

**Interpreting Recent Research on Schooling in Developing Countries**

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### **ABSTRACT**

Schooling policy in developing countries has frequently been viewed as necessitating an undesirable choice—provide broad access *or* provide high quality schools. Recent evidence, however, suggests that this is a very bad way to think about human capital development. Students respond to quality schools in ways that lessen existing inefficiencies, perhaps even sufficiently to recoup immediately investments in quality. Promoting high quality schools is, nonetheless, more difficult than many have thought. This difficulty suggests that inefficiency is only going to be tackled by introduction of substantial performance incentives in schools and by more directed evaluation of educational experiments. Incentives, decentralized decision making, and evaluation are, of course, very alien terms to education, in both developed and developing countries. Yet, they seem to hold the key to improvement that has eluded policy makers pursuing traditional policies.

## **Interpreting Recent Research on Schooling in Developing Countries**

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Recent research into schooling has begun to paint a consistent picture of education that relates directly to policy matters. The new policy, however, differs sharply from much that we have seen in the past. In particular, it points to more attention to performance incentives and less to regulatory and input-based policies. Moreover, the recent research underscores the importance of developing high quality schools, even if this appears to impinge on access to schools.

This discussion will concentrate primarily on questions of performance of schools and on alternative policies to improve performance. As such, it will not focus primarily on traditional questions of finance. Nevertheless, it will consider alternative organizational issues, and these have a variety of financial implications.

Three fundamental findings flow from the now-available research. First, education around the globe is a very inefficient exercise. Strong evidence indicates that too much is being paid for the performance obtained from schools. Second, education has proved to be a very complicated subject, and available research has not yielded very specific guidance on standard regulatory and spending policies. This basic state of research, I believe, calls for fundamental changes in the way we conceptualize educational policy. In particular, various forms of performance incentives appear to offer considerably greater hope for improving schools, even if we currently have little experience with developing such policies. Third, the importance of high quality schooling keeps coming up in the research. Developing countries have, in their rightful interest in expanding the availability of schools,

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tended to put in place very low quality school opportunities. This approach appears quite unfortunate, because students react to low quality schools in ways that damage the very policy of expanded access.

This paper first considers each of the three major findings. It then sketches a set of natural policies which flow from these findings.

## **I. Pervasiveness of Inefficiency**

It is natural to begin with what is known about the impact of differential resources on student performance. Much of school policy is traditionally thought of as an exercise in selecting and ensuring that the optimal set of resources, somehow defined, is available. Matched with this policy perspective has been a line of research considering the relationship between resource usage and student performance. If the effectiveness of different resources or combinations of resources were known, it would be straightforward to define an optimal set of resources. Moreover, we could often decide about policies that would move us toward such an optimal set of resources. Unfortunately, this alludes us.

Although research into the determinants of students' achievement takes various approaches, one of the most appealing and useful has been what economists call the *production function* approach, or in other disciplines the *input-output* or *cost-quality* approach. In this, attention is focused primarily on the relationship between school outcomes and measurable inputs into the educational process. This research has been most refined and most extensive in the United States. But there is now significant related evidence about performance in developing countries.

The underlying model guiding the analysis of school performance is very straightforward. It postulates that the output of the educational process—that is, the achievement of individual students—is related directly to a series of inputs. Policy makers directly control some of these

inputs—for instance, the characteristics of schools, teachers, and curricula. Others, those of families and friends plus the innate endowments or learning capacities of the students, generally cannot be affected by public policy. Further, although achievement is usually measured at discrete points in time, the educational process is cumulative; past inputs affect students' current levels of achievement.

Starting with this model, statistical techniques, typically some form of regression analysis, are employed to identify the specific determinants of achievement and to make inferences about the relative importance of the various inputs into student performance. The accuracy of the analysis and the confidence warranted by the answers depend crucially on a variety of issues regarding measurement and technical estimation. This summary sets aside these issues. Instead it highlights the overall findings, and subsequently relates these to potential policies toward schools.

Most studies of educational production relationships measure output by students' scores on standardized achievement tests, although significant numbers have used other quantitative measures, such as school attendance rates, and school continuation or dropout rates. The general interpretation is that they are all plausible indicators of future success in the labor market. (This interpretation is supported by a variety of studies).<sup>1</sup>

Empirical specifications have varied widely in details, but they have also had much in common. Family inputs tend to be measured by sociodemographic characteristics of the families such as parental education, income, wealth, and family size. Peer inputs, when included, are typically aggregate summaries of the sociodemographic characteristics of other students in the school. School

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<sup>1</sup>Evidence on the value of measured cognitive skills comes from a variety of sources. The evidence on direct returns to achievement differences in developing countries tends to be considerably stronger than for developed countries. The larger relative earnings implications of test differences could simply reflect shortages in minimal skills in LDCs. See, for example, Behrman and Birdsall (1983), Jamison and Moock (1984), Boissiere, Knight, and Sabot (1985), and Knight and Sabot (1987). In addition to the direct returns (rewards to achievement in general earnings models with levels of schooling), indirect returns also come from the increased probability of further attending school.

inputs include measures of the teachers' characteristics (education level, experience, sex, religion, and so forth), of the schools' organization (class sizes, facilities, administrative expenditures, and so forth), and of district or community factors (for example, average expenditure levels). Most empirical work in the United States has relied on data, such as the normal administrative records of schools, that were constructed for other purposes. In contrast, much of the work in developing countries has employed specialized data sets developed more for studying schooling.

