LIFTING ALL BOATS? FINANCE LITIGATION, EDUCATION RESOURCES, AND STUDENT NEEDS IN THE POST-*ROSE* ERA

David P. Sims
Economics Department
Brigham Young University
Provo, UT 84602
davesims@byu.edu

Abstract

Rose v. Council for Better Education (1989) is often considered a transition point in education finance litigation, heralding an era of increasing concern for measurable adequacy of education across a broad spectrum of student needs. Prior research suggests that post-*Rose* lawsuits had less effect on the distribution of school spending than older litigation. This article suggests that this focus on the raw resource distribution masks the important effect of contemporary lawsuits in redistributing money to districts with greater student needs. My findings suggest that a successful lawsuit does raise revenues to a variety of districts but provides more money to those districts with higher plausible indications of student needs.

1. INTRODUCTION

In its 1819 decision in *Commonwealth v. Dedham* the Supreme Judicial Court of Massachusetts first articulated a principle that has been a key to the modern development of education finance, namely, "It is the wise policy of the law to give all the inhabitants equal privileges, for the education of their children in the public schools."[1] Although the nineteenth-century development of public education in the United States led to a mostly local system of education finance, the persistent idea that federal and state governments have a responsibility to ensure some level of public education for all students led to extensive court interventions from 1971 to the present that have effectively overridden more than a century of local finance precedents in many states.

This series of lawsuits, spanning more than thirty-five years since the landmark *Serrano v. Priest* case in California (1971), articulated arguments for increased school funding based on state constitutional provisions that require equity in school funding among districts as well as adequate funding for all students. Although equity and adequacy claims often coexist within the same court case, education finance researchers commonly cite Kentucky's 1989 Supreme Court ruling in *Rose v. Council for Better Education* (1989) as an important landmark in the development of legal claims based on adequacy grounds and the beginning of the contemporary era of education finance litigation.[2]

The importance of these lawsuits has led to a large education finance literature that considers the average effect of successful finance litigation on school resources. A common finding of this literature is that more recent plaintiff victories have failed to compress the measured distribution of district resources in a manner similar to earlier lawsuits of the late 1970s and early 1980s. Instead this litigation has resulted in broad funding increases across most districts (Berry 2007; Corcoran and Evans 2007; Springer, Liu, and Guthrie 2009). However, in this focus on the shape of the funding distribution, prior research has largely downplayed the relationship between lawsuit-induced funding increases and observable indicators of student need. Unlike an equity goal, educational adequacy cannot be defined simply by reference to the funding level of other districts, so it is unclear why we should expect lawsuits based on adequacy claims to compress the funding distribution.

This article extends the existing literature on the effect of courtroom victories in the post-*Rose* era (1989–2002) on the distribution of district-level education resources by showing how the increases in district resources from finance litigation correspond to measurable indicators of student needs. In

1. The description of the *Dedham* case comes from the National Access Network Web site, www.schoolfunding.info/states/ma/lit_ma.php3.
2. For example, Corcoran et al. (2004).

other words, it considers whether a rising tide of lawsuit-induced education spending lifts all boats or selectively targets high-need districts.

I find that a disproportionate share of lawsuit-induced resource gains accrues to the districts with the highest fraction of free lunch and special education students, though the targeting of resources to the highest-need districts is imperfect. I also attempt to reconcile this finding of selectivity in the funding distribution with evidence of similar proportional gains across the resource distribution. Due to the complex nature of finance lawsuits, the lessons of this era may prove a better guide to the likely consequences of contemporary legal action than decisions from the 1970s.

The remainder of the article is organized as follows: section 2 briefly examines the most important institutional details and the data, section 3 examines the average resource effects of court decisions in my time frame, section 4 considers the effects of lawsuits on the distribution of resources among districts, and section 5 concludes.

2. BACKGROUND AND DATA

Rationale and Research

Beginning with *Serrano*, over one-third of U.S. states have seen their courts rule that their education finance systems violate state constitutional obligations.[3] Several other states spent time in court and ultimately prevailed. The main legal theory underlying early state court finance cases was based on duties of the state to provide its citizens with an equitable public education. As the prevailing system of local property tax funding for public schools led to a structural funding disadvantage for lower property wealth districts, the most common remedy in school funding cases involved state intervention to break the link between property wealth and school spending through some combination of supplementation and redistribution.

Given the wide availability of data on school resources and expenditures and the measurable nature of claims about funding disparities, it seems inevitable that a literature would emerge to examine the effects of school finance lawsuits on the level and equitable distribution of school resources. Studies by Evans, Murray, and Schwab (1997) and Murray, Evans, and Schwab (1998) use district-level panel data from most states in five-year intervals from 1972 to 1992 to evaluate the average effects of successful finance litigation during this time period. Using various index measures of inequality, they conclude that court-ordered finance reforms reduced within-state inequality by 19–34

3. Although the initial *Serrano* decision was primarily based on the equal protection clause of the U.S. Constitution, the Supreme Court ruling in *San Antonio Independent School District v. Rodriguez* (1973) forestalled this line of argument. Thus the focus of future litigation centered on state constitutions.

percent. This is primarily the result of lower-spending school districts increasing spending vis-à-vis the higher-spending districts. Furthermore, the authors contrast the effects of court-ordered finance reform with legislative reform absent a court mandate, concluding that the latter is ineffective in increasing education resources or reducing resource inequality.

While early court-ordered finance reforms resulted in more education spending and a narrower distribution of spending among districts, it is less clear that the increasing equity in financial resources translated into more equal education. Studies of the link between school finance reform and student outcomes fail to provide a consensus with some documenting positive achievement effects (see, e.g., Downes and Figlio 1998; Card and Payne 2002) and others finding negative or zero effects (see, e.g., Husted and Kenny 1997; Hoxby 2001). Observers, such as Hanushek (2003), raise questions about the weak correlation of raw spending and desired student outcomes, incentive structures that might discourage schools from achieving an efficient conversion of money into student outputs, and failure to account for the differential needs of students across districts.

The idea that education finance should recognize need-based differences across districts and should aim to provide adequate, rather than simply equal, funding for all students had already been advanced in earlier court cases (e.g., *Robinson v. Cahill* 1973; *Pauley v. Kelly* 1979). However, the *Rose* decision is commonly referenced as a watershed moment in education finance litigation because it placed adequacy considerations into the center of the education finance debate. In the *Rose* case the court accepted the plaintiff's argument that the state constitution guaranteed all Kentucky students an adequate education as determined by the court. Since all state constitutions give the state some responsibility for education, the ruling set an important precedent. The adequacy doctrine combined with the expanding standards movement in education to expand the ground on which finance lawsuits were contested. Though lawsuits were still argued under both equity and adequacy grounds post-*Rose*, it seems reasonable that such changes in institutional setting might lead to different litigation effects on the distribution of education resources. Even in the absence of adequacy principles, the effects of court decisions on spending distributions might be different in an environment of evolving awareness of student needs brought on by the movement to increase school accountability.

Indeed, recent research from Corcoran et al. (2004) and Corcoran and Evans (2007) suggests that extending the time horizon of the earlier national studies to include all years from 1972 to 2002 causes the estimates of court finance reform's effect on spending equity to attenuate considerably, though a positive effect on average spending remains. When only the 1989–2004

period is considered, Corcoran and Evans (2007) find that the effect of court rulings on various inequality indices are not significantly different from zero.

Berry (2007) and Springer, Liu, and Guthrie (2009) attempt to differentiate the resource effects of separately categorized adequacy and equity lawsuits, especially in the 1990–2000 period. Using standard inequality indices, the latter concludes that adequacy reforms were less successful on average at reducing inequality than were equity suits. This result is borne out by the findings of Corcoran and Evans (2007), who move beyond the simple use of single indices of inequality to consider the effect of recent lawsuits on some unconditional percentiles of the spending distribution. They conclude that spending increases due to lawsuits look to have increased spending at the median, 5th percentile, and 95th percentile by comparable amounts.

Another branch of the literature looks at the effects of litigation-induced finance reform on specific state spending distributions in the post-*Rose* period.[4] In contrast with the national studies, these articles often find some level of district finance equalization associated with litigation success. Indeed, Clark (2003) and Flanagan and Murray (2004) both find that the *Rose* decision itself led to a substantial increase in state aid for lower-spending districts and a consequent fall in a number of index measures of resource inequality. Dee and Levine (2004) and Downes (2004) find more modest compression in spending due to the reforms in Massachusetts and Vermont, respectively. In general these studies also find an increase in average education spending.

