

Reforms, Tax Limits, and the Distribution of School Expenditures: A Quantile Approach

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Abstract

A quantile regression approach is used to analyze the effects of both school finance reform and tax limitation measures on the distribution of expenditures per student across U.S. school districts from 1970 to 1990. Passed singly, reforms lead to a small reduction in expenditure per student in high-spending districts, with some tendency toward increased spending among low-spending districts. When passed together, finance reforms and tax limits have a marked effect on the distribution of expenditures per student, lowering expenditures in all districts, with disproportionate reductions in high-spending districts.

1. Introduction

Local jurisdictions traditionally have been responsible for school funding in the United States. With local jurisdictions relying heavily on property taxes to fund expenditures, the tax base for school expenditures tends to be much higher in areas with high property values. The resulting disparities in school spending have led to legal challenges to the constitutionality of educational finance systems throughout the country, with more than two thirds of the states now having some combination of statutory and legislative school reform. In general, the objective of the reforms is to limit, centralize, and equalize funding across districts (Evans, Murray, and Schwab, 1997; Figlio, Husted, and Kenney, 2004). The primary objective is typically to influence the *distribution* of expenditures by reducing inequality across districts; whether the ultimate level of spending increases or decreases on average is apt to be a secondary issue.

School finance reforms can reduce inequality in various ways. For example, expenditures could be increased in all districts with a relatively larger increase in poorer districts or expenditures could be decreased in all districts with a relatively larger decrease in wealthier districts. Hoxby (2001) characterizes the former pattern as “leveling up” and the latter as “leveling down.” She finds that extreme equalization measures – those aiming for near equality of per-pupil spending – lead to leveling down. Indeed, she concludes that, “students from poor households would actually have better funded schools if their states did not attempt such complete equalization” (Hoxby, 2001, p. 1228). Hoxby also concludes that less drastic equalization schemes have either effect, with moderate leveling down being the most common pattern in practice.

A second prominent form of de facto school finance reform has been tax limitation measures, which had been passed in 43 of the 48 contiguous states as of 2005. In contrast to

school finance reforms, tax limitation measures are explicitly designed to reduce or slow the growth in property taxes. Thus, the primary objective is to lower expenditures, leaving equalization as the secondary issue. While tax limitation measures may or may not reduce inequality, they clearly are expected to decrease average expenditure levels across school districts, a finding that is borne out in empirical studies (e.g., Dye and McGuire, 1997; Dye, McGuire, and McMillen, 2005; Figlio, 1997; Merriman, 1987; Rueben, 2000).

The empirical evidence on school finance reforms is somewhat mixed, with most studies concluding that equalization measures have tended to increase average spending per student, with disproportionate increases among low-income districts. Using an annual panel of 50 states for 1970-1990, Downes and Shah (2006) find that per-pupil expenditures increased on average following legislative reforms in school finance systems, while expenditures decreased following court-ordered reforms. Although the direct effects of reforms were statistically insignificant, reforms led to statistically significant changes in the coefficients of other determinants of expenditures. Also using an annual panel of states for 1970-1990, Manwaring and Sheffrin (1997) find that both legislation and reforms increased spending per student on average. Other studies have focused more directly on relative spending levels across jurisdictions within states. Using data for more than 16,000 school districts, Evans, Murray, and Schwab (1997), Murray, Evans, and Schwab (1998), and Corcoran et al. (2004) find that local revenues were “roughly unchanged” following court-ordered school finance reforms while state-level funding increased. Total revenues increased among low-income school districts while remaining unchanged in wealthy districts, i.e., “leveling up.” Card and Payne (2002) find that states increased the relative funding of low-income school districts after their school finance systems were declared unconstitutional in the 1980s, and that the increase in funding led to a disproportionate increase

in spending among low-income districts. Focusing on California, Fernandez, and Rogerson (1999) find that an increase in spending among low-income districts came at the expense of large reductions in wealthier districts.

The main reason that results differ somewhat across studies of school finance reforms is that different equalization schemes have different implications for expected relative spending levels. California – the most studied and most prominent example of a leveling-down state – is an extreme case, requiring near complete equalization across districts. Since wealthy districts cannot add to school expenditures by increasing local tax rates, funding has, in effect, been transferred to the state. Whereas average expenditure levels in the state may once have been determined by the averages of the levels desired by median voters in each district, they now are likely to reflect the much lower level desired by the median voter for the *state* (Silva and Sonstelie, 1995). Most equalization programs are more moderate, and are less likely to lead to severe leveling down effects. However, in contrast to the findings of some of the studies cited above, Hoxby (2001) estimates that only 8 of the states have even modest levels of “leveling up,” i.e., general increases in spending with disproportionate increases among low-income districts. Hoxby characterizes the remaining 39 states in her sample as leveling-down states.

Although the intention of most school finance reform is explicitly to alter the distribution of school expenditures across districts, the statistical measures used to characterize the distributional changes are limited. Indeed, studies such as Downes and Shah (2006) and Manwaring and Sheffrin (1997) focus exclusively on changes in average expenditures without explicitly considering other parts of the distribution. Card and Payne (2002) estimate the response of state funding per capita and expenditures per capita to income, and find that the combination implied a net transfer of state revenue from higher to lower income districts

following school finance reforms. Evans, Murray, and Schwab (1997), Hoxby (2001), Murray, Evans, and Schwab (1998), and Corcoran et al. (2004) measure distributional changes across school districts through simple summary statistics, including coefficients of variation, ratios of expenditures across various percentiles of the distribution (e.g., the 5th percentile and the 95% percentile), Gini coefficients, and Theil indices. Similarly, Figlio, Husted, and Kenny (2004) show that voter preferences, the tax and political structure within the state, and the legal standing of education within the state constitution can explain variation in several school-expenditure inequality measures. Nonetheless, none of these papers directly compare the effects of school finance reforms and tax limitations on the actual distribution of school expenditures.

This paper differs from previous work by using a quantile regression approach to explicitly analyze the effects of both school finance reforms and tax limitation measures on the distribution of school expenditures across the entire country. The quantile approach provides estimates of the effects of reforms and tax limits on the full distribution of expenditures rather than relying on a few simple summary statistics to characterize distributional changes. Thus, the approach provides a much more complete picture of the changes in the distributions of expenditures over time. Our analysis covers the entire country rather than focusing on individual states. This approach seems natural since the movement toward tax limits and reforms was so pervasive during the 1970-1990 interval covered by our data set. With nearly all states having adopted some combination of reforms and limits, it is important to know whether the changes significantly altered the overall distribution of expenditures per student.

