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Lotteries, Litigation, and Education Finance

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In 1993, the state of Georgia instituted a lottery that earmarked new funds for instructional and capital expenditures in public schools. In that same year, Tennessee began court-ordered education finance reforms that were also designed to promote instructional and capital expenditures. Using district-level panel data, this study presents empirical evidence on how these disparate policies influenced the patterns of educational revenues by source and expenditures by function. The results suggest that both state policies increased the state aid to the poorest districts and promoted some spending on the targeted functions. However, the results also suggest that these reforms influenced spending in several other functional areas.

1. Introduction

Historically, the finance of public education in the United States has been largely based on revenue raised locally through property taxation. However, this system has been routinely criticized on the grounds that the resulting distribution of educational opportunity is inequitable and that it may lead us to forgo socially productive investments in education. Over the past several decades, these concerns have motivated extensive litigation in almost every state, challenging the constitutionality of property-based education finance. To date, the supreme courts in 17 of these states have encouraged increased state aid to school districts by concluding that their current systems of education finance were indeed unconstitutional. However, over this same period, 37 states have also turned to new state lotteries as a way to increase state funding for services like education. There are two shared reasons that these disparate approaches to directing new resources to education might prove to be ineffective. First, it is by no means clear that these reforms would actually increase state or local educational spending. State legislatures may respond slowly, if at all, to a court ruling that encourages increased aid. Furthermore, states that earmark new lottery revenues for education may simply choose to then reduce their education appropriations from other sources. And, even if these reforms do increase state aid, the effect on educational spending may be undone by reductions in education revenues raised from local and federal sources.[1] Second, even if new state aid were to increase local education spending, it is not clear that these new resources would be allocated in ways that actually improved school quality. The extensive though controversial literature on educational productivity suggests that new resources would not be used effectively (e.g., Burtless 1996).

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[1] The available evidence indicates that the earliest 11 court-ordered reforms increased state aid per pupil as well as district spending (e.g., Evans, Murray, and Schwab 1997; Murray, Evans, and Schwab 1998; Card and Payne 2002). However, state lotteries have often not increased the overall level of state aid to education (e.g., Clotfelter and Cook 1989; Borg, Mason, and Schapiro 1991; Clotfelter 1994; Spindler 1995).

This study provides novel, empirical evidence on these policy-relevant concerns by examining the consequences of two of the most recent and noteworthy education finance reforms. In 1993, Tennessee began court-ordered education finance reforms while the neighboring state of Georgia simultaneously initiated a lottery explicitly designed to promote educational spending. These policy experiments are partly of interest simply because of their narrow consequences for the patterns of educational finance and spending within these states. However, they are also likely to be of more general interest because they provide evidence on a question of importance to policymakers and voters everywhere. Specifically, the consequences of these reforms provide evidence on how plausibly exogenous increases in available educational resources are actually spent. Though there is an extensive literature on educational productivity, there is surprisingly little evidence on how school districts allocate resources.[2] Furthermore, these evaluations may also prove of more general interest because each of these states adopted rather unique and interesting strategies to earmark new state aid for specific educational expenditures. In Tennessee, the court-ordered education finance reforms were accompanied by broader educational reforms designed to ensure the efficacy of the new state aid. These measures included accountability measures for schools and school districts based on controversial, value-added measures of student test performance; the phasing in of mandatory class-size regulations; and a high-profile initiative to promote technological investments in classrooms. Georgia also attempted to earmark new lottery revenues for specific educational spending by promising new and highly visible educational initiatives (i.e., Helping Outstanding Pupils Educationally [HOPE] Scholarships, prekindergarten [pre-K] programs, and technological investments for classrooms). The absolute and comparative success of these earmarking attempts can provide evidence to other communities that hope to promote spending on specific educational functions.

This study presents evidence on how these policies influenced per pupil revenues by source (federal, state and local) and expenditures by function. The panel data used in these evaluations are from the unified school districts in Georgia, Tennessee, and South Carolina and the annual "F-33" Survey of Local Government Finances combined for the 1990, 1992, 1994, and 1995 fiscal years. The F-33 survey divides expenditures into six broad categories: instruction, support services, noninstructional services, functions unrelated to elementary and secondary education, capital expenditures, and other expenditures. While more detailed data on the allocation of expenditures would have been welcome, this taxonomy still allows us to address the broad question of interest, namely, whether these reforms were effective in increasing the targeted functions of student instruction and capital improvements.[3] The empirical results presented here are based on two-way fixed effect specifications that exploit the panel nature of the available data to purge the potentially confounding determinants of revenue and expenditure outcomes that are specific to each school district and to each fiscal year.[4] The results indicate that both policy initiatives were successful at increasing state aid to school districts as well as district spending.[5]

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[2] Lankford and Wyckoff (1995) and Rothstein and Miles (1995) provide useful descriptive evidence on how districts allocate resources and how these allocations have changed over time. However, these results do not exactly parallel the thought experiment of interest because the observed changes in available resources are not necessarily exogenous.

[3] The F-33 data from the 1992, 1994, and 1995 fiscal years do provide some additional detail on the subcomponents of these six categories. I discuss the results of models that exploit these data to examine the effects of these reforms on the particular components of capital expenditures (i.e., land and construction purchases, equipment purchases).

[4] The validity of inferences based on this research design relies critically on the assumptions that each of the state reforms was exogenously given and that the data from South Carolina provide valid controls for the shared time-series variation that is unrelated to the state reforms and other controls. A variety of anecdotal and empirical evidence in support of these maintained assumptions is discussed.

[5] However, this evidence suggests that the spending effects of the new state aid were modestly attenuated by corresponding reductions in revenues from local and federal sources.