Perhaps surprisingly, there is considerable similarity in the findings of this research across developed and developing countries. In fact, I will exploit some of the similarities in reaching conclusions about both the interpretation of the results and about the implications for policy.

There have been several different reviews of this literature, all telling a similar story.<sup>2</sup> The central conclusion is both simple and startling: The research of the past quarter century into educational input-output relationships has indicated clearly that schools around the world pursue very inefficient policies. This simple statement has strong implications for the course of policy, which will follow. Before delving into the policy statements, however, it is important to understand the nature of the evidence.

For the efficiency discussion, we employ the simplest possible notions of efficiency: do the resources purchased and employed by schools improve student performance? At this stage, we are not even concerned with the magnitude of any results, just whether there are positive returns to the application of school resources. The conclusions about inefficiency can be seen from Table 1, reproduced from Harbison and Hanushek (1992).

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<sup>2</sup>In developed countries, reviews include Hanushek (1986, 1989), Guthrie *et al.* 1971; Averch *et al.* 1974; Bridges, Judd, and Moock 1979; Murnane 1981; Glasman and Biniaminov 1981; and Murnane and Nelson 1984. For developing countries, see Fuller (1985, 1994), Harbison and Hanushek (1992), and Velez, Schiefelbein, and Valenzuela (1993).

A total of ninety-six underlying studies form the basis for the analysis.<sup>3</sup> Table 1 divides the available studies into statistically significant (by sign of coefficient, or direction of effect) and statistically insignificant. (The insignificant findings, unfortunately, cannot be divided by direction of effect.) The table duplicates similar compilations for the U.S. studies (Hanushek 1989; Harbison and Hanushek 1992). It begins with the characteristics directly related to instructional expenditure per student and then goes to other attributes of schools.

The evidence in table 1 from research in developing countries provides no support for policies of reducing class sizes. Of the thirty studies investigating teacher-pupil ratios, only eight find statistically significant results supporting smaller classes; an equal number are significant but have the opposite sign; and almost half are statistically insignificant. These findings qualitatively duplicate those in the U.S., but are particularly interesting here.<sup>4</sup> Class sizes in the developing-country studies are considerably more varied than those in the U.S. studies and thus pertain to a wider set of environments. The insignificance here provides even stronger evidence that the broad class size reduction policies so favored around the world do not make much sense.

The analysis of the effect of teacher experience yields results that are roughly similar to those in the U.S. studies. Although 35 percent of the studies display significant positive benefits from more

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<sup>3</sup>More recent reviews (Velez, Schiefelbein, and Valenzuela 1993) contain a larger number of cited studies, but the general conclusions I would draw are the same.

<sup>4</sup>The United States evidence about the unimportance of class size variations is even stronger. Econometric evidence through 1994 reveals 268 separate estimates of the effects of teacher-pupil ratios of which only 15 percent have positive and statistically significant impacts on student performance (Hanushek, Rivkin, and Taylor(1995)). An almost like proportion finds negative and statistically significant impacts. The econometric evidence is consistent with experimental evidence about the lack of a consistent relationship between class size and student performance; see Glass and Smith (1979), Word *et al.* (1990). Some debate about school resources and subsequent earnings follows from Card and Krueger (1992), but recent discussions further question the estimated relationships found there (see Betts 1994; Heckman, Layne-Farrar, and Todd 1994).

**Table 1**

**Summary of Estimated Expenditure Parameter Coefficients from  
96 Studies of Educational Production Functions: Developing Countries**

| Input               | Number of Studies | Statistically Significant |          | Statistically Insignificant |
|---------------------|-------------------|---------------------------|----------|-----------------------------|
|                     |                   | Positive                  | Negative |                             |
| Teacher/Pupil Ratio | 30                | 8                         | 8        | 14                          |
| Teacher Education   | 63                | 35                        | 2        | 26                          |
| Teacher Experience  | 46                | 16                        | 2        | 28                          |
| Teacher Salary      | 13                | 4                         | 2        | 7                           |
| Expenditure/Pupil   | 12                | 6                         | 0        | 6                           |
| Facilities          | 34                | 22                        | 3        | 9                           |

Source: Harbison and Hanushek (1992)

teaching experience (the analogous figure for U.S. studies is 29 percent), the majority of the estimated coefficients still are statistically insignificant.

The results for teacher education, on the other hand, diverge in relative terms from those seen for the United States. A majority of the studies (thirty-five out of sixty-three) support the conventional notion that providing more education for teachers is valuable. In the U.S. studies, teacher education provided the least support of all the inputs for the conventional wisdom. Although still surrounded by considerable uncertainty (since twenty-six estimates are insignificant and two display significantly negative effects), these noticeably stronger results in developing countries clearly suggest a possible differentiation by stage of development and general level of resources available.

The teacher salary findings in developing countries contain no compelling support for the notion that better teachers are systematically paid more. Since they aggregate studies across very different countries, school organizations, and labor markets, however, it is difficult to take these results too far. For policy purposes, one would generally want information on what happens if the entire salary schedule is altered (as opposed to simply moving along a given schedule denominated, say, in experience, education, or other attributes of teachers). It is not possible to distinguish, however, between studies reflecting differences in schedules and those reflecting movements along a schedule.

