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## Throwing Money at Schools

*Eric A. Hanushek*

### **Abstract**

*The conventional wisdom about public schools is that they face serious problems in terms of performance and that improving schools requires additional money. However, the available evidence suggests that there is no relationship between expenditures and the achievement of students and that such traditional remedies as reducing class sizes or hiring better trained teachers are unlikely to improve matters. Furthermore, there is little reason to believe that schools will move toward more efficient operations, either on their own or through consumer pressures. More attention should be given to developing direct performance incentives.*

There is a growing consensus that public elementary and secondary schooling in the United States is in trouble. On the one hand, the performance of schools has come into question. Declines in student test performance regularly make front page news. There is open discussion of "functional illiteracy" and the ensuing difficulties at the workplace. Stories about disciplinary problems in schools and about increases in absences and school dropouts are commonplace. On the other hand, and probably not unrelated, we seem to be entering an era of budgetary austerity for schools.

There is a conventional wisdom about the causes and solutions to the performance problems. Large classes prevent teachers from working effectively with individual students and invite disruptions and disciplinary problems, suggesting that smaller classes could improve student performance. Schools are also seen to face problems in attracting and retaining the best teachers, perhaps because of problems within the schools or because of levels of pay. Furthermore, schools seem to need teachers with better training and stronger specializations. Yet improving schools in these dimensions takes money, and here the fiscal squeeze on schools enters. If these pressures increase, can we expect further deterioration in school performance?

When we turn to the available evidence, however, there is little to support the conventional wisdom. Over the past two decades (until perhaps quite recently), schools have consistently spent more on education each year; this added spending has resulted in smaller classes, more experienced teachers, and teachers with more education (along with better paid teachers). Thus, it is difficult to ascribe any deterioration in school quality to declines in resources available to schools. More importantly, a review of a wide range of sophisticated and comprehensive studies of student performance indicates that there is no consistent relationship between school expenditures and student performance. Nor is there any consistent relationship between the usually proposed remedies, such as reducing class sizes, and student performance.

There are real reasons to be concerned about the performance of the public school system. These concerns relate both to absolute levels of student performance and to variations in performance that are related to family incomes, race, and geographic location. There is ample evidence that schooling is related directly to the subsequent opportunities and success of individuals; ameliorating existing variations in performance is—and rightfully should be—an important goal of public policy. There has been considerable debate over whether the goal should be stated as “equality of educational opportunity,” and even more debate over the meaning of “opportunity.” However, in spite of the differences over the definition of goals, a widespread concern exists about school performance. That concern probably goes a long way toward explaining some of the dramatic increases in school expenditures that have occurred. Yet, while wanting to “do good,” policymakers have had an inadequate understanding of the determinants of scholastic performance and of how increased expenditures could be used effectively.

This article summarizes the accumulated evidence about the effects of schools and teachers on the performance of students and relates this evidence to current policies. The findings also dramatize the fact that, in spite of heroic efforts on the part of researchers, our understanding of what makes for effective performance in schools is astonishingly primitive. That fact raises deeper and broader issues about the organization of public activities, issues that could just as easily be raised in a wide range of public policy areas such as health or crime.

Studies of governmental performance can identify problems but often do not provide specific guidance on how to correct them. To correct such problems, one must ordinarily understand in detail how the government produces its services and how consumers respond to these services. With education (and many other areas of governmental activity) we lack both kinds of information. This article identifies some of the critical information that is lacking and suggests where more research is likely to provide the needed guidance.

**AN OVERVIEW OF CHANGES IN SCHOOLS** Elementary and secondary education in the United States has undergone significant changes in recent years. The magnitude of these changes often escapes notice.

As Table 1 shows, expenditures on public elementary and secondary schools grew from 3.3 percent to 4.6 percent of gross national product between 1960 and 1975. Part of this increase simply reflects the "baby boom"; and, in fact, the percentages fell somewhat after 1975 as the relative size of the school population declined.

The pattern of expenditures per student is more informative. These have been characterized by dramatic increases between 1960 and the mid-1970s and a subsequent leveling off. In constant 1978 dollars, total expenditures per student increased from \$992 in 1960 to \$1,898 in 1975—an average growth of 6 percent per year in real expenditures. Moreover, these increases were not simply a reflection of more costly administrative activities; expenditures per pupil for teacher services rose at the same pace.

The bottom portion of Table 1 indicates the components of these increased expenditures. Average student-teacher ratios have fallen steadily over the entire period—from 26.5 to 19.9 students per teacher. The proportion of new teachers, after rising during the shortages of the late 1960s, fell to new lows by 1978. The propor-

**Table 1.** Public elementary and secondary school operations, 1960–1978.

|                                  | 1960               | 1965     | 1970               | 1975     | 1978                 |
|----------------------------------|--------------------|----------|--------------------|----------|----------------------|
| Expenditures as percent of GNP   | 3.3                | 3.8      | 4.6                | 4.6      | 4.3                  |
| Expenditures per student         |                    |          |                    |          |                      |
| Total in current dollars         | \$451              | \$613    | \$992              | \$1,560  | \$1,855              |
| Total in 1978 dollars            | \$992              | \$1,264  | \$1,664            | \$1,898  | \$1,855              |
| Instructional in current dollars | \$262 <sup>a</sup> | \$337    | \$610 <sup>b</sup> | \$884    | \$957 <sup>c</sup>   |
| Instructional in 1978 dollars    | \$571 <sup>a</sup> | \$696    | \$983 <sup>b</sup> | \$1,069  | \$1,030 <sup>c</sup> |
| School operations                |                    |          |                    |          |                      |
| Students per teacher             | 26.5               | 25.0     | 22.3               | 20.4     | 19.9                 |
| Teacher experience               |                    |          |                    |          |                      |
| % four or less years             | d                  | 22.4     | 25.8               | 19.2     | 18.5                 |
| Teacher education                |                    |          |                    |          |                      |
| % higher than Bachelor's degree  | d                  | 22.6     | 27.1               | 37.5     | 44.6                 |
| Average teacher salary           |                    |          |                    |          |                      |
| Current dollars                  | \$4,995            | \$6,195  | \$8,635            | \$11,690 | \$14,247             |
| 1978 dollars                     | \$11,616           | \$13,431 | \$15,488           | \$15,367 | \$14,247             |

Source: U.S. Statistical Abstract, various years.