Using 1970 as a base year with neither finance reforms nor tax limits, we present graphs showing a counterfactual distribution of expenditures – the distribution in the absence of either reforms or tax limits – across all school districts in the country in 1980 and 1990. We then

compare the distributions to the estimates (1) with tax limits, (2) with school finance reforms, and (3) with both limits and reforms. We find that tax limits without reforms have a modest dampening effect on the growth in expenditures per student, while reforms without limits have a quite small but statistically significant tendency to increase spending in low-expenditure districts while decreasing expenditure in high-expenditure districts, other things being equal. Only when limits and reforms are combined is there a meaningful change in the distribution of per-student expenditures. Together, limits and reforms serve to reduce expenditures, with disproportionate reductions among high-expenditures districts. Thus, the combination of limits and reforms lead to “leveling down,” whereas neither policy alone has a large effect on the distribution of expenditures.

2. The Data

The data set for the study, which was generously provided to us by Sean Corcoran and Robert Schwab, has previously been used in studies by Evans, Murray, and Schwab (1997); Murray, Evans, and Schwab (1998); and Corcoran et al. (2004). It is unique in its combination of district-level data on expenditures with demographic data from the U.S. Census. A detailed description of the data collection process can be found in Harris (1999). The panel data set has a distinct advantage over such alternative data sources as the Common Core of Data (CCD) because it includes information on real expenditures per student for all U.S. districts for 1972, a time that pre-dates the wave of tax limitation measures, legislative reforms, and court-ordered school reforms that largely occurred in the 1970s and 1980s (Sokolow, 1998; Payne and Card, 2002; Anderson, 2006). We present estimates of the determinants of the distribution of real students per expenditures across the nation’s school districts in 1970, 1980, and 1990. The

determinants of expenditures (discussed below) are drawn from the 1970, 1980, and 1990 U.S. Censuses.

An advantage of this unique data set is that it permits a comparison of district level expenditures per student among four major groups: (1) states with school finance reforms, (2) states with tax limits, (3) states with both reforms and limits, and (4) a base group of states that did not adopt either reforms or limits through 1970-1990. The control group with neither reforms nor limits is straightforward. The comparison groups are more difficult to classify, with somewhat surprising differences in classifications among published studies. We adopt the inclusive list of 40 states that adopted tax limits and/or educational reforms that are listed in Sokolow (1998), Downes and Figlio (1998), and Mullins and Wallen (2000). Table 1 shows the years when tax limitation measures and/or school finance reforms were enacted in each of the 48 contiguous states. The seven states with neither reforms nor limits during this period include Delaware, Nebraska, Nevada, New York, North Carolina, Oregon, and Pennsylvania. Twelve states adopted both reforms and limits at some point between 1970 and 1980, while 18 adopted only finance reforms and 11 adopted only tax limits. Approximately 8900 school districts are represented across the 47 states included in the analysis.¹ The natural logarithm of real expenditure per student serves as the dependent variable for all of our regressions, with the school district as the unit of analysis.

We include three variables indicating that states have adopted either limits, reforms, or both by the time in question. For example, if a state adopted a reform in 1981, the variable labeled “Reform in Place” equals zero in 1970 and 1980, and switches to one in 1990. However,

¹ Montana was dropped from the analysis in Corcoran et al. (2004) because it has few unified districts, and it was not included in the data set sent to us. Missing observations in some years prevent the data set from being a balanced panel. The exact number of districts in 1972, 1982, and 1992 with complete data is 8889, 8922, and 8910.

Table 1 reveals much variation in the times when the limits and reforms are adopted, and we might expect a reform adopted in 1981 to have more effect by 1990 than a reform adopted in 1988. Thus, we follow Murray, Evans, and Schwab (1988) and include controls for the number of years that reforms, limits, or both have been in place. To be concrete, consider the case of South Carolina, which adopted reforms in 1977 and limits in 1985. The variables representing the effects of these policies appear as follows in our model:

Variable	1970	1980	1990
Reform in Place	0	1	1
Tax Limits in Place	0	0	1
Both Reforms and Limits in Place	0	0	1
Years since Reform in Place	0	3	13
Years since Limits in Place	0	0	5
Years since both Reforms and Limits in Place	0	0	5

Unlike previous studies, this model specification allows us to distinguish between the effects of reform and limits on expenditures. It also allows us to determine whether the reforms and limits have more effect on expenditures when combined than when only one of the policies is adopted.

Our approach does not distinguish between court-ordered reform and legislative reforms; nor do we attempt to control for various characteristics of the reforms and tax limit policies. Although our approach is consistent with much of the empirical literature, it differs from such authors as Downes and Shah (2006) and Hoxby (2001), who attempt to control for the variety of reforms adopted across the United States. The simple discrete-variable approach is a useful starting point for an analysis that distinguishes between reforms and tax limits using a new empirical approach, and it allows us to determine whether broad sets of policies have had a discernible effect on the distribution of expenditures per student. Moreover, most policies do not

change substantially once they have been put in place. To control for time-invariant variation in policies across states, we include state-level fixed effects in all of our empirical models. Since the policies under consideration are adopted at the state level, the state fixed effects will control for differences in policies across states.

Our specification of the remaining explanatory variables for real expenditures per student follows Harris, Evans, and Schwab (2001). The variables are drawn from the U.S. Census Bureau's Census of Government School System Finance (F33) File for 1972, 1982, 1992, with the underlying tract-level data matched to school districts. The explanatory variables are listed in Table 2, along with descriptive statistics for each sample year. All of our empirical models are estimated using the pooled data set, with fixed effects for states and year.

3. The Empirical Model

The base empirical model is:

$$y_{it} = X_{it}\beta + \sum_{j=1}^3 (Z_{j,it}\gamma_j + D_{j,it}\delta_j) + \epsilon_{it} \quad (1)$$

where y_{it} represents real expenditures per student in district i at time t , where $t=1970, 1980, 1990$; and X_{it} is a vector of explanatory variables, including demographic variables, state fixed effects, and year fixed effects. $Z_{1,it}$, $Z_{2,it}$, and $Z_{3,it}$ are dummy variables indicating that a district is in a state that has adopted (1) school finance reforms, (2) tax limits, or (3) both reforms and limits as of time t . $D_{1,it}$, $D_{2,it}$, and $D_{3,it}$ represent the number of years that (1) reforms, (2) limits, or (3) both reforms and limits have been place; these variables equal zero if the policy has not yet been adopted as of time t .

Standard linear regression procedures focus on the conditional mean. A predicted value from a standard regression measures the expected value – i.e., the *conditional mean* value – of y given values for X_{it} , $Z_{j,it}$, and $D_{j,it}$. Each possible set of values for X_{it} , $Z_{j,it}$, and $D_{j,it}$ implies one value for the conditional mean. Thus, a regression also implicitly predicts a full distribution of values for y – one conditional expectation for each set of values of the explanatory values. However, changes in the explanatory variables are constrained to have only very simple effects on the distribution of the dependent variable. For example, if $D_{1,it}$ switches from 0 to 1, the predicted distribution for y at time t simply shifts to the left or right according to the estimated value of γ_1 , while the new distribution is constrained to have the same variance as the original distribution. Under this standard regression specification, a school finance reform could change the conditional mean value of expenditure, but it could not lead to more egalitarian distribution, i.e., a distribution with lower variance than before the reform.