These contrasting results are not necessarily difficult to reconcile. Indeed, given the institutional differences involved across state finance reforms and the large standard errors presented in the national studies, there may be small positive average equalization effects of post-*Rose* lawsuits, or the effects in these particular states may be unusually large.[5]

While both the state- and national-level studies have continued to focus on the equity of resources, the effect of lawsuits on the distribution of spending across districts with different types of students has received less attention. While Manwaring and Sheffrin (1997) and Downes and Shah (2006) argue

4. There are also several single-state studies of older reforms, such as the *Serrano* case in California (Silva and Sonstelie 1995). While valuable, these are not directly comparable to the present study.

5. This state-level literature points out that in national level studies, "any attempt to classify finance reforms will be imperfect" (Downes 2004, p. 290). It is certainly true that studies such as the present one can miss important differences in institutional detail that might modify our prediction of the effects of certain reforms. However, single-state studies have their own attendant problems, most notably constructing a counterfactual based on non–time series variation and obtaining statistical precision (districts in the same state may not be independent observations). Given this, both types of studies have important roles to play, and a synthesis is often informative. This study considers whether court-ordered reform on average results in funding to address student needs. Studies of how individual state reforms attempted to meet student needs would clearly address an important complementary question.

that the nature of state finance reform depends on the demographic profile of the state, and Aaronson (1999) and Downes and Figlio (1999) suggest that finance reform can change that profile, there is still little evidence on whether these reforms systematically increase revenues in line with preexisting district indicators of student need (besides preexisting revenues) beyond the work of Card and Payne (2002), which I discuss later. This study begins to fill that gap by using free lunch status, racial composition, and special education status to measure how district resource gains from finance litigation match student needs.

Although court-ordered reform might well induce a distribution of resources that respects student needs, there are reasons to suspect that this might not occur in practice. For example, while there is some agreement that an adequate education should be based on standards set by the state, there is no uniform standard of how to figure what adequate funding should mean. Though a series of methods for "costing out" has developed, there is sufficient variation in results to support an enormous range in what might be considered adequate spending. This has led critics of these methods, such as Hanushek (2005), to refer to them as "alchemy." This uncertainty about the details of adequate funding might affect the decisions of legislators and state boards of education. While recent court decisions may provide them with greater latitude to determine where to send money to meet student needs, the removal of an equity constraint might instead motivate them to add funding for their local, possibly high-resource, districts as a condition of voting for a finance measure. Alternatively, expected cutbacks in local funding among high-resource districts in response to increased state aid may not materialize.

Table 1 lists states that had a binding school finance court decision between 1989 and 2002 as well as the case name and whether the state or plaintiff prevailed. In a few states where the court reconvened to issue additional reinforcing rulings on the initial case, only the initial ruling is noted. The geographical reach of the finance litigation is striking, with almost two-thirds of U.S. states experiencing a decision in a fourteen-year time period. Also notable is the apparent lack of a clear regional or geographic pattern in state versus plaintiff rulings. Moreover, the balance between plaintiff and state victories appears to have been close to even during this period.

Data

I draw data from a couple of governmental sources. Yearly data on per student revenues and spending for school districts in the United States for school years from 1989–90 to 2001–2 come from the Longitudinal School District

Table 1. Initial High Court Rulings on Education Finance during 1989–2002

State	Case	Year	Decision
Alabama	*Alabama Coalition for Equity v. Hunt*	1993	Plaintiff
	Ex Parte James	1997	State
Alaska*	*Kasayulie v. State*	1999	Plaintiff
Arizona	*Roosevelt Elementary School District No. 66 v. Bishop*	1994	Plaintiff
Arkansas	*Lake View School District N. 25 v. Huckabee*	2002	Plaintiff
Connecticut	*Sheff v. O'Neill*	1996	Plaintiff
Florida	*Coalition for Adequacy v. Chiles*	1996	State
Illinois	*Committee for Educational Rights v. Edgar*	1996	State
	Lewis v. Spagnolo	1999	State
Kansas	*Unified School District No. 299 v. Kansas*	1994	State
Kentucky	*Rose v. Council for Better Education*	1989	Plaintiff
Louisiana	*Charlet v. Legislature of State of Louisiana*	1998	State
Maine	*Maine School District #1 v. Commissioner*	1995	State
Massachusetts	*McDuffy v. Secretary of the E.O. of Education*	1993	Plaintiff
Minnesota	*Skeen v. State*	1993	State
Missouri*	*Committee for Educational Equality v. State*	1993	Plaintiff
Montana	*Helena v. State*	1989	Plaintiff
Nebraska	*Gould v. Orr*	1993	State
New Hampshire	*Claremont v. Governor*	1997	Plaintiff
New Jersey	*Abbot v. Burke*	1990	Plaintiff
New Mexico*	*Zuni School District v. State*	1999	Plaintiff
North Dakota	*Bismarck Public School District No. 1 v. North Dakota*	1994	State
Ohio	*DeRolph v. State*	1997	Plaintiff
Oregon	*Coalition for Equitable School Funding v. Oregon*	1991	State
Pennsylvania	*Marrero v. Commonwealth*	1998	State
	Pennsylvania Assoc. of Rural and Small Schools v. Ridge	1998	State
Rhode Island	*Pawtucket v. Sundlun*	1995	State
South Dakota*	*Bezdicheck v. South Dakota*	1994	State
Tennessee	*Small Schools v. McWherter*	1993	Plaintiff
Texas	*Edgewood v. Kirby*	1989	Plaintiff
Vermont	*Brigham v. State*	1997	Plaintiff
Virginia	*Scott v. Commonwealth*	1994	State
W. Virginia*	*Tomblin v. Gainer*	1995	Plaintiff
Wisconsin	*Kukor v. Grover*	1989	State
	Vincent v. Voight	2000	State
Wyoming	*Campbell County School District v. State*	1995	Plaintiff

*The Alaska, Missouri, New Mexico, and West Virginia decisions were trial court verdicts that were acted on by legislatures before any high court confirmatory rulings. The South Dakota trial ruling was not appealed. The table excludes repeat decisions (returns to court) on the same case (e.g., Claremont III–VII in New Hampshire) listing only the first in a series of high court decisions on funding.

Source: Author's compilation based on his reading of data at the National Access Network Web site and Corcoran and Evans 2007, appendix table 19A.1.

Fiscal-Nonfiscal Detail File (LFNF) maintained by the National Center for Education Statistics (NCES) and collected in collaboration with the U.S. Census Bureau.[6] I combine these data with elements of the NCES Common Core of Data series tracking districts' racial composition, free lunch eligibility, and special education eligibility. The resulting data set is ideal for examining changes in spending while controlling for certain district characteristics that may also change over time.

In order to track responses to school finance decisions I match the above data with the information from table 1 about the timing of court decisions. I generate two dummy variables: one if a district's state has had a plaintiff verdict within my time frame but before the year in question, and a second if the state has had a defendant verdict. Any permanent differences in those states' financing due to court decisions preceding the *Rose* era are accounted for by the inclusion of state fixed effects in my models. I further generate variables that measure the number of years since a verdict and a dummy variable to track states that have experienced a legislature-induced systemic finance reform.[7]

To represent district resources, I use the natural logarithm of per student total district revenues as the dependent variable in most of my analysis.[8] Prior studies (Murray, Evans, and Schwab 1998; Card and Payne 2002) have found that school finance litigation increases both state aid revenues and overall revenues. While my primary interest is investigating how litigation increases total resources and their distribution among districts, regardless of the ultimate source of the funding, I also examine specifications that consider revenues from state sources as the dependant variable. Using revenues rather than current spending also prevents an inconsistent treatment of capital items from the analysis, since the revenue streams used to pay off capital debt may more uniformly approximate the use of capital in education than the varying state cost depreciation methods.

As with all data, there are potential pitfalls in this data set. The NCES identifies 405 district-year observations in the data set that are considered to be financial outliers due to uncommonly high reported per pupil resource figures that vary significantly from prior years. In most cases this is due to large changes in the number of students in the district and a lag in reapportionment of resources. For example, one district loses almost 90 percent of its

6. Irregular districts, such as those run for institutional populations, are excluded from the data by the NCES.
7. For the remainder of the article, school year designations refer to the spring. Thus the 1992 school year began in fall 1991 and concluded in spring 1992.
8. All revenue data are transformed to constant 1990 dollars using the Consumer Price Index before taking logarithms.

students but very little of its funding, resulting in per student financing of over $50,000 for a year. I exclude these observations from the analysis as well as observations for which race, free lunch, and special education data are missing. Since I wish to address the distribution of resources among districts within a state, I also exclude the state of Hawaii and the District of Columbia because each represents a single district.[9] These exclusion standards are actually more conservative in some respects than those in the previous literature.

