Volume Title: When Public Sector Workers Unionize

Volume Author/Editor: Richard B. Freeman and Casey Ichniowski, eds.

Volume Publisher: University of Chicago Press

Volume ISBN: 0-226-26166-2

Volume URL: http://www.nber.org/books/free88-1

Publication Date: 1988

Chapter Title: On Estimating the Effects of Increased Aid to Education

Chapter Author: Ronald G. Ehrenberg, Richard Chaykowski

Chapter URL: http://www.nber.org/chapters/c7912

Chapter pages in book: (p. 245 - 270)

9 On Estimating the Effects of Increased Aid to Education

Ronald G. Ehrenberg and Richard P. Chaykowski

9.1 Introduction

The 1983 report, *A Nation at Risk,* of the National Commission on Excellence in Education decried the state of public education in the United States and suggested a number of reforms. Among their recommendations was increased federal aid for education. The view was that this would lead to desirable outcomes such as reduced class sizes and higher teacher salaries, with the latter aiding in the recruitment and retention of high-quality teachers.

Somewhat surprisingly, previous research on the economics of education provides us with very few insights about what the effects of such proposals might be. For example, while there is an extensive literature on the determinants of cross-section variations in teachers' salaries and teacher/student ratios, virtually nothing has been written on how changes in aid levels influence changes in salaries, teacher/student ratios, other expenditure levels, and local tax rates.[1] Similarly, while there are many studies of how grants-in-aid affect overall expenditure levels and some studies of the determinants of cross-section variations in the share of expenditures spent on various categories (e.g., instructional and administrative), virtually nothing has been written on how changes in aid affect the various expenditure shares.[2]

Ronald G. Ehrenberg is Irving M. Ives Professor of Industrial and Labor Relations and Economics, Cornell University, and research associate of the National Bureau of Economic Research. Richard P. Chaykowski is assistant professor, Department of Economics and School of Industrial Relations, Queen's University, Canada.

This research was partially supported by the NBER's Public Sector Labor Relations project. However, the authors alone are responsible for the contents of this paper. Without implicating him for what remains, the authors are extremely grateful to Richard Murnane for his detailed comments on an earlier draft.

To provide answers to some of these questions, our paper examines data from a panel of approximately 700 school districts in New York State over a five-year period (1978–79 to 1982–83) and tries to infer how school districts will respond to future changes in aid from how they responded to changes in state aid during the period. We focus on how past aid changes have influenced teacher salaries, tax rates, teacher/student ratios, and other staff/student ratios. The analyses exploit the fact that although school aid formulas change frequently in New York State, each district is usually guaranteed at least the same aid level as the previous year ("save harmless" provisions). As a result, over any given two-year period, the percentage increase in aid varies widely across districts. This provides a convenient form of natural experiment.

The organization of our paper is as follows. Section 9.2 discusses how state aid to education in New York State was allocated to school districts in 1978–79 and then how the allocation formulas changed during the sample period. Section 9.3 describes our methodological approach and the data base we have collected. Empirical results are presented in section 9.4, which is then followed by some brief concluding remarks.

9.2 State Aid to Education in New York State

By far the largest form of state aid to education in New York State[3] is general operating aid; this category ranged between 68 and 75 percent of total state aid during the sample years. Operating aid is based on an aid-ratio formulation in which low "wealth" districts receive more aid than high "wealth" districts. Specifically, the state establishes two expenditure/pupil levels, E_L and E_H, and then for average "wealth" districts pays 49 percent of district operating expenditures up to E_L and 20 percent of any district operating expenditures between E_L and E_H. For other districts, the share paid by the state of expenditures up to E_L is

$$r_{i1} = (1 - (W_i/\bar{W}) * .51), \tag{1}$$

and the share paid by the state of expenditures between E_L and E_H is

$$r_{i2} = (1 - (Y_i/\bar{Y}) * .8). \tag{2}$$

In these equations W_i and Y_i are measures of the district's wealth relative to the number of "aidable pupil units" in the district, while $\bar{W}$ and $\bar{Y}$ are comparable statewide average measures. "Aidable pupil units" depend upon average daily attendance in the district, with extra weight being given for secondary school pupils, handicapped pupils, and pupils with special needs (those who scored low on standardized tests). Throughout the sample period, W_i and $\bar{W}$ were always based on the full value of property, and Y_i and $\bar{Y}$ were similarly defined through 1980–81. However, in 1981–82 and 1982–83 the latter were redefined in terms of 1979 and 1980 community income, respectively (as reported from Internal Revenue Service records).

The level of general operating aid received by a district changes over time because of changes in E_L and E_H (these grew from $1,450 and $1,500 in 1978–79 to $1,885 and $2,155 by 1982–83, with the magnitude of the increase varying widely across years), changes in district wealth and school enrollment (both relative to statewide changes), and changes in the definition of wealth (in eq. [2]) in the latter two years.[4] In addition, "save harmless" provisions were always in effect. They stated that general operating aid could never be reduced, typically either on a total or per pupil basis, with the school district allowed to choose the option that was most beneficial to it. As a result school districts in which wealth was increasing rapidly or enrollments declining did not have to face a loss of general aid. These "save harmless" provisions substantially influenced the distribution of state aid; for example in 1979–80 over 35 percent (249) of the districts received aid under them.

In principle, the level of general operating aid received by a district might also increase simply because the district had increased its per pupil expenditures. That is, as long as the district was spending less than E_H, increases in expenditures (up to E_H) would induce increases in state aid through the matching formulas (eqs. [1] and [2]). In such a situation, it would not be meaningful to refer to an increase in aid "causing" the expenditure increase. However, the majority of districts appeared to have spent more than E_H each year, so this concern is probably not relevant for our data.

Although general operating aid is by far the largest component of state aid to education in New York, numerous other forms of aid exist. Some, like transportation aid (which comprises typically 10 percent of total aid), are based on formulas in which the state pays a specified share of mandated costs. Others, like construction aid, are based on a sharing of actual expenses. These latter categories, however, represent only a small share of total aid.

Of special interest to us are two other forms of aid that existed during at least part of the period. First, in 1979–80, prior to the inclusion of income as a measure of wealth in the general operating aid formula, the state instituted a "low-income aid" component of school aid; this provided between $1.76 (highest income district) and $27.50 (lowest income district) on a per pupil basis. This type of aid represented roughly 10 percent of the increase in state aid that occurred that year.

Second, each year the state provided special aid programs for "city" school districts. In most school districts in New York State the voters implicitly set the tax rate each year via budget referenda. In city districts, however, the tax rate is set by local school boards, subject to the rate not exceeding a state constitutionally determined maximum. The special aid programs for these city districts were designed to help out those districts that were near their constitutional maximum tax rate and to help other city school systems meet special needs that their districts faced.

Because of all of the above provisions and other types of aid to education, the percentage change in total aid varied widely across school districts in any given year. As mentioned above, this provides a convenient form of natural experiment in which we can try to infer how districts would respond to changes in federal aid in the future from observations on how they have responded to changes in state aid in the past. In much of what follows we shall treat these aid changes as exogenous; however, our discussion suggests that a district's percentage changes in full value of property, in enrollment, in level of income, and whether the district is a city school district, all influenced the district's percentage change in state aid in a highly nonlinear manner that both varied from year to year and depended upon whether a "save harmless" provision was an effective constraint on a district in a given year. As a consequence, in places below we have attempted to obtain instrumental variable estimates for changes in aid and have used these in our analyses.

