

**School Finance Judgments and Spending on Education:
A Review of the Evidence**

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Introduction

Beginning with California's *Serrano v. Priest* in 1971, the constitutionality of school finance systems in U.S. states has been under attack for nearly 35 years. During this time, 37 states have had their education funding systems challenged on constitutional grounds. In 25 of these states, the school financing system has been ruled unconstitutional in one or more court challenges. Taken together, these school finance equalization rulings (SFEs) represent perhaps the most important reform movement in American public education since the end of racial segregation in schools with *Brown v. Board of Education* in 1954.

With *Rose v. Council for Better Education* in Kentucky in 1989, SFE challenges changed importantly. Prior to *Rose*, court challenges to school finance systems rested on equity grounds, that is the argument that equality of educational opportunity, as guaranteed in many state constitutions, requires equality of educational resources. Simply put, the equity cases challenged the inequality in spending that favored property rich districts over poor districts in many states. *Rose*, however, ushered in a series of recent cases challenging state funding systems on adequacy grounds. These adequacy cases are based on the argument that an adequate education, as guaranteed in many state constitutions, requires adequate resources. According to this line of reasoning, students with greater "needs" than others may in fact require greater resources than others in order to obtain an adequate education. Although both equity and adequacy cases have usually been grounded in a demand to provide greater resources for poor students, adequacy

cases acknowledge that resources may be distributed unequally, to the extent that high needs students require greater resources to obtain a satisfactory level of education.¹

With the exception of Springer, Liu, and Guthrie (2005), the existing literature on the empirical effects of SFEs has not differentiated between equity and adequacy cases. In this paper, I review the previous studies and re-estimate some their most important results allowing for differential equity and adequacy effects. In addition, I make a small methodological contribution. Because few of the prior empirical SFE studies using panel data have accounted for serial correlation when estimating standard errors, I re-estimate some of the most influential models using state-clustered standard errors, as recommended by Bertrand, Duflo, and Mullainathan (2004). The results indicate that several of the findings from prior studies are based on standard errors that were likely too small.

Review of Empirical Literature on Fiscal Effects of SFEs

Although school finance equalization lawsuits have attracted significant attention in the legal community, and a variety of state-specific studies have been produced, nationwide empirical studies of the effects of SFEs on school finance have been relatively few. The first major study on a national scale was Murray, Evans, and Schwab (1998).² Studying the period from 1972 to 1992, these authors used district-level spending data to compute within-state inequality measures, which they then regressed on an SFE indicator variable and controls. They conclude that SFEs have reduced within-state spending inequality by 19 to 34 percent. They argue that gains in spending equality

¹ See Guthrie (2004) for a discussion of the different underlying conceptualization of resource distribution in equity and adequacy lawsuits.

² A similar paper was published as Evans, Murray, and Schwab (1997).

were achieved by increasing spending in the poorest districts, while leaving spending in the richest districts unaltered. All of the increased spending, they find, is financed by aid from the state government rather than through local sources. In addition, they argue that additional state funding for education resulting from SFEs was financed through higher taxes rather than shifting resources from other functions.

Card and Payne (2002) study the effects of SFEs on the distribution of school spending and student test scores. Using district-level data on school spending and state aid for 1977 and 1992, they find that states redistribute aid in favor of poor districts after SFEs. Specifically, they find that the slope of the relationship between state aid and local income becomes more negative after SFEs. In turn, they show that total spending in poor districts increases relative to rich districts. Card and Payne (2002) also demonstrate that only court decisions in which the school financing system is found unconstitutional have these effects, whereas cases in which a state's system is challenged but upheld have no measurable effects. These authors go on to provide evidence that the equalization of spending resulting from SFEs leads to a narrowing of test scores across family background groups.

Baicker and Gordon (2004) examine how increased state aid for education resulting from SFEs affects total local revenue and spending. Using county-level data for 1982 to 1997, they find that when states increase spending on education as a result of SFEs, they reduce aid to localities for other programs. Baicker and Gordon (2004) estimate that each dollar of increased state aid for education resulted in a reduction of state aid for other purposes of about 20 cents. They also find that local governments respond to increases in state education spending by reducing their own revenue and

spending on education, as well as other programs. These authors estimate that only about 53 cents of each dollar in additional state aid for education ends up in local spending. In other words, Baicker and Gordon conclude, there is evidence that localities “undo” state efforts at increasing education spending after SFEs.

Springer, Liu, and Guthrie (2005) are the first to attempt to differentiate the effects of equity- and adequacy-based SFEs on spending inequality. Using data from 1972 to 2002, these authors undertake an analysis similar to Murray, Evans, and Schwab (1998), but allowing for different effects of equity and adequacy SFEs. The evidence of a difference is mixed. Over the entire study period, the authors do not find a statistically significant difference in the effects of adequacy reform on any of their inequality measures. For the period 1990 to 2000, however, and using a different data set, they do find a significant difference, with adequacy reform having a smaller effect in reducing inequality. However, for the same period, when the authors look at the effects of SFEs in changing the correlation between state aid and local resources needs, measured by the percent of students on free or reduced price lunch, they find no significant difference between equity- and adequacy-based reforms. The authors conclude that the evidence at present is too muddled to make firm conclusions about differential effects of the two types of SFEs.

In an important paper that differs methodologically from the others described thus far, Hoxby (2001) abandons the SFE dummy variable approach in favor of computing economically meaningful parameters of school finance equalization plans on a state-by-state basis. Hoxby (2001) emphasizes that SFEs can have quite different effects depending on the price and income effects they impose, whereas the dummy variable

estimates capture at best the average effects. Depending on the tax price of local spending imposed in the financing system, SFEs may have “leveling up” or “leveling down” effects. In states that make the tax price of additional school spending too high, such as California and New Mexico, students in poor districts may actually end up with less education spending than before the equalization plan was imposed, because of extreme leveling down effects. Hoxby’s (2001) analysis of the unintended consequences of SFEs, on housing values and private school attendance in particular, is another significant contribution. In the context of the present paper, the most important point of Hoxby’s paper (2001) is that the common SFE dummy variable approach masks significant heterogeneity in the experiences of different states implementing court-ordered school finance equalization reforms.

Motivation and Objectives

The motivation of this paper is threefold. First, the various paper on the fiscal effects of SFEs described above use different time periods, data sources, and model specifications, making comparison of results across the analyses less straightforward the one would like. Thus, one objective of this paper is to estimate the most important models in the existing literature using a common time frame and set of control variables. In addition, I will be able to update some of the results that are now a bit dated relative to the pace of SFE litigation, such as Murray, Evans, and Schwab (1998), whose study period ends in 1992.