Data for total expenditure per pupil are rarely available in analyses of developing countries. The twelve studies for which estimates can be found are evenly split between statistically significant and statistically insignificant. Given questions about the quality of the underlying data, not too much should be inferred from the findings for direct expenditure measures.<sup>5</sup>

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<sup>5</sup>While data on expenditure are more plentiful in United States studies, the quality of these analyses is low (see Hanushek, Rivkin, and Taylor(1995)). They tend to be highly aggregated, to have relatively poor measures of school and family factors, and to be estimated in level form. Because of quality concerns, much more reliance is made of the results for teacher characteristics and teacher-pupil ratios. These factors largely determine variations in instructional expenditure per pupil,

One of the clearest divergences between the findings in developing and in developed countries is for facilities, again suggesting that differences in school environments are of some importance. The measures of facilities in developing countries (which incorporate a wide range of actual variables in specific studies) indicate more likely effects on student performance than found in U.S. studies. Some twenty-two of the thirty-four investigations demonstrate support for the provision of quality buildings and libraries. The specific focus of spending on facilities is unclear and almost certainly depends on local conditions.

In summary, the results of studies in developing countries do not make a compelling case for specific input policies. They do, however, indicate that some direct school resources might be important in developing countries, particularly in the provision of minimal school resources and attributes. Nevertheless, considerable uncertainty remains. Moreover, as in the U.S. research, the estimated models of educational performance undoubtedly fail to capture many of the truly important inputs to the educational process—something that will be documented below.

As with the U.S. studies, a variety of other factors has been investigated in the course of the developing-country analyses, including an assortment of curriculum issues, instructional methods, and teacher training programs. Many of these are difficult to assess (at least in a quantitative, comparative way) given the multicountry evidence and the probable importance of local institutions. One intervention that has widespread endorsement, although as much for conceptual reasons as for solid empirical ones, is the provision of textbooks. The relationship of textbooks and writing materials to student performance is found with reasonable consistency to be important in developing countries, but there are relatively few studies of this. Investigations of technological or organizational differences have led to mixed results. Because of scattered settlement in many rural areas, several approaches to "distance education" have been investigated. In three extensive investigations

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but they are available at the school and classroom level.

(Nicaragua, Kenya, and Thailand), the use of interactive radio has proved effective. However, this conclusion should not be generalized to all possible uses of new technology. In particular, there is little evidence at this time that the widespread introduction of computers is sensible (Lockheed and Verspoor 1991).

The available studies cover a wide range of schooling circumstances. They also look at a wide range of inputs into schooling. But they are also subject to considerable uncertainty. The standards of data collection and analysis are so variable that the results from this work tend to be quite uncertain. Much of the analysis of input-output relationships for developing countries is not published in standard academic journals, and thus it does not have that basic level of quality control. Even more important, the data for many of these studies do not come from regular collection schemes, are difficult to check for quality, and miss key elements of the educational process. Therefore, even if the analytical approaches are state-of-the-art, many questions will remain.

To supplement these findings, I have noted the similarity of the work for developing countries to that of the United States. A similar search of studies in the United States produces over 300 separate studies that related resources to student performance. These studies (see Harbison and Hanushek 1992; Hanushek, Rivkin, and Taylor 1995) come to many of the same conclusions. Little evidence suggests that smaller classes are better than large classes. Additionally, things that affect teacher salaries—particularly teacher education levels and teacher experience—are not systematically related to student performance. Neither are teacher salaries or aggregate differences in expenditure per student across schools.

These studies all point to serious inefficiencies. Schools, in both developing and developed countries, continue to pay for things that have little systematic pay-off in terms of student performance. In simplest terms, pursuing policies that are expensive but that have no return implies obvious inefficiencies.

Because this message conflicts with considerable conventional wisdom and because it has important policy implications, I want to make sure that it is understood.

First, these findings indicate that there are not very systematic relationships between key inputs and student performance. This does not say that there never is such a relationship. In fact there is reason to believe that each of these resources is sometimes productive. It is just that quite frequently, these resources are also ineffective. And there is no simple description of when and when not these resources will be effective.<sup>6</sup>

Second, and related, it does not say that resource differences could never be important, just that they have not been given the way schools are organized. Part of this could, in the case of developing countries, reflect variations across countries. There may be certain countries that are organized to promote performance and efficient resource usage. These effective countries might be lost in the pool of all countries where others are quite ineffective. Here, however, the similarities of results with the United States appear instructive, because the U.S. variation does not appear to reflect any simple differences in school organization.

Third, these findings do not imply that all schools are the same. Quite to the contrary, schools differ dramatically. These results suggest that the measurable factors, factors that often determine policy, are not related. This aspect of schools is taken up in the next section.

Fourth, some evidence exists to suggest minimal levels of basic school resources are important. The existing evidence tends to point to availability of textbooks, the provision of minimal facilities, and the like is important in student achievement (see Lockheed and Verspoor 1991; Lockheed and Hanushek 1988; Harbison and Hanushek 1992). These findings are not uniform in the

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<sup>6</sup>While controversy exists about the best way to summarize the results of existing econometric studies, there is complete agreement on the policy implications of the secondary analyses. The fact that some schools may use resources better than others is confirmed in the U.S. by Hedges, Lane, and Greenwald (1994). However, since no characterization of when resources are effective and when they are not is available, knowing this has no policy value (Hanushek 1994).

statistical analyses, but they are common enough to receive more attention than most of the other findings.

