A Distributional Difference-in-Difference Evaluation of the Response of School Expenditures and Class-Sizes to Reforms and Tax Limits

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Abstract

Using the Common Core of Data, we compare changes in the distribution of district-level real expenditures per student and class sizes across school finance regimes and over time. The results suggest that both tax limitation measures and school finance reforms affect the full distribution of school service levels. Reforms and tax limits tend to yield greater equality of expenditures by reducing the number of districts in the tails of the distribution. Tax limits tend to yield more low-expenditure districts, while reform measures lead to more equalization around the mean. The results suggest that the number of districts with relatively high average class sizes increased in states that have adopted tax limits. Several regressions suggest that the observed changes in the distribution of school service levels directly relate to the degree of state control afforded by the school finance regime.

1. Introduction

Historically, responsibility for U.S. educational funding has resided at the local government level (Shaprio, Puryear, and Ross, 1979; de Bartolome, 1997), but dissatisfaction with tax burdens and school performance has fostered a movement toward greater reliance on state funding for education. The property tax revolts that began in California in the 1970s rippled through the continental United States with 43 of 48 states adopting explicit limits on property taxation by 2005 (Anderson, 2006). In addition, legal challenges to educational finance systems served as a catalyst for both statutory and legislative school finance reforms in more than two thirds of the states. In general, the objective of these reforms is to limit, centralize, and equalize funding across districts (Evans, Murray, and Schwab, 1997; Figlio, Husted, and Kenny, 2004).

Tax limitation measures and school finance reforms may differentially affect outcomes such as educational expenditures, class sizes, student test scores, and teacher quality. By limiting revenues and overall expenditures, binding tax limits are likely to reduce school quality across most, if not all, school districts (e.g., O'Sullivan, Sexton, and Sheffrin, 1995). In contrast, the objective of school finance reforms is explicitly distributional – to reduce the variance of school expenditures across districts within a state by increasing the relative funding levels for the poorer districts (e.g., Murray, Evans, and Schwab 1998). Thus, successful reforms need not harm wealthier districts, because they may yield a rising tide of increased state funding across districts in a state (Hoxby, 1998). Of course, neither tax limits nor school finance reforms yield unambiguous effects: School districts facing a highly restrictive tax measure may prove effective in finding substitute funds to avoid cuts associated with reductions in local funding, whereas school reforms may equalize expenditures by reducing funding to wealthy districts without increasing spending in poorer districts.

Empirical tax-limit and finance-reform studies suggest that such policies have adversely affected class sizes, student test scores, and teacher quality (e.g., Card and Payne, 2002; Matsuaka, 1995; Silva and Sonstelie, 1995; Downes and Figlio 1998, Downes, Dye, McGuire, 1998; Figlio and Rueben, 2001). However, the findings are not monolithic and their effects often differ among states and across school districts within a given state. This study extends prior work by explicitly analyzing the full distribution of education outcomes. Using the Common Core of Data (CCD) for school districts and a kernel density approach, we exploit differences across types of limitations and the fact that their effects on educational outcomes likely occur over time in order to compare the response of per-pupil expenditure and class size distributions to tax limitations and/or educational finance reforms that began in the late 1970s. We compare changes in the distributions between 1990 and 2000 across three school finance regimes: a base group of states that had neither tax limits nor reforms, states that had finance reforms but not tax limits, and states that adopted tax limits.

The results provide unique insights into apparent differences across states in the extent to which tax limits, litigation and school finance reform have affected district level school outcomes (e.g., Sokolow, 1998). Overall, we find that relative to the base group of states, expenditures became more concentrated in the middle of the distribution in reform states. On the other hand, tax limits tended to skew the distribution yielding more districts in the lower tail of the per-student expenditure distribution and in the upper tail of the class-size distribution. However, several regressions that model the coefficient of variation of per capita expenditures and average class size suggest that the various school finance regimes affected the variation in school outcomes primarily by concentrating funding at the state versus the local level, rather than through the regime itself.

In the subsequent section, we define the school finance regimes and provide descriptive evidence regarding how school service levels differ across the regimes. Section 3 describes the empirical approach and the kernel density results for the population of school districts in the continental United States in 1990 and 2000. Sections 4 and 5 describe the difference in the density differences by school finance regime for expenditures per student and class size, respectively. In Section 6, the regression results for two models of the variation in real, per capita expenditures and average class size are presented. The final section concludes.

2. The Data

Our analysis uses the Common Core of Data (CCD) that comprises the primary database on public elementary and secondary education from the U.S. Department of Education. This comprehensive, annual, national database was first published in 1987 and includes both administrative and financial data for every U.S. school district. Our analysis uses school-district data for 1990 and 2000. The earlier date follows the wave of tax revolts and educational finance reforms that began largely in the decade prior to 1990 (Sokolow, 1998; Payne and Card, 2002; Anderson, 2006).

The straightforward base group is comprised of school districts in states that never passed a substantive tax limitation measure and did not enforce a court-mandated or legislative educational finance reform since the decade of the 1970s when the tax and reform movements began. The comparison groups, on the other hand, could include a number of alternative pairings of states that passed a tax limitation measure or adopted educational finance reforms at various points in time. In practice, we find two groups are sufficient to capture the policy effects: (1) districts in states that had finance reforms but not tax limitation measures between 1970 and 2000, and (2) districts in state that adopted tax limitation measures during this time period.

We use the findings of previous work to help determine the placement of the states into the various school finance regimes, which inevitably involves some value judgments. For example, Indiana is included in our base category because, while they adopted a tax limit in 1974, Sokolow (1998) indicates that it was effectively non-binding. Alternatively, Manwaring and Sheffrin (1997) argue that many states adopted only weak legislative finance reforms to head off further litigation after a failed court case. For example, Idaho is included in the control states because it adopted a weak legislative reform in 1978 after an unsuccessful court case in 1975 that was later repealed in 1992. The broad conclusions are not sensitive to the necessary judgment calls regarding the school finance regimes. The list of states in each group is shown in Table 1.

The school-resource variables used in the analysis are district-level measures of average class-size and real, per-pupil expenditures. These variables have both been used as measure of public service levels and/or school quality in prior work (e.g., Figlio, 1998). Although the evidence linking such measures of school quality and student outcomes such as test scores or post-school earnings are mixed, parents, business leaders, and policy analyst have focused explicitly on class-size and per-pupil expenditures as key metrics in the evaluation of the public provision of education (e.g., Betts, 1995). Thus, because public perception of the quality of local schools is so closely linked to both class size and per pupil expenditures, these measures capture the tradeoff (real or perceived) between school performance and tax limitation measures and/or legal reform.