<sup>a</sup> 1961 data.

<sup>b</sup> 1971 data.

<sup>c</sup> 1977 data.

<sup>d</sup> Not available.

tion of teachers with a Master's or more advanced degree steadily increased, reaching 45 percent by 1978.

Changes in the average salary figures reflect two things: changes in the salary of teachers of a given type (say, the salaries of new teachers on entry), and changes in the composition of the teaching force (such as increases in the proportion with advanced degrees). Average salaries in constant dollars increased by 32 percent over the 1960–1975 period, or slightly above 2 percent per year over and above inflation. In contrast, average real weekly earnings of all private, nonagricultural employees grew by only 12 percent over the same period. While real teacher salaries fell by some 7 percent between 1975 and 1978, the gains over the entire 1960–1978 period remain substantially above those of workers elsewhere in the economy.

From these data, it is hard to claim that we have been skimping on education. In both expenditures and real resources (class sizes and qualifications of the teacher force), there have been consistent and dramatic increases.

Evidence regarding the performance of students over this recent period is harder to come by. However, the available data suggest that there has been little overall improvement; indeed, there is the distinct possibility of real declines in performance. The most celebrated evidence of actual declines is the progressive drop in average SAT scores—a fall of some 10 percent since the mid-1960s. While this change could be explained by a variety of factors (such as changes in the composition of those taking the tests), there is evidence that this is not simply a statistical artifact.<sup>1</sup>

Longitudinal data for a national sample of 17 year olds (from the National Assessment of Educational Progress) also suggest constancy or declines in performance. Test scores in science knowledge, civics, and social studies fell from 1970 to the mid-1970s, while reading ability remained virtually constant.<sup>2</sup>

The aggregate picture thus is one of steadily increasing expenditures on schools accompanied by constancy or actual declines in student performance. This picture—which casts considerable doubt on the efficacy of traditional prescriptions for improving schools through increased expenditures—is confirmed by more detailed studies of the performance of individual students.

**THE DETAILED STUDIES OF SCHOOL PERFORMANCE** A decade and a half ago, the U.S. Office of Education released its monumental analysis of elementary and secondary education, *Equality of Educational Opportunity* (or, more commonly, the “Coleman Report”).<sup>3</sup> This report received immediate public attention—and quite deservedly. One key finding was that variations in the level of students’ achievements bore little or no relationship to the resources or programs of their schools.

**Their Scope**

The reaction to the Coleman Report was predictable. Seminars were held, the Coleman Report was scrutinized for flaws, and new studies were undertaken. Perhaps as predictably, the discussion turned to more and more technical issues. The new analyses, often

based on data no better than those in the Coleman Report or upon small, nonrepresentative samples, appeared on first impression to provide few consistent findings that differed from the original one. The debate receded to the pages of academic journals, and interest in the subject soon waned.

This, it turns out, is unfortunate. The results of these further studies, whatever their individual faults, are of considerable importance and allow a number of strong statements to be made with a high degree of confidence. This discussion documents the findings of these school performance studies and then explores their implications.

The Coleman Report investigated the factors affecting achievement in a variety of subject areas and at different grade levels; its conclusions were based on a national sample of almost 600,000 students found across grades 1 to 12 in over 3000 schools. None of the subsequent studies has equaled its scope. Yet there have been some 130 similar investigations; scattered in academic journals and books, these create a comprehensive mosaic of American education. They look at different measures of educational performance (including various standardized test scores, measures of attitudes, and data on school attendance patterns and school dropouts); at performance at different grade levels; and at urban, suburban, and rural school systems. These studies provide some real insights into the operations of the nation's schools.

The results presented here are based on an exhaustive compilation of available studies that analyze the relationship between student performance and school expenditures, or the determinants of such expenditures. Some studies were probably missed; nevertheless, as shown in the Appendix, 29 published works covering 130 separate analyses are included.<sup>4</sup>

Table 2 provides an indication of the breadth and character of past inquiries into school performance. Three-fifths of the available studies measured performance on the basis of standardized tests, while the remainder were based on other outcome measures such as school dropout rates, rates of continuation to further schooling, attendance patterns, attitudes, or school grades. In terms of sample design characteristics, 60 percent of the studies considered secondary school performance (grades 7–12) and 40 percent considered elementary school performance (grades 1–6). (At the elementary school level, however, over 80 percent of the studies used test measures of performance.) Slightly more than one-half considered performance of individual students, while the remainder considered the aggregate performance of students in a school building or school system. A majority of the studies analyzed performance across different school systems, while the remainder drew their observations entirely from a single system.

Clearly, the studies also differed in the precise formulation of the models, in the statistical methodology used, and ultimately in the "quality" of the research. Nevertheless, taken as a whole, the studies represent a broadly based inquiry into public schooling in America.