Second, with the exception of Springer, Liu, and Guthrie (2005), prior studies have not attempted to differentiate the effects of equity- from adequacy-based SFEs. In

part, this is due to the fact that adequacy-based SFEs did not become widespread until the mid-1990s, at the end of the study period for many of the earlier studies. Therefore, I will re-estimate some of the important models from prior studies allowing for differential equity and adequacy effects. Although adequacy-based SFEs are still relatively new, there is now a sufficient track record—177 state-years of experience—to hope to differentiate their effects from equity-based SFEs—which have accumulated 301 state-years of experience.

Third, and most important, recent work by Bertrand, Duflo, and Mullainathan (2004) has underscored the importance of accounting for serial correlation in panel data analyses of the sort that have been used to study the effects of SFEs. Although it has long been known that OLS standard errors are inconsistent when residuals are serially correlated, these authors dramatically demonstrate the possible consequences of ignoring the problem in a typical state panel data analysis. For instance, they randomly generate “placebo laws” in state-level data on female wages and find “effects” significant at the 5 percent level for up to 45 percent of the randomly generated interventions using OLS. Bertrand, Duflo, and Mullainathan (2004) then use Monte Carlo simulations to test various remedies to the serial correlation problem, and find that clustering standard errors by state to allow for an arbitrary autocorrelation process (White, 1984; Arellano, 1987) works quite well and provides an easily implemented solution.³ They recommend that clustered standard errors become the standard practice in applied work.

Unfortunately, of the SFE studies discussed above, only Hoxby (2001) takes account of serial correlation. She uses state-clustered standard errors as recommended by Bertrand, Duflo, and Mullainathan (2004). All of the other studies use OLS standard

³ In STATA, for instance, this correction can be estimated by using the *cluster* option.

errors, ignoring serial correlation. In other words, the standard errors reported in much of the existing literature are likely to be too small, leading to overconfidence in the estimated effects of SFEs. Thus, an important objective of this paper is to re-estimate several of the important models in the SFE literature while allowing for state-clustered standard errors. The results will show whether the relationships identified as significant in earlier studies hold up after accounting for serial correlation in the residuals.

Data and Empirical Strategy

I compiled several data sets for this paper. To begin, I assembled SFE-related variables by state for the years 1970 to 2003. The first is simply a dummy variable set equal to one if the state had a court decision overruling the school finance system on SFE-related grounds in the current year or a prior year. Second, I create two separate dummy variables for equity-based and adequacy-based SFEs. Third, I created a variable counting the number of years since an SFE decision, set to zero for states without an SFE decision. Finally, I created another dummy variable indicating whether the state's school finance system has been upheld as constitutional in an SFE-related court challenge in a prior or the current year.⁴ The information for creating all of these variables is taken from Springer, Liu, and Guthrie (2005), who updated, and in some cases corrected, information contained in Card and Payne (2002) and Baicker and Gordon (2004). A summary of these variables over time is presented in Appendix Table A1.

I next compiled several data sets of fiscal and demographic variables over time. Summary statistics of the main outcome and control variables are provided in Appendix

⁴ Some states faced initial challenged in which the school system was upheld, only to be later ruled unconstitutional. In cases where the financing system is first upheld and later overruled, the upheld dummy variable is switched back to zero at the point when the system is overruled.

Table A2. First, from the National Center on Education Statistics (NCES), I obtained state level data on the sources of public school revenue for the years 1971 to 2002. Published first in the *Digest of Education Statistics* and later in the *Common Core of Data*, these data show the level and proportion of revenue received by public elementary and secondary schools from local, state, and federal sources. I use this data set to analyze the effects of SFEs on state and local funding for education.

Second, from the Census of Governments, I collected a variety of additional state finance data. Published as *State Government Finances*, these data are available as an interrupted time-series starting with 1972 and then annually from 1977 to 2003. I use this data set to analyze the effects of SFEs on own-source revenue of state governments, as well as non-education spending by state governments.

Third, I compiled a district-level data set with a variety of fiscal variables, also from the Census of Governments and covering the years 1972 and 1977 through 2002. I use this data set to compute four within-state measures of inequality in school spending: the gini coefficient, coefficient of variation, theil index, and log of the ratio of spending by the 95th percentile district to the 5th percentile district.⁵ Because several of these inequality measures are highly sensitive to extreme values, I follow Murray, Evans, and Schwab (1998) in using the following algorithm to delete potential outliers. Within each state and year of observation, I identified the 5th percentile and 95th percentile of district in terms of per pupil spending. I deleted any district whose spending was greater than 150 percent of the 95th percentile value or less than 50 percent of the 5th percentile value. In addition, districts were weighted by enrollment in computing the inequality indices.

⁵ These inequality measures are explained and defined in Murray, Evans, and Schwab (1998) and Berne and Stiefel (1983).

These state-level aggregate measures of inequality then become the dependent variables in second stage models, wherein inequality is related to SFE-related variables.

For each of the dependent variables mentioned, the same four basic model specifications were estimated. In the first model, the dependent variable of interest is regressed against a dummy variable indicating whether the state had an SFE-related court decision ruling the school financing system unconstitutional in the current year or a previous year, state and year fixed effects, and demographic controls including proportion of the population aged over 65, proportion of population aged 5 to 17, per capita income and its square, and population and its square. In the second specification, an additional dummy variable is added indicating whether the state's school finance system has been upheld, as described above. If it is the SFE ruling itself that matters, rather than other unobservable characteristics that make a state susceptible to an SFE challenge, then we should be able to demonstrate that decisions overruling the school finance system have different effects from decisions upholding it. In the third model, I use a variable counting the number of years since the school finance system was overruled, rather than the dummy variable. This specification is intended to capture the possibility that SFE decisions take time to have an effect, and so may increase in their impact over time.⁶ In the final model, I use separate dummy variables for equity- and adequacy-based SFE decisions overruling the state's school finance system in the current or earlier years. If the two types of cases have different impacts, we should be able to reject the hypothesis that these coefficients are equal. Some states have had both equity-

⁶ Of course, there is no reason to expect the effects of SFEs to grow linearly over time. Springer, Liu, and Guthrie (2005) estimate some specifications in which they use both years after an SFE decision and its quadratic. However, they never find the quadratic terms to be significant. In the present context, I take the linear specification to capture the idea that the effects grow over some finite time horizon, rather than as a precise depiction of the functional form of this growth.

and adequacy-based SFE decisions, and in principle an interaction term could also be included to estimate to multiplicative effect of having both type of judgments. However, given that there are relatively few observations of states operating after both equity and adequacy SFEs, I have not attempted to estimate this interaction.