As seen in Table 1, the expenditure values suggest that tax limits and finance reforms typically occur in states that have relatively high per student expenditures.[1] In particular, the base group of districts that were not subject to tax limits or reform had lower per student expenditures in 1990 than those who experienced either a tax limit or a reform over the period of study. This pattern continued in 2000, after the limits and reforms had been in place for some time. On the other hand, average class size yields a less consistent pattern across regimes, with sharper drops in states with reforms than in states that had both reforms and tax limits.

To focus on distributional service-level change comparisons that are independent of initial differences, the empirical analysis focuses on the average class-size and per-pupil expenditures normalized by their 1990 level. Normalization is necessary because states differ systematically in their levels of expenditures and class sizes. For example, as can be seen in Table 1, Tennessee's school districts had the lowest average expenditure per pupil in 1990 at $3127. Though the average increased to $4688 in 2000, Tennessee's average expenditure per pupil remained well below the national average of $5032. Similarly, whereas California's average class size fell from 22.5 in 1990 to 20.2, it had the highest average class among the 48 contiguous states in both years.[2] It follows that, if the data are not normalized, these changes would be observed only as a slight change at the extremes of the national distribution.

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[1] The U.S. city average CPI (all items) was used to express all expenditures terms of 1990 dollars.
[2] These figures are calculated as the simple average of the values from each jurisdiction in the state.

Table 2 shows that the normalized average class size and real expenditure per pupil always have an average for 1990 of 1.0 across each state's jurisdictions. The 1990 data thus measure how a jurisdiction's average class size or expenditure differs from the state-wide average. The 2000 data are also normalized by the 1990 state-wide mean. Thus, if a jurisdiction's value for the normalized average class size rose from 1.0 to 1.2 between 1990 and 2000, then its average class size began the decade at the state average but rose to a level 20% higher than the 1990 average. Thus, the normalization allows us to pool data across states while maintaining the ability to determine whether the overall distributions have shifted to the right or left over time.

Although the normalization yields a mean level of service of one in each of the three categories for 1990 in Table 2, the mean values of the real per-student expenditures in 2000 are larger than one for each regime, indicating a general rise in real expenditures between 1990 and 2000. The normalized value of average class size is less than one in 2000 for each category, suggesting that the general trend toward smaller classes was shared by states having reforms and tax limits. Interestingly, the standard error of both normalized expenditures and class size tend to be larger in reform and tax limit states than in the base category, suggesting that legal and political pressure may be driven by relative district-level inequality in the state. The empirical analysis compares the 1990 and 2000 revenue and class-size distributions for reform and tax limit regimes to comparable distributions for the base category of states that imposed no property tax limits or school finance reforms on their local school districts.

3. Empirical Approach

Our objective is to analyze changes in the distributions of per-student expenditures and average class sizes between 1990 and 2000, and to determine whether the changes in the distributions differ for states with tax reforms and property tax limitation measures. The basic tool for this descriptive analysis is a nonparametric estimator of the underlying density function. Using x_{90} to represent either the normalized average class size or expenditure per capita in 1990, the kernel density function estimate at a target value x is:

$$\hat{f}_{90}(x) = \frac{1}{nh}\sum_{i=1}^{n} K\left(\frac{x_{90i} - x}{h}\right) \tag{1}$$

where n is the number of observations. Similarly, the density at the target value x for 2000 is:

$$\hat{f}_{00}(x) = \frac{1}{nh}\sum_{i=1}^{n} K\left(\frac{x_{00i} - x}{h}\right) \tag{2}$$

The change in the density between 1990 and 2000 is simply:

$$\Delta(x) = \hat{f}_{00}(x) - \hat{f}_{90}(x) \tag{3}$$

The difference between the changes in densities across any two regimes i and j is simply:

$$\Psi_{ij}(x) = \Delta_j(x) - \Delta_i(x) \tag{4}$$

We use a Gaussian kernel for all calculations: $K(u) = \varphi(u)$, where φ is the standard normal density function. For both average class size and expenditure per student, we calculate the density functions at 400 equally spaced alternative values of *x*, and then use graphs to summarize the results.[3]

Confidence intervals can be calculated easily for the estimated density functions, given by equations (1) and (2). Following Silverman (1986) or Pagan and Ullah (1999), the 95% confidence interval for an estimated density at target point *x* is

$$\hat{f}(x) \pm 1.96(nh)^{-1/2}\left[f(x)\int K^2(\psi)\,d\psi\right]^{1/2} \quad (5)$$

For the Gaussian kernel, $\int K^2(\psi)\,d\psi = 0.2821$. Though the analytic standard errors are easy to calculate for $\hat{f}(x)$, they are more complex for the differences in densities because they require an estimate of the covariances.[4] Thus, we use a simple bootstrap algorithm to construct our standard error estimates. We draw with replacement from each regime's series of average class sizes and expenditures for 1990 and 2000, and re-calculate the density functions (equations 1 and 2) and their changes (equation 3). After repeating this process for 100 replications, the bootstrap standard error estimates are simply the sample standard deviations of the 100 estimates.[5] The bootstrap estimator provides estimates of $\text{var}\left(\hat{f}_{90}(x)\right)$, $\text{var}\left(\hat{f}_{00}(x)\right)$, and $\text{var}\left(\hat{\Delta}(x)\right)$ for each group of states. Following these calculations, the variance for the differences between density changes is

[3] The kernel density function is the same conceptually as a smoothed histogram. The degree of smoothing is controlled by the bandwidth, *h*. Following Silverman (1986), we use a simple rule of thumb to determine the bandwidths: $h = 1.06\,\text{var}(x)\,n^{-.20}$. With three categories of states and two time periods, this rule of thumb implies six alternative bandwidths. Though wider band widths tend to yield a higher degree of monotonicity and smoother distributions, experimentation with alternative bandwidths produced only minor variation in the appearance of the estimated density functions. We use the average of the six values of *h* as the bandwidth for all calculations.

[4] The formula for the variance is $\text{var}\left(\hat{\Delta}(x)\right) = \text{var}\left(\hat{f}_{90}(x)\right) + \text{var}\left(\hat{f}_{00}(x)\right) - 2\,\text{cov}\left(\hat{f}_{90}(x), \hat{f}_{00}(x)\right)$.

