Do Better Schools Lead to More Growth? Cognitive Skills, Economic Outcomes, and Causation*

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Abstract

We investigate whether a causal interpretation of the robust association between cognitive skills and economic growth is appropriate and whether cross-country evidence supports a case for the economic benefits of effective school policy. We develop a new common metric that allows tracking student achievement across countries, over time, and along the within-country distribution. Extensive sensitivity analyses of cross-country growth regressions generate remarkably stable results across specifications, time periods, and country samples. In addressing causality, we find, first, significant growth effects of cognitive skills when instrumented by institutional features of school systems. Second, home-country cognitive-skill levels strongly affect the earnings of immigrants on the U.S. labor market in a difference-in-differences model that compares home-educated to U.S.-educated immigrants from the same country of origin. Third, countries that improved their cognitive skills over time experienced relative increases in their growth paths. From a policy perspective, the shares of basic literates and high performers have independent significant effects on growth, and the estimates suggest that the high-performer effect is larger in poorer countries.

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Schooling and human capital investments have been a central focus of development policy, but doubts have arisen as disappointments with results grow. Nowhere is this more apparent than in the case of growth policy, where schooling investments have not appeared to return the economic outcomes promised by theoretical growth models. While prior analyses have investigated various issues in the specification of empirical cross-country growth models, a warranted scepticism about the identification of causal effects remains. Our analysis suggests, however, that the most significant problem underlying these prior concerns is the valid measurement of skill differences across countries and that, once remedied, a strong case can be made for identification of true causal impacts in more elaborate econometric specifications.

As a simple summary observation, world policy attention today focuses on the lagging fortunes of Sub-Saharan Africa and of Latin America. Considerably less attention goes to East Asia, and, if anything, East Asia is proposed as a role model for the lagging regions. Yet to somebody contemplating development policy in the 1960s, none of this would be so obvious. Latin America had average income exceeding that in Sub-Saharan Africa and the Middle East and North Africa regions, and both of these exceeded East Asia (see Appendix Table A1).[1] Further, Latin America had schooling levels that exceeded those in the others, which were roughly equal. Thus, on the basis of observed human capital investments, one might have expected Latin America to pull even farther ahead while having no strong priors on the other regions. The unmistakable failure of such expectations, coupled with a similar set of observations for separate countries in the regions, suggests scepticism about using human capital policies to foster development. But, this scepticism appears to be more an outgrowth of imperfect measurement of human capital investments than an empirical reality.

The measurement issues become apparent when we introduce direct measures of cognitive skills from international tests of math and science into the growth picture. The entire picture changes. Figure 1 plots regional growth in real per capita GDP between 1960 and 2000 against average test scores after conditioning on initial GDP per capita in 1960.[2] Regional annual

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[1] Japan was significantly ahead of the rest of the East Asia region, but its exclusion does not change the regional ordering (see Appendix Table A1).

[2] Regional data come from averaging all countries with available data in a region. The 50 countries are not chosen to be representative but instead represent the universe of countries that participated in international tests and had available the requisite economic data. Still, Appendix A shows that the average 1960 incomes for all countries in each region are quite similar to those for our subset of countries. The division of Europe into three regions illustrates the heterogeneity within OECD countries, but a combined Europe also falls on the line in Figure 1.

growth rates, which vary from 1.4 percent in Sub-Saharan Africa to 4.5 percent in East Asia, fall on a straight line with an R^2=0.985. But, school attainment, when added to this regression, is unrelated to growth-rate differences. Figure 1 suggests that, conditional on initial income levels, regional growth over the last four decades is completely described by differences in cognitive skills. But should we interpret the tight relationship between cognitive skills and growth as reflecting a causal relationship that can support direct policy actions?

Because of the limited data and multiplicity of factors affecting observed international economic outcomes, little headway has been made in clarifying the underlying causal structure of growth. Many have simply been convinced by demonstrations of the general sensitivity to alternative samples and specifications of cross-country empirical models that these are not fruitful policy investigations. Perhaps the strongest evidence on causality has been related to the importance of fundamental economic institutions using identification through historical factors (Acemoglu, Johnson, and Robinson (2001, 2005)), but this has not yielded clear advice about the kinds of feasible policies that will lead to national payoffs, and it itself has been subject to question (Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004)).