Another potential pitfall is the presence of sample- as opposed to universe-level data for a few states in three early years of the LFNF record. In the 1991, 1993, and 1994 school years between seven and twelve states provided survey financial data on samples of school districts covering roughly between 20 and 50 percent of the universe. Thus the NCES has imputed values for non-sampled districts in those states for those years. Fortunately, most of these states were unaffected by school finance litigation in these years. Furthermore, some basic testing has shown that excluding the non-sampled district-year observations has little effect on the estimation results, so I leave them in the sample.

Descriptive statistics for the data are provided in table 2. Approximately 22 percent of the districts in my sample have experienced a plaintiff ruling in their state, while only 14.5 percent have experienced rulings that favor the state. Also notable is the small fraction of minority students: 6.9 percent Hispanics and 6.4 percent blacks associated with the use of school district as opposed to student-level averages. Nevertheless an average district has a high proportion of economically disadvantaged students, with a quarter eligible to receive free school lunches. Almost an eighth of the average district's students have an individualized education plan, the contemporary designation for special education status. I also use the modal school locale information in the data to generate dummy variables to mark urban and suburban districts.

3. FINANCE LITIGATION AND AVERAGE RESOURCES

Investigating the distribution of funding gains due to school finance litigation across different types of school districts presumes there is some plausibly causal positive link between court decisions and average school resources. This section demonstrates that court decisions of this era are positively correlated with average district resources and that the variation they induce is plausibly exogenous.

9. There are 183,962 observations in the LFNF file for which there is at least one school and ten students. Of those I lose 405 observations to outliers, 26 from excluding Washington, DC, and Hawaii, 582 observations with missing race information, 5,153 observations with missing free lunch information, and 655 observations with missing special education information. I also exclude 10 observations that report more minority or special education students than total students. This leaves me with $N = 177{,}131$ observations. These exclusions total about 3.7 percent of the original sample.

Table 2. Descriptive Statistics

Variable Name	Mean (s.d.)
Log per student revenues	8.600 (0.332)
Has a court ruling favoring plaintiff (1989–2002)	0.220 (0.414)
Has a court ruling favoring state (1989–2002)	0.145 (0.352)
Has a legislature-initiated reform (1989–2002)	0.073 (0.262)
Students	3,117.17 (13,851.18)
Fraction Asian	0.014 (0.035)
Fraction black	0.064 (0.153)
Fraction Hispanic	0.069 (0.157)
Fraction Amerindian	0.025 (0.105)
Fraction free lunch eligible	0.258 (0.187)
Fraction special education	0.118 (0.055)
Number of schools	6.05 (16.82)
Unified school district	0.762 (0.426)
Elementary/middle school district	0.165 (0.166)
Urban district	0.061 (0.239)
Suburban district	0.218 (0.413)
District 1989 median household income	28,961.00 (11,361.52)

Notes: Revenues are based on constant 1990 dollars. $N = 177{,}131$ except for 1989 median household income, which is calculated from the 150,526 districts with relevant data.

A commonly posited statistical model of the relationship between education resources and finance lawsuits is:

$$R_{ijt} = \alpha_j + \tau_t + \delta P_{jt} + X'_{ijt}\gamma + \varepsilon_{ijt}, \quad (1)$$

where i indexes district, j state, and t year. R is the natural logarithm of per pupil revenues, P is a variable that represents lawsuit decisions, and X is some

vector of district-level controls. Also included are state and year fixed effects to control for permanent state-level differences and common disturbances across states, as well as an error term.[10]

Table 3 presents estimates of this average relationship between lawsuit outcomes and resources. As with all subsequent analysis, the reported standard errors are corrected for clustering at the state level.[11] In addition, all regressions are weighted by the number of students enrolled in a district. My base parameterization of P is a single dummy variable for all district years in a state after a successful plaintiff lawsuit.

Column 1 of table 3, using this parameterization and controlling only for state and year fixed effects, suggests that a plaintiff victory raises subsequent district resources by an average of 6.2 percent relative to all other states. Given an average per student expense during this period of about $5,430, this represents a roughly $335 per pupil increase in revenues. Column 2 adds controls for district characteristics such as number of students, student race, special education, and free lunch status and finds roughly the same coefficient of interest. Of note, districts with high fractions of minority or special education students are associated with higher resource levels, ceteris paribus, while those with high free lunch percentages see less funding.

Column 3 adds controls for other district characteristics such as grade levels included, number of schools, and whether the district is urban or suburban as opposed to rural. Here the measured relationship between plaintiff victories and district resources remains the same. In all three cases the measured effect is significantly different from zero. A comparison of R^2 measures across the three regressions suggests that the addition of controls does not add much explanatory power to the model.

Do the reported coefficient estimates plausibly have a causal interpretation? The key identifying assumption of this model is the absence of omitted variables that affect both P and R. If some other, unmeasured factor leads to both finance lawsuits and resource increases in a particular state, the resulting omitted variable leads to an upward bias in the estimated effect of lawsuit victories on resources. For example, perhaps lawsuits reflect a public desire for more education spending that will come to pass regardless of the court's decision. While the assumption that court decisions may be treated as plausibly exogenous variation in resources is ubiquitous in the literature and is supported by studies stressing the unpredictable nature of court finance rulings

10. Similar models form the basis of much of the national-level literature on court-induced reform, including Murray, Evans, and Schwab 1998, Corcoran et al. 2004, and Corcoran and Evans 2007.
11. This follows the recommendation of Bertrand, Duflo, and Mullainathan (2004) for panel data situations with a high probability of serial correlation.

Table 3. Effect of a Lawsuit Victory on Average District Resources

	Dependent Variable: Natural Logarithm of Total per Pupil Revenues								
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**	**(9)**
Plaintiff win	0.062	0.058	0.058	0.063	0.053	0.052	0.060	0.060	0.050
	(0.019)	(0.019)	(0.019)	(0.019)	(0.018)	(0.019)	(0.019)	(0.019)	(0.017)
Years since					0.001				
					(0.003)				
State win						−0.034			
						(0.023)			
Legislative reform							0.031		
							(0.036)		
Urban district			0.033						
			(0.014)						
Suburban district			0.035						
			(0.013)						
Black		0.264	0.226	0.265	0.264	0.265	0.264	0.215	0.281
		(0.055)	(0.046)	(0.058)	(0.055)	(0.055)	(0.055)	(0.041)	(0.054)
Hispanic		0.217	0.186	0.220	0.217	0.217	0.218	0.167	0.244
		(0.059)	(0.051)	(0.060)	(0.058)	(0.059)	(0.059)	(0.035)	(0.060)
Free lunch		−0.167	−0.127	−0.175	−0.166	−0.167	−0.168	−0.099	−0.201
		(0.050)	(0.039)	(0.052)	(0.050)	(0.051)	(0.050)	(0.059)	(0.051)

Special education		0.455 (0.112)	0.450 (0.118)	0.436 (0.119)	0.452 (0.111)	0.464 (0.112)	0.466 (0.116)	0.564 (0.123)	0.574 (0.104)
State fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	No
State time trends	No	No	No	No	No	No	No	No	Yes
District type/size	No	No	Yes	No	No	No	No	No	No
Median income	No	No	No	No	No	No	No	Yes	No
Full sample	Yes	Yes	Yes	No	Yes	Yes	No	Yes	Yes
R^2	0.60	0.63	0.64	0.63	0.63	0.63	0.63	0.65	0.64

Notes: Standard errors reported in parentheses are corrected for clustering at the state level. $N = 177{,}131$ except column 4, where states with no court decision at all through 2002 are omitted, leaving a sample of 165,285 districts, and column 8, where $N = 150{,}526$ due to lack of data availability for median incomes in some districts.

favoring the plaintiff (see, e.g., Figlio, Husted, and Kenny 2004; Baicker and Gordon 2006), some specification checks with this particular data might help buttress the case.

