrium will be one in which there are two opposing coalitions of voters—one comprised of high- and low-income households who demand a low level of expenditure on public education, and another made up of middle-income households who demand a high level of school spending. The coalition of high- and low-income families oppose education spending for different reasons: the low income group has less to spend on public goods, while high income families make greater use of private schools. In this case, greater income inequality increases the likelihood of an "ends against the middle" outcome, with low spending on public education and high rates of private schooling.[7]

Evidence favoring the "ends against the middle" hypothesis can be found in the expansion of secondary schooling in the U.S. in the early 20th century, as demonstrated by Goldin and Katz (2009). They found communities that supported the expansion of secondary education had more equal income distributions and more ethnically and religiously homogeneous populations. The more heterogeneous communities lagged behind in the growth of secondary schooling.

Dimensions of community heterogeneity other than income have also been shown to play an important role in the political support for public education. A commonly cited example is the age distribution. Poterba (1997) found that states with higher growth in the fraction of residents over age 65 experienced slower rates of growth in per-child educational expenditure, an observation consistent with a preference to vote down public programs they do not benefit from themselves. He found the effect is magnified to the extent the elderly population is racially incongruent with the school-age population. Others who have tested for effects of a growing elderly share on local, as against state, support for education have found a less stark relationship, in part because the elderly benefit from schools through their property values (Ladd and Murray, 2001; Harris, Evans, and Schwab, 2001; Hilber and Mayer, 2007).

2.3. Local Population Heterogeneity and the Tiebout Model

Unlike analyses of national income redistribution, studies of local and state spending must contend with Tiebout sorting (1956), the workhorse economic model of local public good provision.

[7] See Glomm and Ravikumar (1998) for an alternative model with private schooling options, where the median voter is decisive.

In the standard Tiebout framework, households sort into homogenous communities offering their preferred level of public services and taxation. While the pure Tiebout model has restrictive and generally unrealistic assumptions, empirical research has found that local taxes and school quality are capitalized into property values, a key implication of the Tiebout model (Black, 1999; Ross and Yinger, 1999; Fischel, 2001).

Our efforts to estimate the impact of income inequality on the support for local public schools are complicated if the strict Tiebout model is a correct characterization of the real world. For example, under Tiebout sorting, it is unclear why communities in equilibrium would be heterogeneous at all. Moreover, with the ability to sort, one must be concerned that within-district income inequality is itself a response to the level of public spending rather than the converse.

The Tiebout model predicts homogeneity of *demand* for public goods within communities, not necessarily homogeneity in observable population characteristics (Epple and Platt, 1998). In practice, we do not observe preferences but rather correlates with demand for public goods, such as income, wealth, age, race, ethnicity, home ownership, and the like. Yet even if these observable characteristics are noisy measures of preferences, communities are more heterogeneous than Tiebout might predict. This should not be too surprising. While metropolitan areas in the U.S. exhibit features of the Tiebout model to varying degrees, in practice, few come close to its ideal, given barriers to perfect sorting. Communities may also tolerate population heterogeneity in exchange for economics of scale in the production of public goods (Alesina, Baqir, and Hoxby, 2000). Ultimately, the weaker are Tiebout forces, the more likely conflicting household demands for public goods will be resolved through the political process.

Rhode and Strumpf (2003) found that Tiebout sorting—that is, movement due to local tax and spending policies—may be of only second order importance in the locational decisions of U.S. households. Juxtaposed against dramatically falling transportation, commuting, and communication costs—all of which in theory should increase sorting and stratification across communities—they found *less* stratification of household characteristics and policy outcomes across municipalities over the 1850 – 1990 period. In their analysis of the Boston Metropolitan Statistical Area (MSA)—often held up as an archetype of Tiebout competition—they found no evidence of increased sorting in the post-World War II period. Within-municipality income distributions in suburban Boston have changed little, and the between-community component of income inequality in Boston has risen only slightly since 1949. Cutler, Glaeser, and Vigdor (1999) and Kremer (1997) similarly found that sorting between neighborhoods has remained constant or declined in recent decades.

We stress that none of this research refutes the Tiebout hypothesis, and it is certainly the case that local public goods and taxation are important factors in household locational decisions. The results above, however, suggest that forces other than Tiebout sorting have been important enough to maintain a relatively high level of within-community heterogeneity—a condition that is key to our analysis. We do take the potential endogeneity of the income distribution seriously, and as we outline below, use an instrumental variables strategy to identify exogenous variation in inequality.

3. Data and Empirical Strategy

3.1. Data Sources

Our analysis draws upon a balanced panel of demographic and financial data for more than 10,300 local school districts spanning 1970 – 2000. The panel was constructed by merging eight large national databases: the *Census of Population and Housing* special school district tabulations for 1970, 1980, 1990, and 2000 (U.S. Census Bureau 1973, 1982, 2002a; U.S. Department of Education 1994), the *Census of Governments: School Districts* for 1972, 1982, and 1992 (U.S. Census Bureau 1987, 1992, 1993), and the F-33 *Annual Survey of School Finances* for 2002 (U.S. Census Bureau 2002b).[8] The Census tabulations provide detailed information about household income and demographics for each school district, while the *Census of Governments* and *Annual Surveys* represent the primary historical source of school finance data in the United States. These eight databases are supplemented by a number of others, as described in the Data Appendix.

The construction of a matched panel database of school districts spanning more than three decades had several challenges. First, some school district boundaries changed over this period as a result of consolidations, splits, and unifications (mergers of separate elementary and secondary districts). We identified these district changes primarily by contacting state departments of education where such changes occurred. Our districts are defined based on their 2002 geographic definitions, such that if District A and District B merged or unified in 1995, we combine the data from these two districts in all prior years for comparability with 2002.[9] Districts involved in splits have been dropped from the panel, though there were very few of these cases. Second, we lose a number of school districts due to missing data in 1970 and 1990. In the 1970 Census, districts with fewer than 300 students were aggregated into one pseudo-district in each of 39 affected states, accounting for a loss of roughly 1,500 mostly rural districts. In the 1990 Census, a small number of counties in California did not participate in the special school district mapping, accounting for a loss of 196 districts. After excluding districts with outlying values of per-pupil expenditure, our balanced panel

[8] Similar matched panel datasets of school districts were used by Hoxby (1996, 2001) and Harris, Evans, and Schwab (2001). Prior to 1989-90, school district financial data was only available from the Census of Governments in years ending in a two or five. We thus match the 1970 and 1980 cross-sections of Census data to financial data from the 1972 and 1982 fiscal years. For consistency across the four cross-sections, we match the 1990 and 2000 Census data to financial data from the 1992 and 2002 fiscal years.

[9] When an elementary and secondary district covering the same geographic territory consolidate to form a unified (K-12) district, we do not aggregate Census data from the two districts—this would be double-counting—but rather use Census data from the larger area (usually, the secondary district).

contains 10,359 school districts observed in four years, for a total of 41,436 observations.[10] While this panel comprises only 75.6 percent districts in existence in 2002, these districts account for 95.2 percent of U.S. enrollment in that year.

Income inequality measures are generally not available at geographic levels smaller than states. Accordingly, we use Census data on the counts of families falling into ordered income categories to calculate inequality measures for every school district in each panel year.[11] To do this, we assume a flexible functional form for the CDF of family income in each district, and use the grouped income data to estimate the parameters of this distribution via maximum likelihood. With these parameters, we can then generate estimates of various measures of inequality.

The procedure is implemented as follows. Suppose in a particular year there are K income groups and n_{ik} is the number of families in income group k from district i. The K groups are families with incomes $\leq a_1$, ($> a_1$ and $\leq a_2$), …, ($> a_{K-2}$ and $< a_{K-1}$), and $> a_{K-1}$ where $a_1 < a_2 < \ldots < a_{K-1}$. Let y represent income and the CDF of the income distribution $\Pr(y \leq a) = F(a|\beta_i)$, where β_i are the parameters of the assumed distribution of income for district i. Let P_{ik} be the probability of observing income in group k, where $P_{i1} = F(a_1|\beta_i)$, $P_{i2} = F(a_2|\beta_i) - F(a_2|\beta_i)$, … $P_{iK-1} = F(a_{K-1}|\beta_i) - F(a_{K-2}|\beta_i)$, and $P_{iK} = 1 - F(a_{K-1}|\beta_i)$. Therefore, the likelihood function for district i in this year is $L_i = \Sigma_k n_{ik}\ln(P_{ik})$ which is maximized through the choice of β_i.

Based on McDonald (1984), we elected to fit a three-parameter Dagum (1980) distribution, also known as the Burr Type III distribution. In his paper, McDonald fit a series of statistical distributions to U.S. income in 1970 and 1980 and concluded that the Dagum distribution out-performed all other three-parameter models, as well as some four-parameter models, in terms of fit.

[10] School districts are excluded if their per-student local revenues are more than twice the 95th percentile nationwide, or less than 25 percent of the 5th percentile nationwide, in any year. Alaska, Hawaii, and the District of Columbia are also excluded (the latter two consist of only a single school district).
[11] We use family income as opposed to household income due to the 1970 Census, which only reports the income of families and "unrelated individuals."

This distribution also has the advantage of having a straightforward, closed-form solution for its moments. Our main inequality measure in this paper is the ratio of mean to median family income, both of which are straightforward to calculate given the estimated parameters of the Dagum distribution. For comparability, we compute alternative measures of income inequality, including the Gini coefficient, Theil index, and natural log of the ratios of the 95th to 50th, and 50th to 5th percentiles of family income. The specific functional form of the Dagum distribution and how it can be translated into specific inequality measures is described in the Data Appendix.