Furthermore, in both cases, this new state aid was targeted toward the poorest districts within each state, and therefore improved the equity with which state resources were distributed. Additionally, in both states, the new reform-driven aid led to statistically significant increases in spending on the targeted functions (e.g., student instruction, equipment purchases). However, significant amounts of the new aid were also allocated to functions that were not clearly targeted (e.g., support and noninstructional services). A comparative assessment of each state's earmarking efforts suggests that Georgia's were most effective, perhaps because they were linked to new and highly visible educational initiatives.

2. Lotteries and Litigation

Over the past several decades, an extensive body of litigation has attempted to improve the equity with which educational resources are distributed. Beginning with the influential 1971 *Serrano* decision in California, court rulings in almost every state have assessed the constitutionality of systems of education finance based largely on local property wealth. To date, the supreme courts in 17 states have ruled in favor of the plaintiffs, deeming the systems of education finance unconstitutional.[6] These decisions have typically been based on interpretations of the equal protection clauses as well as the education articles of state constitutions. However, the rulings in recent years have addressed equity issues indirectly, focusing instead on the unique needs of students in poor communities and the inadequacy of locally available resources (Verstegen 1994). Recent empirical evaluations have demonstrated that the earliest court-ordered reforms were effective in encouraging states to direct new resources to poorer school districts (Evan, Murray, and Schwab 1997; Murray, Evans, and Schwab 1998; Card and Payne 2002).[7] For example, in a study based on district-level panel data from the 1972–1992 period, Murray, Evans, and Schwab (1998) conclude that the earliest 11 state reforms increased spending in the poorest districts by 11% while leaving spending in the wealthiest districts unchanged.

This study assesses the effects of an important and more recent ruling, the 1993 Tennessee decision in *Tennessee Small School Systems v. McWherter*.[8] The litigation in Tennessee was initiated in 1988 by a group of small, largely rural school districts. Over the next four years, a combination of contradictory lower-court rulings and controversial legislative efforts to identify a tax base for new state funding left the issue largely unresolved. However, in March 1993, the Tennessee Supreme Court ruled unanimously that the state's foundation program for school funding was unconstitutional. Like other recent rulings, this decision emphasized the state's role in ensuring the equal availability of a quality education. The plaintiffs sought to have the state immediately improve funding levels. However, the state was instead allowed to implement the new Basic Education Program (BEP) created by the Education Improvement Act (EIA), which phased in $1 billion of new aid over the next five years (Green 1997; Goldhaber and Callahan 2001). The money for these increases came largely from a half-cent increase in the state sales tax.[9] The new BEP funding formula, one of the most complex in the nation, adjusted state aid in response

[6] For an overview of this litigation, see http://nces.ed.gov/edfin/litigation/Contents.asp.

[7] There is also evidence linking the resources generated by these reforms to increases in test scores (Guryan 2001; Card and Payne 2002), Tiebout mobility (Aaronson 1999), and increases in residential property values and rents (Dee 2000).

[8] The other states where state supreme court rulings have recently invalidated the educational finance system include Alabama (1993), Massachusetts (1993), New Hampshire (1993), Ohio (1997), and Vermont (1997).

[9] The impending court ruling was widely understood as the underlying (and independent) impetus for Tennessee's education finance reforms, which began to take effect in fiscal 1993. However, a mild caveat is nonetheless appropriate since the 1992 sales tax increase that funded the reforms actually preceded the court's ruling. The available empirical evidence indicates that legislatively motivated efforts to reform education finance have been ineffective in the absence of such court rulings (Evans, Murray, and Schwab 1997).

to local fiscal capacity as well as cost differentials. However, the amount of aid generated by the formula was also based on the district-specific costs associated with 33 explicit classroom functions and 10 nonclassroom functions deemed necessary by the state for educational adequacy (Green 1997; Goldhaber and Callahan 2001). The state allocations associated with the BEP categories were explicitly designed to reallocate funds toward "direct student or classroom needs" (Green 1997). Interestingly, the increased funding created by the EIA was also bundled with other comprehensive educational reforms intended to target the new spending productively. The earmarking efforts included the phasing in of mandatory class-size reductions and an initiative to invest in high-technology "21st-century classrooms." However, the EIA also instituted several controversial accountability measures intended to promote the productive use of the new aid. These included exit exams for graduating students; performance standards for schools and districts based on test scores, dropout rates, and student attendance; and state authority to place underperforming schools and districts on probation.

Over the same period, the neighboring state of Georgia was pursuing an alternative path to funding and reforming its public schools, a new state lottery.[10] For most of the 20th century, no state raised revenues by means of a lottery. However, beginning with New Hampshire in 1964, 37 states have introduced them, making it the fastest-growing source of state revenue. However, this expansion has not gone unchallenged. In particular, lottery critics have pointed to the evidence that state-sponsored gaming is a fairly regressive way of raising revenues.[11] Proponents of lotteries have countered that the expenditure of earmarked lottery revenues can attenuate this regressivity. However, since the state appropriations from other funding sources can respond to the presence of earmarked lottery revenues, it is not clear that earmarking is an effective way to promote targeted expenditures. In fact, there is evidence from several states that lottery revenues earmarked for educational expenditures simply crowded out other revenue sources and did not increase overall state aid (e.g., Borg, Mason, and Shapiro 1991; Clotfelter 1994; Spindler 1995). However, Clotfelter and Cook (1989) note that the earmarking of lottery revenues is more likely to be effective in this regard if the designated expenditures are large relative to the conventional funding levels. Earmarking is also more likely to be successful if the funds are linked to a new and highly visible public service. In this sense, Georgia's lottery provides a particularly interesting case.