We approach these issues from a number of angles and conclude that there is strong evidence that the influence of cognitive skills described in Figure 1 can reasonably be interpreted as causal. Each of our individual approaches to the issue addresses a different set of the potential concerns about causation. While each may be individually inconclusive – not least because data limitations set clear limits for analyses identified by cross-country variation – the set of consistent findings provides much more persuasive evidence on the issue.

A related aspect of these separate causal investigations is the pinpointing of a specific policy role for improved school quality. While variations in cognitive skills can arise from various influences – families, culture, health, and ability – we provide evidence that schools are one avenue for improvement available to policy makers.

The overall investigation relies on a new series of cognitive skills for 50 countries (Section III) that is central to our evaluation of the impact of human capital. We first show that the relationship between cognitive skills and economic growth is extraordinarily robust to alternative samples defined by different time periods and sets of countries and to different specifications of the skills measure and of the growth relationship (Section IV). We then turn to a series of investigations that follow approaches now common in microeconometric studies.

To avoid potential reverse causation problems and to pinpoint the possible role of school policies, we instrument our test-score measures by characteristics of the school systems in the countries (Section V). This approach provides information on how variations in student outcomes that are related to institutional school policies affect growth and supports the potential for school policies to affect economic growth.

The aggregate instruments of institutional features could still be correlated with other features of economies such as cultural variations, health differences, or country-specific efficiency differences found both in the economy and in the organization of schools. Therefore, we develop two approaches in the spirit of a difference-in-differences analysis that separately factor out different potential threats to the identification of the impacts of cognitive skills.

The first difference-in-differences approach focuses on U.S. labor-market outcomes for immigrants (Section VI). We consider whether schooling-based cognitive-skill differences between immigrants educated at home and educated in the U.S. are related to how well a country's immigrants will do economically, holding constant cultural and other differences by country-of-origin fixed effects. Because we consider earnings in a single economy, country differences in basic economic institutions, the structure of industry, and the like are held constant, permitting clearer identification of the economic returns to cognitive skills. The results show that aggregate country differences in cognitive skills directly affect individual wages of immigrants – but only if they received their schooling in their home country.

The second difference-in-differences approach, using the intertemporal dimension of our newly devised database, ignores variations in levels of student achievement across countries altogether and investigates whether countries that have improved their cognitive skills over time have seen commensurate increases in their growth rates (Section VII). This analysis shows a close association between secular improvements in cognitive skills and increases in growth rates over time across OECD countries.

A final issue is that average test scores do not adequately reflect the range of policy options. Specifically, one could institute policies chiefly directed to the lower end of the cognitive distribution, such as the Education for All initiative, or one could aim more at the top end, such as the focused technological colleges of India. We investigate the impact on growth of a concentration on either end of the achievement distribution along with how these might interact with the nation's technology, enabled by the within-country-distribution dimension of our new database (Section VII). We find improving both ends of the distribution to be beneficial and complementary. The importance of the highly skilled is even more important in developing countries that have scope for imitation than in developed countries that are innovating.

In sum, the simple premise that improving the schools can produce benefits in national growth rates is strongly supported.

I. Existing Literature on Schooling and Growth

Recent interest in economic growth has led to an upsurge of empirical analyses of why some nations grow faster than others. The standard method for establishing the effect of education on economic growth is to estimate cross-country growth regressions where countries' average annual growth in gross domestic product (GDP) per capita over several decades is expressed as a function of measures of schooling and a set of other variables deemed to be important for economic growth. Following the seminal contributions by Barro (1991, 1997) and Mankiw, Romer, and Weil (1992), a vast literature of cross-country growth regressions, mostly using the important internationally comparable data on average years of schooling provided by Barro and Lee (1993, 2001) as the proxy for the human capital of an economy, has tended to find a significant positive association between quantitative measures of schooling and economic growth.[3] Various branches of subsequent work have attempted to distinguish among alternative mechanisms behind this association and have delved into measurement and specification issues – while generally supporting a role for schooling in determining growth.[4]

But, all analyses using average years of schooling as the human capital measure in cross-country analyses implicitly assume that a year of schooling delivers the same increase in knowledge and skills regardless of the education system. For example, a year of schooling in