To test whether states that have lawsuits are systematically different than those that do not, I conduct a number of specification checks. For example, it could be that states that have never experienced a finance lawsuit are fundamentally poor controls to use in estimating the counterfactual. Thus column 4 repeats the regression from column 2 while omitting from the sample all states that have never had a finance ruling as of 2002. This results in only a slight change in the coefficient of interest.

I also test different parameterizations of lawsuit decision measures.[12] Column 5 allows a plaintiff success to change resources through both a one-time shock to the intercept and a change in the slope of future increases, measured as years since the lawsuit victory. The results suggest that the effect of lawsuits is best represented as an intercept shift since the plaintiff dummy coefficient attenuates slightly while the variable measuring the years since a plaintiff decision has a small and imprecisely estimated coefficient. In column 6 I instead add a dummy variable equal to one if there has been a court decision favoring the state in prior years. While the coefficient on plaintiff victories still suggests that they increase funding by 5.2 percent, the results suggest that a state victory actually decreases future funding by about 3.4 percent, although the coefficient is not significantly different from zero. This makes a story linking all lawsuits and finance increases to an omitted factor unlikely.

Column 7 tests the effects of legislative reforms versus those of court-ordered reform. It adds a dummy variable equal to one if there has been a legislature-induced finance reform in that state in prior sample years. The coefficient is positive, although quite imprecisely estimated, and if taken as given would imply an effect that is only about half the magnitude of the court-induced reform. In all three of these specification checks, the inclusion of alternative parameters to control for finance reform does not significantly reduce the effect of a court-ordered reform, which remains between 0.05 and 0.06.

Another possible explanation is that states with a plaintiff victory are already on a different finance trajectory than other states.[13] Columns 8 and 9 consider this possibility. Column 8 adds a 1989 measure of district median income to

12. Berry (2007) and Corcoran and Evans (2007) find that adequacy lawsuits are associated with higher average increases in revenues than equity suits. However, the latter claim that this is not true in the immediate post-*Rose* period. However, the sorting of lawsuits into clear categories is not always straightforward, as Corcoran and Evans (2007), National Access Network (2009), and Springer, Liu, and Guthrie (2009) differ in which lawsuits are considered adequacy versus equity lawsuits.
13. This could be due, for example, to previous finance decisions in some states.

Table 4. Relationship between Lawsuit Decisions and Past Revenues

	Natural Logarithm of Total per Pupil Revenues	
Dependent Variable	**(1)**	**(2)**
Lawsuit decision (at t + 1)	0.0001	
	(0.0167)	
Lawsuit decision (at t + 2)		−0.0148
		(0.0188)

Notes: N = 177,131. The regressions use an indicator variable for future passage only in the relevant year. Standard errors reported in parentheses are corrected for clustering at the state level. Both columns control for state and year fixed effects, district-level student demographics, free lunch, and special education status as well as number of students.

the regression to see if the results can be explained by thus distinguishing high-income districts. Column 9 instead allows each state to follow a separate time trend. In both cases the plaintiff coefficient estimate is quite robust.[14]

Table 4 investigates whether revenues predict a future lawsuit decision. Such a correlation would support a reverse causality story, whereby states experience court-ordered reform because of abnormal levels of school finance. Alternatively it might indicate that changes in educational revenues caused by the mere existence of the lawsuit might predate the final court decision. In each regression of log revenues on district characteristics, future court rulings have been inserted as a dummy variable in the relevant state-year observations. The estimated coefficient of column 1 effectively indicates zero correlation between revenues and lawsuit decisions that will happen one year in the future. Considering lawsuits that will have decisions two years in the future actually produces a negative estimated coefficient for the effect on contemporary revenues, but it is small and statistically insignificant.

While there is a large amount of variation in the duration of finance lawsuits, the key point for identification is that the timing of the reform inducing decision, the court ruling, is not correlated with unobserved factors that drive spending. These regressions show that funding levels do not predict affirmative rulings. The regressions in table 3 also suggest that the plaintiff victory effect on resources is robust to the inclusion of preexisting trends in state spending. While there may be state funding disparities that could affect the timing of lawsuit filings, if they have nothing to do with the timing of the court rulings, the strategy holds.

14. There are significant differences in the estimated time trend coefficients across states (conditional on controls), with a few states registering essentially zero real growth once demographics are accounted for and others with real positive resource trends as high as 3.5 percent. However, their failure to move the lawsuit coefficient suggests that the correlation of plaintiff decisions with base spending trends is low.

Although it is impossible to rule out all sources of bias, these simple specification checks provide more confidence that the average increase in school resources is a real phenomenon and that the table 3 estimates are a good approximation of its magnitude.

4. HOW ARE THE GAINS FROM FINANCE LITIGATION DISTRIBUTED?

Resource Increases and Observable Student Need

The most important previous national study to consider how court finance reform affects districts with variable student needs is Card and Payne (2002). Specifically they sought to determine whether plaintiff victories in lawsuits changed the linear relationship between district spending and median household income. Using district-level data from 1977 and 1992, they found that a plaintiff victory in that time span reduced the size of this coefficient, decreasing the relationship between income levels in a district and district resources, while a state court victory had no effect. To examine whether court rulings direct resources toward particular needs, I adopt an analogous, albeit simpler, framework.[15]

Their choice of a median income measure likely reflected both the data at hand—Card and Payne had two years of data that match up reasonably closely with Census Bureau income estimates—and a lingering focus on funding equity. A median household income measure, through its relationship to median property values, separates property poor and property rich districts. Because education funding was traditionally tied to property taxes, lawsuits might affect spending equity by bridging gaps in spending between districts that arise from differences in property tax base levels.

However, median household income may be less useful as an indicator of student needs. Most importantly, median incomes do not measure a characteristic of students actually in the public schools but rather of all people living in a particular area. Because of this, some districts with high median incomes, such as those in dense urban areas, may have a high proportion of poor or disadvantaged students in public schools.[16] I attempt to address this by measuring the relationship between school resources and three potential measures of student need: the fraction of students in a district eligible for

15. The Card and Payne analysis was done in two stages, allowing each state its own coefficient describing the relationship between district resources and income. However, my decision to cluster correct standard errors at the state level rules out such an approach.

16. Wilson, Lambright, and Smeeding (2006) find that studies that use district averages to calculate the equity of the resource distribution across children overstate the true inequity found in household income data. However, such school-level resource distributions actually understate the inequity in the gap between public spending and student needs.

Table 5. The Relationships among Court-Ordered Reform, Resources, and Demographic Indicators of Need

	All district revenues				
Demographic measure	**Free Lunch**	**Fraction Minority**	**Fraction Black**	**Fraction Hispanic**	**Fraction IEP**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Plaintiff win	0.026	0.063	0.051	0.061	0.013
	(0.024)	(0.027)	(0.024)	(0.020)	(0.039)
Demographic main effect	−0.179	0.260	0.202	0.149	0.361
	(0.050)	(0.056)	(0.046)	(0.068)	(0.109)
Interaction	0.114	−0.020	0.044	−0.030	0.370
	(0.052)	(0.061)	(0.081)	(0.045)	(0.298)
N	177,131	177,131	177,131	177,131	177,131

Notes: The dependent variable for columns 1–5 is the natural logarithm of district revenues. All columns control for state and year fixed effects, district-level student demographics, free lunch, and special education status as well as number of students and district urbanicity. Standard errors reported in parentheses are corrected for clustering at the state level. IEP = individual education plan.

free lunch, the fraction of minority students, and the fraction of students with special education status.

In statistical terms I estimate:

$$R_{ijt} = \alpha_j + \tau_t + \delta P_{jt} + \lambda D_{ijt} + \mu(P \times D)_{ijt} + X'_{ijt}\gamma + \varepsilon_{ijt}, \quad (2)$$

where D_{ijt} is a continuous variable measuring student needs in district i in state j at time t, and I also include an interaction of the need variable D_{ijt} and the lawsuit indicator P_{ijt}. In this framework the coefficient estimate of λ gives the linear relationship between student needs and resources in the absence of a successful finance lawsuit, while the coefficient estimate of μ indicates how a plaintiff victory changes the relationship, and δ represents the effect of a plaintiff victory in a district with a zero value for the chosen demographic measure. Table 5 presents estimates of equation 2 for each demographic need variable. The specification follows the base specification in column 3 of table 3 except for the addition of an interaction term between a particular demographic variable and the plaintiff victory dummy variable.