The improbable adoption of a state lottery in Georgia was driven largely by the zeal of then-Governor Zell Miller. On November 2, 1992, a fairly slim majority (52.2%) of Georgia's voters voted in favor of an amendment to the state constitution that was necessary to allow the governor to initiate lottery operations. Part of what made this new lottery politically palatable was that its stated purpose was to provide new funds for educational initiatives in three highly visible areas.[12] One is the HOPE Scholarship program, which provides qualified high school graduates with tuition or tuition subsidies at in-state postsecondary schools. The remaining earmarked lottery revenues were dedicated to pre-K programs for four-year-old children and the equipment and capital needs of elementary and secondary schools.[13] The first sale of state lottery tickets began on June 29, 1993. The per capita sales in the

[10] A 1981 decision by the Georgia State Supreme Court found that their system of education finance was constitutional.

[11] See Clotfelter and Cook (1989) and Price and Novak (1999) for evidence of regressivity and Cohn and Cooper and (1994) for an alternative viewpoint. The implicit tax on lottery purchases is also targeted toward minorities and those with low educational attainment. Lotteries have also been criticized for being an administratively costly mechanism for raising revenue. However, Clotfelter and Cook (1989) argue convincingly that the administrative cost of lotteries should be understood differently than the cost of administering other tax revenues.

[12] Georgia law requires that roughly a third of lottery revenues be reserved for education, half returned as prizes. The remainder is for operating costs and commissions.

[13] Many, but not all, of the pre-K providers were unified school districts.

lottery's first year were surprisingly strong, and over the 1994 and 1995 fiscal years, the state of Georgia allocated roughly $115 million of lottery revenues for pre-K programs, $309 million for technological investments in schools, and $168 million for school construction (Byron and Henry 1999). The lottery-driven funds for pre-K programs were administered independently by the newly created Office of School Readiness, which is not part of the state Department of Education. The funds for school construction and technology were disbursed by the state Department of Education, in part through discretionary grants that been criticized on the basis of political favoritism (White and Walston 1994; Holmes and Dayton 1997).

Since Tennessee's reforms relied on a sales tax and Georgia's on a lottery, these policies could each be criticized on regressivity grounds. However, full consideration of the regressivity issue has to take into account the incidence of any new expenditure generated by these policies. *A priori,* there are a number of reasons to suspect that the expenditure effects of these policies would be largely irrelevant. First, as noted previously, the earmarking of any new revenue sources for state aid to education might not actually increase overall state aid since legislators could simply reduce the appropriations from other sources. Second, even if these policies did increase state aid to local school districts, we might expect these resources to be effectively transferred to local taxpayers. They could do this by simply choosing to lower the amount of locally raised revenues for education.[14] Third, these policies could also generate an effective reduction in revenue from federal sources because of program requirements or reduced district-level incentives to seek out such support. And, finally, even if local school spending did increase as a result of new, reform-driven state aid, it is by no means clear that this spending would actually increase school quality. An extensive literature has cast considerable doubt on the claim that school districts allocate resources in ways that improve school quality (e.g., Burtless 1996). However, that view is a controversial one. Evaluating how new, reform-driven resources were actually spent can provide useful intermediate insights into that issue. Furthermore, evidence on how new aid was spent would also indicate how successful each state's unique approach to targeting the desired expenditures was. The next two sections introduce the data and methods used to address these questions.

3. Data

The evaluations presented here are based on panel data from the school districts in Tennessee, Georgia, and South Carolina. Most of these data were drawn from the annual F-33 Survey of Local Government Finances. The U.S. Department of Education's National Center for Education Statistics (NCES) sponsors the survey in conjunction with the Government Division of the U.S. Census Bureau. The F-33 survey is an annual questionnaire that gathers financial data from school districts on the sources of their revenues (federal, state, and local) as well as data on the functional areas to which they allocated these resources (Dee, Evans, and Murray 1999). The expenditure categories are described in more detail later. In years ending with a 0, 2, or 7, all school districts were surveyed. In other years, only a sample of districts from particular states was surveyed. The panel data set analyzed here was

[14] Prior evidence on the effects of intergovernmental grants suggests that this sometimes happens (e.g., Fisher and Papke 2000). However, there is also considerable evidence that such disbursements tend to "stick" to the local governmental unit beyond what would be expected based on the median-voter model and the income elasticity of such spending (the so-called flypaper effect; Hines and Thaler 1995). Furthermore, it is also possible that locally raised revenues would increase if new state support invigorates the local commitment to and interest in public education.

constructed by pooling district-level F-33 survey data from the 1990, 1992, 1994, and 1995 fiscal years.[15] For each of these four years, the universe of districts was surveyed in these three states. However, as in prior studies (e.g., Evans, Murray, and Schwab 1997), the data set was limited to unified school districts (i.e., K–12), which are similar in scale and organizational goals.[16] Furthermore, special, nonoperating 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 nonstandard districts (O'Leary and Moskowitz 1997). The final data set consists of 1560 observations over the four fiscal years.[17]

