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School finance reform, the distribution of school spending, and the distribution of student test scores

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Abstract

This paper studies the effect of school finance reforms on the distribution of school spending across richer and poorer districts, and the consequences of spending equalization for the relative test performance of students from different family backgrounds. We find that states where the school finance system was declared unconstitutional in the 1980s increased the relative funding of low-income districts. Increases in the amount of state aid available to poorer districts led to increases in the spending of these districts, narrowing the spending gap between richer and poorer districts. Using micro samples of SAT scores from this same period, we then test whether changes in spending inequality affect the gap in achievement between different family background groups. We find evidence that equalization of spending leads to a narrowing of test score outcomes across family background groups.

1. Introduction

The U.S. system of public education was founded on the principles of local financing and local control. In the 1920s there were over 125,000 school districts in the county, funded almost exclusively by local property taxes. Although state governments have gradually taken on a bigger share of public school financing,

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local funding remains a critical and contentious aspect of almost all state systems. The tendency for wealthier districts to spend more per student than poorer districts has led to constitutional challenges of the school financing systems in many states, and to State Supreme Court orders overturning the systems in 21 states between 1971 and 1992.[1] At the same time, voter resistance to rising property taxes has led to limits on local tax revenues in many states, forcing legislatures to redesign the state aid systems (Figlio, 1997).

This paper analyzes the nature and consequences of school finance reforms over the 1980s. We begin by examining the record of litigation and legislative changes in state aid formulas from the late 1970s to the early 1990s. We then turn our attention to quantifying the effects of school finance reform. We characterize each state's aid system by the slope of the relationship between state funding per student and median family income in a district. By this metric a state aid system is more equalizing if the slope is more negative (i.e., if districts with higher income receive less state aid). We find that states where the financing system was found unconstitutional tended to adopt more equalizing funding formulas over the 1980s. We also find that legislatively-induced school finance reforms that reduced or eliminated flat grants and enlarged the share of state funding based on the district's ability to pay led to equalization in many states.

While changes in funding formulas shift the relative amounts of state aid received by richer and poorer districts, they do not necessarily lead to corresponding changes in spending. School districts may reduce local taxes in response to an increase in the amount of state aid. We study the extent of this fiscal substitution in a simultaneous equations framework, using judicial and/or legislative actions as instrumental variables for the changes in the slope of the relationship between state funding and district income. Consistent with previous research on the 'flypaper effect' of targeted grants, our findings suggest that a one-dollar increase in state aid increases district education spending by 50–65 cents. Nevertheless, the inequality of local revenues per student widened between richer and poorer districts during the 1980s, offsetting the equalizing effects of changes in the state aid formulas of many states.

The second part of the paper focuses on the consequences of school finance reform. Some observers argue that a narrowing of the gap in spending between richer and poorer school districts will narrow the gap in student outcomes between richer and poorer families. To evaluate this argument we construct average SAT scores for children from different parental education groups using large samples of SAT-takers from the late 1970s and early 1990s. A limitation of the SAT is the non-randomness of test participation. The fraction of high school seniors who write the test varies widely across states, and mean test scores tend to be higher in states with lower participation rates (Dynarski, 1987). We use estimates of the

[1] A 1971 California court case, *Serrano v. Priest*, is credited with launching the school finance reform movement of the past two decades (see Murray et al. (1997), Table 1).

fraction of high school students who write the SAT in each family background group and state to control for systematic selection biases. We fit a series of models relating test score outcomes for different family background groups to the degree of spending inequality in a state. The estimates show a robust effect of spending inequality on the gap in test scores between students from the highest family background group and those from the middle. More parsimonious specifications also show a significant effect on the test score gap between the middle and lowest parental education groups. Finally, we find some evidence that the equalization of spending across districts narrows the gap in test participation between higher and lower background groups.

2. The evolution of school financing formulas over the 1980s

In the early part of the twentieth century most school spending in the United States was financed by local property taxes. Modest levels of state aid were distributed in some states based on the number of students or teachers in individual school districts (Augenblick et al., 1991). During the 1930s a wave of school finance reform led many states to modify their aid formulas to take account of the property tax bases in different districts. Many states also increased the total funds available for elementary and secondary schools. As a consequence of these reforms the average share of state funding rose to 30 percent by 1940, and gradually increased to 40 percent by 1970. Since the 1960s the federal government has also played an increasing role in the financing of public education: federal grants contributed an average of 7 percent of school district revenues in the early 1990s, with a higher share in poor southern states, e.g. Mississippi (17 percent in 1993) and Alabama (13 percent in 1993).[2]

A new wave of school finance reform began in the mid-1970s and has continued to the present. Several underlying factors have contributed to the recent interest in reform. Most obviously, inequalities in family incomes and in the property tax bases of different school districts have risen.[3] These disparities have led to rising inequality in spending across districts and demands for equalization. At the same time, educators and legislators have become increasingly concerned with assisting 'special needs' students, thereby introducing a new source of funding disparities across districts with differing student populations. Finally, taxpayers in many

[2]For additional discussion of historical aspects of school finance, see Reschovsky (1994).

[3]The Census data we use below show the average coefficient of variation in median family incomes across districts in a state widened by about 30 percent between 1979 and 1989. A related issue is the divergence between tax rates for residential and commercial property. Because the distribution of these two types of properties varies across the school districts, this also is believed to have affected the distribution of resources across school districts.

states have became disgruntled with the level of property taxes, leading to pressure to find other sources of revenue for school spending.

These forces have culminated in two sources of explicit pressure for school finance reform. In states like California and Massachusetts voters approved ballot initiatives (Proposition 13 in California and Proposition $2\frac{1}{2}$ in Massachusetts) that placed strict limits on local property tax rates. By the late 1980s as many as 20 states had adopted limitations on local spending or revenue (Figlio, 1997). At the same time parents in poorer school districts have launched legal challenges to the school finance systems in many states. Typically these challenges argue that the financing system violates a provision of the state constitution guaranteeing a basic level of education for all children.[4]

Table 1 summarizes the education financing plans and the state Supreme Court decisions in the 48 mainland states during the period from 1975 to 1991. Twenty-seven states had a Supreme Court ruling on the constitutionality of the school financing system during this period. In the 12 states listed in panel I, the court found the system unconstitutional and directed the state to revise it, while in the 15 states in panel II, the court ruled that the funding system satisfied the constitution. Finally, in the 21 states listed in panel III, there was either no challenge or no court ruling between 1975 and 1992.[5]

School funding formulas are classified into three broad categories in Table 1: a flat grant formula, so-called 'minimum foundation' plans, and variable grants. Many state aid systems incorporate two or more of these alternatives, although the share of aid allocated through a particular formula may be small. While not shown in the table, most states also offer categorical aid for such purposes as special education, gifted student programs, and transportation.

A flat grant (FG) formula provides a fixed dollar sum per student to each school district. By their nature, flat grant plans have little effect on the equality of resources across districts, nor do they shift the marginal cost to the district of spending one extra dollar on educational expenditures (the so-called tax price of local expenditures). The other two systems provide differing amounts of aid to different districts. Under a minimum foundation plan (MFP), a state provides the difference between the minimum amount it expects to be spent per pupil in all districts (the 'foundation level') and a level of local revenue that a given district is

[4]A pivotal California case, *Serrano v Priest*, successfully argued that the state's financing plan violated the equal protection clause of the U.S. Constitution. Subsequently the U.S. Supreme Court ruled that education is not a fundamental right under the U.S. Constitution. Since that ruling, all cases have been filed in state courts. See Fischel (1989) for more information on *Serrano*.

[5]There are several reasons why lawsuits have been brought in some states but not others. These include: (1) the language of the state constitution, which determines the likely success of a suit; (2) The structure of the state court systems and/or the political disposition of the state legislature, which affect the likelihood that advocates for reform will bring a lawsuit, versus work through the legislative process; (3) the degree of disparity in school spending across districts. See Campbell and Fischel (1996).

Table 1
State finance plans and court rulings[a]

State	Year of ruling	Funding formulas in use:	
		1975–76	1990–91
(a) States with court decision finding school finance system unconstitutional			
Arkansas	1983	FG	MFP
California[b]	1971, 1977	MFP + FG	MFP + FG
Connecticut[b]	1977, 1982	FG	VG
Kansas[b]	1976	VG	VG
Kentucky	1989	MFP + FG	MFP + FG
Montana[b]	1989	MFP + VG	MFP + VG
New Jersey[c]	1973, 1976, 1989, 1991	FG	MFP
Texas[b]	1989, 1991	MFP + FG	MFP + VG
Washington	1978, 1991	MFP	VC
West Virginia	1978, 1988	MFP	MFP
Wisconsin[b]	1976	VG	VG
Wyoming	1980	MFP	MFP
(b) States with court decision finding school finance system constitutional			
Arizona[c]	1973	FG	MFP
Colorado	1982	VG + FG	MFP
Georgia	1981	MFP	MFP + VG
Idaho	1975, 1990	MFP	MFP
Louisiana	1976, 1987	MFP	MFP
Maryland	1972, 1983	MFP	MFP + FG
Michigan	1973, 1984	VG	VG + FG
Minnesota[c]	1971	MFP + FG	MFP
New York	1972, 1982, 1987	MFP + FG	VG + FG
North Carolina	1987	FG	FG
Ohio	1979, 1991	VG + FG	MFP
Oklahoma	1987	MFP + VG + FG	MFP + VG
Oregon	1976, 1991	MFP + FG	MFP
Pennsylvania	1975, 1979, 1987, 1991	VG + FG	VG
South Carolina	1988	FG	MFP
(c) States with no court decision by 1992			
Alabama[c]		MFP	MFP
Delaware		VG + FG	VG + FG
Florida		MFP + FG	MFP
Illinois		MFP + VG + FG	MFP + VG + FG
Indiana		MFP	MFP + FG
Iowa		MFP + FG	MFP + FG
Maine		MFP + VG	MFP
Massachusetts[c]		VG + FG	VG
Mississippi		MFP + FG	MFP
Missouri[c]		MFP	MFP + VG + FG
Nebraska		MFP + FG	MFP
Nevada		MFP	MFP

Table 1. Continued

State	Year of ruling	Funding formulas in use:	
		1975–76	1990–91
New Hampshire[c]		MFP + FG	MFP
New Mexico		MFP	MFP
North Dakota		MFP	MFP
Rhode Island[c]		VG + FG	VG
South Dakota		MFP + FG	MFP
Tennessee		MFP	MFP
Utah		MFP	MFP
Vermont		VG + FG	MFP + FG
Virginia		MFP + FG	MFP

[a] The state funding formulas are: flat grants (FG) — systems that provide a uniform amount per student; minimum foundation plans (MFP) — systems that pay an amount per student that is higher for districts with lower tax bases (at a fixed assessment rate); and variable guarantee VG) plans — systems that provide matching grants that vary with the actual revenues raised by the district.

