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The Fate of New Funding: Evidence from Massachusetts' Education Finance Reforms

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In 1993, the state of Massachusetts implemented comprehensive reforms that included increased state aid to local school districts. This study presents empirical evidence on whether these reforms actually increased educational spending and, if so, in what functional areas. These evaluations are based on relatively detailed annual financial data from the unified school districts in Massachusetts and two neighboring states over the 1990–1996 period. The results indicate that the reforms did increase state aid to and spending by those districts that spent the least prior to the reforms. Furthermore, most of the new spending in these low-spending districts was directed toward student instruction and capital investments.

Keywords: *equity, expenditures, finance*

THE CONTENTIOUS and ongoing debates over proposals for reforming public education often turn on issues related to establishing school accountability and enhancing fiscal capacity. A central concern regarding fiscal capacity has been that the ability of the neediest communities to fund their own improvements is limited because education finance has historically been based on local property taxes. However, over the last 30 years, advocates have sought to increase the amount of educational resources available to poorer communities through litigation. More specifically, the constitutionality of locally based systems of education finance has, to date, been challenged in 40 states.[1] In 17 of these states, the supreme courts ruled in favor of the plaintiffs, invalidating the property-based systems of education finance. Recent research has indicated that the earliest state court decisions had a dramatic impact on the structure of education finance (e.g., Murray et al., 1998; Evans et al., 1997).[2] In particular, court rulings that declared state systems of education finance unconstitutional appear to have achieved their proximate goal by encouraging states to increase their per-student aid to poorer school districts.[3]

However, there are at least two accountability-related reasons that the reform-driven increases in state aid may not have achieved the ultimate goal of increased school quality and educational opportunity. One is that most of the new state aid may simply be returned to local taxpayers by generating a corresponding reduction in locally raised revenues. More specifically, the residents of jurisdictions that receive increased state aid should view this windfall like any increase in income and choose only relatively small increases in total educational spending.[4] The second concern with the efficacy of these reforms is that, even if new state aid did increase educational spending, overall school quality may not improve. In particular,

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it is a widely held, though controversial, view that school districts typically allocate new resources towards unproductive uses (e.g., Burtless, 1996; Hanushek, 2003). This empirical study presents novel evidence on both issues by evaluating how Massachusetts' education finance reforms influenced district-level revenues by source and expenditures by function. In 1993, Governor Weld signed into law the Massachusetts Education Reform Act (MERA), an omnibus education reform bill that dramatically reshaped state support for local public schools.[5] In particular, this legislation established aggressive, new "foundation" targets for per-pupil spending, regulated local tax effort and committed a substantial amount of new state aid to help some districts meet the foundation targets.[6] Interestingly, the state exercised relatively little direct oversight over how school districts spent these new resources (e.g., Hart, 1997). However, this legislation did include new efforts to establish accountability through "bottom-up" reforms such as increased parental involvement and increased authority for principals.

The reduced-form evaluations presented here are based on annual district-level panel data from the unified school districts in Massachusetts, Maine and Connecticut from the 1990, 1992 and 1994–1996 fiscal years. These data were drawn from annual Surveys of Local Government Finances (the "F-33" surveys), which are collected by the National Center for Education Statistics (NCES) and the US Census Bureau (Dee, Evans, & Murray, 1999). These recent F-33 surveys identify the source of school district revenues (Federal, state, and local) but also include data on how these resources were spent across six functional expenditure categories. As in other recent studies, the evaluation results presented here exploit the panel nature of the available data by relying on fixed effect specifications that include unambiguous controls for the unobserved district-specific and year-specific determinants of the revenue and expenditure outcomes. The results of these fixed effect models indicate that Massachusetts' finance reforms did lead to a significant increase in state aid to local school districts. In high-spending districts, the increased state aid had a relatively modest effect on total per-pupil revenues because of offsetting reductions in locally raised revenues. However, we observe a particularly strong "flypaper effect" among districts that had been low spenders prior to the reforms. More specifically, in the low-spending districts, these reforms increased total per-pupil revenues by roughly 7% ($556 per pupil in 1996 dollars). Where were these new resources directed? Our results indicate that most of these resources were allocated towards activities directly related to student instruction, to support services and to capital expenditures.[7] These results do not provide direct evidence on whether these reforms actually improved school quality. However, they do indicate that the intended beneficiaries of these reforms (i.e., the low-spending districts) directed a substantial proportion of these new resources to areas typically viewed as priorities. Furthermore, other recent evidence of test score gains (Card & Payne, 2002; Guryan 2001) and capitalized increases in local property values and residential rents (Dee, 2000) provides complementary evidence on the possible relationship between reform-driven increases in educational spending and school quality.[8]

Education Finance Reforms

In a landmark 1971 ruling, the California Supreme Court ruled that the state's property-based system of education finance was unconstitutional. Since then, similar legal challenges have been mounted in most states. To date, the supreme courts in 17 states have deemed their state's system of education finance unconstitutional. Much of the early state-level litigation concentrated on arguments that related the equal protection clauses found in state constitutions to issues of educational equity. However, since 1989, litigants in several states (including Massachusetts) have successfully argued cases based instead on issues of educational "adequacy" (Verstegen, 1994; Fossey, 1994; Long, 1999). Several recent empirical studies have evaluated the efficacy of education finance reforms in influencing the distribution of education resources (e.g., Murray et al., 1998; Evans et al., 1997; Card & Payne, 2002). These studies have been based on pooled cross-sections of nationally representative district-level revenue and expenditure data from the 1970s up to 1992. The evidence from this research indicates that court-ordered education finance reforms significantly increased per-pupil state aid and spending in poorer school districts.[9] In other words, these results suggest that the court-ordered reforms "leveled-up" the educational resources available to poorer communities and generated a substan-

tial "flypaper effect" (i.e., increased educational spending).[10] This study provides direct evidence on whether Massachusetts' more recent experience with education finance reform generated similar increases in state aid and educational spending. Furthermore, by exploiting relatively detailed data on expenditure by function, this study also evaluates how new resources were actually spent. This latter contribution is a potentially useful one since reforms cannot be considered entirely successful if they led to increased spending in possibly unproductive areas.[11] The limited, prior evidence available on this issue (e.g., Goertz & Natriello, 1999) suggests that districts tend to allocate new resources across all functional categories in proportions largely consistent with prior spending patterns.