Column 1 of table 5 considers the fraction of students eligible for free lunch as a measure of the economic status of the students in the district. The estimated coefficient for the fraction free lunch eligible variable suggests a significant negative relationship between districts with high-need students and revenue levels. However, the predicted resource disparity of roughly −0.18 log points between an entirely free lunch district and a district with no free lunch eligible students seems small compared to commonly

held beliefs about massive funding inequality. Nevertheless, coefficient estimates for the interaction term suggest that success in a plaintiff lawsuit mitigates the negative relationship by almost two-thirds. This result is roughly analogous to the Card and Payne estimates for median income and resources.

Although free lunch eligibility might be considered an excellent proxy for student need and is the closest approximation to a true student poverty measure available, it has some potential limitations. Cruse and Powers (2006) highlight data issues with the free lunch measure in the NCES data including district nonparticipation, partial district nonresponse, differential patterns of enrollment, and effort at enrollment and conclude that the resulting prediction error is sufficiently high to preclude their use in making formal school district poverty estimates.

Thus I also present estimates using the fraction of minority students as my demographic measure of need in column 2. Here the effect appears to be reversed. The estimated coefficient on fraction minority is large and positive, suggesting that districts with a high proportion of minorities have larger per student resources. Of course many of these may also be in urban areas with high relative costs for teacher salaries and other expense categories. Most interestingly, while the coefficient is small and not statistically different from zero, the point estimate actually suggests that court reform slightly flattens the positive relationship between minority percentage and log revenues for a district.

One possibility is that minority percentage is actually an amalgam of traditionally disadvantaged groups with other groups such as Asian students that traditionally perform above state averages. Under these conditions it might not serve as a good indicator of student needs. To check this possibility, columns 3 and 4 repeat the analysis using the district fraction of black and Hispanic students as the respective need indicators. In both cases the positive preexisting relationship between minority students and funding levels remains. However, in the case of black students the point estimate suggests that plaintiff finance decisions may have a small positive effect on resource progressivity, though the standard errors are too large to draw firm conclusions.

I repeat the above exercise in column 5 using one other potential indicator of student need, the fraction of students that have an individual education plan (IEP).[17] The resource requirements of these students likely differ in fundamental ways from those of economically disadvantaged students. The results

17. This category is analogous to the traditional category of special education, though it has been broadened to incorporate a wider variety of students with special individual needs.

suggest that districts with a large proportion of IEP students actually receive about 0.36 log points more funding in the absence of a finance lawsuit. The interaction term further suggests that a lawsuit doubles the funding advantage of these districts.

Taken at face value, these regressions suggest that lawsuits likely have a disproportionately large and positive impact on the funding for districts with a higher proportion of free lunch or IEP students. However, just as the estimation of a conditional mean relationship between resources and lawsuits might obscure differential lawsuit effects across the funding distribution, the imposed linearity of the demographic lawsuit interaction term may hide important patterns. Consider, for example, the patterns in minority enrollment among districts, where the transition from very small rural schools to medium suburban schools is forced to have the same effect as the transition to very large urban districts.

To relax this restriction, I replace the plaintiff intercept and linear slope interaction terms from equation 2 with four interactions, one between each quartile of a particular demographic variable and the plaintiff victory dummy variable. Thus each regression from panel A, columns 1–4 of table 6 reports four interaction coefficients, one for each quartile of districts sorted by student need within each state-year combination. Quartile order is increasing in the fraction of free lunch students.

In column 1 an interesting pattern emerges: a plaintiff lawsuit appears to provide more resources to districts in the extremes of the free lunch student distribution compared with those in the center. Thus lawsuits are associated with resource increases of 6–7 percent for districts with extremes of economically disadvantaged students, while those districts in the middle of the distribution of economic need receive only 4 percent increases. A test across all four coefficients rejects the null hypothesis of quartile effect equality, and pairwise tests across coefficients confirm that the top and bottom quartile effects differ significantly from those on middle quartiles. Although the difference in not statistically significant, the point estimates also suggest that schools with the most free lunch students may gain slightly relative to those with the fewest.

Pursuing the same exercise for the fraction of minority students in column 2 suggests, perhaps unexpectedly, that the greatest gains from court reform occur in schools with the fewest minority students. In fact the 10 percent gain in resources for schools in the lowest minority quartile is significantly different from the effects in all other quartiles, 4 percentage points higher than those in the second quartile and around 6 percentage points greater than those in the 3rd and 4th quartiles. While not reported, similar regressions using fraction black or fraction Hispanic students as the need indicator

Table 6. Court-Ordered Finance Reform and Student Need: Nonlinear Effects

	Contemporary Need Measures				1989 Need Measures		
	Free Lunch	Fraction Minority	Fraction IEP	Fraction IEP	Free Lunch	Fraction Minority	Fraction IEP
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
A. Total revenues							
Need main effect	−0.156	0.288	0.385	0.210	−0.114	0.289	0.315
	(0.051)	(0.061)	(0.110)	(0.116)	(0.056)	(0.060)	(0.141)
Plaintiff win x							
Need quartile 1	0.066	0.108	0.042	0.028	0.061	0.094	0.055
	(0.022)	(0.030)	(0.020)	(0.022)	(0.023)	(0.031)	(0.020)
Need quartile 2	0.043	0.064	0.047	0.037	0.051	0.062	0.055
	(0.021)	(0.030)	(0.019)	(0.020)	(0.022)	(0.027)	(0.020)
Need quartile 3	0.044	0.045	0.057	0.050	0.042	0.045	0.055
	(0.024)	(0.021)	(0.021)	(0.021)	(0.023)	(0.021)	(0.022)
Need quartile 4	0.069	0.047	0.102	0.096	0.081	0.043	0.098
	(0.020)	(0.022)	(0.021)	(0.022)	(0.019)	(0.021)	(0.026)
p-value [quartile equality] − F(3,48)	[0.014]	[0.000]	[0.000]	[0.000]	[0.004]	[0.001]	[0.027]
N	177,131	177,131	177,131	148,008	170,248	170,248	170,248
B. State revenues							
Need main effect	0.866	−0.038	0.825	0.545			
	(0.179)	(0.097)	(0.381)	(0.487)			
Plaintiff win x							
Need quartile 1	0.093	0.206	0.018	−0.029			
	(0.058)	(0.047)	(0.048)	(0.065)			
Need quartile 2	0.133	0.121	0.132	0.086			
	(0.054)	(0.042)	(0.039)	(0.057)			
Need quartile 3	0.098	0.081	0.186	0.161			
	(0.046)	(0.042)	(0.051)	(0.063)			
Need quartile 4	0.165	0.126	0.219	0.196			
	(0.102)	(0.059)	(0.070)	(0.078)			
p-value [quartile equality] − F(3,48)	[0.000]	[0.001]	[0.017]	[0.035]			
N	177,131	177,131	177,131	147,887			

Notes: Each panel-column represents the results of a separate regression. The dependent variable for panel A is the natural logarithm of district revenues, except in column 4 where revenues directed specifically to special education categories are omitted. Panel B uses as its dependant variable only the natural logarithm of state revenues to each district. Quartile order is increasing in the respective demographic variable. All columns control for state and year fixed effects, district-level student demographics, free lunch, and special education status as well as the number of students and district urbanicity. Standard errors reported in parentheses are corrected for clustering at the state level. Numbers in brackets are *p*-values for F-tests of coefficient equality across all quartiles. IEP = individual education plan.

also produce decreasing coefficients from quartiles 1–3 with a slight uptick in quartile 4.

Curiously, the only demographic measure where resources increase monotonically with the size of the potentially disadvantaged population is the fraction

of special education. Indeed, the results of column 3 suggest that districts with the most IEP students receive almost 2.5 times the funding increase of those with the fewest such students.

Such a strong result suggests that categorical special education funding may be driving this finding. Thus column 4 repeats the analysis after removing targeted funds for special education from the dependent variable resource measure.[18] The pattern from column 3 persists, while the difference between funding in the top and bottom quartiles is actually larger and remains statistically significant. This suggests that finance lawsuits disproportionately increase funding to districts with large IEP populations in categories beyond targeted IEP funding itself.

One potential worry is that these results are artifacts of incentives that might be embodied in court-ordered finance reform for districts to increase the number of students designated as high need. This might work in a similar way to the pattern of increased disability diagnoses incentivized by high-stakes accountability documented by Figlio and Getzler (2006). For example, if there are financial rewards to having more IEP students, districts might be more likely to give marginal students that designation. A quick check with my data shows that a lawsuit victory has no predictive power on the future reported number of IEP students.[19] As further evidence against this possibility, columns 5–7 of table 6 show that the same pattern of results emerges even when district need indicators from 1989, the beginning of my study period, are used to sort districts into need quartiles. This suggests that district reclassification of students is unlikely to explain my results.

