

# Economic Inequality and the Provision of Schooling

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The school finance landscape has changed dramatically in the past thirty years. Most states have undertaken major changes to their school finance programs, motivated principally by the notion that the unequal school resources associated with unequal incomes and community sorting lead to unequal educational and labor market outcomes. This paper describes the empirical evidence on the relationship between school finance reforms and student outcomes and presents new evidence on the effects of these policies on community and school composition.

## BACKGROUND

During the past several decades, federal and state governments have pursued redistributive policies aimed at fostering “equality of economic opportunity”—the idea that although people’s incomes may vary, this variance should be due primarily to factors such as individual ability and effort, not to differences in circumstance. This goal has motivated social welfare policies at both the state and federal levels. Despite decades of redistributive policies, numerous empirical studies (such as Solon [1992],

Zimmerman [1992], Corcoran et al. [1992], and Shea [1997]) continue to find evidence of a substantial level of income persistence across generations, even after holding constant many individual characteristics. Shea’s findings are particularly compelling, as he contends that only parental income correlated with parental ability (rather than “luck”) affects children’s future incomes. This finding suggests that cash transfers to parents may have little effect in influencing their children’s labor market outcomes.

What might account for this link between parental income and children’s income? Many economists believe that this relationship is due in large part to differential human-capital investment between high-income and low-income families. High-income parents can invest in more (and better) education for their children, in a manner that low-income parents cannot, due to credit market imperfections. Since credit markets are imperfect, because parents cannot borrow against their children’s future earnings to finance human-capital investment, low-income parents may face binding liquidity constraints and, consequently, may underinvest in their children’s human capital (Loury 1981; Becker and Tomes 1986; Mulligan 1995). This is only one possible explanation, however, and may carry less weight given Shea’s finding that parental money per se does not matter in determining their children’s outcomes.

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The persistence of income inequality, together with the existence of market imperfections that may help perpetuate this inequality, has motivated policymakers to explore various options designed to make opportunities more equal. Many in the policy arena have suggested that opportunities could be further equalized via the implementation of changes in the system of school finance that make education spending more equal. This argument is bolstered by substantial evidence that premarket factors play a significant role in determining subsequent labor market outcomes (see, for example, Neal and Johnson [1996] and Bishop [1989]). Hence, school finance reforms could serve, to some degree, to sever the link between parental income and the human capital accumulation that leads to improved outcomes of their children.

Thus, the goal of fostering increased income mobility through equality of economic opportunity is a major motivation of the dozens of recent school finance reforms, either court-ordered or enacted by legislative edict. These policies have experienced a resurgence in the last several years, with state supreme court decisions mandating equalization in states such as Kentucky, Texas, Vermont, and New Hampshire, further altering a school finance landscape that has changed dramatically since 1970.

The best available evidence on the impact of these major finance reforms, and of other lesser changes in the systems of school finance in the individual states, supports the conclusion that these reforms have reduced within-state inequality in education spending (Murray, Evans, and Schwab 1998) by weakening the link between school district property wealth and spending. This weakening of the link between property wealth and spending does not imply that there has been a commensurate weakening of the link between education spending and *current* income. Since low-income individuals reside in both low- and high-property-wealth districts, as do high-income individuals, the impact of finance reforms on the relationship between spending and current income must be examined independently. Support for this argument is provided by Brunner and Sonstelie (1999), who show that finance reforms in California have not changed the distribution of spending across income groups.

To examine the impact of school finance changes in the 1970s and 1980s on the relationship between education spending and income, we combined data drawn from school-district-level extracts from the 1970 and 1990 Censuses of Population and Housing with financial data drawn from the 1972 Census of Governments and the 1989-90 Survey of School District Finances. For each district in each cross-section, we were able to observe per-capita income, the fraction of the population in poverty, total expenditures per pupil, current expenditures per pupil, total expenditure per pupil relative to the state average, and the fraction of revenues generated locally. We also had complete data for a substantial number of these districts in both 1970 and 1990, although—given the nature of the 1970 school-district-level extract—data on small, rural districts were far less likely to be available than for their urban and suburban counterparts.