In the 15 years prior to 1993, Massachusetts experienced several legislative and judicial attempts to improve the equity of available educational resources. However, these early efforts were widely viewed as unsuccessful. According to an analysis of data from the 1991–92 school year (GAO, 1997), Massachusetts ranked near the bottom among states in terms of equalization effort and the wealth neutrality of school spending. In part, the failure of reforms prior to 1993 may have reflected the close links between the generosity of state aid and the health of the state economy (Anthony & Rossman, 1993). However, another possibly important complication to Massachusetts' municipal finances over this period was the voter approval of Proposition 2½ in 1981. This proposal lowered local property taxes and placed restrictions on their future growth. The effects of these restrictions were substantially curtailed by subsequent legislative amendments (e.g., an allowance for taxes on new construction), increases in state aid and voter overrides. However, Cutler et al. (1997) conclude that Proposition 2½ did constrain local governments, particularly during economic downturns when new construction slowed and the generosity of state aid fell. Since the period prior to the Massachusetts' 1993 reforms coincided with a recession, the existence of such constraints on locally raised revenues creates a possible source of bias for the evaluations presented here. In particular, by possibly constraining locally raised revenues in Massachusetts' school districts during the pre-reform period, Proposition 2½ could impart a positive bias to the estimated effects of the 1993 reforms on revenues and expenditures. However, the empirical relevance of this source of bias appears to be negligible.[12]

Like the rulings in other states, the 1993 *McDuffy* decision focused on the state constitutional language on education and its interpretation of this language. In particular, like the 1989 ruling in Kentucky, the *McDuffy* decision emphasized the role of the state in providing for an "adequate" education. Shortly after the *McDuffy* decision, Governor Weld signed into law the Massachusetts Education Reform Act (MERA) of 1993.[13] MERA committed substantial new state resources to public education and revamped the funding formula to direct more state resources to schools (Anthony & Rossman, 1994).[14] Interestingly, the Act also included a variety of other educational reforms intended to ensure that the new state aid did improve school quality. These included "programmatic" reforms related to curricular standards, time spent in school and early childhood programs (Anthony & Rossman, 1994). But they also included several "organizational" reforms intended to decentralize decision-making power within districts (Anthony & Rossman, 1994). For example, the Act attempted to promote site-based management though the creation of school councils that include parents and community members as well as teachers and the principal. The Act also gave principals increased authority in disciplining students and firing teachers and authorized explicit interventions into schools deemed "chronically underperforming" based on student performance.

In the years prior to this reform, the finance system in Massachusetts was a "loosely configured" foundation program that based state aid on the fiscal capacity and "environmental" traits of local communities (Anthony & Rossman, 1993, 1994). The new state funding system attempted to establish educational adequacy by insuring that all districts would eventually provide a higher foundation level of spending. In the first year of the reform, this foundation level averaged $5,577 per pupil across districts (Ardon & Costrell, 2001).[15] The structure of the new funding formula dramatically expanded the role of state support while promoting the traditionally local character of education finance. In particular, the determination of state generosity to a particular district depended not only on how its spending compared to its foundation target but also on its

effort in raising additional revenues locally. The calculation of each district's state support began with an amount of "base aid" at least equal to what they received during the 1993 fiscal year (a "hold harmless" clause of the reform). However, the receipt of additional aid depended in part on a district's spending relative to its foundation target and whether it had met the state's required standard of effort. Anthony and Rossman (1994) discuss the fiscal implications for districts defined along these two dimensions (i.e., above or below the foundation target and above or below their standard of local effort). Districts with spending below their foundation target were required to close an explicit percentage of this spending gap in each year. The low-spending districts with high tax effort would receive additional "foundation" aid to close the spending gap as well as "equity" aid to reward their tax effort. Districts with low tax effort that failed to meet their foundation target were required to close their spending gap with increased local revenues, and, possibly some state support. The higher-spending districts that received little or no foundation aid were eligible for minimum aid initially equal to at least $25 per pupil. The high-spending districts with high tax effort were also rewarded with "equity" aid that could be redirected to non-educational functions. However, the high-spending districts with low tax effort were required to increase their local contribution to meet the mandated standard of effort.

The design of this new formula suggests that the effects of the aid should have been highly heterogeneous. In particular, the combination of an aggressive foundation target, large increases in state aid targeted to low-spending districts and some constraints on using the new aid for tax relief should have dramatically reduced the inequity of educational spending in Massachusetts. However, other detailed features of how the funding formula was implemented make the distributional implications of these reforms more uncertain. For example, the mandated increases in local contributions by low-effort districts may have been attenuated by exemptions and ad-hoc caps placed on the "municipal revenue growth factors" used to calculate those requirements (Moscovitch, 1996; Ardon & Costrell, 2001). Furthermore, because the state relied on the pre-reform pattern of aid to determine each district's base aid, some of the prior inequities in state support were left unchanged. The "hold harmless" feature of the new funding formula, which in most cases guaranteed each district at least the amount of aid it received in the prior year, carried these historical patterns forward (Wulfson 1998). There may also have been additional distortions to the base aid since it has no direct connection to changing student enrollments (Wulfson 1998).

Anthony and Rossman (1993) note that these reforms (e.g., the treatment of base aid, the creation of minimum aid) were fairly generous to affluent school districts and argue that this reflected "political sagacity" on the part of legislators as well as a desire to maintain high spending in these districts. In other words, it created broad political support for a foundation program that held the promise of dramatically increasing the level of educational spending in poorer communities. In our evaluations, we provide direct empirical evidence on these possibly heterogeneous fiscal responses to the reforms across such school districts. We do so explicitly within the context of the reform's intended goal of increasing spending in the lowest-spending districts. Specifically, we examine the reform's unique effects among districts that had been low, medium and high spenders *prior* to the 1993 ruling. However, it should be noted that this simple taxonomy of districts obscures how the effects of the new funding formula depended explicitly on both spending and tax effort.

The effects of these reforms depend not only on their varied incentives and constraints but also on how school districts make budgeting decisions. The most basic model for decision-making by local governments assumes these decisions are made in accord with the wishes of the median voter. To the extent that this is true, an increase in state aid should be viewed like any increase in income and generate only modest increases in educational spending.[16] This model implies that most new state aid would be directed towards local tax relief. The Massachusetts school districts with high-tax effort were clearly encouraged to use the new equity aid in this manner. In contrast, the school districts with low-tax effort were ostensibly constrained from using the new aid to provide tax relief. The empirical literature generally finds that state educational grants lead to relatively little tax relief even in the absence of such regulations on tax effort (Hines & Thaler, 1995; Fisher & Papke, 2000). The existence of

these surprisingly large "flypaper effects" suggests several possible limitations of the basic median-voter model. For example, one widely held interpretation of this phenomenon is that self-serving local bureaucrats exploit their informational advantage over local voters by spending new aid instead of providing tax relief. Another possibility is that "flypaper effects" reflect other behavioral tendencies among local voters like a failure to view state grants as fungible or a relative aversion to income losses (Hines & Thaler, 1995). Interestingly, these explanations of "flypaper effects" suggest that Massachusetts' programmatic and organizational reforms, which coincided with the changes in state funding, could have also influenced how these resources were allocated. For example, increases in parental involvement and the authority of principals may have had fiscal implications by devolving decision-making and increasing the information available to parents and voters. As a practical matter, the reduced-form evaluations presented here do not distinguish the direct fiscal effects of changes in the funding formula from the influence of these other comprehensive reforms. Instead, we argue that Massachusetts' experience with finance reforms provides a potentially useful contrast with those in other states that recently bundled new aid with more centralized, comprehensive reforms (e.g., Tennessee).

