Do State Governments Matter?

A Review of the Evidence on the Impact on Educational Outcomes of the Changing Role of the States in the Financing of Public Education

Thomas A. Downes*

During the past several decades, federal and state governments have pursued a variety of redistributive policies aimed at fostering the idea of "equality of economic opportunity." This concept implies that although people's incomes may vary, the variance should be caused by factors such as individual ability and effort, not by differences in circumstance. Many in the policy arena have suggested that opportunities could be further equalized by implementing changes in the way elementary and secondary education is financed and delivered. Hanushek and Somers (1999) detail the most prominent state and federal policy initiatives aimed at reducing income inequality by modifying education finance and delivery.

This paper focuses on three sets of changes to the school finance landscape, and attempts to summarize the evidence on the effects of these changes on education quality and, ultimately, on the extent of inequality in American society. The first set of changes considered will be school finance reform and the large-scale changes in the formulas states use to determine aid to local school districts. For many years, those concerned with the persistence of income inequality in the United States have argued for reforms to the method of financing public elementary and

*Associate Professor of Economics, Tufts University and Visiting Scholar, Federal Reserve Bank of Boston. The author thanks Sheila Murray, Larry Kenny, Julian Betts, Eric Hanushek, and participants in the Federal Reserve Bank of Boston's conference, "Education in the 21st Century: Meeting the Challenges of a Changing World," for their helpful comments and suggestions.

secondary schools that would make education spending more equal. These arguments, which have been buttressed by substantial evidence that pre-market factors play a significant role in determining subsequent labor market outcomes (see, for example, Murnane, Willett, and Levy 1995; Neal and Johnson 1996; Bishop 1989), have been cited by those who have argued in the courts for fundamental reforms of the way in which public schools are financed (see, for example, Campaign for Fiscal Equity 2001). These court challenges have experienced a resurgence in the last several years, with state supreme court decisions mandating equalization in states such as Kentucky, Texas, Vermont, and New Hampshire further altering a school finance landscape that has changed dramatically since 1970. The end result of these court challenges is that, in almost every state in the nation, the system of financing the public schools has been fundamentally altered, with state governments becoming an ever more important part of the educational financing landscape. In this paper, I review the empirical evidence on the effects of these changes, concentrating on the relationship between school finance reforms and student outcomes.

The second set of changes summarized in this paper are those attributable to tax and expenditure limitations. Limitations on the ability of local governments to raise revenues or to make expenditures, like those imposed by Proposition 13 in California and Proposition 2½ in Massachusetts, have typically forced state governments to increase state-level taxes and state aid to public schools (Galles and Sexton 1998; Cutler, Elmendorf, and Zeckhauser 1999). From a distributional perspective, the effects of tax limitations on school accountability efforts are also important. I make no attempt to evaluate high-stakes testing and other existing accountability measures; I will leave that for other papers in this volume. Nevertheless, I will review evidence that suggests that tax limitations have significant imbedded incentives that may result in outcomes that are different from the intended consequences of increased fiscal accountability. Many of these incentives are present in other types of accountability systems, so it would behoove today's policymakers to take to heart the lessons of the tax revolt.

The third set of changes I address in this paper involves school choice. While no state has implemented the type of voucher system that Friedman (1955) advocates, small-scale, publicly and privately funded voucher plans exist in several localities. Further, 37 states and the District of Columbia currently have charter school laws, all of which, to a greater or lesser extent, allow for increased choice within the public system. In this paper, I will limit myself to recounting the evidence on the impact of the charter school movement, since this movement has been driven by state-level policy changes and because it represents the most widespread challenge to the traditional system of school finance. Documenting the

effect of charter schools will, over the next few years, be one of the most important tasks for researchers.

Each of the three changes to the school finance landscape could have numerous effects beyond student performance, which is the focus of the research summarized in this paper. Take, for example, school finance reforms. Researchers have attempted to quantify the effect of these reforms on house values (Dee 2000), community composition (Aaronson 1999; Downes and Figlio 1999b), private school attendance (Downes and Schoeman 1998), private school supply (Downes and Greenstein 1996), and private contributions to public schools (Brunner and Sonstelie 1997). My decision to restrict my discussion to the impact on student performance is driven by two considerations. First, policymakers are particularly interested in the impact of policy changes on student performance on standardized tests. This is made readily evident by the increasing prevalence of high-stakes testing and by the provisions in the No Child Left Behind Act of 2001 that make federal aid contingent upon measurable improvement in the quality of services provided. Second, as Hanushek and Somers (1999) note, recent research has documented a strong link between standardized test scores and earnings. Thus, policies that reduce dispersion in standardized test scores should, ultimately, reduce dispersion in earnings. Nevertheless, I do not want to leave the reader with the impression that the impact of these policy changes on the distribution of some of the other determinants of social well-being that are catalogued by Wolfe and Haveman (2002) is either uninteresting or unimportant. Quantifying these impacts unquestionably will be necessary in order to estimate the overall welfare implications of these policy changes.