[b] These states also had court rulings that upheld the state's school finance finance system: California (1986); Connecticut (1985); Kansas (1981); Montana (1974); Texas (1973); Wisconsin (1989).

[c] These states also had court rulings that overruled the state's school finance system after 1992: Alabama (1993); Arizona (1994); Massachusetts (1993); Minnesota (1993); Missouri (1993); New Hampshire (1993); New Jersey (1995); Rhode Island (1994).

expected to generate. The latter is typically based on an estimate of the property tax base of the district and, thus, is independent of the amount raised from local revenues.[6] The state may or may not require a district to meet a minimum local revenue target to receive state funding and, usually (California being an exception), the state does not restrict the maximum revenue a district may raise. Thus, MFP's do not affect the tax price of local education expenditures.

Variable grant (VG) schemes differ from the other two schemes in that the amount of state aid received by a district varies with the amount of local revenues actually raised. Under a 'guaranteed tax yield' system, for example, the state pays the difference between a targeted revenue level and the district's actual yield.[7] In principle the state grant could be negative under such a system — a situation known as 'recapture' — although most states limit the minimum grant per student.[8] An alternative VG system, known as a 'percentage equalization' scheme, varies the state grant with actual expenditures per pupil, multiplied by a ratio that is declining in the fiscal capacity of the district (usually the local property tax base calculated

[6] The minimum foundation amount is often a budgetary residual, determined by working backward through the funding formula given the state budget. See Ohio Governor's Education Management Council (undated) for a discussion of this phenomenon in Ohio.

[7] Let B represent the district tax base per student and t the tax rate. State aid per student is then $R^*(t) - tB$, where $R^*(t)$ is the guaranteed yield per student at tax rate t. A 'guaranteed tax base' system sets $R^*(t) = tB^*$, where B^* is the guaranteed tax base per pupil.

[8] Exceptions in the early 1990s include Michigan, Wyoming and Utah.

at a fixed assessment rate). Variable grant schemes offer higher state grants to poorer districts, and would be expected to equalize spending across districts. They may also distort the marginal tax price of additional education spending. For poorer districts a VG scheme operates like a matching grant, implying a marginal tax price of less than $1 for education spending. For richer districts a VG scheme may lead to a marginal tax price of over $1 if 'recapture' is permitted: otherwise the marginal tax price for higher districts is $1.[9]

The second and third columns of Table 1 show the incidence of the three types of formulas in the mid-1970s and early 1990s. The most common state funding formula is a MFP: 15 states relied exclusively on MFPs in 1975–1976, while 25 states did so in 1991–1992. In the mid-1970s 13 states used a combination of MFP and flat grants: by the early 1990s this number had fallen to 6 (mainly by the elimination of the FG component). Flat grants were used as the sole basis of funding in 6 states in the mid 1970s but in only one state in the early 1990s (North Carolina).

Among the 12 states in which the supreme court found the school finance system unconstitutional, 5 explicitly changed the structure of their financing plans between 1975–1976 and 1990–1991. By comparison, among the 15 states where the court ruled that the aid system satisfied the constitution, 12 changed the structure of their financing scheme between 1975–1976 and 1990–1991. Finally, of the 21 states with no court rulings between 1970 and 1992, 12 changed the structure of their financing schemes between 1975–1976 and 1990–1991. While these comparisons suggest that court decisions had little effect on the decision to switch aid formulas, the analysis presented below suggests that the direction of change was systematically different in states with a supreme court ruling against the existing aid system.

3. Modeling between-district inequality in state funding and expenditures

3.1. Overview

Given that many states use multiple funding formulas to allocate different fractions of their total aid budget, it is difficult to characterize a state's school funding system simply in terms of the formulas that are used. Moreover, many funding formulas are restricted by minimum or maximum aid amounts per district, and by the amount of funds available to be allocated by the formula. In view of these considerations, we decided to adopt a simple empirical characterization of the funding systems in different states, based on the correlation between state

[9]Hoxby (1996) has emphasized the effect of different state funding schemes on the marginal tax price of school expenditures. See also Downes and Figlio (1997).

funding per student and family income in a district.[10] There are three reasons for this focus. First, much of the controversy over school financing arises from the disparity in spending between richer and poorer school districts. Our reading of the constitutional challenges that have been mounted against school finance systems is that spending inequality per se is not a primary concern. Rather it is the fact that districts can vary the tax assessment of property allowing wealthier districts in many instances to spend more per pupil than poorer districts, while imposing lower property tax rates, that has troubled judges and legislators.[11]

Second, conventional economic models suggest that incomes in a district will be a key determinant of school spending. For example, the median voter model developed by Bergstrom and Goodman (1973) implies that a district's school spending choice is determined by the median-income household in the district. Strict Tiebout-style models predict that households with similar incomes will sort into homogenous communities, leading to spending differentials across districts that reflect the Engel curve for education.[12] More recent 'political economy'-based models of spending choices (e.g. Romer et al., 1992) also emphasize the role of median (or average) income in a district as a determinant of district spending.

Lastly, from a pragmatic perspective, the partial correlation between median income and state revenues or spending in a district provides a simple 'reduced form' summary of a state's financing system that can be easily compared across states and used to quantify the effects of a reform.

To proceed, let S represent state aid per student granted to a school district in a given state in a given year, let E represent total spending per student in the district, let I represent median family income in the district, and let X represent a vector of observable factors that affect school spending, such as the range of grades offered

[10] Earlier studies, using data on average per pupil spending at the state level, also focused on the relationship between spending and average income to investigate whether spending in states with court-ordered reforms differed from states without court-ordered reforms (Downes and Shah, 1995; Silva and Sonstelie, 1995; Manwaring and Sheffrin, 1994). On a priori grounds one might prefer to use property wealth per student, rather than family income, to characterize different districts. Our choice of family income was motivated by the difficulty of obtaining property wealth data, and by the fact that property wealth tends to be highly related to family income (as one would expect in a Tiebout framework). To check the correlation, we used a measure of property wealth in our 1977 data available for about 75 percent of districts. A regression of the log of property wealth per student in a district on the log of median family income has a coefficient of 0.93 ($t=32.3$) controlling for state effects and urban/rural location.

[11] This is spelled out very clearly in the New Hampshire Supreme Court decision finding the state's school finance system unconstitutional. The court wrote that ... 'compelling taxpayers from property-poor districts to pay higher tax rates and thereby contribute disproportionate sums to fund education is unreasonable' (New Hampshire Supreme Court, 1997).

[12] As noted by Goldstein and Paully (1981), if demand for public education expenditures depends on both income and tastes, the slope of the relationship between district-level spending and district-level incomes will overstate the slope of the true individual-level Engel curve for education spending, conditional on tastes.

by a district and urban/rural location. Consider a set of state- and year-specific projections of state funding per capita and expenditures per capita on I and X:

$$S = \alpha_1 + \beta_1 I + \gamma_1 X + u, \tag{1}$$

$$E = \alpha_2 + \beta_2 I + \gamma_2 X + v. \tag{2}$$

The coefficient β_1, summarizes the redistributive character of the school finance system in a given state and year. In particular, a more negative value of β_1 implies a more equalizing state funding formula. Similarly the coefficient β_2 summarizes the degree of spending inequality across higher and lower income districts in a state.

These two coefficients are linked by the district budget constraint and by the behavioral reaction of local revenues to changes in the level of aid provided by state or federal authorities. To see this, note that

$$E = S + L + F,$$

where L represents local revenues raised per capita, and F represents per-capita federal grants received by the district. Assume that federal aid is distributed by a formula that generates the same income gradient (β_3) in all states:

$$F = \alpha_3 + \beta_3 I + \gamma_3 X + w. \tag{3}$$

Finally, assume that the desired level of spending per student in a district is determined by a function $E^*(Y, G, \tau; \delta)$ where Y represents total available resources per student in the district (including family income and outside aid), G represents the amount of outside aid per student ($G = S + F$), τ is the tax price of an additional dollar of school spending per capita in the district, and δ represents a set of observed and unobserved characteristics. Bergstrom and Goodman's (1973) model suggests that E^* is the demand function for education spending by the median-income voter in the district. In this case E^* will depend on total resources per student available to the median-income family and on the price of school spending, but will be independent of G. More generally, however, E^* summarizes the political process of spending determination. In this case, targeted aid may affect spending choices through an effect on total resources, and/or through a 'flypaper effect' (Gramlich, 1977).