9.3 Methodological Framework and Data

The outcomes we focus on are percentage changes in teachers' salaries—minimum salary with a bachelor's degree ($P1$), maximum with a bachelor's degree ($P2$), minimum with a master's degree ($P3$), and maximum with a master's degree ($P4$)—percentage changes in the property tax rate on the full value of property in the district ($P5$), and percentage changes in staff per pupil ratios—teacher/pupil ($P6$), nonprofessional staff/pupil ($P7$), other professional staff/pupil ($P8$), and paraprofessional staff/student ($P9$).[5] Given the initial level of these outcomes that prevail in a school district in a period, their percentage changes are determined by a complex process that involves bargaining between a school board and a union and, in the case of small districts in New York State, a voter referendum on the proposed school district budget. Rather than attempting to model this process formally, we pursue a strategy of estimating reduced-form equations of the form:

$$(3)\qquad P_i = F^i(Gg, Y, A, N, Z, CB, d, B_i), \qquad i = 1, 2, \ldots 9 .$$

Here G is the annual percentage change in state aid received by the school district; this is multiplied by the base-year share of state aid in the district's budget (g) to allow the effects of percentage changes in aid to depend upon aid's initial "importance" to the district. Y and A are the percentage changes in income and the full value of property in the district, respectively; they are measures of changes in the district's ability to pay for education.[6] N is the percentage change in student enrollment in the district. Z is a vector of sociodemographic variables expected to influence the community's "taste" for education and hence the various outcomes (e.g., community education level, student test scores, percent of households with children). CB is a vector of collective bargaining contract provisions (to be discussed below), while d is a dummy variable that indicates whether the school district is a "city" school district where tax rates are set by the school board (subject to constitutional tax limitations) rather than by the voters in an annual budget referendum. Finally, B_i, which differs for each outcome, is a measure of the base-period position of the district on a variable relating to the outcome.

A description of the specific variables included in each equation and their sources are found in the notes to table 9.1 As indicated there, we have collected data from a variety of sources for over 700 school districts in New York State for a five-year period (1978–79 to 1982–83). Since the outcome variables are expressed as changes, a maximum sample size of roughly 2,800 observations exists. However, due to missing data and problems, described below, actual sample sizes are much smaller.

The teacher salary variables were available only for school districts represented by the New York State United Teachers, an AFL–CIO affiliate, and hence districts represented by National Education Association (NEA) locals were excluded from the wage equations. These salary data had to be hand-coded from printed material, and many gaps in the data further reduced sample sizes. In addition, one might argue that since teacher contracts are often multiyear in nature, the process governing the outcomes may differ in years when collective bargaining negotiations took place. Consequently, results are presented below for both year-district observations when negotiations took place (table 9.1) and all observations (table 9.2) In the latter case, a dummy variable for whether negotiations took place in the year is included as a separate explanatory variable.[7]

Three collective bargaining contract provision variables are included in the analyses.[8] The first two capture the presence of employment-related provisions in the agreement, while the third represents a measure of bargaining strength. The first variable indicates the presence or absence of a provision governing staff reductions. The second variable indicates the presence or absence of a provision affecting the determination of class size. Together, the presence of these variables capture the extent to which the collective bargaining agreement constrains employer discretion over the determination of employment levels: staff reduction procedures affect the level of employment directly, while class size provisions affect employment levels indirectly. The third variable is a contract strength index that is meant to capture the relative bargaining power of the union. It is the sum of the number of times that other provisions that unions might want in a contract appear (with a maximum score of 58). Ceteris paribus, the higher the contract index the more bargaining power the union has and thus the more favorably the outcomes in equation (3) will be to the union.

The city school district dummy variable is included to test whether the way tax rates are set influences the growth rate of taxes and/or the other outcome variables. One's intuition is that when voters directly vote on tax increases that the rate of increase is likely to be lower.

The measure of the base period salary positions of teachers (B_i) is taken to be the initial category (BAmin, BAmax, MAmin, MAmax) salary in the district relative to the comparable category's salary average across districts in the county. For all other outcomes, the base period variable is taken to be the initial level of the variable. We expect that the phenomenon of "regression to the mean" will cause each of these base period variables to be negatively related to the percentage change in the outcome.

Finally, among the sociodemographic variables included in the analyses was an index of student test scores in the district relative to the statewide average during the 1979–80 to 1981–82 period. In each of the three years covered by this period, all third and sixth graders in New York State were given standardized math and reading tests. A "state reference point" was established for each test in each year; students who scored below this point were deemed to have special needs and to require remedial help. Letting $S(i, j, t)$ be the proportion of students in district i who scored below the state reference point on test j in year t and $S(A, j, t)$ the comparable proportion statewide for test j in year t, we define a district's relative test score index as

$$R_i = \sum_t \sum_j [S(i, j, t)/S(A, j, t)] , \tag{4}$$

where t = 79–80, 80–81, 81–82 and j = 3R,3M,6R,6M.

Low scores for R_i indicate districts whose students performed better on the standardized tests.

9.4 Empirical Results

Table 9.1 presents estimates of the change in outcome equations, with the sample restricted to year-observation combinations in which

Table 9.1 Change in Outcome Equations: Restricted to Observations Where Negotiations Took Place in Year (absolute value *t* statistic)

	P_1	P_2	P_3	P_4	P_5	P_6	P_7	P_8	P_9
C	0.070 (2.2)	0.082 (2.1)	0.043 (1.8)	0.216 (5.5)	−0.009 (0.2)	0.119 (3.5)	0.012 (0.3)	0.021 (0.3)	0.076 (0.7)
X1	−0.045 (1.7)								
X2		−0.034 (1.4)							
X3			−0.010 (0.6)						
X4				−0.102 (3.5)					
X5					−0.034 (5.1)				
X6						−2.003 (7.2)			
X7							−0.947 (2.5)		
X8								−21.616 (7.7)	
X9									−7.060 (3.6)
X10	0.001 (0.3)	0.008 (1.0)	−0.001 (0.3)	−0.000 (0.0)	−0.012 (1.3)	−0.010 (1.5)	0.012 (1.1)	−0.005 (0.2)	−0.017 (0.6)
X11	−0.040 (1.4)	−0.154 (3.0)	−0.053 (1.9)	−0.141 (3.0)	0.077 (1.3)	0.118 (2.5)	0.093 (1.3)	0.539 (3.2)	−0.277 (1.5)
X12	0.010 (0.4)	−0.072 (1.8)	0.003 (0.1)	−0.102 (3.0)	0.130 (2.7)	0.003 (0.1)	0.004 (0.1)	−0.194 (1.5)	−0.154 (1.1)
X13	0.000 (1.3)	0.000 (0.5)	0.000 (1.1)	0.001 (2.1)	0.000 (0.2)	0.000 (0.4)	−0.001 (1.9)	0.005 (2.5)	0.006 (2.9)
X14	0.011 (2.0)	0.007 (0.7)	0.014 (2.6)	−0.002 (0.3)	−0.027 (2.2)	−0.006 (0.6)	−0.020 (1.4)	0.001 (0.0)	0.006 (0.2)
X15	0.046 (0.6)	0.210 (1.5)	0.041 (0.6)	−0.103 (0.9)	0.228 (1.3)	0.201 (1.5)	0.377 (1.8)	−0.769 (1.6)	0.299 (0.6)
X16	0.014 (3.9)	0.044 (7.2)	0.015 (4.3)	0.021 (3.7)	0.037 (5.4)	−0.006 (1.1)	−0.000 (0.0)	0.007 (0.4)	−0.071 (3.4)
X17	0.020 (5.2)	0.035 (5.9)	0.019 (5.3)	0.033 (5.8)	0.012 (1.8)	−0.012 (2.1)	−0.017 (1.9)	0.008 (0.4)	−0.074 (3.3)
X18	0.017 (3.5)	0.085(10.4)	0.017 (3.7)	0.057 (7.5)	−0.005 (0.5)	0.003 (0.5)	0.016 (1.3)	−0.020 (0.7)	−0.010 (0.3)
X19	−0.001 (0.5)	−0.003 (0.7)	−0.001 (0.2)	0.004 (1.1)	0.009 (1.8)	0.002 (0.5)	−0.006 (0.9)	−0.005 (0.3)	−0.012 (0.8)
X20	−0.001 (0.5)	−0.000 (0.1)	−0.004 (1.5)	0.003 (0.9)	−0.004 (0.9)	−0.001 (0.4)	0.005 (0.7)	0.005 (0.3)	0.007 (0.4)
X21	0.000 (0.7)	−0.000 (0.1)	−0.000 (0.9)	−0.000 (0.7)	0.000 (0.4)	−0.000 (0.9)	0.000 (0.0)	0.000 (0.4)	−0.001 (0.6)
X22	0.004 (0.7)	0.004 (0.4)	0.005 (0.9)	0.011 (1.2)	0.019 (1.7)	−0.001 (0.5)	0.009 (0.7)	0.015 (0.4)	0.033 (1.0)
X23	−0.014 (0.3)	0.106 (1.4)	−0.033 (0.8)	0.087 (1.2)	−0.145 (1.8)	−0.693(10.4)	−0.776 (7.7)	−0.737 (3.1)	−0.494 (2.0)
X24	−0.051 (1.4)	−0.052 (0.8)	−0.039 (1.1)	−0.069 (1.1)	−0.145 (2.0)	−0.115 (2.0)	−0.116 (1.3)	0.415 (1.9)	−0.165 (0.8)
X25	0.025 (1.0)	0.028 (0.6)	0.021 (0.9)	−0.009 (0.2)	−0.820(14.3)	−0.011 (0.3)	0.058 (0.9)	0.124 (0.9)	0.278 (1.8)
$\bar{R}^2/N$	0.068/424	0.228/598	0.070/412	0.271/419	0.314/718	0.180/800	0.090/751	0.096/734	0.058/675