The absence of good measures for many of the determinants of social well-being will make it difficult to quantify the link between these policy changes and the distribution of social well-being. But even quantifying their effect on the distribution of student performance, which is easily observed, has proven to be challenging, since none of these policy changes has occurred in isolation. One of the problems researchers have had to overcome is how to isolate the impact of one change in the system of school finance and delivery from all of the other changes, large and small, that are being implemented contemporaneously. Improvements in available data and in econometric techniques have, in recent years, resulted in an increasing number of studies attempting to isolate the effects of these policy changes on the distribution of student performance. Accompanying this literature have been several papers that critically review the literature. I have drawn heavily from these reviews, and I strongly encourage the interested reader to turn to these reviews for more exhaustive summaries of the existing state of knowledge. For school finance reforms, Murray (2001), Downes and Figlio (1999b, 2000), and Card and Payne (2002) offer alternative views of the effects. Downes and

Figlio (1999a, 2001) and Kirchgässner (2001) summarize the literature on the impact of tax limitations. And the recent monograph authored by Gill et al. (2001) represents a thorough, careful, and dispassionate overview of the evidence on the effects of voucher programs and charter schools; Miron and Nelson (2001) summarize and critically evaluate much of the research on the impact of charter schools on student achievement.

There are no pithy remarks that can summarize the lessons from this paper. The reality is that, while much progress has been made on quantifying the effects of these changes in the school finance landscape, much work remains to be done. Only for tax and expenditure limitations has any consensus concerning their effects on mean achievement begun to develop, and even for tax and expenditure limitations there is much still to be learned about their distributional implications. The main lesson, then, is that there is considerable room for additional research into the achievement effects of each one of these sets of policies.

The next section of the paper summarizes research examining the links between school finance reforms and student achievement. A review of the evidence on the effects of tax and expenditure limitations follows, as does a very brief discussion of the implications of this evidence for other accountability measures. The limited work on the impact of charter schools on student achievement is then presented. The paper closes with some suggestions for future research.

A Review of Research on the Impact of School Finance Reforms

The school finance reforms implemented in California after the *Serrano* v. *Priest* case and Proposition 13 represent a watershed both in the debate over the structure of school finance reforms and in the direction of research into the impact of those reforms. In the post-*Serrano* period, the California reforms and their supposed effects on the schools in that state have been discussed in every state in which school finance reforms have been implemented.[1]

The California reforms also shifted the focus of research to the impact of school finance. Prior to the reforms, the focus in the literature was almost solely on the impact of finance reforms on spending inequality. After *Serrano*, the scope of the analysis broadened to include the impact of finance reforms on the level and distribution of student achievement, on housing prices, on the supply of private schooling, and

[1] For instance, in Vermont, where Act 60 represents the most radical of the recent school finance reforms, examples of references to California include McClaughry (1997) and Mathis (1998).

even on the composition of affected communities.[2] The California reforms also became the touchstone for theoretical work. Papers like those of Nechyba (1996, 2000), Bénabou (1996), and Fernandez and Rogerson (1997, 1998) use a California-like system as the post-reform case when trying to reach predictions about the likely effects of finance reform.

The problem with using the California case as a benchmark is that the case has proven to be the exception, not the rule. First, the limits imposed on local control over spending have not been duplicated in any other state. Even in Michigan and Vermont, the states in which the most extensive post-*Serrano* reforms have been implemented, some degree of local control over taxes and spending is permitted. Further, the population of students served by California schools changed more dramatically than the population of students in any other state in the nation. From 1986 to 1997, the proportion of the California public school student population identified as minority increased from 46.3 percent to 61.2 percent. Nationally, the minority share grew far more slowly, from 29.6 percent to 36.5 percent. As Downes (1992) notes, these demographic changes make it difficult to quantify the impact of the finance reforms in California on the cross-district inequality in student achievement.[3]

The possibility that California might be the exception and not the rule pushed a number of researchers to pursue national-level studies attempting to document the impact of finance reforms. On the spending side, Silva and Sonstelie (1995), Downes and Shah (1995), and Manwaring and Sheffrin (1997) each take slightly different approaches to quantifying the effect of finance reforms on mean per pupil spending in a state. Because they use district-level data, Hoxby (2001a), Evans, Murray, and Schwab (1997), and Murray, Evans, and Schwab (1998) are able to consider not only the effects of finance reforms on mean spending but also the extent to which spending inequities were reduced by those reforms. As a result, these studies provide the most obvious sources for predictions of the long-run effects of school finance reforms.

The problem is that these studies generate contradictory predictions. The case of Act 60 in Vermont helps make concrete the disparity in predictions. Hoxby's results would lead us to expect leveling down, since Act 60 dramatically increases tax prices in towns with more property

[2] The papers dealing with these varied topics are too numerous to cite. Evans, Murray, and Schwab (1997) and Downes and Figlio (1999b, 2000) cite many of the relevant papers.