Another explanation for this pattern of results is that the legislative response to court-ordered finance reform operates under political or legal constraints, which make it infeasible to perfectly target increased money to high-need districts. If this is the case, a similar pattern of results should emerge in the effects of court-ordered reform on state revenues to school districts. Panel B of table 6 considers this possibility by substituting revenues from state sources as the dependent variable. The resulting estimates for baseline need effects suggest that state aid follows a different pattern than total revenues in the absence of a plaintiff lawsuit victory. That is, states give much more money to districts with a large proportion of IEP students or free lunch students, while there is little state aid difference across minority status measures. In addition,

18. This includes both federal and state funding for special education and handicapped education as well as any level of revenue categorized as specifically for IEP students. Because these categories are available only in the revenue data following the 1991 school year, the first two years of the FNF data are dropped from the column 3 regressions. This mechanically increases standard errors.

19. The relevant coefficient estimate on the plaintiff dummy is 0.004 with a resulting 95 percent confidence interval of (−0.010, 0.020).

the results suggest that the average effect of lawsuits on state dollars is greater than that on total revenues, as we might expect.

However, many of the effects of student need on the distribution of court-ordered spending increases show patterns similar to those seen with total revenues. While there are some differences from the exact pattern of panel A (e.g., the 2nd quartile of free lunch districts receives more revenue than either of its neighbors, and there is a slight uptick in revenues to the highest minority districts), the results continue to suggest that there is some targeting of money to the highest-need districts but that the targeting is imperfect.

Some of the results of table 6 might be due to the collinear nature of the variables. Perhaps, for example, the slower growth in resources for high minority districts might be an artifact of the greater resources given to districts with high proportions of IEP students. Table 7 considers this possibility by examining the simultaneous effect of court finance reform on districts across two jointly considered demographic measures. The procedure is much the same as above, except instead of forming four interaction effects defined by the quartiles of one demographic variable, each regression considers sixteen effects from a four-by-four grid of two demographic variables.[20] Thus we can look at how lawsuit-acquired resources accrue to high-minority, low free lunch districts versus low-minority, high free lunch districts.

Each panel of table 7 considers the results for the different possible pairings of potential student need indicators in a separate regression, with included level controls for other demographic variables, district urbanicity, and state and year effects, though these are not reported. Panel A, for example, shows that a plaintiff court decision increases resources for districts with the lowest proportion of both free lunch and IEP students by 6.6 percent. However, the effect on the districts with the highest proportion of special needs and free lunch students is significantly greater at 12 percent. While this suggests that post-*Rose* lawsuits promoted some matching of resources to needs, there appear to be more complex patterns at work, as evidenced by the fact that fairly high-need districts in the third quartile of both special education and free lunch students only saw a 3 percent increase in resources. Although this resource change cannot be statistically differentiated from zero, it is significantly less than the resource gains of either the lowest- or highest-needs districts.

Indeed, these results largely support the patterns seen in table 6. With the exception of districts in the lowest quartile of free lunch students, resource gains are monotonically increasing in special education students, and even in this exceptional case the districts with the most special education students see

20. Although it is possible to repeat the same process for a sixty-four-cell grid considering all three demographic variables, the use of state-level clustered standard errors makes this impractical.

Table 7. The Effects of Court Reform on Resources across Joint District Demographic Characteristics

		Free Lunch Quartile						*Minority Quartile*			
	A	1	2	3	4	B	1	2	3	4
IEP quartile	1	0.066 (0.023) [0.026]	0.016 (0.023) [0.000]	0.031 (0.023) [0.000]	0.024 (0.019) [0.000]	*IEP quartile*	0.103 (0.035) [0.949]	0.064 (0.032) [0.127]	0.035 (0.022) [0.000]	0.021 (0.022) [0.001]
	2	0.041 (0.020) [0.001]	0.038 (0.022) [0.000]	0.033 (0.027) [0.000]	0.072 (0.024) [0.021]		0.077 (0.030) [0.287]	0.038 (0.024) [0.002]	0.031 (0.023) [0.000]	0.049 (0.019) [0.002]
	3	0.066 (0.025) [0.031]	0.045 (0.022) [0.000]	0.033 (0.026) [0.000]	0.075 (0.025) [0.016]		0.105 (0.030) [0.996]	0.058 (0.031) [0.059]	0.040 (0.020) [0.000]	0.051 (0.023) [0.015]
	4	0.099 (0.033) [0.501]	0.084 (0.027) [0.033]	0.082 (0.028) [0.031]	0.120 (0.022) [N/A]		0.152 (0.027) [0.067]	0.100 (0.033) [0.852]	0.069 (0.025) [0.059]	0.105 (0.020) [N/A]
		Free Lunch Quartile						*Free Lunch Quartile*			
	C	1	2	3	4	D	1	2	3	4
Minority quartile	1	0.109 (0.033) [0.148]	0.080 (0.037) [0.610]	0.081 (0.030) [0.498]	0.156 (0.029) [0.000]	*Black quartile*	0.114 (0.037) [0.239]	0.097 (0.036) [0.479]	0.086 (0.030) [0.585]	0.128 (0.022) [0.013]
	2	0.062 (0.029) [0.925]	0.051 (0.033) [0.837]	0.057 (0.035) [0.948]	0.091 (0.033) [0.374]		0.086 (0.032) [0.605]	0.050 (0.031) [0.744]	0.054 (0.030) [0.810]	0.071 (0.026) [0.845]
	3	0.036 (0.022) [0.318]	0.036 (0.023) [0.173]	0.044 (0.029) [0.633]	0.069 (0.036) [0.829]		0.051 (0.024) [0.715]	0.029 (0.022) [0.142]	0.039 (0.022) [0.352]	0.048 (0.020) [0.613]
	4	0.066 (0.028) [0.730]	0.028 (0.024) [0.103]	0.030 (0.024) [0.197]	0.059 (0.022) [N/A]		0.042 (0.027) [0.189]	0.037 (0.022) [0.198]	0.036 (0.026) [0.395]	0.064 (0.026) [N/A]

Notes: Each panel reports the results of a separate regression, analogous to those of table 6. Coefficients reflect the marginal resource response of districts to court-ordered finance reform for each cell of a quartile grid in two demographic indicators. The dependent variable is the natural logarithm of district revenues. Quartile order is increasing in the respective demographic variable. All panels control for state and year fixed effects, and level values of other demographic controls. Standard errors reported in parentheses are corrected for clustering at the state level. Numbers in brackets are *p*-values for F-tests of coefficient equality with the highest-need quartile pair in each panel. IEP = individual education plan.

the greatest gains. On the other hand, the free lunch demographic measure generally appears to generate a U-shaped pattern of higher resources for the districts in the extremes of the free lunch distribution than for those in the middle.

Panel B considers the combination of a district's IEP level and fraction minority. While the lowest- and highest-need districts appear to make roughly equal gains by these measures, it is noteworthy that the largest gains by far are made by the districts with the highest fraction of special education students and the lowest fraction of minority students. Although the relationship is

not as clear as that in panel A, it seems generally true that looking along the rows of the table the highest-minority districts receive smaller funding increases than low-minority districts, conditional on special education needs. Looking in the other direction, conditional on the fraction of minority students, districts with a higher fraction of students with IEPs experience larger resource gains.

The evidence of panel C largely confirms these patterns. In most cases there appears to be a U-shaped relationship between free lunch eligible students and district resource gains and a negative relationship between fraction minority and resource gains. The point estimates further suggest that the lowest-need districts receive more resources from finance litigation than the highest-need districts, yet a significantly greater increase is reserved for the districts with the fewest minorities and the most free lunch students. Panel D shows that these relationships persist when need is defined in terms of a particular minority group presence, in this case black students.[21]

Discussion

The effectiveness of post-*Rose* lawsuits in selectively increasing funding to the districts with higher indications of student need appears mixed. Court-ordered reform appears to successfully allocate more resources to schools with more IEP students and does target more resources to districts with the highest free lunch needs. However, it appears that the lawsuits also funnel significant resources to districts with few free lunch students and that the redistribution of resources is negatively related to the fraction of minority students. The greater responsiveness of resources to poverty or special education measures than to race likely reflects legal and political factors that limit the use of race as an explicit component in finance decisions. It is also noteworthy that minority students are almost equally likely to come from a district in the top versus bottom half of the revenue distribution.