[3] For extensive reviews of the literature, see Topel (1999), Krueger and Lindahl (2001), and Pritchett (2006). The robustness of the association is highlighted by the extensive analysis by Sala-i-Martin, Doppelhofer, and Miller (2004): Of 67 explanatory variables in growth regressions on a sample of 88 countries, primary schooling turns out to be the most robust influence factor (after an East Asian dummy) on growth in GDP per capita in 1960-1996.
[4] Hanushek and Woessmann (2008) discuss the alternative branches and their findings. These investigations are driven by varying, and contradictory, theoretical models about different mechanisms through which education may affect economic growth. First, education may increase the human capital of the labor force, which in turn increases labor productivity (as in augmented neoclassical growth theories; see, for example, Mankiw, Romer, and Weil (1992)). Second, education may increase the innovative capacity of the economy (as in theories of endogenous growth; see, for example, Lucas (1988), Romer (1990), Aghion and Howitt (1998)). Third, education may facilitate the diffusion and transmission of knowledge needed to implement new technologies (see, for example, Nelson and Phelps (1966), Welch (1970), Benhabib and Spiegel (2005)). Jamison, Jamison, and Hanushek (2007) provide some suggestive tests among these alternatives, and our estimates below relate to the predictions of the different models.

Peru is assumed to create the same increase in productive human capital as a year of schooling in Japan. Equally as important, this measure assumes that formal schooling is the primary source of education and that variations in the quality of nonschool factors have a negligible effect on education outcomes.

An alternative perspective, developed over the past ten years, concentrates directly on cognitive skills. This work, which forms the foundation for our current analysis, alters the assessment of the role of education in the process of economic development significantly. When using data from the international student achievement tests through 1991 to build a measure of cognitive skills, Hanushek and Kimko (2000) find a statistically and economically significant positive effect of cognitive skills on economic growth in 1960-1990 that dwarfs the association between years of schooling and growth. In their model, adding cognitive skills to a base specification including only the initial level of income and years of schooling increases the variance in average annual growth rates of real GDP per capita that can be explained by the model from 33 to 73 percent among the 31 countries in their sample. At the same time, the effect of years of schooling is greatly reduced by including cognitive skills, leaving it mostly insignificant, while adding a variety of other factors leaves the effects of cognitive skills basically unchanged. The general pattern of results is duplicated by a series of other studies that pursue different tests and specifications along with different variations of skills measurement.[5]

However, the extent to which the associations found in these studies can be interpreted as a causal effect of cognitive skills remains unclear. Questions about the identification of underlying causal effects in cross-country growth models have existed for a long time. Beginning with the analysis of Levine and Renelt (1992), evidence of the sensitivity of results to model specification has been plentiful. In terms of schooling, Bils and Klenow (2000) provide convincing evidence of the endogeneity of school attainment in growth models. Further, it is unclear to what extent prior attempts to deal with endogeneity such as the panel data approaches of Barro (1997) or Vandenbussche, Aghion, and Meghir (2006) have been successful in a setting where the dominant information is in the cross-country variation.[6]

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[5] Detailed discussion of the innovations and findings of past work is available in Hanushek and Woessmann (2008).
[6] Aghion, Boustan, Hoxby, and Vandenbussche (2005) approach causality by relying on within-country variation.

II. A Simple Growth Model with Cognitive Skills

We begin with a very simple growth model: a country's growth rate (g) is a function of the skills of workers (H) and other factors (X) that include initial levels of income and technology, economic institutions, and other systematic factors. Skills are frequently referred to simply as the workers' human capital stock. For simplicity in equation (1), we assume that H is a one-dimensional index and that growth rates are linear in these inputs, although these are not important for our purposes.[7]

(1) $$g = \gamma H + \beta X + \varepsilon$$

It is useful at this stage to understand where the skills (H) might come from. As discussed in the extensive educational production function literature (Hanushek (2002)), these skills are affected by a range of factors including family inputs (F), the quantity and quality of inputs provided by schools (qS), individual ability (A), and other relevant factors (Z) which include labor market experience, health, and so forth as in:

(2) $$H = \lambda F + \phi(qS) + \eta A + \alpha Z + \nu$$

The schooling term combines school attainment (S) and its quality (q).