[3] Generating comparable numbers for earlier years is difficult. Nevertheless, the best available data support the conclusion that these sharp differences in trends in the minority share pre-date the *Serrano*-inspired reforms. For example, calculations based on published information for California indicate the percent minority in 1977–78 was approximately 36.6 percent. Nationally, estimates based on the October 1977 Current Population Survey indicate the percent minority was 23.9 percent.

wealth. Murray, Evans, and Schwab conclude that court-mandated reforms like Act 60 typically result in leveling up.

The same lack of a clear prediction would be apparent to the reader of national-level attempts to determine how the distribution of student performance in a state is affected by a finance reform. Hoxby (2001a) represents the first attempt to use national-level data to examine the effects of finance reforms on student performance. She finds that dropout rates increase about 8 percent, on average, in states that adopt state-level financing of the public schools. Although Hoxby's work does not explicitly address the effect of equalization on the within-state distribution of student performance, it seems likely that much of the growth in dropout rates occurred in those districts with relatively high dropout rates prior to equalization. In other words, these results imply that equalization could adversely affect both the level and the distribution of student performance.

While the dropout rate is an outcome measure of considerable interest, analyses of the quality of public education in the United States tend to focus on standardized test scores and other measures of student performance that provide some indication of how the general student population is faring. Husted and Kenny (2000) suggest that equalization may detrimentally affect student achievement. Using data on 37 states from 1987–88 to 1992–93, they find that the mean SAT score is higher for those states with greater intrastate spending variation. However, the period they consider post-dates the imposition of the first wave of finance reforms. Thus, the data do not permit direct examination of the effects of policy changes. In addition, because they use state-level data, Husted and Kenny cannot examine the degree to which equalization affects cross-district variation in test scores. Finally, since only a select group of students take the SAT, Husted and Kenny are not able to consider how equalization affects the performance of all students in a state.[4]

Card and Payne (2002) explore the effects of school finance equalizations on the within-state distributions of SAT scores. They characterize a school finance policy as more equalizing the more negative is the within-state relationship between state aid to a school district and school district income. They find that the SAT scores of students with poorly educated parents (their proxy for low income) increase in states that, under their definition, become more equalized. Data limitations, however, make it impossible for Card and Payne to examine the effects of

[4] Husted and Kenny do find evidence consistent with the conclusion that, in states in which school finance reforms had reduced the dispersion in per pupil expenditures, these reforms have had no impact on the standard deviation of SAT scores. Since, however, the standard deviation of test scores could be unchanged even if cross-district inequality in performance had declined, this evidence fails to establish that finance reforms do not reduce cross-district performance inequality.

policy changes on students residing in school districts in which the changes had the greatest impact. Moreover, while Card and Payne correct for differences in the fractions of the population taking the SAT, it is still very likely that the students who come from low-education backgrounds but take the SAT are a very select group and are extremely unlikely to be representative of the low-income or low-education population as a whole.[5]

Downes and Figlio (2000) attempt to determine how the tax limits and finance reforms of the late 1970s and early 1980s affected the distribution of student performance in states in which limits were imposed. They also examine how student performance has changed in these states relative to student performance in states where no limits or finance reforms were imposed. The core data used in the analysis were drawn from two national data sets, the National Longitudinal Study of the High School Class of 1972 (NLS-72) and the 1992 (senior year) wave of the National Educational Longitudinal Study (NELS). The NELS data were collected well after the passage of most finance reforms. This permits quantification of the long-run effects of these reforms by analyzing changes in the distributions of student performance between the NLS-72 cross-section and the NELS cross-section.

Downes and Figlio (2000) find that finance reforms, in response to court decisions, result in small and frequently insignificant increases in the mean level of student performance on standardized tests of reading and mathematics. Further, they note that there is some indication that the post-reform distribution of scores in mathematics may be less equal. This latter result highlights one of the central points of the paper: Any evaluation of finance reforms must control for the initial circumstances of affected districts. The simple reality is that finance reforms are likely to have differential effects in initially high-spending and initially low-spending districts.

Downes and Figlio's (2000) finding that court-ordered finance reforms may be associated with increased dispersion in student performance is echoed by results produced by Hanushek and Somers (1999). Hanushek and Somers use data on earnings of workers who are 25 to 37 years old in 1990 to calculate within-cohort variation in earnings. Like Husted and Kenny (2000), they do not directly estimate the effect of finance reforms, choosing instead to relate the extent of equalization in a state to the extent of earnings variation among those who were born in that state. They find that, for white males and females, earnings variation

[5] For instance, among the students in Card and Payne's low-parental-education group, in 28 states in 1978 (25 states in 1990) fewer than 10 percent took the SAT examination and in 20 states in 1978 (15 states in 1990) fewer than 3 percent took the SAT. Further, in 1978 no state had more than 36.2 percent of the low-parental-education group take the SAT.

is negatively related to the extent of spending variation across high schools in the cohort's birth state at the time the cohort attended high school. Only for black females is there any evidence that reductions in school spending variation are associated with reductions in earnings variation.