[5] The bootstrap standard error estimates are virtually identical to the analytic errors for the estimated density functions.

simply $\text{var}\left(\hat{\Psi}_{ij}(x)\right)=\text{var}\left(\hat{\Delta}_i(x)\right)+\text{var}\left(\hat{\Delta}_j(x)\right)$ because by construction states are in the same regime for both 1990 and 2000, which results in $\text{cov}\left(\hat{\Delta}_i(x),\hat{\Delta}_j(x)\right)=0$.

4. Difference in Density Differences by School Finance Regime

Table 2 shows that, for the full sample of school districts, expenditure per student increased and average class size fell between 1990 and 2000. However, prior work suggests that these broad U.S. trends may differ distinctly across different school finance regimes and over time. Thus, in this section, we use a difference-in-differences approach between regime and years to determine whether the change in the densities differs across three state groupings that characterize distinct school finance regimes – (1) a base group with no binding finance reforms or tax limits, (2) states that have adopted binding reforms but not tax limits, (3) states that have adopted tax limits.

Figure 1 shows the estimated density functions for real expenditure per student in 1990 and 2000 for each group of states. The graphs show that real expenditure per student increased substantially over time. The same information is shown in a different way in Figure 2, which shows the 95% bootstrap confidence intervals for the changes in the estimated densities from 1990 to 2000. In each regime, the number of school districts with high values for real expenditure per student rose significantly, while the number of districts with low values declined.

Interesting results emerge when we compare the change in densities between 1990 and 2000 across groups. The top two panels of Figure 3 show the 95% bootstrap confidence intervals for changes relative to the base group, i.e., $\Delta_1(x)-\Delta_0(x)$ and $\Delta_2(x)-\Delta_0(x)$, where the function $\Delta(x)$ is given in equation (3), the base group is denoted by 0, state with finance reforms but not limits are denoted by 1, and 2 denotes the group of state that have adopted tax limits. Similarly, the bottom panel of Figure 3 shows the 95% bootstrap confidence interval for $\Delta_2(x) - \Delta_1(x)$.

The top panel shows that compared with the base group, the number of school districts with expenditures per pupil in the middle of the distribution increased markedly for states that have adopted school finance reforms. This push toward the middle of the distribution can be interpreted as a form of egalitarianism: compared with the base group, states adopting reforms closed the 1990s with fewer districts with low per-student expenditures, but also fewer districts with high levels of expenditure per student. Thus, the results in the top panel suggest that finance reforms reduced the variance of per-student expenditures relative to the base group. The reduction in the variance was influenced both by a decline in the number of low expenditure districts and by a reduction in the number of districts with high levels of per-student expenditures.

The middle panel of Figure 3 shows comparable density differences for districts in states that have adopted tax limits. The effect of the tax limits was to increase (relative to the base states) the number of districts in the middle of the per-student distribution and to decrease the number of districts in either tail. The reduction in the number of high-spending districts is pronounced. The bottom panel of Figure 3 alters the comparison group to consider differences between density changes for states with finance reforms and states with tax limits. Compared with states with school finance reforms only, the number of districts with relative low per-student expenditures rose significantly in states with tax limitations.

Overall, the Figures 1 through 3 suggest that both reforms and tax limits do indeed influence the distribution of per-student expenditures. Both reforms and tax limits tended to produce a more equal distribution of per-student expenditure over the 1990s by reducing the number of districts with unusually high and unusual low expenditures, as compared with the base group of states. This tendency toward egalitarianism is most pronounced in states with school finance reforms. Tax limitation measures tended to increase the number of districts with per-student expenditures that are below but close to their 1990 values.

Next, we present a series of graphs showing the difference-in-difference density function estimates for average class size. As was the case for per-student expenditures, Figure 4 shows that the base density function estimates for 1990 and 2000 look similar across the three state groupings. Figure 5 shows the 95% bootstrap confidence intervals for the changes in the density functions between 1990 and 2000. These results also show that the distribution of average class sizes shifted to the left over the decade for all three groups, i.e., average class sizes decreased.

Figure 6 shows the confidence intervals for the difference in the density function differences. Although the results are somewhat noisy for the top two panels, the overall pattern from these two panels suggests that, relative to the base group of states, the number of districts with high average class sizes decreased in states with reforms and increased in states with tax limits. The bottom panel of Figure 7 shows the results for a comparison of reform to tax limits states. The results imply strongly that tax limits led to a much larger number of districts with high average class sizes when compared to the group of states with school finance reforms.

5. Coefficient of Variation Regressions

The kernel density figures provide compelling descriptive evidence that the variations in real expenditures and average class size have changed systematically and differently across the state finance regimes. In particular, with regard to per-student expenditures, both reforms and tax limits appeared to reduce the number of districts with unusually high and unusual low levels relative to the base group of states, with a more pronounced move towards egalitarianism in states with school finance reforms. However, high average class sizes decreased in states with reforms and increased in states with tax limits such that the distributional effects appear to differ with the school finance regime. In either case, the results clearly suggest that the school finance regime affects, in addition to the mean level of school services, higher moments of both average class size and per-student expenditures, which has not systematically been examined in prior work.

Prior work has found that tax limitation measures and school finance reforms tend to move responsibility for school funding from local districts towards the jurisdiction of the state, which has been offered as a possible explanation for the observed changes in the variation in the mean level of student services (e.g., Figlio, 1998). Thus, it is useful to examine whether the school finance regime and the associated concentration of school funding at the state level account for differences across states in the variation in real expenditures and average class size conditioned on other factors that have been found to relate to school district behavior.

To capture the variation in real expenditures and class size, we use the within state, across school district coefficient of variation in real per capita expenditures and average class size. The coefficient of variation is a useful measure of district-level variation because it normalizes the standard deviation by the mean, which permits a comparison across states that begin with distinctly different base school funding. This normalization is similar to that used in the kernel density analysis. However, unlike kernel densities approach, the regression analysis permits us to empirically condition for differences in the average level of variation between the 1990 and 2000. Thus, rather than normalizing both the 1990 and 2000 distributions by the 1990 means as conducted in the kernel density analysis, the coefficient of variation normalizes the district-level variation within a state by the state-level mean for each decade, 1990 or 2000.