Data on demographic and socioeconomic variables that may influence district revenue and expenditure decisions are generally unavailable for intercensal years. Nonetheless, these evaluations assess the empirical relevance of such omitted variables by exploiting county-by-year data from the U.S. Census Bureau. Since the three states contain approximately 390 unified school districts over this period and 300 counties, the degree of aggregation implied by using county-level variables is limited. Specifically, county-level data on income (real median household income) and poverty (percentage of related children, aged 5–17 in families in poverty) are available for 1989, 1993, and 1995. I matched these data to school district observations from the 1990, 1994, and 1995 fiscal years, respectively. The short lag in these control variables is appropriate given district-level budgeting processes. However, since these data were unavailable for 1994, I matched the 1995 data to the fiscal 1995 observations from the F-33. Furthermore, since county-level socioeconomic data were also unavailable for districts observed during the 1992 fiscal year, I imputed the income and poverty variables by averaging the county-level data from 1989 and 1993. I also matched the district-by-year observations to county-by-year demographic data: the percentage of the population that is elderly (aged 65 or over), the percentage black, and the percentage Hispanic (Table 1). These population data, defined as of midyear in 1990, 1991, 1993, and 1994, were matched to the fiscal year data for 1990, 1992, 1994, and 1995, respectively. Changes in household income, poverty, and the racial/ethnic composition of communities are likely to influence the demand for public schooling in general as well as for particular functional expenditures. And changes in the concentration of elderly residents may also be relevant since there is some evidence that an increase in the percentage elderly reduces per pupil educational spending (Poterba 1997). All the revenue, expenditure, and income data employed here were converted to 1996 dollars using the consumer price index.

As noted previously, the F-33 survey provides useful information on each district's annual revenues by source and expenditures by function. The revenue data identify the financial backing from three general sources: federal, state, and local. The descriptive statistics in Table 1 indicate that most of the school district revenues over this period (56%) were provided by the state. On average, 34% of revenues were generated locally, and roughly 10% came from the federal government. On average, these districts spent $4897 per pupil. The NCES decomposes these expenditures into six major categories (Fowler 1990). Instructional expenditure per pupil, accounting for 55% of the total, is the

[15] This provides two "pretreatment" and two "posttreatment" observations of each district. The 1992–1995 data are available in SAS format on the Census Bureau's Web site. The 1990 F-33 data were drawn from NCES's Common Core of Data (CCD) CD-ROM.

[16] Evans, Murray, and Schwab (1997) note that roughly 90% of public school students are in unified districts.

[17] This is a very mildly unbalanced panel. 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 of these four years.

Table 1. Descriptive Statistics, Unified School Districts in Georgia, South Carolina, and Tennessee, 1990, 1992, 1994, and 1995 Fiscal Years

Variable	Mean	Percentage of Total
Total revenues per pupil	$4837	100%
State revenues per pupil	$2717	56%
Federal revenues per pupil	$467	10%
Local revenues per pupil	$1653	34%
Total general expenditures per pupil	$4897	100%
Instructional expenditures per pupil	$2685	55%
Support service expenditures per pupil	$1279	26%
Noninstructional expenditures per pupil	$309	6%
Nonelementary/secondary expenditures per pupil	$52	1%
Capital expenditures per pupil	$486	10%
Other LEA expenditures per pupil	$85	2%
County-level variables		
Real median household income	$29,240	—
Percentage of children in poverty	.24	—
Percentage elderly	.13	—
Percentage black	.23	—
Percentage hispanic	.01	—
Urban	.03	—
Suburban	.27	—
Rural	.70	—
Sample size	1560	

All expenditure, revenue, and income data are unweighted and in real 1996 dollars. The expenditure and share statistics may not sum accurately because of rounding errors.

largest of the six categories (Table 1). These expenditures apply to "activities dealing directly with the interaction between teachers and students," including not only teacher activity but also the services of teacher aides and other classroom assistants and textbook purchases (Fowler 1990, p. 88). Support services (26% of total expenditures) 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, p. 88). More specifically, this category encompasses a diverse list of support programs, such as social work, attendance accounting, psychological and health services, teacher training, plant operation and maintenance, student transportation, and school and general administration. The third-largest category (10%) involves capital expenditures, which include school construction, instructional equipment, and land purchases. Six percent of expenditures go to noninstructional functions that are nonetheless related to elementary and secondary education. These functions include food services and businesslike enterprise operations such as bookstores (Fowler 1990). The nonelementary/secondary category includes expenditures on adult education and various community services (e.g., swimming pools, child care). The final expenditure category 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 district functions among which we might like to discriminate. However, since the F-33 taxonomy clearly identifies instructional and capital expenditures, these data can provide a rough sense of how effective the earmarking of reform-driven state aid was in influencing the actual pattern of expenditures. Additionally, some of the results discussed here make use of per pupil

expenditure data on the subcomponents of capital expenditures (construction expenditures, land purchases, equipment purchases). However, these more detailed F-33 data are not available for the 1990 fiscal year.

4. Specifications

The evaluation results presented here are based on fixed-effect specifications that exploit the panel nature of the pooled F-33 data to control for the many unobserved determinants of the district revenue and expenditure outcomes. More specifically, the specification takes the following form:

$$Y_{dst} = \beta \mathbf{X}_{st} + \mu_d + \nu_t + \varepsilon_{dst},$$

where Y_{dst} is the per pupil expenditure or revenue outcome for district d in state s in year t. The terms μ_d and ν_t are district-specific and year-specific fixed effects. Since the districts are of varying sizes, the error term, ε_{dst}, is unlikely to have a constant variance, and heteroscedastic-consistent standard errors are reported.[18] In the sparsest specifications, the matrix, $\mathbf{X}_{st}$, includes only the regressors of interest, two state-specific dummy variables equal to 1 for the Georgia and Tennessee districts when observed in the "posttreatment" 1994 and 1995 fiscal years. However, the robustness of the estimated effects associated with these variables is examined by incrementally introducing the county-level controls for income, poverty, and demographic traits. Since the three states differ somewhat in their urbanicity, another source of omitted variables may be due to unobserved trends unique to rural, suburban, and urban districts. For example, the districts in Georgia and Tennessee are more likely to be rural than those in South Carolina. To the extent that rural communities had unique trends influencing their revenue and expenditure decisions over this period, these results could be biased in an uncertain direction. The final specification accommodates such possibilities unrestrictively by allowing for urbanicity-specific year fixed effects (i.e., interactions between ν_t and district level-indicators for rural, suburban, and urban status). Furthermore, since the effects of these reforms on the state-specific patterns of equity are of particular interest, some results are also based on subsamples of districts defined by their prereform spending. More specifically, I calculated for each district the average of real per pupil expenditures in the 1990 and 1992 fiscal years and then identified whether each district was in the bottom, middle, or top third of its state-specific distribution for this variable.[19] The results for revenue equations are reported for models based on the full data set as well as for models estimated separately for each of these three groups.