## **II. The Complexity of Education**

The foregoing evidence is generally unsatisfying to policy makers. What, they typically ask, can we do if providing more resources will not reliably lead to improvements in student performance? The preceding findings about resource effects have led to several continuing strands of research, each designed to aid policy makers in one way or another.

One line of research has attempted to add other measures of schools, of the educational process, of the preparation of teachers, and of other measurable factors related to schools and education. A recent, and particularly rich, example of consideration of a wide range of inputs is Glewwe, Grosh, Jacoby, and Lockheed (1993). Their study of Jamaican achievement suggests that various pedagogical factors may be important as student achievement responds to such things as the amount of time devoted to instruction or the use of written assignments. By looking at both "resource" and "process" factors, they suggest that the broader view is clearly preferable to the more narrow, resource concentration that has been more often pursued. This corresponds also to a recent review of school performance in developing countries by Fuller and Clarke (1994).

The idea behind this line of work is that prior research just has not yet found the right descriptors of education and schools. The overall notion is that we can build up to a more complete picture by adding measures that capture some of the things that appear to represent important differences. Such more complete descriptions could then be used to develop more refined policies. I am not, however, persuaded about the usefulness of either the research strategy implicit in these approaches or the policy implications that would necessarily follow.

Almost all studies that include enriched descriptions of schools, teachers, organization, and pedagogy find a set of these factors to be significantly related to student performance. These studies often conclude then by suggesting that policies directed at these factors could be beneficial. Moreover, these studies invariably point to the fact that the inclusion of better descriptions of what goes on in the classroom better explains achievement than resources alone. There are a different ways to look at these data, however.

First, the correct comparison is not between pedagogical variables and resource measures—because we have fairly conclusive evidence that measured resources are not systematically related to student performance. The better comparison is to true differences in teacher and school quality, whether measured or not. This distinction is very important. The measured resources do not give a clear indication of differences in quality of teachers or schools. Thus, it would not be surprising that an individual study could do better (in terms of significant relationships with achievement). But, do these factors truly explain a large portion of the systematic differences across schools? None of the available studies of other school factors address this issue.

Second, a large number of individual studies find "strong" evidence that some specific factor, say teacher training or the structure of instructional time, is important. This kind of finding is readily seen in Table 1. For example, a total of 35 studies (out of 63) find positive and statistically significant relationships between teacher education and student performance. Each of these, taken by itself, would give "strong" evidence. But looked at from the opposite vantage point, an almost equal number do not give very strong support of the conventional wisdom. This suggests that some skepticism is required in looking at any individual study, no matter how good the study is in a scientific sense. We are apt to be fooled by any individual study. The other factors in Table 1 suggest even greater skepticism about findings from single studies. One simple measure is to look at the separate estimated educational production functions often contained within a given study. For

example, some studies will estimate a relationship for math achievement and a relationship for reading achievement. Within individual analyses, where presumably the separate estimated relationships are highly correlated by virtue of being the same students, the same schools, and so forth, there is frequently little confirmation of any specific findings for resources or measured pedagogical factors. With one measure of outcome, for example, teacher's education level may be important while the same might not true for another outcome measure.

Third, and related to the previous point, generalizations are difficult, because the factors considered are quite idiosyncratic. Individual studies, because of peculiarities in the available data and different perspectives of the researchers, tend to pursue different sets of measures of school and teacher factors. The result is individual studies, each with its own "new findings." It would, of course, be foolhardy to think of making policy on the basis of these individual factors (and few of the original authors would actually suggest doing this).

My own interpretation of existing evidence, based on both U.S. and developing country results, is that important differences exist among schools but that we cannot describe what causes these differences very well. Let me provide a specific example through some recent work by Victor Lavy and myself (Hanushek and Lavy 1994). We investigated differences in school quality across a sample of Egyptian primary schools. We defined school quality implicitly. After allowing for individual differences among students in prior achievement and in parental education, schools that had large gains in student achievement in a given year are labelled high quality schools; those with small gains are low quality. Indeed, we developed a continuous measure of school quality by looking, in essence, at growth in student achievement (after considering family and other influences on achievement growth). In this exercise, we found enormous differences in school quality. For the 60 sampled schools, Table 2 indicates the variation in school quality by looking at achievement growth relative to an arbitrarily chosen base school. In both urban and rural areas, there is a huge variation

**Table 2. Distribution of Estimated School Quality in Egyptian Primary Schools  
(Proportional deviations from Taha Hussein School)**

|                | <b>All schools</b> | <b>Rural</b> | <b>Urban</b> |
|----------------|--------------------|--------------|--------------|
| <b>Mean</b>    | <b>-.084</b>       | <b>-.111</b> | <b>-.057</b> |
| <b>Minimum</b> | <b>-.62</b>        | <b>-.62</b>  | <b>-.52</b>  |
| <b>Maximum</b> | <b>.30</b>         | <b>.30</b>   | <b>.21</b>   |

Source: Hanushek and Lavy, 1994

in the quality of schools. Across all schools, the worst school has an average achievement gain 62 percent below the base school, while the best is 30 percent above. These results indicate dramatically that schools differ in quality, and differ in quality by enough to be very relevant to policy.