We regress the state-level coefficient of variation measures of real per capita expenditures and average class size on a relatively parsimonious set of controls that vary across state between 1990 and 2000. This yields a total of 96 observations for the 48 states used in the kernel density analysis for 1990 and 2000. We include two binary variables for the two school finance regimes used in the kernel density analysis, school finance reform without tax limits (regime 1) or with tax limits (regime 2). Moreover, to examine whether the key factor is the regime or its postulated effect of greater concentration of school district funding at the state level, we include a control for the K-12 funding that is provided by the state government.

In addition, we include a control for the number of school districts because, all else equal, a greater number of districts might permit greater Tiebout sorting and strategic political competition for resources that would be expected to increase the variation in real expenditures and average class size. We also include a control for the percent of school funding spent on administration because equalizing expenditures may require greater administrative resources, but these non-instructional expenditures might reduce resources going towards instruction and aversely affect the variation in class sizes. We also include three spatial binary variables for the Northeast, South, and West (relative to the exclude Midwest region) and a binary variable for 2000 to permit the variation in real expenditures and average class size to differ by region and decade.

The regression results for two separate regressions of the coefficient of variation of real, per capita expenditures and average class size are presented in Table 3. The results show that each of the explanatory variables is significant in at least one of the specifications and that the specifications explain approximately 60 percent of the variation in the dependent variables. Thus, the parsimonious empirical models exhibit a relatively high degree of explanatory power with regard to the variation in real per capita expenditures and average class size.

The real per capita expenditure regression is presented in column 1 of Table 3. The coefficients on two school finance regime measures and the percent of state funding are negative, but only the state-funding variable is significant. This result suggests that the regime itself does not explain the variation in real per capita expenditures, but that a greater centralization of funding that has been associated with these fiscal limitations does tend to reduce across-district variation in expenditures. Likewise, the negative and significant coefficient for percent of total expenditures spent on administration does suggest that reducing variation in real expenditures across districts requires greater administrative oversight. Interestingly, the magnitude of the coefficient on the school finance regime increases (becomes positive) but remains insignificant when the percent of total expenditures spent on administration is excluded from the model (not presented).

This finding suggests that school finance regime does not erode total expenditures by redirecting funding towards other services such as administration.

The coefficients on Northeast and South are both insignificant suggesting that the variation in expenditures in these regions do not differ from the Midwest, whereas the West has significantly greater variation in real per capita expenditures. Finally, the coefficient on the binary variable that equals one for 2000 is negative and significant, confirming the general finding from the kernel density figures that the variation in expenditures has declined over time. Such a difference over time might also relate to the tax limitation measures and school finance reform, although coefficients on interactions between the school-finance regime variables and the binary variable for year 2000 (not presented) are insignificant.

Unlike the coefficient of variation for real expenditure regressions, the coefficient on the school finance regime variables are both positive and significant in the coefficient of variation regression for average class size in column 2 of Table 3. However, the coefficient on percent of state funding is negative in both columns of Table 3. Thus, the results suggest that a greater concentration of state funding tends to reduce the variation in school district outcome measures but, conditional on this concentration of school funding, the limitations placed on school districts by these two school financing regimes tends to increase the variation. This finding suggests that some districts may have greater ability to supplement their funding with local sources than others within a state.

The coefficient on the percent of total expenditures spent on administration is positive and significant in column 2 of Table 3, which is opposite in sign to column 1. This suggests that greater administrative expenditures may well regulate variation in real expenditures, but that it also may reduce funding for instruction that yields an increase in the variation of class size. However, the significant impact of the school finance regime on class size disappears when the percent of total expenditures spent on administration is excluded from the model (not presented), suggesting that the school finance regime can influence the allocation of expenditures between instruction and other school services. More broadly, the fact that the school finance regime is found to significantly affect class-size but not expenditures conditioned on the percent of total expenditures spent on administration suggests that tax limits and school finance reforms are relatively more effective at controlling where resources are spent as opposed to the overall level of expenditures.

The coefficient on the regional binary variables is negative (positive) and significant for the South (West), suggesting that there is significant variation in average class size consistent with the observed variation for real expenditures. However, the coefficient on the binary variable for year 2000 is negative, but insignificant, indicating that the variation in class sizes did not change significantly over the two time periods net of other controls. Thus, the greater egalitarianism in class size observed over time in the kernel density analysis appears to be fully explained by changes in other factors including the increased concentration of school funding at the state level.

Overall, the results suggest that the variation in real per capita expenditures and average class size did decline with the concentration of funding at the state level. On the other hand, the school finance regime controls are insignificant for expenditures or positive for class sizes, suggesting that regimes did not directly improve the equity of school funding net of other possible indirect effects. It is important to emphasize, however, that the coefficient of variation measures only account for the first two moments of the distributions, which the kernel density analysis suggests may be an incomplete characterization of the distributional affects of tax limits and school finance reform.

6. Conclusions

Prior work has found that property tax limits and school finance reforms, on average, tend to reduce school service levels and student-level performance, but that such initiatives can also lessen the inequality across school districts and yield improvements in relative resources of economically disadvantaged districts (e.g., Figlio, 1998; Evans, Murray, and Schwab, 1997). This paper builds on this prior work by using the Common Core of Data (CCD) and nonparametric kernel density techniques to compare how changes in school finance regimes (i.e., tax limits and school finance reforms) affect school-district level real, expenditures per student and class sizes across the whole distribution and in comparison to districts that did not adopt tax limits and/or school finance reforms. The results provide compelling evidence that both tax limitation measures and school finance reforms affect the full distribution of school service levels relative to districts that do not adopt such initiatives. On the other hand, the results also show that the joint effect of tax limits and school finance reforms is different than that of reform on its own and that the school finance regime ultimately impacts the variation in school services through a movement of school funding away from local governments and towards the state.

In general, both reforms and tax limits appear to yield greater equality of expenditures by reducing the number of districts in the tails of the distribution. However, tax limitation measures tended to yield a greater number of low-expenditure districts, whereas expenditure reform measures led to more equalization around the mean. At the same time, although class sizes uniformly declined between 1990 and 2000 across all school finance regimes, the results suggest that the number of districts with relatively high average class sizes increased in states that have adopted tax limits.

Overall, the results provide strong evidence that the fiscal federalism movement that began in California with Proposition 13 acted to redistribute and equalize resources across districts, but did not generate incentives for a general race to the bottom. The observed narrowing of the school service distribution does not necessarily imply an overall improvement in social welfare because the differences in the distribution are likely to reflect genuine differences in preferences regarding the value placed on school resources. Moreover, this equalization in public spending with regard to education may yield private responses to educational spending that may undo this public policy. Thus, further work needs to be done to understand the general equilibrium effects of tax limitation and school finance reform policies.