Human capital is nonetheless a latent variable that is not directly observed. To be useful and verifiable, it is necessary to specify the measurement of H. The vast majority of existing theoretical and empirical work on growth begins – frequently without discussion – by taking the quantity of schooling of workers (S) as a direct measure of H.

In our opinion, a more satisfying alternative is to focus directly on the cognitive skills component of human capital and to measure H with test-score measures of mathematics, science, and reading achievement.[8] The use of measures of cognitive skills has a number of potential advantages. First, they capture variations in the knowledge and ability that schools strive to produce and thus relate the putative outputs of schooling to subsequent economic success. Second, by emphasizing total outcomes of education, they incorporate skills from any source –

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[7] The form of this relationship has been the subject of considerable debate and controversy. As we write it, it is consistent with a basic endogenous growth model. We generally keep this formulation, in part because we cannot adequately distinguish among alternative forms.

[8] Some researchers have suggested that test scores should be thought of as a measure of school quality (q), leading to use of test scores times years of schooling as a measure of H, but this ignores the influence of family factors and other elements of equation (2) that have been shown to be very important in determining cognitive skills.

families, schools, and ability. Third, by allowing for differences in performance among students with differing quality of schooling (but possibly the same quantity of schooling), they open the investigation of the importance of different policies designed to affect the quality aspects of schools.[9]

III. Consistent International Measures of Cognitive Skills

This analysis starts with the development of new measures of international differences of cognitive skills. The measures developed here extend those developed in Hanushek and Kimko (2000) to add new international tests, more countries, and intertemporal and within-country dimensions. They also deal with a set of problems that remained with the early calculations.

Between 1964 and 2003, twelve different international tests of math, science, or reading were administered to a voluntarily participating group of countries (see Appendix Table B2). These include 36 different possible scores for year-age-test combinations (e.g., science for students of grade 8 in 1972 as part of the First International Science Study or math of 15-year-olds in 2000 as a part of the Programme on International Student Assessment). Only the United States participated in all possible tests.

The assessments are designed to identify a common set of expected skills, which were then tested in the local language. It is easier to do this in math and science than in reading, and a majority of the international testing has focused on math and science. Each test is newly constructed, usually with no effort to link to any of the other tests.

We wish to construct consistent measures at the national level that will allow comparing, say, math performance of 13-year-olds in 1972 to that in 2003. This would permit us to compare performance across countries, even when they did not each participate in a common assessment, as well as track performance over time. It would also provide the ability to aggregate scores across different years, ages, and even subjects as appropriate. The details of this construction along with the final data are found in Appendix B. Here we simply sketch the methodology.

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[9] Some recent work has introduced the possibility that noncognitive skills also enter into individual economic outcomes (see importantly Bowles, Gintis, and Osborne (2001), Heckman, Stixrud, and Urzua (2006), and Cunha, Heckman, Lochner, and Masterov (2006)). Hanushek and Woessmann (2008) integrate noncognitive skills into the interpretation of general models such as above and show how this affects the interpretation of the parameter on school attainment and other estimates. While there are no agreed-upon measures of noncognitive skills, at the aggregate level they might well be incorporated in "cultural differences," something that we address in the analysis below.

Because the test distribution is normal within the OECD sample, our construction of aggregate country scores focuses on transformations of the means and variances of the original country scores in order to put them each into a common distribution of outcomes.

Test-Score Levels across Assessments. Comparisons of the difficulty of tests across time are readily possible because the United States has participated in all assessments and because there is external information on the absolute level of performance of U.S. students of different ages and across subjects. The United States began consistent testing of a random sample of students around 1970 under the National Assessment of Educational Progress (NAEP). By using the pattern of NAEP scores for the U.S. over time, it is possible to equate the U.S. performance across each of the international tests.

Test-Score Variance across Assessments. The comparison of performance of other countries to the U.S. requires a distance metric for each test. Each assessment has varying country participation and has different test construction so that the variance of scores for each assessment cannot be assumed to be constant. Our approach is built on the observed variations of country means for a group of countries that have well developed and relatively stable educational systems over the time period.[10] We create the "OECD Standardization Group" (OSG) by using the thirteen OECD countries that had half or more of the relevant population attaining a secondary education in the 1960s (the time of the first tests). For each assessment, we then calibrate the variance in country mean scores for the subset of the OSG participating to the variance observed on the PISA tests in 2000 (when all countries of the OSG participate). The identifying assumption of this approach is that the *variance* in the mean performance among a group of relatively stable education systems does not change substantially over time.