The coefficients of quantile regressions vary by q , where q denotes the quantile.² The quantile version of equation (1) is:

$$y_{qit} = X_{it}\beta_q + \sum_{j=1}^3 (Z_{j,it}\gamma_{qj} + D_{j,it}\delta_{qj}) \quad (2)$$

Thus, each variable's marginal effect differs by quantile. Since the coefficients vary across quantiles, equation (2) implies a distribution of values for y at each set of possible for the explanatory variables. The distribution may appear to be normal, in which case the values of the coefficients at $q = 0.50$ and $q = 0.51$ will be much closer together than the values at $q = 0.90$ and $q = 0.91$, but there is no restriction that the distribution be either symmetric about the mean or have constant variance.

² The quantile estimator was proposed by Koenker and Bassett (1978), and has been used extensively following influential studies by Buchinsky (1994, 1998a, 1998b, 2001). Buchinsky (1998b), Koenker (2005), and Koenker and Hallock (2001) present useful surveys.

To simulate the distribution of y for given values of X_{it} , $Z_{j,it}$, and $D_{j,it}$, we could first estimate β_q , γ_{qj} , and δ_{qj} for different quantiles (e.g., $q = 0.01, 0.02, \dots, 0.99$). Conditional on the values of X_{it} , $Z_{j,it}$, and $D_{j,it}$, the predicted median value of y_{it} is given by equation (2) with $q = 0.50$, the predicted value at the 10th percentile is given by equation (2) with $q = 0.10$, and so on. We could simulate the conditional distribution by drawing from the (uniformly) distributed values of q , using equation (2) to estimate the value of y conditional on the values of values of X_{it} , $Z_{j,it}$, and $D_{j,it}$, and then using the estimated values to calculate the implied distribution of y given X_{it} , $Z_{j,it}$, and $D_{j,it}$. After repeating this bootstrap-style procedure many times, the implied density function $f(y_t|X_t, Z_t, D_t)$ can be estimated using such standard procedures as a kernel density function or a simple histogram.

Machado and Mata (2005) generalize this approach by also simulating the distribution of the covariates, which transforms the conditional density function $f(y_t|X_t, Z_t, D_t)$ to the unconditional density, $f(y_t)$. For the model given by equation (2), the steps are:

1. Estimate quantile regressions for Q values of q . The estimates are $\hat{\beta}_q$, $\hat{\gamma}_{qj}$, and $\hat{\delta}_{qj}$.
2. Draw with replacement from the Q sets of coefficient vectors. Similarly, draw with replacement from the complete matrix of explanatory variables, $\Lambda = (X \ Z \ D)$, to form a new, bootstrapped sample that is the same size as the original sample.³ The individual draws are denoted $\hat{\beta}_{qb}$, $\hat{\gamma}_{qjb}$, and $\hat{\delta}_{qjb}$, and $\Lambda_b = (X_b \ Z_b \ D_b)$ where $b = 1, \dots, B$.
3. Calculate $\hat{y}_b = X_b \hat{\beta}_b + Z_b \hat{\gamma}_b + D_b \hat{\delta}_b$.

³ By drawing from the full panel data set, the bootstrapped sample of explanatory variables will generally have a different number of observations for each year and for each state than the original sample. The algorithm could be modified to keep the number of observations the same across years or within states, but the effects on the simulations are unlikely to be large with the sample size considered here.

4. After B repetitions of steps 1-3, calculate the density function for the estimated values, \hat{y}_b .

We set $B = 100,000$ and use a simple kernel density function with a constant bandwidth, h , for all density estimates.⁴

This algorithm can easily be modified to simulate the effects of school finance reforms and tax limits. Define indicator variables I_{70} , I_{80} , and I_{90} that equal one if the observation drawn in repetition b is from 1970, 1980, and 1990, respectively. For draws from 1970, none of the observations have reforms or tax limits (i.e., $Z_b = D_b = 0$), so the base estimate for 1970 is simply the estimated values with $\hat{\delta}_b$ set to zero, i.e., $\hat{y}_{b,70} = X_b\hat{\beta}_b I_{70b}$. Comparable calculations lead to the following results for 1980 and 1990:

	1970	1980	1990
Base	$X_b\hat{\beta}_b I_{70b}$	$X_b\hat{\beta}_b I_{80b}$	$X_b\hat{\beta}_b I_{90b}$
Reforms		$(X_b\hat{\beta}_b + Z_{1b}\hat{\gamma}_{1b} + D_1\hat{\delta}_{1b})I_{80b}$	$(X_b\hat{\beta}_b + Z_{1b}\hat{\gamma}_{1b} + D_1\hat{\delta}_{1b})I_{90b}$
Limits		$(X_b\hat{\beta}_b + Z_{2b}\hat{\gamma}_{2b} + D_2\hat{\delta}_{2b})I_{80b}$	$(X_b\hat{\beta}_b + Z_{2b}\hat{\gamma}_{2b} + D_2\hat{\delta}_{2b})I_{90b}$
Both		$(X_b\hat{\beta}_b + Z_{1b}\hat{\gamma}_{1b} + D_1\hat{\delta}_{1b} + Z_{2b}\hat{\gamma}_{2b} + D_2\hat{\delta}_{2b})I_{80b}$	$(X_b\hat{\beta}_b + Z_{1b}\hat{\gamma}_{1b} + D_1\hat{\delta}_{1b} + Z_{2b}\hat{\gamma}_{2b} + D_2\hat{\delta}_{2b})I_{90b}$

By calculating kernel density functions for each cell in the table, we can estimate counterfactual densities. For example, we can isolate the effect of school finance reforms on the distribution of per-student expenditures in 1980 by comparing the estimated density functions for the base/1980 and reforms/1980 entries in the above table. Similarly, we can compare the combined effects of

⁴ Since experimentation with alternative bandwidths had little effect on the overall results, we simply used the defaults in Stata.

school finance reforms and tax limits to their (counterfactual) absence in 1990 by comparing the estimated density functions for the base/1990 and both/1990 entries.

When comparing results across years and policies, it is important to bear in mind that three factors determine the change in densities. First, more states had reforms and tax limits in 1990 than in 1980. Second, the states that had tax reforms in 1980 had them in place longer by 1990. This second factor is taken into account by the D_j variables, which measure the number of years that the policies have been in effect in a state. Finally, the estimated quantile coefficients vary across policies. Thus, the apparently simple empirical specification is capable of producing a rich set of results for the distributions of expenditures across time and over policies.⁵

4. Regression Results

OLS regression results are presented in Table 3. The dependent variable for the regression is the natural logarithm of real expenditure per student. The sample includes all school districts in the contiguous United States with the exception of those in Montana and the District of Columbia. State fixed effects are included in the regressions but not reported here.