When we examined the relationship between income and spending, using all of the districts in each of the cross-sections, we observed a slight decline in the strength of the relationship between per-capita income and each of the spending measures.<sup>1</sup> For these same cross-sections, the strength of the relationship between the poverty rate and each spending measure exhibited a more substantial decline, sometimes even changing direction.<sup>2</sup> It appears, however, that these changes were driven primarily by the impact of the finance changes on rural districts. When we limited our analysis to districts for which we had complete data in both 1970 and 1990, the relationship between per-capita income and the spending measures actually strengthened between 1970 and 1990.<sup>3</sup> For these districts, the strength of the relationship between the poverty rate and the spending measures did decline, but the decline was not as substantial as that observed when the analysis was based on the two cross-sections.<sup>4</sup> Although these results may seem surprising, they should not be completely unexpected, since the correlation between property wealth and income is strongest in rural areas.

The implication of these results is that, while finance reforms have shifted additional resources to income-poor rural districts, the reforms have not shifted resources toward income-poor urban and suburban districts to the

same extent. Thus, we find for the nation, as Brunner and Sonstelie found for California, that the school finance changes of the 1970s and 1980s may have done little to redistribute spending across income groups.

In light of these results, the question of whether finance reform policies will, in fact, weaken intergenerational persistence in income remains an open one. Equally uncertain are the impacts of these reforms on school and community composition. This uncertainty concerning the impact of school finance reforms has motivated a number of economic theorists to explore the potential results of these policies by crafting models that extend the classic Tiebout (1956) model to develop predictions concerning the impact of school finance reforms on various aspects of schooling provision. A number of papers—including Bénabou (1993, 1996), Fernández and Rogerson (1996, 1997, 1998), Nechyba (1996, 1999), Glomm and Ravikumar (1992), and Barse, Glomm, and Ravikumar (1996)—address the likely impact of these reforms on community composition, public sector outcomes, and private school enrollments.

The papers in this line of research that consider the impact of finance reforms on social mobility often derive contradictory implications for the likely effects of these reforms. On the one hand, Glomm and Ravikumar (1992), along with many others, suggest that public provision of education increases social mobility. On the other hand, Bénabou (1996) and Durlauf (1996) each show that public provision of education can lead to *decreased* social mobility in the event of income-stratified communities and local financing of public schools (both of which arguably are characteristic of schools in much of the United States). The fact that plausible theoretical models yield substantially divergent predictions makes this inherently an empirical question.<sup>5</sup>

A review of the theoretical literature also makes it clear that the impact of finance reforms on school and community composition can only be ascertained empirically. The papers in this literature generally confirm the insight of Fischel (1989); restrictions on the ability of individuals to consume their desired level of education services in the public sector will tend to break down the tendency of individuals to sort on the basis of income or parental education.

Furthermore, as Nechyba's (1999) insight in a related context makes clear, many of the high-income individuals who move from previously high-spending to previously low-spending school districts will, at the same time, be choosing to opt out of the public schools.

This tendency of school finance reforms to induce significant changes in community composition, however, depends on the extent to which the ability of schools to produce outcomes owes to the quality of peers in the schools. Nechyba (1999) observes that if the peer effect is sufficiently strong, individuals will either opt out of the public sector or, by extension, will choose the public sector only if they are able to reside in homogeneous communities. Pursuing the logical implication of this reasoning, we expect that if parents feel that peers are sufficiently important, school finance reforms and tax limits could accentuate the tendency of individuals to sort both across communities and across schools.<sup>6</sup> This logic appears to be confirmed by other theoretical work. For example, while his focus is on the general equilibrium effects of the introduction of private school vouchers, Nechyba (1996) shows that comparison of alternative systems of school finance depends not only on the structure of each of these finance systems but also on the parents' perceptions of the link between spending and student performance and of the importance of peer group effects. Bénabou (1996) shows that the effects on student performance of a move from a system of locally financed schools to a system of state-financed schools depends critically on the importance of both peer effects and purchased inputs in production and on the extent of cross-community migration that the move to a state-financed school induces.