Each of the models described above has been estimated twice. First, the models were estimated using robust (Huber-White) standard errors to account for heteroskedasticity, but ignoring serial correlation. The models were estimated a second time using robust, state-clustered standard errors, to account for both heteroskedasticity and serial correlation, as recommended by Bertrand, Duflo, and Mullainathan (2004). In the tables shown below, I report both types of standard errors below the regression coefficients. (The coefficients themselves are unaffected by the type of standard errors used.) Although theory suggests that the state-clustered standard errors are preferred (Bertrand, Duflo, and Mullainathan, 2004), I show both so as to make clear exactly when accounting for serial correlation causes us to be unable to reject a null hypothesis that we otherwise would have rejected. Seeing both forms of the standard errors is also useful in comparing my results with those of prior studies, which have ignored serial correlation.

Aggregate Budgetary Effects of SFEs

Table 1 shows the relationship between SFEs and total funding for public schools. The data are from NCES and represent the revenue received by public elementary and secondary schools from all sources, including federal, state, and local revenue. In Model 1 of Table 1, the SFE indicator—that is, a dummy variable equal to one if the state had its school finance system ruled unconstitutional in the current year or a previous year—has

coefficient of \$261 and an un-clustered standard error of \$62, a relationship that is highly significant statistically. This effect represents about 4 percent of average education revenue in the estimation sample, at \$6900 per pupil. However, the state-clustered standard errors, shown in brackets, are about three times as large as the un-clustered standard errors. In other words, when serial correlation is accounted for by clustering standard errors by state, the relationship between SFEs and total education revenue is no longer statistically significant at conventional levels.

In Model 2, we add a dummy variable equal to one if the state's education system has ever been challenged and upheld. The upheld indicator is not significant under either version of the standard errors. Once again, however, the SFE indicator fails to obtain statistical significance with state-clustered standard errors. Moreover, as shown in the tests at the bottom of Table 1, we are unable to reject the hypothesis that the effect of being overruled and being upheld are equal when standard errors are clustered by state.

Model 3 of Table 1 replaces the SFE indicator with a variable measuring the number of years an SFE has been in place; that is, the number of years since the state's school finance system was found unconstitutional. The point estimate suggests that an SFE increases per pupil education revenue by \$30 per year that it is in place. This relationship is highly significant using conventional standard errors, and remains significant at the six percent level when using state-clustered standard errors. This result suggests that SFEs may take time to have an effect on education funding, and the simple SFE dummy variable, which measures the average effect of SFEs over all years, may understate their long-term impact.

In Model 4 of Table 1, I use separate indicator variables for equity-based SFEs and adequacy-based SFEs, using Springer, Liu, and Guthrie's (2005) classification. The point estimates indicate that equity-based decisions have had a much greater impact on education revenue than have adequacy-based decisions. However, neither of these variables is significant with state-clustered standard errors, nor can we reject the hypothesis that the effects of equity- and adequacy-based reforms are equal, as shown at the bottom of Table 1.

In sum, the results shown in Table 1 demonstrate that accounting for serial correlation by using state-clustered standard errors leads to a substantially lower confidence in interpreting the effects of SFEs on education revenue. Of the various SFE-revenue relationships examined in Table 1, only the years since SFE judgment variable remains significant using state-clustered standard errors, and even this is just shy of the conventional five percent threshold of statistical significance. At the same time, it is worth noting that other variables are also affected importantly by the serial correlation correction. For instance, the proportion of the population over age 65 shows a highly significant negative effect on education revenue in all of the models in Table 1 using conventional standard errors, but is never significant using state-clustered standard errors. In contrast, the proportion of the population aged 5 to 17 is highly significant in all of the models regardless of the form of the standard errors.

Previous studies have found that SFEs have greater effects on funding by state governments as opposed to local governments (Card and Payne 2002; Evans, Murray, and Schwab, 1998) and Tables 2 and 3 examine state and local revenue separately. Again, the revenue data are from NCES and represent funding for public elementary and

secondary schools received from the state government and from local sources, respectively. Table 2 repeats the model specifications shown in Table 1 with state government revenue as the dependent variable. For brevity, the coefficients for the control variables are not shown in Table 2 or subsequent tables. Model 1 of Table 2 shows a positive effect of SFEs on state education revenue, which is statistically significant under either version of the standard errors. States after SFE decisions provide roughly \$450 per pupil in additional revenue. This effect represents about 14 percent of mean per pupil state education revenue in the estimation sample of \$3209. Model 2 of Table 2 reveals a negative coefficient for the upheld indicator. This result suggests that states spend less on education after the school finance system has been upheld as constitutional, although the effect is not significant with state-clustered standard errors. We can, however, reject the hypothesis that upholding and overturning a state's education finance system have the same effects, as shown at the bottom of Table 2. In addition, the years after SFE variable is again highly significant in Model 3 even with state-clustered standard errors, and indicates that states increase education revenue by nearly \$50 per year after an SFE decision. Finally, Model 4 of Table 2, examines equity- and adequacy-based SFEs separately. We find neither to be significant individually, nor can we reject the hypothesis that the two coefficients are equal, after state-clustering. In other words, while we are able to get fairly precise estimates of the overall effects of SFEs on state education revenue, we do not obtain precise estimates for the two types of SFEs individually.

Table 3 repeats the specifications of the prior tables using local education revenue as the dependent variable. The SFE coefficients in Table 3 are generally negative,

indicating that local governments reduce their education funding after an SFE decision, consistent with Card and Payne (2002) and Baicker and Gordon (2004). However, none of the relationships between local education revenue and SFEs estimated in Table 3 remain significant after accounting for serial correlation via state-clustered standard errors. One provocative result is that the upheld indicator in Model 2 of Table 3 carries a positive coefficient, suggesting that court judgments upholding school finance systems lead to lower state funding and higher local funding, exactly the inverse of the results for judgments overruling finance systems. These results are not more than provocative, however, as we cannot reject that the upheld and overturned indicators are equal using state-clustering the standard errors. Also intriguing are the results of Model 4 in Table 3, which show a positive effect of equity-based reforms on local revenue, suggesting that equity-based reforms lead to increases in both state and local revenue, while accountability-based reforms generate higher state revenue but with offsetting decreases in local revenue. These results account for the much larger effect of equity-based reforms on total revenue estimated in Model 4 of Table 1. Unfortunately, however, we cannot draw such conclusions with much confidence, as we are unable to reject that equity- and adequacy-based judgments have the same effect after state-clustering, as shown at the bottom of Table 3.