Table 9.1 (continued)

Sources: New York State Education Department, "Basic Educational Data System" (BEDS), school district tapes for 1978–79, 1979–80, 1980–81, 1981–82, and 1982–83 (P_6,P_7,P_8,P_9,$X6$,$X7$,$X8$,$X9$,$X23$).

New York State Education Department, "Financial Data System" (ST3), school district tapes for 1978–79, 1979–80, 1980–81, 1981–82, and 1982–83 (P_5,$X5$,$X14$,$X24$,$X25$).

New York State Education Department, "New York State Pupil Evaluation Program" (PEP), test scores for 1978–79, 1979–80, 1980–81, 1981–82, and 1982–83 ($X22$).

New York State United Teachers, *Salary Schedule Rankings*, 1978–79, 1980–81, 1981–82; *Salary Schedules*, 1979–80; unpublished computer printouts, 1982–83 (P_1,P_2,P_3,P_4,$X1$,$X2$,$X3$,$X4$,n). Information on the latter variable also came from contracts on file in the Labor-Management Documentation Center at the New York State School of Industrial and Labor Relations.

U.S. Bureau of the Census, *1980 Census of Population*, school district data file for New York State ($X10$,$X11$,$X12$,$X13$).

U.S. Department of Commerce, Bureau of Economic Analysis, unpublished tabulations, 1978, 1979, 1980, 1981, 1982 ($X15$).

New York State United Teachers, "Contract Provisions," tape for 1976–77 ($X21$).

Notes: P_1 = percentage change in the bachelor's-level minimum salary.
P_2 = percentage change in the bachelor's-level maximum salary.
P_3 = percentage change in the master's-level minimum salary.
P_4 = percentage change in the master's-level maximum salary.
P_5 = percentage change in the school district's property tax rate on full value.
P_6 = percentage change in the teacher/student ratio.
P_7 = percentage change in the nonprofessional/student ratio (nonprofessionals include secretaries, maintenance, bus drivers, school lunch workers).
P_8 = percentage change in the other professional staff/student ratio (other professionals include administrators, psychologists, guidance counselors, librarians).
P_9 = percentage change in the paraprofessional/student ratio (paraprofessionals include teaching assistants, teacher aides, pupil personnel service aides, library aides, health aides).

$X1$, $X2$, $X3$, $X4$ = district salary level/average salary level in the country: $X1$ (bachelor's minimum), $X2$ (bachelor's maximum), $X3$ (master's minimum), $X4$ (master's maximum).

$X5$, $X6$, $X7$, $X8$, $X9$ = level at the start of the period of the outcome variable: $X5$ (school district's tax on full value), $X6$ (teacher/student ratio), $X7$ (nonprofessional/student ratio), $X8$ (other professional staff/student ratio), $X9$ (paraprofessional/student ratio).

$X10$ = percent urban residents of the school district in 1979.

$X11$ = percent nonwhite residents of the school district in 1979.

$X12$ = percent of households in the school district with children at home in 1979.

$X13$ = median family income in the school district in 1979 (in thousands).

$X14$ = 1 = city school district where school board sets tax rate; 0 = voters vote on tax rate in annual referendum.

$X15$ = percentage change in per capita personal income in the county between the calendar years.

$X16$ = 1 = 1980–81 academic year; 0 = otherwise
$X17$ = 1 = 1981–82 academic year; 0 = otherwise
$X18$ = 1 = 1982–83 academic year; 0 = otherwise
} 1979–80 is the reference year.

$X19$ = 1 = class size provision is in the teachers' contract; 0 = no provision.

$X20$ = 1 = reduction-in-force provision is in the teachers' contract; 0 = no provision.

$X21$ = index of number of provisions present in the teachers' contract (mean = 20, variance = 24 in the sample).

$X22$ = index of student test scores in the district relative to statewide average during the 1979–80 to 1981–82 period (1 = mean, low index equals higher test scores; see text).

$X23$ = percentage change in enrollment in the district.

$X24$ = percentage change in state aid received by the district multiplied by the initial share of state aid in the district's budget.

$X25$ = percentage change in the full value of property in the school district.

n = 1 = negotiations with teachers over salary took place in the year; 0 = no negotiations.

contract negotiations took place. Table 9.2 presents similar estimates without this restriction on the sample; not surprisingly, sample sizes in the latter table are typically twice as large (many teachers' contracts in New York State were for two years during the period). The estimates in the latter table do contain a dummy variable (n) for whether negotiations took place in the year, and both tables also include year dummy variables to control for omitted year-specific factors like the average growth of wages and prices in the state. The inclusion of these year dummies is not an innocuous modification; in principle they may capture the effects of year-to-year variations in the average rate of change of other included variables, leaving the coefficients of these variables (e.g., state aid) to capture only within-year variations of rates of change across districts. However, as we shall indicate later, excluding the year dummies rarely significantly altered the coefficients of other variables of interest (e.g., state aid).

The results in tables 9.1 and 9.2 are not particularly impressive; statistically insignificant coefficients predominate and the explanatory power of the models is not high. Nonetheless, there are a few results worth noting. First, we are much more successful in explaining salary increases for experienced teachers than we are in explaining salary increases for newly hired teachers. This is not surprising as during the period enrollments were not expanding and there was little hiring of new teachers. As such, teachers' unions paid relatively little attention to starting salaries, and in a number of cases no increases were given at that level.