In large part because of the relative newness of the school finance reforms of the last two decades, there is no empirical evidence on the relationship between these policy changes and income inequality. As an intermediate step, several authors (Husted and Kenny 1996; Hoxby 1998; Downes and Figlio 1998; Card and Payne 1998) have empirically attacked the question of how school finance reforms have affected the level and distribution of student performance. Such an approach seems natural in light of the well-documented link between outcomes on

standardized tests and future earnings (Loury and Garman 1995). Downes and Figlio (1998) and Card and Payne (1998) are particularly noteworthy because they show that student-level data can be used to evaluate the long-run impact of policy changes on standardized test performance. The differences in the conclusions reached by these two pairs of authors also make it clear that reaching consensus on the precise impact of finance reforms requires further work.<sup>7</sup>

Once consensus is reached on the impact of finance reforms on student performance, is that the end of the story? In our view, the answer is no for several reasons. First, other effects of these policies may be interesting to examine in their own right. Second, studying these impacts in other areas allows us to begin to pinpoint the determinants of the observed performance changes. Third, by broadening the scope of our study of the effects of school finance reforms, we can begin to complete our understanding of the impact of these policy changes on the schooling experience for all children, not just for those children who remain in the public schools. This third point is particularly important, because careful understanding of the distributional consequences of a public policy change requires that the entire distribution of students be studied.

To date, there have been few attempts to quantify the magnitude and the nature of the interdistrict and intersector mobility predicted in the theoretical work of Nechyba (1996, 1999), Bénabou (1996), and Fernández and Rogerson (1996, 1997, 1998). In this paper, we summarize some of our own recent work on school finance reforms and community and school composition, and interpret the school performance results in the context of these findings. In addition, we present new evidence on the effects of school finance reforms on the differential selection into public and private schools of central-city students from high-income and highly educated families.

#### SCHOOL FINANCE REFORMS AND THE DISTRIBUTION OF STUDENT OUTCOMES

Although a vast literature exists on the relationship between school spending and student outcomes, the question of whether additional dollars spent on schools will improve outcomes remains unresolved—and hotly

contested. Indeed, in papers prepared for a special issue of the Federal Reserve Bank of New York's *Economic Policy Review* devoted to education in America, Eric Hanushek and Alan Krueger evaluate the existing evidence in different ways. Hanushek (1998) concludes that "the current organization and incentives of schools do little to ensure that any added resources will be used effectively" (p. 23). Krueger (1998), however, asserts that "the U.S. public school system has not deteriorated and may in fact be reasonably efficacious" (p. 38). While these authors certainly disagree about the degree to which American schools are "broken," and about how to read the existing evidence on school spending and student outcomes, it is reasonably certain that neither believes there to be a mechanical production relationship between dollars and achievement, as might be implied by the unfortunately popular name of the "education production function" literature.

The realization that it was fruitless to utilize an education production approach to quantify the impact of finance reforms led several researchers to explore directly the performance effects of school finance reforms. The first paper in this line of research is Downes (1992), in which the extensive school finance reforms in California in the late 1970s were analyzed. This work indicated that greater equality across school districts in per-pupil spending was not accompanied by greater equality in measured student performance. In part, this failure of performance to converge was attributable to growing cost differentials between high-performance and low-performance districts. Nevertheless, the paper raised troubling questions about the efficacy of finance reforms of the type implemented in California. Because this research focused on the possibly unique California case, however, the generalizability of the conclusions is debatable.