The contrast between the results of Card and Payne and those of Hanushek and Somers highlights the challenge facing anyone trying to predict the impact of potential reforms on any state's system of school finance. The fundamental reason for the absence of clear predictions of the impact of finance reforms has been mentioned by a number of authors (see for example, Downes and Shah 1995; Hoxby 2001a; Evans, Murray, and Schwab 1997), all of whom have emphasized the tremendous diversity of school finance reforms. In a national-level study, any attempt to classify finance reforms will be imperfect. Even though there is general consensus that the key elements of a finance reform are the combined effects of the reform on local discretion and on local incentives, and the change in state-level responsibilities in the aftermath of reform (Hoxby 2001a; Courant and Loeb 1997), different authors take different approaches to account for the heterogeneity of the reforms. The result is variation in predictions generated by studies that are asking the same fundamental question. The answer is not, it seems, to try to improve the methods of classifying reforms but is, instead, to complement these national-level studies with case studies of canonical reforms. Only national-level studies can reveal if, in a state in which school finance reforms have been implemented, the mean performance of students has changed relative to what this performance would have been in the absence of the finance reforms. If, however, the research question is whether the finance reforms have altered the distribution of student performance, both state-level and national-level studies can provide results that can be used to answer the question. And only state-level case studies can convincingly indicate which, if any, characteristics of reforms are linked to success in reducing the extent of performance inequality.

The most direct antecedent in this case-study approach to analyzing finance reforms is Downes (1992), who shows that the extensive school finance reforms in California in the late 1970s generated greater equality across school districts in per pupil spending but not greater equality in measured student performance. For all the reasons noted above, replicating this style of analysis for other states is imperative. Downes's (2002) work on Vermont, Flanagan and Murray's (2002) work on Kentucky, and Duncombe and Johnston's (2002) work on Kansas offer examples of recent case studies of canonical reforms.

The diversity of school finance reforms is apparent as one looks across these case studies. What is striking is the similarity across studies in the estimated achievement effects. Pre-finance reform data on student test scores are not available to Duncombe and Johnston; they find no

evidence of diminished dispersion in performance when examining post-finance-reform test scores. They also document some recent relative improvement in dropout rates in high-poverty districts, though they also find increased dispersion in dropout rates when comparing pre- and post-finance-reform data.

The bottom line of Duncombe and Johnston's analysis of dropout rates is that reform has resulted in small relative improvements. Downes (2002) and Flanagan and Murray reach similar conclusions—post-reform dispersion in schooling outcomes has declined, but this decline in dispersion has been small. Downes finds that there have been, at most, small relative improvements in the test performance of fourth and eighth graders in those school districts with lower pre-reform per pupil spending and per pupil property wealth. Flanagan and Murray find that relative increases in post-reform spending were translated into relative gains in post-reform test performance, but these gains were quantitatively small. Somewhat surprisingly, then, the results of these new case studies tend to echo the results of the earlier work on California. Thus far, the case studies have confirmed a conclusion that was reached by many of the researchers who executed national-level analyses: The types of finance reforms that have been implemented in response to court orders appear to have little, if any, impact on the distribution of student test performance.

Do Tax and Expenditure Limits Affect Student Performance?

Like research into the impact of school finance reforms, research into the effects of tax limits blossomed after a major policy change in California. Much of this research focused, however, on the fiscal implications of tax limits; see Fisher (1996) for an interpretive review of this work. The passage of Proposition 13 in California in 1978, followed by the 1981 approval of Proposition 2½ in Massachusetts, did not stimulate immediate research on the impact of tax and expenditure limits on student performance. That there was a lag between implementation of these limits and research on the link between the limits and schooling outcomes is not surprising, since limits are unlikely to affect the performance of most public students in the short term. What is surprising is that by 1990 there were few studies in which the impact of limits on service provision was examined. Further, the studies that existed were exclusively case studies that considered the effects of limits on a variety of services, including public education, but that did not use explicit measures of student performance to gauge the effects of limits on those served by the schools.

Nevertheless, case studies like those of the Joint Budget Committee (1979) and Schwadron (1984) for California and Greiner and Peterson

(1986) for Massachusetts present a relatively consistent picture of the short-run effects of tax limits on service quality. In general, residents of the states considered by the referenced studies perceived a drop in service quality. That this perception reflected reality was sometimes, though not always, confirmed by objective measures of service quality (Greiner and Peterson 1986). Government officials responded to the limits by first making cuts in capital expenditures and in areas of current expenditure that these officials felt were peripheral. For example, in California, school administrators sought to protect the core academic subjects, choosing instead to make cuts by pursuing such strategies as reducing the diversity of course offerings and the number of pupil service employees.[6]

Given their timing, these case studies could not be used to draw any conclusions about the long-run effects of tax and expenditure limits. Also, even though these case studies moved beyond examination of the fiscal impacts of limits, the concerns raised in the introduction to this paper imply that the results of these case studies could not be used to predict with confidence the effect of limits on student outcomes. Only by examining student outcomes directly and by determining how these outcomes had changed relative to the pre-limit baseline, could researchers ascertain the effect of limits.