At the same time, measured attributes of teachers and schools explain only a small portion of the estimated differences in school quality. From our estimation, only 16 percent of the variance in school quality is related to the measures of teacher attributes (such as education and sex) and of school attributes (such as class size and facilities). While we did not look farther, I seriously doubt that adding more detailed measures of resources, or of pedagogy, or of curricular differences would do significantly better at explaining the observed school differences.

A similar approach to this was undertaken in rural Brazil (Harbison and Hanushek 1992). That work pointed to very similar conclusions: There are very large differences among schools in their ability to improve student achievement. These differences, nonetheless, are not highly correlated with measured characteristics of teachers and schools.

In short, the previous findings summarized in Table 1 do not indicate that schools and teachers are all the same. Very large differences exist, even though these differences are not captured by the simple measures commonly employed. Neither, it appears, are they captured by more detailed measures of classroom organization, pedagogical approach, and so forth.

This evidence all leads me to conclude that education is very complicated, and that we really do not understand it very well. We cannot describe what makes for a good or a bad teacher, or a good or a bad school. I even go beyond this. I do not believe that we are going to be able to describe the educational process very well in the near future. Some people suggest that educational policy could all be straightened out if we could just do the one great study. I see no evidence that this is the case.

My summary of the research is that we should learn to live with the fact that research will not be able to provide the definitive answer. Living with it to me implies finding policies that are built upon this fundamental ignorance.

### **III. Quality/Access Concerns**

A third major aspect of current and on-going research into schools relates to school quality and its central position in the consideration of policy. In particular, I am concerned about ideas related to school quality differences and perceived trade-offs between quality and access.

One way of motivating general concerns about school quality decisions is a traditional one something like the following. In making decisions about schools, there are limited budgets. If there are the commonly accepted two objectives of expanding access and of improving quality, these objectives will conflict because they must compete for the same budget. Thus, by this standard formulation policy makers are faced with a particularly unpleasant dilemma: choose between broad availability of schools and good schools.

There is a second way of motivating this that, while apparently different, is actually quite closely related. Analyses of labor market implications and the rate of return to schooling in developing countries suggest strongly that schooling is a very good investment. A year of schooling typically shows a 25-30 percent real rate of return. Such a return looks noticeably better than other investment alternatives. At the same time, school completion rates in low-income countries are very low (see, e.g., Lockheed and Verspoor 1991). These two facts do not go together. If it is such a high rate of return activity, why aren't people taking advantage of those high returns?

I am going to suggest that the developing work on school quality has something to say about both elements of education policy. First, I believe that the simple trade-off story about access and

quality is very misleading, if not wrong in important ways. In fact, in many circumstances there may not really be the trade-off suggested. Second, the unifying idea is that school quality may be an important explanation for the "strange" investment behavior that does not take advantage of the available high returns.

The central theme is that school quality is directly related to decisions about attending schools and to promotion through schools. High quality schools raise student achievement and speed students through primary (and perhaps secondary) schools, thus conserving on costs. Additionally, students respond to school quality in deciding whether or not to drop out of school. They tend to stay in high quality schools and drop out of low quality schools.

Both of these mechanisms indicate a direct relationship between the quantity of schooling attained and the quality of that schooling. Thus, studies of the rate of return to schooling which only consider quantity of schooling produce a misleading estimate of the potential gains. Estimation of the rate of return to schooling that does not account for quality differences will systematically overstate the productivity gains that are associated with additional years of schooling, because the estimates will include quality differences that are correlated with quantity. As documented below, those who do not complete a given level tend to have received lower quality schooling. If the policy simply pushes people to stay in school longer, without changing the fundamental quality of the schools, the newly-induced school completers will only get the returns associated with years of schooling and not with quality. Thus, they will not be able to gain as much as the rate of return estimates suggest.

The evidence on school quality and its relationship to school completion comes from studies in Brazil and in Egypt.<sup>7</sup> A simple summary of the key elements will fix the ideas.

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<sup>7</sup>The Brazilian work is summarized in Harbison and Hanushek (1992), Gomes-Neto and Hanushek (1994), and Hanushek, Gomes-Neto, and Harbison (1994). The Egyptian analysis is reported in Hanushek and Lavy (1994).

Many countries face very high grade repetition rates, frequently prompting direct interventions to ensure regular promotion through school (Lockheed and Verspoor 1991). Here the clear message is that repetition is best dealt with improving the quality of schools. In studying primary school students in the rural northeast of Brazil, Ralph Harbison and I discovered that there is a very direct relationship between what a student knows and the student's promotion probabilities. Students who learn more that is required by the curriculum (as measured by specially designed criterion referenced tests) are significantly more likely to be promoted through primary schools than those not learning what is expected. Combined with this, schools, not surprisingly, have an important impact on student achievement.<sup>8</sup> These findings suggest that policies that improve the quality of schools—that is, enhance student achievement—will simultaneously lead to more rapid progress by students through the grades. The magnitude of the overall effects, when converted to a monetary metric, is truly remarkable. If we consider investing \$1 in useful things like textbooks or better facilities, we can get an immediate savings of over \$10 from speeding people through schooling faster.<sup>9</sup> The savings, which are estimated based on the time savings in completion of a given amount of schooling, come from a series of underlying estimated promotion models and production functions. But, as seen in Table 3, they do not appear to be very sensitive to the uncertainty in the underlying estimates. Variations in software (books and writing materials) and hardware (school facilities) are consistently important. The bottom end of confidence intervals which allow for uncertainty of the

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<sup>8</sup>As discussed below, the specific components of schools that are estimated to increase student achievement are not necessarily the standard elements of educational policy. Specifically, common elements of teacher quality plus class size measures are not systematically related to student performance.