When the state funding formula provides a level of state aid that is independent of local revenues (as in flat grant or MFP systems), total resources per student are $Y = I + S + F$ and $\tau = 1$. In this case,

$$E = E^*(I + S + F, S + F, 1; \delta)$$

and

$$L = E^*(I + S + F, S + F, 1; \delta) - S - F.$$

Let $e = \partial E^*/\partial Y$ denote the partial derivative of desired spending with respect to total income and let $\lambda = \partial E^*/\partial G$ denote the partial derivative of desired spending with respect to targeted aid (i.e., the flypaper effect). Using this notation, the derivative of local spending with respect to a rise in state aid is $e + \lambda - 1$, which varies with the size of the income effect on the demand for education spending and the magnitude of the flypaper effect. Assuming that the derivatives of state and federal aid with respect to district family income are β_1, and β_3, respectively, the derivative of education spending with respect to income is

$$\beta_2 = e + (e + \lambda)(\beta_1 + \beta_3). \tag{4}$$

A change in the state funding formula that leads to a change $\Delta\beta_1$, in the income gradient of state aid therefore leads to a change $\Delta\beta_2 = (e + \lambda)\Delta\beta_1$, in the income gradient of per capita expenditures.

If state aid varies with local revenues, as in a variable grant funding system, the expressions for β_1, and β_2 are more complex. Suppose that $S = S^0 + \sigma L$, where S^0 is a component of aid that is independent of local revenues, and σ can be either positive or negative. In this case, the tax price of local expenditure is $1/(1 + \sigma)$, and local revenue is determined by $L = E^*(I + S + F, S + F, 1/(1 + \sigma); \delta) - S - F$. Now consider a school finance reform shifts the relationship between S^0 and district income, but has no effect on σ. Then it can be easily shown that $\Delta\beta_2 = (e + \lambda)\Delta\beta_1$, which is the same expression as in the case when state aid is fixed.[13]

The degree of 'fiscal substitution' between state aid and local revenues is determined by the magnitude of $(e + \lambda)$. Given the typical budget share of educational spending, a plausible estimate for the income derivative of spending is about 0.1.[14] Thus, if there are no flypaper effects, a \$1 increase in state aid per student might be expected to lead to a 90 cent cut in local taxes and only a 10 cent increase in spending. On the other hand, the existing literature (Gramlich, 1977; Fisher, 1982; Hines and Thaler, 1995) suggests that λ may be relatively large, implying that local revenues will not fall by as much. In the next section we present a simple procedure for estimating $(e + \lambda)$ given estimates of $\Delta\beta_1$, and $\Delta\beta_2$ and information on the judicial and legislative changes that are presumed to have led to changes in β_1.

3.2. Data on school district expenditures

We use data from the 1977 and 1992 Censuses of Governments, merged with district characteristics from the 1980 and 1990 Censuses of Population to estimate Eqs. (1) and (2) and study the effects of judicial and legislative actions on state

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[13] For a linear state grant system $\beta_1 = (\beta_0 + \sigma e))/(1 + \sigma(1 - e - \lambda))$ and $\beta_2 = (e + (e + \lambda)\beta_0 + \sigma e))/(1 + \sigma(l - e - \lambda))$, where β_0 is the regression coefficient of S^0 (the non-contingent state aid component) on district income.

[14] If the median voter has an income elasticity of demand for schooling of 1, and average school spending represents about 10 percent of median family income then one would expect $e = 0.1$.

Table 2
Enrollment-weighted means of state support and current expenditures per student[a]

	Number of districts	Median family income 1979	State revenue per student			Current expenditures per student		
			1977	1992	Change	1977	1992	Change
All States	13,036	39,109	1532	2435	903 (59%)	3355	4090	1554 (46%)
States with court decision finding school finance system unconstitutional								
Average of 12 States	4377	40,459	1494	2771	1276 (85%)	3358	4886	1528 (46%)
Connecticut	154	46,695	927	2763	1836 (198%)	3768	7435	3668 (98%)
Texas	811	39,338	1287	2109	822 (64%)	2609	3979	1370 (53%)
States with court decision finding school finance system constitutional								
Average of 15 States	4350	38,913	1617	2395	777 (48%)	3503	5251	1748 (49%)
New York	641	39,305	2162	3307	1145 (53%)	4812	7619	2807 (58%)
Louisiana	66	34,724	1565	2372	807 (52%)	2665	4079	1414 (53%)
States with no court decision by 1992								
Average of 21 States	4349	37,934	1457	2132	675 (46%)	3150	4469	1318 (42%)
Massachusetts	294	42,405	1357	1520	163 (12%)	4348	5403	1055 (24%)
Alabama	109	31,854	1327	1970	643 (48%)	2276	3103	827 (36%)

[a] Number of districts matched refers to number of school districts matched between the 1977 and 1992 Census of Government files and the 1980 Census STF3F file with positive enrollments in 1977 and 1992 and valid data on mean family income in the district in 1979 and number of schools in the district in 1977. All means are weighted by average enrollment in 1977 and 1992. Current expenditures exclude construction, land, and equipment expenditures. All dollar amounts are in 1992 dollars. Numbers in parentheses are percentage changes in the amount of state aid per student or current expenditures per student.

funding formulas and district-level spending outcomes. We began by merging information for the roughly 15,000 school districts that reported data in both years of the Censuses of Governments.[15] We then merged observations to district-level records from the 1980 and 1990 Censuses (the STF3F file for 1980 and the School District Data Book for 1990). Our final working sample excludes districts with no enrollment in either 1977 or 1992, and districts in Alaska, Hawaii, and Washington D.C., yielding a total of 13,036 districts in 48 states.[16] This sample represents about 81 percent of the roughly 16,000 districts in the 48 states in 1992, and more than 90 percent of total enrollment.

Table 2 provides an overview of the school district data. We present statistics for the three groups of states identified in Table 1 based on state supreme court decisions over the 1977–1992 period. In addition we selected 6 representative states to illustrate the state-level variation within the groups. In each case, one of the representative states is from the northeastern region and the other is from the southeast region of the United States. The first column shows the number of districts in our matched 1977–1992 sample, while the second reports the average median family income across the districts within each state or group of states. Family incomes are slightly higher in the group of states in which the court overruled the state financing system, although the differences across groups are not large.

The remaining columns of the table show average state revenues per student and average operating expenditures per student in 1977 and 1992.[17] On average across all 13,036 districts in our sample, state aid per student rose by $903 dollars — a gain of 59 percent. The rise was substantially larger in states with a court decision overruling the existing financing system (85 percent) than in the other two groups of states (46–48 percent), suggesting that court pressure may have led to a general infusion of funds into the state aid system as well as possible changes in how the funds were allocated across districts. Interestingly, expenditures per student rose by roughly the same percentage in states where there was a court decision overruling the existing financing system as in those where the court upheld the existing system, but by less in states where there was no court decision. As illustrated by the experiences of the six representative states, however, within each of the three state groups there were notable differences in the relative growth of state aid and spending per student.

[15] There are 16,859 districts in the 1977 Census of Governments data file, and 16,236 districts in the 1992 data file. After some data cleaning and manual adjustments to the Census of Governments, we successfully merged 15,008 districts for the two years.

[16] We excluded districts with zero or missing expenditures and revenue data, and districts with expenditures per pupil above the 99th percentile or below the 1st percentile of the overall distribution of expenditures. In addition, a total of 235 districts are excluded from our 1992 sample because of missing data in the School District Data Book. Of these, 195 were located in California. These are similar to the exclusions adopted by Murray et al. (1997).

[17] Operating expenditures include salaries, benefits, supplies, maintenance costs, and food and transportation expenditures, but exclude new capital expenditures.

The range of experiences in spending growth across states is illustrated in the upper panel of Fig. 1, which plots 1992 average current expenditures for each state against the corresponding value in 1977. For reference, we have superimposed a line representing a uniform 46 percent increase in real spending per student. As the