By combining the adjustments in levels (based on the U.S. NAEP scores) and the adjustment in variances (based on the OECD Standardization Group), we can directly calculate standardized scores for all countries on all assessments. Each age group and subject is normalized to the PISA standard of mean 500 and individual standard deviation of 100 across OECD countries. We can then aggregate scores across time, ages, and subjects as we desire.

[10] The development of aggregate scores by Hanushek and Kimko (2000) and by Barro (2001) assumed that the test variances across assessments were constant, but there is no reason for this to be the case. Our approach is in the spirit of Gundlach, Woessmann, and Gmelin (2001).

IV. Stability of the Cognitive Skills-Growth Relationship

The basic growth model in equation (1) is estimated for the 50 countries with cognitive-skill and economic data over the period 1960-2000.[11] Cognitive skills are measured by the simple average of all observed math and science scores between 1964 and 2003 for each country.

As a comparison to prior cross-country analyses, the first column of Table 1 presents estimates of a simple growth model with school attainment.[12] While this model explains one-quarter of the variance in growth rates, adding cognitive skills increases this to three-quarters of the variance. The test score is strongly significant with a magnitude that is unchanged by excluding school attainment (col. 2), including initial attainment in 1960 (col. 3), or including average attainment over the period (col. 4). School attainment is never statistically significant in the presence of the direct cognitive-skill measure of human capital.

One standard deviation in test scores (measured at the OECD student level) is associated with a two percentage points higher average annual growth rate in GDP per capita across 40 years.

The remaining columns of Table 1 provide alternative perspectives on these basic results. Estimating the model with regression techniques robust to outliers yields virtually identical coefficient estimates to those including Nigeria and Botswana, the two significant outlier countries in the growth equation (col. 5).[13] Because the robust model assigns essentially zero weight to these two observations, they are dropped from the remaining models. Including fixed effects for the eight world regions depicted in Figure 1 (so that no between-region variation in test scores is used in the estimation) reduces the estimated test effect to 1.7 (col. 6).

The final two columns consider economic institutions. Acemoglu, Johnson, and Robinson (2001, 2005) argue that historic factors surrounding the colonization of nations affected economic institutions and that we can thus isolate the causal impact of institutions on growth. On the other hand, Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004) argue that the colonists brought human capital in addition to knowledge of good societal institutions and that it

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[11] See Appendix B for details on the country sample. The source of the income data is version 6.1 of the Penn World Tables (cf. Heston, Summers, and Aten (2002)). The data on years of schooling are an extended version of the Cohen and Soto (2007) data. Descriptive statistics are found in Appendix Table C1.

[12] While not the focal point of this analysis, all specifications include GDP per capita in 1960, which provides consistent evidence for conditional convergence, i.e., countries with higher initial income tend to grow more slowly.

[13] The specific robust regression technique reported is Stata's *rreg* command, which eliminates gross outliers with Cook's distance measure greater than one and iteratively downweights observations with large absolute residuals. The OLS estimate of the test effect in the 52-country sample is 1.752 (*t*-statistic 5.75). Nigeria and Botswana each participated only in a single international test.

is more likely that better human capital led both to the development of good institutions and higher economic growth. This latter perspective highlights the difficulty of using measures that reflect economic institutions from near the end of the observed growth period and that might better be thought of as outcomes of growth itself. We add institutional differences for openness of the economy and security of property rights (col. 7)[14] and for these plus fertility rates and location in the tropics (col. 8) into our growth models.[15] These reduce the estimated test-score effect to around 1.25, but the effect of cognitive skills remains strongly statistically significant. In the spirit of the Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004) critique, we interpret the reduced estimates of test scores as a lower bound on the true effect, since the institutional measures include any direct effects of cognitive skills on the development of good institutions. Finally (not shown), the stock of physical capital per adult in 1960 does not enter the basic growth model significantly and does not affect the test-score coefficient.