Hoxby (1998) uses national-level data to characterize how finance reforms change the incentives facing local districts and, thus, per-pupil spending. She also considers how these changes affect dropout rates. She finds that, on average, dropout rates increase about 8 percent in states that adopt state-level financing of the public schools. And, while Hoxby's work does not explicitly address the effect of equalization on the within-state distribution of

student performance, it seems likely that much of the growth in dropout rates occurred in those districts with relatively high dropout rates prior to equalization. In other words, these results imply that equalization could adversely affect both the level and the distribution of student performance.

Although Hoxby raises an important point, her approach misses key features of school finance reforms that are relevant for exploring the effects on student outcomes. Because she does not explicitly account for the imposition of tax or expenditure limits, which we demonstrate in Downes and Figlio (1998) to be important determinants of student outcomes, and because the passage of these limits is often roughly contemporaneous with school finance reforms, it is unclear whether the changes in performance observed by Hoxby are attributable to school finance reforms or to the imposition of tax or expenditure limits. Furthermore, Hoxby's method focuses on local incentives and does not explicitly account for changes in direct state support of public schools. Large changes in the fiscal incentives provided to school districts have generally been associated with large changes in the ways in which school spending is allocated at the state level (Brunner and Sonstelie 1999).

While the dropout rate is an outcome measure of considerable interest, analyses of the quality of public education in the United States tend to focus on standardized test scores and other measures of student performance that provide some indication of how the general student population is faring. Recent work of Husted and Kenny (1996) suggests that equalization may detrimentally affect student achievement. Using data on thirty-seven states from 1987-88 to 1992-93, they find that the mean Scholastic Aptitude Test (SAT) score is higher for those states with greater intrastate spending variation. Like Hoxby, however, Husted and Kenny fail to control for the imposition of tax or expenditure limits, and, because they use state-level data, Husted and Kenny cannot examine the intrastate impact of equalization. Finally, since only a select set of students take the SAT, Husted and Kenny are not able to consider how equalization affects the performance of all students in a state.

Card and Payne (1998) also use SAT scores to explore the relationship between school finance reforms and student achievement. The authors tend to find that school finance equalization improves outcomes for lower income students, indicating that it may have some positive redistributive consequences. While Card and Payne adjust SAT scores to a larger degree than Husted and Kenny to account for selectivity, many of the concerns associated with Husted and Kenny's paper are relevant for their work as well.

To date, the only paper to investigate the effects of school finance reforms on the full distribution of students is Downes and Figlio (1998). In this paper, as well as in our other work, we use variants of an event analysis approach to quantify the impact of finance reforms and tax limits. Since tax limits and finance reforms differ (sometimes dramatically) from state to state, such an approach is imperfect for isolating the effects of these policies. Although we recognize this limitation of the event analysis approach, we also feel that compromises must be made if we are to attain a national perspective on the impact of these policies. Thus, to partly account for the heterogeneity of school finance reforms, we categorize the reforms according to whether they are or are not court-mandated, as suggested by Downes and Shah (1995).

We recognize that our classification of school finance reforms as court-mandated or legislatively mandated is somewhat crude, since there exists considerable heterogeneity across school finance reforms. Thus, in Downes and Figlio (1998), we also adopted Hoxby's (1998) suggested classification of reforms as "pro-spending" or "anti-spending."<sup>8</sup> More work on classifying and identifying school finance reforms, as well as more individual-state analyses, are certainly in order.<sup>9</sup>

Downes and Figlio (1998) use information from two rounds of individual-level data on student attributes and test scores: the National Education Longitudinal Survey (surveyed students were high school seniors in 1992) and the National Longitudinal Survey of the High School Class of 1972. We linked students to their schools and estimated separate effects of school finance reforms, as well as tax or expenditure limits, for students in schools "leveled up" by

school finance policies and those in schools “leveled down” (in relative terms) by the policies. We found that court-mandated and legislatively mandated school finance reforms tended to increase average public school performance and that students in initially low-spending school districts tended to benefit the most from legislative reforms. We also found that, if anything, anti-spending reforms led to increased student outcomes and pro-spending reforms led to decreased student outcomes.