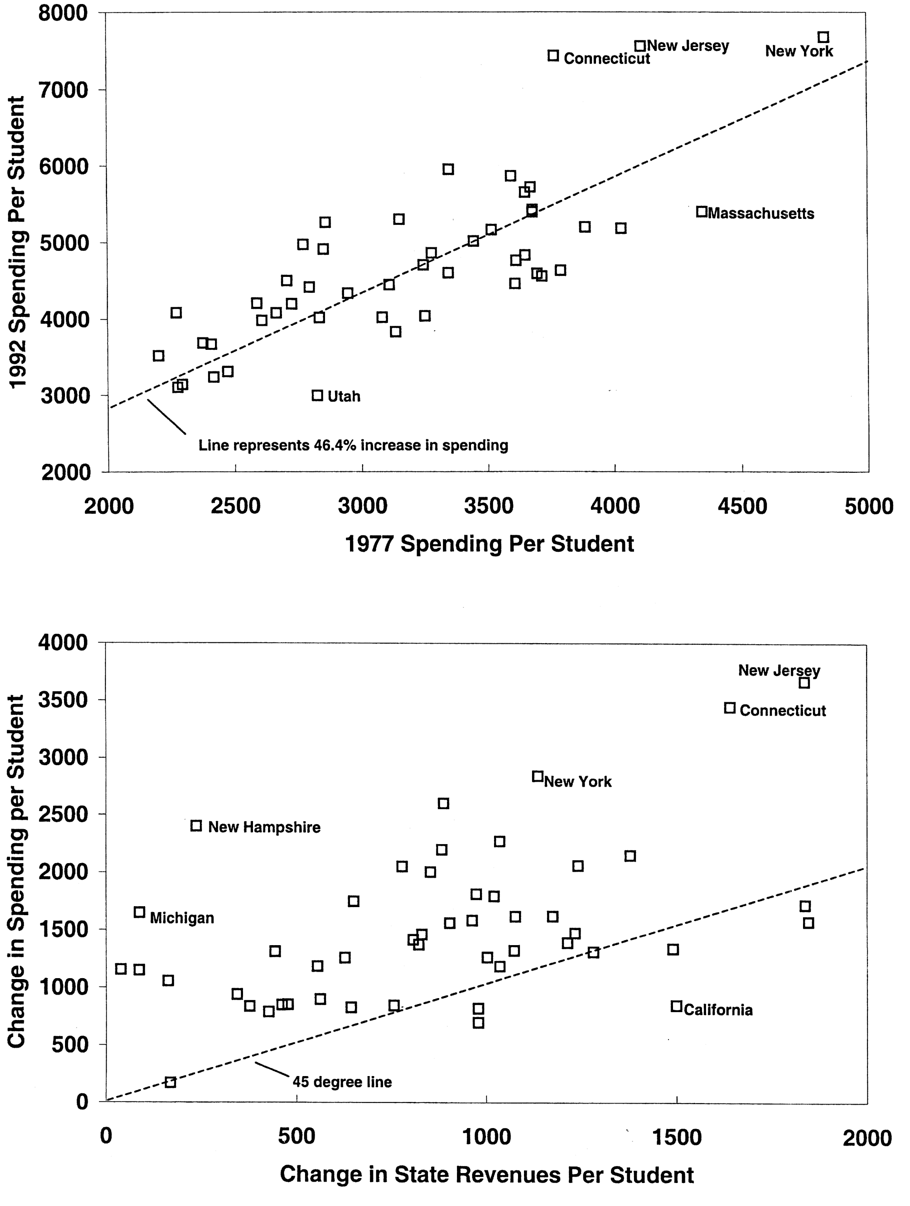

Fig. 1. Differences in spending changes and state revenue changes across states, 1977–1992..

figure makes clear, this is not a bad approximation to the data, although several states had much larger or smaller increases — particularly New Jersey and Connecticut on the high side and Utah and Massachusetts on the low side.

Across states there is a strong positive correlation between the change in state funding per student over the 1980s and the change in total spending per student. This is illustrated in the lower panel of Fig. 1, where we have plotted the changes in expenditures for each state against the corresponding changes in state aid per student. For reference, we have super imposed a 45 degree line in the figure: observations would lie along this line if, as a state increased grants to school districts, each district maintained a constant level of real local revenues per student. The scatter of points suggests the local revenues per student tended to rise in most states over the 1980s, even as per capita state grants increased, although California is a notable outlier.[18] The best fitting unweighted regression line has a slope of 0.71 (standard error 0.19, r-squared 0.23). Ignoring differences in income growth across states and other unobserved state-level factors, this simple regression coefficient can be interpreted as an estimate of the derivative of desired school spending with respect to a dollar increase in state aid per student. Taken at face value, the magnitude of the estimate suggests a substantial 'flypaper' effect.

3.3. Estimates of the changing association between spending and income

We estimated Eq. (1) and (2) separately for each of the 48 mainland states in 1977 and 1992. In addition to the key income variable (median family income in the district) the models include controls for the range of grades offered by the district, the average size of schools in the district, the location of the district (urban versus suburban or rural), and the fractions of blacks and Native Americans in the district. Table 3 presents the average values of the coefficients β_1, and β_2 for the three groups of states identified in Table 1, and for the six representative states. We also show the changes in these regression coefficients between 1977 and 1992. For convenience we have multiplied the estimates by 1000: an estimate of $\beta_1 = -1.0$ therefore means that state aid per student is \$1 lower for each additional \$1000 in median family income in the district. In 1980, the 10th percentile of the distribution of median family income across all school districts in our sample was \$28,732 (1992 dollars) while the 90th percentile was \$51,720. Thus, a value of $\beta_1 = -1.0$ would imply that state aid was \$230 higher per student at the 10th percentile district than the 90th percentile district — a relatively modest equalization effect given average spending of about \$3400. A coefficient of -5.0,

[18] Presumably this is a reflection of property tax limitations in California. Silva and Sonstelie (1995) assert that approximately one-half of the decrease in spending in California is attributable to reforms resulting from the *Serrano* decision and tax limitations. Manwaring and Sheffrin (1994) also note that California is an outlier with respect to changes in per pupil spending.

Table 3
Effect of district family income on levels of state support per student and total current spending per student[a]

	State support per student			Current spending per student		
	1977	1992	Change	1977	1992	Change
(a) States with court decision finding school finance system unconstitutional						
Average of 12 States	−1.37	−4.44	−3.06	1.58	1.50	−0.09
	(0.21)	(0.28)	(0.25)	(0.17)	(0.22)	(0.20)
Connecticut	0.15	−5.95	−6.10	3.65	4.39	0.74
	(0.08)	(0.56)	(0.40)	(0.40)	(0.47)	(0.44)
Texas	−1.73	−7.24	−5.52	1.13	0.60	−0.53
	(0.13)	(0.31)	(0.24)	(0.21)	(0.20)	(0.20)
(b) States with court decision finding school finance system constitutional						
Average of 15 States	−1.28	−3.14	−1.87	2.56	3.84	1.28
	(0.11)	(0.14)	(0.13)	(0.14)	(0.19)	(0.17)
New York	−2.08	−5.12	−3.04	6.25	7.96	1.71
	(0.22)	(0.29)	(0.26)	(0.33)	(0.36)	(0.34)
Louisiana	−0.38	1.34	1.72	1.92	5.57	3.65
	(0.26)	(0.66)	(0.50)	(0.62)	(1.23)	(0.97)
(c) States with no court decision by 1992						
Average of 21 States	−1.08	−3.10	−2.02	1.56	1.84	0.28
	(0.42)	(0.43)	(0.43)	(0.28)	(0.29)	(0.29)
Massachusetts	−0.49	−3.60	−3.11	2.20	3.69	1.49
	(0.26)	(0.32)	(0.29)	(0.47)	(0.47)	(0.47)
Alabama	−0.20	−0.06	0.14	1.21	2.85	1.64
	(0.11)	(0.14)	(0.13)	(0.39)	(0.47)	(0.43)

[a] Standard errors in parentheses. Table entries represent regression coefficients of median family income in models for state support or current expenditures by student, with standard errors in parentheses. All models are fit by state, and include dummies for the type of school district (elementary, secondary, or unified), and district location (central city, suburban, or rural), as well as controls for the fractions of blacks and Native Americans in the district, the log of the average school size in the district, and dummies for average school size under 100 pupils, between 100 and 199 pupils, and between 200 and 299 pupils.

however, raises this differential to $1149, implying a substantial equalization effect.

The estimates in Table 3 suggest that state funding formulas became more equalizing over the 1980s. The biggest shift toward cross-district equalization occurred in states where the school finance system was declared unconstitutional (average change $= -3.06$). The average value of β_1 also declined in states where the school system was challenged in court but upheld (average change of -1.82) and in states where there was no court ruling (average change of -2.02). In contrast to the tendency toward greater equalization in state aid, the estimates of β_2 suggest that cross-district inequality in spending was rising over the 1980s. The increase was largest in the group of states where the supreme court upheld the existing school finance system, and slightly negative in the states where the school finance system was found unconstitutional.

Looking at the data for the selected states in Table 3, the estimates of β_1 suggest that shifts in the equalizing effect of the state funding formulas were far from uniform within the three state groups. The average value of β_1 declined substantially in Connecticut and Texas, and also fell in New York and Massachusetts, but rose slightly in Louisiana and Alabama. Similarly, the estimates of β_2 show considerable dispersion both across states in a given year and over time within states. We draw two main conclusions from the data in Table 3. First, the equalizing effect of changes in state funding formulas between 1977 and 1992 varied across the states, with evidence of a move toward more equalization in states where there was a court ruling against the existing aid system. Second, there was a widening in the inequality of spending between richer and poorer districts, even in the face of more equalizing aid programs in most states.

3.4. Effects of 'reforms' on the distributions of state funding and expenditures

Have the school finance reforms initiated over the 1980s led to any narrowing of the inequality in spending across richer and poorer districts? To answer this question we compare changes in the coefficients β_1 and β_2 in states that had court rulings on their financing system and in states that added or dropped specific components of their funding system over the 1977–1992 period. The upper panel of Table 4 presents results from regression models of the form

$$\Delta\beta_{1j} = Z_j\theta_1 + \eta_{1j},$$

$$\Delta\beta_{2j} = Z_j\theta_2 + \eta_{2j},$$

where $\Delta\beta_{1j}$ is the change in the coefficient β_1 from Eq. (1) in state j between 1977 and 1992, $\Delta\beta_{2j}$ is the change in the coefficient β_2 from Eq. (2) in state j, and the Z_j's are sets of dummy variables for various judicial events or funding formula changes in the state.[19] Columns 1 and 4 present models in which the Z's are simply dummies for either a court ruling overturning the state funding system or a ruling declaring the system constitutional. The estimates show a systematic equalizing effect of an unconstitutional ruling: the income gradient of state aid falls by 1.89, on average, while the income gradient of spending falls by 1.4. By comparison, the effects of a court ruling upholding the state aid system are smaller and insignificantly different from 0.