Although the income data we use in this project is categorical, the procedure outlined above generates accurate estimates of income inequality.[12] Evans, Hout, and Mayer (2004) used categorical data on family income at the state level from various census years to estimate the parameters of the Burr III distribution for each state, and then compared the implied Gini coefficient from these estimates with those reported by the Census from the entire long form sample. The correlation coefficients from the computed state-level Ginis in family income and the reported values for the 1970, 1980, and 1990 census were 0.998, 0.996, and 0.980, respectively.

School district demographics were taken from the special Census tabulations, described in the Data Appendix. From the Census data on race, we calculated a standard index of racial heterogeneity as one minus the sum of the squared population shares of four race categories: white, black, Asian/Pacific Islander, and other (following Alesina, Baqir, and Easterly, 1999; Vigdor, 2002; Alesina and La Ferrara, 2002). This index ranges from a lower bound of zero (perfect homogeneity) to an upper bound of 0.75 (maximum heterogeneity with four groups).

Descriptive statistics are presented in Table 1. All observations are weighted by public school enrollment, such that our statistics characterize the school district in which the average public

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[12] The number of income categories reported in the Census varies over time. However, as we show in the Data Appendix, changes in the number of categories do not materially affect our estimates of income inequality, nor do they create a systematic bias.

school student resides. Several trends are worth noting. Mean local revenues per pupil rose 62 percent in real terms from 1972 to 2002, at the same time local funds as a share of overall per-pupil spending fell from an average of 54.5 percent to 42.3 percent. Real median family income rose an average of 18 percent over the sample and at the same time, within-district income inequality increased considerably. For example, inequality as measured by the ratio of mean to median family income rose 9 percent, on average. Other measures of income inequality more sensitive to changes throughout the distribution show much larger changes. The Gini coefficient increased 15 percent on average from 1970 to 2000, while the average Theil index rose a more sizable 39 percent. As was true nationally, income inequality grew more in the top half of the distribution: the rise in the average (log) ratio of the 95^{th} to 50^{th} percentile of income was 18 percent as compared to an 11 percent increase in the average (log) 50^{th} to 5^{th} ratio. We elaborate on this growth in inequality within school districts in section 4.1.

Notably, school districts became more racially diverse from 1970 to 2000, as evidenced by the near doubling of the mean index of race fractionalization, and the doubling of the mean percent nonwhite over this period. The elderly share in the average district rose almost three percentage points, from 9.5 to 12.1.

3.2. Empirical Strategy

Our goal is to examine how rising income inequality has affected the fiscal support for public elementary and secondary education. We begin by examining the relationship between within-district income inequality and locally raised revenues for public schools. Because public education in the U.S. has traditionally been financed through property rather than income taxes, the literal version of the Meltzer-Richard model will not apply. Inequality in this context (e.g., the ratio of the mean to median income) is not itself the (inverse) tax price, but a proxy for the tax price, which may be a function of both income and property wealth. Identifying how education finance departs from the traditional Meltzer-Richard framework enables us to identify possible omitted variables from our analysis and to construct tests to assess their importance, as described below.

To fix ideas, assume districts fund schools purely through local property taxes (ignore intergovernmental aid and sorting for the moment). The district's per-pupil budget constraint is then $e = t(\Sigma V_j + C)/P$, where V_j is the property wealth of household j, C is the value of non-residential property, t is the property tax rate, and P is enrollment. Holding fixed V, C, and P, the choice of e defines a required tax rate t. Household i's budget constraint is $y_i = z_i + tV_i$, where z_i is spending on other goods and tV_i is the property tax bill. Substituting in for the tax rate t, the latter can be written: $tV_i = ePV_i / (\Sigma Vj + C)$. The tax price for household i, the cost of an additional dollar in per pupil spending, is $d(tV_i)/de$. In the simplest case where C=0 and each household has one pupil ($P = N$), the tax price is V_i/V_m, where V_m is the mean property value. Holding V_i fixed, an increase in mean property lowers the tax price of school spending to household i. With non-residential property, the tax price falls with C, and when $P \neq N$ the tax price rises with the number of pupils per household. Given income, preferences, and tax price, household i votes for a level of school spending that maximizes its utility.

This model underlies the empirical analysis of demand for local public goods pioneered by Borcherding and Deacon (1972) and Bergstrom and Goodman (1973). In those papers, and the many that followed, observed expenditure on local public goods reflects the expenditure demanded by the median voter, which in turn depends on her income, tax price, and taste for public goods. In the above, the tax price would be a function of the ratio of median to mean property wealth (V_{med}/V_m), the value of non-residential property, and the number of pupils per household. All else equal, an increase in V_m relative to V_{med} will be associated with a rise in public spending (analogous to Meltzer-Richard). "Tastes" are proxied using a vector of population characteristics associated with demand for these public services, e.g., age, race, educational attainment, school attendance, and homeownership (Rubinfeld and Shapiro, 1989; Harris, Evans, and Schwab, 2001; Hoxby, 2001).

If income and property values are positively related, the above helps illustrate the potential effects of rising inequality on local spending. For example, suppose that inequality increases due to a rise in income in the top of the distribution, with median income unchanged. As the ratio of mean to median income rises, so too does the ratio of mean to median property wealth. The decline in the tax price in turn encourages greater spending on public schools. Unfortunately, we do not observe values of residential and non-residential property wealth by district for this period, so we cannot capture this effect directly. If income inequality were related to expenditure only through this mechanism, it would be a suitable proxy for tax price. However, if it is correlated with omitted factors related to the tax price (such as the value of commercial property or available state aid), it may lead to biased estimates of the effect of income inequality on spending.

Our empirical strategy addresses these issues in several ways. First, we exploit the panel nature of our data and include school district fixed effects to capture the effects of time-invariant district characteristics on local spending. These include long-run variation in preferences for taxes and school quality, and permanent features of the local tax base that affect the tax price, such as the presence of taxable commercial or vacation property and natural resources.[13] Second, we estimate some models using an instrumental variable that should be uncontaminated by variation in local property wealth. (This strategy is also designed to address Tiebout sorting, as discussed below). Finally, we provide separate estimates for subgroups of districts that are more or less likely to be affected by omitted variables bias. These include rural or suburban districts that rely less on non-residential property, and high-income or high property-wealth districts that tend to receive little in state aid.

Our baseline empirical specification for local revenues per student is a traditional model of

[13] Much of the variation in local revenues is between districts—not within districts over time. In a regression of real local revenues per student on district fixed effects and year dummies alone, the R^2 is 0.79.

demand for local public goods, with income inequality included in the model.[14] In school district *i* in state *j* in year *t* (y_{ijt}), this is given by:

$$(1) \qquad y_{ijt} = \boldsymbol{X}_{ijt}\beta + inequality_{ijt}*\gamma + S_{ijt}*\theta_s + F_{ijt}*\theta_f + \delta_i + \delta_{jt} + u_{ijt}$$

where $\boldsymbol{X}_{ijt}$ is a vector of population and housing characteristics (including median family income, percent below poverty, percent of adults that are college graduates, percent aged 5-17, percent aged 65 and older, percent living in an urbanized area, percent of housing units that are owner-occupied, percent nonwhite, and the index of racial heterogeneity), $inequality_{ijt}$ is income inequality, S_{ijt} is real state aid per pupil, F_{ijt} is real federal aid per pupil, δ_i is a school district fixed effect, and δ_{jt} is a state-by-year effect intended to capture state-specific trends in the level of spending. The variable u_{ijt} is an idiosyncratic error term representing all other time-varying determinants of local spending.

Our coefficient of interest in equation (1) is γ, the impact of within-district income inequality on local education revenues per pupil, holding constant observable district characteristics, time-varying shocks at the state level, and state and federal aid. Our use of district fixed effects means that variation within districts over time is used to identify γ and other coefficients in the model. All regressions are weighted used total public K-12 enrollment.

Local revenues represent a sizable fraction of spending on public education in the U.S., but the local share varies significantly across states and over time (Corcoran and Evans, 2008).[15] Among

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14 We are not the first include income inequality in a model of local education spending—see, for example, Hoxby (2000, 2001), and Urquiola (2000). However, income inequality is rarely an explanatory variable of primary interest in these papers. For example, Hoxby (2000) included measures of income inequality and racial and ethnic heterogeneity in her district-level regressions assessing the impact of school district competition on school expenditure. These variables were intended to serve as controls, however, and she did not discuss her empirical findings on these measures.

15 School districts in Vermont, for example, provided only 6.1 percent of K-12 education revenues in 2004-05, while local districts in Pennsylvania contributed 53.9 percent to school spending in that year. Thirty years

other things, this variation reflects differences in revenue-sharing practices across states, legislative and court-ordered finance reforms, and economic shocks impacting local districts' ability to raise revenue. School finance reforms of this period sought to equalize spending, or relieve tax burdens in low-wealth districts by providing more generous infusions of aid and offering more compensatory aid formulas (Murray, Evans, and Schwab, 1998; Hoxby, 2001). Our inclusion of δ_{jt} accounts for state-specific variation over time in the level of state aid. We also include per pupil state aid (S_{ijt}) directly in the model, to capture variation across districts in aid and its effects on local revenues.