To summarize the results of Tables 1, 2, and 3, SFEs lead to significant increases in state revenue for public education, but we cannot precisely estimate the effects of SFEs on local education revenue. Given the imprecision in the estimates for local education revenue, it is unsurprising that we cannot confidently estimate the effects of SFEs on total education revenue in Table 1. In other words, we simply cannot be sure as to whether

local changes in education revenue after SFEs partially or completely offset state increases. Based on these results, however, we should be able to show that the state's share of education revenue increases after SFEs, and indeed this is demonstrated in Table 4. Model 1 of Table 4 suggests that the state's share of education revenue increases by about 4 percentage points after SFEs, which represents just under 10 percent of the sample mean state share of funding (47 percent). Model 3 indicates that state governments increase their share of education funding by about 0.4 percentage points per year after an SFE. As shown at the bottom of Table 4, we are able to reject the hypothesis that decisions upholding and overturning school finance systems have equal effects, but we cannot reject that equity-based and adequacy-based decisions have equivalent effects on the state's share of education revenue.

If state governments increase their funding for education as a result of SFEs, where does the money come from? Table 5 examines the relationship between SFEs total own-source revenue of the state government. The revenue data are from the Census of Governments, and represent all revenue of the state government from its own sources—e.g., income and sales taxes, user charges, and such—but excluding intergovernmental aid from the federal government. While the SFE coefficients in Table 5 are generally positive, none of the estimated relationships is statistically significant after using state-clustered standard errors. One surprise is the negative coefficient for adequacy-based reforms in Model 4 of Table 5, although we cannot say with confidence that this coefficient is different from zero.

We next examine the effect of SFEs on non-education state aid to local governments, to explore whether increased education revenue from the state crowds out

other sorts of aid. The revenue data used in Table 6 are from the Census of Governments and represent local governments' total intergovernmental revenue from the state government minus intergovernmental revenue from the state for education. The results shown in Table 6 indicate no significant effect of SFEs on non-education revenue from the state government. Indeed, the only statistically significant relationship estimated in Table 6, using state-clustered standard errors, is a negative effect of decisions upholding the school finance system on non-education revenue, shown in Model 2, which is itself rather puzzling.

As the final analysis in this section, we examine the effect of SFEs on the share of the state's total budget devoted to education in Table 7. The dependent variable is education revenue from the state government (NCES data) divided total general expenditures of the state government (Census of Governments data). Model 1 of Table 7 indicates that the share of the state government's budget devoted to education increases by about 1.7 percentage points after an SFE, and the relationship is significant at the ten percent level with state-clustered standard errors. The hypothesis that court decisions upholding and overruling the school finance system have equal effects on the share of the budget devoted to education can also be rejected at the ten percent level, as shown at the bottom of Table 7. In addition the years after SFE variable in Model 3 is highly significant, under either version of the standard errors, indicating that SFE decisions have increasing effects over time in shifting spending toward education. We are unable to reject the hypothesis that equity- and adequacy-based reforms have equal effects on the share of the budget devoted to education.

The Allocation of State Aid for Education

The one robust finding documented in the preceding section is that SFEs lead to greater spending by state governments on education. The effects of this increased state spending likely depend not just on how much money is spent, but on how it is allocated. Card and Payne (2002) argue that SFEs lead to increased progressivity in state aid for education. That is, they argue that after SFEs the correlation between state aid local income becomes more negative. Specifically, in Card and Payne's (2002) regression of the amount of state education aid received on local income, an SFE dummy, and their interaction, the main effect of income is negative, the main SFE effect is positive, and the interaction term is negative. In this section, I repeat their analysis using a longer time period and allowing for state-clustering of the standard errors, which they do not estimate. Using data from the Census of Governments, I aggregate intergovernmental revenue for education received from the state by all local governments in a county.⁷ With seven time points at five-year intervals from 1972 to 2002, I have just over 20,000 county-year observations. I then regress state education revenue per pupil in a county on county per capita income, SFE-related variables, and SFE/income interactions. The models also include state and county fixed effects. The interaction terms are of primary interest and indicate the extent to which SFEs alter the slope of the relationship between state aid and local income. A more negative slope is taken to signify greater progressivity in state education spending.

⁷ Card and Payne (2002) use school district-specific data for 1977 and 1992. They are able to compute district-level income by matching districts to Census data for 1980 and 1990. Unfortunately, district-level income measures are not available for all the time points in my panel, and so I must aggregate the data to counties in order to match with local income data. I use county-level per capita income data from REIS. The approach of aggregating district data to counties is also used by Baicker and Gordon (2004).

Table 8 presents the analysis of the effects of SFEs on the progressivity of state education spending. In Model 1, all of the variables take the expected signs. The main effect of income is negative, indicating that state aid is targeted disproportionately to low-income areas. The SFE indicator is positive, suggesting that SFEs increase the overall level of state education aid received, which is consistent with the analyses shown in the preceding section. And the SFE-income interaction is negative, suggesting that state education aid becomes more progressive after a court decision ruling the state's school financing system unconstitutional. However, none of these relationships is statistically significant when state-clustered standard errors are used, and in fact the SFE-income interaction is insignificant under either version of the standard errors. Similar results are obtained when the upheld indicator is added in Model 2 of Table 8: the coefficients take the expected signs, but none is significant with state-clustered standard errors. Nor can we reject the hypothesis that the upheld-income and SFE-income interaction terms are equal (not shown).

An important relationship is shown in Model 3 of Table 8. When the years after SFE variable is used rather than the indicator, both its main effect and its interaction with income are highly significant under both versions of the standard errors. The main effect indicates that local governments receive an increase of \$48 per pupil in state aid each year after an SFE ruling. More to the point, the income interaction suggests that the distribution of state aid becomes increasingly progressive after an SFE decision. That the years after SFE variable attains significance where the indicator variable does not may imply that the effects of SFE are increasing over time and therefore the average effect may not approximate the long-term effect of SFEs.

Finally, Model 4 of Table 8 shows that the adequacy-income and equity-income interaction terms carry different signs, suggesting that only adequacy-based reforms produce greater progressivity in state education aid. However, neither of the interaction terms in Model 4 is significant, nor can we reject the hypothesis that the two terms are equal, when state-clustered standard errors are used.