Columns 2 and 4 present models in which we include changes in indicators for the presence of the three main types of funding programs: flat grants, minimum

[19]These regressions are weighted by the inverse sampling variances of the dependent variable.

Table 4
Unrestricted reduced forms and structural of state revenues on district spending estimates of the effect[a]

(A) Unrestricted reduced form estimates

	Effect on income-slope of state revenues per capita			Effect on income-slope of total spending per capita		
	(1)	(2)	(3)	(4)	(5)	(6)
Court rulings:						
Upheld	−0.81	—	−0.47	0.20	—	0.44
	(0.67)		(0.65)	(0.52)		(0.53)
Unconstitutional	−1.89	—	−1.05	−1.10	—	−0.79
	(0.62)		(0.65)	(0.48)		(0.53)
Changes in funding formulas:						
Flat grant	—	1.01	0.90	—	0.62	0.65
		(0.42)	(0.43)		(0.36)	(0.35)
Minimum foundation	—	−0.61	−0.34	—	−0.17	0.23
		(0.72)	(0.75)		(0.61)	(0.61)
Variable grant	—	−1.48	−1.08	—	−0.67	−0.10
		(0.58)	(0.64)		(0.36)	(0.53)
R-squared	0.18	0.28	0.32	0.17	0.14	0.24

(B) Structural estimates of the effect of state revenues on district spending

		IV with instrumental set:		
	OLS	A	B	C
Estimate	0.31	0.66	0.53	0.57
	(0.11)	(0.28)	(0.21)	(0.20)
P-value of over-identification test	—	0.18	0.95	0.60

[a] Reduced-form and structural estimates are estimated on sample of 48 states. Models are weighted by inverse sampling variance of the estimated change in the income-slope of total spending per capita between 1977 and 1992. See text. Instrument set A includes a dummy for a court ruling that found the state funding system unconstitutional, and another dummy for a court ruling finding in favor of the state funding system. Instrument set B includes 3 first-differenced indicators for the presence of flat grant, minimum foundation, and guaranteed yield funding formulas. Instrument set C includes all 5 instruments in sets A and B.

foundation plans, and variable grant plans. The coefficients associated with these first-differenced dummies can be interpreted as estimates of the net effect of the presence of each of the different funding formulas on the slopes β_1 and β_2. As expected, the presence of a flat grant raises β_1 (the income-gradient of state funding per pupil across districts) while the presence of either alternative funding formula lowers β_1. Similar patterns are found for the effects of the various formulas on β_2 (the income-gradient of current spending per pupil across districts), although the coefficient estimates are uniformly smaller in magnitude. The estimates imply, for example, that dropping a flat grant and replacing it with a variable grant formula would be expected to lower β_1 by 2.5 and lower β_2 by 1.3. Finally, columns 3 and 6 present models in which we include indicators for both court rulings and formula changes. Evidently, there is some collinearity between the court case outcomes and the formula change indicators.[20] Nevertheless, the covariates are highly significant as a group.[21]

The relative magnitudes of the coefficients in the models for $\Delta\beta_1$ and $\Delta\beta_2$ in the upper panel of Table 4 can be used to identify the responsiveness of local expenditures to a rise in the amount of state aid provided to a school district. Recall that a shift in the income gradient of state aid across districts will lead to a proportional shift in the income gradient of expenditures, with $\Delta\beta_2 \approx (e + \lambda)\Delta\beta_1$. Using estimates of $\Delta\beta_{1j}$ and $\Delta\beta_{2j}$ for the 48 states, this equation can be estimated by OLS. Alternatively, using either judicial decisions or changes in the state funding formula as instruments for the change in $\Delta\beta_{1j}$, it can be estimated by IV. The results are presented in the lower panel of Table 4.

The OLS estimate in the first column of the lower panel of Table 4 suggests that school finance reforms have had a modest effect on spending inequality: each additional dollar of state funding is estimated to increase total spending by 31 cents. In contrast, the IV estimate based on the court decision dummies (instrument set A) points to a larger effect of finance reforms. One explanation for the difference is that states have tended to adopt more redistributive formulas when there is an underlying trend toward rising inequality in the state — thus, the OLS estimates are downward-biased by the endogeneity of the decision to adopt more equalizing aid rules. The IV estimate based on changes in the state funding formulas (instrument set B) is slightly smaller, but still above the OLS estimate. Finally, if we use both the court decisions and formula changes as a combined instrument set (set C) we obtain a point estimate of $(e + \lambda) = 0.57$. Regardless of the choice of instruments, then, the results point toward a downward bias in the OLS estimate.[22]

One aspect of school spending that has attracted much attention over the past decade is funding for students with physical or learning disabilities. Special education funds are a rising share of total education spending in the United States,

[20] Formula changes were more likely to occur in states that had court rulings overturning the state system. We estimated first-differenced linear probability models for the incidence of MFP, VG, and FG plans and found a significant positive effect of an unconstitutional ruling on the probability of a VG system in 1992 (coefficient $= 0.38$, $t = 2.2$) and a significant negative effect on the probability of an FG system in 1992 (coefficient $= -0.47$, $t = 2.1$).

[21] We also investigated the effect of the presences of a state property tax limitation on the degree of spending inequality across districts (see Figlio (1997)). We added a dummy variable indicating the presence of such limitations to the models in the upper panel of Table 4. The resulting estimates suggest that, on average, tax limits have small and statistically insignificant effects on the change in spending inequality across richer and poorer districts.

[22] A Hausman test that the IV estimate using the combined instrument set is different than the OLS estimate yields a z-statistic of 1.58.

and account for some of the rise in state aid over the 1980s (see Rothstein and Miles, 1995, and Hanushek et al., 1998). Although our 1977 data do not allow us to separately identify special education funds, we can do so in 1992. To assess the sensitivity of the results in Table 4 to the role of special education funding, we re-defined state aid and operating expenditures in 1992 to exclude special education funds, and then formed new estimates of β_1 and β_2 for each state in 1992, and new estimates of $\Delta\beta_1$, and $\Delta\beta_2$. Since special education funds made up a very small share of total funding in the late 1970s (Rothstein and Miles, 1995) these estimates approximate the true changes in the inequality of state aid and spending that would be observed if we ignored all special education funding. The revised estimates of $\Delta\beta_1$, and $\Delta\beta_2$ are very highly correlated with estimates that include special education funding, and lead to very similar parameter estimates for the models in Table 4. Based on this analysis, we conclude that our inferences about the effect of school finance reform are not sensitive to the treatment of special education funding.

To summarize, our analysis of the school finance data points to four main conclusions. First, states in which the supreme court found the school financing system unconstitutional *have* altered their funding systems so as to redistribute aid to lower income districts. The magnitude of these changes is economically and statistically significant. We estimate that the gap in state aid between a poor district (median family income at the 10th percentile of the national distribution) and a rich district (median family income at the 90th percentile of the national distribution) widened by about $300 per student more in states where the financing system was found unconstitutional than in other states. Second, some states altered their financing formulas even without the pressure of a court decision, and these changes have tended to increase the extent to which state aid is targeted to low-income districts. Third, increases in state aid to lower-income districts have resulted in rises in the relative spending of these districts, with only modest fiscal substitution effects. Fourth, reforms to the system of state funding in many states were offset by widening inequality in local revenues between richer and poorer districts. Thus, even in states like Connecticut where the state funding system became significantly more re-distributive, spending inequality rose over the 1980s.

Our findings on the effects of court-ordered reforms collaborate those of Murray et al. (1997), who examined the effects of judicial decisions on the overall inequality of spending across school districts (e.g. the coefficient of variation in spending across districts in a state). Their analysis suggests that court-ordered reforms decrease spending inequality, primarily by raising the relative expenditures of districts at the bottom of the spending distribution.[23]

[23]The results in Table 4 with respect to changes in state financing formulas are less consistent with the results in a subsequent paper by the same authors (Evans et al., 1997), which concludes that non-court-ordered finance reforms have little or no systematic changes in spending inequality.

4. The effects of school finance reform on student outcomes

Recent policy interest in school finance reform has been stimulated in part by the large difference in test score performance between children from richer and poorer families, and a concern that some of this gap may be attributable to differences in school resources received by students from different backgrounds. However, there is relatively little direct evidence linking school finance reforms to student outcomes.[24] Moreover, research on the generic effects of school spending is controversial (see Hanushek, 1986 and Krueger, 2000). As in the school quality literature, a major problem in evaluating school finance reforms is the lack of information on student outcomes by school district. Our explicit focus on the effect of school finance reforms across richer and poorer districts suggests an alternative approach that sidesteps the need for district-level outcome data. Specifically, since families tend to sort into districts based on family income, any equalization of spending across richer and poorer districts would be expected to lower the gap in spending between schools attended by students from richer and poorer families. Indeed, since spending levels are very similar at the schools in a given district, the overall correlation between school spending and family income is determined by the distribution of spending across districts, and by the differential sorting of richer and poorer families into different districts. In principle, then, if school resources affect relative test scores, a school finance reform that reduces inequality across districts should lead to a narrowing of the achievement gap between students from different family backgrounds.