Second, at the maximum salary level there did tend to be a policy of "regression to the mean," in the sense that the higher a district's salary relative to the county average, the smaller the district's increase would be. This effect was quite small though; a district whose salary level was 10 percent above the county average would moderate its increase by no more than 1 percent. Similar results hold for the tax rate outcome; districts with high initial tax rates have smaller rates of growth of tax rates, ceteris paribus.[9]

Third, the growth of "ability to pay" measures, such as county income ($X15$) and the full value of property in a district ($X25$), appeared to rarely influence the outcome variables. The only exception was the district's tax rate on full value ($P5$); indeed one cannot reject the hypothesis that a 10 percent increase in property values was associated with an equivalent percent decrease in the tax rate.

Fourth, contrary to our expectations, city school districts ($X14$) in which the school board sets the tax rate tended to have lower rates of tax rate increase than districts in which voters approved the tax rate at an annual budget referendum. Quantitatively, however, this effect was very small, less than 0.03 percent a year.

Table 9.2 **Change in Outcome Equations: All Observations[a] (absolute value t statistic)**

	P_1	P_2	P_3	P_4	P_5	P_6	P_7	P_8	P_9
C	0.036 (1.8)	0.030 (1.2)	0.052 (3.1)	0.142 (5.8)	0.019 (0.8)	0.121 (5.1)	0.055 (1.8)	0.227 (3.1)	−0.010 (0.1)
n	−0.003 (2.6)	−0.002 (0.8)	−0.004 (2.8)	−0.006 (2.5)	−0.001 (0.2)	−0.002 (0.7)	−0.008 (2.0)	0.008 (0.9)	−0.011 (1.2)
$X1$	0.013 (0.8)								
$X2$		0.009 (0.5)							
$X3$			−0.005 (0.4)						
$X4$				−0.061 (3.4)					
$X5$					−0.027 (6.2)				
$X6$						−1.985(10.0)			
$X7$							−1.500 (6.1)		
$X8$								−21.990(11.4)	
$X9$									−10.009 (7.8)
$X10$	0.000 (0.1)	0.010 (2.1)	0.002 (0.7)	0.004 (1.0)	−0.011 (1.8)	−0.010 (2.2)	0.002 (0.2)	0.002 (0.1)	−0.037 (2.2)
$X11$	−0.011 (0.6)	−0.044 (1.3)	−0.026 (1.4)	−0.053 (1.9)	0.008 (0.8)	0.098 (3.0)	0.026 (0.5)	0.243 (2.1)	0.104 (0.8)
$X12$	−0.001 (0.0)	−0.041 (1.6)	−0.005 (0.4)	−0.031 (1.4)	0.104 (3.2)	0.005 (0.2)	0.006 (0.1)	−0.272 (3.0)	−0.047 (0.5)
$X13$	0.000 (1.4)	0.000 (0.4)	0.000 (1.3)	0.000 (0.4)	−0.000 (0.5)	0.000 (0.0)	0.000 (1.0)	0.005 (3.7)	0.006 (4.4)
$X14$	0.003 (0.9)	−0.002 (0.4)	0.005 (1.3)	0.001 (0.1)	−0.017 (2.0)	−0.007 (1.1)	−0.003 (0.3)	−0.022 (1.0)	−0.017 (0.7)
$X15$	−0.069 (1.3)	0.088 (0.9)	−0.011 (0.2)	−0.095 (1.2)	0.250 (2.2)	0.083 (0.9)	0.364 (2.5)	−0.040 (0.1)	0.313 (1.0)
$X16$	0.009 (4.2)	0.028 (7.0)	0.009 (3.6)	0.014 (4.3)	0.043 (9.3)	0.000 (0.0)	−0.000 (1.7)	0.023 (1.7)	−0.067 (4.7)
$X17$	0.018 (7.8)	0.027 (6.8)	0.018 (7.5)	0.032 (8.7)	0.014 (2.7)	−0.004 (1.1)	−0.026 (0.4)	−0.001 (0.1)	−0.063 (4.0)
$X18$	0.014 (5.2)	0.076(14.8)	0.017 (5.7)	0.065(14.7)	−0.001 (0.3)	0.008 (1.5)	0.003 (0.5)	0.000 (0.0)	−0.017 (0.8)
$X19$	0.001 (0.8)	0.000 (0.1)	0.001 (0.6)	0.004 (1.8)	0.004 (1.2)	0.003 (1.2)	0.002 (0.3)	0.002 (0.2)	0.007 (0.6)
$X20$	0.002 (1.0)	0.003 (1.2)	0.002 (1.2)	0.002 (1.0)	−0.003 (1.0)	−0.004 (1.2)	0.005 (1.0)	0.007 (0.7)	0.004 (0.4)
$X21$	−0.000 (1.5)	0.000 (0.2)	−0.000 (2.0)	−0.000 (0.9)	0.000 (0.0)	−0.001 (1.8)	−0.001 (2.9)	0.000 (0.3)	−0.000 (0.1)
$X22$	0.006 (1.5)	0.006 (0.9)	0.006 (1.6)	0.004 (0.8)	0.011 (1.5)	0.006 (0.9)	−0.001 (0.1)	0.018 (0.8)	0.024 (1.0)
$X23$	0.014 (0.5)	0.059 (1.3)	0.016 (0.6)	0.033 (0.8)	−0.029 (0.5)	−0.717(15.3)	−0.792(11.5)	−0.436 (2.8)	−0.723 (4.3)
$X24$	−0.024 (1.1)	−0.049 (1.2)	−0.007 (0.3)	−0.028 (0.7)	−0.081 (1.5)	−0.062 (1.5)	0.004 (0.1)	0.018 (0.1)	0.154 (1.0)
$X25$	−0.001 (0.1)	0.032 (1.1)	0.000 (0.0)	−0.015 (0.6)	−0.860(21.5)	−0.016 (0.6)	0.017 (0.3)	−0.070 (0.7)	0.088 (0.8)
$\bar{R}^2/N$	0.095/922	0.239/1,205	0.100/893	0.358/904	0.321/1,504	0.168/1,664	0.111/1,553	0.084/1,509	0.081/1,390

[a]See notes to table 9.1 for variable definitions.

Fifth, the results in table 9.2 suggest that teacher salary increases were actually marginally lower in the years that contracts were negotiated than they were in second and third years of existing contracts. This may reflect the well-known preferences of management to "backload" salary increases in multiyear contracts to reduce the present value of the cost.

Finally, we turn to the effects of changes in state aid to education—the primary focus of our study. Table 9.3 summarizes the aid coefficients from tables 9.1 and 9.2 as well as comparable coefficients that came from models that excluded the year dummy variables and, in the case of the "whole sample results" the negotiations dummy. Exclusion of the year dummies allow us to test if their presence captures some of the effects of across-year variations in increases in aid to education.

Rows A1, A2 and B1 to B4 of table 9.3 report estimates of the effects of changes in state aid when such changes are treated as exogenous. Although these coefficients are often insignificant and/or differ across specifications, some tentative conclusions can be drawn. Changes in state aid levels did *not* appear to influence teacher salary increases (*P*1, *P*2, *P*3, *P*4). There is some evidence in the specifications that include the year dummy variables that they negatively affected tax rate growth; however, this relationship was far from one to one. Unexpectedly, in some specifications increases in aid were associated with *decreases* in teacher/student ratios, but *increases* in other professional/student ratios.