Table 9 repeats the same models from Table 8 using state non-education aid as the dependent variable, which is computed by subtracting local intergovernmental revenue from the state for education from total intergovernmental revenue from the state. Interestingly, the signs of the SFE-related variables generally carry the opposite signs from what was seen in Table 8. In Model 1 of Table 9, for instance, the SFE main effect is negative while its interaction with income is positive, which suggests that local governments receive less non-education aid from the state after an SFE decision, but that it is distributed less progressively. Again, these results fall short of statistical significance when serial correlation is addressed by using state-clustered standard errors. As has become par for the course, neither the equity-based nor the adequacy-based reform variables are significant in Model 4, nor can we reject the hypothesis that they are equal.

As with the analysis of education aid in Table 8, however, the years after SFE variable is highly significant under both forms of standard errors in Model 3 of Table 9. The positive interaction term between years after an SFE and income indicates that state non-education aid becomes increasingly less progressive after an SFE. In other words, states may compensate more affluent areas for relative losses in education aid by

providing additional non-education aid. This provocative results may warrant further analysis in future research.

SFEs and Spending Inequality

The ultimate goal of many school finance equalization law suits is to reduce inequality in per pupil spending. On the other hand, some adequacy-based suits seek to increase resources for needy students without regard for spending inequality per se. This section investigates the effects of SFEs on spending inequality. Using district-level data from the Census of Governments, I compute four measures of within state inequality in per pupil spending: the gini coefficient, the coefficient of variation, the theil coefficient, and the log of the ratio of the 95th percentile district's spending to the 5th percentile district's spending.⁸ I multiply each of these indices by 100 to facilitate presentation of the results. Maryland, North Carolina, and Virginia are omitted from this analysis because they contain no independent school districts as classified by the Census of Governments.⁹ Montana and Vermont are also excluded, following Springer, Liu, and Guthrie (2005). This analysis utilizes a panel of school districts observed at seven time points in five-year intervals from 1972 to 2002. Producing state level summary measures of inequality based on the district-level spending data yields a panel of 42 states at seven time points for a total of 294 observations.

Table 10 presents four models of the effects of SFEs on the gini coefficient of spending inequality. The SFE indicator in Model 1 of Table 10 is significant at $p < .01$ using either version of the standard errors. The point estimate indicates in states with an

⁸ The four indices of inequality are reviewed in Murray, Evans, and Schwab (1998), and discussed in detail in Berne and Stiefel (1983).

⁹ In a future version of this paper, I hope to find an alternative data source for these states.

SFE judgment, inequality is reduced by 1.2 gini points which amounts to 16 percent of the mean gini value in the estimation sample of 7.5. In Model 2, we soundly reject the hypothesis that a court decision upholding the state's school financing system has the same effect as a decision overturning the system, as shown at the bottom of Table 2. However, despite the dissimilar bases of equity and adequacy lawsuits, neither the equity- nor the adequacy-based reform indicator is individually significant, nor can we reject that the two are equal, in Model 4.

The results shown in Table 10 indicate that SFEs have had a significant effect in reducing spending inequality as measured by the gini coefficient. Results of analyses using the coefficient of variation and the theil index of inequality, shown in Tables 11 and 12, yield similar results and will not be discussed separately. The exception is the analysis of the log of 95th/5th percentile spending, shown in Table 13. Although the coefficients carry the same signs as in the other inequality analyses, they are never statistically significant, using either form of the standard errors.

Aside from the anomalous results in Table 13, the preceding analysis suggests that SFEs have produced reductions in within-state spending inequality. However, given the complexity of the inequality indices, it is not clear how the reductions have come about. For instance, a reduction in spending by high-spending districts, without changing spending at the low end of the distribution, would lead to a lower inequality index. On the other hand, starting from a relatively equal distribution of spending, a significant increase in spending at the low end could conceivably increase inequality. In order to better understand how SFEs might reduce spending inequality, I next analyze the effect of SFEs on spending by the 5th, 50th, and 95th percentile districts in each state. Again, the

data are from the Census of Governments and cover the period 1972 to 2002, at five-year intervals. For each state, I identify the district at the 5th percentile of spending for each of the seven time points. And so too for the 50th and 95th percentile districts. I then regress spending at each point in the distribution on SFE-related variables and controls.

Table 14 shows the models of spending at the 5th percentile. The SFE indicator in Model 1 shows a positive effect on spending, which is significant under either version of the standard errors. In Model 2, we easily reject the hypothesis that court decisions upholding and overruling school finance systems have the same effects. However, the years after SFE variable in Model does not approach statistical significance, regardless of the form of standard errors used. In Model 4, we cannot reject the hypothesis that equity-based and adequacy-based reforms have equal effects, as shown at the bottom of Table 14.

Tables 15 and 16 repeat the same sequence of analyses for the median and 95th percentile district's spending. In Models 1 and 2, the SFE effect declines for the 50th percentile district, and again for the 95th percentile, suggesting that SFEs increase spending more at the low end of the distribution. While these point estimates suggest a plausible explanation for how SFEs might lead to a reduction in spending inequality, unfortunately we are just shy of being able to reject the hypothesis that the SFE coefficients in Model 1 for the 5th, 50th, and 95th percentiles are equal. Nor for the years after SFE variable in Model 3, can we reject the hypothesis that the coefficients are equal for the 5th, 50th, and 95th percentiles.¹⁰ In none of the analyses are we able to reject the hypothesis that equity- and adequacy-based reforms have equal effects,

¹⁰ In order to test these hypothesis, I estimated the equations for the 5th, 50th, and 95th percentiles by seemingly unrelated regression and then tested the equality of the SFE coefficients. For Model 1, the chi-

Further Issues

To return to an issue raised earlier in the paper, the results presented above based on SFE-related dummy variables reveal at best the average effects of SFEs across states and over time. As Hoxby (2001) has rightly emphasized, however, all SFEs do not have equal effects. Substantial heterogeneity in state experiences with SFEs is hidden with the dummy variable coefficients in the models presented above. Better understanding the differences in state outcomes is probably a more important line of future research than further work attempting to refine the SFE dummy variable-style models presented here and in much of the earlier literature. For instance, we need to understand the institutional and political factors that cause states to create and apply school finance equalization plans that differ so widely from one another. And finally, regardless of how the fiscal effects of SFEs are estimated, we are left with the question of whether any of the additional spending associated with school finance reform affects its intended target, student achievement.

square statistic is 4.52 with a p -value of 0.105. For Model 3, the chi-square statistic is 4.04 with a p -value of 0.13.