While the estimated effect of test scores varies some across these different specifications, the cognitive-skill coefficients are always very significant and the variation is quite limited: A move of one standard deviation of individual student performance translates into 1.2-2.0 percentage points difference in annual growth rates, other things equal. How much is one standard deviation in performance? The difference between the U.S. average and the top performers on the PISA tests is approximately 0.4 standard deviations, while the difference between the average Mexican student and the rest of the OECD was approximately one standard deviation.

Two important questions that relate to interpretation arise, and we consider a wide range of alternative specifications that demonstrate the stability of the estimates. The first set of issues is whether the sample of countries or years of observation heavily influences the results, thus implying that the results are potentially driven by other, unmeasured factors. The second is whether the specific measure of cognitive skills drives the estimates.

Table 2 provides the matrix of estimated cognitive-skill coefficients across different samples of observations. The columns consider sample sensitivity and concentrate on whether the overall

[14] The measure of openness is the Sachs and Warner (1995) index reflecting the fraction of years between 1960 and 1998 that a country was classified as having an economy open to international trade, based on five factors including tariffs, quotas, exchange rate controls, export controls, and whether or not a socialist economy. Following Acemoglu, Johnson, and Robinson (2001), the measure of security of property rights is an index of the protection against expropriation risk, averaged over 1985-1995, from Political Risk Services, a private company which assesses the risk that investments will be expropriated in different countries.

[15] While openness and security of property rights enter the model (jointly) significantly, fertility and tropical location do not.

results are driven by specific subsets of countries, which might indicate that the cognitive-skill measures simply proxy for other facets of the economies. The two rows consider basic sensitivity to test measurement, because the measurement may interact with the sample sensitivity. The top row focuses on the average of all observed math and science scores – as presented previously – while the second includes just lower-secondary-school scores. Each entry comes from a separate regression that includes GDP per capita in 1960 and school attainment.

The first two comparisons (col. 2-3 and col. 4-5) present evidence on whether cognitive skills are more or less important in developed countries. The first comparison divides the estimation into the 23 OECD countries and 27 non-OECD countries, while the second comparison divides countries into above and below the median level of per-capita GDP in 1960. The statistically significant difference of high-income and lower (below median)-income countries indicates that developing countries are somewhat more affected by cognitive skills than developed countries.[16] This larger impact of skills in low-income countries is consistent with the arguments by Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004) that nearly all poor countries in 1960 were dictatorships, some of which developed better societal institutions as an outcome of growth rather than a cause. The countries that did better in terms of growth were those with higher human capital, supporting the larger coefficient on human capital in the poor countries. Nonetheless, variations in math and science skills remain very important in distinguishing among growth rates of the developed countries.

A portion of the influence of cognitive skills comes from the high growth of East Asian countries. As shown in column 6, excluding the ten East Asian countries lowers the estimated impact of math and science skills to 1.3, but it remains highly significant in the remaining countries. In other words, the overall estimates, while influenced by the East Asian growth experience, are not simply identifying the high growth – high test-score position of East Asia, which would raise the possibility that the growth relationships might be driven by other factors that were simply correlated with East Asian test performance if true.

The growth estimates are meant to identify long-run factors, but the sample period of 1960-2000 includes subperiods of world stagnation, fast growth, and financial crises. Some have suggested, for example, that the observed growth rates are dominated by the early-period growth

[16] While not shown, the school attainment measures are insignificantly related to growth even among the developing countries where the levels are low and where there is considerable cross-country variance.

explosion of East Asia and that this changed considerably with the financial crises of the late 1990s (Ramirez, Luo, Schofer, and Meyer (2006)). Our results (col. 7 and 8) indicate, however, a consistent impact of cognitive skills across the period that, if anything, has grown stronger in the second half of our observations. Indeed, the estimated impact doubles in the most recent period, consistent with various arguments that, at least for the U.S. and OECD countries, the importance of skills has increased (Murnane, Willett, and Levy (1995), Katz and Autor (1999), and Goldin and Katz (2008)). The same impact on 1980-2000 growth is found when restricting the test scores to measures obtained before 1985 (available for only 22 countries), so that test scores nearly fully pre-date the growth period (col. 9).