The equalizing nature of state aid suggests that aid will typically be endogenous to local spending, except where administered as a flat grant. For example, local districts may reduce their own tax burden in response to greater state aid or increase spending to receive matching aid. To address this, in most specifications of (1), we instrument for S_{ijt} with an interaction of dummy variables for court-ordered finance reform at the state level and district *i*'s initial position in its state's income distribution in 1970. Equation (2) shows the first stage of this regression:

$$(2) \qquad S_{ijt} = \sum_{k=1}^{3} overturn_{jt} * incomeq_{ijk,70}\varphi_k + \boldsymbol{X}_{ijt}\Pi + inequality_{ijt}*\lambda + \theta_i + \theta_{jt} + \varepsilon_{ijt}$$

where $overturn_{jt}$ is a dummy variable that equals one if state *j* experienced a court-ordered school finance reform prior to year *t* and the $incomeq_{ijk,70}$ are dummy variables that equal one if district *i* was in quartile *k* of income in state *j* in 1970 (k = 1, 2, 3). Most existing research has made a strong case for the exogeneity of court-ordered finance reforms (e.g., Card and Payne, 2002; Figlio, Husted, and Kenny, 2004; Baicker and Gordon, 2006). Consequently, most empirical research has treated court-mandated reforms as exogenous events, and we make this assumption here as well.

earlier, Vermont localities provided a significantly higher 57.9 percent of revenues, while Pennsylvania districts contributed a lower 46.2 percent (U.S. Department of Education 2008).

Our instrumental variables approach to state aid fails to address an additional issue: under certain aid formulas (such as a foundation program), state aid can respond mechanically to changes in local wealth. If a district is below the foundation level, an increase in its mean income (when coupled with an increase in mean property wealth) will generate a reduction in state aid. In this case, the tax price effects of inequality will be partially reflected in the state aid coefficient, biasing the coefficient on income inequality downward. Without direct measures of property wealth or details about the state aid formula, these cannot be completely disentangled. In a set of sensitivity checks, we focus attention on subsets of districts (such as high-income districts) where state aid constitutes a much smaller share of revenues.

Finally, our discussion of Tiebout sorting leads us to be concerned that changes over time in inequality are themselves a reflection of the spending or performance of local school districts. High-income households without children who perceive school taxes to be too high, for example, may relocate to a neighboring district, affecting the income distribution in both the sending and receiving district (Fernandez and Rogerson, 1996). School finance equalization and the number of area jurisdictions may also influence the extent of income inequality between and within school districts (Aaronson, 1999; Urquiola, 2000).

As one strategy for addressing mobility's effect on inequality, we use higher moments of the local income distribution as instruments for the mean-to-median income ratio. In the strict form of the Meltzer-Richard hypothesis, the median voter's tax price is defined as the ratio of median to mean income. To this voter, the distribution of income that determines the tax price is irrelevant—what is important is the tax price of public services. To illustrate this, consider two districts with the same mean and median income, but District A has a more positively skewed income distribution than District B, perhaps driven by a larger share of income coming from the top few percentiles of income. In this simple case, holding all else constant, the median voter model would predict the same level of spending on local goods in Districts A and B, since the tax share is identical for the decisive voter. However, in a cross-section of districts we would anticipate that as the skewness of income rises, the mean rises faster than the median and the tax share will fall. Thus, if the median voter model is correct, then the skewness of income should be a valid instrument for the tax price. In the median voter model (and holding mean income constant to eliminate income effects) an increase in skewness in the income distribution will only increase spending through a reduction in the tax price. In this situation, a change in skewness will only change the burden of who pays for the public goods.

The advantage to this instrument is that by definition, if the median voter model is correct, higher-order moments of the local income distribution should have no direct impact on local spending levels. However, if spending outcomes deviate from those predicted by the median voter model, the instrument may not satisfy the exclusion restriction. For example, if higher local spending attracted a larger share of high-income families who wielded disproportionate influence on local elections, the concentration of income in the right tail may directly impact local spending.

To guard against this possibility, we re-estimate our models using an alternative instrument similar to that used by Boustan et al. (2010).[16] We note that district characteristics measured in 1970 are predictive of whether the area experienced above-average growth in income inequality over the next 30 years. For example, large districts, districts in MSAs, and districts with initially low levels of inequality had substantially larger increases in inequality over time. Exploiting this fact, we assign districts to one of 72 "synthetic cohorts," based on their enrollment, locale, and inequality level in 1970. In assigning these groups, we use six size categories, four quartiles of inequality (as measured

[16] Boustan et al. (2010)'s data is similar in structure to ours, but they use spending at the local government level. As in our paper, their measure of inequality is constructed with counts of families in income groups. To construct their instrument, the authors replace the local counts of families in the 1980-2000 years with the 1970 counts, which fixes the distribution at the 1970 levels. Next, the authors inflate the average income levels in each group by the national change in income for each income category. Finally, they calculate the instrument using the synthetic income distribution.

by the Gini coefficient), and three locale types (city, suburban, and rural).[17] For district i in year t, we aggregate counts of families by income bin for all districts in the same group, excluding district i. We then estimate the parameters of the Dagum distribution for each synthetic cohort and construct group-level inequality measures (e.g., the Gini and mean-to-median ratio). Since we utilize the 1970 conditions to construct the groups, and inequality is one of the group characteristics, we are only able to construct the instrument for 1980-2000, resulting in a loss of one-quarter of our sample.

These predicted values of inequality are positively related to the actual measures in that they signal what has happened nationally to income inequality in other similarly-defined districts over time. Because the instrument is calculated using other districts, and not the specific district itself, and because we control for district fixed-effects which absorb the initial characteristics used to construct the groups, it should not directly enter into the local spending equation.

4. Results

4.1. Income Inequality in School Districts

Table 2 provides descriptive statistics for income inequality in U.S. school districts over the 1970 – 2000 period. Panel A shows the distribution of 1970 – 2000 growth in inequality as measured by percent changes in the ratio of mean to median income, the Gini coefficient and Theil index for the districts in our panel. In calculating the average, districts are weighted by school enrollment.[18] Panel B shows the average level and growth of income inequality at the metropolitan area level for the same period, again weighting by school enrollment. We also decompose the MSA Theil index into its within- and between- district components, and present mean within and between shares over

[17] The six enrollment groups are as follows, with the percentage of districts from our balanced panel in 1970 reported in parentheses: ≤500 students (12.2%), 501 – 750 students (11.4%), 751>1500 students (22.0%), 1501 – 3000 students (23.9%), 3001 – 6000 students (16.5%) and >6000 students (14.1%).

[18] We use 2000 K-12 enrollment in public schools as weights. Weighting using 1970 enrollment does not substantially affect these results.

MSAs.[19]

We find that most school districts experienced growth in income inequality between 1970 and 2000. Over 90 percent of districts saw an increase in inequality as measured by the ratio of mean to median income, or 70 percent using the Gini and Theil indices. The average (median) student was enrolled in a school district that witnessed a 9.0 (8.1) percent rise in income inequality as measured by the mean to median ratio, a 15.6 (15.7) percent rise according to the Gini coefficient, and a 42.5 (39.2) percent rise as measured by the Theil index. More than one in five districts saw increases of 20 percent or more, as measured by the Gini. Taken together, the vast majority of students were in districts where income inequality rose, and a substantial fraction lived in districts where inequality increased by 25-30 percent or more.

To examine changes in within- versus between-district income inequality, we look at inequality in MSAs in Panel B. We observe a similar overall increase in income inequality in MSAs, with mean percent increases of 10.1, 17.1 and 44.3 in the mean/median ratio, Gini, and Theil, respectively. Decomposing the Theil index into its within- and between-district components, we find that—for the average MSA—89 percent of income inequality was within school districts in 2000, while only 11.3 percent was between districts. This latter fraction has increased since 1970, when an average of 7.9 percent of MSA-level inequality was between districts.[20]

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[19] School districts are mapped to MSAs based on their 2002 MSA assignment in the NCES Common Core of Data (N=329 MSAs). Thus a district *i* that was not a part of MSA *m* in 1970 but joined that MSA before 2002 would be counted as part of that MSA for the entire period. For a given MSA, our Theil decomposition is calculated as follows:

$$T = T_W + T_B = \sum_{k=1}^{m}\left[\frac{n_k}{n}\frac{\bar{y}_k}{\bar{y}}\right]T_k + \sum_{k=1}^{m}\left[\frac{n_k}{n}\frac{\bar{y}_k}{\bar{y}}\right]\ln\left[\frac{\bar{y}_k}{\bar{y}}\right]$$

where T_W is the within-school district component of income inequality in that MSA and T_B is the between-school district component. m is the number of districts within the MSA, n and n_k are total enrollment in the MSA and district k respectively. $\bar{y}_k$ is mean income in district k, while $\bar{y}$ is mean income in the MSA.

[20] In 2000 the MSA with the highest share of inequality between districts was Newark, NJ, where 31.7 percent of the overall Theil index was due to between-district inequality. As one might expect, the between-district component and the number of districts in the MSA are highly correlated, reflecting greater opportunities to

In sum, we find that the rise in income inequality observed at the national level is reflected in the income distributions of most local school districts. This finding is not axiomatic. With nearly 15,000 local districts in the U.S., sorting by income in theory could have had a strong moderating effect on inequality within local jurisdictions. Instead, we find that the average student attended a district in which income inequality rose anywhere from 9 to 42 percent, depending on the measure. A substantial fraction of students attended districts in which inequality rose 25 percent or more. Even in metropolitan areas—where the greatest opportunities for Tiebout sorting exist—we observe relatively low between-district income inequality over the full 1970 to 2000 period. There is little doubt that Tiebout sorting by income exists, and to a greater degree in MSAs with a larger number of districts. But forces other than income sorting appear to have been sufficiently important to maintain a relatively high level of heterogeneity within districts.