**Table 2.** Number of studies by various characteristics and by performance measures.

|                         | Performance measure |                 |             |
|-------------------------|---------------------|-----------------|-------------|
|                         | Test score          | Nontest measure | All studies |
| Grade level             |                     |                 |             |
| Elementary (grades 1-6) | 43                  | 10              | 53          |
| Secondary (grade 7-12)  | 36                  | 41              | 77          |
| Unit of analysis        |                     |                 |             |
| Individual student      | 46                  | 22              | 68          |
| Aggregate performance   | 33                  | 29              | 62          |
| School system sample    |                     |                 |             |
| Single system           | 40                  | 16              | 56          |
| Multiple systems        | 39                  | 35              | 74          |
| All studies             | 79                  | 51              | 130         |

**The Results** There is considerable variation in expenditure levels across schools. The most universal premise is that better schools cost more money. A corollary of this proposition is that schools with higher expenditures should, other things being equal, have higher student performance. Thus, the logical starting point is a consideration of whether expenditure differences are positively related to achievement differences.

We begin with differences in instructional expenditures, the largest item in total school expenditures.<sup>5</sup> Instructional expenditures per student are determined by teacher–student ratios and salaries of teachers; salaries, in turn, are chiefly determined by levels of education and experience. Therefore, a majority of the studies of student achievement have concentrated directly upon the impact of teacher–student ratios, teacher education, and teacher experience levels. A lesser number of studies have also considered the additional impacts of differences in administration and facilities. The key question is whether or not these analyses generally support the proposition that more of these resources lead to higher student performance.

Uncovering the determinants of scholastic performance involves a certain amount of detective work and generally calls for the use of sophisticated statistical methods. Variations in school performance, however measured, clearly reflect a variety of factors. Students have different innate abilities; they come to school with different family backgrounds; and they have different experiences in schools. Furthermore, these factors are not independent: Wealthier parents, for example, tend to send their children to schools with higher expenditure levels and “better” school re-

sources. There have been few experiments appropriately designed to isolate the effects of any single factor, such as class size, on achievement. Instead, researchers have relied on data generated in the ordinary course of operations of schools and have attempted to disentangle the influences of the separate factors on student achievement through the use of statistical techniques. This was the procedure of the Coleman Report as well as the subsequent investigations.

Accordingly, the reported studies consider the impact on achievement of specific inputs (as reflected in such measures as teacher–student ratios) after allowing for differences in family background and other characteristics of teachers and schools. There is, however, always some ambiguity in such analyses. Each relies on a relatively small sample of schools; most depend on data generated for other purposes such as administrative records of schools; most fail to consider more than a few of the many extraneous factors that might contribute to observed achievement differences. Therefore, within any given analysis of student performance, there is the possibility that the estimated relationship between a specific factor and achievement is spurious—an artifact of the given sample of schools and students. To assess this possibility, the “statistical significance” of estimated effects is calculated. From the regression analysis that is used to disentangle the separate influences, one can also derive the probability that the seeming relationship between school performance and any given factor is due to pure chance, induced by random variations in the sample chosen for the study. An estimated relationship is said to be “statistically significant” if the probability that the observed relationship occurred by chance in a randomly drawn sample is less than 5 percent.\*

Table 3 summarizes two aspects of the results from the 130 separate studies of educational performance: the direction of the estimated effect, and whether an estimated effect is statistically significant. The sign, or direction, of an estimated relationship simply indicates whether higher expenditures in a given area are likely to be associated with higher student achievement. (Each of the characteristics, according to conventional expectations, should have a positive sign; for example, larger teacher–student ratios should be linked to higher achievement.) The test of whether an

\*Statistical tests of significance, at the same time, do not take into account the variance created by factors other than sampling errors, such as the failure to consider some important causal factors or the use of nonrandom samples. Such nonrandom variation in achievement can affect both the estimated relationship and the statistical significance that is subsequently calculated. For a detailed discussion, see Eric A. Hanushek, “Estimation of Educational Production Functions,” *Journal of Human Resources*, 14(3) (Summer 1979): 351–388, and Eric A. Hanushek and John E. Jackson, *Statistical Methods for Social Scientists* (New York: Academic, 1977). To assess the importance of these factors, an extensive further investigation of the study results that stratified the 130 studies by unit of analysis, by statistical methodology, by form of the estimating equation, and by subjective “quality” ratings was conducted. This work (not reported) leaves the conclusions unaltered.



**Table 3.** Relationship between educational inputs and student performance (130 available studies).<sup>a</sup>

| Input                               | No. of studies considering given input | No. of studies with statistically significant coefficients |          | No. of studies with statistically insignificant coefficients |          |          | Unknown sign |
|-------------------------------------|--|--|----------|--|----------|----------|--------------|
|                                     |  | Positive   | Negative | Total  | Positive | Negative |              |
| <i>All 130 studies</i>              |  |  |          |  |          |          |              |
| Teacher-student ratio               | 109                                    | 9  | 13       | 87   | 25       | 41       | 21           |
| Teacher education                   | 101                                    | 6  | 4        | 91   | 23       | 31       | 37           |
| Teacher experience                  | 104                                    | 30   | 6        | 68   | 32       | 21       | 15           |
| Teacher salary                      | 60                                     | 9  | 1        | 50   | 15       | 11       | 24           |
| Expenditures per student            | 55                                     | 5  | 3        | 47   | 23       | 13       | 11           |
| Quality of facilities               | 71                                     | 7  | 5        | 59   | 14       | 14       | 31           |
| Quality of administrators           | 54                                     | 4  | 1        | 49   | 11       | 14       | 24           |
| <i>Studies based on test scores</i> |  |  |          |  |          |          |              |
| Teacher-student ratio               | 66                                     | 7  | 4        | 55   | 15       | 19       | 21           |
| Teacher education                   | 66                                     | 2  | 4        | 60   | 13       | 20       | 27           |
| Teacher experience                  | 67                                     | 22   | 1        | 44   | 20       | 9        | 15           |
| Teacher salary                      | 40                                     | 7  | 0        | 33   | 6        | 3        | 24           |
| Expenditures per student            | 35                                     | 3  | 2        | 30   | 12       | 7        | 11           |
| Quality of facilities               | 45                                     | 6  | 2        | 37   | 9        | 7        | 21           |
| Quality of administrators           | 36                                     | 2  | 1        | 33   | 4        | 6        | 23           |