In more recent work (Downes and Figlio 1999), we have explored the effects of school finance reforms on *private* school performance, using an identical empirical approach to the one we used to measure public school performance effects. In that paper, we observed that, while our models yielded a statistically insignificant distributional test score effect of legislative school finance reforms, we found a strong, statistically significant distributional effect of court-mandated school finance reforms. Specifically, we found that the relationship between school finance reforms and private school student test scores increased with the ratio of the initial county expenditures relative to the average per-pupil expenditures in the state. Taken together with our finding in earlier work of no impact of court-mandated reforms on the distribution of public school test scores or even on the level of public school test scores in states other than California, this finding could indicate that court-mandated finance reforms are widening slightly the dispersion in overall (that is, public *and* private) student performance. In such a case, the school finance policies intended to decrease the dispersion of student outcomes may actually tend to increase this dispersion.

#### SCHOOL FINANCE REFORMS AND COMMUNITY COMPOSITION

The preceding discussion provides a strong motivation for why it is so important to consider the school choice implications of school finance reforms. Students are not tied to a particular school or even to a particular neighborhood or community. This point, which has become a central theme of much of the theoretical work mentioned above, indicates the possibility that the distributional effects, if any, of

redistribution of resources per se in the public sector could be undone by geographic resorting and public-private selection of students, particularly if peer effects and teacher quality (both of which, alas, are difficult to measure) matter more than school spending.

To date, the empirical literature has been extremely quiet with regard to the possible school choice responses to school finance reforms. Only one study, Aaronson (1999), directly addresses the impact these policies have on the degree of homogeneity of affected districts. Aaronson finds that the extent of income sorting is unaffected by a state supreme court decision ruling the existing system of school financing unconstitutional. Only in states in which the system of school financing has been upheld by the courts does Aaronson detect any evidence of changes in the composition of affected districts. His results indicate that, in the aftermath of a supreme court decision for the state, the fraction of low-income individuals increases in districts in the bottom portion of the state’s across district income distribution. Also, the only significant compositional effect Aaronson uncovers when examining the fraction of high-income individuals who live in high-income districts is a decline in the fraction of high-income individuals in those districts that have both high average incomes and low median house values.

Aaronson’s work is ground-breaking, careful, and thoughtful, but it also has flaws that could affect the conclusions. First, for 1970, Aaronson is unable to create school-district-level measures for nonurban districts. Thus, most of his empirical work is based on 1980 to 1990 changes. The concern, therefore, is that the base year is too close to the policy implementation. While Aaronson’s conclusions are the same if he examines 1970 to 1990 changes for those districts for which he has data and if he omits those states with policy changes close to 1980, the possibility still exists that the preferred estimates of the policy effects understate their actual effect.

The second flaw in Aaronson’s approach is that he groups districts on the basis of their location in the state’s income distribution, not on the basis of their location in the distribution of education spending. Thus, for example,

Aaronson asks if, in low-income districts, the fraction of families with low incomes changes in the aftermath of a court-mandated finance reform. The problem with this approach is that these policies differentially affect districts on the basis of their education spending, not on the basis of their personal income. There are numerous examples of districts with low per-capita incomes and high levels of education spending. In short, the policy variables should be interacted with initial levels of per-pupil spending and not with initial income levels.

The final drawback to Aaronson's approach is that he looks only at the impact the policies have on income sorting. While much of the theoretical literature concentrates on income sorting, it is because in most theoretical models demand for education is perfectly correlated with income. In reality, demand for education may be as strongly correlated with parental education levels as with income. Thus, the need exists to consider the impact of the policies on the extent of sorting by education.

In recent work (Downes and Figlio 1999), we evaluate the effects of school finance reforms on community composition, using the school-district-level data described above. In that paper, we find evidence of policy-induced resorting, reflected in changes in the observed composition of school districts. This pattern is consistent qualitatively, if not always statistically significant, across measures of community composition for both legislative and court-mandated reforms, suggesting that a classic Tiebout story best describes the post-finance reform dynamic. Specifically, in a Tiebout-world, finance reforms reduce the incentive to sort on the basis of tastes for education. Thus, in initially low-spending districts, such policies should result in relative increases in per-capita income, in relative declines in the poverty rate, and in relative increases in the fraction of college-educated people. This is exactly the pattern of changes that we observe.