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Table 1
Average Service Level by Tax and Reform Regime

State	Number of Districts	Exp. Per Student 1990	Exp. Per Student 2000	Avg. Class Size 1990	Avg. Class Size 2000
No Reforms or Tax Limits					
AL	127	3546.91	5113.78	19.05	15.11
DE	16	5251.92	7159.87	17.18	15.70
ID	102	3731.13	5163.31	18.03	16.04
IN	291	4562.79	6163.13	17.96	17.25
MS.	148	3171.04	4658.94	18.09	16.11
NV	16	6251.01	6471.76	17.66	15.62
NC	117	4526.73	5689.38	16.01	15.12
NC	157	4798.65	5657.32	14.83	12.36
PA	498	5785.02	6743.07	16.38	16.40
RI	31	5961.94	7037.99	14.58	13.66
All	1503	4764.43	5992.87	16.99	15.81
Reforms without Tax Limits					
AK	307	3298.08	4490.00	14.11	12.99
CT	123	7924.97	7709.32	14.08	13.93
GA	172	4113.19	5476.91	16.35	15.58
IL	488	4098.72	5882.90	16.71	15.06
KS	297	5195.46	5833.65	13.44	13.13
KY	171	3130.26	5272.06	17.32	14.82
LA	66	3909.56	4791.71	16.38	14.77
ME	116	5467.51	6777.78	14.88	12.39
MD	24	5671.78	6248.58	17.41	16.40
MO	448	4044.01	5210.91	14.90	13.19
NH	71	5868.31	6263.46	15.28	14.12
NJ	255	7557.31	8947.62	14.41	13.57
NY	623	8259.70	9317.65	13.82	13.16
OK	416	3754.43	4684.77	14.20	13.93
SC	84	4348.34	5780.85	16.90	14.77
SD	158	4400.50	5675.30	12.73	12.36
TN	118	3126.88	4687.84	19.01	15.67
TX	962	4831.59	6538.27	14.28	12.69
UT	40	3726.70	4728.54	21.85	19.58
VT	61	6603.41	7876.34	16.78	12.49
VA	131	4949.17	5985.04	15.36	13.52
WA	246	5830.13	6238.47	18.63	18.27
WV	55	3763.39	5849.57	15.31	13.84
WI	377	5508.79	7205.68	15.29	14.26
WY	46	6879.73	7205.34	12.62	12.19
All	5855	5119.89	6391.80	15.07	13.85

Reforms and Tax Limits

AZ	99	5305.84	5690.98	17.65	16.99
CA	377	4965.97	5749.96	22.50	20.21
CO	171	5576.85	6561.29	14.16	13.86
FL	67	5544.79	5673.02	16.86	17.83
IA	341	4415.89	5886.41	14.53	14.13
MA	224	5907.39	7083.80	16.94	13.08
MI	524	4763.04	6479.31	15.18	17.63
MN	288	5279.77	6474.68	16.70	14.78
MT	165	6132.57	6401.23	12.31	12.38
NE	234	5276.09	5915.07	12.49	12.43
NM	88	5330.73	6720.24	16.60	14.74
OH	610	4195.98	5573.83	19.40	17.18
OR	165	5442.44	6176.85	16.50	17.03
All	3353	4992.90	6125.27	16.78	16.00

Note. Expenditures are in 1990 dollars.

Table 2
Normalized Service Levels By Local Expenditure Regime

Variables	Base Group - No Reform or Tax Limits	Reform, but no Tax Limits	Reform & Tax Limits
Normalized Expenditures in 1990 by 1990	1.00	1.00	1.00
	(0.21)	(0.27)	(0.25)
Normalized Expenditures in 2000 by 1990	1.28	1.28	1.24
	(0.29)	(0.35)	(0.40)
Normalized Class Size in 1990 by 1990	1.00	1.00	1.00
	(0.13)	(0.16)	(0.17)
Normalized Class Size in 2000 by 1990	0.93	0.92	0.96
	(0.13)	(0.15)	(0.19)
Number of Observations	1503	5855	3353

Note. Standard deviations are in parentheses.

Table 3
Coefficient of Variation Regressions for Real Expenditures and Average Class Size[a]

Variables	**Coefficient of Variation for Per Capita Real Expenditures**	**Coefficient of Variation for Average Class Size**
Regime 1	-0.0058	0.0232*
	(0.0164)	(0.0101)
Regime 2	-0.0026	0.0251*
	(0.0190)	(0.0117)
Number of Districts[b]	0.1679*	0.0566*
	(0.0368)	(0.0227)
Percent State Funding[b]	-0.0363*	-0.0170*
	(0.0138)	(0.0085)
Percent Administrative Expenditures	-0.4467*	0.5299*
	(0.2061)	(0.1273)
Northeast	0.0322	-0.0192
	(0.0208)	(0.0128)
South	-0.0033	-0.0389*
	(0.0186)	(0.0115)
West	0.1226*	0.0728*
	(0.0194)	(0.0120)
Year 2000	-0.0906*	-0.0331
	(0.0337)	(0.0208)
Constant	0.3592*	0.0103
	(0.0721)	(0.0445)
R^2	0.5959	0.6229
Number of Observations	96	96

-a Standard Errors in Parentheses. b – Coefficient and standard error are multiplied 1000.
* - Significant at 5 percent level