The base regression explains approximately 80% of the variation in the dependent variable. All of the Census variables reach high levels of statistical significance. Expenditures tend to be higher in districts in which the relatively small population has high median household incomes and a large percentage who have finished college. Expenditures also tend to be higher

⁵ Although our approach was inspired by Machado and Mata (2005), it is somewhat different. Machado and Mata generalize the Oaxaca decomposition to a quantile approach. Their approach allows them to decompose the total change in the distribution of a variable over time into the proportion due to changes in the quantile coefficients and the proportion due to changes in the distribution of the covariates. We are imposing more structure on the model by pooling the data and using dummy variables to capture time effects. We have experimented with estimating separate models for each year, and found similar results for the changes in the distributions of expenditures over time. The advantage of the additional structure is it simplifies the presentation of the results and focuses on the issue we are attempting to address, the effects of tax limits and reforms on the distribution of expenditures per student.

in districts with fewer owner-occupied households, more non-white residents, and high poverty levels. A large proportion of school-age residents tends to decrease expenditures per student, with the opposite pattern for districts with a large proportion of residents aged 65 years or older. Both state and federal revenues tend to increase local expenditure per student. Controlling for other factors, real expenditure per student rose by more than 15% between 1970 and 1980 and by (approximately) another 31% between 1980 and 1990.

The regression results imply that real expenditures per student fall by approximately 1.16% when school finance reforms are first put in place. In contrast, tax limits are not estimated to have a statistically significant effect on expenditures when first put in place. However, the combination of reforms and tax limits has a highly statistically significant effect on expenditure: if school finance reforms and tax limits were to be adopted at the same time, our results indicate that real expenditure per student would rise by approximately 6.21% ($0.0116+0.0041+0.0464$) in the short run. While reforms lead to an increase in expenditures in the short run, the effect diminishes over time and turns negative after about 5 and a half years ($.0116/.0021$). Tax limits are estimated to become more binding over time, with expenditures declining by approximately 0.37% per year that a tax limit is in effect. The combination of reforms and limits has a highly significant cumulative effect on expenditures per student over time: with each year that passes since the combination is enacted, real expenditure is estimated to decline by approximately 1.31% ($0.0021+0.0037+0.0073$).

The base regression results thus imply that reforms without tax limits have a small but statistically significant positive effect on the predicted conditional mean of real expenditure per student in the short run, with the effect unraveling over time. Without reforms, tax limits tend to have no effect on the predicted conditional mean in the short run, but have a modest tendency to

reduce expenditures over time. In combination, school finance reforms and tax limits have a much stronger effect than either has alone, with an economically meaningful effect that grows over time.

Quantile regression results are presented in Table 4 for selected target values of q . The dependent variable is the same as before – the natural logarithm of real expenditures per student across school districts in the contiguous United States – and the regressions again include unreported state fixed effects. Table 5 presents statistical tests for the differences between the quantile regression coefficients across the 10%-90% and 25%-75% quantiles.⁶

Quantile regression results are more difficult to interpret than standard regressions. Consider the effect of median household income as an example. The ordinary least squares regression of Table 3 indicates that when the natural logarithm of real median household income increases by 1, the predicted conditional mean value of real expenditure per student rises by 0.2772. The quantile regression results of Table 4 indicate that this result varies across quantiles, with a much stronger effect of income on expenditures at higher quantiles. Table 5 shows that differences across the 10%-90% and 25%-75% quantiles are statistically significant at the 0.1% level. Thus, income has a higher marginal effect on spending in districts that have higher expenditure than expected given their values for the other explanatory variables. An alternative interpretation is that the quantile regressions imply that the variance of the distribution of per-student expenditures increases as income increases: since the slope is lower for quantile lines below the median than for lines above the median, the spread between low and high quantile regression lines is increasing with income.

⁶ All standard errors are calculated using Stata's default bootstrapping algorithms.

The coefficients for reforms are positive at low quantiles and negative but statistically insignificant at high quantiles. For a district with spending below the level expected given the other explanatory variables by the amount associated with the 10% quantile, the log of real expenditure per student rises by 0.0455 when a reform is put in place. In contrast, the log of real expenditure per student is predicted to *fall* by 0.0262 when a reform is put in place for a district with spending above the level expected given the other explanatory variables by the amount associated with the 90% quantile. This result implies that the difference between expenditures per student narrows across quantiles when school finance reforms are passed. In other words, finance reforms lead to a narrowing of the distribution of per-student expenditures. These results support the findings of earlier studies such as Evans, Murray, and Schwab (1997), Murray, Evans, and Schwab (1998), and Corcoran et al. (2004), which find that reforms led to a more equal distribution of expenditures across districts. However, these results suggest a modest degree of “leveling down” since expenditures are estimated to decline somewhat at very high quantiles.

The same pattern is evident for tax limitation measures that are enacted without finance reforms. Interestingly, tax limits do not appear to necessarily lower expenditures for all districts immediately following their enactment: expenditures are estimated to increase for low-quantile districts. There appears to be a slight counteracting tendency toward a more variable distribution when both reforms and limits are enacted at the same time: the coefficients for “both reforms and limits in place” are higher at higher quantiles, but they are not highly statistically coefficient. Since all three variables work concurrently when reforms and limits are enacted jointly, the overall effect is to reduce the variance of the per-student expenditure distribution when both reforms and limits are put in place.

The estimated coefficients for the variables representing the time that reforms and limits have been in place suggest that the short and long run effects of policies may differ. Whereas reforms increase expenditures for low quantiles and decrease expenditures at high quantiles in the short run, each additional year that a reform has been in place serves to lower expenditures at low quantiles and increase them (or have no effect) at high quantiles. Thus, the initial gains of low-quantile districts erode over time and the distribution of expenditures tends back toward inequality. Tax limitation measures become more binding over time, reducing expenditures per student across all quantiles. Similarly, the combination of reforms and limits tends to lower expenditures at across all quantiles.

5. Predicted Distribution Changes

The quantile estimates imply that school finance reforms and limits have statistically significant effects on the distribution of per-student expenditures. Quantile regressions cannot be summarized easily in tables because they produce different estimates for each quantile. Having estimated separate quantile regressions for 97 target values of q ($q = 0.02, 0.03, \dots, 0.98$), we are only able to summarize a small subset in Table 4. In this section, we use the simulation procedure described in Section 3 to predict the effects of reforms and limits on the full distribution of per-student expenditures. We use kernel density estimates to summarize the results of 100,000 replications of steps 1-3 of the simulation procedure. Graphs of the density functions make it much easier to assess the economic significance of the distributional changes.

Before simulating the effects of reforms and tax limits, it is useful to compare kernel density estimates for the actual distribution of the natural logarithm of real expenditures per student to the quantile regression estimates. The quantile estimates are based on predicted values

from step 3 of our simulation algorithm, $\hat{y}_b = X_b\hat{\beta}_b + Z_b\hat{\gamma}_b + D_b\hat{\delta}_b$. Separate kernel density estimates for each year are presented in Figure 1. With an R^2 of 0.80 for the base OLS regression, it is not surprising that the quantile density estimates track the actual densities quite accurately.