The results that support these conclusions are shown in Table 1. The differential effect of the policies on districts with different prereform levels of spending is revealed by the estimated coefficients on the interactions of the policy dummy with the ratio of the 1972 level of

per-pupil spending in the district to the state average.<sup>10</sup> The fact that the policies differentially affect districts is supported by these results; per-capita income has grown less rapidly in high-spending districts in states in which a legislative reform of the school financing system has been implemented. This result is paralleled by a finding that the fraction of individuals with a college degree has declined relatively in initially high-spending districts in legislative reform states. For court-ordered reforms, the only apparent compositional impact is a relative decline in the fraction of individuals with a college degree.

These estimated compositional effects are relatively easy to reconcile with the estimated impacts of these policies on the standardized test performance of public school students as described in Downes and Figlio (1998), as the reduction in dispersion could be attributable to relative changes in peer group quality resulting from the apparent resorting associated with legislative school finance reforms. Only by determining if the new residents of low-spending districts choose the public schools, however, can we check the validity of this argument.

*Table 1*  
DIFFERENCES-IN-DIFFERENCES-IN-DIFFERENCES RESULTS  
Estimated Effect of School Finance Policies on School District Demographics

Policy Variable: Interaction of District Spending Relative to State Average with	Dependent Variable: Change in <sup>a</sup>			
	Log of Per- Capita Income	Fraction in Poverty	Fraction without a High School Education	Fraction with a College Degree
Court-mandated reform	-0.0428 (0.0449)	0.7765 (1.1651)	1.1443 (1.0933)	-2.5436 (1.1204)
Legislative reform	-0.0894 (0.0395)	1.2212 (1.2030)	0.9675 (0.6713)	-2.5435 (1.1362)
R <sup>2</sup>	0.5538	0.3513	0.4820	0.5832

Source: Authors' calculations.

Notes: The regressions also include the 1970 value of the log of per-capita income, the fraction below poverty, the fraction with less than a high school diploma, the fraction with a college degree, the fraction that are Hispanic, and the fraction that are African-American. Also included are controls for the 1972 fiscal status of the district, as well as a constant, state-specific effects, and urbanicity dummy variables reflecting seven urban status possibilities (large central city, midsized central city, suburb of large central city, suburb of midsized central city, large town, small town, and rural, as defined by the U.S. Census Bureau). In addition, the regressions control for tax limit status. Standard errors that are robust to heteroskedasticity and within-state correlation of the error terms appear in parentheses below the parameter estimates.

<sup>a</sup> All changes are measured as the 1990 level minus the 1970 level.

## SCHOOL FINANCE REFORMS AND SCHOOL SECTOR SELECTION

The results mentioned above describe the changes in the composition of different types of *communities* following large-scale school finance reforms. But they provide very little evidence regarding the changes in the composition of *public schools* in the wake of finance reforms. Changes in public school composition could look much different from changes in school district community composition as a whole since communities include childless families, families with children not of school age, and residents who send their children to private schools. We therefore now explore the effects of school finance reforms on the characteristics of student selection into public and private schools.

To this end, we use data from the 1970 and 1990 Public Use Microsamples (PUMS) of the U.S. Census of Population to explore the school choices of residents of central cities of metropolitan areas. We focus on two family characteristics: the household's income and the education level of the household head. For the purposes of this analysis, we consider a household to be high-income if its income exceeds four times the poverty rate, and we consider a household to be highly educated if the household head has a four-year college degree. We estimate the effect of school finance reforms on public school enrollment rates of different types of people using a "difference-in-difference" strategy: we compute a policy effect by calculating the estimated difference between cities subject to a school finance reform and those not subject to a reform in the change within a city from 1970 to 1990 in the fraction of the public school student population that comes from a highly educated or high-income household. In this approach, we also control for changes in tax limit status.