Data and Specifications

This study employs district level data on revenue by source and expenditure by function to evaluate Massachusetts' experience with education finance reform. More specifically, the data and empirical models introduced here are used to ask the following questions. Did education finance reform lead to more state support, particularly for low-spending districts? Were the spending effects of new state aid, to some extent, undone by reductions in locally raised revenues? If not, in what areas did local school districts choose to spend the new state aid? This section presents the data and research methods used to evaluate these questions.

Survey of Local Government Finances for School Systems

The "F-33" Survey of Local Government Finances is sponsored by the U.S. Department of Education's National Center for Education Statistics (NCES) in conjunction with the Government Division of the U.S. Census Bureau. These surveys are sent annually to at least a sample of school districts and "to all districts in years ending in a zero, two or seven" (Dee, Evans, & Murray, 1999). Fortunately, for the fiscal years just before and after Massachusetts' 1993 reforms, the F-33 surveys provide financial data on all school districts in Massachusetts as well as those in Connecticut and Maine. Connecticut and Maine were the only two neighboring states that did not also experience major education finance reforms over this period.[17] These financial data include information on the sources of school district revenues (Federal, state, and local) as well as data on the functional areas to which school districts allocated these resources. Those categories are described in more detail below. All of the financial data were converted to 1996 dollars using the consumer price index (CPI).

The panel data set analyzed here was constructed by pooling district-level F-33 survey data from the 1990, 1992 and 1994–1996 fiscal years.[18] As in prior studies (Evans et al., 1997; Dee 2000), this data set was limited to unified school districts, which are similar in scale and organizational goals. Furthermore, special, non-operating and administrative districts were also eliminated. Following the procedures recommended by O'Leary and Moskowitz (1997), remaining districts with "VOC," "TECH," "SPEC," "AGRIC" or "ADMIN"appearing in their names were individually checked and edited. In the remaining districts, the share of students with individual education plans (i.e., special education) was also examined to identify unusual districts (O'Leary & Moskowitz, 1997). The final data set consists of 1,983 observations over the three fiscal years. For the 1996 fiscal year, 112 of the district-level observations are from Connecticut, 174 from Massachusetts and 110 from Maine.[19] These counts of regular, unified school districts are almost exactly equal to other published numbers (Hoffman, 1998, Table 4).[20]

As noted above, the F-33 survey provides useful information on each district's annual revenues by source and expenditures by function. The revenue data break down each district's total per-pupil revenues into three mutually exclusive and exhaustive categories: Federal, state, and local. We use these data to identify whether the finance reforms led to increased state support for school

TABLE 1
Descriptive Statistics, Unified School Districts in Maine, Massachusetts, and Connecticut, 1990, 1992, 1994, 1995, and 1996 Fiscal Years

Variable	*M*	*SD*	Minimum	Maximum
Total revenues per pupil	7,631	1597	4,184	19,917
Federal revenues per pupil	272	263	2	5,091
State revenues per pupil	2,635	1470	91	13,671
Local revenues per pupil	4,724	2096	322	12,882
Total general expenditures per pupil	7,480	1687	3,981	18,800
Instructional expenditures per pupil	4,306	905	2,042	8,231
Support service expenditures per pupil	2,177	602	1,059	6,390
Non-instructional expenditures per pupil	246	129	0	1,392
Non-EL/Sec expenditures per pupil	66	69	0	646
Capital expenditures per pupil	343	858	0	10,766
Other LEA expenditures per pupil	342	322	0	3,712
Number of observations	1,983			

Note. All expenditure and revenue data are unweighted and in real 1996 dollars.
EL/Sec = Elementary/Secondary.

districts and any changes in local or Federal support.[21] It should be noted that these revenue categories are broader than those directly influenced by some of the reforms. In particular, the F-33 definition of state revenue includes all revenues from the state while the reforms largely focused on formula-driven state support for operating expenditures. However, the F-33's inclusive measure of state revenues may actually be preferable for the reduced-form evaluations presented here. More specifically, it allows us to identify the overall effects of the reforms even if the state contemporaneously adjusted other sources of aid.

As the descriptive statistics in Table 1 indicate, most of the school district revenues in these three states over the studied period were locally generated (roughly 62% of the total revenue). State contributions were approximately 35% of the total revenues. The total expenditures per pupil for all three states were $7,480 on average (see Table 1). The NCES decomposes these expenditures into six major categories (Fowler, 1990). Instructional expenditure per pupil, amounting to $4,306 represents the largest of the six categories. These expenditures apply to "activities dealing directly with the interaction between teachers and students" (Fowler, 1990). This is a broad measure that does not allow us to examine exactly how instructional resources are spent. In particular, this variable does not allow us to distinguish changes in teacher salaries from changes in class sizes. However, this variable is still quite useful since it allows us to examine whether reform-driven resources ended up being applied to the classroom.

Support services constitute the second largest educational expenditure category. Such services are defined as "administrative, technical (such as guidance and health) and logistical support to facilitate and enhance instruction" (Fowler, 1990). More specifically, this category encompasses a diverse list of support programs such as social work, attendance accounting, psychological and health services as well as school and general administration and student transportation. On average across the three states $2,177 were expended towards per-pupil support services. Non-instructional expenditures per pupil averaging $246 per district across the three states include food services, enterprise operations (e.g., book stores) and other related educational expenses. The smallest of the six expenditure categories, non-elementary/secondary expenditures (averaging $66 per pupil), refers to activities such as community services and adult education. Capital expenditures (on average, $343 per pupil) are applied towards construction, instructional and other equipment and land expenses.[22] The final expenditure category averages $342 per pupil and includes other district expenditures such as payments directed to state and local governments, payments to other school systems and interest on debt. These six expenditure categories undoubtedly obscure many of the detailed functions to

TABLE 2
Difference-in-Differences Estimate of Effect of Court-Ordered Reforms on Real State Revenues Per Pupil

States	Mean State Revenues Per Pupil		Difference
	After 1993	Before 1993	
Massachusetts	$2,270	$1,795	$475
Connecticut and Maine	$3,004	$3,167	−$163
Difference in Differences			638‡ (65)
R^2			0.1242

These results are based on annual observations of unified school districts from Connecticut, Maine, and Massachusetts in the 1990, 1992 and 1994–1996 fiscal years. The absolute value of the heteroskedastic-consistent standard error is reported in parentheses.
‡Statistically significant at the 1% level

which districts routinely apply resources. However, this broad taxonomy still provides a useful way to gauge how reform-induced resources were allocated, particularly with regard to direct student instruction.