In the absence of either tax reforms or limits, the predicted value of the dependent variable is simply $X_b\hat{\beta}_b I_{80b}$ in 1980 and $X_b\hat{\beta}_b I_{90b}$ in 1990, i.e., the value including state fixed effects but with the coefficients for the six policy variables set to zero. Figure 2 presents the estimated density functions for these base values, along with the predicted values including the effects of school finance reform combined with the years since the reforms were put in place, or $(X_b\hat{\beta}_b + Z_{1b}\hat{\gamma}_{1b} + D_1\hat{\delta}_{1b})I_{80b}$ in 1980 and $(X_b\hat{\beta}_b + Z_{1b}\hat{\gamma}_{1b} + D_1\hat{\delta}_{1b})I_{90b}$ in 1990. The quantile regression results presented in Table 4 imply that without tax limits, reforms tend to increase spending among low-quantile districts in the short run while decreasing spending among high-quantile districts. The short run gain among low-quantile districts tends to erode over time. Figure 2 shows that, while these effects may be statistically significant, they have little effect on the appearance of the overall distribution of per-student expenditures. Reforms have virtually no effect on the distribution of expenditures in 1980 even though Table 1 shows that 17 states had reforms in place by this time. With the addition of another 11 states during the 1980s (not including Montana, which is not in the sample), the distribution has shifted very little compared with its predicted value in the absence of reforms. Figure 2 implies a very modest level of leveling down, with small decreases in expenditure per student in the high-spending part of the distribution and with modest increases in expenditure per student in the very low-spending part of the distribution. The net effect is to have a slightly more egalitarian distribution of spending in 1990 than would have been the case without reform.

Figure 3 compares the estimated distributions of real expenditures per student in 1980 and 1990 under the base (no reform or limits) and with tax limits (but not reforms). Table 4 suggests that the effects of tax limits and reforms are similar in the short run – decreases in spending among high-quantile districts and increases among low quantiles. The regressions imply that the effects of tax limits become more binding over time, with decreases in spending among all quantiles. Table 1 shows that 16 states in the sample had adopted tax limits by 1980, while another 6 adopted them after this date. Figure 3 reveals that the tax limit legislation had only negligible effects on the overall distribution of per-student spending in 1980. By 1990, the effects have accumulated to the point that the distribution has shifted noticeably to the left, with larger declines in expenditure among high-spending districts.

Figure 4 shows the combined effects of school finance reforms and tax limits. There now is a noticeable effect of the policies on the distribution of the natural logarithm of real expenditure per student. In 1980, the combination of reforms and limits leads to lower per-student expenditures among high-spending districts, with an increase in the number of districts falling in the middle of the distribution. For low-spending districts, the tendencies toward increased spending from reforms and reduced spending from tax limits cancel out, leading to virtually no change in the distribution of expenditures in the low-spending part of the distribution. By 1990, the effects of the policies have accumulated to the point that the distribution has clearly shifted to the left. The leveling-down tendency is clearly evident, with the distribution shifting further to the left in the high-spending portion of the distribution.

Together, these results suggest that while reforms have a modest tendency toward leveling down and tax limits tend to reduce expenditures among all districts in the long run, it is only in combination that the two policies lead to clear shifts in the distribution of per-student

expenditures. The combined effect of tax limits and reforms is to shift the entire distribution to the left, with more pronounced shifts in the high-spending part of the distribution. While the combination of school finance reforms leads to a more equal distribution of spending across districts, it does so by lowering spending disproportionately among high-expenditure districts.

6. Conclusion

This study differs from previous education research in its use of a quantile regression approach to analyze the effects of both school finance reform and tax limitation measures on the distribution of expenditures per student across U.S. school districts. The quantile approach is a natural one because the intended effects of finance reforms, in particular, are explicitly distributional. Rather than relying on a few summary statistics to measure distributional changes, quantile regressions allow us to directly estimate the effects of reforms and tax limits on the full distribution of per students from 1970 to 1990.

The results indicate that both reforms and tax limits have very modest effects on the distribution of per-student expenditures in the short run. Reforms have a modest tendency toward increases in expenditures in low-spending expenditure districts, with decreases in high-spending districts. Tax limits have a similar effect in the short run, while over time per-student expenditures tend to decline significantly in all districts following the implementation of tax limits. In contrast to the modest effects indicated for reforms and limits when passed in isolation, when both are adopted their combined effect is considerable. By 1990, the distribution of per-student expenditures is quite different for districts in states having both finance reforms and limits. Compared to the predicted distribution in states with neither reforms nor limits, per-student expenditure is predicted to decline substantially in all districts residing in states adopting both reforms and limits, with disproportionate decreases among high-spending districts.

Although the overall distribution is more egalitarian after reforms and limits are jointly adopted, greater equality comes at the expense of high-spending jurisdictions, with a pronounced tendency toward “leveling down.”

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Table 1: Reform and Tax Limit Adoption Dates

State	Reform	Limit	State	Reform	Limit
Alabama		1975	Nebraska		
Arizona		1980	Nevada		
Arkansas	1983	1981	New Hampshire		1976
California	1977	1978	New Jersey	1976	
Colorado	1973		New Mexico	1974	1979
Connecticut	1977		New York		
Delaware			North Carolina		
Florida	1973		North Dakota		1981
Georgia	1986		Ohio		1976
Idaho		1979	Oklahoma	1987	
Illinois	1973		Oregon		
Indiana		1973	Pennsylvania		
Iowa	1972	1972	Rhode Island		1985
Kansas	1976	1974	South Carolina	1977	1985
Kentucky	1989	1979	South Dakota	1986	
Louisiana	1988	1978	Tennessee	1977	
Maine	1978		Texas	1984	
Maryland	1987		Utah	1975	
Massachusetts	1985	1980	Vermont	1987	
Michigan		1978	Virginia	1975	
Minnesota	1973	1972	Washington	1978	
Mississippi		1983	West Virginia	1984	
Missouri		1980	Wisconsin	1973	
Montana	1989	1987	Wyoming	1980	1983