<sup>a</sup>Statistical significance refers to the 5-percent level of significance. All variables are normalized to have an expected positive effect; for example, the sign on a variable measuring student-teacher ratios is reversed for the tabulations.

input has the “correct” sign (positive) in its relationship with student achievement is clearly a very weak condition for making conclusions about the appropriateness of increased expenditures on the item. The relationship could be positive but quantitatively small, and any information about the quantitative magnitude of the estimated effects is ignored at this point. If, however, an input was related negatively with achievement, there would be no way to justify increased expenditures on it, regardless of the magnitude of the estimated effect.

Since many of the studies fail to consider all of the input possibilities listed in Table 3 (usually because of lack of data), the first column indicates the total number of studies relevant for each factor. For example, 109 of the 130 studies considered the possible effects of teacher–student ratios, while only 54 considered the effects of differences in administrative inputs. The next two columns record only the statistically significant results, classified according to the direction of estimated effect. While conventional wisdom suggests each factor should be positively related to student performance, the actual findings are quite different. Consider, for example, teacher–student ratios. Thirteen studies find significant *negative* associations for teacher–student ratios (that is, smaller class sizes are associated with *lower* achievement), and only nine find a statistically significant relationship in the expected positive direction. Most of the studies (87 out of 109 do not find a statistically significant relationship between teacher–student ratios and achievement, and thus offer little basis for assuming that there is any relationship between the two. Yet, because insignificant relationships can arise from insufficient data (as opposed to simply no relationship between achievement and the given factor), the table also reports available information about signs of coefficients identified as statistically insignificant. (Such information is not always provided; for teacher–student ratios, only 66 of the 87 studies reporting insignificant coefficients also indicate the direction of the relationship.) In the case of teacher–student ratios, again, a majority of the reported insignificant coefficients also have the “wrong” sign.

The remaining entries in Table 3 tell much the same tale. For each of the specific inputs, there is a set of studies finding the expected relationship with student performance, but one can also find an uncomfortably large group of other studies providing exactly the opposite evidence.

In this sea of insignificant and inconsistent results, teacher experience does appear to be different qualitatively from the other specific inputs. Thirty percent of the studies that cover teacher experience find a statistically significant relationship in the expected direction, and 70 percent of all studies reporting the sign find a positive association. Restricting attention to studies using standardized test scores as the measure of student performance (presented in the lower panel of the table), one finds even somewhat higher percentages in accord with conventional expectations. While others may disagree, I regard this as relatively weak support

for any judgments or policy statements. To begin with, these results are probably due in part to the fact that more experienced teachers prefer more able students and, by contract terms, custom, and individual behavior, exercise choice over school assignments.<sup>6</sup> Unfortunately, statistical relationships of the sort presented here cannot distinguish the direction of the causal flow. For our purposes, we are interested in the impact of more experience on achievement, and not the reverse, but the reported studies include both effects. Indeed, even if higher experience of teachers had no direct causal impact on students' achievement, a positive relationship between the two could be estimated solely on the basis of the school and classroom selection mechanism for teachers.

Moreover, the teacher experience results, while strong relative to the other results, are not very convincing in an absolute sense; if teacher experience actually had a significantly beneficial effect, it is unlikely that so few studies would pick up that fact. (If teacher experience had no effect, we would expect half the estimated coefficients to have a positive sign.) Finally, the evidence presented says nothing about whether any causal effect is strong enough to justify the increased expenditures that characteristically go to more experienced teachers; indeed, many of the estimated relationships (including many that are statistically significant) are quantitatively quite small. For example, 6 of the 22 positive and statistically significant experience coefficients come from a single source. Yet, these results also indicate that the gains from experience are almost completely confined to the first two years of teaching experience.<sup>7</sup>

The message, taken as a whole, is that the inputs on which schools tend to concentrate—and which lead to differences in expenditures—appear to have no consistent payoff in terms of higher student performance. The only thing we can say with any confidence is that “improving” schools in the ways conventionally suggested will increase school costs.

**DO SCHOOLS AND TEACHERS MATTER?** Many critical presumptions about school effectiveness—presumptions reflected in the choices of schools and codified in teacher salary schedules—are unsupported by the facts. At the same time, most parents perceive that the differences in schools are real and important, and they act on these beliefs through their choices of residential locations, through their involvement in school decisions, and so forth. Therefore, it is useful to consider whether there are attributes of schools and teachers—attributes other than those previously considered—that systematically affect student performance.

One line of research has aimed at describing teachers and schools in many more dimensions, a broadly based quest to find the elusive factors that determine student performance. To this end, a wide range of teacher attributes has been considered, including measures of the quality of teacher schooling, teacher attitudes, interactions between teacher characteristics and pupil

backgrounds, internal classroom management, and teacher presentational styles. Some of this work is found in the studies previously cataloged, but the work goes far beyond those specific studies. It is not possible, within the confines of this brief article, to do justice to this extensive body of research. It is also not possible to aggregate the studies in a fashion similar to that above; the hypotheses investigated are much more varied, and specific findings of any given study are seldom replicated in different samples and situations.