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Table 1

	Total Education Revenue			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	260.95 {62.25} [182.34]	290.17 {72.74} [210.26]		
Upheld indicator		49.39 {50.31} [130.14]		
Years since SFE (Springer)			29.69 {5.39} [15.2]	
Equity indicator				441.09 {89.42} [244.81]
Adequacy indicator				97.74 {68.38} [185.73]
Proportion of the population aged >65	-131.17 {49.03} [141.39]	-131.89 {49.09} [141.5]	-118.49 {52.06} [155.36]	-100.02 {50.73} [144.23]
Proportion of the population aged 5-17	-252.52 {21.76} [43.56]	-254.5 {21.92} [43.83]	-246.42 {21.56} [42.45]	-245.99 {22.27} [46.05]
Per Capita Income (\$1000s)	81.19 {44.42} [102.39]	76.09 {45.06} [103.81]	133.87 {45.89} [103.59]	59.31 {45.16} [103.46]
Per Capita Income Squared	1.95 {0.6} [1.52]	2.02 {0.61} [1.54]	1.05 {0.62} [1.45]	2.13 {0.61} [1.5]
State Population (in millions)	-294.33 {47.68} [137.89]	-291.5 {47.42} [135.47]	-237.76 {48.26} [141.31]	-279.75 {47.34} [132.59]
State Population Squared	4.41 {0.96} [2.56]	4.4 {0.96} [2.53]	2.6 {1.02} [2.71]	4.1 {0.95} [2.46]
Constant	12286.45 {1484.93} [3681.4]	12377.79 {1503.17} [3734.77]	10792.16 {1598.15} [4069.1]	12188.17 {1532.84} [3828.1]
Adj R-squared	0.88	0.88	0.88	0.88
N	1436	1436	1436	1376
F-tests	Overturned = Upheld		Equity = Adequacy	
F stat without state-clustered standard errors	14.8		8.25	
p-value	0.000		0.004	
F stat with state-clustered standard errors	1.78		1.14	
p-value	0.189		0.291	

Data represent annual observations of the 48 mainland U.S. states for 1971-2002, missing 1981.

Robust standard errors in curly braces; robust state-clustered standard errors in brackets.

Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars. Model 4 excludes Montana and Vermont, as explained in the text. Data for Virginia were not reported in 1985-87.

Table 2

	Education Revenue from State			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	452.92 {77.72} [196.03]	379.98 {84.5} [213.79]		
Upheld indicator		-123.22 {48.12} [136.92]		
Years since SFE (Springer)			46.81 {4.32} [9.24]	
Equity indicator				332.44 {73} [205.63]
Adequacy indicator				237.88 {80.38} [167.83]
Adj R-squared	0.72	0.72	0.73	0.74
N	1437	1437	1437	1377
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	40.7		0.6	
p-value	0.000		0.439	
F stat with state clustering	6.18		0.12	
p-value	0.0165		0.729	

Data represent annual observations of the 48 mainland U.S. states for 1971-2002, missing 1981. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars. Model 4 excludes Montana and Vermont, as explained in the text. Data for Virginia were not reported in 1985-87.

Table 3

	Local Education Revenue			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	-201.2 {74.44} [184.24]	-100.11 {81.43} [198.78]		
Upheld indicator		170.88 {51.06} [146.4]		
Years since SFE (Springer)			-18.79 {4.64} [13.09]	
Equity indicator				98.39 {71.11} [162.79]
Adequacy indicator				-135.25 {79.63} [191.85]
Adj R-squared	0.54	0.54	0.54	0.57
N	1436	1436	1436	1376
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	12.7		3.49	
p-value	0.000		0.0619	
F stat with state clustering	1.91		0.62	
p-value	0.174		0.435	

Data represent annual observations of the 48 mainland U.S. states for 1971-2002, missing 1981. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars. Model 4 excludes Montana and Vermont, as explained in the text. Data for Virginia were not reported in 1985-87.

Table 4

	State Share of Education Revenue			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	4.05 {0.86} [1.89]	3.09 {0.91} [2.04]		
Upheld indicator		-1.62 {0.54} [1.37]		
Years since SFE (Springer)			0.39 {0.06} [0.11]	
Equity indicator				1.58 {0.91} [1.72]
Adequacy indicator				2.25 {0.84} [1.81]
Adj R-squared	0.16	0.16	0.16	0.14
N	1436	1436	1436	1376
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	27.43		0.25	
p-value	0.000		0.618	
F stat with state clustering	5.73		0.07	
p-value	0.0207		0.793	

Data represent annual observations of the 48 mainland U.S. states for 1971-2002, missing 1981. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars. Model 4 excludes Montana and Vermont, as explained in the text. Data for Virginia were not reported in 1985-87.

Table 5

	State Government Own-Source Revenue			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	77.91 {20.95} [64.77]	49.91 {25.18} [81.6]		
Upheld indicator		-46.37 {15.95} [48.11]		
Years since SFE (Springer)			5.69 {1.73} [4.24]	
Equity indicator				84.43 {31.14} [97.52]
Adequacy indicator				-18.6 {22.69} [59.84]
Adj R-squared	0.87	0.87	0.87	0.87
N	1536	1536	1536	1472
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	22.03		6.58	
p-value	0.000		0.0104	
F stat with state clustering	2.56		0.65	
p-value	0.117		0.423	

Data represent annual observations of the 48 mainland U.S. states for 1972-2003. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars. Model 4 excludes Montana and Vermont, as explained in the text.

Table 6

	Local Non-Education Revenue from State			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	10.7 {6.21} [14.13]	-3.8 {6.66} [14.69]		
Upheld indicator		-25.57 {6.23} [12.27]		
Years since SFE (Springer)			-1.49 {0.64} [1.28]	
Equity indicator				-6.68 {9.82} [21.94]
Adequacy indicator				7.6 {7.19} [16.26]
Adj R-squared	0.35	0.36	0.35	0.35
N	1248	1248	1248	1196
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	9.67		1.19	
p-value	0.002		0.276	
F stat with state clustering	2.02		0.2	
p-value	0.161		0.655	

Data represent annual observations of the 48 mainland U.S. states for 1972 and 1977-2002. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars. Model 4 excludes Montana and Vermont, as explained in the text.