Figure 1
Estimated Density Functions for Real Expenditure per Student

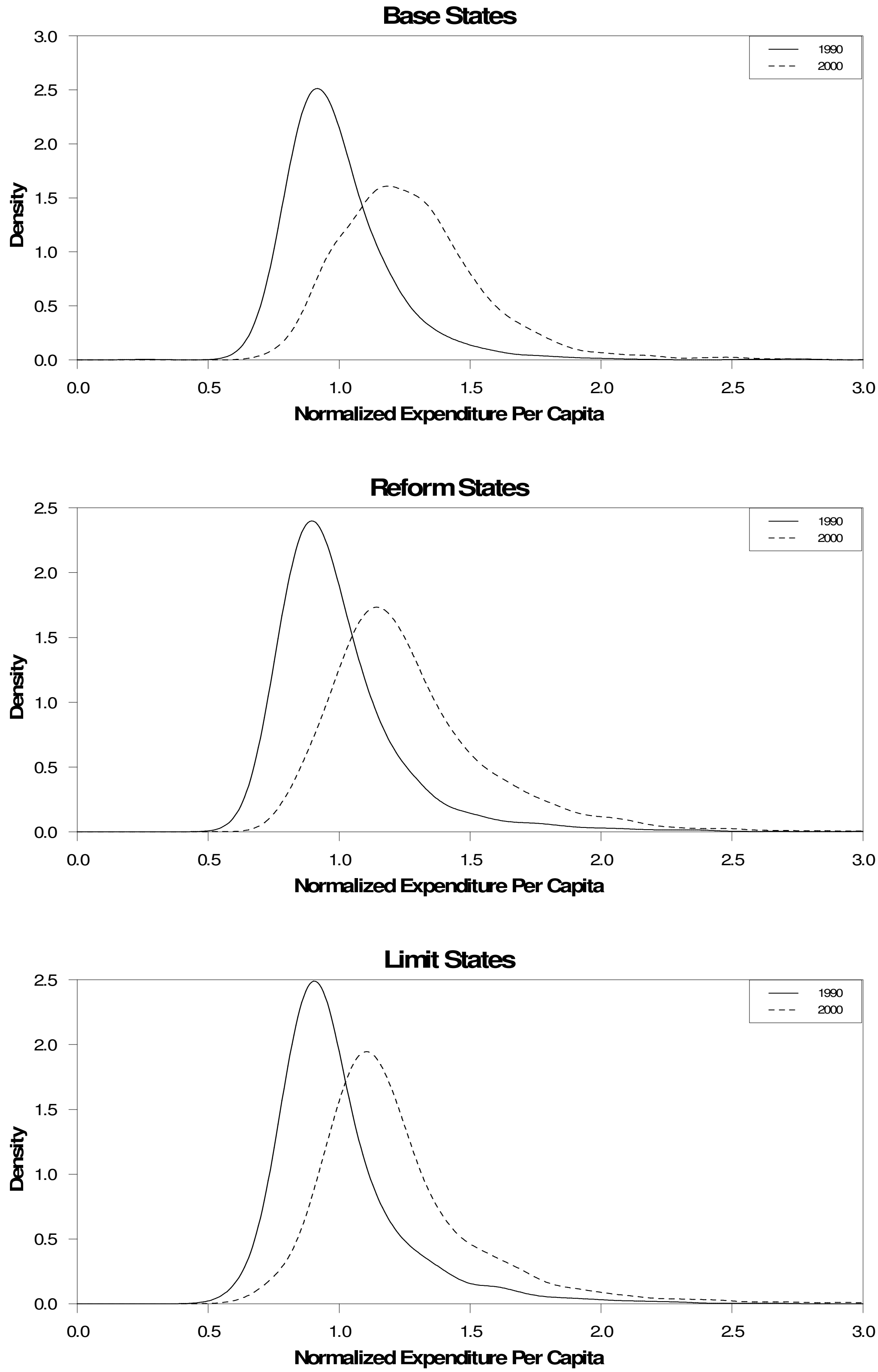

Figure 2
Ninety-Five Percent Bootstrap Confidence Intervals for Difference between 1990 and 2000 Densities for Real Expenditure per Student

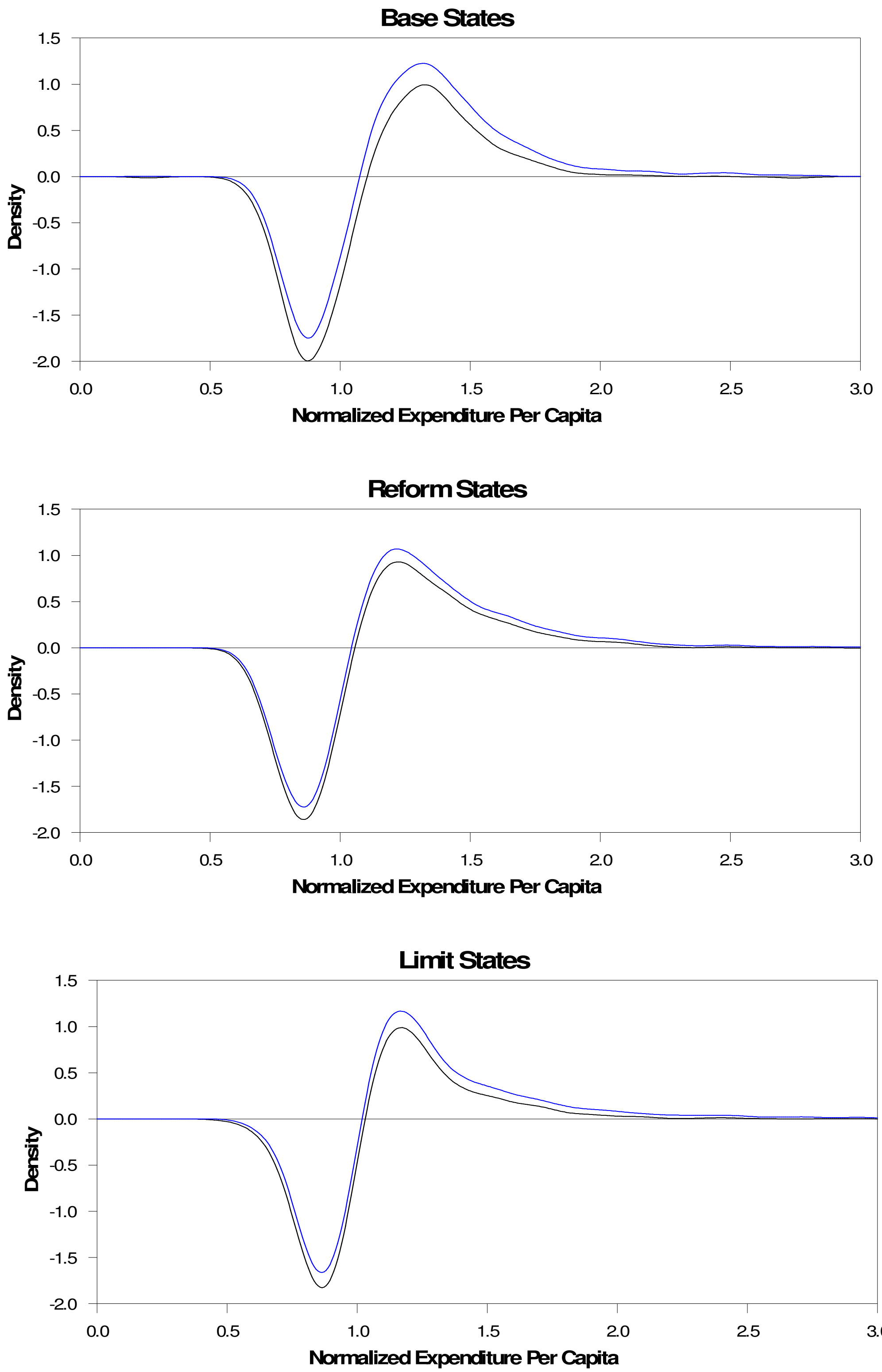

Figure 3
Ninety-Five Percent Bootstrap Confidence Intervals for Density Differences for Real Expenditure per Student