<sup>9</sup>Note that these savings are pure efficiency savings that include none of the increased productivity benefits that typically justify schooling investments. Increases in future productivity simply reinforce the efficiency gains.

**Table 3: Estimated Savings per \$1 Expenditure to Improve School Quality in Northeast Brazil**

|                                  | Point estimates | Interval estimates       |                   |                        |                   |
|----------------------------------|-----------------|--------------------------|-------------------|------------------------|-------------------|
|                                  |                 | Achievement coefficients |                   | Promotion coefficients |                   |
|                                  |                 | low <sup>a</sup>         | high <sup>b</sup> | low <sup>c</sup>       | high <sup>d</sup> |
| <b>YEARS SAVED</b>               |                 |                          |                   |                        |                   |
| Software                         | 0.42            | 0.12                     | 0.69              | 0.24                   | 0.58              |
| Hardware                         | 0.11            | 0.03                     | 0.18              | 0.06                   | 0.15              |
| <b>DOLLARS SAVED<sup>e</sup></b> |                 |                          |                   |                        |                   |
| Software                         | \$12.52         | \$3.54                   | \$20.58           | \$7.15                 | \$17.55           |
| Hardware                         | \$3.17          | \$0.80                   | \$5.48            | \$1.83                 | \$4.50            |

a. Lower bound estimated by setting software or hardware coefficients in models of Portuguese and mathematics from IV with selection models simultaneously at lower bound of 90 percent confidence interval.

b. Upper bound estimated by setting software or hardware coefficients in models of Portuguese and mathematics from IV with selection models simultaneously at upper bound of 90 percent confidence interval.

c. Lower bound estimated by setting Portuguese and mathematics coefficients in models of on-time promotion simultaneously at lower bound of 90 percent confidence interval.

d. Upper bound estimated by setting Portuguese and mathematics coefficients in models of on-time promotion simultaneously at upper bound of 90 percent confidence interval.

e. Years saved valued at US\$30 per student-year.

Source: Hanushek, Harbison, and Gomes-Neto, 1994.

effects of achievement on promotion and of school resources on achievement still indicate substantial efficiency gains from improving the quality of schools.

These results highlight the importance of providing minimal resources for schools. They are consistent with previous findings about the importance of basic textbooks, materials, and facilities. But these estimates—as startling as they are—may not represent the largest opportunities.

For the reasons described previously, I think these are really lower-bound estimates on the potential for change. Specifically, all of the research on Brazil (and elsewhere) points to the importance of the teacher.<sup>10</sup> The variations in teacher quality appear to be much more important than the variations in software or hardware. Unfortunately, as described above, we do not know how to hire particularly effective teachers. Nor do we know what it would cost. Therefore, we cannot calculate such straightforward benefit-cost ratios. Nevertheless, I think the returns from ensuring high quality teachers would be even greater than those identified in Table 3.

The Egyptian work that Victor Lavy and I have done pursues a related question. In simplest terms, it looks at whether school quality affects student dropout decisions. The school quality estimates, presented previously in Table 2, were included as one of the determinants of individual student decisions. Additionally, the analysis considered the student's own achievement and ability and the student's earnings opportunities outside of school. Holding constant own achievement and opportunities, students going to high quality schools are much more likely to stay in school than those going to low quality schools. This makes sense. If a student is not going to get anything out of school, why waste the time?

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<sup>10</sup>The basic findings of variations across teachers comes from attempts to estimate "total" teacher effects through general covariance models. In these variations in the average growth of student achievement across teachers is compared. The most conclusive estimates of variations in teacher quality are found in studies of U.S. schools (e.g., Hanushek (1971, 1992), Murnane (1975), Armor *et al.* (1976)). The Brazilian analysis considers differences across schools, but, because of the small size of schools, these differences are frequently associated with individual teachers (Harbison and Hanushek 1992).

The magnitude of the effect is particularly important. The sampled Egyptian primary schools had average dropout rates in 1980 of 9.3 percent. If all of the schools were at the quality level of the best, this dropout rate would drop to 3.2 percent or less. This indicates a truly huge impact of quality on school attainment.

The Brazilian and Egyptian stories have similar conclusions. School quality has large and direct effects on school access and school attainment. These effects are complements, not substitutes as suggested by the simple budgetary analysis that is commonly employed. And both indicate that working on the quality dimension is extremely important in thinking about schooling in developing countries. Finally, both must confront the policy challenges described in the earlier sections. Inefficiency and general lack of knowledge about the production function imply that dealing with quality will require new and innovative approaches.

Other evidence provides some confirmation. The work of Glewwe and Jacoby (1994) on Ghana shows the direct relationship between school quality factors and school attainment. Improving aspects of the schools, in their case the facilities, will tend to hold students in school longer, other things equal. They do not obtain estimates of total school quality effects (such as those in Egypt), but the indication that measured effects have this influence confirms the quantity-quality correlation. This in turn confirms the bias in rates of return flowing from analyses which ignore variations in school quality.