Nevertheless, when one looks for common findings across the studies, few consistent results emerge. The only reasonably consistent finding seems to be that “smarter” teachers do better in terms of student achievement. “Smartness” of teachers, measured by teacher performance on verbal ability tests, was originally identified in the Coleman Report as possibly being important. Although information of this sort has been available infrequently, its importance has been fairly strongly supported in subsequent analyses. In particular, teacher verbal ability was found to be associated positively with student test performance in 14 out of the 18 studies where such investigations could be found, although only 5 of the 14 positive coefficients were statistically significant.

The essence of all these investigations is a search for a small set of descriptive measures of teachers and schools that are related consistently to student performance and that might be used to prescribe a set of policies which, when implemented across schools, would give some fair assurance of improvement in student performance. However, the evidence demonstrates that the elements of teaching skill are not easily identified or measured. Indeed, given our current knowledge of the achievement process, it appears that teacher performance is quite idiosyncratic.

A second line of research takes just this view. Instead of attempting to measure the specific characteristics of teachers, presentational styles, and other factors that could influence student achievement, a simpler question is asked: Does it matter which teacher a student has? In such studies, the mean achievement gain of students is estimated, after allowing for differences in entering achievement and family backgrounds.<sup>8</sup> Although relatively few studies of this sort have been conducted, the results strongly indicate that “it matters a lot which teacher a student has.” Measured by test score gains over a single school year, students with the best teacher gain one whole grade level more than students with the worst teacher.<sup>9</sup> In these studies, “best” refers to the teacher who obtains the largest average gains in achievement from a classroom (after allowing for differences among students in entering achievement levels and family backgrounds); “worst” refers to the teacher obtaining the smallest gains. While the studies are not entirely conclusive, it appears that the estimated performance of a given teacher is consistent across different school years; in other words, the estimated achievement gains by students are not artifacts of particular classrooms but instead reflect underlying differences in teacher performance. Moreover, it must be

emphasized that only a small portion of the estimated differences among teachers can be explained by any direct measures of teacher characteristics, including “smartness” or experience of the teacher.

What we currently know about student performance can be stated succinctly. First, there is a large dispersion in student achievement. Second, while not reviewed here, there is overwhelming evidence that a student’s performance is strongly affected by the student’s family background. Third, there are important differences among teachers, differences that lead to different student performance over and above the influences of family background. Fourth, differences in teacher performance cannot be described by any simple set of characteristics such as the backgrounds of teachers, classroom organizational techniques, presentational styles, and so forth. Finally, and most important for this discussion, higher school expenditures per pupil bear no visible relationship to higher student performance.

Because the last finding is so at odds with conventional wisdom and has such large policy ramifications, the precise nature of the evidence should be underscored. All of the studies consider variations in inputs and expenditures within the current structure of schools. From these results, one cannot infer that students would learn the same amount if they never attended school. Furthermore, the evidence does not indicate that money, or the specific inputs, could not have a beneficial effect. The evidence does indicate that, given the current operations of schools, there should be no presumption that simply providing more money or improving schools in conventionally accepted ways will have any perceptible impact on student achievement.

**IMMEDIATE POLICY IMPLICATIONS** In light of our limited understanding of what makes for improved student performance, any formula for improving the problems of schools is exceedingly vulnerable. Indeed, as we shall presently see, there are a series of fundamental questions about the underlying behavior of schools and citizens that, while currently unanswered, are crucial for deciding on corrective policies. Nevertheless, the previous evidence does provide some direct policy guidance.

The findings summarized here have a direct bearing on what is perhaps the most hotly debated issue of school policy today—the method of financing public schools. Since the landmark California court case of *Serrano v. Priest*, the method of raising and distributing school tax revenues has been debated in courtrooms and legislatures in virtually every state. The central focus has been variations in school expenditures that result from heavy reliance on local property taxes. A majority of states have altered their financing formulas over the past decade, and school finances promise to be prominent on the policy agenda for the future. For example, after more than a decade the *Serrano v. Priest* case itself is still in litigation.

The “reform” movement is partly guided by the belief that poor people live in areas with low property wealth—an assumption that is not uniformly true across states. For example, New York City and the other large cities of New York State have a disproportionate share of poor people at the same time that they have above-average levels of property wealth per pupil; because of this, these cities found it in their interest to join the plaintiffs (in *Levittown v. Nyquist*) and to introduce new issues (municipal overburden or high demands for noneducational services) into that case in order to protect themselves from any changes in state school financing. More important, however, the educational equity arguments embodied in these discussions rest squarely on the validity of the assumption that district expenditures provide an adequate index of educational quality, an assumption that appears untenable.

Most financing discussions have focused on the distribution of taxes, subsidies, and school expenditures across school districts.\* However, typically ignored in these discussions is the fact that new financing schemes invariably lead to increased aggregate expenditures on schools. It is commonly accepted that reducing expenditures in high spending districts is politically infeasible, and therefore most plans call for increasing expenditures in low spending districts while leaving high spending districts unchanged. Thus, the search for equity usually generates a response that increases the total costs of school operations without ensuring any increases in the performance of students or the efficiency of schools.

The findings of the school performance research also point to a number of other current practices and policies that make little policy sense. A wide variety of state and federal programs are linked to school expenditures and have their success monitored and evaluated on the basis of observed expenditures. Moreover, other policies relate to specific resource use and requirements. For example, many states require teachers to obtain a Master’s degree within a fixed period of time in order to maintain certification. (Such requirements might be expected to induce a supply of institutions offering “low-grade” credentials, and this could easily explain the absence of any relationship, reported earlier, between student performance and teacher education.) State laws requiring that teachers be tenured in a period as short as three years are common, as are legal and contractual prohibitions against substituting teachers of lesser experience for incumbents. Similarly, limitations on class size lead to resource allocations that cannot be justified in terms of performance.