Of course, as noted in section 9.2, changes in state aid to school districts in New York State were not truly exogenous during the 1978–79 to 1982–83 period. Rather they depended each year on (among other things) changes in the district's full value of property and enrollment, changes in the district's income level (after 1980–81), whether the district was a city school district, and "save harmless" provisions, with the effect of each of these variables often varying across years. Treating aid changes as being exogenous may distort our estimates, given these facts.

To address this problem, table 9.4 reports estimates of two simple specifications used by us to analyze the determinants of the percentage change in state aid. In the first, percentage changes in aid are regressed on year dummy variables and these variables interacted with the city school district dummy, the percentage change in assessed value, the percentage change in enrollment, and the 1979 income level in the community. The second attempts to approximate the existence of "save harmless" provisions by adding a dummy variable if the percentage change in enrollment in the year is negative and interacting one minus that variable with the changes in full value of district property and enrollment, and with income.[10]

Table 9.3 **Effects of State Aid Changes on Outcomes: Various Specifications[a] (absolute value *t* statistics)**

Specification [number of observations]	*P*1 [425]	*P*2 [599]	*P*3 [413]	*P*4 [420]	*P*5 [728]	*P*6 [801]	*P*7 [752]	*P*8 [735]	*P*9 [676]
A) Contracts Negotiated in the Year									
A1) Include Year Dummies	−0.051	−0.052	−0.039	−0.069	−0.145	−0.115	−0.116	0.415	−0.165
	(1.4)	(0.8)	(1.1)	(1.2)	(2.0)	(2.0)	(1.2)	(1.9)	(0.8)
A2) Exclude Year Dummies	−0.057	−0.022	−0.040	−0.113	−0.081	−0.123	−0.108	0.428	−0.241
	(1.5)	(0.3)	(1.1)	(1.8)	(1.0)	(2.2)	(1.2)	(2.0)	(1.1)
A3) Include Year Dummies (IV1)	0.066	0.071	0.119	0.007	−0.122	−0.951	−0.490	−0.156	−2.216
	(0.5)	(0.3)	(1.0)	(0.0)	(0.5)	(4.3)	(1.5)	(0.2)	(2.9)
A4) Include Year Dummies (IV2)	0.279	0.014	0.138	0.061	0.189	−0.762	−0.293	−0.474	−1.492
	(2.6)	(0.1)	(1.4)	(0.4)	(0.9)	(4.5)	(1.1)	(0.7)	(2.5)
	[923]	[1,206]	[894]	[905]	[1,504]	[1,665]	[1,554]	[151]	[1,391]
B) All Years' Data									
B1) Include Year and Negotiations Dummies	−0.024	−0.049	−0.007	−0.028	−0.081	−0.062	0.004	0.018	0.154
	(1.1)	(1.2)	(0.3)	(0.7)	(1.5)	(1.5)	(0.1)	(0.1)	(1.0)
B2) Exclude Negotiations Dummies	−0.019	−0.017	−0.005	−0.045	0.019	−0.060	−0.013	0.073	0.046
	(0.8)	(0.4)	(0.2)	(1.1)	(0.3)	(1.5)	(0.2)	(0.5)	(0.3)
B3) Exclude Year Dummies	−0.034	−0.030	−0.029	−0.023	−0.103	0.050	−0.034	0.090	0.085
	(1.6)	(0.7)	(1.3)	(0.7)	(2.1)	(1.3)	(0.5)	(0.6)	(0.6)
B4) Neither Included	−0.026	−0.004	−0.024	−0.038	−0.006	−0.049	−0.048	0.135	−0.009
	(1.1)	(0.1)	(1.0)	(1.0)	(0.1)	(1.3)	(0.8)	(0.9)	(0.9)

[a]Coefficients come from equations in tables 9.1 and 9.2 and similar equations which included the indicated dummy variables. To derive elasticities with respect to state aid, these coefficients should be divided by the share of state aid in the initial budget—approximately 40 percent in the sample—where outcomes are the percentage changes in:

*P*1 = minimum salary with bachelor's degree.
*P*2 = maximum salary with bachelor's degree.
*P*3 = minimum salary with master's degree.
*P*4 = maximum salary with master's degree.
*P*5 = school district property tax rate on full value.
*P*6 = teacher/student ratio.
*P*7 = nonprofessional/student ratio.
*P*8 = other professional/student ratio.
*P*9 = paraprofessional/student ratio.
IV1(IV2) = instrument for percentage change in aid obtained from columns (1) and (2) of table 9.4.

Table 9.4 **Annual Percentage Change in State Aid Equations: Observations When Negotiations Took Place (absolute value of t statistics)**

Explanatory Variables	(1)	Explanatory Variables	(2)
C	0.056 (5.6)	*C*	0.066 (3.3)
Y80	0.048 (2.6)	Y80	0.038 (2.7)
Y81	0.047 (2.5)	Y81	0.010 (1.7)
Y82	0.076 (3.9)	Y82	0.018 (3.0)
*X*14*Y79	0.050 (3.5)	*X*14*Y79	0.052 (3.7)
*X*14*Y80	0.039 (2.6)	*X*14*Y80	0.047 (3.3)
*X*14*Y81	−0.007 (0.4)	*X*14*Y81	−0.007 (0.5)
*X*14*Y82	−0.009 (0.5)	*X*14*Y82	0.008 (0.5)
*X*25*Y79	0.004 (0.1)	*X*25*Y79*(1 − *D*)	−0.141 (0.5)
*X*25*Y80	−0.097 (1.4)	*X*25*Y80*(1 − *D*)	−0.203 (0.6)
*X*25*Y81	−0.169 (2.0)	*X*25*Y81*(1 − *D*)	−1.400 (4.8)
*X*25*Y82	−0.087 (1.0)	*X*25*Y82*(1 − *D*)	−0.053 (1.4)
*X*23*Y79	0.270 (2.3)	*X*23*Y79*(1 − *D*)	−0.126 (0.2)
*X*23*Y80	0.197 (2.0)	*X*23*Y80*(1 − *D*)	0.498 (2.0)
*X*23*Y81	0.530 (4.1)	*X*23*Y81*(1 − *D*)	0.321 (0.3)
*X*23*Y82	0.198 (1.5)	*X*23*Y82*(1 − *D*)	0.885 (1.9)
*X*13*Y80	−0.001 (1.2)	*X*13*Y80	−0.001 (0.1)
*X*13*Y81	−0.001 (0.8)	*X*13*Y81*(1 − *D*)	0.011 (5.2)
	—	*D*	−0.020 (1.0)
$\bar{R}^2/N$	0.083/1,001		0.108/1,001

Note: C = intercept term.
Y_j = 1 = change from academic year 19j − 1 to 19j to academic year 19j to 19j + 1; 0 = other.
D = 1 = percentage change in enrollment was negative, 0 = not negative.
X13, X14, X23, X25 are as defined before.

The estimates in table 9.4 can then be used to obtain instruments (IV1 and IV2) for the percentage change in state aid in each district, these estimated values interacted with the initial share of state aid in the district's budget, and then the outcome equations reestimated using the instruments rather than the actual values of percentage changes in state aid. Rows A3 and A4 of table 9.3 contain the estimated state aid coefficients from equations estimated for the "negotiations only" sample when year dummies were included, along with the instruments for percentage change in state aid. Unfortunately, these results do differ substantially from the OLS ones. While in the preferred specification (IV2, row A4) there is evidence that higher state aid was associated with higher bachelor's level entry salaries, we no longer observe increased state aid displacing increases in the tax rate. Moreover, increases in state aid now appear to be negatively associated with *both* teacher/student and paraprofessional/student ratios. The magnitude of the latter relationship is sufficiently large to make one question what we have found.[11]

9.5 Concluding Remarks

Sometimes the best laid plans of mice and men (and economists) go astray. To be blunt, the results presented above shed very little light on the effects of changes in state aid and other financial variables on teacher salaries, tax rates, and staff/student ratios. While there was some evidence that aid increases moderate tax rate increases, this relationship was not sufficiently robust across different model specifications to draw firm conclusions.