Difference-in-Differences

Our approach to evaluating the effects of these reforms is to estimate a variety of reduced-form models where the key dependent variables are the real, per-pupil revenues by source and expenditures by function.[23] The critical features of this research design involve exploiting the panel nature of the repeated district-level observations to eliminate the biases that could be created by all the unobserved features of a particular district or a particular fiscal year.[24] We can illustrate the basic logic of this identification strategy (as well as its critical, maintained assumptions) by appealing to a simple "difference-in-differences" estimator (Meyer, 1995). This sparse research design simply involves comparing the mean outcomes in Massachusetts, the "treatment" state, before and after the 1993 reforms with the contemporaneous changes in Maine and Connecticut, the "control" states. This interpretation can be demonstrated more formally by considering the following basic regression model:

$$Y_{ist} = \alpha + \beta\mu_s + \delta\nu_t + \gamma EFR_{st} + \varepsilon_{ist},$$

where α is an intercept, μ_s is a Massachusetts dummy variable, ν_t is a dummy variable for districts observed after the 1993 ruling and ε_{ist} is a mean-zero random error for district i in state s in year t.[25] The regressor of interest is EFR_{st}, a dummy variable equal to one only for Massachusetts' districts observed after 1993 (i.e., the treatment state in the post-treatment period). It is straightforward to show that the least squares estimate of the coefficient of interest, γ, is equal to the difference in two differences:

$$\begin{aligned}\gamma_{DD} = &[E(Y_{ist}|\mu_s = 1, \nu_t = 1) \\ &- E(Y_{ist}|\mu_s = 1, \nu_t = 0)] \\ &- [E(Y_{ist}|\mu_s = 0, \nu_t = 1) \\ &- E(Y_{ist}|\mu_s = 0, \nu_t = 0)].\end{aligned}$$

The first bracketed difference is the change in the average value of Y_{ist} in Massachusetts after the 1993 ruling (i.e., $\delta + \gamma$). For example, in Massachusetts, the average state revenue per pupil increased by $475 after the reforms (Table 2). However, this first difference cannot necessarily be understood as an unbiased treatment effect (i.e., γ) since there may have been other relevant time-series variation over this period as well (i.e., δ). Most obviously, the regional economic recovery that occurred over this period (and the possible increase in state revenues from income and sales taxes) could explain Massachusetts' increase in state generosity to public schools. The difference-in-differences estimate controls for that possibility by subtracting the contemporaneous change in the control states that were unaffected by the 1993 reforms (i.e., δ). Over this period, real state revenues in Maine and Connecticut's school districts actually experienced a modest decline of $163 per pupil (Table 2). This simple analysis suggests, therefore, that the 1993 ruling generated a statistically significant increase in state aid to Massachusetts' school districts of $638 per pupil (i.e., [475] − [−163]).[26]

A key virtue of this research design is that the first differencing within the treatment and control states eliminates the possibly confounding influence of the many, unobserved (and time-invariant) determinants unique to each cross-sectional group

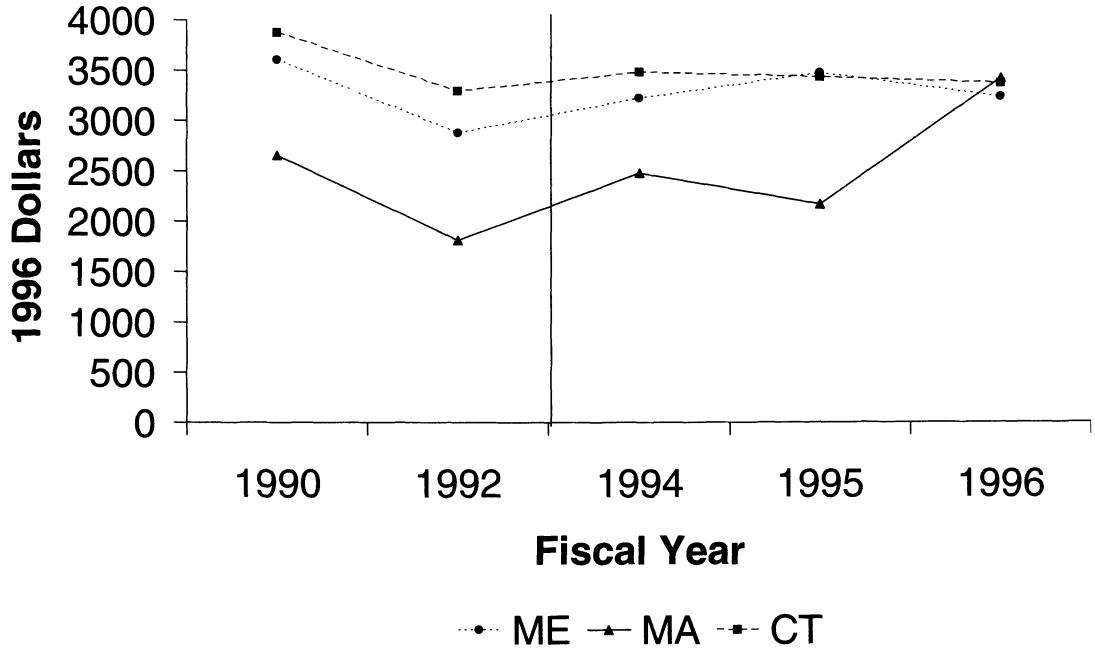

FIGURE 1. *State revenues per pupil.*

(i.e., μ_s). And, then, computing the differences in these differences eliminates the biases that may be related to the shared but unobserved, time-series determinants (i.e., ν_t). However, this approach also relies on the important maintained assumption that the neighboring states provide valid controls for the contemporaneous changes in Massachusetts that were unrelated to the 1993 decision. This view is supported by the geographic proximity of these states as well as by our anecdotal knowledge of what was occurring in these states with regard to education finance over this period. For example, the recession-induced decrease in state support for local districts in Massachusetts during the early 1990s was clearly understood as a shared, region-wide phenomenon (e.g., Frahm, 1992; Pipho, 1992). Interestingly, the data in Figure 1 also support this view: average state revenues per pupil in Connecticut and Maine track those in Massachusetts well during the two observed years prior to the 1993 ruling. However, the robustness of this study's results will be evaluated more directly through the use of the more detailed and flexible regression models introduced in the next section.