4.2. The Relationship between Income Inequality and Education Finance

Table 3 presents our baseline estimates of the relationship between income inequality and local education revenues per pupil, as well as models for current operating expenditures and state aid per pupil. We begin in the first column by estimating our baseline model without any measure of inequality. Then, in column (2), we add the ratio of mean to median income as our primary measure of income inequality. (Alternative measures are explored in Table 5). Column (3) estimates the analogous model for operating expenditures per pupil, and (4) the model for state aid. All models are estimated using ordinary least squares with district and state-by-year effects as described in section 3.2. Standard errors that allow for within-district correlation in errors are reported in parentheses.

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sort by income. For example, in 2000, Cleveland (75 area districts), Chicago (258), and St. Louis (113) had relatively high between-district Theil indices, while Miami, Las Vegas, and Shreveport—all comprised of only one or several districts—had low between-district inequality. Unlike Rhode and Strumpf (2003), we do find a small increase over time in the between-school district component in Boston and several other large MSAs. However—like those authors—we estimate the between-district component to be small relative to the within-district component (¼ the size or less).

Our estimated coefficients in column (1) are generally of the expected sign, and similar to those found in other empirical estimates of local demand functions for education. Per-pupil revenues increase with median family income, with a $1,000 rise in income associated with a $34 increase in local spending per pupil.[21] Revenues tend to be higher in districts with high poverty rates, higher proportions of college graduates, higher proportions of renters, and a higher elderly share. Revenues tend to be lower in more urbanized districts, and districts with larger school-aged cohorts.[22] We find no statistically significant relationship here between the racial composition of the district and local revenues. Consistent with the flypaper effect literature, local revenues are lower in districts receiving greater aid, though the reduction is not one-for-one. (We address the endogeneity of state aid in Table 4). Federal revenues per pupil have a positive relationship with local spending, as these programs typically require a local contribution.

Next, in column (2), we add income inequality to the model as the ratio of mean to median family income. Consistent with the Meltzer-Richard hypothesis, we estimate a positive and statistically significant relationship between income inequality and local revenues, suggesting rising inequality encourages greater local spending. The estimated effect is also economically significant. The average 1970 to 2000 growth in the ratio of mean to median income was 0.10, with a standard deviation of 0.11. This implies that districts with one standard deviation above-average growth in the mean/median ratio would be predicted to have $182 higher local revenues per pupil—substantial when compared against the standard deviation of local revenue growth of $1,486. To put these results in a different light, if the average 1970 – 2000 growth in the mean/median ratio generated $165 higher local revenues per pupil, it would be responsible for roughly 11 percent ($165/$1,486)

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[21] This implies an approximate income elasticity for local spending of 0.5 at the mean.

[22] See Poterba (1997) for evidence on cohort size effects on school spending, and Oates (2005) for a survey of the "renter effect" literature. The renter effect found here is sizable: we estimate that a one standard deviation decrease in the fraction of housing units that are owner-occupied (about 0.146) is associated with a $166 increase in per-student revenues, almost 1/10 of a standard deviation in local revenues per student.

of the overall growth in local expenditure per student over this period.

Because education is a shared responsibility of local and state governments, this increase in local taxes for public schools may not translate into greater spending per student, if these revenues are offset by a similar reduction in state aid. Column (3) looks at the net effect of income inequality on current operating expenditures per student. All of the same covariates are included as in column (2), but with state and federal aid excluded. We also control directly for the effects of school finance reforms at each quartile of the district income distribution. We find that, on net, rising income inequality was associated with higher per student spending, though the magnitude is lower than that found in (2). The reason for the attenuation becomes clear in column (4): while local revenues rose with income inequality, state aid per student is inversely related to inequality. This could be due in part to the "mechanical" relationship between district wealth and state aid, mentioned in section 3.2. On the other hand, this result suggests that the effects of income inequality found in (1) are not a reflection of more generous matching grants in districts with growing inequality.

In Table 4 we address two shortcomings of our OLS models, the endogeneity of state aid and the potential influence of inter-district sorting on our estimate of the coefficient on income inequality. In column (2), we present two-stage least squares (2SLS) estimates where the level of real state aid per student is instrumented using interactions between a dummy variable equal to one in years following a court-ordered finance reform and the district's 1970 position in its state income distribution. Because our model contains district fixed effects as well as state-year interactions, only three of the income quartile interactions are identified. The first-stage estimates for this model are reported in column (1) of Appendix Table 1. Consistent with Evans, Murray, and Schwab (1997), the first stage demonstrates that court-ordered finance reform increased state aid the most among the poorest districts. Our results indicate that after reform, state revenues per pupil increased by \$1,365, \$486, and \$403 in the lowest three income quartiles, relative to what happened in the top income quartile group. All of these results are statistically significant at conventional levels, and the first-stage *F*-test that the excluded instruments are all zero is 54, indicating that finite sample bias is not a concern (Bound, Jaeger, and Baker, 1995). Note that given an instrument many others have argued is exogenous and a strong first stage fit, the 2SLS estimates in column (2) differ little from the OLS estimates repeated in column (1), save for a lower local elasticity to state aid (a larger "flypaper effect") and a larger standard error, which one expects from 2SLS models. Note as well that the *p*-value on the test of over-identifying restrictions is relatively large meaning that we cannot reject the null hypothesis that the model is correctly specified.

A second concern with our OLS model is the possibility that households sort into or out of districts in response to changes in school spending (and in particular, local taxes). To the extent this mobility varies by income, such sorting will affect the income distribution. To illustrate, suppose an initially heterogeneous district witnesses a rise in inequality through growth at the top of the income distribution. The median voter responds to this change in tax price through greater local spending (our observed correlation between inequality and local revenue). High-income households, however, respond over time to the rise in taxes by moving to another district, which reduces income inequality in the sending district. Such a response would tend to bias our estimate of the coefficient on income inequality downward. This example also generates a prediction that the effect of income inequality on local spending should be greatest in districts with the least opportunity for sorting, a point we return to in the next section.

Column (3) presents 2SLS estimates using the skewness of family income as an instrument for the ratio of mean to median income. The first stage estimates for these three models are reported in columns (2) - (3) of Appendix Table 1. In this model, we have two endogenous variables (real state revenues per pupil and income inequality), so we make use of both the finance reform/income quartile and inequality instruments. In the appendix table, where the first stage dependent variable is real state revenues per student, the three reform instruments generate estimates nearly identical to the baseline in column (1). In column (3), the dependent variable is the within-district mean/median income ratio. As expected, income skewness is positively correlated with the mean/median ratio, and the *t*-statistic on this instrument is almost 19. The *F*-statistics for the null hypothesis that the coefficients on the identifying instruments in the first stage are all zero are of a size so as to make finite sample bias not a concern.

The resulting 2SLS estimate in column (3) of Table 4 is nearly twice as large as that found in columns (1) and (2), and is statistically significant. This may confirm the hypothesis that the OLS estimate is downwardly biased. We cannot, however, reject the null that the OLS estimate in column (1) and the 2SLS estimate in column (3) are the same. For districts one standard deviation above the mean in income inequality, we estimate that local revenues per student are almost $321 higher, on average, or 22% of growth in local spending per student over this period. As with the results in column (2), the *p*-value on the test of over-identifying restrictions is rather large, especially given the precision of the first stage models.

In Column (4) of Table 4, we show 2SLS estimates using our alternative instrument, the mean-to-median ratio of family income in a synthetic cohort of districts of similar size, locale, and initial income inequality. As noted, the sample size drops by one quarter due to the loss of 1970 cases. First stage estimates are presented in columns (4)-(5) of Appendix Table 1, and as with previous models, finite sample bias is not a concern. The coefficient estimate on income inequality continues to be large, positive, and statistically significant, and is larger than that found in column (3). Thus, to the extent the skewness instrument fails the exclusion restriction, our alternative instrument provides supporting evidence of a positive impact of inequality on local spending.[23]

In Table 5, we experiment with alternative measures of within-district income inequality: the

[23] The larger magnitude of our estimate in column (4) is due in small part to the larger mean local revenues per student over the 1980-2000, as against 1970-2000, period.

Gini coefficient, Theil index, natural logarithm of the ratio of 95th to 5th percentiles of income, and a measure that divides overall income inequality into two components—inequality in the top half of the distribution (the log 95/50 ratio) and inequality in the bottom half of the distribution (the log 50/5 ratio). Estimates in columns (2) and (4) are 2SLS estimates of the effects of income inequality on local revenues using the skewness of income as an instrument for inequality.

As in Table 3, all estimated coefficients on income inequality have positive signs, excepting the log 50/5 ratio, which has a negative relationship with spending. All are statistically significant, with the exception of the log 95/5 ratio. Our point estimate based on the Gini coefficient finds an effect comparable to that found using the mean to median ratio in Table 3: districts where growth in income inequality was one standard deviation higher than average (5.0) spent about $140 more per student on average (or $675 based on the 2SLS estimate).