While it is possible that the U-shaped pattern of resource response across the distribution of economic need reflects problems with the data or factors unrelated to finance litigation, free lunch eligibility was the most often used criteria for determining at-risk students in state education finance decisions in the 1990s (Thompson and Silvernail 2001). The most likely explanation for this peculiar pattern of resource distribution is rooted in the legislative process. Support for increasing funding to the neediest districts likely requires legislative compromises that raise support, sometimes selectively, for other districts.

21. A regression using Hispanic percentage as the racial need indicator also produces monotonically decreasing effects as Hispanic fraction increases. Regressions using revenues from state sources as the dependent variable produce similar patterns but are much less precise and are not reported.

The estimates of table 6, panel B, tend to support this story of legislative exigency.

The positive resource effect for districts with a high proportion of IEP students is also curious, because the lawsuits considered in this case are distinct from those filed on behalf of special education students.[22] One possibility is that the fraction of IEP students in a district is correlated with other types of student needs or district organization and should be regarded as a broader proxy of need.

However, there is some evidence to suggest that this effect may be real. A periodic analysis of state special education finance systems suggests that six of the eight states with plaintiff decisions in the 1989–1993 period had undertaken major reforms of their special education finance systems by 1994 (Parrish et al. 1997). During the 1995–2000 period several states facing court rulings also reformed their finance systems. Wyoming increased its cost reimbursement from special education funding from 85 to 100 percent, Arizona increased its pupil weights substantially, and states like North Carolina, Ohio, New Jersey, and Alaska chose to adopt completely new funding mechanisms (Parrish et al. 2003). Because many of these programs work through some sort of general or census aid grant, they are likely to show up in these data outside targeted special education funds. In either case, the finding supports the notion that adequacy lawsuits are directing more funding to districts that show objective signs of greater need.

Reconciliation with the Literature

A final issue is reconciling the results that indicate nearly equal resource increases across the spending distribution found by Berry (2007), Corcoran and Evans (2007), and Springer, Liu, and Guthrie (2009) with my results suggesting a shift in funding toward the districts with the highest free lunch and special education needs. I begin in table 8 by showing that these seemingly contrary results are not artifacts of data set differences.

Panel A presents the results of two regressions of state by year-level Gini and Theil indices of spending inequality on a dummy for a plaintiff school finance decision and the control variables used in table 3, column 3.[23] Though

22. There were literally hundreds of special education lawsuits that received judgments in the 1990s (Zirkel 1997). However, these lawsuits in this era are most often filed against a specific district or the state for failing to follow appropriate procedures or to assign the child to the appropriate program rather than over the general state funding of programs. The most notable exception is the Michigan decision *Durant v. State of Michigan* (1997), where the state was ordered to increase funding reimbursements for special education.
23. The derivation of the Gini and Theil indices can be found in Murray, Evans, and Schwab (1998).

Table 8. Effect of a Lawsuit Victory on the Distribution of School Resources

A. Inequality index		
	Gini	−0.0023 (0.0047)
	Theil	−0.0020 (0.0027)
B. Mean effect		0.058 (0.019)
C. Conditional quantile		
	.05	0.062 (0.023)
	.10	0.062 (0.023) [0.97]
	.25	0.065 (0.022) [0.78]
	.50	0.061 (0.023) [0.72]
	.75	0.047 (0.023) [0.13]
	.90	0.072 (0.025) [0.12]
	.95	0.060 (0.034) [0.57]

Notes: Each reported coefficient is from a separate regression. In panel A the regressions reflect state-level observations, and the dependent variable is the stated equality index multiplied by 100. Panel B restates ordinary least squares regression results from table 3, column 3. Standard errors reported in parentheses in panels A and B are corrected for clustering at the state level. Panel C presents the results from a series of quantile regressions for the listed quantiles using log revenues as the dependent variable. Standard errors are derived through a 100 repetitions bootstrap that incorporates the clustered data design. All regressions include student levels, race, free lunch status, and special education controls as well as state and year fixed effects. Numbers in brackets are *p*-values of an F test with null hypothesis that the coefficient equals the coefficient of the preceding quantile.

both coefficients are negative in sign, they are small with relatively large standard errors. By comparison, these coefficient estimates are very close to those of Corcoran and Evans (2007) for the post-*Rose* era and suggest a comparable lack of evidence that these lawsuits compressed the spending distribution.

Another approach in these studies, most notably the recent work of Corcoran et al. (2004) and Corcoran and Evans (2007), uses values for different centiles of the unconditional state spending distribution instead of a state mean

or inequality index as the dependent variable in their regressions. Combining data from the post-*Rose* period with this methodology, the latter study is unable to reject equal positive resource effects at various points in the unconditional resource distribution. Panel C of table 8 presents a slightly different take on the same question. Because I wish to control for the possible effects of potential confounding variables, I use quantile regression to look at the effect of school finance litigation on the conditional quantiles of the resource distribution.[24]

The quantile regression estimates are reported in table 8, panel C.[25] The bracketed numbers are *p*-values for a test of coefficient equality with the preceding quantile. While my methodology differs slightly from those of previous studies, the take-home message is the same. The coefficient estimates for the .05, .10, and .25 quantiles suggest that a court verdict raises resources in the lower tail of the distribution by about 6 percent. However, in percentage terms these gains are nearly identical to the gains at the distribution median and the .95 quantile. From this baseline, the point estimates for the .75 quantile appear to be somewhat lower and those for the 0.9 quantile somewhat higher, though we cannot statistically reject the hypotheses that neither is significantly different from the effects on other quantiles. Thus the evidence seems to favor a story under which court-ordered finance reform post-*Rose* leads to roughly equal gains for the districts at almost all points in the resource distribution.

Table 9 provides a crucial insight in reconciling these findings with my results on student needs. It presents the percentage of districts from each of the 1989 revenue quartiles in my data that fall into a particular quartile of student need. For reference, a completely uniform distribution would have all cell values equal to 25 percent. The missing piece of the puzzle here is the fact that the initial distribution of student needs across revenue quartiles is surprisingly flat. Thus among the highest revenue districts, 26.3 percent of them come from the lowest free lunch quartile while 32 percent of them are from the highest. In no case is a need quartile representative of less than 19 percent of the observations of its revenue quartile, and a full half of the

24. The algorithm proposed by Koenker and Bassett (1978) chooses parameters to minimize the sum, across observations, of absolute deviations between the dependent variable and the linear combination of independent variables and parameters. Each observation is weighted by a check function that scales for the proper quantile and ensures that all deviations are taken in the correct direction. The estimated coefficients from a quantile regression model are commonly interpreted as marginal effects on the conditional distribution of the dependent variable measured at a particular conditional quantile. Thus quantile regression allows me to compare the effect of a lawsuit victory at the top of the conditional resource distribution—say, the 95th quantile—with the effect on the bottom of the distribution—for example, the 5th quantile.

25. The standard errors for the reported quantile regressions are obtained via a 100-iteration bootstrap, where the sampling procedure accounts for the clustered nature of the data. The estimated variance covariance matrix allows testing of coefficient equality across quantiles. For more about interpreting these quantile regression results in education policy contexts, see Eide, Showalter, and Sims (2002).

Table 9. Distribution of Preexisting District Need Given District Revenues

A. Revenues versus free lunch

		Revenue Quartile			
		1	2	3	4
Free lunch quartile	1	28.7%	23.0%	22.3%	26.3%
	2	27.7%	26.7%	25.2%	20.2%
	3	24.4%	28.3%	26.1%	21.4%
	4	19.3%	21.9%	26.4%	32.0%

B. Revenues versus special education (IEP)

		Revenue Quartile			
		1	2	3	4
IEP quartile	1	25.9%	22.6%	22.3%	30.7%
	2	26.2%	26.5%	25.4%	20.7%
	3	26.9%	26.5%	26.9%	19.8%
	4	20.9%	24.4%	25.5%	28.8%

Notes: The table shows the percentage of year 1989 district observations from a given revenue quartile that fall into the indicated quartile of student need. Quartiles are from smallest values (1) to largest (4). Numbers may not sum to 100 percent due to rounding. IEP = individual education plan.

cells in the table are within 2 percentage points of the uniform 25 percent measure. This relatively even distribution across revenue quartiles suggests that selective funding increases for the districts with the greatest needs can be consistent with the findings of minimal changes to the shape of the resource distribution since those high-need districts are drawn from across the resource distribution rather than from one particular section.