Fixed-Effects Specifications

The preferred regression models evaluated in this study exploit the panel nature of the available data more fully by including the fixed effects for each of the fiscal years and for each of the nearly 400 school districts. More specifically, the basic regression model is:

$$Y_{ist} = \alpha + \beta\mu_i + \delta\nu_t + \gamma EFR_{st} + \lambda X_{ist} + \varepsilon_{ist}$$

where μ_i and ν_t now represent district and year fixed effects and X_{ist} represents additional controls for other determinants varying within districts over time. While this specification is less restrictive than the sparse differences-in-differences model, the fundamental logic behind this approach is quite similar. The fixed-effects model effectively compares the changes within the Massachusetts districts, which experienced the reforms, to the contemporaneous changes in Connecticut's and Maine's districts, which did not.

However, the introduction of district fixed effects eliminates a substantial amount of the unexplained variation in the revenue and expenditure outcomes (see the R^2 in Table 3). Furthermore, this flexible multiple regression model allows us to examine some of the key maintained assumptions of this research design. For example, we vary somewhat the set of regression controls to examine whether our results are sensitive to the presence of unobserved variables varying within each state over time. There appears to be little reason to expect biases of this sort since it is unclear what un-

TABLE 3
The Estimated Effects of Court-Ordered Finance Reforms on Per-Pupil Revenues by Source

Dependent Variable	Estimate	R^2	Estimate	R^2	Estimate	R^2
Total revenue per pupil	292‡	0.3990	302‡	0.8351	507‡	0.8415
	(89)		(99)		(113)	
State revenue per pupil	644‡	0.1573	638‡	0.8626	776‡	0.8697
	(65)		(71)		(77)	
Federal revenue per pupil	−2	0.0615	−3	0.8244	25†	0.8293
	(13)		(15)		(10)	
Local revenue per pupil	−350‡	0.2241	−334‡	0.9373	−295‡	0.9401
	(67)		(72)		(87)	
State fixed effects?	Yes		No		No	
District fixed effects?	No		Yes		Yes	
Urbanicity-specific year fixed effects?	No		No		Yes	

Note. These results are based on annual observations of unified school districts from Connecticut, Maine, and Massachusetts in the 1990, 1992 and 1994–1996 fiscal years. All models include year fixed effects. The absolute values of heteroskedastic-consistent standard errors are reported in parentheses.
†Statistically significant at the 5% level.
‡Statistically significant at the 1% level.

observed and state-specific determinants also had a persistent covariance with the timing of Massachusetts' reforms. Furthermore, the use of a relatively "short" panel (i.e., observations from a few closely bunched years) should reduce the pernicious influence of unobserved, long-term trends. However, we also address this issue more directly by considering the results of specifications that vary sharply in their use of such controls. The first specification includes state instead of district fixed effects. The second specification replaces the state fixed effects with district fixed effects. Other possible sources of omitted variable bias are the unobserved demographic and socioeconomic trends that influence revenue and expenditure outcomes within districts. These confounding and unobserved changes may be specific to districts in rural, suburban and urban areas. For example, Massachusetts' districts are, on average, more suburban than those in the control states, particularly Maine. If there were unobserved revenue or expenditure patterns unique to suburban districts over this period, this research approach could falsely attribute those effects to the finance reforms. To examine this possibility, the final specification introduces fixed effects that control for such unobserved time-series variation in an unrestrictive manner: year fixed effects interacted with dummy variables for each district's urbanicity status.

In an attempt to reduce the inequity of school spending within the state, the new funding formula directed particularly large amounts of aid to districts that had low spending and high tax effort in the pre-reform period. However, as noted earlier, other aspects of these reforms suggest that the resulting improvements in equity may have been attenuated. To assess the overall effects of the reforms on the within-distributions of aid and spending, some of the results presented are based on sub-samples of the districts defined by their pre-reform spending. More specifically, we calculated the average, real expenditures per pupil for each district over the 1990 and 1992 fiscal years. We then divided the districts into thirds by their position in their state-specific distributions of average, pre-reform spending.[27] The intent of the 1993 ruling and reforms was to encourage increased spending among the low-spending districts that were in the bottom third of the state's pre-reform distribution. The allocation of new resources among these districts is also of particular interest since one goal of the reforms was the provision of an "adequate" education.

Results

This section presents and discusses the results of two-way fixed effects model that evaluate the effect of Massachusetts' education finance reforms on real per-pupil revenues by source and expenditures by function.

Full Sample

In Table 3, we present the key results from separate reduced-form models where the dependent

variables are real, per-pupil revenues by source. The results in the second row of this table present point estimates that are quite similar to the one based on the "difference in differences" analysis though the R^2 is noticeably higher. For example, the econometric model that includes state fixed effects suggests that the reforms increased real state revenues per pupil by a statistically significant $644. The next two specifications introduce district fixed effects and urbanicity-specific year fixed effects. Despite the dramatic increase in the number of regression controls, the estimated increase in state revenues due to the reforms proves quite robust. The most inclusive specification suggests that the reforms increased state revenues by $776 per pupil. The robustness of the estimates to these dramatic changes in controls provides suggestive, ad-hoc evidence that these evaluation results do not reflect the pernicious influence of unobserved determinants varying within states over time. The estimated effect from the most inclusive specification in Table 3 is somewhat larger than that implied by the simple differences-in-differences comparison. However, this difference is small relative to the sampling variation (i.e., within two standard errors). We also examined the robustness of these results by assessing a simple counterfactual. If the apparent effects of the Massachusetts' 1993 reforms on state aid actually reflected an unrelated and state-specific trend, we might expect to find a Massachusetts-specific effect for the 1992 fiscal year. However, we constructed a dummy variable equal to one for the Massachusetts districts in the 1992 fiscal year and found that it had a very small and statistically insignificant effect. This result is consistent with the maintained hypothesis that the 1993 reforms were the source of a sharp and exogenous change in state support.

Did school districts spend this reform-driven state aid or simply return it to local taxpayers? These evaluations suggest that the spending effects of the reforms were attenuated by corresponding reductions in locally raised revenues. More specifically, the results in Table 3 indicate that the reforms were accompanied by a reduction in locally raised revenues per pupil of $295 to $350. These reductions constitute 38%–54% percent of the new state aid. The overall effects of this tax relief can also be seen in the model for total revenues per pupil. These models suggest that the reforms increased total revenues by somewhat attenuated amounts (i.e., only $292 to $507 per pupil). One useful way to frame these effect sizes is to consider how they would compare to the expected effects associated with similar increases in community income. Over this period, the real, per capita income in Massachusetts was approximately $18,000. A $776 increase in per-pupil state aid is, therefore, equivalent to roughly 4.3% of per-capita income. Assuming an income elasticity of 0.5 (Fisher & Papke, 2000), this new state aid might be expected to increase per-pupil revenues by about 2.15%. In Massachusetts over this period, total per-pupil revenues averaged approximately $7,300 so the expected increase would amount to roughly $157 per pupil. The estimated total revenue effect from the final specification in Table 3 (i.e., $507) is noticeably larger, suggesting the existence of a "flypaper effect" (Hines & Thaler, 1995). In other words, the amount of tax relief created by the new state aid actually appears to be less than what would be predicted by a median-voter model. Because only the equity aid provided to some districts was intended for tax relief, these results suggest that state regulation of local contributions may have been weakened by ad-hoc adjustments and exemptions to mandated local contributions (Moscovitch, 1986; Ardon & Costrell, 2001). Some communities may also have had the latitude to provide real, per-pupil tax relief by failing to increase nominal tax support in the face of increasing enrollments or price inflation.