The identification of policy effects in these models effectively exploits comparisons of the within-district changes in each "treatment" state to the contemporaneous within-district changes in the neighboring "control" state, South Carolina. For example, the trends in Figure 1 indicate that, after 1993, the real state revenues per pupil in Georgia and Tennessee increased dramatically. But we should not necessarily attribute these increases to the effects of Georgia's new lottery or Tennessee's court-ordered finance reforms. The shared trends in these two states could plausibly be driven by other region-specific determinants (most obviously, a procyclical increase in real state aid following the earlier recession-related declines). However, the contemporaneous data from South Carolina, which indicate only a relatively modest growth in state aid after 1993, suggest that this is not the case. But this sort

[18] The error structure for these models could also reflect shocks that are specific to each state and year (Moulton 1990). However, I found that models that accommodated this possibility generated results quite similar to those reported here.

[19] Evans, Murray, and Schwab (1997) and Dee (2000) adopt a similar approach in their finance-reform evaluations. Models that examine the heterogeneity across these three groups omit eight observations for whom these rankings are not defined.

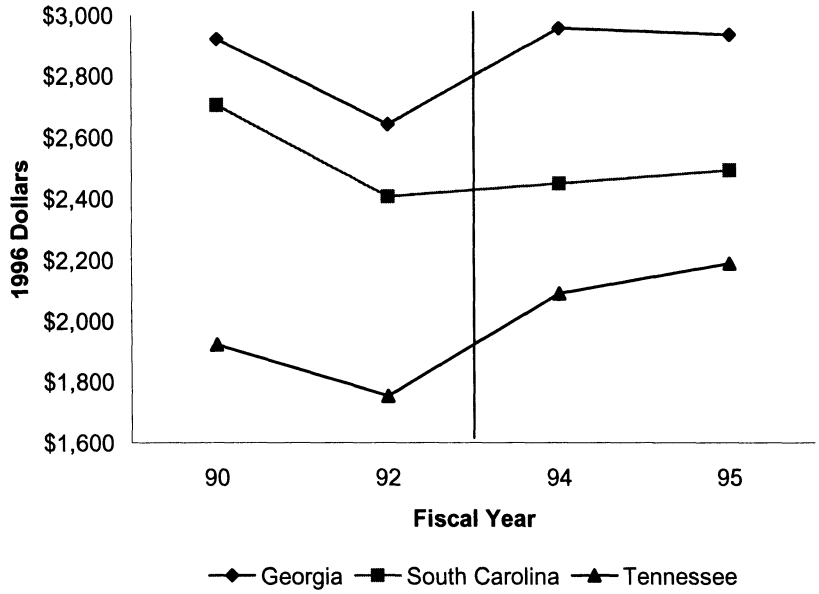

Figure 1. Per Pupil State Revenues by State and Year

of "difference-in-differences" inference relies on the implicit assumption that the data from South Carolina provide valid controls for the shared time-series variation in the dependent variable that is unrelated to the state-specific reforms in Georgia and Tennessee. One way to frame concerns about this maintained assumption is to suggest that there may have been state-specific trends in relevant socioeconomic and demographic variables that happened to be contemporaneous with the timing of each state's reforms. However, because these panel data span a relatively short time period and the estimated effects of these policies are often quite large, the possibility that such omitted variables could impart a substantive bias seems remote. Furthermore, the state trends in socioeconomic and demographic variables generally track each well over this period. And the available evidence suggests that the timing of the reforms in Georgia and Tennessee was independent of within-state trends.[20] Nonetheless, the possible sensitivity of this study's inferences is evaluated directly by comparing the results from models that introduce the available control variables.

The available evidence also suggests that the variation within South Carolina over this period is not confounded by state-specific events.[21] Prior to this study period (i.e., in the late 1970s and early 1980s), South Carolina made substantive changes to its school funding that sharply increased state aid and reduced funding inequities (Cohn 1984; Flanigan and Richardson 1994). And, by 1988, partly as a result of these earlier state initiatives, the South Carolina Supreme Court rebuffed a court challenge

[20] In Georgia, the adoption of the lottery was an idiosyncratic initiative driven by the governor, while the reforms in Tennessee were driven by the state supreme court's interpretation of the state constitution and the time-invariant inequities in the prior system of education finance.

[21] As a robustness check, I also considered extending the set of controls to include the districts from the other neighboring states (e.g., Alabama, Florida, and North Carolina). However, the available evidence suggests that these districts would provide poor controls because each of these states experienced unique changes in education finance over this period. Florida, which instituted an "education" lottery in 1988, actually saw a unique and distinct decline in real, per pupil state support over this period (Herrington and Cistone 1994). In Alabama as in Tennessee, a 1993 state supreme court ruling invalidated their system of education finance. However, it is unclear that this decision generated any real reforms over this period, and a 1997 court ruling reaffirmed their prior assessment of unconstitutionality. In North Carolina, there was a major state initiative over this period to direct new state funds to local districts (Mesibov 1995).

and concluded that the state's system of education finance was constitutional.[22] Over the next seven years, there was no evidence of unusual state-specific education appropriations in South Carolina. In particular, the data in Figure 1 indicate that, over the first two observed years of the study period (i.e., the 1990 and 1992 fiscal years), the recession-related declines in real state per pupil aid in South Carolina tracked the corresponding declines in Georgia and Tennessee well. However, on the basis of concerns about the appropriateness of South Carolina as a "control," I did reject the opportunity to extend the data set to include the 1996 fiscal year. Both the available data and contemporaneous news accounts indicate that there was a large and idiosyncratic increase in state educational spending in South Carolina for that year. News accounts imply that this increase was due in part to the unanticipated growth in state revenues that resulted from the decision to reopen to the nation a low-level nuclear waste site in Barnwell County. The availability of only two credible "posttreatment" observations serves as an effective reminder that we may need to be agnostic about the long-term revenue and expenditure implications of these reforms.