We use SAT scores of high school students to measure the effects of school finance reform. There are two advantages of the SAT. First, the test is administered nationally and is clearly an important metric of student performance. Second, large micro samples of SAT scores are available that include information on the test-taker's state of residence and family background. A key disadvantage of the SAT is that not all students take the test. The fraction of high school seniors who write the test varies from under 5 percent in states where the SAT is not required for admission to the state university to over 60 percent in states where the test is required. Moreover, SAT-takers are self-selected: they tend to be from wealthier families and to have better high school grades (see below). A related issue is that

[24] Downes and Figlio (1997) use reading and mathematics test scores for students in the National Longitudinal Survey of the Class of 1972 and the National Educational Longitudinal Survey administered in 1992. They find that the relative test scores of high school seniors in low-spending districts rise following both court-mandated and legislatively-induced reforms. Hoxby (1996) uses dropout rates measured at the district level in the Census of Population. She concludes that reforms that increase spending by low-spending districts lower the dropout rate, while reforms that reduce the spending of high-spending districts have the opposite effect. Weglinksy (1998) uses data from the National assessment of academic progress (NAEP) linked with school level data and finds that equalization of spending has an equalizing effect on test scores within schools. Downes and Figlio (1997) present a review of other related studies.

the SAT is designed to predict success in college, not to test substantive knowledge or forecast success in the labor market. Even ignoring selectivity issues, mean SAT scores provide only a limited assessment of spending changes, especially for non-college-bound students.

4.1. Overview of the SAT data

Our SAT data are drawn from a series of random samples of about 100,000 test-takers in each year from 1978 to 1992. Available information for each student includes test scores, class rank, age, grade, state of residence, and parental education and income.[25] Our working sample includes students from the 48 mainland states who were attending a public high school in either the 11th or 12th grade when they wrote the test. Since the samples are unstratified, the number of test-takers per state varies from under 50 per year (for small states with low SAT participation rate) to over 5000 per year (for larger states). To increase the sample sizes we elected to pool the 1978, 1979, and 1980 SAT samples into a single cross-section representing the late 1970s, and the 1990, 1991, and 1992 samples into a single cross-section representing the early 1990s.

Table 5 gives a brief overview of the SAT data, including sample sizes, average test scores, and test participation rates for the three groups of states defined in Table 1, and for representative states in each group.[26] Inspection of the table suggests that average test scores are strongly negatively related to test participation rates: scores are higher in Alabama and Louisiana than in Connecticut, New York, or Massachusetts. Indeed, across the 48 states the correlation of average test scores with the test participation rate was −0.66 in 1978–1980 and −0.71 in 1990–1992. An obvious explanation for this phenomenon is selective test participation. In states with low participation rates (such as Alabama or Louisiana) only high-achieving students who are applying to selective out-of-state institutions write the SAT.[27] In other states a majority of students write the test. The power of this explanation is confirmed by the correlation between test participation rates and the average class rank of test-writers.[28] In both years, this correlation is −0.90, implying that test-writers are selectively drawn from the upper tail of their class.

Another feature of the data in Table 5 is the variation in test participation rates and scores over time. On average, the SAT participation rate rose from about 43 to 55 percent over the 1980s, with wide variation across individual states. At the

[25] Most of the information other than the test scores comes from the Student Descriptive Questionnaires filled out by students at the time of the SAT test. About 80 percent of test-takers fill out this questionnaire.

[26] A complete tabulation for all 48 states is provided in Card and Payne, 1998, Table 4.

[27] Many states require the ACT, rather than the SAT, for admission to their state college systems. This leads to very low SAT participation rates in states such as Iowa and North and South Dakota.

[28] Test takers are typically informed of their class rank by guidance councillors as they fill out the descriptive information section of the SAT.

Table 5
Characteristics of SAT samples[a]

	1978–1979–1980 sample				1990–1991–1992 sample			
	Sample sizes		Mean SAT	Fraction writing SAT	Sample sizes		Mean SAT	Fraction writing SAT
	SAT	CPS			SAT	CPS		
All states	222,436	70,855	893	42.8	241,001	46,200	893	55.0
Example states:								
(a) States with court decision finding school finance system unconstitutional								
Connecticut	6326	893	900	59.1	5651	375	897	77.3
Texas	14,719	3603	874	33. 4	21,333	2467	874	49.5
(b) States with court decision finding school finance system constitutional								
New York	30,188	4657	895	49.7	28,532	3262	881	66.2
Louisiana	418	1251	955	3.0	578	569	994	4.8
(c) States with no court decision by 1992								
Massachusetts	12,470	1622	891	62.7	10,094	1449	896	83.6
Alabama	626	1284	926	4.5	770	615	991	7.2

[a] SAT samples include students in 11th or 12th grade enrolled in public high schools in the 48 mainland states. CPS samples include children age 14–17 in the March and October Current Population Surveys (October 1978, 1979, 1980 and March 1979, 1980 and 1981 for the '1978–1979–1980 Sample' and October 1990, 1991, 1992 and March 1991, 1992 and 1993 for the '1990–1991–1992 Sample'). Fraction writing SAT is estimated from a combination of data sources (see text).

national level, average SAT scores were constant, although scores rose or fell in different states. As in the cross-sectional samples, there is a systematic negative correlation between the change in the participation rate in a state and the change in the average test score, underscoring the role of the test participation process.

4.2. Modeling SAT outcomes at the individual and group level

To evaluate the effect of school finance reforms on SAT outcomes, we begin by specifying a model for the mean test scores of students from different family background groups in different states. Let y^*_{igjt} represent the potential test score of individual i from family background group g in state j and year t (if he or she were to write the test), and assume that

$$y^*_{igjt} = a_{gjt} + X_{igjt}b + \varepsilon_{igjt},$$

where X is a vector of observed characteristics (age, grade at the time of the test, race/ethnicity), a_{gjt} represents a standardized score for students from family background group g in state j and year t (which depends on resources received by the group and other factors), and ε is an idiosyncratic residual. We do not observe

y_{igjt} for all students: instead, we observe $y_{igjt} = y^*_{igjt}$ for a sample of those who decide to write the test. Assume that the probability of writing the test is determined by whether the random variable $v_{igjt} = Z_{gjt}C_g + \upsilon_{igit}$ is positive or negative, where Z_{gjt} is a set of variables that vary by family background group, state, and year (such as the admission policy of the state college system in state j and year t), and υ_{igjt} is an idiosyncratic component with $E(\upsilon_{igjt}|Z_{git}) = E(\upsilon_{igjt}|X_{igjt}, Z_{gjt}) = 0$. In this case, the mean observed scores for each state, year, and family background group will satisfy

$$E(y_{igjt}|\, i \text{ writes the test}, X_{igjt}, Z_{gjt}) = a_{gjt} + X_{igjt}b + h_g(\pi_{gjt}), \tag{7}$$

where $h_g(\pi_{gjt})$ is a control function (Heckman and Robb, 1985) that depends on π_{gjt}, the probability of test participation for individuals in group g in state j and year t. For simplicity, we assume that ε_{igjt} and υ_{igjt} are jointly normally distributed with a constant distribution for each family background group. In this case, $h_g(\pi_{gjt}) = -\xi g\lambda(\pi_{gjt})$, where $\lambda(\cdot)$ is the inverse Mill's ratio function and ξ_g is a constant that depends on the correlation between ε_{igjt} and υ_{igjt}.[29]

A key issue in evaluating the effect of school finance reforms on the distribution of test score outcomes is the definition of family background groups. Given our use of average family incomes in Eq. (1) to characterize state finance systems, a natural choice would be to define groups based on family income. Unfortunately, the income information reported in the SAT appears to be of relatively low quality. A comparison of reported family incomes for test-takers in high-SAT-participation states with income data derived from the March Current Population Survey (CPS) showed relatively large discrepancies, leading us to conclude that high school test takers have limited information on their parents' incomes. On the other hand, the distributions of parental education derived from the test-takers' responses are much closer to measures derived from the CPS. Based on these findings, we decided to define family background groups based on a combination of mother's and father's education. After some experimentation we settled on a set of 5 groups.[30]

To illustrate how differences in school spending across districts translate into differences in resources at the schools attended by different parental education

[29]Specifically, ζ_g is the product of the correlation coefficient between ε_{igjt} and υ_{igjt} multiplied by the standard deviation of ε_{igjt}. This is the group selection correction proposed by Gronau (1974) and generalized by Heckman (1979). More generally, Eq. (7) will hold for some control function — see Ahn and Powell (1993).

[30]The groups are defined as follows: (1) one or both parents has less than a high school degree; (2) father has exactly 12 years of education, mother has 12–15 years of education; (3) father has 13–15 years of education, mother has 12–15 years of education; (4) both parents have at least some college and one parent has a college degree or more; (5) father has some post-graduate education and mother has at least some college.

groups, assume as in Eq. (2) that expenditures per student in district d of state j (E_{dj}) are related to average family income in the district (I_{dj}) by

$$E_{dj} = \alpha_{2j} + \beta_{2j} I_{dj} + v_{dj},$$

Let f_{gdj} denote the fraction of students in background group g and state j who live in district d. Since mean spending per student for those in group g in state j is just a weighted average of spending levels in different districts, we obtain:

$$E_{gj} = \alpha_2 j + \beta_{2j} \left\{ \sum_d f_{gdj} I_{dj} \right\} + \sum_d f_{gdj} v_{dj}.$$

The differential in mean spending for students in background group g (E_{gj}) relative to the mean for all students in the state (E_j) is

$$E_{gj} - E_j = \beta_{2j} I_j \theta_{gj} + \xi_{gj}, \tag{8}$$

where $\theta_{gj} = \Sigma_d \ (f_{gdj} - f_{dj}) \times (I_{dj}/I_j)$ represents the covariance between the extra fraction of group g who live in a district ($f_{gdj} - f_{dj}$) and the relative level of family income in the district, and $\xi_{gj} = \Sigma_d \ (f_{gdj} - f_{dj}) \times v_{dj}$ is the covariance between the extra fraction of the group in a district and the component of district-level spending that is unrelated to family income. The term θ_{gj}, measures the differential sorting of group g across high and low income districts: θ_{gj} will be positive for high education groups (since parental education and family income are positively correlated across districts) and negative for low education groups.[31] Holding constant the distribution of families across districts, Eq. (8) therefore shows that a rise in β_{2j} will widen the gap in spending per pupil between students with more and less-educated parents, with a bigger effect, the greater the degree of sorting of family background groups into higher and lower income districts.