The pattern of estimated coefficients in column (6)—where income inequality is split into two components—is consistent with the predictions of the Meltzer-Richard model. Changes in income inequality that increase mean income relative to the median should lower the tax price of the median voter (promoting higher spending), while changes that decrease mean income relative to the median should increase the tax price (promoting lower spending). We find exactly this pattern here. As the 95th percentile of income rises relative to the median within a district, we observe increases in per-pupil spending on average. As the 5th percentile of income falls relative to the median, we observe decreases. Given the standard deviation of growth in these variables of .15 and .28, respectively, a one standard deviation higher growth in the log 95/50 ratio is associated with $117 higher spending per pupil, and one standard deviation higher growth in the log 50/5 ratio is associated with $85 lower spending per pupil.[24]

[24] Of course, a one percentage point rise in the log 95/50 ratio will not have an equivalent effect on the tax price as a percentage point fall in the log 50/5 ratio. Because households at the top of the income distribution

4.3. Extensions and Robustness Checks

As noted in section 3.2, there remain several threats to our strategy for identifying the effects of income inequality on local education spending. The first is the potentially important omission of non-residential property wealth. If access to such wealth is correlated with changes in inequality, our coefficients may be biased. We partially address this issue through the use of district fixed effects, which should capture the influence of fixed differences in this revenue source. However, it is possible that the taxable value of these sources vary over time in ways that are correlated with income inequality. Second, equalizing state aid formulas, such as foundation programs, can generate "mechanical" relationships between local wealth and state aid. The effects of rising property wealth from growing income at the top of the distribution may be partially or completely offset by reductions to state aid. The textbook case is California: since its *Serrano* ruling, the legislature has fixed the level of school expenditure through "revenue limits," to which the state and districts contribute. As a result of the property tax limitation set by Proposition 13 (1978), districts collect property taxes at a fixed rate of 1 percent, while the state fills the remaining gap between property taxes and the revenue limit.[25]

Because consistent data on residential and non-residential property wealth is unavailable for all school districts in our panel, and the modeling of state aid formulas for each state and year is prohibitive, we conducted a set of sensitivity tests to assess the robustness of our results to alternative samples and specifications. For example, to the extent non-residential property wealth is

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earn a disproportionate share of aggregate income, an increase in the log 95/50 ratio will do more to increase mean income than an equivalent decline in the log 50/5 ratio.

[25] The system is actually more complex than this, and there is more spending inequality in California than is commonly believed. This is due in part to a local override option, which allows districts to exceed the revenue limit, and the practice of raising funds through private educational foundations (see Brunner and Sonstelie, 2006). Still, the vast majority of spending on public education is dictated by the revenue limits.

an important omitted variable in our model, it should be less important outside of cities.[26] In Table 6 we report the results of our models estimated separately for districts in six different locale types, including city, suburban, and rural districts. Similarly, high-income and high-wealth districts are the least likely to be impacted by foundation aid programs, which target greater aid to low-wealth districts. We again estimated our model separately for districts according to their within-state quartile of median income or median owner-occupied housing value in 1970 (from the Census). Finally, we estimated our model for the full panel excluding California districts.

Table 6 presents the results of our subsample analyses. Each cell of this table represents a coefficient estimate for inequality from a separate regression. For the results by locale type (Panel A) we show OLS and two sets of 2SLS estimates, analogous to those in Table 4. Results for the full panel are repeated in first row. For the income and housing value subsamples (Panel B), we can only produce OLS estimates, as our instrument for state aid relies on variation across income quartiles.

We find a positive relationship between income inequality and local revenues per pupil in all locale subsamples with the results being statistically significant in all versions except the OLS models for midsize cities and urban fringe of midsize cities (*p*-value of 0.11). Though the coefficients vary in magnitude, the effect is positive in city, suburban, town, and rural districts. In general, the effect is largest in city districts (suggesting that omitted property wealth related to inequality may be a greater concern in these districts), but there is a robust positive relationship between inequality and local spending in rural and suburban districts as well. The results are also not sensitive to whether California is excluded from the sample.

The coefficient on income inequality is also positive in the OLS models where we break the

[26] We collected detailed property wealth by school district for six states in 1992 and 2002 (Connecticut, Georgia, Iowa, Massachusetts, New York, and Ohio) and found that residential property represented the large majority of taxable district property wealth in these states. Across districts in these states, commercial and industrial property represented 27-36 percent of taxable property in cities, but only 17-19 percent in suburbs, and 9-10 percent in rural areas. Notably, the residential composition of property wealth changed little in the averaged district between 1992 and 2002.

samples by income and house value quartiles. It is positive for initially low, middle, and high-income districts, as well as those with initially low, middle and high median housing values. We find larger coefficients on inequality in districts with the highest initial income and median housing values, districts in which state aid represents a lower share of overall revenues. This provides assurance that our overall finding is not primarily driven by mechanical relationships between inequality, local revenues, and state aid (through foundation-type formulas, for example).

We have also conducted a number of additional sensitivity checks not shown here.[27] For example, we interacted our income inequality measure with an indicator of the extent of between-district sorting opportunities: quartiles in the number of districts per student within a 25-mile radius (Hoxby, 2000). Districts in the lowest quartile tend to be in states with a relatively small number of geographically large districts (such as the South and West), while those in the highest quartile tend to be in states with many community-based districts (such as the Northeast and Midwest). We find that the largest coefficient on income inequality is indeed among districts with the fewest sorting opportunities. The effect for districts in all other quartiles is substantially smaller, and these differences, compared to the lowest quartile, are statistically significant at conventional levels.[28]

Additionally, we re-estimated our baseline models using local revenues per *school aged child*, rather than per public school pupil. This allows for the possibility that increases in spending associated with inequality are a reflection of movement into private schools which mechanically reduces public enrollment and inflates public spending per student, if resources are slow to adjust.[29]

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[27] All of these are available from the authors upon request.

[28] For districts in the lowest quartile of districts per student (i.e., those with the lowest sorting opportunities), the estimated coefficient on the ratio of mean to median income was 2,117 ($p<0.001$), about 28 percent greater than the pooled OLS estimate in Table 3. Districts in other quartiles had markedly lower coefficients, although they did not decline monotonically with greater district availability. The estimated difference between the 1st and other quartiles were 2,147, 1,889, and 1,376, for the 2nd, 3rd, and 4th quartiles, respectively. All were statistically distinguishable from the coefficient for the 1st quartile ($p<0.001$).

[29] de la Croix and Doepke (2009) build a general equilibrium model of school finance with endogenous fertility and sector choice, and in this setting, the authors found that increased inequality leads to a flight from

We find a point estimate only marginally smaller than that found in our original model. In addition, we have estimated models that (1) restrict the sample to unified (K-12) districts, (2) do not use enrollment weights (an alternative way of limiting the influence of large districts on the results), (3) use a measure of *household* income inequality as opposed to inequality in family income (which precludes the use of 1970), and (4) restrict the panel to 1980 – 2000 (which recaptures 2,600 districts). The results of these models are qualitatively similar to those found in our baseline case.

4.4. The Relationship between Income Inequality and Private School Enrollment

In our look at the impact of rising income inequality on public school expenditure, we found little evidence favoring the "ends against the middle" hypothesis in which rich and poor households limit the growth of educational spending. Rather, we find results more consistent with a median voter model in which a lower tax price stimulates higher public spending. However, it may still be the case that growing income inequality affects support for public education in other ways—in particular, through enrollment in private schools. For example, de la Croix and Doepke (2009) develop and test a model of education spending with endogenous fertility decisions when private schools are available. In their model, the authors find that higher inequality leads to greater demand for private schooling, especially in the case where the public option is of low quality.

In Table 7 we use our school district panel to look directly the relationship between within-district income inequality and enrollment in private school. Our empirical model for the percent of school-aged children enrolled in private school ($private_{ijt}$) roughly mirrors that presented earlier in equation (1):

(3) $$private_{ijt} = \boldsymbol{X_{ijt}}\beta + inequality_{ijt}{*}\gamma + EXP_{ijt}{*}\theta + \delta_i + \delta_{jt} + u_{ijt}$$

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the public to private sector. In a median voter setting, however, the authors find that the drop in spending is lower that the fall in public school attendance, leading to higher spending per pupil in the public sector.

where $\boldsymbol{X}_{ijt}$ and $inequality_{ijt}$ are defined as in section 3.2 and EXP_{ijt} is the level of current operating expenditures for public K-12 education in school district *i* in year *t*. As before, we include school district (δ_i) and state-by-year fixed effects (δ_{jt}) to capture fixed differences across districts in private schooling rates and temporal changes at the state level.[30] Of course, it is likely that public expenditure is responsive to private schooling rates, so in column (2) we instrument for current operating expenditure per student using our school finance reform/income quartile interactions introduced above, relying solely on variation in expenditure that occurs through exogenous changes in school funding formulas. In Table 8, we present OLS and 2SLS results using two measures of income inequality: the mean to median ratio of income, and the Gini coefficient.

Private schooling is found to rise on average with median family income, and fall with district poverty, the percent nonwhite, and the size of the school-aged population. We find a positive but statistically insignificant relationship between racial fractionalization and private school enrollment. The results are less clear for the role of inequality. In columns (1) and (3) we estimate OLS models using the ratio of mean to median income and the Gini coefficient, respectively, as the measure of inequality. The coefficients on inequality in these two models switch signs and are statistically insignificant.

Our estimated positive coefficient on expenditures per student in columns (1) and (3) is somewhat counter-intuitive if we believe expenditure is linked to public school quality (admittedly a tenuous assumption). However, expenditures may in part be a response to past school performance, where poor or under-performing school districts receive targeted spending designed to improve outcomes. These districts also may be more likely to have high rates of private schooling. As might be expected, this point estimate changes sign in our 2SLS models (columns (2) and (4) – (6)), where our instrument makes use of exogenous variation in expenditure due to school finance reforms.