There are many possible explanations for our failure to find systematic significant effects of financial variables. There were *many* errors in the underlying financial data we used, which we tried to correct but may not have always succeeded. School district residents' income was unavailable, save for 1979, so instead we had to use county income as a proxy. Parts of aid increases were often tied to specific uses (e.g., textbooks, transportation, special needs), and our use of total state aid rather than general operating aid may have distorted our findings.

More importantly, it is possible that school districts did not make major allocation decisions based on changes in financial variables that were thought to be uncertain to persist in the future. For example, granting teachers a large salary increase in a year of generous aid increases would come back to haunt a district if aid remained constant or fell in the next year. As such, we may well have performed the wrong conceptual experiment. Our quasi-experimental design may not have been appropriate to capture what the effects would be of a "permanent" federal program that mandated increased aid to education.

In fact, while year-to-year average percentage increases in state aid varied during the sample period, the average share of state aid in total school district revenue remained roughly constant at 40 percent.[12] Changes in state aid formulas during the period *did* serve to redistribute state aid across school districts each year, however the aid changes were so weakly correlated across school districts over time that for all practical purposes above, or below, average increases were probably treated as random events.[13] As such, they probably affected things like the timing of capital expenditures and the issuance and retirement of debt much more than they did the outcomes on which we have focused in this paper.

Lest we appear too pessimistic, however, we hasten to stress that our paper has found some results, primarily relating to more "institutional" variables, that are of interest. For example, teacher salary increases tended to be smaller in years when contracts were negotiated;

school districts seemed to be successful in "backloading" wage increases in multiyear contracts. Or to take another example, city school districts, where elected school boards set the tax rate, appeared to have *lower* tax rate increases than other districts. In addition, we observed a regression to the mean phenomenon as school districts whose teachers' salaries were above the county average tended to have lower rates of salary increase. The latter result is exactly what one would expect to see with a system of impasse resolution (mediation and fact finding), where neutrals place a weight on comparability in making decisions.[14] While we attempted to obtain data on whether each negotiation went to impasse in order to test whether comparability considerations mattered primarily in negotiations that went to impasse, unfortunately such data were not available.

Finally, it is interesting to note that state legislatures have begun to realize what we have found econometrically; namely, that past state aid increases to school districts (at least in New York State) have not been used to increase teachers' salaries. As a result, recently enacted laws in a number of states have provided increased state aid earmarked for specified higher minimum salaries for teachers (e.g., New Jersey) or increased aid for general teachers' salaries, with school districts and teachers' unions to negotiate how these funds are to be allocated (e.g., New York).[15] Whether these targeted programs actually serve to increase teachers' salaries above the level that would have existed in their absence, especially in the long run, is a subject worthy of future empirical investigation.

Notes

1. See Ehrenberg and Schwarz (1987) and Lipsky (1982) for surveys of the teacher compensation literature.

2. Examples of studies on the effects of grants on public school expenditure levels are Denzau (1975) and Feldstein (1975). Studies on determinants of cross-section variations in expenditure shares include Carroll (1976) and Monk (1984).

3. Much of the discussion in this section is derived from our reading of New York State Education Department (1978, 1980) and New York State Division of the Budget (1979, 1981, 1982). We are grateful to David Monk for calling this material to our attention.

4. The following table, which lists the values of E_L and E_H each year and their percentage changes from the previous year (in parentheses), illustrates the variability in the percentage changes over time.

	1978–79	*1979–80*		*1980–81*		*1981–82*		*1982–83*	
E_L	\$1.450	\$1.500	(3.4%)	\$1.600	(6.7%)	\$1.650	(3.1%)	\$1.885	(14.2%)
E_H	\$1.500	\$1.550	(3.3%)	\$1.700	(9.7%)	\$1.885	(10.9%)	\$2.155	(14.3%)

5. Nonprofessional staff includes secretaries, maintenance workers, bus drivers, and school lunch workers. Other professional staff include administrators, psychologists, guidance counselors, and librarians. Paraprofessionals include teaching assistants, teacher aides, pupil personnel service aides, library aides, and health aides.

6. In actuality, annual income data were available only at the county level.

7. Whether a contract negotiation took place in a year was obtained from information in the wage data sources and a search of teachers' contracts on file in the data archives of the Labor-Management Documentation Center of the New York State School of Industrial and Labor Relations.

8. These collective bargaining provision variables were available on tape only for the 1975–76 academic year and thus represent the strength of the contract prior to the first year in the sample. We are grateful to Randall Eberts of the University of Oregon for providing these data. They have previously been used in Eberts and Stone (1986).

9. The large negative coefficients of the initial level of the outcome variable in columns *P*6 to *P*9 are undoubtedly biased in a negative direction since in each case were are regressing the log $(Ot/Ot - 1)$ on $Ot - 1$ (where O represents an outcome).

10. In specification 1 district income was *not* interacted with the 1979–80 year dummy because income did *not* enter the aid allocation formula until the next year. Similarly, in specification 2 the interaction of income and the 1980–81 year dummy was not further interacted with D because the "save harmless" provisions did *not* apply to aid based on district income in 1980–81. Deflating median family income by the proportion of households with children at home (to approximate a persistent income measure) did not appreciably alter any of the estimates. Income for 1979 in the district was used throughout because it was the only year that district (as opposed to county) income data were available.

11. One further extension warrants being briefly reported here. During the period covered by the study, enrollment was declining in many school districts in New York State; indeed in roughly 80 percent of the district/year observations in our sample enrollment fell. Discussants of previous drafts of our paper have suggested that the response of school districts to changes in state aid may differ between increasing and decreasing enrollment districts. (See Calvin, Murnane, and Brown 1985 for evidence that in Michigan the response of school district expenditure levels to enrollment changes differed between increasing and decreasing enrollment districts.)

To test for this possibility, the equations that underlie rows B1 and B3 of table 9.3 were reestimated for both increasing enrollment and decreasing enrollment subsamples of school district–year observations. The patterns of results observed were very similar to those reported in table 9.3 with the vast majority of coefficients again proving statistically insignificant. The absolute magnitudes of the coefficients tended to be larger for the increasing enrollment districts, but these differences across enrollment change types were all statistically insignificant.

12. More precisely the shares of state aid in total school district revenue in New York State (excluding New York City) were 39.9, 39.9, 39.8, and 40.4, respectively, for 1979–80, 1980–81, 1981–82, and 1982–83. See New York State Office of the Comptroller (1985, table C).

13. For the roughly 700 school districts in our sample, the computed correlations of total and per student state aid changes (from the previous academic year) across school districts over time were as follows:

	Total Aid				*Per Student Aid*		
1980–81	0.221			1980–81	0.052		
1981–82	0.151	−0.202		1981–82	0.175	−0.096	
1982–83	−0.011	0.060	−0.102	1982–83	−0.022	−0.048	−0.053
	1979–80	1980–81	1981–82		1979–80	1980–81	1981–82

14. Previous studies of actual public sector labor markets have found that when *arbitration* statutes are present area wage differentials tend to be compressed. See Ehrenberg and Schwarz (1987) for citations to these studies.