Table 7

	Education as % State Spending			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	1.72 {0.38} [0.9]	1.81 {0.41} [0.97]		
Upheld indicator		0.15 {0.26} [0.62]		
Years since SFE (Springer)			0.17 {0.03} [0.06]	
Equity indicator				1.02 {0.41} [0.91]
Adequacy indicator				1.30 {0.41} [0.89]
Adj R-squared	0.2	0.2	0.21	0.21
N	1437	1437	1437	1377
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering		18.42		0.17
p-value		0.000		0.677
F stat with state clustering		3.04		0.04
p-value		0.0875		0.845

Data represent annual observations of the 48 mainland U.S. states for 1971-2002, missing 1972 and 1981. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars. Model 4 excludes Montana and Vermont, as explained in the text.

Table 8

	Education Revenue from State			
	Model 1	Model 2	Model 3	Model 4
Income per capita (\$1000s)	-0.0309 {0.0038} [0.0225]	-0.0184 {0.0041} [0.0213]	-0.015 {0.0038} [0.0139]	-0.0327 {0.0039} [0.0218]
SFE indicator	0.2716 {0.0488} [0.2737]	0.3343 {0.049} [0.2642]		
SFE x income	-0.0037 {0.0042} [0.0264]	-0.0184 {0.0044} [0.0234]		
Upheld indicator		0.0289 {0.0368} [0.1405]		
Upheld x income		-0.0143 {0.0031} [0.0105]		
Years since SFE			0.0477 {0.0028} [0.0074]	
Years since SFE x income			-0.0021 {0.0002} [0.0005]	
Equity indicator				0.1348 {0.0559} [0.3974]
Equity x income				0.0005 {0.0048} [0.0381]
Adequacy indicator				0.5544 {0.067} [0.18]
Adequacy x income				-0.0214 {0.0056} [0.0197719]
Constant	1.2948 {0.0359} [0.1972]	1.1851 {0.0377} [0.1903]	1.1587 {0.0353} [0.13]	1.3225 {0.0361} [0.1926]
Adj R-squared	0.70	0.70	0.70	0.71
N	20377	20377	20377	19916

Data represent annual county-area observations for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. All models include county and year fixed effects. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table 9

	Non-Education Revenue from State			
	Model 1	Model 2	Model 3	Model 4
Income per capita (\$1000s)	-0.002 {0.0007} [0.0019]	-0.0036 {0.0008} [0.0023]	-0.0025 {0.0007} [0.0017]	-0.0019 {0.0007} [0.0019]
SFE indicator	-0.0302 {0.006} [0.0244]	-0.0468 {0.0067} [0.0275]		
SFE x income	0.0027 {0.0005} [0.0019]	0.0039 {0.0006} [0.0021]		
Upheld indicator		-0.046 {0.0065} [0.0199]		
Upheld x income		0.0035 {0.0006} [0.0018]		
Years since SFE			-0.0036 {0.0005} [0.0013]	
Years since SFE x income			0.0002 {0} [0.0001]	
Equity indicator				-0.0432 {0.0072} [0.0334]
Equity x income				0.0025 {0.0006} [0.0028]
Adequacy indicator				-0.0114 {0.0072} [0.0177]
Adequacy x income				0.0017 {0.0006} []
Constant	0.1317 {0.0064} [0.0166]	0.1474 {0.0069} [0.0186]	0.1374 {0.0064} [0.0152]	0.1337 {0.0065} [0.0165]
Adj R-squared	0.10	0.11	0.10	0.1
N	20189	20189	20189	19736

Data represent annual county-area observations for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. All models include year and county fixed effects. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table 10

	Education Spending Gini Coefficient			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	-1.2312 {0.3328} [0.3982]	-1.0623 {0.3563} [0.4037]		
Upheld indicator		0.2868 {0.2849} [0.3533]		
Years since SFE (Springer)			-0.0752 {0.0215} [0.0288]	
Equity indicator				-1.0498 {0.4916} [0.6425]
Adequacy indicator				-0.5786 {0.3511} [0.4692]
Adj R-squared	0.76	0.76	0.75	0.75
N	294	294	294	294
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	13.92		0.47	
p-value	0.000		0.492	
F stat with state clustering	8.68		0.28	
p-value	0.005		0.6	

Data represent annual observations of 43 U.S. states for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. Excluded states are Alaska, Hawaii, Maryland, Montana, North Carolina, Vermont, and Virginia. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table 11

	Education Spending Coefficient of Variation			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	-2.1108 {0.6547} [0.7394]	-1.6251 {0.7204} [0.8086]		
Upheld indicator		0.8246 {0.5595} [0.6598]		
Years since SFE (Springer)			-0.1244 {0.0415} [0.0491]	
Equity indicator				-1.1781 {0.9528} [1.1858]
Adequacy indicator				-1.5278 {0.7023} [0.8743]
Adj R-squared	0.67	0.67	0.66	0.66
N	294	294	294	294
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	12.49		0.07	
p-value	0.000		0.792	
F stat with state clustering	9.21		0.05	
p-value	0.004		0.832	

Data represent annual observations of 43 U.S. states for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. Excluded states are Alaska, Hawaii, Maryland, Montana, North Carolina, Vermont, and Virginia. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table 12

	Education Spending Theil Coefficient			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	-0.3754 {0.1205} [0.1157]	-0.2628 {0.1296} [0.1104]		
Upheld indicator		0.1911 {0.1079} [0.1243]		
Years since SFE (Springer)			-0.0117 {0.0088} [0.0098]	
Equity indicator				-0.1233 {0.179} [0.2011]
Adequacy indicator				-0.2828 {0.1334} [0.1469]
Adj R-squared	0.46	0.46	0.45	0.45
N	294	294	294	294
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	12.12		0.4	
p-value	0.001		0.529	
F stat with state clustering	10.4		0.31	
p-value	0.002		0.578	

Data represent annual observations of 43 U.S. states for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. Excluded states are Alaska, Hawaii, Maryland, Montana, North Carolina, Vermont, and Virginia. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table 13

	ln(95th/5th Percentile Spending)			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	-0.0486 {0.0411} [0.0378]	-0.0243 {0.0457} [0.0374]		
Upheld indicator		0.0412 {0.0403} [0.0443]		
Years since SFE (Springer)			0.0018 {0.0037} [0.0044]	
Equity indicator				0.0407 {0.0625} [0.0765]
Adequacy indicator				-0.0651 {0.0449} [0.0412]
Adj R-squared	0.37	0.37	0.37	0.37
N	294	294	294	294
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	2.06		1.2	
p-value	0.15		0.274	
F stat with state clustering	1.9		0.97	
p-value	0.176		0.33	