Heterogeneity by Pre-Reform Spending

The prior results suggest that the reforms led to significant increases in state aid but that the effects of this aid on local spending were noticeably attenuated by corresponding reductions in locally raised revenues. However, results based on the full sample may distort the unique effects of these reforms for school districts that had been high or low spenders in the years prior to reform. The court ruling that helped motivate these reforms specifically intended to direct new state aid to the lowest-spending districts and to increase their spending. However, as discussed earlier, the reforms also appeared to direct new aid to more affluent districts and attempted to prevent some districts with low tax effort from using all of the new aid for tax relief. This section presents empirical evidence on these possibly heterogeneous responses by replicating the evaluations

TABLE 4
The Estimated Effects of Court-Ordered Finance Reforms on Per-Pupil Revenues by Source and Prior Expenditures

Dependent Variable	Estimated Effect		
	Bottom Third	Middle Third	Top Third
Total revenue per pupil	556‡	556‡	314
	(169)	(128)	(253)
State revenue per pupil	584‡	854‡	920‡
	(103)	(104)	(173)
Federal revenue per pupil	30†	25	22
	(15)	(22)	(14)
Local revenue per pupil	−58	−323‡	−627‡
	(137)	(117)	(173)
Number of observations	695	636	641

Note. These results are based on annual observations of unified school districts from Connecticut, Maine, and Massachusetts in the 1990, 1992 and 1994–1996 fiscal years. These models are estimated separately for districts in the bottom, middle and top thirds of their state-specific, pre-reform distribution of per-pupil expenditures. All models include district fixed effects and urbanicity-specific year fixed effects. The absolute values of heteroskedastic-consistent standard errors are reported in parentheses.
†Statistically significant at the 5% level.
‡Statistically significant at the 1% level.

in Table 3 among three particular and mutually exhaustive subsets of the entire data set. More specifically, the school districts were divided into three distinct groups by their position within their pre-reform, state-specific distribution of total, real expenditures per pupil. The districts in the bottom third of these distributions had the lowest per-pupil expenditures within their states and were, by and large, the intended beneficiaries of Massachusetts' finance reforms.

The evaluation results presented in Table 4 identify the distinct effects of Massachusetts' reforms on total revenue per-pupil as well as per-pupil revenues by source among these three groups. These results are based on a preferred specification that includes district fixed effects and urbanicity-specific year fixed effects.[28] The results indicate that Massachusetts' reforms did have highly heterogeneous effects. Somewhat paradoxically, the results in Table 4 suggest that the higher-spending districts actually had the larger reform-driven increases in state aid. However, the reform-driven increases in *total* per-pupil revenues were larger in the lower-spending districts because higher-spending districts had larger reductions in locally raised revenues. While these results suggest that the new state aid was not distributed equitably, it should be noted that these data only cover the first three years of the reforms and there are several reasons to believe that state support for low-spending districts grew over time. Most obviously, low-spending districts benefited particularly from foundation aid, which grew with each passing year as their spending gap with respect to the foundation level was closed.[29]

To examine these timing issues more carefully, we estimated similarly specified models for real, per-pupil state revenues where the key independent variable (i.e., being in Massachusetts during the post-reform period) varies by fiscal year. The results, which are presented in Table 5, indicate that the average, reform-driven increase in state aid did grow over each of these three years. More specifically, regression estimates based on the full sample indicate that real, per-pupil state aid grew by $520 to $590 during the 1994 and 1995 fiscal years and by $1,217 by the 1996 fiscal year. The remaining results in Table 5 also indicate that, during the 1994 and 1995 fiscal years, relatively little of the new state aid was directed to the lowest-spending districts. However, by the 1996 fiscal year, the lowest spending districts were receiving an estimated $1,345 per-pupil in new state aid. This increase is more than two standard errors larger than the estimated increase received by the highest spending districts in that year (i.e., $1,056). These results suggest that the distribution of new state aid grew more equitable over time, an inference also supported by the fact that, within a few more years, nearly all districts had reached

TABLE 5
The Estimated Effects of Court-Ordered Finance Reforms on Per-Pupil State Revenues by Fiscal Year and Prior Expenditures

	Estimated Effect			
Independent Variable	Full Sample	Bottom Third	Middle Third	Top Third
MA × 1994 fiscal year	520‡	358‡	620‡	595‡
	(77)	(90)	(98)	(185)
MA × 1995 fiscal year	590‡	49	746‡	1109‡
	(118)	(167)	(152)	(252)
MA × 1996 fiscal year	1217‡	1345‡	1195‡	1056‡
	(82)	(127)	(130)	(160)
Number of Observations	1983	695	636	641

Note. These results are based on annual observations of unified school districts from Connecticut, Maine, and Massachusetts in the 1990, 1992 and 1994–1996 fiscal years. These models are also estimated separately for districts in the bottom, middle and top thirds of their state-specific, pre-reform distribution of per-pupil expenditures. All models include district fixed effects and urbanicity-specific year fixed effects. The absolute values of heteroskedastic-consistent standard errors are reported in parentheses. MA = Massachusetts.
‡Statistically significant at the 1% level.

their foundation spending level (Franklin 1999). However, the long-term impact of these reforms should nonetheless be considered an open empirical question.

These results suggest that Massachusetts' finance reforms had the desired effect of increasing the level of spending in the state's lowest-spending districts. However, whether these reforms are viewed as successful depends in large part on how these resources were spent. A priori, there is ample reason to be concerned about how school districts allocate new resources. Most notably, there is an extensive, though controversial, literature on whether school districts use resources ineffectively (e.g., Burtless, 1996; Hanushek, 2003). But this possibility is also suggested clearly by the very existence of such a strong "flypaper effect" in the lowest-spending districts. More specifically, the fact that little or no state aid was returned to taxpayers in these districts suggests that the school authorities may have been out of step with the wishes of the median voters in these communities (Hines & Thaler 1995). The possible existence of such an agency problem implies that school districts may also misallocate new resources.