The pressures to increase expenditures per student and to institutionalize the current structure are likely to become stronger over time as the school population shrinks. Indeed there is evi-

\*The discussion here ignores issues of “taxpayer equity” and the distributional impact of capital gains and losses derived from the influence of school financing on housing values. While perhaps important, these issues are ancillary to the educational basis of these finance discussions.

dence of this in the outcomes of contract negotiations.<sup>10</sup> The future performance of the school system, judged in terms of effective resource allocation, is directly related to our ability to resist and even reverse such movements.

Finally, what does this evidence say about expenditure limitation measures? If such measures do no more than hold expenditures constant, we should expect little effect on student performance. If such measures call for actual cuts, the predictions are less certain. We simply have little information about how schools might react. The previously presented evidence suggests that an obvious policy would be to absorb budget cuts through increased class size. However, depending on how such a policy was implemented, the atmosphere and environment of schools might deteriorate and might adversely affect the attitudes of the teachers that remain. Virtually all of our knowledge about schools is generated during times of expansion, and times of decline may not be symmetrical to these experiences.

**THE REMAINING PUZZLE** The analysis up to this point stops well short of suggesting how the schooling system might be restructured to improve its future performance. Providing guidance on this critical issue is related directly to our ability to answer a deceptively simple question: Why is school policy so impervious to the facts?

**Supplier Incentives** An obvious starting place is the organizational structure of schools and the incentives facing decision-makers. Schools are complex organizations, and decisions are not made by any single, identifiable individual. State and federal agencies, local school boards, administrators, teachers, and even students make decisions that have a direct bearing on the educational process and the performance of schools. However, many of these actors face a real conflict of interest—they are torn between the interests of the community, the welfare of the students, and their own private interests.

The conflict is most obvious in the case of teachers and teacher unions. Teachers have traditionally served as “experts” determining both what is taught and how it is taught; yet teacher unions also have traditional union objectives of securing favorable pay and working conditions. At the beginning of the movement toward collective bargaining by teachers, teachers openly discussed whether traditional unions were consistent with professionalism, and unionization was not instantly accepted. Today teacher unions are expected to act just like any other union, attempting to secure favorable pay and working conditions for their members. Decisions on class sizes, length of the school day, curriculum, and similar matters may affect the quality of education offered; but, at the same time, they have an unmistakable influence on the demand for teachers and the conditions under which they work. Such issues now frequently appear on the contract negotiation

agenda, and the discussions and outcomes appear seldom to turn on the educational merits of the policies.

The conflicts are clearly not restricted to teachers. Administrators, who are typically drawn from the ranks of teachers, share many conceptions about what is "right." Moreover, it generally serves the interests of administrators to accept teachers' arguments about reducing class sizes, raising salaries, and increasing expenditures; such policies increase the administrators' domain, lessen conflicts with their employees, and ultimately must affect their own salaries. Teacher training institutions, the chief purveyors of conventional wisdom about organization and teaching methods and the traditional source of most research in education, have a direct stake in choices that influence the aggregate demand for teachers and for teacher training, particularly class sizes and requirements for graduate degrees. State and federal education agencies tend to view themselves as representing teachers and teachers' interests; note, for example, the source of pressures to create a U.S. Department of Education. Finally, even local school boards are affected by such conflicts; rarely representative of the general population, they have usually been composed of individuals closely associated with the schools and individuals generally favoring "quality" education.

The fact that school policies are so hard to evaluate makes it easier for teachers and school officials to be influenced by their personal interests. There is a lack of consensus about how to measure educational output; there are technical difficulties in separating the various influences on school performance; and there is a reluctance or incapacity to engage in experiments in order to evaluate programs. Without direct evaluations, teachers and school officials need not confront the possibility that their privately preferred policies may be useless for the student and bad for the community.

**Consumer Behavior** The incentives existing within schools appear to provide a compelling explanation of public school inefficiencies, but consumer behavior must also be considered. Indeed one might think that the public—the consumers in the school market—could essentially force better performance from the schools. Those using schools usually see a direct connection between what they get and what they pay for, a connection created by the importance of local taxation in the financing of schools. Moreover, those using school services often appear to have a real choice; by moving to another neighborhood, they can "buy" a different level of services. Accordingly, one can envisage something like a market for the sale of school services. Yet the evidence summarized earlier indicates that the public makes expenditures that yield no apparent benefits.<sup>11</sup> Why?

One explanation may be that consumers of school services are simply poor buyers. A variety of factors may explain poor buying behavior. Again, performance measurement is difficult, and the



effects of a good or bad education are not usually observed until after the student leaves school—when it is too late to correct any mistakes. Besides, the arguments for reducing class sizes, hiring more qualified teachers, and so forth, seem on first consideration inherently plausible; anyone who argues that such policies will improve student performance is arguing on the side of reason, even if not supported by the evidence. (The basic arguments are, importantly, often made in terms outside the relevant range of policies. For example, reducing class sizes from 25 to 5 may well improve performance, even though the more relevant change from 25 to 23 has no perceptible impact.) Finally, it is costly for the consumers of school services to respond to a bad bargain. The costs of moving to another neighborhood can be high; in any case other considerations go into such decisions.

Of course, the consumer also has the option of remaining within a district and attempting to influence school decisions. However, improving school effectiveness in this way is not easy. At times, consumers can vote on budgets, but this is generally a restricted choice—yes or no on a given proposal. Moreover, voting parents can aspire to shift part of the costs to taxpayers in general, inasmuch as such parents generally exercise their voting rights more regularly than other taxpayers. That hope may be strengthened by the fact that taxpayers without children in schools are often reluctant to vote against increasing school budgets. The essential conclusion remains: Consumers do not appear to offer much countervailing force to movements toward inefficient operations that are generated inside the school system.