Assume that a_{gjt}, the mean potential test score for students from family background group g in state j and year t, depends on an intercept d_{gt}, and on spending per pupil at the schools attended by the group g and other unobserved factors (η_{gjt}):

$$\begin{aligned} a_{gjt} &= d_{gt} + \gamma E_{gjt} + \eta_{gjt} \\ &= d_{gt} + \gamma E_{jt} + \gamma \theta_{gjt} \beta_{2jt} I_{jt} + \gamma \xi_{gjt} + \eta_{gjt}. \end{aligned}$$

Given our data limitations we cannot directly observe θ_{gjt}. Instead, we assume that θ_{gjt} can be decomposed into a group effect, a state$\times$year effect, and a residual component: $\theta_{gjt} = \theta_g + \theta_{jt} + \varphi_{gjt}$. Then

$$a_{gjt} = d_{gt} + d_{jt} + \gamma E_{jt} + \gamma \theta_g \beta_{2jt} I_{jt} + \mu_{gjt} \tag{9}$$

where d_{jt} is an unrestricted state$\times$year intercept (which captures the interaction

[31] Data from the 1980 and 1990 Censuses suggest that 60 percent of the variance in median family income across school districts in a state is explained by the fractions of adults in 5 education classes.

term $\gamma\beta_{2jt}I_{jt}\theta_{jt}$ and other factors that are constant across groups in a given state), and

$$\mu_{gjt} = \gamma\beta_{2jt}I_{jt}\varphi_{gjt} + \gamma(\xi_{gjt} - \xi_{jt}) + (\eta_{gjt} - \eta_{jt})$$

is a residual component. For high parental education groups θ_g is positive, and if $\gamma > 0$ a rise in the inequality of spending across districts (measured by β_{2jt}) will lead to a rise in test scores. For low education groups θ_g is negative and a rise in spending inequality will lead to a fall in test scores if resources really matter.

Eqs. (7) and (9) form the basis for our analysis of the effect of changes in the distribution of school spending on student test scores. We estimate these equations using a two step method. In the first step, we estimate a model for observed individual test score outcomes that controls for the test-taker's grade and ethnicity, and includes a complete set of dummy variables for each state and family background group. Denote the estimated coefficients of the state×family background indicators in year t by $A_{gjt.}$ According to Eq. (7) $A_{gjt} = a_{gjt} + h_g(\pi_{gjt}) + e_{gj}$, where e_{gj}, represents a component attributable to sampling error. In the second stage we therefore fit models of the form

$$A_{gjt} = d_{gt} + d_{jt} + \gamma E_{jt} + \gamma\theta_g\beta_{2jt}I_{jt} + h_g(\hat{\pi}_{gjt}) + \mu'_{git} \tag{10}$$

where $\hat{\pi}_{gjt}$ represents an estimate of the SAT participation rate for group g in state j and year t, and μ'_{gji} incorporates the residual component of Eq. (9) plus the sampling error in the estimate of A_{gjt}.

A final issue is the estimation of test participation rates by state and family background group. To obtain reliable estimates of the number of high school students in each state and family background group (i.e., the denominator of the test participation rate), we pooled samples from the March and October Current Population Surveys. We matched children age 14–17 in the October and March CPS surveys with their mothers and fathers (if a father was present in the family).[32] Children who lived with both parents were assigned to one of the five family background groups, while those who lived with a single mother were allocated using the assumption that the probability of parental separation is independent of father's education, conditional on mother's education. We use counts from the 1978–1980 October CPS and 1979–1981 March CPS to estimate the number of potential SAT writers in the late 1970s, and counts from the 1990–1992 October CPS and 1991–1993 March CPS to estimate the number in the early 1990s.

To form the SAT participation rate, we then divided the number of test takers in our SAT sample for each state and family background cell by the number of children in the CPS samples in the same cell. We re-scaled the ratios so that the

[32] We dropped the small number of children who did not live with their mother.

implied national probabilities of writing the SAT in 1978–1980 and 1990–1992 matched available estimates of the fraction of high school seniors who took the SAT nationwide in 1979 and 1991, respectively. The participation rates from this two sample procedure are generally reasonable, although in a few cases they are bigger than 1 for the highest family background group, and for smaller states the rates are noisy. In an effort to smooth the data slightly, we fit a series of 3-parameter models to the participation rates of the 5 family background groups in each state and year, and used to the predicted probabilities from these models as our estimates of $\hat{\pi}_{gjt}$[33]. As expected, the estimated SAT participation rates are higher for students with better-educated parents, ranging from an average of 13 percent for the lowest education group to 70 percent for the highest education group in the late 1970s; and from an average of 18 percent for the lowest education group to 70 percent for the highest education group in the early 1990s.[34]

4.3. Estimation results

Estimation results for several alternative versions of Eq. (10) are presented in Table 6. Although we have data for a total of 480 observations (48 states $\times$5 parental education groups$\times$2 years) we have excluded observations for 5 states with very small numbers of observations in the SAT sample: North and South Dakota, Mississippi, Utah, and Wyoming. All of the specifications in the table assume that the coefficient of the inverse Mills' ratio control function is constant across groups (i.e. $h_g(\pi_{gjt}) = -\zeta \cdot \lambda(\pi_{gjt})$). The models are fit by weighted least squares, using the inverse sampling variances of the estimated A_{gjt}'s as weights. For ease of interpretation, we have normalized the income gradient terms by dividing each state's mean income by the average income for all states in the same year (i.e., the income gradient variable used in the regression models is $\beta_{2jt}I_{jt}/I_t$).

The first column of Table 6 presents a model that excludes permanent state effects. This very parsimonious specification shows a powerful selection effect (the *t*-ratio for the Mill's ratio term is over 35), and a positive and significant effect of mean expenditures on average test scores. Contrary to expectations, the coefficients of the income-gradient term ($\beta_{2jt}I_{jt}$) are positive for all five groups, suggesting the presence of unobserved state-level factors that are positively correlated with test scores and with the income gradients.

The coefficients are increasing in magnitude for higher education groups,

[33]We assumed that the participation rate of group $g = 1, 2, \ldots 5$ in a given state and year is equal to $p(g) = 1 - \exp(\omega_o + \omega_1 g + \omega_2 g^2)$. The parameters $(\omega_0,\ \omega_1,\ \omega_2)$ were estimated by non-linear least squares separately by state and year.

[34]An appendix table, available on request from the authors, shows mean scores and participation rates for each state and family background group in the late 1970s and early 1990s.

Table 6
Estimated models for adjusted SAT scores by state and family background group (pooled time series cross sections)[a]

	(1)	(2)	(3)	(4)
Selection term	70.10	59.21	46.27	15.07
	(1.99)	(5.18)	(4.79)	(8.46)
Mean spending[b]	8.44	1.33	—	—
(1000s of 1992$)	(0.97)	(1.74)		
Group-specific income gradient terms				
Group 1	0.63	−4.57	−2.25	1.79
	(0.79)	(1.24)	(0.60)	(1.80)
Group 2	0.82	−3.93	−1.47	0.00
	(0.72)	(1.18)	(0.56)	(1.66)
Group 3[a]	2.41	−2.42	0.0	0.0
	(0.58)	(1.12)		
Group 4	3.89	−0.69	1.83	2.54
	(0.66)	(1.15)	(0.53)	(1.52)
Group 5	5.17	0.93	3.76	3.82
	(0.80)	(1.23)	(0.63)	(1.75)
Other effects:	Group × year (9)	Group × year; state (51)	Group × year; state × year (93)	Group × year; group × state; state × year (261)
r-squared	0.95	0.97	0.98	0.99

[a] Models estimated by weighted least squares on sample of 430 observations (43 states, 5 family background groups, 2 years) Selection correction term is inverse Mills ratio based on estimated fraction of students in cell who write SAT. Regression weights are inverse sampling variances of the adjusted mean SAT scores by state, family background cell, and year. Standard errors in parentheses. Number of effects included in square brackets.

[b] With state × year effects, mean spending and one group's income gradient term are absorbed. The effect of the income gradient for group 3 is arbitrarily set to 0.

however, implying that a steeper income gradient widens the test score gap between students with more- and less-educated parents in a state. A set of time-invariant state effects are added to the model in column 2. This addition causes the effect of mean expenditures to fall substantially, and shifts the estimated income gradient coefficients uniformly down, so that only the scores of the highest background group are positively related to spending inequality. As in column 1, spending inequality is still predicted to widen test score outcomes across family background groups.

An even less restrictive model that includes year-specific state effects is shown in column 3 of Table 6. As noted in the derivation of Eq. (9), time-varying state effects should be included if the degree of sorting of different education groups across school districts includes a component that varies over time within states, or if other unobserved determinants of test scores vary over time within states (such as state-wide curriculum reforms). When state × year effects are included, only the relative effect of the income gradient variable on different parental education groups is identified. Hence, we have normalized the effect of the income gradient variable to be 0 for the middle education group. (We also have to drop the state average expenditure variable). Even though the magnitude of the coefficient on the selection correction term drops somewhat relative to the models in columns 1 or 2, the Mill's ratio term is still highly significant. This finding confirms that our attempt to estimate group-specific SAT participation rates has been relatively successful, since the effect of the selection correction is only identified by differences in the SAT participation rates of different groups within a state in any year.