This study presents evidence related to these concerns by evaluating where the new resources were actually spent. More specifically, these fixed-effects evaluations are based on expenditure data for the 695 observations of districts from the lowest third of their state's pre-reform spending distribution. As noted earlier, these expenditure data identify six broad and exhaustive functional areas to which school districts allocate resources (Table 1). Key results from the econometric evaluations in which these expenditure data are the dependent variables are reported in Table 6. The results in the first row indicate that Massachusetts' reforms increased total spending by a statistically significant $962 per pupil. It should be noted that this spending increase is nearly twice as large as the $556 per-pupil revenue increase identified in Table 4 for these districts. These somewhat incongruous estimates could reflect changes in the cash or debt held by these districts. Furthermore, these differences could also reflect, to some degree, the sampling variation in these data. However, the accounting procedures employed in the F-33 data with regard to capital expenditures over this period are also likely to play an important role. The capital expenditures reported by districts over this period reflect the "expensing" of new assets. More specifically, the reported capital expenditures reflect the total outlay in a year instead of some proportion of the cost calculated over the expected life of the asset (Fowler, 1990). The results from the capital-expenditure equations suggest that this accounting convention may be particularly important. The estimate in Table 6 indicates that the reforms increased capital expenditures by $167 per pupil. This statistically significant marginal effect represents a

TABLE 6
The Estimated Effects of Court-Ordered Finance Reforms on Per-Pupil Expenditures by Function in Low-Spending Districts

Dependent Variable	Estimate	R^2
Total general expenditures per pupil	962‡ (154)	0.7236
Instructional expenditures per pupil	593‡ (81)	0.8672
Support service expenditures per pupil	249‡ (70)	0.7414
Non-instructional expenditures per pupil	−128‡ (22)	0.5920
Non-El/Sec expenditures per pupil	−73‡ (8)	0.7017
Capital expenditures per pupil	167† (83)	0.2711
Other LEA expenditures per pupil	154‡ (39)	0.6641

Note. These results are based on annual observations of unified school districts from Connecticut, Maine, and Massachusetts in the 1990, 1992 and 1994–1996 fiscal years. These observations are from districts that were in the bottom third of their state-specific, pre-reform distribution of per-pupil expenditures. All models include district fixed effects and urbanicity-specific year fixed effects. The absolute values of heteroskedastic-consistent standard errors, adjusted for state-year clustering, are reported in parentheses.
†Statistically significant at the 5% level.
‡Statistically significant at the 1% level.

49% increase in the average level of capital expenditures per pupil (Table 1). However, this sharp increase should be interpreted with caution since it reflects the full current value of long-lived assets purchased in anticipation of sustained increases in state revenues.

How did the increased state aid influence spending in other areas? Notably, the results in Table 6 indicate that the largest increase ($593 per pupil) was on instructional spending. There were also smaller but significant reform-driven increases in support services and other district expenditures (intergovernmental payments and debt interest). Interestingly, these evaluations suggest that there were actually small but significant decreases in the two expenditure categories unrelated to student instruction and elementary and secondary education. These provocative results may reflect the variety of organizational and procedural reforms that accompanied the education finance reforms and decentralized decision-making authority within districts. As a more general matter, the evaluation results in Table 6 do not provide evidence on whether school districts allocated the new resources efficiently or inefficiently. However, they do indicate that a substantial proportion of new spending was directly on student instruction and capital projects, two areas frequently targeted by educational policy makers (e.g., GAO, 1995).

Another way to frame these results is to compare them to how school districts allocated resources on average. In particular, instructional spending comprised 58% of average expenditures (Table 1) and 62% of the new, reform-driven spending (Table 6). This comparison suggests that school districts spent the new aid as they would have spent any new resources. However, that interpretation may be too generous since it might have been reasonable to expect that a much larger-than-average fraction of the marginal aid dollar would have "reached the classroom."[30] It should also be noted that other recent studies provide related evidence that the new state support elicited by education finance reforms can increase school quality. For example, using data on school districts nationwide, Dee (2000) presents evidence that the new resources generated by court-ordered reforms were capitalized into property values and residential rents. This Tiebout response suggests the value that parents placed on this spending. Other studies have reported evidence of test score gains due to such reforms (e.g., Card & Payne, 2002; Guryan, 2001). In particular, the test score gains reported by Guryan (2001) are based on Massachusetts' data and the exogenous variation in spending created by these 1993 reforms.

Conclusions

Several recent studies have demonstrated that the earliest court-ordered education finance reforms increased state aid to school districts, particularly to the poorest districts in the reform states. The evaluation results presented here have demonstrated that Massachusetts' 1993 court-mandated reforms were, basically, no exception. However, the existence of this exogenous variation in state aid to school districts raises other questions that are relevant both to our understanding of state-level education finance reforms and to the broader question of the likely effects of providing additional resources to schools. More specifically, one important question involves whether new financial aid will actually lead to

significant increases in educational spending by local districts. A second, critical question has to do with how districts allocate new resources. This study has presented novel evidence on both questions by evaluating the consequences of Massachusetts' reform-driven increases in state support for local school districts. More specifically, using district-level panel data from Massachusetts and neighboring states, this study has evaluated how court-ordered education finance reforms influenced the level and composition of school spending.

The results clearly indicate that, in the lowest-spending districts for which these reforms were intended, the new state revenues did lead to a substantive increase in educational spending. Furthermore, these results also indicate that 79% of new spending was directed towards either capital expenditures or the direct, instruction-related interactions of students and teachers. These results do not indicate that the resources spent in these areas were not wasted or that the money spent elsewhere was. However, these findings do demonstrate that a substantial proportion of new resources were allocated to functional areas that are typically regarded as important priorities by educational policymakers. Another interesting dimension to these results and the Massachusetts experience is that this allocation of new revenues occurred despite the conspicuous absence of state-level regulations on how school districts should spend these new resources. Instead, these finance reforms were accompanied by organizational changes that promoted the local involvement and decision-making authority of parents, principals and teachers. Whether such institutional traits play a critical role in how school districts allocate new state aid is an important issue. Future evaluations may provide useful comparative evidence on this topic by examining the effects of recent, finance-related reforms in states characterized by different institutional traits.

Notes

[1]A similar case was brought before the U.S. Supreme Court in 1973 (*San Antonio Independent School District v. Rodriguez*). However, the Court rejected the argument that education finance based on local property taxes violated the U.S. Constitution. This decision effectively returned judicial action regarding education finance to the states. Arguments at the state level have largely been based on the equal protection and education clauses of state constitutions. However, more recent rulings have also emphasized issues of adequacy (Verstegen, 1994; Long, 1999).

[2]Card and Payne (2002) report similar results. Prior research had focused on the experiences within specific states like California and New Jersey (e.g., Megdal, 1983; Downes, 1992; Fischel, 1994; Silva & Sonstelie, 1995)

[3]In general, state aid to wealthy districts was not influenced by court-ordered reforms (Murray et al., 1998; Evans et al., 1997). However, Hoxby (2001) emphasizes the heterogeneous experiences within particular states.