The first research to compare pre- and post-limitation measures of student performance was Downes (1992). In that study, data on district means of performance on the California Assessment Program test were assembled for 170 unified (K–12) districts in 1976–77 and 1985–86. In these districts, the measure of student performance actually increased by 5 points, on average. Further, the cross-district distribution of student performance was essentially unchanged between 1976–77 and 1985–86. The bottom line of this research, it would seem, is that Proposition 13 did not produce a long-run reduction in student performance at any point on the performance distribution.

Such a conclusion would be unwarranted, however. As was noted above, contemporaneous with the state and local response to Proposition 13 was state implementation of school finance reforms made necessary by the *Serrano* decision.[7] This observation raises a problem that faces any researcher attempting to isolate the impact of tax limits on public

[6] The results in Downes (1996) suggest that school administrators in California did not respond to the limits by cutting the administrative staff. For a national cross-section, Figlio (1997) also finds no evidence of cuts in administration.

[7] Fischel (1989, 1996) makes a strong case that, in fact, the prospective school finance reforms that were compelled by the *Serrano* decision stimulated enough additional support for tax limits to make passage of Proposition 13 inevitable. If this logic is right, any observed changes in the distribution of student performance in California should ultimately be attributed to the finance reforms, not the resultant tax limits.

schooling. Frequently, states have implemented major school finance reforms close in time to the passage of tax limits. Thus, the effects of either school finance reforms or tax limits can be isolated only by looking across states or by examining the long-run experience in a state in which a limit was passed and no major changes in the school finance system had occurred.

Three recent papers take this lesson to heart and, thus, provide a model for future empirical research on the impact of tax and expenditure limits. Using a cross-section of student-level data from the National Education Longitudinal Survey (NELS), Figlio (1997) finds that, all things equal, the performance of tenth graders on mathematics, reading, science, and social studies tests was significantly lower in those states in which local school districts faced either revenue or expenditure limits. Since, however, Figlio's variation was cross-sectional, he was unable to rule out the possibility that some combination of sorting and unobserved tastes for education resulted in both the passage of limits and less rapid improvement in student performance.[8]

To avoid this problem, Downes, Dye, and McGuire (1998) examine the recent imposition of property tax limits on school districts in the Chicago suburbs. They conclude that, in the short term, these limits translated into slower growth in the performance of third graders on a standardized test of mathematics. Similar slowing of growth is not observed for third-grade reading test scores or for the test scores of eighth graders. The authors also note that the effects of these limits varied across districts. What the authors could not do is argue that their results provide a definitive picture of the long-term effects of any tax or expenditure limits, since they observe only three post-limit years and since the Illinois case could be exceptional. Their paper is difficult to draw conclusions from because, like Figlio's paper, Downes, Dye, and McGuire's conclusions are driven by unobserved differences between the "control" and "treatment" groups in the analysis.

The third paper, Downes and Figlio (2000), discussed earlier, builds on the strengths of these two studies. One lesson from these two papers is that evaluating the effects of tax limits requires not only before and after data on students in districts subject to limits but also a control group of students from states in which no limits have been enacted. With this observation in mind, Downes and Figlio attempt to determine how the tax limits of the late 1970s and early 1980s affected the distribution of

[8] The same problem plagues the work of Shadbegian (2001), who finds that student test performance is lower in those Massachusetts districts forced to cut property taxes in the aftermath of Proposition 2½. Unfortunately, because he has no data on pre-Proposition 2½ test performance, Shadbegian is unable to rule out the possibility that there exist unobservable factors that resulted in lower test performance and that are correlated with the extent to which a locality was constrained by Proposition 2½.

student performance in states in which limits were imposed and how student performance has changed in these states relative to student performance in states in which no limits were imposed.

The results in Downes and Figlio confirm, in part, the results of Figlio (1997). Specifically, the imposition of tax or expenditure limits on local governments in a state reduced student performance on standardized tests of mathematics skills by 1 to 7 percent, depending on model specification. However, there was no general evidence that tax limits affected student performance on standardized tests of reading skills, except when tax limits were treated as endogenous—that is, when the researchers estimated a regression model in which the possible reverse causality between test scores and tax limits was taken into account.[9] This latter result—no general finding of an effect on reading performance—parallels one of the findings of Downes, Dye, and McGuire. It is sensible, given the age of the test-takers, to believe that high school mathematics differences may be more attributable to differences in schooling than are high school reading differences, so the generally stronger effect of tax limits on mathematics than on reading should not come as much of a surprise.