5. Results

Revenues by Source

Table 2 presents estimates, based on the entire data set, that identify the effect of Georgia's lottery and Tennessee's court-ordered education finance reforms on real per pupil revenues by source. The estimates from the first specification control only for district and year fixed effects. However, even these sparse models have R^2 that range from 0.81 to 0.93, indicating that they explain a great deal of the variation in the revenue variables. The results in the three subsequent columns are based on models that introduce the county-level income and poverty variables, the county-level demographic variables, and, finally, urbanicity-specific year fixed effects. The results of the saturated specifications presented in column 4 indicate that the introduction of Georgia's lottery was associated with a $359 increase in real state revenue per pupil, while Tennessee's reforms led to a $442 increase. These statistically significant estimates demonstrate that each of these reforms had, in a proximate sense, their intended effect: increased state aid to schools. However, the other models indicate that these intended effects were somewhat undone by corresponding reductions in local and federal support. More specifically, both reforms implied a statistically reduction in federal revenues of roughly $35 per pupil. Similarly, the availability of new state aid led to reductions in local revenues of roughly $67 per pupil in Tennessee. While these reductions in local and federal revenues were not trivial, they were insufficiently large to negate the overall effect of each state's reforms. Tennessee's court-ordered reforms implied that total revenues per pupil rose by a statistically significant $340. Georgia's lottery-funded education spending increased the per pupil resources available to schools by $307. Furthermore, rough calculations suggest that these increases in total per pupil revenues are quite consistent with the existence of strong "flypaper effects" (Hines and Thaler 1995). For example, Tennessee's reform-driven increase of $442 in real per pupil state aid constitutes about 1.5% of real median household income (Table 1). Since the demand for educational spending is income-inelastic ($\eta = 0.5$; Fisher and Papke 2000), we might have expected per pupil spending to increase by only about $36 (i.e., 0.75% of $4,837). Another notable feature of the results in Table 2 is that the estimates are generally quite invariant to the introduction of the additional controls, which suggests that these results are not biased by unobserved trends varying within states

[22] However, this litigation is still pending since it was remanded to a lower court that has yet to act.

Table 2. The Estimated Effects of Lotteries and Court-Ordered Education Finance Reforms on Per Pupil Revenues by Source

	(1)		(2)		(3)		(4)	
Dependent Variable	Georgia Lottery	Court-Ordered Reform, Tennessee	Georgia Lottery	Court-Ordered Reform, Tennessee	Georgia Lottery	Court-Ordered Reform, Tennessee	Georgia Lottery	Court-Ordered Reform, Tennessee
Total revenue per pupil	222*	366*	229*	351*	318*	350*	307*	340*
	(53)	(33)	(56)	(40)	(79)	(42)	(79)	(43)
State revenue per pupil	354*	506*	351*	470*	376*	454*	359*	442*
	(37)	(15)	(38)	(23)	(63)	(27)	(63)	(27)
Federal revenue per pupil	−34*	−40*	−31*	−39*	−33**	−34*	−35**	−35*
	(8)	(9)	(8)	(9)	(16)	(9)	(16)	(9)
Local revenue per pupil	−98*	−100*	−91*	−80*	−25	−70**	−17	−67**
	(33)	(27)	(34)	(28)	(38)	(29)	(38)	(30)
Income/poverty controls	No		Yes		Yes		Yes	
Demographic controls	No		No		Yes		Yes	
Urbanicity-specific year fixed effects	No		No		No		Yes	

All models include district and year fixed effects. Heteroscedastic-consistent standard errors are reported in parentheses. *, ** Denote significance at the 1% and 5% levels, respectively.

over time. The one qualified exception is the effect of the Georgia lottery on local revenues per pupil. After introducing the county-level demographic controls (column 3), the estimated effect of the lottery falls in absolute value and becomes statistically insignificant.[23] However, the relevance of this mild sensitivity should not be overdrawn since these point estimates are within the sampling variation indicated by the standard errors.

One of the limitations of the results in Table 2 is that, since they were based on all the unified districts in these states, they fail to differentiate the effects of each reform among the districts that had been low, medium, and high spenders within each state prior to the reforms. Table 3 provides evidence of such response heterogeneity by presenting the key regression results from separate models based on data from districts that were in the bottom third, middle third, and top third of their state's prereform distribution of real per pupil expenditures.[24] The results of these evaluations indicate that while each of these reforms led to increased state aid for all districts, the increases were particularly large for those in the bottom third of their state's prereform spending distribution. More specifically, in Georgia, the lottery increased state revenues per pupil by a statistically significant $593 in the lowest-spending districts but by only $59 in the highest-spending districts. In Tennessee, court-ordered reforms increased state revenues per pupil in the lowest-spending districts by a statistically significant $454 but by only $316 in the highest-spending districts.[25]

[23] This sensitivity is driven by controlling for the percentage black in the population, which has a negative and statistically significant effect on local revenues per pupil.

[24] These and all the remaining regression results are based on models that include the county-level controls and urbanicity-specific year fixed effects.