The effects of higher spending disparities across richer and poorer districts are relatively precisely estimated in column 3, and indicate a statistically significant widening of test score outcomes in states with a higher income gradient of per capita school expenditures. For example, the estimates imply that the greater inter-district spending inequality in Georgia ($\beta_{2j} = 3.89$ in 1992; $\beta_{2j}(I_J/I) = 3.42$) than in Florida ($\beta_{2j} = -0.37$ in 1992; $\beta_{2j}(I_j/I) = -0.31$) contributed to a 22 point widening in the gap in SAT scores between the lowest and highest parental education groups.[35] This is a modest effect relative to 200 point standard deviation of SAT scores across individuals, but a sizeable effect relative to the standard deviation of the gap in scores between the highest and lowest parental education groups across states (about 50 points in 1992). In Section 3 we estimated that school finance reforms in the 12 states that had a court decision overturning their funding systems lowered the income gradient of spending by about 1.1 (see Table 4, column 4). According to the estimates in column 3 of Table 6, this change would be expected to close the gap in SAT scores between children of the most- and least-educated parents in these states by about 7 points.

The results from the final specification in column 4 of Table 6 suggest the estimates in column 3 must be interpreted carefully, however. In column 4 we include group × year, state × year, and group × state effects. The addition of interactions between the state and family background group effects allows for unrestricted state-specific differences in the test-score differentials between different family background groups. This specification is numerically equivalent to a 'difference-in-differences' model in which we first compute the change in test scores for each family background group in each state between the late 1970s and

[35] In the early 1990s the gap in test scores between the highest and lowest family background groups was 174 points in Georgia and 147 in Florida. The two states had fairly similar SAT participation rates.

early 1990s, and then regress the changes on unrestricted state intercepts and the change in the income gradient, allowing group-specific coefficients on the income gradient.

In contrast to the more parsimonious models in the table, the specification in column 4 shows a statistically insignificant selectivity effect. Given the obvious importance of the selectivity process, this finding suggests that the model is over-parameterized relative to the quality of the underlying data (the model includes 266 coefficients). Moreover, although a rise in the income gradient of school spending still has a significant widening effect on the gap in test scores between the highest parental education group and the middle group, the effect on the gap between the middle and lowest parental education groups is insignificant. A cautious conclusion from this specification is that once state-specific differences in the test score gaps between different parental education groups are introduced, it is impossible to isolate an effect of spending inequality on lower parental education groups. Nevertheless, the effect on middle and upper education groups is apparently robust.[36]

A potential issue in the interpretation of the models in Table 6 is the endogeneity of the income gradient of spending across districts in a state. We suspect that endogeneity biases are likely to cause the models in Table 6 to *understate* the effect of spending inequality on the test score gaps across different background groups. Specifically, we suspect that pressure for school finance reforms that equalize the distribution of spending is more likely when test score gaps are rising. In this situation, unobserved factors that cause a widening of the test performance between high and low family background will be correlated with reductions in the cross-district income gradient of spending, causing the estimated income gradient effects for high education groups to be biased downward (toward zero) and those for low education groups to be biased upward (toward zero). Note that this type of endogeneity bias will not be eliminated by using court challenges or funding formula changes as instruments for the income gradient of spending. Nevertheless, as a check, we used court decisions and changes in the presence of the three types of funding formulas, interacted with dummies for the different background groups, as instrumental variables for the interactions of the income gradients with the group dummies. In a specification like the one in column 3 of Table 6, the IV estimates were imprecise, but indicated a marginally significant negative coefficient for the income gradient coefficient of the lowest education group (−1.93, standard error 1.00), a smaller negative effect on the second group

[36] A somewhat more parsimonious model that includes interactions of the background groups with 9 Census division dummies yields a coefficient of −1.23 on the income gradient term for the lowest parental education group (with standard error 0.78), providing modest evidence that a rise in spending inequality raises the test score gap between the middle and lowest parental education groups.

(−1.21, standard error 0.96), a small positive effect for the fourth group (0.64, standard error 0.95), and a small positive effect for the highest parental education group (0.34, standard error 1.12). Using a Hausman test, these estimates are not significantly different from the OLS results. Estimates using only the court decisions are instruments were similar in pattern but even less precise, and essentially uninformative.

We have estimated many variants of the models in Table 6 on a range of different samples. Changes in the sample (such as including or excluding data for states with very small numbers of observations in the SAT data set), or in the sample weighting have little effect on the estimates. Similarly, the use of other functions to approximate the control function and the introduction of group-specific coefficients on the control functions have little appreciable effect on the estimated income gradient terms.[37] We also re-estimated the spending inequality coefficients using several different specifications (e.g., models with no other controls, and models with an extended set of controls for education and household composition in different school districts), and found very similar results.

A final issue we have investigated is whether the equalization of school spending across districts leads to any closing of the gap in the fraction of high school students from different family backgrounds who write the SAT. Since writing the SAT shows an intention to attend college, a relative rise in the SAT participation rate of students from lower parental education groups could be interpreted as evidence of a relative improvement in the quality of schooling. Table 7 presents estimation results for four models similar to those in Table 6, but with the SAT participation rate as a dependent variable. The results for the models in columns 1 and 2 are similar to those for the parallel specifications in Table 6, and suggest that a rise in spending inequality leads to a widening of the gap in the fraction of test writers from different family backgrounds, although the differences for the lowest three family background groups are small and insignificant. The model in column 3, which includes separate state-specific intercepts in each year, also suggests that a rise in spending inequality raises the gap in the test participation rate between upper parental education groups and the middle group, but has no effect on the gap between the middle group and lower groups. Finally, the specification in column 4, which includes state×year and state×group dummies, shows no significant effects of the income gradient term on the relative participation rates of different groups. Overall, the results for test participation are weaker than the results for the mean SAT scores, but lead to broadly similar findings. More parsimonious specifications suggest that a narrowing of spending

[37] Use of the raw SAT participation rates to construct the control function (rather than the smoothed rates), leads to a slightly attenuated coefficient on the correction term but has little effect on the other estimates.

Table 7
Estimated models for log odds of SAT participation by state and family background group (pooled time series cross sections)[a]

	(1)	(2)	(3)	(4)
Mean spending[b]	0.81	−0.67	—	—
(1000s of 1992$)	(0.20)	(0.36)		
Group-specific income gradient terms				
Group 1	0.01	−0.18	0.02	0.53
	(0.12)	(0.19)	(0.20)	(0.40)
Group 2	0.01	−0.16	0.00	0.55
	(0.13)	(0.19)	(0.03)	(0.40)
Group 3[b]	0.03	−0.12	0.0	0.0
	(0.14)	(0.19)		
Group 4	0.24	0.10	0.23	−0.04
	(0.18)	(0.21)	(0.20)	(0.50)
Group 5	0.44	0.27	0.38	0.38
	(0.27)	(0.26)	(0.20)	(0.72)
Other effects: (number in parentheses)	Group × year (9)	Group × year; state (51)	Group × year; state × year (93)	Group × year; group × state; state × year (261)
r-squared	0.35	0.63	0.69	0.87

[a] Models estimated by weighted least squares on sample of 430 observations (43 states, 5 family background groups, 2 years) Dependent variable is log odds of estimated SAT participation rate. Regression weights are number of observations in CPS sample for the state, family background group, and year. Standard errors in parentheses.

[b] With state × year effects, mean spending and one group's income gradient term are absorbed. The effect of the income gradient for group 3 is arbitrarily set to 0.

inequality across districts would narrow differences in test participation across different family background groups, while the least restrictive model is uninformative. Taken together with the results in Table 6, we believe the evidence points to a modest effect of spending equalization on the relative performance of students from more disadvantaged family backgrounds.

5. Conclusions

In this paper we have tried to answer three questions about the recent wave of school finance reforms. The first is whether court decisions declaring a state's financing system unconstitutional lead to any substantive change in the system.

Our answer is yes: we find that in the aftermath of a negative court decision states tend to increase the relative funding available to lower-income districts. The second question is whether shifts in the amount of funding available from state sources lead to any change in the relative spending of richer and poorer districts. Our answer is again yes: our estimates suggest that each additional dollar of state aid received by a school district leads to a 30–65 cent increase in spending. The third question is whether relative shifts in the spending of richer and poorer districts in a state result in relative shifts in the SAT scores of children from more- and less-educated families in the state. Here our answer is more tentative. We believe that the evidence points to a modest equalizing effect of school finance reforms on the test score outcomes for children from different family background groups. Our most precise estimates imply that the spending equalizations that followed unconstitutional court rulings in 12 states over the 1980s closed the gap in average SAT scores between children with highly-educated and poorly-educated parents by about 8 points, or roughly 5 percent. We also find modest evidence that equalization of spending leads to a relative rise in the fraction of students from lower family background groups who write the SAT.

We interpret our findings as complementary with the results in two other recent studies of school finance reform. Murray et al. (1997) find that adverse court rulings led to some reduction in the overall dispersion in per capita spending across districts in the affected states. This is consistent with our finding of a narrowing in the dispersion in spending across richer and poorer districts. Downes and Figlio (1997) find that the relative test scores of high school seniors in low-spending districts rise following both court-mandated and legislatively-induced finance reforms. This is consistent with our analysis of the dispersion in SAT test scores between children of more- and less-educated parents. Much additional research is needed, however, to fully understand the nature and consequences of the school finance reforms that have occurred or are ongoing in many states.

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