Finally, the level of schooling and cognitive scores are correlated across our sample (r=0.62), in part because of the differences between developed and developing countries. The separation of the impact of cognitive skills from that of school attainment in our estimation relies upon information where these two diverge, and it might be a peculiar set of countries in terms of growth where the pattern of school attainment and skills varies most. The final two columns divide countries based on deviations of cognitive scores from school attainment. Specifically, the "score-schooling outliers" are the 25 countries with the largest residuals when test scores are regressed on attainment, and the "score-schooling core" are the 25 with the smallest residuals. Interestingly, the relationship between cognitive skills and growth is virtually the same across these two samples, revealing that the results are not driven by "peculiar" countries in the production of cognitive skills.

The preceding results hold looking across columns, but the pattern also obtains for the alternative measures of test scores. The estimated coefficients using only lower-secondary-school math and science scores are systematically a little smaller than those from all scores, which may reflect attenuation bias when using fewer test observations in the construction of the cognitive-skill measure, but there are no changes in patterns across any of the columnar comparisons. This test-score measure excludes any test in primary schooling or in the final year of secondary education. Test scores at the end of the secondary level, which combine the knowledge accumulated over primary and secondary schooling, may be most relevant for the labor force, but, at the same time, the duration of secondary education differs across countries, so that tests performed in the final year of secondary schooling may not be as readily comparable across countries. Further, given differing school completion rates, tests in the final year of secondary schooling may produce samples with differential selectivity of test takers. Yet neither the primary-school tests nor the tests in the final secondary year are crucial for the results.

Table 3 provides more detail on sensitivity to the measure of cognitive skills, comparing several additional plausible alternatives for the aggregation of scores, including using math, science, and reading scores separately. We also provide breakdowns by OECD and non-OECD countries, although this breakdown makes little qualitative difference and we concentrate on the variations in aggregate test information found in the table rows.

Results are qualitatively the same when using only scores on tests performed since 1995 (row A). These recent tests have not been used in previously available analyses and are generally viewed as having the highest standard of sampling and quality control. Likewise, results are robust to using tests scores since 1995 for just lower secondary grades (row B).

A drawback of using only the more recent tests is that such an approach requires a strong version of the assumption that test performance is reasonably constant over time, because it relates test performance measured since 1995 to the economic data for 1960-2000. To make sure that higher previous economic growth is not driving the measured test performance, the test-score measure used in row C disregards all tests since the late 1990s. Our results turn out to be robust, with a point estimate on the test-score variable that is significantly higher (sample reduced to 34 countries). Our results are also robust to using the average early test scores as an instrument for the average of all test scores in a two-stage least-squares regression, in order to utilize only that part of the total test-score measure that can be traced back to the early test scores (row D). In sum, the results are not driven by either early or late test scores alone.

The remainder of the table investigates different combinations of the math, science, and reading tests. While we were concerned about the reliability of the reading tests and thus have focused on math and science, the use of reading tests provides similar results in the growth models (rows E-G). In a specification that enters the different subjects together (panel H), the three are always jointly significant at the 1% level and higher, even though the science effect gets smaller and the reading effect loses significance in the joint model.

The overall picture from this sensitivity analysis is that the estimated effect of cognitive skills on growth is robust to a range of samples, specifications, and measurements. This finding contrasts sharply with many previous analyses that use years of schooling as the human capital measure, beginning with Levine and Renelt (1992) and continuing through Pritchett (2006).

V. Variations in Cognitive Skills Driven by Schools: Instrumental Variable Models

The main subject of this paper is that the stylized facts from cross-sectional growth regressions using existing variation across countries may be hampered by endogeneity biases. Endogeneity of cognitive skills could arise because nations with conditions favorable to economic growth and performance also produce high test performance. This correlation could arise because of cultural factors, historically good economic institutions, variations in health status, or any other set of factors that lead to strong economic performance might also be systematically related to high cognitive skills. Indeed, it does not matter whether such relationships are causal or purely associational. If these factors are omitted from the growth estimation, they will tend to bias the coefficient on cognitive skills. Likewise, there might be reverse causality if economic growth facilitates investments in the school system or increases family resources that improve cognitive skills.

Even if the cognitive skills-growth relationship is causal, the results presented so far would only be relevant for school policy if the variation in cognitive skills emanating from school policies is in fact related to economic growth. As noted, cognitive skills are likely to depend not only on formal schooling but also on non-school factors such as families, peers, and ability. Therefore, it is important to establish any links with school policy levers.