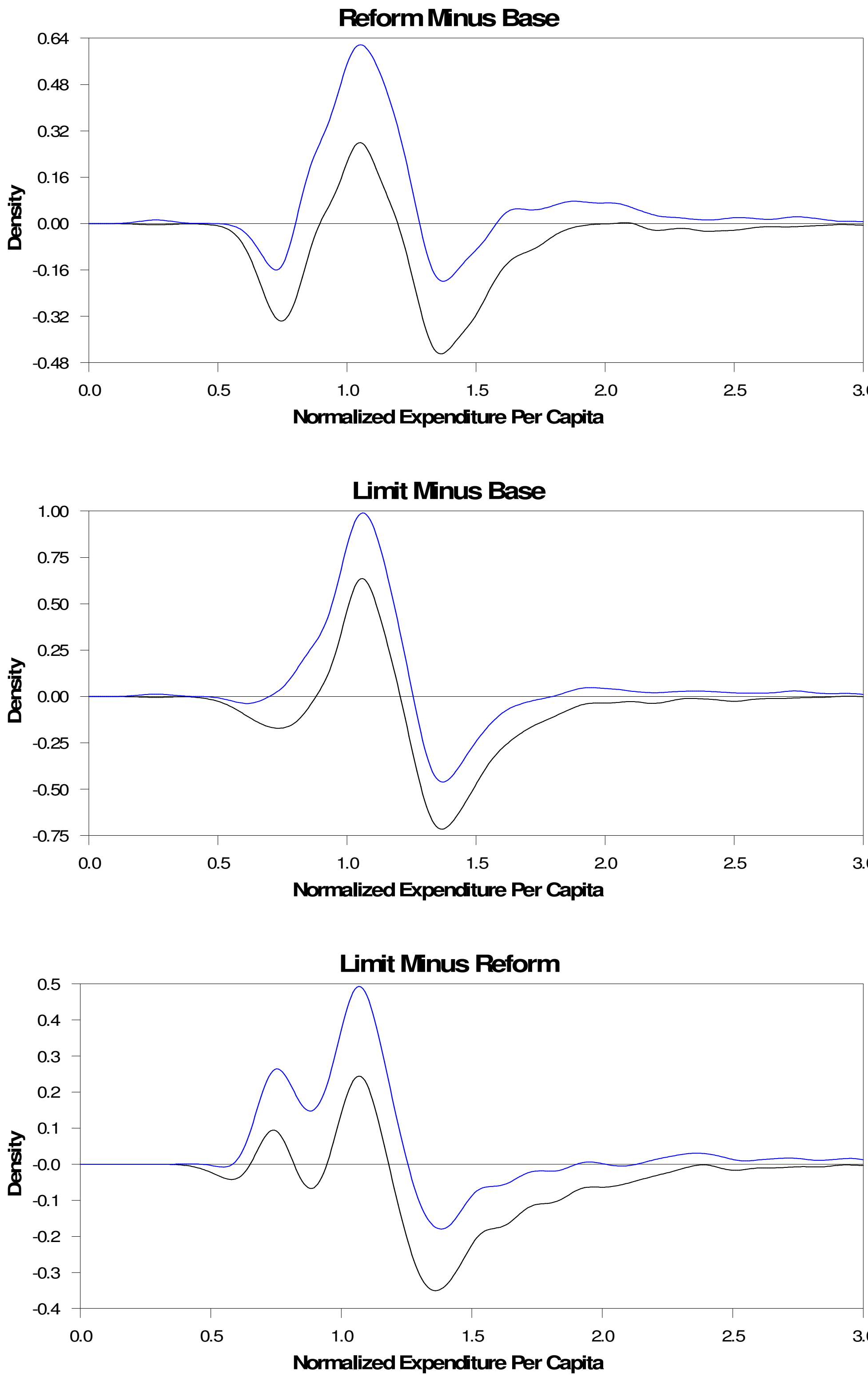

Figure 4
Estimated Density Functions for Average Class Size

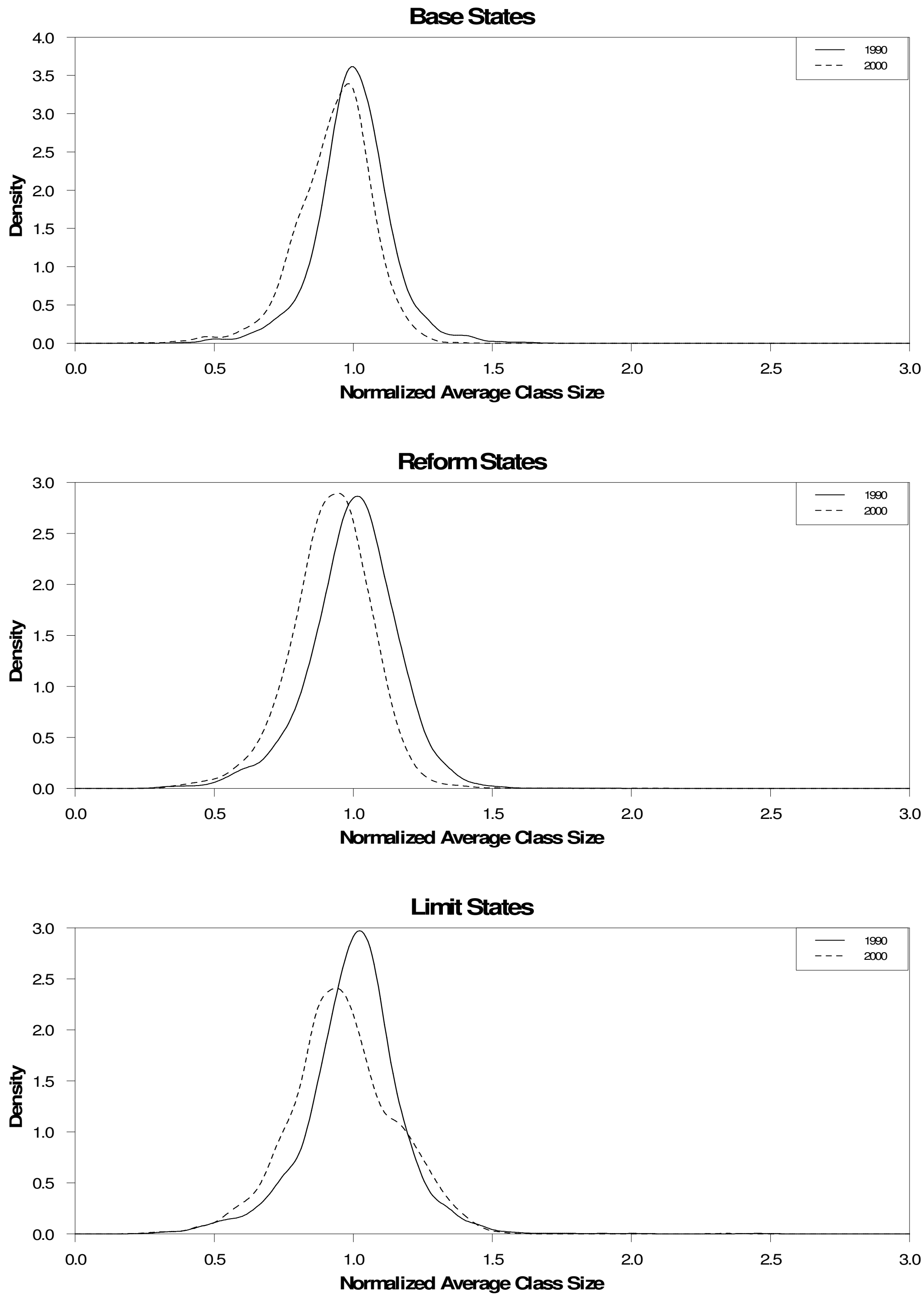

Figure 5
Ninety-Five Percent Bootstrap Confidence Intervals for Difference