15. For brief discussions of the New York and New Jersey programs, see David Dunlap (1986) and Alfonso Narvaez (1985), respectively. These programs raise a host of other issues including their effects on mandated and bargained fringe benefit costs, on the seniority structure of teachers' salaries, and on collective bargaining in education, per se, which we do not discuss here.

References

Calvin, Edward, Richard Murnane, and Randall Brown. 1985. School district responses to enrollment changes: The direction of change matters. *Journal of Education Finance* 10:426–40.

Carroll, Stephen. 1976. School district expenditure behavior. *Journal of Human Resources* 11:317–27.

Denzau, Arthur. 1975. An empirical survey of studies in public school spending. *National Tax Journal* 28:241–48.

Dunlop, David. 1986. City may reject state aid for teachers' raises. *New York Times* 135 (12 April):31.

Eberts, Randall, and Joe Stone. 1986. On the contract curve. *Journal of Labor Economics* 4:66–81.

Ehrenberg, Ronald, and Joshua Schwarz. 1987. Public sector labor markets. In *Handbook of labor economics*, ed. Orley Ashenfelter and Richard Layard. Amsterdam: North-Holland.

Feldstein, Martin. 1975. Wealth neutrality and local choice in public education. *American Economic Review* 65:75–89.

Lipsky, David. 1982. The effect of collective bargaining on teacher pay: A review of the evidence. *Educational Administration Quarterly* 18:14–42.

Monk, David. 1984. The conception of size and the internal allocation of school district resources. *Educational Administration Quarterly* 20:39–67.

Narvaez, Alfonso. 1985. Minimum teacher pay set at $18,500 in Jersey. *New York Times* 134 (10 September):B2.

New York State Division of the Budget. 1979. Description of New York State public school aid provisions—1979–80 school year. Albany, New York.

———. 1981. Description of the 1981–82 New York State aid programs relating to state support for public schools. Albany, New York.

———. 1982. Description of 1982–83 New York State aid programs relating to state support for public schools. Albany, New York.

New York State Education Department. 1978. *Understanding financial support of public schools, 1978–79*. Albany, New York.

———. 1980. 1980–81 state aid to school districts. Albany, New York.

New York State Office of the Comptroller. 1985. *Financial data for school districts for fiscal year ended June 30, 1984*. Albany, New York.

Comment Richard J. Murnane

The Ehrenberg-Chaykowski (henceforth, E-C) paper examines the effects that changes in state aid to local school districts have on teachers' salaries, local property tax rates, teacher/student ratios, and other staff/student ratios. Their data consist of information on a panel of 700 school districts in New York State over the five-year period from the 1978–79 school year through the 1982–83 school year. The school district is the unit of analysis in their empirical work. Their methodology exploits the fact that the percentage increase in state aid going to individual school districts varied widely over the five-year period.

E-C's analysis strategy is to estimate nine reduced-form models. The dependent variables in these models include the percentage change in a school district's minimum salary, the percentage change in the district's maximum teacher's salary, the percentage change in the property tax rate, and the percentage changes in a number of staff-to-student ratios. The explanatory variables include a measure of the percentage change in state aid, demographic characteristics of each school district in the base year (1979), information about the teachers' contract (such as whether it contains a reduction-in-force clause), and 1979 values for the outcome variable (included to account for the possibility that the percentage change in the outcome variable may depend on the initial value). The models are estimated both with ordinary least squares and, for reasons that I explain below, with instrumental variables.

I will briefly summarize the state aid results and then discuss these and other results in more detail.

1. Changes in state aid levels did not influence teacher salary increases.
2. State aid increases did have a negative impact on property tax rates.
3. State aid increases were positively associated with increases in the ratio of "other professionals" (not teachers) to students, but were associated with *decreases* in teacher-student ratios. (In other words, more aid leads to larger class sizes, a result even more puzzling than the Kleiner-Petree (chap. 11, this volume) finding that unionization leads to larger classes.)

When I read an earlier version of E-C's paper six months ago, I was somewhat surprised by the lack of findings. Since then two colleagues and I have been doing similar work using longitudinal data on Michigan school districts, and now I do not find the results so surprising (cf. Murnane, Singer, and Willett 1986). In fact, in many respects our results are extremely similar to E-C's.

Richard J. Murnane, an economist, is a professor at the Harvard Graduate School of Education.

I want to start my comments by focusing on three common findings.

E-C found that increases in state aid are not associated with increases in teacher salaries in New York State school districts. I found the same thing to be true in Michigan districts. The Michigan finding is not surprising, however, given prior research indicating that local school districts in that state responded to state aid increases by reducing local property tax rates and keeping per pupil expenditures roughly constant (Carroll 1982). In other words, when state aid increased, local property tax rates decreased, but local school superintendents did not get larger school budgets. Given that school budgets did not increase when state aid increased, it is not surprising that teacher salaries did not increase.

Can E-C's finding that teachers' salaries in New York State school districts did not increase when state aid increased be explained in the same way? It is not possible to provide a precise answer to this question because, to my knowledge, no research has explored how changes in state aid formulas in New York influenced school districts' per pupil expenditures.[1] Thus, we do not know whether changes in New York State school aid formulas resulted in superintendents having larger budgets (denominated in per pupil terms) or not.

We do know from the E-C work that some part of the increases in state aid went for property tax relief. E-C's estimates reported in table 9.3 imply that a 10 percent increase in state aid is associated with a 2–5 percent reduction in property tax rates (see footnote *a* of E-C's table 9.3 for a description of the method for calculating elasticities). Is this a big enough reduction in property tax rates to absorb totally the increased state aid, leaving school districts with no more funds to spend on teachers' salaries or anything else? Apparently E-C do not think so, because they comment that although increases in state aid levels negatively affected tax rate growth, "this relationship was far from one to one." It does not need to be one to one, however. How large the elasticity of tax rate change with respect to state aid change must be to keep per pupil expenditures constant depends on the relative importance of state aid and local property taxes in supporting school expenditures. For plausible values of state aid and property tax revenues per pupil, E-C's estimates of the sensitivity of property tax rates to state aid are large enough to support the conclusion that changes in the New York State school aid formula over the early 1980s did not result in increased teacher salaries because they did not result in increased expenditures per pupil.[2]

If, in fact, the reductions in property tax rates brought on by the increase in the generosity of the New York State school aid formula were large enough to absorb the extra revenue made available to local school districts, then there is no puzzle. Teacher salaries did not increase because there was no increase in funds made available to local school districts. I believe that E-C's estimates are consistent with this hypothesis, but without longitudinal data on per pupil expenditures, we cannot know for sure.

What if the property tax rate reductions did not absorb all of the increases in state aid? What happened to the rest of the increased aid? If it did not go into higher teacher salaries and if it did not go into smaller class sizes (E-C find that more aid is associated with larger classes—a real puzzle), where did the money go? The only category of resources that E-C find to increase in response to increased state aid is "other professionals" per student (i.e., not teachers). It is puzzling that "other professionals" per student would increase in response to increased state aid, but teachers per student would decrease.

Another possible use of the resources made available through increased state aid is maintenance. Carroll (1982) found that noninstructional expenditures, including maintenance, were more sensitive to state aid than instructional expenditures were. This is a plausible finding for New York as well as Michigan. Many school districts in these two states were experiencing severe fiscal stress at the end of the 1970s. Rapid inflation—especially in the price of fuel oil—was making it difficult to balance the budget. Many communities were not willing to pay for the higher taxes needed to increase revenues. Moreover, many school expenditures, especially teacher and administrator salaries, were fixed contractual commitments. The only place school officials could save money was by skimping on maintenance. After several years of such skimping, however, roofs begin to leak, and the need for catch-up expenditures to save the physical plant is great. Meeting this need may have absorbed the increased resources that state aid in Michigan made available to some local school districts.