Other possibilities, however, could explain the behavior of the American public in buying school services. One such possibility is that in choosing a school parents do not see themselves as primarily buying differences in educational services. Instead they may be selecting a social and physical environment for themselves and their children, including pleasant surroundings, athletic facilities, cultural advantages, and other students with compatible backgrounds. Simply put, higher expenditures may have a consumption element and may aid higher income parents in segregating their children.

In the end, however, it must be acknowledged that our understanding of consumer behavior in this area is quite primitive. The recent wave of popular support for limitations on local expenditures, such as Proposition 13 in California or Proposition 2½ in Massachusetts, might reflect public sentiment toward limiting the variety of environments offered by different school districts. However, it might also reflect public despair about being able to make schools responsive to public interests. The hard fact is that so far the motivations of the American public in influencing the level of school expenditures are difficult to divine.

**Why It Matters** Knowing what motivates and determines consumer choices has direct implications for the policies the nation may choose to pursue. If the efficiency of our school systems is due to poor

incentives for teachers and administrators coupled with poor decision-making by consumers, it would be unwise to expect much from programs that seek to strengthen “market forces” in the selection of schools. Voucher plans that give parents added latitude in choosing among competing schools, for instance, would offer little hope for improving the performance of the schooling system. On the other hand, if we think that consumers are primarily motivated by consumption and “sorting” goals, other policies, with distinctly different purposes, are suggested.

There is, however, no clear consensus on what kind of policies would be preferred socially. Those who think it desirable to let consumers have what they want would be prepared to tolerate or encourage wide differences in school expenditures from one district to the next; those who think a wide choice of schooling environments is undesirable would want to suppress such differences. (Note, however, that neighborhood variations in housing values would still provide considerable sorting.) In any event, we would expect little impact on the performance of schools in achievement terms, and different types of policies are still needed.

**FOCUS OF FURTHER STUDIES** Calling for more research is predictably the first refuge of academics. In this case, however, the call seems especially warranted. There are several areas of research that, while relatively neglected in the past, hold promise of providing guidance for future school policy.

As the previous section makes clear, understanding the process of consumer choice should have a high position on the research agenda. This is clearly a difficult problem, but a few areas could provide useful information in the short run. First, the power of citizens to vote on budgets and to affect their composition varies greatly from one school system to the next. Do these different structures lead to different outcomes, and from these can we infer anything about the character of consumer choices? Second, what information about schools do consumers rely on in making school choices, and what kinds of information could be usefully provided?

Apart from such questions about the demand for school services, there are also investigations on the supply side that deserve immediate attention. One is to determine the nature and consequences of organizational restrictions currently embedded in our school system. The other is to explore the implications of various incentive schemes designed to improve the performance of schools in achievement terms.

There are several obvious sources of constraints imbedded in existing school systems. The first is locally imposed restrictions, of which teacher contract provisions are probably the most important. With declining student enrollments, teacher unions appear to have worked increasingly hard at implanting a wide variety of restrictions into contracts; but no good description of current contract provisions exists—let alone any analysis of the impact of such provisions. Above the local level, state and federal laws and

policies operate to impose added restraints. Here, there is some information about certification requirements for teachers and administrators, about process requirements such as provision of programs for the handicapped, and about major court determinations as in desegregation or student rights cases. Again, the actual extent of these restrictions is unclear. There is virtually no information about the actual implementation of these restrictions or the responses of local schools to them. One urgent need is simply a compilation of relevant data in these areas.

Moreover, if we take the view suggested by the evidence that many of these restrictions are wasteful and unproductive, we still have to consider how to eliminate them. Clearly, many of the items are "purely political," and traditional research might not be too useful in devising strategies for their elimination. However, other items, particularly local contract terms, seem more amenable to analysis, including analysis that might provide some indication of what it would cost to gain agreement on the removal of undesirable restrictions.

The second area of inquiry builds upon the evidence summarized earlier about the determinants of achievement. Most research so far has been directed at discovering aspects of curricula, training methods, organizational structures and other aspects of the teaching process that seem to have been successful in general. The basic objective has been the identification of school policies and programs that could be reproduced and applied widely. So far, these efforts have been quite unsuccessful.

An alternative approach, which seems more productive given our current state of knowledge, is to begin with the presumption that the teaching process is idiosyncratic and that the ingredients of successful teaching are indefinable. This view suggests that it would be more profitable to encourage individual teachers and administrators to innovate, and then to reward good performance where it appears. Such an approach could conceivably pay off even if one never identified the ingredients of success; improved performance would simply depend on teachers and administrators finding their own way to a promised reward.

On this point, existing research is suggestive. A limited number of the studies that have evaluated "total" teacher performance have also compiled information about principals' evaluations of teachers. These show that the two sets of evaluations are highly correlated.<sup>12</sup> Nevertheless, the evaluations of principals have had no material impact on school operations; they rarely affect salaries or determine employment decisions, having long since been neutralized by contract provisions and regulations. Yet, if these evaluations had some real impact in terms of salary and employment decisions, the performance of schools might be quite different.

Incentive schemes of various sorts might be a fertile area for experimentation. The essential questions involve alternative types of incentive schemes and the behavioral responses of teachers and

administrators. For example, would direct performance incentives encourage individual teachers to “hoard” information about successful techniques? Would administrators modify their rating behavior if it had real impact? Answers to such questions are currently unavailable.