[30] In a regression of the proportion of K-12 enrollment in private school on district fixed effects and year dummies alone, the R^2 is 0.85 (adjusted $R^2 = 0.80$).

Here, districts with higher spending on average have lower rates of private schooling.

In our 2SLS estimates, both point estimates on the inequality coefficients are now positive, and much larger in size, but only the coefficient on the Gini is statistically significant. The results are of modest qualitative significance. A 10-point increase in the Gini, which is a two standard deviation change in the Gini over the 1970 to 2000 at the district level, leads to a 0.4 percentage point increase in private school enrollment. Therefore, large changes in inequality lead to modest movements in the private school enrollment rate.

In columns (5) and (6) we interact our income inequality measure with the quartile measures of district sorting opportunities (as measured by districts per student within a 25-mile radius). One would expect the relationship between inequality and private schooling to be moderated in districts were greater sorting opportunities exist. In the case of the Gini coefficient (column (6)), we do find that the point estimate on income inequality is largest among districts in the lowest quartile of district choice (0.0843), but the difference is only statistically significant between the 1st and 2nd quartile. A similar pattern exists for the mean to median measure.

5. Conclusion

As income inequality has risen in the U.S. and its population has grown more racially and ethnically diverse, scholars have begun to examine whether this growing heterogeneity will alter the extent to which governments provide basic public services and a social safety net. Recent theoretical and empirical work suggests that public goods provision and the generosity of welfare benefits are lower in more racially and ethnically diverse jurisdictions. Models specific to public education suggest a similar outcome. With respect to income, growing income inequality may encourage a battle of the "ends against the middle," where high income families opt out of public schooling into the private sector and lower income groups choose lower taxes and greater private consumption over investments in public education. As a result, forces at the ends of the income distribution may reduce support for public schools in economically diverse populations.

In contrast, growing income inequality may have unanticipated effects on the provision of local public goods. In a simple voting model like that of Meltzer and Richard, growing wage inequality at the top of the distribution reduces the tax price of public goods to the median voter, thereby encouraging greater spending on government services. We examined the impact of growing income inequality on local support for public schools using panel data for over 10,000 school districts over three decades. In contrast to other studies, our results suggest that the median voter model is a more accurate description of the experience in this governmental sector. As income inequality has grown in local school districts, so too have local dollars into elementary and secondary education. Our results indicate that 11 percent of the growth in local per-student revenues over the past 30 years can be explained by a decline in the tax share facing the median voter, a consequence of rising income inequality primarily concentrated in top of the distribution.

The Meltzer-Richard hypothesis has been extensively tested in the past, in a number of settings, with conflicting results. The strength of the results in this paper may be driven by a number of factors. First, much of the previous work has used national or state level data while our analysis focuses on local school districts. Common wisdom in the public finance literature suggests that the collective choice process in local government is much more likely to approximate the assumptions of the median voter model assumptions (Fischel 2001; Mueller 2003). Second, we examine changes in spending in more than 10,000 school districts during a period of rapidly changing income inequality, giving us tremendous statistical power. Third, the panel nature of our data allows us to limit our exposure to potentially contaminating omitted variables bias.

Given the important redistributive nature of education, our results suggest that some of the potentially negative consequences of rising social inequality may have been counteracted by local government's ability to raise additional funds from growing incomes at the top of the distribution. The long term benefits of such a transfer are beyond the scope of this paper, but are obviously an important topic for future discussion.

Data Appendix

A.1. Sources – District Panel

Our balanced panel of school districts consists of matched demographic and financial data on U.S. school districts from 1970 to 2000. We constructed this panel by merging eight large national databases: the *Census of Population and Housing* school district tabulations for 1970, 1980, 1990, and 2000 (U.S. Census Bureau 1973, 1982, 2002a; U.S. Department of Education 1994), the *Census of Governments: School Districts* for 1972, 1982, and 1992 (U.S. Census Bureau 1987, 1992, 1993), and the F-33 *Annual Survey of School Finances* for 2002 (U.S. Census Bureau 2002b).

These eight databases were supplemented by a number of others. First, because the 2000 Census school district tabulation (U.S. Census Bureau 2002a) failed to include a table for public and private school enrollment, we used Census tract level data to compute private enrollment in every school district. This procedure required overlaying boundary files for census tracts with those for unified and elementary school districts, and aggregating enrollment counts to the district level.[34] While tracts are almost always smaller than school districts, they are not necessarily contained entirely within the boundaries of one district. In cases where tracts crossed district boundaries, we allocated public and private enrollment to districts based on the fraction of tract land area in each district. Of course, this method only works well when enrollment is uniformly distributed over the tract—a less plausible assumption in rural, suburban or geographically diverse areas. In densely populated urban areas, tracts are usually contained in only one district. Thus, measurement error in our private school variable is likely to be highest in districts with the smallest populations. The use of enrollment weights in our analysis should ameliorate at least some of this error.

Second, all of our court rulings on state school finance systems are taken from Corcoran and Evans (2008), which updates Murray, Evans, and Schwab (1998) and others. Only rulings from the highest state court on the constitutionality of school funding are included. Finally, all school district consolidations, splits, and unifications (a merger of separate elementary and secondary districts) between 1970 and 2002 were researched individually, using dozens of sources, including official state documents, news accounts, minutes from school board meetings, and school district websites. All of school district observations are based on their 2002 geographic definitions, such that if District A and District B merged or unified in 1995, we have combined the data from these two districts in all earlier years, for comparability with 2002. Data was combined by aggregating across districts, or by taking a weighted average of component district characteristics as appropriate. Districts involved in splits have been dropped from the panel, although there were very few of these cases.

A.2. Inequality Measures

As described in section 3, we fit the 3-parameter Dagum (or Burr Type III) distribution to Census grouped income data in local school districts to calculate measures of income inequality. For

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[34] Boundary files can be downloaded from http://www.census.gov/geo/www/cob/ bdy_files.html (last accessed August 9, 2003). The 2000 School District Tabulation does include one table (PCT23) that reports school enrollment for males and females age three and older, by various age categories: 3-4, 5-9, 10-14, 15-17, 18-19, etc., with no public-private distinction. Together with K-12 public enrollment counts from the *Common Core of Data*, it would be possible to use the residual (census enrollment – CCD public enrollment) as an estimate of the number of children enrolled in private school. This would, however, require the use of two different data sources in the construction of one variable, as well as critical assumptions about the fraction of five-year olds in kindergarten and the fraction of 15-17 and 18-19 year olds in secondary school.

a random variable z, the cumulative distribution function for the three-parameter Dagum distribution is as follows, for $z \geq 0$ and $(a, b, p) > 0$:

$$(4) \qquad F(z) = \left[1 + \left(\frac{b}{z} \right)^{a} \right]^{-p}$$

Given the parameters *a, b,* and *p,* the r^{th} moments of this distribution are defined as:

$$(5) \qquad E[z^{r}] = pb^{r} \beta\left(1 - \left(\frac{r}{a} \right), p + \left(\frac{r}{a} \right) \right),$$

where $\beta(.)$ is the complete beta function. The *c*th percentile of the income distribution is found using:

$$(6) \qquad x_{c} = b\left[c^{-(1/p)} - 1 \right]^{-(1/a)}.$$

Dagum (1980) showed that the Gini coefficient can be calculated directly as:

$$(7) \qquad Gini = -1 + \frac{\beta(p, p)}{\beta(p, p + (1/a))},$$

and the Theil index (or generalized entropy 1) is calculated as (McDonald, 1984):

$$(8) \qquad Theil = \frac{\psi\left(p + \frac{1}{a} \right)}{a} - \frac{\psi\left(1 - \frac{1}{a} \right)}{a} - \Gamma\left(p + \frac{1}{a} \right) - \Gamma\left(1 - \frac{1}{a} \right) + \Gamma(p) + 1,$$

where $\psi(z)$ is the digamma function $\psi = \frac{\Gamma'(z)}{\Gamma(z)}$.

In addition to its high level of accuracy at the state level (Evans, Hout, and Mayer 2004), we were also interest in seeing how this procedure would perform in smaller geographic areas. Using the same maximum likelihood procedure, we estimated county-specific parameters of the Dagum distribution for 1970, 1980 and 1990. While the Census does not report Gini coefficients at the county level, they do report several other aggregate measures of the income distribution. For example, in 1990 the Census reports the fraction of families in each county earning $50,000 or more. We compared these fractions to the same fraction calculated with our estimated Dagum parameters (i.e. 1 – F(50,000; *a,b,p*)). Again, the correlation between these values is high: 0.996 for 1990. Analagously, we calculated average family income in each county using the moment generating function for the Dagum distribution, and compared these to the average family income reported by the Census. The correlation between the actual and predicted values in this case was 0.997. On the whole, it appears our maximum likelihood procedure performs remarkably well.

Finally, we were concerned that changes in the number of income categories reported over time in the Census might affect our estimates of income inequality. To test for this possibility, we collapsed the 16 income groups reported in the 2000 Census to 8, and re-estimated the Dagum parameters and Gini coefficients. In a regression of *Gini_16* (the Gini coefficient estimated using 16 income groups) on *Gini_8* (the Gini estimated using 8 groups), we estimate an intercept of 0.01 and a slope of 0.96, but cannot reject the null hypotheses that the intercept is zero and the slope is one. This implies that there is no systematic bias in using a smaller number of income groups (a smaller number of groups creates classical measurement error).