We present in Table 2 the estimated policy effects of court-mandated and legislative school finance reforms. We observe that court-mandated school finance reforms are associated with differential *increases* in public sector rates of household education and income that are statistically distinct from zero. Legislative school finance reforms also apparently differentially increase public sector rates of household education and income, although these differ-

ences are not statistically significant at conventional levels. Therefore, the evidence suggests that school finance reforms are associated with increases in the rates of highly educated and high-income households sending their children to urban public schools.

Even this comparison, however, does not provide complete evidence on the sorting story. To fill in the rest of the picture, we also perform the same comparisons using data from the private sector. The second row of Table 2 presents the same type of difference-in-difference analysis in which we are interested for the set of students who reside in the central city of a metropolitan area and attend *private* school. We observe that court-ordered school finance reforms are associated with significant increases in the fraction of high-income and highly educated families among central-city residents who are private school attendees. Moreover, this difference is qualitatively larger for the private sector than for the public sector and is statistically distinct from the public sector (at the 8 percent level) in the case of highly educated families. In the case of legislative school finance reforms, a similar pattern emerges, but is only significantly different between public and private sectors with regard to the education characteristic.

*Table 2*  
DIFFERENCE-IN-DIFFERENCE RESULTS  
Estimated Effects of Tax Limits and School Finance Reforms  
on Public and Private School Composition

Policy Variable	Composition Variable			
	Household Income Exceeds Four Times the Poverty Rate		Household Head Is College Graduate	
	Court-Mandated Reforms	Legislative Reforms	Court-Mandated Reforms	Legislative Reforms
Public school students	0.071 (0.029)	0.036 (0.022)	0.043 (0.022)	0.010 (0.019)
Private school students	0.124 (0.041)	0.037 (0.034)	0.141 (0.039)	0.054 (0.036)
<i>p</i> -value of difference	0.151	0.970	0.082	0.063

Source: Authors' calculations.

Notes: The results in the table are based on the set of students in the 1 percent sample of the decennial census residing in central cities of metropolitan areas. The reported figures are the estimated difference between policy cities and no-policy cities (for each of the various policies) in the difference between 1990 and 1970 composition measures. The regressions also control for tax limit status. Standard errors that are robust to heteroskedasticity and within-city error correlation appear in parentheses below the parameter estimates.



In summary, the evidence is wholly consistent with the notion of highly educated families and, to a lesser extent, high-income families moving to the central cities in response to school finance reforms and sending their children to private schools. Therefore, the community composition results described above are, as we suspected, almost surely not reflective of changes in the student body population. The results are also consistent with the performance findings that suggest that both public school student and private school student test scores increase in communities that are “leveled up” by school finance reforms. These results suggest that the performance findings may be either directly or indirectly reflective of the compositional changes that we are noticing herein.

In related work (Downes and Figlio 1999), we look at public and private school composition for a broader set of communities—not just central-city students—using data from the National Education Longitudinal Survey and the National Longitudinal Survey of the High School Class of 1972 and find similar distinctions between selection into public and private schools. In that paper, we report that court-mandated school finance reforms are associated negatively with the income level and parental education level of public school students, but the interaction with initial per-pupil expenditure is significantly positive, suggesting that the negative compositional effect of court-mandated reforms is primarily a property of relatively low-spending schools.<sup>11</sup> Selection into private schools as a function of prereform county per-pupil expenditures is qualitatively a mirror image of selection into public schools as a function of prereform per-pupil expenditures. For instance, we observe that court-mandated reforms tend to lead to positive income selection into private schools, but this selection is attenuated as the relative spending level of the county increases. The results are the same, qualitatively, in the case of parental education—there appears to be positive selection into the private sector in the wake of school finance reforms, but less so (if any) as initial levels of relative public school spending increase.