For the most part, when researchers have examined the impact of tax limits on student performance, they have confined their analysis to students who remain in the public schools. Bradbury, Case, and Mayer (1998) represents a break from this norm, analyzing the relationship between grade-level enrollment patterns and various indicators of the bindingness of tax limits. Since differences between actual enrollment patterns and the patterns of enrollment implied by the decennial Censuses reflect primarily withdrawal from the public schools, either to private schools or nonenrollment status, the results from their paper shed some light on the effect of tax limits on dropout rates. Bradbury, Case, and Mayer find that the share of the potential student population served by the public schools is lower in districts in which more initial cuts were necessary when the limits were first imposed. This result suggests that limits could increase dropout rates, though further research on this question is clearly needed.

Another recent paper, Downes and Figlio (1999b), provides the first attempt to study the performance effects of tax limitations (and school finance reforms) on private school students. This study uses a similar methodology to that used by Downes and Figlio (2000) to investigate the effects of tax limits on public school performance. While their results are more compelling for school finance reforms than for tax limitations, Downes and Figlio (2000) find limited evidence of a modest (though

[9] See Figlio (1997) or Downes and Figlio (2000) for more of a discussion of the potential endogeneity biases, as well as a detailed treatment of the issue of reverse causality.

imprecisely estimated) negative effect of tax limits on student test scores in the private sector. This result, if one considers only the magnitude and not the statistical significance of the finding, could be interpreted in several ways. One possibility is that tax limits may tend to lower the quality of the private sector, either because of lower competition from the public sector or for other reasons, such as peer effects. Another possibility is that the lower test performance is a manifestation of increased selection into the private sector by students less able than those who populated the private sector before the limitations' passage (though still, on average, more able than the typical public school student).[10] Though this line of research provides a first look at the overall distributional consequences of tax limitations, it is clear that much more work is needed on this topic.

Evidence on the impact of tax and expenditure limits on the cross-district distribution of student performance, while consistent across studies, is less compelling than evidence on the impact of these limits on mean performance. Specifically, Downes, Dye and McGuire (1998) and Downes and Figlio (2000) find that student performance appears to deteriorate more in economically disadvantaged localities, though these cross-locality differences—while consistent in direction across specifications—frequently proved to be statistically insignificant. Nevertheless, this limited evidence on the nonuniformity of the effects of tax limits suggests the need for further research on the dependence of these effects on a district's initial conditions and demographics.

While it is not clear whether tax limitations are good policy, arguably this literature does clarify that policies with one set of desired outcomes may have another set of unintended consequences—both favorable and unfavorable. These lessons are interesting on their own merits, but they are also important because of the possible applicability of these lessons from the fiscal accountability-driven tax revolt to the new wave of public accountability.

Since the early 1990s, there has been a national trend toward increased school-level accountability in education. Today, almost every state in the United States conducts regular testing of students, and most have high stakes attached to student test performance, such as potential grade retention or failure to graduate from high school. Because these accountability policies are so new, there has been virtually no formal evaluation of their effects. However, we know from the literature on tax and expenditure limitations that one possible reason for reductions in

[10] Epple and Romano (1998) theoretically describe stratification patterns between the public and private sectors that predict precisely this result—that reduced public sector spending leads to the movement of "top" public school students into the private sector, reducing the average performance level of both the private and public sectors. Epple, Figlio, and Romano (1998) offer some empirical justification of the stratification patterns identified in the theoretical model.

student performance in excess of what might be expected given the change in financial resources is that the incentives associated with tax limits might lead to reduced, rather than increased, efficiency (Figlio and O'Sullivan 2001). It is true that, in the case of increased accountability, the incentives are less one-sided. Specifically, even if the rent-seeking administrator model is a correct representation of school decision-making, this model is consistent with increased resources and attention being paid to factors that might improve student outcomes in an atmosphere of increased accountability. On the other hand, the same types of models would suggest that school administrators might substitute resources away from productive uses not covered under the accountability system to improve performance in the areas specifically being considered. The evidence from tax and expenditure limitations, therefore, implies that increased accountability may not lead to increased efficiency. Accountability policies should be structured with this lesson in mind.

Does Public Sector Competition Raise All Boats? Impact of Charter Schools on Student Performance

For a segment of the education market that serves such a small fraction of students (about 1 percent nationally in 2000–01), charter schools have received a seemingly inordinate amount of attention in the popular press and in general discussion of education reform. The centrality of charter schools in the popular discussion of education reform is signaled by the fact that public school choice and increased federal support for charter schools were two of the major provisions of the No Child Left Behind Act of 2001 that was approved by Congress and signed by President Bush in January 2002. Across the political spectrum, policymakers appear to accept the argument of proponents of charter schools—they "can strengthen public education by promoting competition and liberating innovators from the shackles of tradition" (Toch 1998, p. 34).

Whether charter schools will, in fact, fulfill this promise remains uncertain. What is certain is that the character of a state's charter schools depends critically on how state policymakers spell out the details of charter school financing (see Gill et al. 2001 for further discussion). The financing decisions state policymakers must wrestle with include how much money follows each pupil who enrolls in a charter school, whether start-up funds will be available for charter schools, and whether state moneys will be made available to assist charter schools in securing facilities. Even decisions about whether to allow existing private schools to convert to charter status have significant financial implications. Charter schools, therefore, have the potential to necessitate major changes in a state's system of school finance. And charter schools certainly alter the

education landscape, as public school officials in Mesa, Arizona (Toch 1998) and Inkster, Michigan (Wildavsky 1999) have seen.