Table 2: Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
1970 (8889 districts)				
Log of Real Expenditure Per Student	7.871	0.279	6.935	9.262
65 Years or Older	0.115	0.044	0.003	0.433
19 Years or Younger	0.389	0.047	0.156	0.594
Owner-Occupied Households	0.737	0.098	0.099	0.976
Non-White	0.087	0.153	0.000	1.000
Population in Poverty	0.166	0.110	0.014	0.812
Log of Real Median Household Income	10.340	0.298	9.013	11.753
Log of Real Federal Revenue Per Capita	0.966	1.463	0.000	6.674
Less than High School, 25+	0.517	0.135	0.048	0.898
High School, 25+	0.315	0.083	0.058	0.558
Some College, 25+	0.091	0.040	0.006	0.328
Log of Real State Revenue Per Pupil	7.132	0.495	1.562	8.567
Log of District Population	8.930	1.137	6.908	15.838
Reform in Place	0.000	0.000	0.000	0.000
Tax Limits in Place	0.000	0.000	0.000	0.000
Both Reforms and Limits in Place	0.000	0.000	0.000	0.000
Years since Reform in Place	0.000	0.000	0.000	0.000
Years since Limits in Place	0.000	0.000	0.000	0.000
Years since both Reforms and Limits in Place	0.000	0.000	0.000	0.000
1980 (8922 districts)				
Log of Real Expenditure Per Student	8.086	0.256	7.193	9.224
65 Years or Older	0.127	0.046	0.005	0.432
19 Years or Younger	0.332	0.041	0.138	0.536
Owner-Occupied Households	0.751	0.094	0.134	0.967
Non-White	0.102	0.160	0.000	1.000
Population in Poverty	0.130	0.075	0.008	0.594
Log of Real Median Household Income	10.301	0.283	9.333	11.626
Log of Real Federal Revenue Per Capita	0.490	1.022	0.000	6.504
Less than High School, 25+	0.372	0.125	0.041	0.804
High School, 25+	0.374	0.079	0.101	0.584
Some College, 25+	0.134	0.046	0.023	0.336
Log of Real State Revenue Per Pupil	7.446	0.507	4.029	9.225
Log of District Population	9.062	1.154	5.746	15.772
Reform in Place	0.344	0.475	0.000	1.000
Tax Limits in Place	0.350	0.477	0.000	1.000
Both Reforms and Limits in Place	0.148	0.355	0.000	1.000
Years since Reform in Place	1.886	2.867	0.000	8.000
Years since Limits in Place	1.626	2.681	0.000	8.000

Years since both Reforms and Limits in Place	0.826	2.195	0.000	8.000
1990 (8910 districts)				
Log of Real Expenditure Per Student	8.409	0.278	7.690	9.687
65 Years or Older	0.145	0.048	0.006	0.345
19 Years or Younger	0.296	0.039	0.060	0.507
Owner-Occupied Households	0.743	0.095	0.049	0.960
Non-White	0.116	0.170	0.000	0.992
Population in Poverty	0.134	0.082	0.000	0.643
Log of Real Median Household Income	10.285	0.329	8.959	11.829
Log of Real Federal Revenue Per Capita	0.463	1.034	0.000	6.596
Less than High School, 25+	0.275	0.108	0.019	0.810
High School, 25+	0.360	0.081	0.058	0.681
Some College, 25+	0.221	0.061	0.052	0.541
Log of Real State Revenue Per Pupil	7.779	0.546	3.801	9.354
Log of District Population	9.098	1.206	4.522	15.806
Reform in Place	0.580	0.494	0.000	1.000
Tax Limits in Place	0.489	0.500	0.000	1.000
Both Reforms and Limits in Place	0.237	0.425	0.000	1.000
Years since Reform in Place	6.459	6.963	0.000	18.000
Years since Limits in Place	6.354	6.943	0.000	18.000
Years since both Reforms and Limits in Place	2.719	5.647	0.000	18.000

Table 3: OLS Regression Results

Variable	Coefficient	Standard Error
65 Years or Older	0.3942	0.0414
19 Years or Younger	-0.9832	0.0399
Owner-Occupied Households	-0.2178	0.0133
Non-White	0.3027	0.0106
Population in Poverty	0.4634	0.0302
Log of Real Median Household Income	0.2772	0.0104
Log of Real Federal Revenue Per Capita	0.0064	0.0009
Less than High School, 25+	-0.5900	0.0235
High School, 25+	-0.3698	0.0236
Some College, 25+	-0.8455	0.0447
Log of Real State Revenue Per Pupil	0.0294	0.0026
Log of District Population	-0.0355	0.0012
Reform in Place	0.0116	0.0054
Tax Limits in Place	0.0041	0.0061
Both Reforms and Limits in Place	0.0464	0.0089
Years since Reform in Place	-0.0021	0.0005
Years since Limits in Place	-0.0037	0.0006
Years since both Reforms and Limits in Place	-0.0073	0.0009
1980	0.1547	0.0045
1990	0.4688	0.0072

Note. State fixed effects are not reported. The number of observations is 26,713. The R^2 is 0.8019.

Table 4: Quantile Regression Results

Quantile	10%	25%	50%	75%	90%
65 Years or Older	0.4879 (0.0643)	0.4398 (0.0424)	0.3542 (0.0455)	0.2686 (0.0556)	0.1996 (0.0788)
19 Years or Younger	-0.7444 (0.0588)	-0.8344 (0.0398)	-0.9729 (0.0438)	-1.1652 (0.0556)	-1.3701 (0.0821)
Owner-Occupied Households	-0.0893 (0.0191)	-0.1214 (0.0130)	-0.1796 (0.0146)	-0.2241 (0.0190)	-0.2987 (0.0283)
Non-White	0.2593 (0.0155)	0.267 (0.0104)	0.2776 (0.0116)	0.3187 (0.0152)	0.3422 (0.0222)
Population in Poverty	0.3359 (0.0439)	0.3692 (0.0298)	0.4335 (0.0332)	0.5561 (0.0428)	0.6147 (0.0637)
Log of Real Median Household Income	0.1889 (0.0150)	0.2170 (0.0103)	0.2430 (0.0114)	0.2966 (0.0147)	0.3543 (0.0215)
Log of Real Federal Revenue Per Capita	-0.0001 (0.0013)	0.0026 (0.0009)	0.007 (0.0010)	0.011 (0.0012)	0.0128 (0.0017)
Less than High School, 25+	-0.5806 (0.0352)	-0.6132 (0.0238)	-0.6505 (0.0258)	-0.6138 (0.0319)	-0.5669 (0.0465)
High School, 25+	-0.2837 (0.0349)	-0.3619 (0.0238)	-0.4652 (0.0259)	-0.4201 (0.0319)	-0.4403 (0.0461)
Some College, 25+	-0.7813 (0.0653)	-0.8732 (0.0445)	-0.9481 (0.0491)	-0.8934 (0.0609)	-0.7980 (0.0881)
Log of Real State Revenue Per Pupil	0.0868 (0.0030)	0.0634 (0.0022)	0.0373 (0.0028)	0.0158 (0.0041)	0.0163 (0.0066)
Log of District Population	-0.0110 (0.0017)	-0.0211 (0.0012)	-0.0282 (0.0013)	-0.0389 (0.0018)	-0.0529 (0.0028)
Reform in Place	0.0455 (0.0078)	0.0241 (0.0054)	0.0040 (0.0059)	-0.0106 (0.0073)	-0.0262 (0.0105)
Tax Limits in Place	0.0325 (0.0084)	0.0201 (0.0060)	-0.0026 (0.0067)	-0.0153 (0.0083)	-0.0117 (0.0118)
Both Reforms and Limits in Place	0.0236 (0.0124)	0.0418 (0.0087)	0.0495 (0.0097)	0.0531 (0.0122)	0.0683 (0.0176)
Years since Reform in Place	-0.0038 (0.0008)	-0.0027 (0.0006)	-0.0008 (0.0006)	0.0006 (0.0007)	0.0005 (0.0010)
Years since Limits in Place	-0.0068 (0.0008)	-0.0056 (0.0006)	-0.0037 (0.0007)	-0.0024 (0.0008)	-0.0013 (0.0012)
Years since both Reforms and Limits in Place	-0.0044 (0.0012)	-0.0061 (0.0009)	-0.0077 (0.0010)	-0.0084 (0.0012)	-0.0095 (0.0017)
1980	0.1256 (0.0062)	0.1294 (0.0044)	0.1461 (0.0050)	0.1534 (0.0065)	0.1463 (0.0095)
1990	0.4362 (0.0096)	0.4469 (0.0069)	0.4645 (0.0079)	0.4645 (0.0103)	0.4466 (0.0152)