5. CONCLUSION

School finance litigation has been a defining feature of the education world for the past thirty-five years. In the wake of the *Rose* decision, the role of adequacy considerations became increasingly important. This article tests whether there is evidence that lawsuits of this era direct resources to districts with plausible indicators of high student need. While I present quantile regression estimates that confirm earlier cross-state research that these post-*Rose* court rulings do not appreciably change the shape of the school resource distribution, I argue that the raw spending distribution as a relative measure of school resources is not the object of primary importance when the goal is providing an adequate education. The idea that adequacy lawsuits are lifting all boats by increasing school resources across the distribution is an overly simplistic description of the situation. Indeed, I show that plaintiff victories appear to lead to a

redistribution of resources that favors districts with the most high-need students. This targeting is imperfect because most districts appear to gain some resources, and districts with the lowest level of free lunch students also capture a disproportionate share. While further research is needed to better understand the legislative mechanism by which this occurs, this study gives reason for limited optimism about the retargeting of resources brought about through school finance litigation.

This article has benefited from the helpful suggestions of Lars Lefgren, Mark Showalter, Eric Eide, participants at the 2007 AEFA Annual Conference, and editorial and referee remarks. I am also grateful for the diligent research assistance of Chad Lee and Biff Jones. Errors remain the author's responsibility.

REFERENCES

Aaronson, Daniel. 1999. The effect of school finance reform on population heterogeneity. *National Tax Journal* 51: 1–29.

Baicker, Katherine, and Nora Gordon. 2006. The effect of state education finance reform on total local resources. *Journal of Public Economics* 90: 1519–35.

Berry, Christopher R. 2007. The impact of school finance judgments on state fiscal policy. In *School money trials: The legal pursuit of educational adequacy*, edited by Martin R. West and Paul E. Peterson, pp. 213–42. Washington, DC: Brookings Institution Press.

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. How much should we trust differences-in-differences estimates? *Quarterly Journal of Economics* 119: 249–75.

Card, David, and A. Abigail Payne. 2002. School finance reform, the distribution of school spending, and the distribution of student test scores. *Journal of Public Economics* 83: 49–82.

Clark, Melissa A. 2003. Education reform, redistribution, and student achievement: Evidence from the Kentucky Education Reform Act. Unpublished paper, Princeton University.

Commonwealth v. Dedham. 1819. 16 Mass. 141, 146 (Mass. 1819).

Corcoran, Sean P., and William N. Evans. 2007. Equity, adequacy and the evolving state role in education finance. In *Handbook of research in education finance and policy*, edited by Helen F. Ladd and Edward B. Fiske, pp. 332–56. New York: Routledge.

Corcoran, Sean P., William N. Evans, Jennifer Godwin, Sheila E. Murray, and Robert M. Schwab. 2004. The changing distribution of education finance, 1972 to 1997. In *Social inequality*, edited by Kathryn Neckerman, pp. 433–65. New York: Russell Sage Foundation.

Cruse, Craig, and David Powers. 2006. Estimating school district poverty with free and reduced-price lunch data. Washington, DC: U.S. Census Bureau, Small Area Estimates Branch.

Dee, Thomas S., and Jeffrey Levine. 2004. The fate of new funding: Evidence from Massachusetts' education finance reforms. *Educational Evaluation and Policy Analysis* 26: 199–215.

Downes, Thomas A. 2004. School finance reform and school quality: Lessons from Vermont. In *Helping children left behind: State aid and the pursuit of educational equity*, edited by John Yinger, pp. 283–313. Cambridge, MA: MIT Press.

Downes, Thomas A., and David N. Figlio. 1998. School finance reforms, tax limits, and student performance: Do reforms level-up or dumb down? Unpublished paper, Tufts University.

Downes, Thomas A., and David N. Figlio. 1999. Economic inequality and the provision of schooling. *Economic Policy Review* 5: 99–110.

Downes, Thomas A., and Mona P. Shah. 2006. The effect of school finance reforms on the level and growth of per-pupil expenditures. *Peabody Journal of Education* 81: 1–38.

Durant v. State of Michigan. 1997. 566 N.W.2d 272 (Michigan Supreme Court).

Eide, Eric R., Mark H. Showalter, and David P. Sims. 2002. The effects of secondary school quality on the distribution of earnings. *Contemporary Economic Policy* 20: 160–70.

Evans, William N., Sheila E. Murray, and Robert M. Schwab. 1997. School houses, court houses and state houses after *Serrano*. *Journal of Policy Analysis and Management* 16: 10–31.

Figlio, David N., and Lawrence Getzler. 2006. Accountability, ability and disability: Gaming the system? In *Advances in microeconomics*, edited by Timothy Gronberg, pp. 35–49. Amsterdam: Elsevier.

Figlio, David N., Thomas A. Husted, and Lawrence W. Kenny. 2004. Political economy of the inequality in school spending. *Journal of Urban Economics* 55: 338–49.

Flanagan, Ann E., and Sheila E. Murray. 2004. A decade of reform: The impact of school reform in Kentucky. In *Helping children left behind: State aid and the pursuit of educational equity*, edited by John Yinger, pp. 195–313. Cambridge, MA: MIT Press.

Hanushek, Eric A. 2003. The failure of input-based schooling policies. *Economic Journal* 113: 64–98.

Hanushek, Eric A. 2005. The alchemy of costing out an adequate education. Paper presented at the Conference on Adequacy Lawsuits, Harvard University, October.

Hoxby, Caroline M. 2001. All school finance equalizations are not created equal. *Quarterly Journal of Economics* 116: 1189–231.

Husted, Thomas A., and Lawrence W. Kenny. 1997. Efficiency in education: Evidence from the states. In *Proceedings of the Eighty-Ninth Annual Conference on Taxation*, pp. 358–65. Washington, DC: National Tax Association.

Koenker, Roger, and Gilbert Bassett. 1978. Regression quantiles. *Econometrica* 46: 33–50.

Manwaring, Robert L., and Steven M. Sheffrin. 1997. Litigation, school finance reform, and aggregate educational spending. *International Tax and Public Finance* 4: 107–27.

Murray, Sheila E., William N. Evans, and Robert M. Schwab. 1998. Education-finance reform and the distribution of education resources. *American Economic Review* 88: 789–812.

National Access Network. 2009. *Litigation*. Available www.schoolfunding.info/litigation/litigation.php3. Accessed 28 April 2009.

Parrish, Thomas B., Jennifer Harr, Jennifer Anthony, Amy Merickel, and Phil Esra. 2003. *State special education finance systems, 1999–2000*. Palo Alto, CA: Center for Special Education Finance.

Parrish, Thomas B., Fran O'Reilly, Ixtlac E. Duenas, and Jean Wolfman. 1997. *State special education finance systems, 1994–1995*. Palo Alto, CA: Center for Special Education Finance.

Pauley v. Kelly. 1979. 255 S.E.2d 859 (WV Supreme Court of Appeals).

Robinson et al. v. Cahill et al. 1973. 303 A.2d 273 (NJ Supreme Court).

Rose v. Council for Better Education. 1989. 790 S.W.2d 186, 60 Ed. Law Rep. 1289.

San Antonio Independent School District v. Rodriguez. 1973. 411 U.S. 1.

Serrano v. Priest. 1971. 487 P.2d 1241 (Cal. Supreme Court 1971).

Silva, Fabio, and Jon Sonstelie. 1995. Did Serrano cause a decline in school spending? *National Tax Journal* 48: 199–215.

Springer, Matthew G., Keke Liu, and James W. Guthrie. 2009. The impact of school finance litigation on resource distribution: Comparing court-mandated equity and adequacy reform. *Education Economics* 17: 421–44.

Thompson, A. Mavourneen, and David L. Silvernail. 2001. *States' provisions of extra funding for economically-disadvantaged students*. Gorham, ME: Education Policy Research Institute.

Wilson, Kathryn, Kristina Lambright, and Timothy M. Smeeding. 2006. School finance, equivalent educational expenditure and the income distribution: Equal dollars or equal chances for success. *Education Finance and Policy* 1: 396–424.

Zirkel, Perry A. 1997. The explosion in education litigation: An update. *West's Education Law Reporter* 120: 369–78.