[4]In fact, this sort of response to intergovernmental grants is seldom observed (Hines & Thaler, 1995). Instead, researchers have typically reported the anomalous existence of a "flypaper effect": grant money tends to stick and increase spending in excess of what would be predicted by income effects. Earlier research on education finance reforms, based on national data generated similar results (Fisher & Papke, 2000). Reform-driven increases in state aid to poor districts led to relatively small reductions in locally raised revenues and, therefore, increased overall educational spending (e.g., Evans et al., 1997).

[5]These reforms were arguably motivated by a contemporaneous state Supreme Court ruling in the *McDuffy v. Robertson* case that found the state's current education finance system unconstitutional. Prior evidence has indicated that, in the absence of such a court ruling, legislatively motivated reforms have typically been ineffective (Evans et al., 1997).

[6]Sixty-five percent of students were initially in districts that failed to meet their standard (Ardon & Costrell, 2001; Crosby, 2001). By 2000, virtually all districts were at foundation spending. "Chapter 70" aid from the state increased from $1.3 billion during the 1993 fiscal year to $2.8 billion in 2000 (Ardon & Costrell, 2001). The funding reforms also established standards of effort intended to elicit additional tax effort from most municipalities.

[7]The reform-driven increase in instructional spending is consistent with the average allocation of resources towards instruction. The results presented here also indicate that these comprehensive reforms led to statistically significant *reductions* in district spending unrelated to instruction or elementary-secondary education.

[8]The existence of a Tiebout response to new state aid raises other important issues (Wyckoff, 1995; Dee, 2000). In particular, the redistributive intent of court-ordered reforms may be attenuated if poorer residents face increased housing costs. A Tiebout response also raises the concern that observed test-score gains may reflect non-random migration instead of true increases in students' cognitive achievement.

[9]In contrast, legislatively motivated education finance reforms appear to have been ineffective in the absence of a court mandate.

[10]However, the possibly heterogeneous experiences within particular states should be noted (Hoxby, 2001).

[11]We recognize that what constitutes a productive educational expenditure is a highly contentious issue and interpret our results accordingly. However, given the direct policy relevance of finance reforms and the broader interest in understanding how districts might spend new resources, an understanding of how new resources are actually allocated is nonetheless important.

[12]Specifically, we examined this possibility by creating a Massachusetts-specific dummy variable equal to one for the 1992 fiscal year, which was prior to the reforms but during the recession. As predicted by Cutler et al.'s (1997) results, this variable was associated with statistically significant reductions in locally raised revenues but only among districts that had been high-spenders prior to the reforms. Furthermore, since we find that the reform-driven increase in aid received by these districts led to reductions in local revenues, the bias created by Proposition 2½ merely implies that our results understate the true magnitude of this effect.

[13]The state legislature actually approved the act and a seven-year funding schedule prior to the court ruling but they did not clearly identify funding sources. Prior evidence indicates that legislatively motivated reforms are typically ineffective in the absence of a pejorative court ruling like the *McDuffy* decision (Evans et al., 1997). Nonetheless, a mild caveat about the true impetus for these reforms (i.e., the state house or the court house) is appropriate.

[14]"Chapter 70" aid from the state increased 11.1% to $1.4 billion dollars in the 1994 fiscal year. By the 1996 fiscal year, this annual amount had increased to $1.8 billion.

[15]The foundation target for each district was based on 18 spending categories and adjusted for 12 different enrollment categories (e.g., pre-K, elementary, secondary, special education, bilingual, low-income, etc.) and local wage levels. The foundation targets were seen by some as ambitiously high; 65% of students were initially in districts that failed to meet the standard (Ardon & Costrell, 2001; Crosby, 2001).

[16]The demand for educational spending appears to be inelastic with respect to income changes, with estimated elasticities ranging from 0.40 to 0.65 (Fisher & Papke, 2000).

[17]Connecticut had court-ordered reforms in 1977. Its system of education finance was deemed constitutional both in 1982 and 1985. Maine's system of education finance was also deemed constitutional in 1995.

[18]The 1992 and 1996 data are available in SAS format on the Census Bureau's web site. The 1990 F-33 data were drawn from the NCES Common Core of Data (CCD) CD-ROM.

[19]This is a mildly unbalanced panel. Overall, there are 397 school districts in the 1990 and 1992 fiscal years and 396 districts in the 1996 fiscal year. The variation in the number of school districts reflects a modest number of district "births" and "deaths." The results presented here are robust to considering only those districts observed in each year.

[20]These unified school districts only account for 50% to 70% of the total number of districts in these states. However, the students enrolled in these districts comprise 81% to 90% percent of the total public school enrollments.

[21]The state reforms could influence the amount of Federal revenues in several ways. For example, new state support could influence a district's eligibility for certain Federal programs. Also, new state aid (as well as increased parental and principal involvement) may create new capacity for applying to Federal discretionary programs.

[22]Massachusetts supports local school construction through a separate program, not through the new "Chapter 70" formula. However, for districts spending above their requirements, the fungibility of any new aid could, nonetheless, imply increased spending on land and buildings. Furthermore, the F-33 definition of capital expenditures, which includes instructional equipment, differs from the state definition.

[23]As noted earlier, the reduced-form nature of these evaluations implies that the effects of the new funding formula cannot be clearly separated from the effects due to the other organizational and procedural reforms. We stress this important caveat when considering the implications of these results for other states (i.e., the "external validity" of these evaluations).

[24]An alternative approach might be to attempt to use observed variables as proxies for the many cross-sectional and time-series determinants of revenues and expenditures. However, the unobserved nature of the relevant socioeconomic characteristics may make such an effort futile. Furthermore, detailed data on the within-district variation in such traits are generally unavailable for such a short time period.

[25]Our standard errors accommodate district-specific heteroskedasticity. This conservative approach to this study's inferences may be appropriate given the possible serial correlation in the fiscal outcomes within districts (Bertrand et al., 2004). These standard errors also reflect a finite sample correction (Davidson & MacKinnon, 1993).

[26]The trend data in Figure 1 on state revenues per pupil also illustrate these comparisons. State revenues per pupil increased in Massachusetts after the 1993 reforms while they changed relatively little in the neighboring control states.

[27]This rank could not be defined for 11 of the 1,983 district-year observations because they were only observed in the post-reform period or only once in the pre-reform period.

[28]However, less inclusive models return results similar to those reported here.

[29]In contrast, higher-spending districts benefited particularly from minimum aid and base aid, which was relatively time-invariant. State appropriations for the reforms also grew over time.

[30]On the other hand, these calculations may understate the share of new spending that went to instruction since the expensing of capital expenditures inflates the apparent increase in total expenditures.

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