Data represent annual observations of 43 U.S. states for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. Excluded states are Alaska, Hawaii, Maryland, Montana, North Carolina, Vermont, and Virginia. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table 14

	5th Percentile District Spending			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	0.1053 {0.0309} [0.0317]	0.0969 {0.0333} [0.0359]		
Upheld indicator		-0.0141 {0.0318} [0.0417]		
Years since SFE (Springer)			0.0025 {0.0029} [0.0032]	
Equity indicator				0.1019 {0.0505} [0.0536]
Adequacy indicator				0.0508 {0.0334} [0.036]
Adj R-squared	0.79	0.79	0.79	0.79
N	294	294	294	294
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	9.76		0.49	
p-value	0.00		0.483	
F stat with state clustering	7.94		0.55	
p-value	0.007		0.464	

Data represent annual observations of 43 U.S. states for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. Excluded states are Alaska, Hawaii, Maryland, Montana, North Carolina, Vermont, and Virginia. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table 15

	Median District Spending			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	0.0756 {0.0161} [0.0248]	0.0774 {0.0181} [0.026]		
Upheld indicator		0.0032 {0.0151} [0.0219]		
Years since SFE (Springer)			0.0041 {0.0013} [0.0017]	
Equity indicator				0.0962 {0.0198} [0.0253]
Adequacy indicator				0.0352 {0.0176} [0.0285]
Adj R-squared	0.96	0.96	0.96	0.96
N	294	294	294	294
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	17.82		4.32	
p-value	0.00		0.039	
F stat with state clustering	7.19		1.8	
p-value	0.011		0.187	

Data represent annual observations of 43 U.S. states for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. Excluded states are Alaska, Hawaii, Maryland, Montana, North Carolina, Vermont, and Virginia. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table 16

	95th Percentile District Spending			
	Model 1	Model 2	Model 3	Model 4
SFE indicator	0.0454 {0.0329} [0.0409]	0.0589 {0.0323} [0.0465]		
Upheld indicator		0.0229 {0.0281} [0.0315]		
Years since SFE (Springer)			0.0004 {0.0018} [0.0022]	
Equity indicator				0.0737 {0.0351} [0.0516]
Adequacy indicator				0.0153 {0.0387} [0.0438]
Adj R-squared	0.93	0.93	0.92	0.93
N	294	294	294	294
F-tests				
	Overturned = Upheld		Equity = Adequacy	
F stat without state clustering	0.89		0.98	
p-value	0.35		0.323	
F stat with state clustering	0.73		0.65	
p-value	0.399		0.425	

Data represent annual observations of 43 U.S. states for 1972, 1977, 1982, 1987, 1992, 1997, and 2002. Excluded states are Alaska, Hawaii, Maryland, Montana, North Carolina, Vermont, and Virginia. Robust standard errors in curly braces; robust state-clustered standard errors in brackets. Regressions include year and state fixed effects, percent of population over 65, percent of population aged 5 to 17, per capita income, per capita income squared, population, and population squared. All dollar figures are in thousands of per capita, real year 2004 dollars.

Table A1

Cumulative Number of States in which the School Finance System was Ruled Unconstitutional				
Year	Overturned	Overturned on		
		Equity Grounds	Adequacy Grounds	Grounds
1970	0	0	0	0
1971	1	1	1	0
1972	2	2	2	0
1973	3	3	3	0
1974	3	3	3	0
1975	3	3	3	0
1976	4	4	4	0
1977	5	5	5	0
1978	6	6	6	0
1979	7	7	7	0
1980	8	8	8	0
1981	8	8	8	0
1982	8	8	8	0
1983	9	9	9	0
1984	9	9	9	0
1985	9	9	9	0
1986	9	9	9	0
1987	9	9	9	0
1988	9	9	9	0
1989	12	10	10	1
1990	12	10	10	2
1991	12	10	10	2
1992	12	10	10	2
1993	17	12	12	6
1994	18	12	12	7
1995	18	12	12	9
1996	18	12	12	11
1997	22	12	12	14
1998	23	12	12	15
1999	23	12	12	15
2000	24	12	12	16
2001	24	12	12	16
2002	24	12	12	17
2003	25	12	12	19
2004	25	12	12	20

Source: Springer, Liu, and Guthrie (2005). Note that a state may have had its system overruled on both equity and adequacy grounds. Excludes Alaska and Hawaii.

Table A2: Summary Statistics

	1972*			1982			1992			2002		
	Mean	Std. Dev.		Mean	Std. Dev.		Mean	Std. Dev.		Mean	Std. Dev.	
Outcome Variables												
Total education revenue per pupil	4664.05	980.59		5717.45	1354.30		7526.78	1645.56		9421.96	1846.12	
State education revenue per pupil	1786.43	645.23		2667.56	824.69		3433.33	987.62		4637.25	1269.36	
Local education revenue per pupil	2401.63	1068.35		2639.98	1328.56		3569.51	1588.63		3953.62	1538.19	
State share of education revenue	39.10	13.34		47.71	13.79		46.63	13.13		49.49	10.71	
State own-source revenue per capita	1533.29	286.27		1780.71	525.25		2347.54	474.53		2745.95	529.46	
Local non-education revenue from state per capita	209.55	193.30		222.65	178.47		280.75	208.74		324.07	218.58	
Education revenue as percent state spending	19.99	5.84		20.42	5.77		18.37	4.73		18.44	3.71	
Gini coefficient (x100) of per pupil spending	7.64	2.87		8.09	2.93		7.68	2.96		6.81	2.24	
Coefficient of variation (x100) of per pupil spending	14.57	5.63		15.28	5.64		14.79	5.84		13.37	4.67	
Theil coefficient (x100) of per pupil spending	1.25	1.28		1.27	0.85		1.17	0.89		0.92	0.65	
ln(95th percentil spending/5th percentile spending)	0.63	0.61		0.57	0.24		0.55	0.22		0.52	0.20	
Control Variables												
Percent of population over 65	10.04	1.67		11.51	1.83		12.81	1.79		12.62	1.60	
Percent of population aged 5 to 17	25.46	1.25		20.00	1.23		18.85	1.93		18.31	1.03	
Income per capita (\$1000s)	19.01	2.62		20.72	2.73		25.74	3.78		31.18	4.55	
State population (millions)	4.30	4.43		4.79	4.89		5.27	5.67		5.95	6.43	

All dollar figures have been adjusted to 2004 dollars using the CPI.

*Total, state, and local education revenue, as well as state share of education revenue, are reported for 1971, as 1972 data were not available.