between 1990 and 2000 Densities for Average Class Size

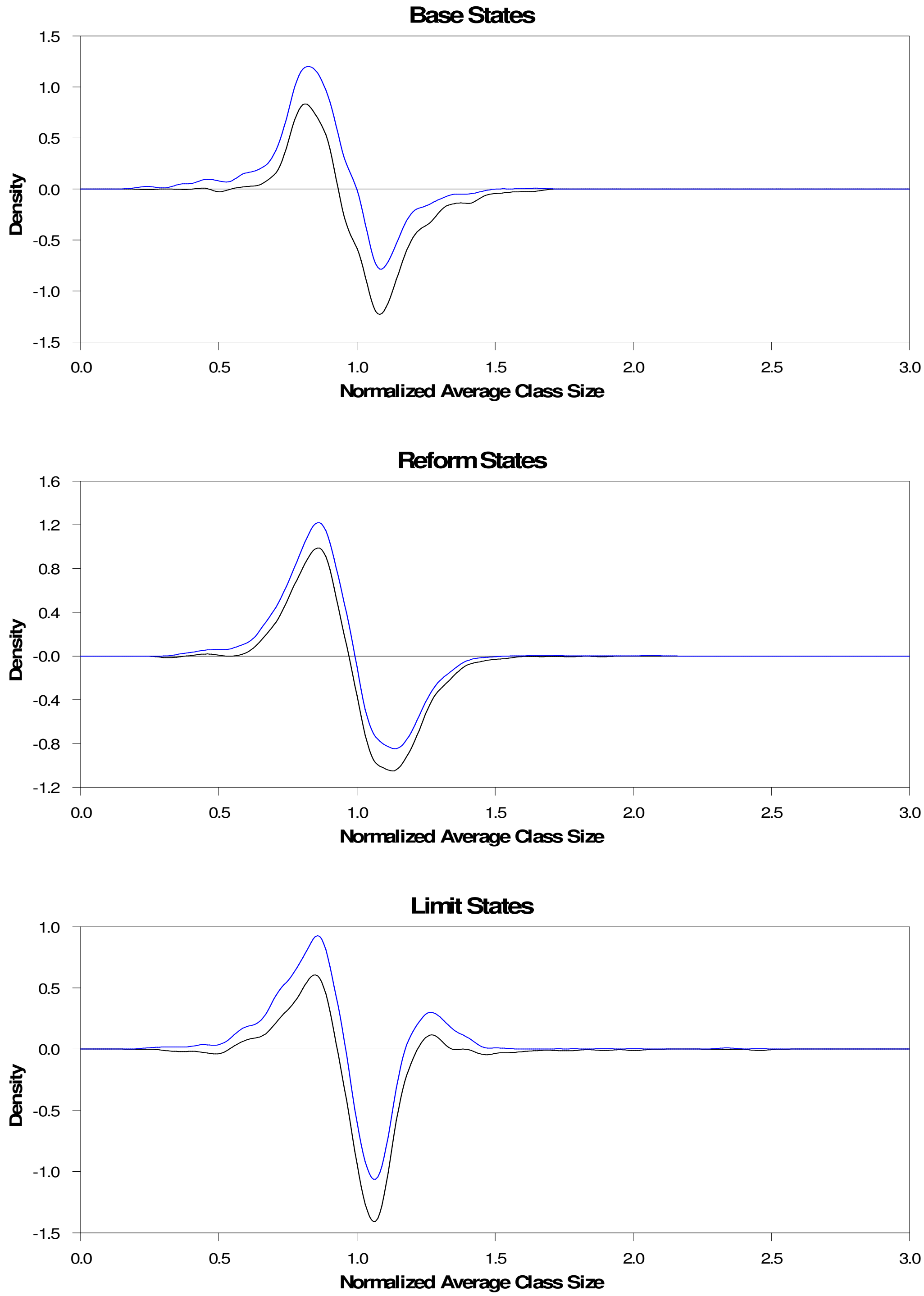

Figure 6
Ninety-Five Percent Bootstrap Confidence Intervals for Differences in Density Differences for Average Class Size