**RECAPITULATION** An individual’s success is highly related to schooling, and for that reason schools have been perceived as an attractive policy instrument. However, students’ achievement cannot be changed by fiat. To change achievement, one must work indirectly through teachers, curriculum, and the organization of schools. Yet the components of a successful school are poorly understood, so there is no way to mandate an effective program by setting requirements on the educational process. Moreover, schools as a whole demonstrate an inability to use available resources effectively. There is little reason to believe that an additional dollar put into a school will improve student achievement.

Effective programs do exist, but effective programs, measured in terms of student performance, are clearly not identified simply by higher expenditures. Within the range of current school operations, variations in expenditures or in other commonly identified determinants of school “quality” bear no systematic relationship to variations in the performance of students. Furthermore, the current structure of public education offers little hope for self-correction. Schools themselves seem incapable or unwilling to adjust programs and expenditures to evidence of performance, and parents as consumers do not appear to be a very effective force in leading schools to change.

These conclusions argue for reversing the movement toward blindly putting more resources into schools and for eliminating indefensible restrictions that are found in laws and contracts today. However, identifying problem areas or potential inequities in schools is insufficient, and we are left with a need to learn how to use schools as an effective policy instrument. In this regard, some fundamental changes in our perspectives on school policies seem required.

In the past, the main focus of educational policy and related research has been the development of a specific blueprint for “successful” schools, a blueprint that could be applied with little modification from one school to the next. Yet, despite extensive research, we are currently quite far from an adequate understanding of either the underlying determinants of scholastic performance or the characteristics of school decision-making. Indeed, the research on these issues has the character of a search for the Holy Grail—noble but futile (at least in the short run). It seems much more profitable to change our basic perspective, to think in terms of altering incentives and basing policies on performance, while admitting that we do not understand exactly what goes on in the classroom.

- APPENDIX** Armor, David, et al., *Analysis of the School Preferred Reading Program in Selected Los Angeles Minority Schools* (Santa Barbara, CA: The Rand Corp., August 1976).
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- NOTES** 1. The Wirtz Commission, in assessing the causes of declines in SAT performance, concludes that a majority of the decline up until about 1970 could be explained by compositional changes of those taking the

- test. However, subsequent pervasive declines in overall performance cannot be similarly explained. See College Entrance Examination Board, Advisory Panel on the Scholastic Aptitude Test Score Decline, *On Further Examination* (New York: College Entrance Examination Board, 1977).
2. A more extensive compilation of the scattered longitudinal information on test performance also supports this picture; see T. Anne Cleary and Sam A. McCandless, "Summary of Score Changes (in Other Tests)," mimeo (New York: College Entrance Examination Board, December 1976).
  3. James S. Coleman, Ernest Q. Campbell, Carol J. Hobson, James McPartland, Alexander M. Mood, Frederic D. Weinfeld, and Robert L. York, *Equality of Educational Opportunity* (Washington, DC: U.S. GPO, 1966).
  4. The 130 studies include separate (and nonduplicative) published estimates of educational performance models for distinct grade levels, samples of students or schools, and performance measures. To be included, a study had to investigate some aspect of teacher or school characteristics (in addition to other differences among students) and had to provide information about the statistical significance of results. (The latter criterion led to elimination of several studies, most notably the Coleman Report.) Sources of the included studies are found in the bibliography, and a description and critique of the methodology of these studies is found in Eric A. Hanushek, "Conceptual and Empirical Issues in the Estimation of Educational Production Functions," *Journal of Human Resources*, 14(3) (Summer 1979): 351–388.
  5. In 1976, 89 percent of school budgets went toward current operating expenditures with the remainder devoted to capital expenditures and interest on past debt. Of the current operating expenditures, 63 percent went toward instructional expenditures (teachers' salaries), 5 percent toward administrative expenditures, and the remainder toward maintenance, utilities, and other services.
  6. See David Greenberg and John McCall, "Teacher Mobility and Allocation," *Journal of Human Resources*, 9(4) (Fall 1974): 480–503.
  7. Richard Murnane, *Impact of School Resources on the Learning of Inner City Children* (Cambridge, MA: Ballinger, 1975).
  8. See Eric A. Hanushek, *Education and Race* (Lexington, MA: D.C. Heath, 1972) and "Families, Schools, and Achievement," Discussion Paper No. 7906, Public Policy Analysis Program, University of Rochester, 1979; Richard Murnane, *ibid.*, and David Armor, Patricia Conry-Oseguera, Millicent Cox, Nicelma King, Lorraine McDonnell, Anthony Pascal, Edward Pauly, and Gail Zellman, *Analysis of the School Preferred Reading Program in Selected Los Angeles Minority Schools* (Santa Monica, CA: The Rand Corporation, 1976).
  9. See Eric A. Hanushek, "Families, Schools, and Achievement," *ibid.*
  10. See the analysis of contract trends between 1970 and 1975 in Lorraine McDonnell and Anthony Pascal, *Organized Teachers in American Schools*, R-2407-NIE (Santa Monica, CA: The Rand Corporation, 1977). The increase in contract provisions related to nonmonetary aspects appears related to increased unionization of teacher bargaining, and teacher strikes over class size restrictions, layoff policies, and similar issues seem, at least by popular account, to be more common in recent years.
  11. For an insightful critique of analytical shortcomings of research into organizational behavior and the interactions of governmental

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suppliers and consumers, see Richard R. Nelson, *The Moon and the Ghetto* (New York: W.W. Norton and Company, Inc., 1977).

12. See Richard Murnane, *op cit.* and David Armor, *op cit.* Also see Paul Berman and Milbrey McLaughlin, *Federal Programs Supporting Educational Change, Volume I: A Model of Educational Change* (Santa Monica, CA: The Rand Corporation, 1974) for additional analyses of incentives for teachers.