[25] A useful, ad hoc way to assess the reasonableness of these regression results is to compare what they imply about the total increase in state funding for school districts to what actually occurred. The enrollment-weighted increases in state aid implied by Table 3's results are roughly consistent with those increases.

Table 3. The Estimated Effects of Lotteries and Court-Ordered Education Finance Reforms on Per Pupil Revenues by Source and by Local Support

	Bottom Third		Middle Third		Top Third	
Dependent Variable	Georgia Lottery	Court-Ordered Reform, Tennessee	Georgia Lottery	Court-Ordered Reform, Tennessee	Georgia Lottery	Court-Ordered Reform, Tennessee
Total revenue per pupil	611*	339*	238**	319*	−25	236**
	(156)	(71)	(95)	(59)	(180)	(91)
State revenue per pupil	593*	454*	342*	499*	59	316*
	(142)	(56)	(62)	(41)	(108)	(48)
Federal revenue per pupil	−34**	−56*	4	−21	−122*	−40*
	(16)	(21)	(27)	(14)	(27)	(15)
Local revenue per pupil	52	−59***	−108***	−160*	38	−40
	(41)	(35)	(65)	(44)	(113)	(68)
Number of observations	517		528		507	

These models are estimated separately for districts in the bottom, middle, and top thirds of their prereform state distribution of real perpupil expenditures. The eight district-year observations for which this could not be defined are omitted. These models include district fixed effects, urbanicity-specific year fixed effects, the income and poverty variables, and the demographic variables. Heteroscedastic-consistent standard errors are reported in parentheses. *, **, *** Denote significance at the 1%, 5%, and 10% levels, respectively.

However, the results in Table 3 also indicate that the expenditure effects of these increases were to varying degrees undone by reductions in the revenues raised locally and from federal sources. While these reductions appeared to occur somewhat uniformly across all three groups of districts, the smaller sample sizes in these separate models imply that these effects are often imprecisely estimated, and several are not statistically distinguishable from zero. Furthermore, for the lowest-spending districts in each state, the reductions in local and federal revenues were again clearly too small to offset the large and statistically significant increases in state aid. So, these results suggest that the overall effect of Georgia's lottery was to increase total per pupil revenues in these districts by $611, while Tennessee's reforms led to an increase of $339. As in evaluations of the earlier court-ordered reforms in other states, these results indicate that Tennessee's court ruling increased the total amount of resources available to low-spending districts.[26] However, perhaps the most striking and policy-relevant result from Table 3 is that Georgia's new lottery was even more effective in making substantial new resources available to low-spending districts. In particular, these results imply that the targeted distribution of Georgia's earliest lottery revenues to the lowest-spending districts attenuated the regressivity with which these resources were raised and produced outcomes similar to what might have occurred if earlier attempts to initiate court-ordered reforms in Georgia had not been rebuffed.

Expenditures by Function

However, the impact of these reforms should also be judged by their effectiveness in increasing the specific types of school expenditures they targeted. In particular, the rhetoric surrounding both types of reforms suggested that the new state aid would be largely focused on instructional expenditures (e.g., smaller classes in Tennessee, new pre-K programs in Georgia) as well as on important capital expenditures, such as facilities and new instructional equipment. Table 4 presents evidence on how these policies influenced real, per pupil expenditures across the six major categories. These results are

[26] However, since state aid to (and spending by) the highest-spending districts also increased rather substantially, the gains in equity were more substantially attenuated in Tennessee.

Table 4. The Estimated Effects of Lotteries and Court-Ordered Education Finance Reforms on Per Pupil Expenditures by Function in Poor Districts

	Estimated Effects		
Dependent Variable	Georgia Lottery	Court-Ordered Reform, Tennessee	R^2
Total general expenditures per pupil	714*	369*	0.7544
	(169)	(131)	
Instructional expenditures per pupil	197*	136*	0.8942
	(28)	(30)	
Support service expenditures per pupil	−65**	51***	0.9222
	(27)	(30)	
Noninstructional expenditures per pupil	94*	73*	0.8159
	(12)	(14)	
Nonelementary/secondary expenditures per pupil	49*	83*	0.7931
	(11)	(12)	
Capital expenditures per pupil	442*	53	0.4433
	(166)	(119)	
Other LEA expenditures per pupil	−2	−28**	0.6902
	(11)	(13)	

These results are based on data from the 517 districts in the bottom third of their prereform state distribution of real expenditures per pupil. These models include district fixed effects, urbanicity-specific year fixed effects, the income and poverty variables, and the demographic variables. Heteroscedastic-consistent standard errors are reported in parentheses. *, **, *** Denote significance at the 1%, 5%, and 10% levels, respectively.

based on the lowest-spending districts, the group of particular policy interest and the one to which the reform-driven aid was targeted. The results suggest that the Georgia lottery increased total per pupil spending in these districts by $714, while Tennessee's reforms increased spending by $369.[27] The remaining results in Table 4 provide evidence on how the availability of new, reform-driven resources was allocated across capital goods and specific operational expenses. In particular, these results indicate that roughly 28% of the new lottery-driven spending in Georgia was allocated directly toward student instruction (i.e., a statistically significant increase of $197 per pupil). In Tennessee, 37% of new per pupil spending was on student instruction, a statistically significant increase of $136 per pupil. Furthermore, the results in Table 4 also suggest that the introduction of the Georgia lottery was particularly successful in promoting capital expenditures. The estimated increase of $442 in per pupil capital expenditures constitutes 62% of the total increase in reform-driven spending. In contrast, these regressions suggest that Tennessee's reforms increased per pupil capital expenditures only by a statistically insignificant $53. However, this aggregate expenditure measure may obscure the targeted effects of Tennessee's reform-driven spending in this area. In fact, models based on more detailed data from the 1992, 1994, and 1995 fiscal years suggest that Tennessee's reforms did lead to a statistically significant increase in expenditures on instructional equipment (roughly $56 per pupil).