The relative newness of the charter school movement[11] has meant that the research on the impact of this movement is in the formative stage. In most states, charter schools are simply too new and too small a part of the education sector for any measurable effect to be expected.[12] There are, however, a few states in which the charter school sector has begun to mature. Several authors have taken advantage of this maturation to quantify the effects of the entry of charter schools.[13]

In the earliest of these studies, Bettinger (1999) uses data from Michigan to address one of the central questions in the public school choice debate: Will the presence of charter schools (or of other choices in the public sector) improve the performance of public school students who do and do not attend choice schools? The available data allowed him to examine school-level performance measures, control for student performance at the time the first cohort of test-takers entered the charter school, and account for a rich set of student demographic characteristics. In his preferred specifications, Bettinger finds little evidence of improvement in charter schools in the test performance of successive cohorts of fourth and seventh graders. In fact, some relative decline in performance is apparent in his estimates. Further, even after accounting for the possible endogeneity of charter school location, Bettinger observes no relationship between student performance in traditional public schools and the extent of charter school entry. These results would appear to support the conclusion that charter schools fail to generate direct or indirect improvement in student performance.

For several reasons, however, the Bettinger results cannot be viewed as the final word on charter schools. First, as Bettinger himself notes, he is unable to quantify the long-run effects of charter schools. Further, he notes that the poor performance of the charter schools in his sample may be attributable to "institutional immaturity" (p. 21). Also, since the charter schools in Bettinger's data are relatively new, many of the students being tested will be finishing their first year in a new school

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[11] The first legislation permitting the creation of charter schools was enacted in Minnesota in 1991. And much of the growth of charter schools has occurred over the last several years, with the number of children in charter schools tripling over the last three academic years. In addition, in 1999–00 only in seven states were more than 1 percent of all students enrolled in charter schools, with the District of Columbia, Arizona, and Michigan exhibiting the most entry.

[12] The relative smallness of the charter school sector would not preclude estimating the effect of charter schools on students attending those schools. But, if charter schools are not seen by traditional public schools as being real competitors, it is unlikely that any competitive effects will be observed.

[13] For far more thorough reviews of the research on charter schools and their effects, see Gill et al. (2001) and Miron and Nelson (2001).

environment. Performance declines would be expected for such students, as such declines after changing schools are well documented in the literature (O'Brien 2002). In addition, Bettinger cannot track cohorts and, therefore, cannot control adequately for pre-charter performance of the cohorts who are tested in the first and second years of charter school operation. Finally, since charter school policies vary dramatically from state to state, lessons from one state may not apply to others. Still, Bettinger's results are hardly a ringing endorsement for charter schools.

Three more recent studies support the argument that Bettinger's results on the impact of charter schools on their own students may understate the long-run impact. In the first of these studies, Eberts and Hollenbeck (2001) examine individual-level test score data on fourth and fifth graders in traditional and charter public schools in Michigan. To create their comparison group of students in traditional public schools, Eberts and Hollenbeck determine the public school district in which each charter school in Michigan was located and include in their sample all of the students in traditional public schools in that district. Like Bettinger, they find some evidence of lower levels of and smaller gains in test scores for students in charter schools.[14] However, when Eberts and Hollenbeck control for the length of time for which a charter school had been opened, they find that the gaps between the performance of students in traditional and charter schools are smaller the longer the charter school had been in operation. Bettinger's suggestion that "institutional immaturity" matters appears to be correct.

It also appears that the performance of individual students increases as those students spend more time in the charter school. Eberts and Hollenbeck do not examine this possibility, in part because they lack the yearly test score data that make examining gains feasible. Two studies, one for Texas (Gronberg and Jansen 2001) and one for Arizona (Solmon, Paark, and Garcia 2001), are able to consider gains and, therefore, can isolate the effect of time spent in the charter school. In both studies, the test scores of students decline in their first year in a charter school. However, Solmon, Paark, and Garcia find that as students spend more time in charter schools their test scores rise relative to their counterparts in traditional public schools.[15] For students in charter schools in which a

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[14] Actually, Eberts and Hollenbeck cannot control for previous test performance in the same subject. In the first of their gains equations, they use a student's fourth grade test score in mathematics as a control when estimating the impact of charter school attendance on fifth grade science test scores. Similarly, they use the student's fourth grade reading score as the pre-test score when examining fifth grade writing test scores.

[15] Nelson and Hollenbeck (2001) raise a number of methodological concerns about the Solmon, Paark, and Garcia analysis. Nelson and Hollenbeck's principal suggestion is that, given the nature of the Arizona data, the evaluation of the impact of charters should be limited to only those students who were in their first year in a charter school. For the reasons noted above, estimating the impact of charters using only recent movers is likely to

disproportionately high share of the students are at-risk, Gronberg and Jansen observe a similar relative increase.