It is possible that maintenance may also be the category that experienced the largest expenditure increase in New York State school districts when changes in the state school aid formula led to increases in state aid. Unfortunately, we do not know whether this is true because E-C do not report results on maintenance expenditures.

One final note on state aid. The New York State school aid program is actually a closed-end matching grant program. Thus, for districts that spend less than a given expenditure level per pupil (E_H in E-C's paper), the state aid level depends on the district's expenditure level. For such districts it is not appropriate to treat the state aid level as if it were a block grant. E-C deal with the matching nature of the grant program in two ways. First, they argue that "the majority of districts appeared to have spent more than E_H each year" and, thus, it is legitimate to treat state aid as exogenous. While this is true for those districts with expenditures above E_H, what about the other districts? Might not the results be different if the statistical work took into account that these districts can influence their aid levels? In fact, the results are different when E-C adopt their second approach (instrumental variables) to estimate the impact of changes in the state aid formula on teacher salaries, tax rates, and staffing ratios. Unfortunately, however, the second set of results do not form a consistent pattern. In E-C's words,

> . . . increases in state aid now appear to be negatively associated with *both* teacher/student and paraprofessional/student ratios. The magnitude of the latter relationship is sufficiently large to make one question what we have found.

I appreciate the honesty of E-C's evaluation of their results. However, the results themselves are troubling. The second-round (instrumental variables) results are sufficiently different from the first-round OLS results to leave serious questions about what inferences should be drawn from the E-C work about the effects of state aid formula changes. I wish that E-C had adopted the strategy of modeling the changes in the matching grant formula as changes in the prices that local school districts pay for education and then had estimated the responses to these price changes. This is the strategy that public finance economists usually employ to examine the effects of matching grant programs.

What would E-C have found if they had modeled the changes in the New York State school aid formula as changes in the prices school districts pay for education? Would they have been similar to their first-round OLS results? Or to their somewhat confusing instrumental variable results? In our Michigan data, my colleagues and I found that modeling changes in the state aid matching grant formula as changes in the prices school districts face produced results almost identical to the results of modeling the aid changes as changes in block grants. With both models, the results were that increasing the generosity of the aid formula did not result in higher teacher salaries. This is E-C's result based on modeling state aid as block grants. Thus, experience with the Michigan data leads me to guess that had E-C chosen to model the formula changes as changes in the prices school districts face, their results would have been similar to their OLS results in which state aid is treated as exogenous.

Looking at E-C's other results, they are more successful in explaining the trends in the salaries of experienced teachers than they are in explaining the trends in starting salaries. I found the same pattern in Michigan. In commenting on their inability to explain changes in starting salaries, E-C state: ". . . teachers' unions paid relatively little attention to starting salaries, and in a number of cases no increases were given at that level." Should we infer from this statement that starting salaries did not change much over the time period and therefore the regression coefficient is small because there is little variation in the dependent variable? E-C do not present the data needed to answer this question. I found in Michigan, however, that there was *more* variation in the log of starting salaries in 1980 than there was in the log of 1970 starting salaries. This was not the case for minimum salaries. I also found that the structure of salary scales became steeper over the decade. In other words, the maximum salary increased at a greater rate than the starting salary did. One plausible explanation for this is union politics. The teaching staffs in most school districts in New York and Michigan aged over the 1970s because few new teachers were hired. Older teachers pushed for increases in the part of the salary scale that pertained to them, and the school districts did not resist because they were not trying to attract new teachers. This hypothesis suggests that the change in the experience distribution of the teaching staff in a school district may be a variable that would help to explain the change in the salary structure.

I also found that starting salaries and maximum salaries increased more in Michigan school districts with growing student enrollments than in districts with declining enrollments. This made sense in that teachers' unions in districts with declining enrollments may have forgone salary increases in order to protect the jobs of union members. I was curious to see whether E-C also found that student enrollment changes influence salary changes and was disappointed to learn that they did not find this pattern.

E-C and I did both find that the variable most important in explaining the salary change over time was the base year salary; the lower the base year salary the larger the change. This is quite plausible; it may just reflect a catching-up phenomenon, as E-C suggest.

One finding in the E-C paper that I did find surprising is that tax rate increases were greater in communities that voted on annual budget referenda than in communities in which the school board set the tax rate. My intuition was in the opposite direction. I wonder whether it is legitimate to interpret this finding as indicating the impact of the method by which the school budget is set. I interpet E-C's discussion of the critical variable as indicating that it is, in effect, a dichotomous variable taking on a value of one for a city school district and a value of zero for a suburban or rural district. The method of determining the school budget is only one of many things that differentiate big city school districts from other districts. I wonder whether it is the method of determining the school budget that really explains why city school districts increased their local tax rates less during the early 1980s than other school districts did.

With this caution in mind, I would support E-C's intuition that examination of the roles institutional considerations, such as how budgets are determined, play in determining tax rates and expenditure patterns is a promising line for future research.

Having expressed a number of reservations about particular findings in the E-C paper, I would like to conclude by endorsing several aspects of E-C's approach. First, their use of longitudinal data is a significant improvement over studies that use cross-sectional data. Second, their interpretation of results is informed by knowledge of collective bargaining practices, such as the tendency to "backload" salary increases to the last contract years. Third, their candor in pointing out that their results contain a number of puzzles is refreshing and all too rare.

Notes

1. Eberts and Stone (1984) reported that a $1.00 increase in state aid per pupil in New York State increased school spending by $0.88 per pupil. However, they treated state aid as block grant aid, while the formula in fact was a closed-end matching grant formula.

2. Assume that a particular school district's budget is financed exclusively from local property taxes and state block grant aid.

Let: S_i = state school aid per pupil provided to the district in year i
S_1 = \$500
S_2 = \$750
V_i = the school district tax base per pupil in year i
$V_1 = V_2$ = \$100,000
r_i = the local property tax rate in year i
r_1 = 0.02
$E_i = S_i + r_i V_i$ = per pupil expenditures in year i
$E_1 = 500 + (0.02)(100{,}000) = \$2{,}500$

Solve for the value of r_2 that keeps the per pupil expenditure level at $2,500 in year 2:

$$r_2 = \frac{E_2 - S_2}{V_2} = \frac{2500 - 750}{100{,}000} = 0.0175\ .$$

Solve for the arc elasticity indicating the response of the local property tax rate to a change in state aid:

$$\begin{aligned}\text{Elasticity} &= \frac{(r_2 - r_1)/[(r_1 + r_2)/2]}{(S_2 - S_1)/[(S_1 + S_2)/2]} \\ &= \frac{-0.0025/0.01875}{250/625} \\ &= \frac{-0.133}{0.40} \\ &= -0.33\ .\end{aligned}$$

This elasticity is within the range estimated by E-C.

References

Carroll, Stephen J. 1982. The search for equity in school finance. In *Financing education: Overcoming inefficiency and inequity*, ed. Walter W. McMahon and Terry G. Geske, 237–66. Urbana, Ill.: University of Illinois Press.

Eberts, Randall W., and Joe A. Stone. 1984. *Unions and public schools*. Lexington, Mass.: Lexington Books.

Murnane, Richard J., Judy Singer, and John Willett. 1986. How did teacher salary schedules change during the 1970s? Harvard Graduate School of Education. Mimeo.

This Page Intentionally Left Blank