These results suggest that both states' efforts to earmark new aid for student instruction were at least somewhat successful. However, one way to gauge explicitly the absolute and comparative degree of success is to consider how the marginal reform-driven dollar was allocated relative to the average.

[27] These estimates are somewhat larger than the per pupil revenue changes identified in Table 3, but these small differences are well within the statistical imprecision suggested by the standard errors. Expenditures and revenues can also differ because of changes in debt obligations and cash reserves and also because capital expenditures are expensed in the year in which they occur.

More specifically, the means in Table 1 indicate that school districts allocated 65% of resources to instructional and capital expenditures on average. Given that districts have already covered most fixed costs, we might expect a larger-than-average share of the marginal aid dollars generated by the reforms to have been spent on these functions. This clearly appears to have been the case in Georgia. The results in Table 4 suggest that 89% of lottery-driven spending (i.e., [197 + 442]/714) was allocated to student instruction and capital expenditures. In contrast, only 51% of Tennessee's reform-driven spending (i.e., [136 + 53]/369) was directed toward these two functions. The relative effectiveness of Georgia's lottery in promoting these expenditures could plausibly reflect the fact that the lottery was linked to new and highly visible programs. By contrast, in Tennessee, the new state aid was combined with test-based accountability measures and class-size mandates that were not yet binding. The results in Table 4 indicate that these earmarking measures were relatively ineffective over the near term.

The remaining results in Table 4 indicate that a substantial amount of the new aid distributed by both states led to increased spending in functional areas that were not necessarily targeted. For example, in Tennessee, the new state aid created by court-ordered finance reforms led to increases in spending on various support services ($51 per pupil), on noninstructional functions ($73), and on functions unrelated to elementary and secondary education ($83 per pupil). Similarly, these results suggest that, in Georgia, the availability of new lottery-based state aid increased expenditures on noninstructional functions by $94 per pupil and on functions unrelated to elementary and secondary education by $49 per pupil. In assessing all these results, it is of course difficult to conclude whether the allocation of the new educational resources created by the state-specific finance reforms did or did not increase school quality. But what these results do suggest is that each state (particularly Tennessee) had some limited success in promoting targeted expenditures on instruction and capital improvements. However, some qualifications are appropriate. For example, the weakly significant increase in expenditures on support services in Tennessee is likely to reflect in part the costs of training teachers to use new high-technology classroom equipment.[28] Similarly, in Georgia, the operation of new pre-K programs should reasonably increase some noninstructional expenses (e.g., food services).[29] Nonetheless, a comparative assessment of these state-specific results suggests how efforts to earmark funds for specific purposes can be more successful. In particular, these results suggest that earmarking was relatively effective when linked to new and highly visible programs (e.g., pre-K programs in Georgia vs. 21st-century classrooms in Tennessee).

6. Conclusions

Improving the amount of educational resources available for student instruction, school infrastructure needs, and high-technology instructional equipment is a widely shared and relatively uncontroversial educational goal. However, the manner by which such resources should be raised is a contentious issue. Over the past 40 years, court challenges and the introduction of state lotteries have been two of the most widely adopted strategies for increasing state aid to local school districts. Unfortunately, there are several reasons that such reform-driven increases in state aid may not prove successful in promoting targeted educational functions. For example, the availability of new revenues

[28] However, Georgia's lottery-driven investments in high-technology equipment were actually accompanied by statistically significant decreases in expenditures on support services.

[29] However, the magnitude of the estimated increase in noninstructional expenses suggests that this is not the sole explanation. Recall from Table 1 that noninstructional expenditures average only 6% of the total. But the lottery-driven increase in noninstructional expenditures is 13% of the total increase in spending (i.e., 94/714).

in state or local budgets would not necessarily increase school spending if it were simply offset by corresponding reductions in the revenues raised from other sources. Furthermore, even if such reform efforts do lead to increased school spending at the local level, the new resources may not be allocated effectively. Such concerns have led to a variety of efforts to earmark state aid for targeted educational functions.

This study presented comparative empirical evidence on these policy-relevant issues by evaluating the effects of two recent education finance reforms (Georgia's new education lottery and Tennessee's court-ordered education finance reforms) on the district-level pattern of per pupil revenues by source and expenditures by function. The results of this study indicate that both reforms were effective at targeting increased state aid to the poorest districts within each state. Furthermore, these reform-driven increases in aid led to spending increases that were only somewhat attenuated by reductions in revenues from local and federal sources. The state of Tennessee attempted to target this new spending toward instructional and capital needs by simultaneously adopting a broad array of centralized educational reforms (e.g., test-based accountability measures, the phasing in of class-size regulations, and a high-profile initiative for technological investments in classrooms). Georgia attempted to target its new aid by promoting initiatives that were both new (e.g., pre-K programs for four-year-olds) and fairly visible (e.g., school construction and technological equipment). This study's results suggest that the reforms in both states were somewhat successful at promoting spending in these targeted areas. However, the comparative results also indicate that Georgia's lottery-based reforms were particularly successful. These comparisons suggest that state earmarking is particularly successful when linked to new and highly visible programs. It should also be noted that the relative success of Georgia's lottery at promoting targeted expenditures within the state's poorest districts also attenuated the regressivity typically associated with raising revenue through lottery sales. This particular combination of lottery-related results may provide motivation as well as guidance to reformers in other states who have been frustrated in their litigious efforts to increase state support to their poorer school districts.

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