These latter two studies do not, however, contradict all of Bettinger's findings. Gronberg and Jansen find relative performance declines in those charter schools serving disproportionately low shares of at-risk students. Further, Gronberg and Jansen's estimates indicate that student performance is particularly low in start-up charter schools.

As this brief review indicates, the evidence on the impact of charter schools on student performance is decidedly sparse. New charter schools need time to become established; relative performance in these schools is likely to be low in their first and even their second year. Whether, in the long run, charter schools in some states raise the test scores of their students remains an open question, particularly given the tremendous variation across states in the structure of charter school programs and the differences between the Solmon, Paark, and Garcia and Gronberg and Jansen studies in the estimates of the long-run impact of charter schools on the performance of non-at-risk students.

The long-run impact of charter schools on students who remain in traditional public schools also remains an open question. As was noted above, Bettinger finds that charter school entry results in no significant change in the relative performance of students who remain in traditional public schools located in the drawing area of the charter school. Eberts and Hollenbeck present a mixed picture of the competitive effects of charter schools. In their preferred specification, they find that fifth grade science and writing scores are relatively higher in those traditional public schools situated in districts in which charter schools are located. However, fourth grade math scores are relatively lower in such schools, and fourth grade reading scores are not significantly different.

Like Bettinger and Eberts and Hollenbeck, Hoxby (2001b) attempts to estimate the competitive impact of charter schools in Michigan. She also examines the competitive impact of charter schools in Arizona, the other state in which the charter school sector is relatively mature. Hoxby argues that only in districts in which charter schools serve at least 6 percent of the students would we expect to see noticeable competitive effects. Thus, her empirical strategy is to ask whether, in those traditional public schools in districts where charter schools serve at least 6 percent, the growth in student achievement has been faster than in those districts in which the 6 percent threshold has not been crossed. In both Michigan and

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understate significantly the long-run impact of charters. Thus, this particular methodological concern seems misguided. The remaining concerns of Nelson and Hollenbeck have considerable merit; whether accounting for these concerns would overturn the results of Solmon, Paark, and Garcia remains an open question.

Arizona, she finds that, particularly at the fourth grade level, the growth has been faster in those districts with substantial charter entry.

The results of Eberts and Hollenbeck's and Hoxby's (2001b) studies would appear to imply that, in fact, charters may well generate a positive competitive effect. And, the fact that both of these studies generate stronger competitive effects than Bettinger could be explained by the fact that Eberts and Hollenbeck and Hoxby are examining public school systems in which the charter school sector is mature and in which traditional public schools have had the opportunity to respond to their new competitors. But, the differences between these latter two studies and that of Bettinger could also be attributable to critical methodological differences. For example, Bettinger correctly observes that controlling for the endogeneity of charter school location is critical. Otherwise, the possibility exists that improvement in the traditional public schools is driven not by charter school entry but by some unobservable factor that drives both charter entry and test score gains in the traditional public schools.[16]

Since neither Eberts and Hollenbeck nor Hoxby account for endogeneity, their estimates of competitive effects must be treated with caution. Similarly, only Bettinger is able to include compelling controls for the pre-test status of students. In other words, when they estimate competitive effects neither Eberts and Hollenbeck nor Hoxby completely rule out the possibility that differences in the cohorts of students tested drive the estimated effects. Finally, while Hoxby's argument that we would expect to see competitive responses only in those districts in which the charter school presence is sufficiently large is compelling, her choice of a 6 percent threshold seems arbitrary. Further, she gives no indication how the results would change if that threshold were lowered. The reality is that the results of Eberts and Hollenbeck and Hoxby do not provide definitive estimates of the competitive effects of charter schools.

What is apparent is that none of the extant research supports the conclusion that the charter school movement will do irreversible damage to the students served by charter schools or to those who remain in traditional public schools. Even the worst-case estimates indicate that relative performance declines in charter schools are small and that students who remain in traditional public schools are essentially unaffected. And, even if the small declines in the performance of charter school students are real, these declines must be balanced against the increased satisfaction of parents of children in charter schools (Gill et al. 2001).

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[16] Betts (2002) gives one example of such a factor.

Concluding Remarks

The preceding review of three major changes in the school finance landscape indicates that, while we have learned much from previous research, much still needs to be learned about the effects of these changes. As is apparent from the most recent charter school studies, new data sets in which students are tracked over time will make it easier for researchers to quantify conclusively the effects of policy changes. What may be less apparent from the preceding discussion is the need for researchers to acknowledge that policies that have the same name in two states may actually be very different. School finance reforms, tax and expenditure limitations, and legislation enabling the creation of charter schools have as many differences across states as they have commonalities. The challenge facing researchers is to determine what lessons can be learned only from national-level analyses and only from state-level case studies and to distill these lessons for policymakers. The recent review by Gill et al. on the evidence of choice is a nice example of the type of work that will need to be an essential part of future research.

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