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Table 1: Descriptive Statistics, Panel of U.S. School Districts 1970 – 2000

	Means					Full sample	
	1970/72	1980/82	1990/92	2000/02	70-00 Change	Mean	Standard Deviation
Real local revenues per student	2385	2400	3323	3776	1486	2985	1957
Real state and federal aid per student	1824	2608	3665	4909	3077	3269	1798
Real current expenditures per pupil	3476	4424	6166	7494	3841	5501	2216
Percent of revenues from local sources	54.5	45.9	45.2	42.3	-11.0	47.0	19.3
Percent of enrollment in private school	10.5	8.8	10.2	10.4	1.1	10.0	6.8
Real median family income (x1000)	47.3	49.8	51.7	55.6	8.7	51.1	15.5
Percent of households below poverty	13.7	12.4	13.5	12.4	-1.3	13.0	8.0
Mean to median ratio of family income	1.13	1.14	1.19	1.23	0.10	1.17	0.11
Gini coefficient of family income (x 100)	33.8	35.1	37.3	38.8	5.0	36.3	5.5
Theil index of family income (x 100)	20.1	21.7	25.1	28.0	7.8	23.8	9.3
Log(95/5) ratio in family income	2.37	2.48	2.64	2.67	0.29	2.54	0.45
Ln(95/50) ratio in family income	0.88	0.91	0.99	1.06	0.18	0.96	0.18
Ln(50/5) ratio in family income	1.49	1.57	1.65	1.61	0.11	1.58	0.31
Mean to median housing value	1.13	--	1.15	1.16	0.03	1.15	0.12
Gini coefficient of housing value	28.0	--	28.7	28.3	0.02	28.3	7.2
Percent college graduates or higher	10.6	13.8	21.0	23.7	13.0	17.3	11.2
Percent school aged (5 – 17)	26.7	34.9	26.4	19.3	-7.6	26.5	6.7
Percent aged 65 and older	9.5	11.4	12.2	12.1	2.7	11.3	4.3
Percent of homes owner occupied	65.9	66.9	65.7	67.3	0.3	66.4	14.6
Percent of population nonwhite	15.7	19.8	23.9	30.9	15.3	22.7	22.2
Percent of population in urbanized areas	71.3	57.7	72.8	78.3	11.0	70.4	37.4
Index of race fractionalization (x 100)	16.5	22.1	26.2	32.9	17.5	24.5	19.9

Notes: authors' calculations using a balanced panel of elementary and unified school districts (N=10,359 in each year). District observations are weighted by public K-12 enrollment. Districts in Alaska, Hawaii, and D.C. are excluded. All monetary values measured in 2002 dollars.

Table 2: Income Inequality in School Districts and Metropolitan Areas – 1970 to 2000

A. Change in income inequality within school districts, 1970 to 2000 (weighted by district size)

Percent change in income inequality:	Mean/ Median Ratio	Gini Coefficient	Theil Index
Mean	8.96	15.60	42.50
10th percentile	0.42	-2.30	-4.20
25th percentile	4.22	6.25	15.86
Median	8.09	15.69	39.17
75th percentile	12.23	24.93	63.77
90th percentile	18.95	32.66	89.03
Mean income inequality in 1970	1.13	33.8	20.1
Mean change in income inequality, 1970-2000	0.10	5.0	7.8
Standard deviation of change in inequality, 1970-2000	0.11	4.6	10.0

B. Income inequality within metropolitan areas (weighted by MSA enrollment)

	1970	1980	1990	2000	% Change
Mean/median ratio	1.14	1.15	1.20	1.25	10.1
Gini coefficient (x 100)	34.7	36.1	38.5	40.6	17.1
Theil index (x 100)	20.8	22.6	26.2	30.1	44.3
% within school districts	92.1	92.1	89.1	88.7	-3.7
% between school districts	7.9	7.9	10.9	11.3	43.6
Mean number of districts	16.3	18.8	18.6	18.9	

Source: authors' calculations using a balanced panel of elementary and unified school districts (N=10,358 in each year; 331 MSAs in Panel B, except 1990 where 328 MSAs are included). Notes: Panel A uses same sample selection criteria as Table 1; Panel B makes use of the full (unbalanced) panel of school districts.

Table 3: OLS Estimates of Real Local Revenues per Pupil, Real Expenditures per Pupil, and Real State Revenues per Pupil

Dependent Variable:	Local revenues per pupil		Expenditures per pupil	State revenues per pupil
	(1)	(2)	(3)	(4)
Real median family income, in thousands	34.4***	38.8***	-8.1	-46.0***
	(6.18)	(5.55)	(4.39)	(4.64)
Ratio of mean to median family income		1652.0**	1132.3*	-660.4*
		(578.5)	(354.5)	(295.3)
Percent of population below poverty line	17.5***	8.58*	8.8*	16.2***
	(4.76)	(4.23)	(3.7)	(4.51)
Real state revenues per pupil	-0.441***	-0.434***		
	(0.032)	(0.031)		
Real federal revenues per pupil	0.318**	0.299*		
	(0.143)	(0.148)		
Percent college graduates or higher	44.7***	35.7***	19.4***	-23.3***
	(7.3)	(6.11)	(4.99)	(4.46)
Percent school aged (5-17)	-57.5***	-58.9***	-62.2***	0.498
	(5.4)	(5.36)	(4.92)	(4.92)
Percent aged 65 and older	17.9*	17.38*	-25.0***	-68.7***
	(7.8)	(7.66)	(7.29)	(7.85)
Percent of housing units owner-occupied	-11.4*	-12.8**	2.74	17.9***
	(5.4)	(5.55)	(3.89)	(3.14)
Percent nonwhite	-2.99	-4.10	9.71***	1.39
	(2.74)	(2.69)	(2.23)	(3.04)
Index of race fractionalization	194.0	410.6	-514.8	-215.1
	(373.1)	(362.7)	(317.1)	(306.7)
Percent living in urbanized area	-2.30***	-1.83***	-1.62***	0.659
	(0.46)	(0.44)	(0.43)	(0.362)
Observations	41,436	41,422	41,422	41,422
R-squared	0.911	0.912	0.948	0.890

Notes: Standard errors that allow for within district correlation in errors in parentheses (*** p<0.001, ** p<0.01, * p<0.05). All models include school district fixed effects plus state x year fixed effects. In models (3) and (4), we also include interactions between the within-state quartiles of median family income in 1970 times a dummy for whether the state has a court-ordered education finance reform in effect that year. (The estimated coefficients on these variables are all positive, statistically significant, and smaller in magnitude for higher income quartiles).

Table 4: OLS and 2SLS Estimates of Real Local Revenues per Pupil Equation

	(1)	(2)	(3)	(4)
Real median family income, in thousands	38.8***	41.9***	46.2***	45.1***
	(5.55)	(5.28)	(6.33)	(6.06)
Ratio of mean to median family income	1652.0**	1704.3***	3205.1***	6091.1***
	(578.5)	(509.5)	(675.4)	(1063.9)
Percent of population below poverty line	8.58*	7.75	-0.448	-21.0**
	(4.23)	(4.03)	(4.82)	(7.90)
Real state revenues per pupil	-0.434***	-0.365***	-0.354***	-0.363***
	(0.031)	(0.067)	(0.067)	(0.084)
Real federal revenues per pupil	0.299*	0.274*	0.256*	0.195**
	(0.148)	(0.130)	(0.133)	(0.078)
Percent college graduates or higher	35.7***	37.7***	29.6***	19.5**
	(6.11)	(5.67)	(6.92)	(6.62)
Percent school aged (5-17)	-58.9***	-58.9***	-60.1***	-39.3***
	(5.36)	(4.65)	(4.63)	(5.02)
Percent aged 65 and older	17.38*	22.8**	22.7**	5.20
	(7.66)	(7.52)	(7.57)	(8.85)
Percent of housing units owner-occupied	-12.8**	-14.5**	-16.0**	-8.19*
	(5.55)	(4.98)	(5.43)	(4.33)
Percent nonwhite	-4.10	-3.86	-4.82*	-3.60
	(2.69)	(2.40)	(2.35)	(3.76)
Index of race fractionalization	410.6	397.2	593.3	783.0**
	(362.7)	(319.5)	(324.8)	(340.3)
Percent living in urbanized area	-1.83***	-1.90***	-1.48***	-0.113
	(0.44)	(0.39)	(0.39)	(0.464)
Instrument for:				
State revenues per pupil	No	Yes	Yes	Yes
Mean/median income ratio	No	No	Yes	Yes
1st stage F-test: Instruments=0				
State revenues per pupil		53.8	42.5	26.9
Mean/median income ratio			84.9	13.2
P-value, test of over-identifying restrictions (DOF of test)		0.328	0.234	0.931
		(2)	(2)	(2)
Observations	41,422	41,422	41,422	31,063
R-squared	0.912			

Notes: Standard errors that allow for within district correlation in errors in parentheses. (*** p<0.001, ** p<0.01, * p<0.05). All models include school district fixed-effects plus state x year fixed-effects. In models (2)-(4), the instruments for real state revenues are interactions between the within-state quartiles (2nd through 4th) in median family income times a dummy for whether the state has a court-ordered education finance reform in effect that year. In model (3) the instrument for the mean/median ratio is family income is the skewness in within-district family income. In model (4), the instrument for inequality is the synthetic group-based inequality measure described in section 3.2.