#### **IV. Policy Perspective with Inefficiency and Complexity**

The central reason for pursuing this research into educational performance, at least from a policy perspective, is to develop a list of inputs, curricular elements, or what-have-you that could be instituted through central policy. If there was a clear understanding of what determined student

performance, it would be possible to tell individual schools what to do. It would also be possible to insist on it in one way or another—through linking funding to specific actions, through regulating certain approaches, or through specific hiring decisions. Such a world has well-defined policy approaches and is easy to think about. Nevertheless, I believe that the available research indicates that it is the wrong approach.

If pursuing policies suggested by the conventional wisdom are uncertain and quite unlikely to lead to overall improvements, what can be done? More, if the process is too complex to understand given current knowledge and research, where can we turn?

In reality there are a number of policies that look to be viable alternatives. The fundamental change is to switch from thinking about input-based policies to thinking about performance-based policies. Performance-based policies would be ones that reward accomplishment of objectives—such as high reading skills or adequate numeracy skills. The policies would specify end goals, provide carrots and sticks related to them, and harness the energies of the actors in the system. The policies, on the other hand, would not specify how things should be done.

What kinds of systems fit into the category? Merit pay for teachers where they are rewarded for what students learn is a simple example of a performance incentive system. It is also an example of a system with little historical experience indicating true success.<sup>11</sup> Other variants of performance based rewards within current systems have been suggested, although rarely evaluated. Various private contracting arrangements are available. In these, people are hired to run certain parts of schools with rewards based on performance of stated objectives. Choice and vouchers are a more extreme version of this. Parents choose schools, and the concomitant flow of resources provides incentives to produce what the parents desire. The unifying theme is that good performance is rewarded while poor performance is not.

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<sup>11</sup>For the United States, see Cohen and Murnane (1986).

This is not a message aimed solely to developing countries. It is the same message that developed countries are beginning to hear and to respond to. Developing countries face the same inefficiencies and uncertainties about the educational process. Both the central ideas and the uncertainties are set out for U.S. schools in *Making Schools Work: Improving Performance and Controlling Costs* (Hanushek with others 1994), which describes the recommendations of a panel of economists on how economic logic can be introduced into the reform of schools.

This is also not a particularly pleasing message for policy makers, because it requires considerable changes from the current state of policy development. Policy makers frequently wish to maintain control of policies and are distrustful of the motives and abilities of local decision makers. Thus, they frequently look for explicit approaches that can be centrally set, and accepting the decentralized decision making that almost certainly comes with performance incentives requires a very different focus.

Moving to performance incentives involves considerable uncertainty. We now have considerable evidence that the current structure is not working appropriately or effectively, but we have little experience with the alternatives. Additionally, in order to apply such a system, we must be able to specify desirable goals and to measure performance toward those goals. This is hard, and sometimes contentious. Finally, it will take considerable experimentation and evaluation of results. These are not things most schools, whether in developed or developing countries, do routinely or easily.

There are a variety of nascent experiments—such as movements to the use of vouchers in Milwaukee, Wisconsin, in Chile, and in Columbia—where some of these ideas are being tried. Given current knowledge, however, we do not really know much about how to institute such approaches. Indeed, my policy recommendation is not a specific approach but a plan of systematic experimentation

and evaluation with alternatives. This, I believe, is the main message of the existing research into the determinants of student performance.

The full description of alternatives, of experimentation, and of evaluation approaches goes considerably beyond the scope of this paper (see Hanushek with others 1994). These topics are being debated extensively around the world. The conclusion here is a simple one: More energy must go into developing new organizational forms if education in developing countries is to improve.

## **V. Evaluation and Innovation**

One consistent observation about schooling around the world is the slowness with which the educational system itself learns. While schools are dedicated to learning activities on the part of students, they do not believe that they should go through the often painful process of evaluation and learning themselves. Therefore, new ideas and approaches are seldom subjected to thorough evaluation and to active decisions about their success or failure. This is a truly unfortunate state of affairs, particularly in the context of developing countries. Because of expansions and attempts to improve the educational systems of developing countries, many natural experiments are currently being conducted. Indeed the World Bank and other international agencies who support the improvement of developing countries' educational systems — both financially and intellectually — frequently insist on new approaches to the organization and conduct of schooling. Even more experimentation comes from the nations' own attempts to improve their educational systems. Yet little systematic information is collected with these efforts, and even less of an attempt is made to evaluate the information.<sup>12</sup>

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<sup>12</sup>The World Bank regularly requires evaluation of loans in which it participates, but these evaluations seldom involve any detailed analytical work that would permit dissemination of new techniques or new organizational forms. For example, see Harbison and Hanushek (1992) on

This state of educational policy might be understandable and acceptable if the results were better. If international organizations and individual countries tended to make good decisions on the effective use of resources and provision of educational services, concern about evaluation and systematic learning would be considerably less. But the evidence we have suggests that the process is not one that leads to confidence, no matter how strongly people believe that the next change will surely lead to improvement.

The programs of the World Bank, which have increasingly been focused on developing the human capital of developing countries, provide a natural array of educational interventions that could, conceptually, provide sorely needed information about productive lines of improvement. But, as most school children know, learning is not easy. It takes work. So it is with understanding educational interventions. Early commitment to evaluation of experiments is essential, but it is alien to the educational systems of most countries.

