

## *Discussion*

# DO STATE GOVERNMENTS MATTER?

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One of the glories of the American education system is its unique local governance structure, which places substantial responsibility for educational policy in locally elected school boards. Historically related to this governance structure has been a system for financing public education, which is also rooted in local communities and, by and large, is tied to local systems of property taxation.

At one time, variations in the property wealth of local communities were limited and resulted in only mild disparities in the funding available for schooling purposes in different communities. Population shifts to the suburbs in recent decades have resulted in huge differentials in the property values of various urban, suburban, and rural communities, and have vastly exacerbated school funding inequities. As a result, in the twenty-first century, the American system of educational finance, with its emphasis on local real estate property taxation, creates serious injustices. In most states, the reliance on local property values has resulted in the anomalous reality that students with the greatest educational need have the least amount of educational resources available to them.

The demographic and economic growth of the suburbs has been accompanied by a trend toward increasing suburban domination of state legislatures. This has made it difficult for reformers to achieve legislative solutions to funding inequities. Accordingly, residents of property-poor school districts have tended to seek relief in the courts. Almost 30 years ago, a major challenge to the inequities in Texas's system of school

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finance was brought before the United States Supreme Court in *Rodriguez v. San Antonio Independent School District*.<sup>1</sup> The Supreme Court sympathized with the plight of the largely Chicano plaintiffs, whose property-tax rate was approximately 25 percent greater than their neighbors' in the nearby affluent Anglo district, but whose schools had only half of the resources available for their children's education. However, having determined that education was not a "fundamental interest" under the federal constitution, the Supreme Court held that the federal courts could not remedy this problem.

To the surprise of many observers, and in one of the most remarkable chapters in the history of state constitutional law, the state courts have, over the past three decades, entered energetically into the fray after the Supreme Court closed the federal courthouse doors. Since the decision in *Rodriguez*, litigations addressing funding inequities have been filed in 44 of the 50 states, and in some states on multiple occasions. Commentators have described various "waves" of outcomes in these cases, marked by varying trends in the reformers' degree of success. (Thro 1990; Levine 1991). Since 1989, there has been a clear trend toward plaintiff success, with reformers prevailing in approximately two-thirds of the 25 major decisions of states' highest courts (Rebell 2002).

Tom Downes's paper provides an excellent survey of the studies that have been undertaken by economists and social scientists in recent years to try to determine the impact of these litigations in terms of (1) reducing disparities in per capita spending among local school districts, (2) increasing overall educational expenditures within a state, and (3) improving student achievement, especially for the most disadvantaged students. Noting the "tremendous diversity of school finance reforms," Downes cautions that any attempt in a national-level study to classify finance reforms will be "imperfect." Accordingly, he recommends complementing these national-level studies with state-level case studies of concrete reforms.

I would go further. My contention is that the tremendous diversity in facts, legal rights and requirements, political context, and specific holdings of courts in various states makes it impossible to draw meaningful conclusions from national-level studies on the impact of fiscal equity litigation. Pursuit of such studies not only misallocates scholarly resources, but the results of these efforts can seriously mislead the public, the press, and policymakers.

I will illustrate this fundamental point by referring to one of the leading studies in this area, undertaken in 1998 by Murray, Evans, and Schwab, which is quoted in Downes's paper.<sup>2</sup> These authors studied the

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<sup>1</sup> No. 71-1332 Supreme Court of the United States 411 US 1; 93 S. Ct. 1278; 1973.

<sup>2</sup> See also Murray, Evans, and Schwab (1999).

outcome of decisions favorable to plaintiffs in 16 states over the period 1972 to 1992. They conclude: "Successful litigation reduced inequality by raising spending in the poorest districts while leaving spending in the richest districts unchanged, thereby increasing aggregate spending on education" (p. 789).<sup>3</sup>

The first problem with this and other national-level studies is the manner in which Murray, Evans, and Schwab identify the states to include in their sample. For example, among the 16 states they study was Alabama, where a trial court issued an extensive reform decision in 1993. Because of the intricacies of Alabama politics, however, the Alabama Supreme Court rejected the remedial order in 1997, and, just this year, formally overruled the 1993 liability decision.<sup>4</sup> Was it reasonable, therefore, to include the Alabama case as an example of a state in which plaintiffs had prevailed? Can any findings concerning a lack of equalization or lack of impact on student achievement over the past 10 years in Alabama fairly be correlated with the ineffectiveness of judicial intervention when there had not actually been any judicial intervention into the educational system?

Consider also Arizona. There, the State Supreme Court issued a major ruling in 1994 that was concerned solely with capital funding.<sup>5</sup> Almost all of the other decisions analyzed by Murray, Evans, and Schwab focus exclusively on operating expenses. Lumping together cases with such different goals and impacts in one analysis of outcomes is highly questionable.

A related point has to do with the time period encompassed by the analysis. The Murray, Evans, and Schwab study covers a broad time frame, incorporating all cases decided during the 23-year period from 1971 to 1994. Since implementation of court decrees is often a lengthy process, it is likely that there will be a greater impact for cases that were decided at the beginning of the study's time period than for those at its end. On the other hand, changes initiated in the early years may be undone by political developments in later years, as occurred in the State of Washington, where reforms initiated to benefit urban areas ultimately came to hurt them (Cipollone 1998).<sup>6</sup>

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<sup>3</sup> I deliberately chose this work to criticize and to illustrate my thesis partially because their conclusion that litigations, by-and-large, result in productive reforms is highly congenial to me, as an advocate for fiscal equity reform and a lawyer who is currently representing plaintiffs in a major education adequacy litigation. (See *Campaign for Fiscal Equity v. State of New York*, 655 NE.2d 661 (NY 1995), 719 NYS.2d 475 (NY Sup. Ct. 2001), reversed 2002 WL 1369966 (N.Y.A.D. 1 Dept.), appeal pending, N.Y. Ct. App.). Nevertheless, as a legal scholar and social-policy analyst, I must question the validity of this methodology.