Note. Standard errors are in parentheses. State fixed effects are not reported. The number of observations is 26,713.

Table 5: Difference in Coefficients across Quantiles

Quantile	90% - 10%	75% - 25%
65 Years or Older	-0.2883** (0.0967)	-0.1712** (0.0603)
19 Years or Younger	-0.6257*** (0.0986)	-0.3308*** (0.0742)
Owner-Occupied Households	-0.2093*** (0.0281)	-0.1027*** (0.0215)
Non-White	0.0829* (0.0337)	0.0516*** (0.0128)
Population in Poverty	0.2789*** (0.0808)	0.1869*** (0.0493)
Log of Real Median Household Income	0.1655*** (0.0209)	0.0796*** (0.0146)
Log of Real Federal Revenue Per Capita	0.0130*** (0.0015)	0.0084*** (0.0012)
Less than High School, 25+	0.0137 (0.0624)	-0.0007 (0.0422)
High School, 25+	-0.1566** (0.0487)	-0.0582 (0.0439)
Some College, 25+	-0.0167 (0.1481)	-0.0202 (0.0770)
Log of Real State Revenue Per Pupil	-0.0705*** (0.0063)	-0.0476*** (0.0048)
Log of District Population	-0.0420*** (0.0023)	-0.0179*** (0.0021)
Reform in Place	-0.0718*** (0.0121)	-0.0347*** (0.0078)
Tax Limits in Place	-0.0442** (0.0155)	-0.0354*** (0.0075)
Both Reforms and Limits in Place	0.0448 (0.0230)	0.0113 (0.0140)
Years since Reform in Place	0.0043** (0.0015)	0.0032*** (0.0006)
Years since Limits in Place	0.0055*** (0.0012)	0.0032*** (0.0008)
Years since both Reforms and Limits in Place	-0.0052* (0.0023)	-0.0023 (0.0012)
1980	0.0207** (0.0065)	0.0240*** (0.0070)
1990	0.0104 (0.0121)	0.0176 (0.0102)

Note. Standard errors are in parentheses. Significance at the 5%, 1%, and 0.1% level is indicated by *, **, and ***.