An additional implication of these suggestions is that much more serious assessment efforts will be required in a variety of countries. Many countries do little systematic testing and evaluation of the performance of their students. This lack of testing and measurement is incompatible with the development of new and improved educational systems. Clearly this is not a small issue, because the development and evaluation of appropriate testing and measurement is itself the subject of intense discussion. Nonetheless, this step cannot be ignored if nations are to develop the knowledge and organizations required for effective educational systems.

## **VI. Implementation Issues**

The simple explanation for the current state of inefficiency is that there are few if any incentives to get higher student performance from added resources. When there are no direct

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evaluation design.

incentives to increase achievement and when education is so heavily influenced by actions in the individual classroom and school, it is not particularly surprising that the system responds as it does. Added resources may or may not be converted into improved performance, based on specific local personnel and factors.

But this explanation begs the larger questions about why there are not built-in performance incentives in the system. While most schooling systems are dominated by publicly provided schools, this by itself would not seem to preclude the development of incentive systems. The common explanations center on some combination of bureaucratic incentives and union restrictions. Each has a basis in important actors pursuing their own self-interest. These motivations are combined with power in decision making about schools. Nonetheless, while these explanations are commonly offered to explain the poor incentives that exist, little explicit analysis of them exists.

Consideration of the source of inefficiency is important because improvement requires changing the existing structure. If the inherent source of inefficiency and of resistance to change is the self-interest of current actors, any reform and improvement program must take this into account. One of the most obvious elements of this is that current teachers must be incorporated into any program of change. The specific approach clearly depends on the new organizational structure that is introduced, but an important element might be to protect existing teachers from arbitrary changes. Something like two-tier contracts, where current teachers are retained under current contracts and new teachers face contracts with very different incentives, offer a way of moving from current systems to new, more responsive systems.

Fair and equitable treatment of existing school personnel is essential even if the long run plan is to replace the majority of existing personnel. Existing teachers will be important over some considerable period of transition, because adjustments must in most conceivable situations be built upon the existing system. And, given the continued importance of the individual teacher and

headmaster, it is typically not possible to force new methods from above without also introducing considerable inefficiency.

If the root problem involves existing government bureaucracy, it is more difficult to see how parallel adjustments of the nature of two-tier contracts can be made. Strong leadership from the top is frequently an essential element. The efficacy of such an approach depends crucially on the specific circumstances of individual countries and local structures, but obviously nothing is likely on a wide spread basis without more central leaders setting high performance goals. There is, of course, a qualitative difference between existing teachers and existing bureaucracies because teachers have much more direct control over student performance and thus are much more easily influenced by student performance incentives. The bureaucracies on the other hand have a more indirect role through facilitating performance of schools, monitoring actions of local schools, and the like. Therefore, leadership requires more complicated reward structures. At this same time, leadership can be exerted through considerably expanded incentives to governmental bureaucrats. Current governmental agencies seldom have much in the way of performance rewards, partly because of limited attention to measuring agency outcomes. The tendency toward insulation of government agencies from performance incentives is frequently reinforced by public employee unions which resist individual differentiation of workers. Finally, however, the introduction of competitive supply may introduce implicit incentives that aid in achieving reform. The conceptual basis for educational vouchers, which permit students to go to either public or private schools, is that competition for students (and the associated resources that go with students) could force schools to be more responsive to student achievement and to efficiency concerns. At the same time, educational vouchers might also offer competition to much of the schooling bureaucracy—i.e., the portion of the bureaucracy involved with direct provision of public schooling—and this competition might have similar incentive effects for

bureaucrats as for teachers. (The bureaucracy involved with administering and monitoring the vouchers would, of course, not feel these competitive pressures).

The overall point is that issues of political economy and the existing school personnel cannot be ignored in any change of organization and incentives in the schools. The schools arrived at their current state with their current personnel, and it is unrealistic to believe that the current personnel will automatically adopt new structures which increase employment risks, which eliminate some of the implicit benefits to their current employment, and which potentially require new and more difficult work. Instead, either these people must be brought into the process of change or mechanisms must be developed to ensure that they do not sabotage any changes.

As noted, most incentive systems require enhanced local initiative. In a wide variety of situations today, local schools do not appear to have sufficient capacity. Little is known about how best to develop such capacity, but clearly trying to replace local capacity with central directive has proved quite difficult.

## **VII. Conclusions**

As I look at the research into the educational process, both in the United States and in the developing world, I see some very distinct pay-offs. In policy dimensions, it appears that we are learning a lot. At the same time, it is not always what was expected when the research was begun.

I think that the research quite conclusively demonstrates an inefficiency in the current organization of schools. Resources are being spent in unproductive ways, ways that do not contribute to student performance. At the same time, correcting these inefficiencies is not simple. There is no blueprint for the "good school" that can be reproduced and handed out to policy makers. In fact, the research has demonstrated to me that we are unlikely to develop such a blueprint in the near future.

Instead, I believe that we must turn toward new organizations and new incentives if we are to improve schools.

In my opinion the most likely path to improvement involves the introduction of performance incentives. While a variety of ways of doing this have been suggested, none have been tried extensively. This implies that an extensive program of experimentation and evaluation is in order. None of this, however, is particularly easy.

Finally, the research underscores the importance of school quality. While there are some difficult questions of translating this into policy, I think there are powerful reasons to believe that this should be a very high item on the policy agenda. Continued expansion of low quality schooling options—often thought to be a on a path to broadly provided but high quality schools—may actually be a self-defeating strategy.

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