With regard to both parental education and family income, differential selection into public schools at different levels of 1972 county relative per-pupil spending is statistically distinct from the estimated differential selection in the private sector. The estimated effects of legislative school finance reforms, however, seem to follow no perceptible pattern and are not statistically significant.

A likely, though not exclusive, explanation for these findings and those summarized in Table 2 is that some high-income and highly educated parents respond to school finance equalizations by moving to relatively poor school districts and selecting into private schools. Such an outcome, forecast by authors such as Nechyba (1996), is consistent with stronger positive selection into public schools from initially high-spending counties and stronger positive selection into private schools from initially low-spending counties.

## CONCLUSION

Hopefully, what this brief review of the literature makes clear is that the impact of school finance reforms on the extent of income inequality in the United States remains to be determined. Nevertheless, one lesson should be clear from this discussion: if the goal is to reduce income inequality substantially, state supreme court decisions mandating relatively specific changes in the school finance system are not particularly effective policy instruments. Even the most optimistic estimates of the impact of court-mandated school finance reforms on the distribution of student performance indicate that these distributional effects are relatively small. And these small gains come at the potential cost of movement of higher income families into the private sector and a concomitant increase in the extent of sorting by income in the schools. The goal of reducing the persistence of income inequality is laudable. However, court mandates that dictate the nature of school finance reforms do not seem to be particularly good tools for accomplishing this goal.

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## ENDNOTES

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1. For example, the simple correlation between the log of current spending per pupil and the log of per-capita income was 0.344 in 1970 and 0.304 in 1990. Similarly, in a regression of the log of per-capita income on the log of current spending per pupil and state-specific effects, the coefficient on the log of current spending per pupil was 0.164 in 1970 and 0.136 in 1990.
2. The simple correlation, for example, between the log of current spending per pupil and the poverty rate was -0.345 in 1970 and -0.155 in 1990; the regression coefficient on the log of current spending per pupil was -1.082 in 1970 and 2.023 in 1990.
3. For instance, the simple correlation between the log of current spending per pupil and the log of per-capita income was 0.342 in 1970 and 0.447 in 1990; the regression coefficient on the log of current spending per pupil was 0.167 in 1970 and 0.399 in 1990.
4. The simple correlation, for instance, between the log of current spending per pupil and the poverty rate was -0.343 in 1970 and -0.296 in 1990; the regression coefficient on the log of current spending per pupil was -1.103 in 1970 and 0.048 in 1990.
5. Theory is also equivocal about the impact of school finance reforms on mean income in a state. For example, Bénabou's (1996) results imply that mean income could decline, while Fernández and Rogerson (1997, 1998) generally find that moving to a state system of financing could increase mean income.
6. The existence of peer effects need not accentuate the tendency to sort. See de Bartolome (1990) and Brueckner and Lee (1989) for models in which peer effects exist and heterogeneous communities form. What is clear from these models is that the degree of sorting will depend critically on the benefits that high-income or high-ability individuals get from mixing with lower income or lower ability individuals.
7. This paper is not the place to discuss the relative advantages and disadvantages of the approaches taken in these two papers. We refer the reader to the individual papers for the relevant discussions.
8. The pro-spending/anti-spending classification is based upon the impact a reform has on the cost to local taxpayers of increasing spending by one dollar, holding constant intergovernmental aid. A pro-spending reform reduces this cost; an anti-spending reform increases it.
9. Aaronson (1999) has suggested a third alternative characterization of finance reforms based on the outcome of court challenges to a state's school finance system.
10. The specifications that generate the estimates in Table 1 include a full set of state dummies. As a result, it is not possible to estimate separately the common impact of any one of the policies on all districts in the state that have implemented that policy. This limitation prevents us, for example, from determining if the outcomes are consistent with Fernández and Rogerson's (1997, 1998) prediction of increasing per-capita income after a shift to state financing.
11. We find similar results, although less statistically significant, in the case of legislative school finance reforms.

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