<sup>4</sup> *Alabama Coalition for Equity v. Siegelman*, Index No. 1950030, S.Ct. Ala (May 31, 2002).

<sup>5</sup> *Roosevelt Elementary School Dist. No. 66 v. Bishop*, 877 P.2d 806.

<sup>6</sup> Evans, Murray, and Schwab (1997) do utilize a ten-year-after reform variable, and conclude that it does not substantially differ from their overall findings (p. 24), but this

The extent to which the years studied can substantially affect the analysis of the impact of a court case in a particular state is starkly illustrated by Michael Heise's 1995 study of the impact of the Connecticut Supreme Court's 1977 ruling in *Horton v. Meskill*.<sup>7</sup> Heise concludes that overall the court decision was "associated with declines in state education funding" (p. 212). At the same time, he notes, however, that there was a marked increase in spending during the period 1984 to 1987. Even though the initial decision was issued in 1977, significant action was not taken by the legislature until, in a 1983 follow-up decision, the Court put the legislature on notice that delays in fully funding the new constitutional scheme would not be tolerated. The fact that there were declines in expenditures during the period of noncompliance is not unexpected. The significant fact is that spending sharply increased after the Court's 1983 follow-up decision.

A related problem in this area is that averages, even if accurate, actually tell us little or nothing. Even if all of the court decisions included in a particular categorization are appropriate and the time period involved somehow is fully inclusive, the fact that on average court decisions in a variety of states do or do not have a particular impact provides little useful information for reformers or analysts. Since we know empirically that some court decisions have great impact and others have little or none, average quantitative results are not meaningful. In a 16-state sample, indicators of positive overall impacts may mean that judicial reforms had very strong impacts in three or four states and minor or negative impacts in a dozen others. Or the converse could be true: Broad positive impacts in many states could be countered by strong negative impacts in a few. Should reformers, therefore, look to the courts for relief? Is an investment in litigation worth the time and expense involved? Conclusions based on the averaging of outcomes provide no useful answers to these questions.

Case studies of outcomes in particular states, on the other hand, do provide meaningful information for answering these questions. They can inform us about the precise impact particular judicial interventions have had over specific periods of time. Reasonable conclusions can be drawn about the success of various legal strategies in such empirical analyses. Advocates and researchers considering the relevance of judicial interventions in another state will then be in a position to consider and compare meaningful specific variables.

In short, well-done case studies of the outcomes of litigations in particular states can provide a rich source of data for these analyses.

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summary snapshot does not consider the impact of timing variables in each of the particular states.

<sup>7</sup> 376 A.2d 359 (Ct. 1977).

Downes's case studies of the impact of judicial decrees in California and Vermont are prime examples of such studies (Downes 1992, 2002). It may also be analytically useful to undertake a large series of related state case studies ("or caselets") and draw conclusions from a qualitative and quantitative assessment of trends revealed by such analyses.<sup>8</sup>

Arguably, the mere fact that litigation has been filed will have a noticeable effect on policymakers and political outcomes. In fact, one study has specifically found that education expenditures tend to rise in states where plaintiffs have filed complaints, whatever the ultimate outcome of the litigation (Hickrod et al. 1992). That conclusion, however, merely substantiates the basic point that judicial interventions can be assessed meaningfully only from within the context of the educational, political, and economic factors at play in a particular state context. The filing of a court case may galvanize attention and push education finance reform to the top of the political agenda. How the matter is handled once it gets that attention, which legal doctrines and which court-ordered remedies do or do not have a positive impact—and why—are the key questions that well-done state-level case studies, but not broad national-level impact studies, can usefully address.<sup>9</sup>

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<sup>8</sup> For example, see Rebell and Block's 1982 study, the methodology of which is based on 65 "caselets" and four major case studies that review aspects of judicial intervention in educational policy litigations.

<sup>9</sup> The extensive debate in the academic literature and the courts for the past two decades on whether "money matters" also illustrates how meta-analyses on a national level can distort major policy discussions in a counterproductive way. More than a decade ago Eric A. Hanushek reported that an overview analysis of approximately 187 distinct studies of the impact of increased funding on student achievement raises serious questions about whether increases in funding can be correlated with positive outcomes in terms of student achievement (Hanushek 1989, 1991). Hanushek's methodologies and conclusion have been strongly challenged (Hedges, Lane, and Greenwald 1994; Card and Krueger 1996).

In the two dozen or so fiscal equity litigations that have taken place since this question arose, huge "battles of the experts" have taken place, and enormous expenditures of time and resources have been devoted to trying to establish whether, in fact, increased funding can lead to improved student achievement. After the dust settled on the academic debate, most of the judges who have focused on this issue in recent cases have reached a common-sense conclusion that money well spent will make a difference, but money merely thrown at the problem may be wasted. (See, for example: *Hoke County Board of Education v. North Carolina*, 95 CVS 1158 (Super. Ct., Wake Co.), "Only a fool would find that money does not matter in education"; *Roosevelt Elementary School District 66 v. Bishop*, 877 P.2d 806, 822 (Ariz. 1994), C.J. Feldman specially concurring, "Logic and experience also tell us that children have a better opportunity to learn biology or chemistry... if provided with laboratory equipment for experiments and demonstrations.") In short then, the issue is not whether money matters, but how to apply appropriate accountability measures to ensure that money that is allocated for education reform is spent in an effective manner.

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