Table 5: OLS and 2SLS Estimates of Real Local Revenues per Pupil Equation with Alternative Measures of Income Inequality

	(1)	(2)	(3)	(4)	(5)	(6)
Income inequality measure:	Gini	Gini	Theil	Theil	Ln(95/5)	Ln(95/50) and ln(50/5)
Income inequality	28.0*	135.0***	16.1*	36.3***	-68.6	
	(11.9)	(25.4)	(7.07)	(8.16)	(122.5)	
Ln(95/50) ratio of family income						781.3**
						(287.2)
Ln(50/5) ratio of family income						-302.4**
						(109.7)
Real median family income, in thousands	36.7***	49.7***	36.8***	42.8***	34.2***	37.9***
	(5.71)	(6.80)	(5.67)	(5.99)	(6.01)	(5.62)
Instrument for:						
State revenues per pupil	No	Yes	No	Yes	No	No
Income inequality measure	No	Yes	No	Yes	No	No
1st stage F-test: Instruments=0						
State revenues per pupil		42.6		42.5		
Income inequality measure		69.9		62.7		
P-value, test of over-identifying restrictions (DOF of test)		0.036		0.181		
		(2)		(2)		
Observations	41,423	41,422	41,423	41,422	41,423	41,423
R-squared	0.911		0.911		0.911	0.911

Notes: Standard errors that allow for within district correlation in errors in parentheses (*** p<0.001, ** p<0.01, * p<0.05). All models include school district fixed-effects, state x year fixed-effects, plus the demographic covariates used in Table 3. In models (2) and (4), the instruments for real state revenues are interactions between the within-state quartiles (2nd through 4th) in median family income times a dummy for whether the state has a court-ordered education finance reform in effect that year. The instrument for the inequality measures is the skewness in within-district family income.

Table 6: OLS and 2SLS Estimates of Real Local Revenues per Pupil Equation:
Coefficient Estimates on Income Inequality from Selected Subsamples

	(1)	(2)	(3)		(4)
A. Locale subsamples:	OLS	2SLS (state aid)	2SLS (state aid and inequality)	B. Income and housing value subsamples:	OLS
All districts	1652.0**	1704.3***	3205.1***	Quartile 1 of	883.6**
	(578.5)	(509.5)	(675.4)	1970 income	(314.4)
Large city	3301.5*	2940.1*	5436.2*	Quartile 2 of	744.6*
	(1651.4)	(1222.7)	(2185.2)	1970 income	(324.1)
Midsize city	1910.2	1885.9*	5187.1*	Quartile 3 of	2212.5***
	(1005.3)	(844.0)	(1809.3)	1970 income	(623.7)
Urban fringe of	1323.1**	1255.4**	2402.2**	Quartile 4 of	2118.5*
large city	(447.2)	(389.5)	(697.2)	1970 income	(841.0)
Urban fringe of	783.1	783.3	3716.9*	Quartile 1 of	268.1
midsize city	(488.0)	(412.2)	(1622.5)	1970 housing	(142.3)
Large or small town	1084.3***	1063.1***	3672.1***	Quartile 2 of	638.12*
	(239.6)	(211.7)	(767.9)	1970 housing	(292.0)
Rural	416.6*	435.9**	1916.9*	Quartile 3 of	1134.2***
	(173.4)	(157.8)	(911.0)	1970 housing	(315.8)
All districts	1460.7*	1480.7*	3099.3***	Quartile 4 of	2605.9***
excluding California	(661.3)	(583,8)	(766.3)	1970 housing	(757.2)

Notes: Standard errors that allow for within district correlation in errors in parentheses. Each cell represents a coefficient estimate from a separate regression. Sample sizes for the locale subsample panels are: 560 (large city), 1760 (midsize city), 8460 (urban fringe of large city), 3596 (urban fringe of midsize city), 7530 (large or small town), and 19516 (rural). Sample sizes for the income and housing quartile subsamples are approximately 10,300 each.

Table 7: Effect of Income Inequality on Private Schooling

Inequality measure:	Mean/ Median (1)	Mean/ Median (2)	Gini (3)	Gini (4)	Mean/ Median (5)	Gini (6)
Real median family income, in thousands	0.120*** (0.014)	0.108*** (0.015)	0.121*** (0.014)	0.109*** (0.015)	0.106*** (0.014)	0.102*** (0.013)
Inequality measure	-0.261 (0.651)	1.243 (0.804)	0.007 (0.019)	0.043* (0.022)	2.215* (1.109)	0.083** (0.028)
Inequality measure x 2nd quartile of districts/student					-4.215* (1.713)	-0.148** (0.045)
Inequality measure x 3rd quartile of districts/student					2.580 (1.589)	-0.008 (0.037)
Inequality measure x 4th quartile of districts/student					1.434 (1.311)	0.011 (0.032)
Percent of population below poverty line	-0.053** (0.017)	-0.046** (0.016)	-0.057*** (0.017)	-0.055*** (0.017)	-0.047** (0.017)	-0.056** (0.017)
Real current expenditures per pupil (in thousands)	0.226* (0.094)	-1.165** (0.432)	0.223* (0.093)	-1.154** (0.428)	-1.235** (0.431)	-1.192** (0.422)
Percent college graduates or higher	0.015 (0.017)	0.034* (0.019)	0.012 (0.017)	0.032 (0.019)	0.036* (0.018)	0.035* (0.018)
Percent school aged (5-17)	-0.058** (0.018)	-0.147*** (0.031)	-0.058*** (0.018)	-0.145*** (0.030)	-0.149*** (0.030)	-0.142*** (0.029)
Percent aged 65 and older	0.077** (0.026)	0.031 (0.029)	0.076** (0.026)	0.027 (0.030)	0.021 (0.028)	0.011 (0.028)
Percent of housing units owner-occupied	-0.014 (0.012)	-0.007 (0.013)	-0.014 (0.012)	-0.006 (0.013)	-0.000 (0.012)	0.002 (0.012)
Percent nonwhite	-0.043** (0.013)	-0.029* (0.012)	-0.043** (0.013)	-0.030* (0.012)	-0.030* (0.013)	-0.034** (0.012)
Index of race Fractionalization	1.417 (1.277)	0.710 (1.218)	1.463 (1.289)	0.611 (1.236)	0.921 (1.225)	1.061 (1.275)
Percent living in urbanized area	0.001 (0.002)	-0.001 (0.002)	0.001 (0.002)	-0.001 (0.002)	-0.001 (0.002)	-0.000 (0.002)
Instrument for expenditures	No	Yes	No	Yes	Yes	Yes
1st stage F-test: Instruments=0		34.0		34.9	33.8	35.1
P-value test of overid. restrictions		0.22		0.25	0.27	0.35
Observations	41,577	41,353	41,578	41,354	41,353	41,354
R-squared	0.896		0.896			

Notes: Standard errors that allow for within district correlation in errors in parentheses (*** p<0.001, ** p<0.01, * p<0.05). Dependent variable measured as the percent of K-12 enrollment in private schools. Regressions weighted using total K-12 enrollment (public + private). All models include school district fixed-effects and state x year effects. The weighted mean of the dependent variable is 10.7 percent private enrollment.

Appendix Table 1: First-Stage Estimates for 2SLS Models from Tables 4, 5, 6 and 7

First-stage estimates corresponding to:	Table 4 Col. (2)	Table 4, Column (3)		Table 4, Column (4)	
Endogenous covariate:	State	State	Mean/ Median	State	Mean/ Median
Instruments:	(1)	(2)	(3)	(4)	(5)
Finance reform x within-state quartile of mean district family income					
x quartile 1	1365.3***	1320.8***	0.0166**	1253.3***	-0.004
	(109.7)	(110.0)	(0.0058)	(129.4)	(0.008)
x quartile 2	486.2***	445.9***	0.0122*	695.2***	-0.013
	(90.9)	(91.2)	(0.0055)	(118.7)	(0.008)
x quartile 3	403.1***	375.5**	0.0115	480.9***	-0.016
	(125.1)	(129.0)	(0.0060)	(94.1)	(0.008)
Income inequality instrument					
Skewness		-0.010***	5.1E-6***		
		(0.002)	(2.8E-7)		
Synthetic cohort				-859.6**	0.447***
				(451.0)	(0.070)
Observations	41,422	41,422	41,422	31,063	31,063
1st stage F-test: Instruments=0	53.7	42.5	84.9	26.9	13.2
R^2	0.834	0.835	0.744	0.806	0.553

First-stage estimates corresponding to:	Table 5, Column (2)	Table 5 Column(4)	Table 2 Column (7)
Endogenous covariate:	Gini	Gini	Mean/ Median
Instruments:	(6)	(7)	(8)
Finance reform x within-state quartile of mean district family income			
x quartile 1	0.066	1.602***	707.6***
	(0.155)	(0.434)	(77.1)
x quartile 2	-0.260	0.874*	331.3***
	(0.145)	(0.415)	(58.2)
x quartile 3	-0.245	0.715	348.4**
	(0.177)	(0.503)	(138.6)
Income inequality instrument			
Skewness	1.2E-4***	5.0E-6***	
	(7.3E-6)	(2.8E-5)	
Observations	41,422	41,422	41,353
1sr stage F-test: Instruments=0	62.7	62.7	34.0
R^2	0.834	0.771	0.918

Notes: Standard errors that allow for within district correlation in errors in parentheses (*** $p<0.001$, ** $p<0.01$, * $p<0.05$). "State" represents real state revenues per pupil. Mean/median refers to the within-district ratio of mean to median family income. All models include school district fixed-effects, plus state x year effects. Demographic covariates shown in Table 3 are also included in these regression models. Columns (4)-(5) use only observations from 1980 – 2000.