Figure 1: Actual and Predicted Expenditure Distributions

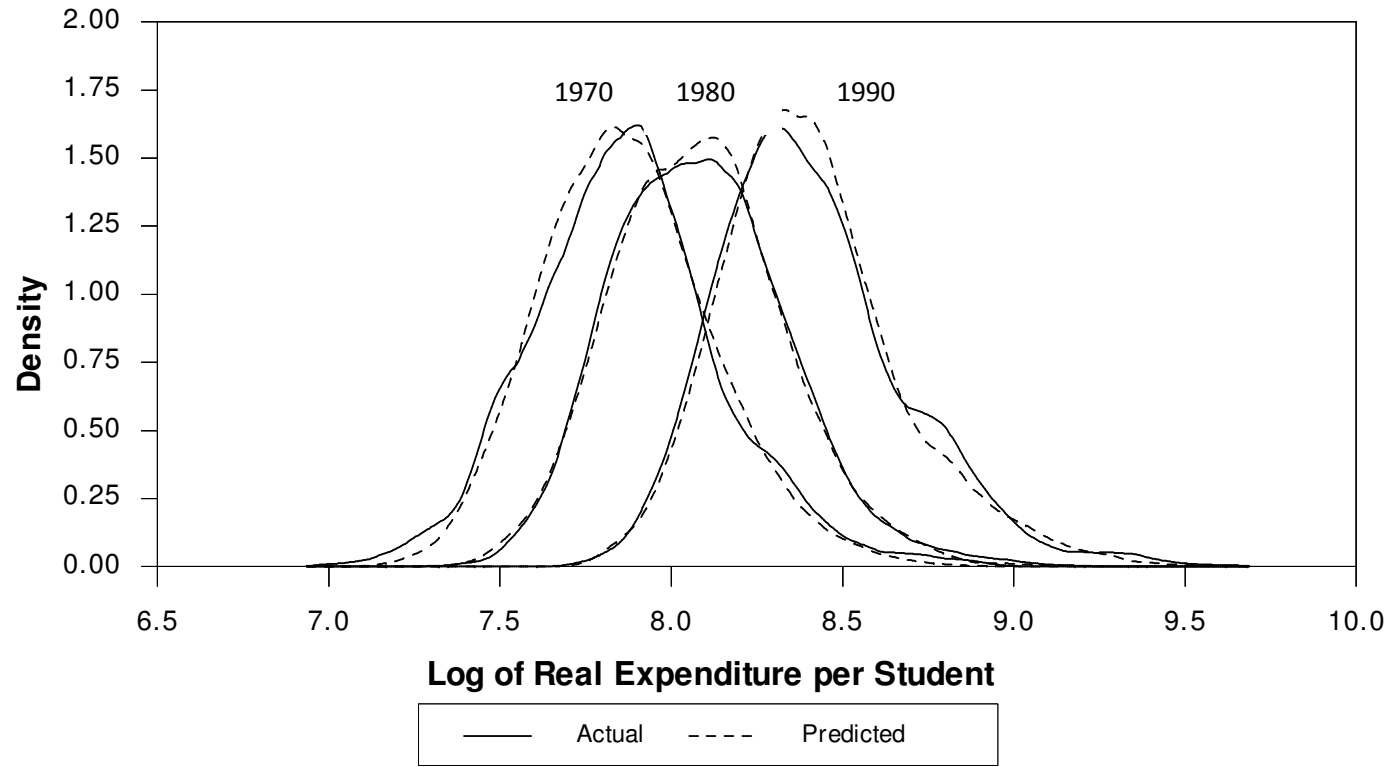


Figure 2: Predicted Change in Expenditure Distributions with Reforms

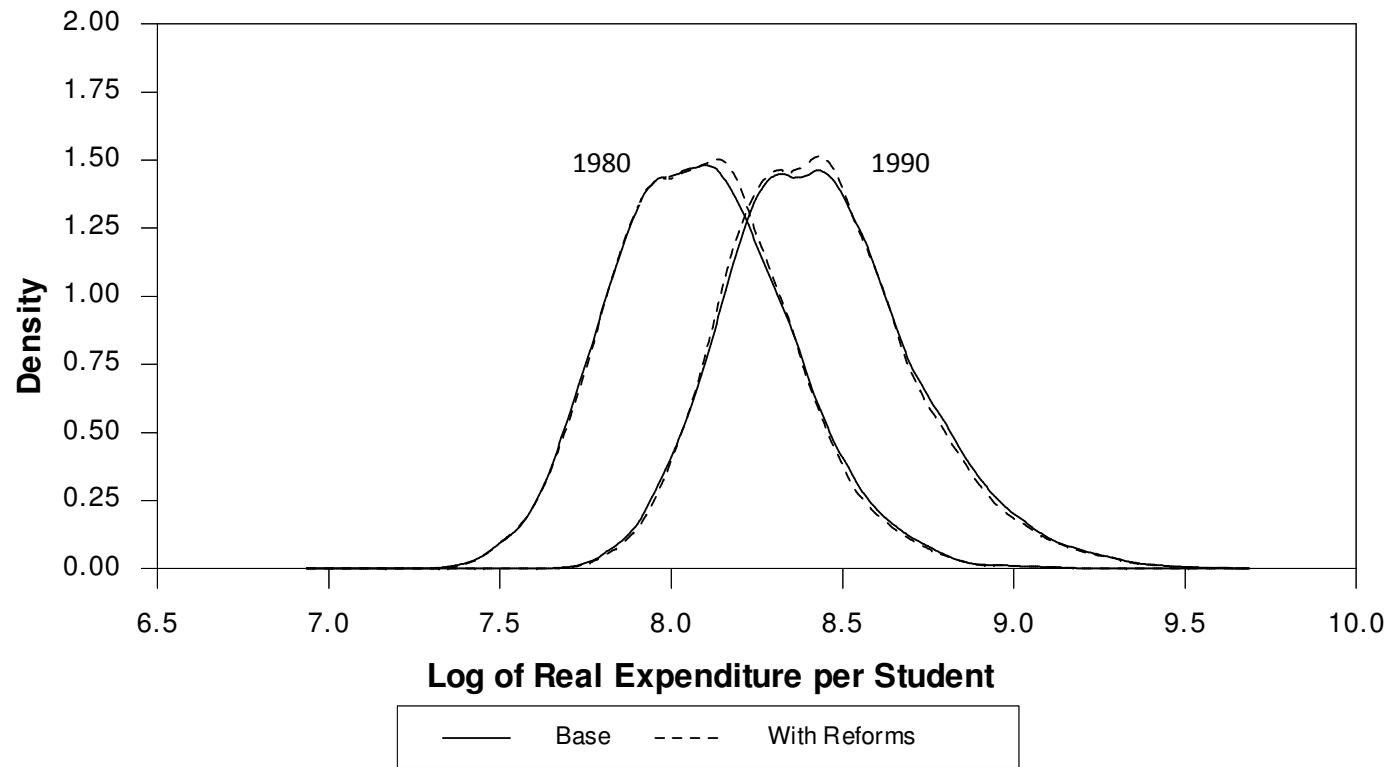


Figure 3: Predicted Change in Expenditure Distributions with Limits

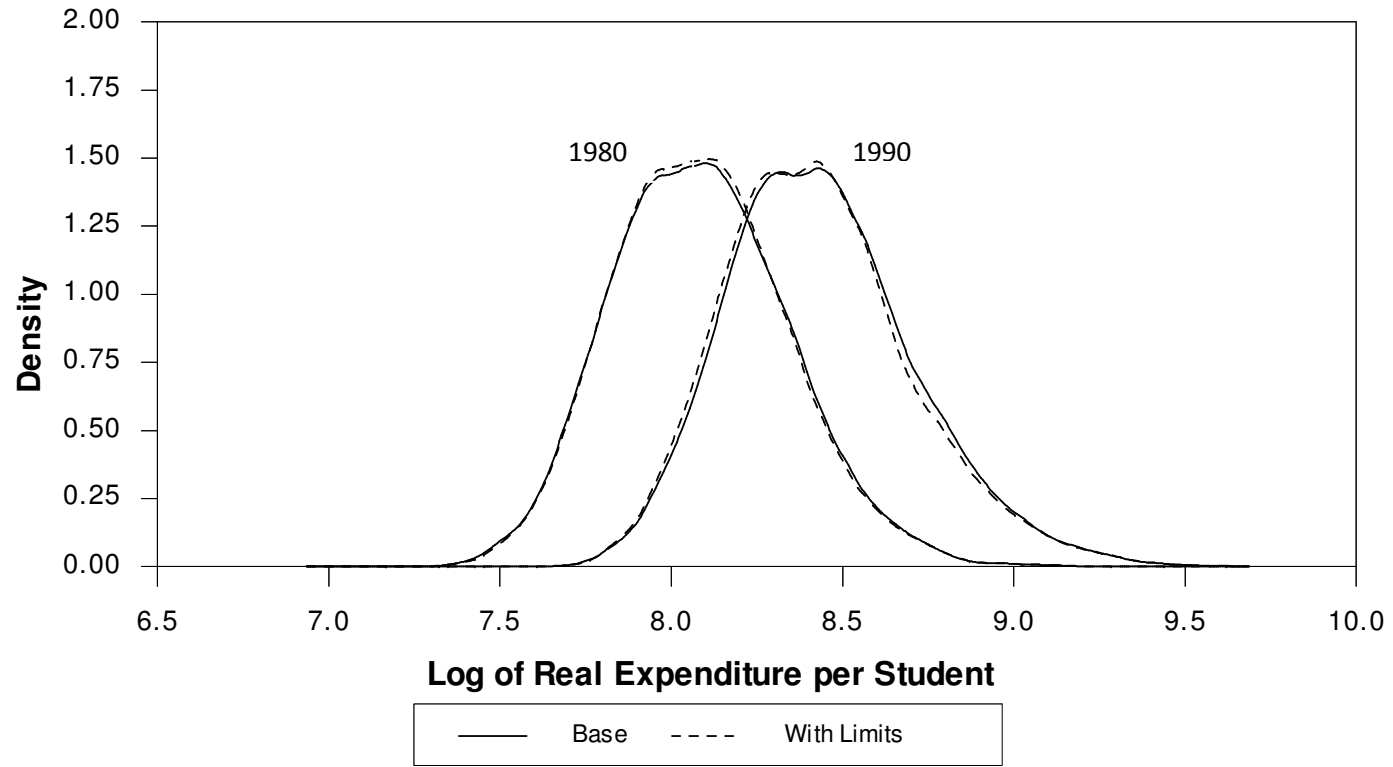


Figure 4: Predicted Change in Expenditure Distributions with both Limits and Reforms