One means of addressing the set of issues is to use measures of the institutional structure of the school systems as instruments for the cognitive-skill measure, thereby focusing only that part of the international variation in cognitive skills that can be traced back to international differences in school systems. We use several institutional features – notably the existence of external exit exam systems, the share of privately operated schools, and the centralization of decision-making – that have been shown in the literature on international educational production to be associated with student achievement (see Woessmann (2007) for a review).

While other school policies such as those surrounding educational spending may well be endogenous to the growth process, these institutional features can plausibly be assumed uncorrelated with the regression disturbances of our models. First, many educational institutions such as the existence and extent of private schooling reflect long-standing polices embedded in education law and thus are not outcomes of the growth process per se (see, for example, the review of private schooling across countries in Glenn and De Groof (2002)). Second, while there have been some trends in these institutions – such as the slow movement toward decentralizing school decision-making – there is no suggestion that this reflects either growth or other systematic differences in cultural and economic systems.[17] Third, there is empirical support from the literature on educational production that these institutional effects on student learning are robust to including regional fixed effects in cross-country analyses, to within-country analyses, and to the use of historical instruments (see Woessmann (2003a, 2007), West and Woessmann (2008)). These results suggest that institutional impacts are not driven by cultural differences and do not suffer directly from reverse causality.

External exit exam systems are a devise to increase accountability in the school system that has been repeatedly shown to be related to better student achievement (see Bishop (2006) for a review).[18] The first specification reported in Table 4 uses the share of students in a country who are subject to external exit exams as an instrument for our measure of cognitive skills in the growth regression. The first-stage results confirm a statistically significant association between external exit exams and cognitive skills. The effect of cognitive skills on economic growth in the second stage of the instrumental variable (IV) estimation is statistically significant and close to the OLS estimate.[19] However, the relatively low *F*-statistic of the instrument in the first stage indicates the possibility of a weak instrument problem. Instruments that are only weakly correlated with the endogenous explanatory variable may actually increase estimation bias and compromise the reliability of the conventional asymptotic approximations used for hypothesis testing. Thus, we also report estimates based on the modification of the limited information maximum likelihood (LIML) estimator by Fuller (1977), but the results are hardly affected.[20] While the confidence band of the conditional likelihood ratio test proposed by Moreira (2003)

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[17] Glenn and De Groof (2002), p. 267, note that "there has been in most Western democracies a slow but very marked shift in the allocation of responsibility for the organization and control of education, in the public as well as the nonpublic education sector, through decentralization of various aspects of decision-making to the local school community." The cross-country details suggest no obvious political or cultural differences in these trends.
[18] Data on external exit exams are available for 43 countries in Woessmann, Luedemann, Schuetz, and West (2009), who update Bishop (2006)'s collection from reviews of comparative-education studies, educational encyclopedia, government documents, background papers, and interviews with national representatives.
[19] The Durbin-Wu-Hausman test does not reject the exogeneity of cognitive skills at conventional levels.
[20] Fuller's modification of the LIML estimator is more robust than 2SLS in the presence of weak instruments and performs relatively well in the simulations by Hahn, Hausman, and Kuersteiner (2004). We set the user-specified constant (Fuller (1977)'s alpha) to a value of one, but our results are hardly affected if we set alpha to four.

and Andrews, Moreira, and Stock (2007) gets large at the upper end in this specification, difference from zero still reaches significance at the 10% level.[21]

Because years of schooling are insignificant in the growth model once test scores are controlled for (both in the OLS and in the IV specification), another possibility is to include years of schooling as a second instrument for test scores. This approach is also suggested by the prior model as long as cognitive skills are a measure of human capital in equation (1). Specification (2) of Table 4 reveals that years of schooling are significantly associated with test scores in the first stage, and the first-stage F-statistic increases substantially. The Sargan test does not reject the overidentification restrictions of the model, suggesting that, if external exit exams are a valid instrument, years of schooling are also valid. Both the 2SLS and the Fuller estimates, as well as inference based on Moreira confidence bands, confirm that schooling-induced differences in cognitive skills are significantly related to economic growth.

School choice, as measured by the share of privately operated schools in a system, consistently shows a positive association with student achievement in OECD countries (see the review in Woessmann, Luedemann, Schuetz, and West (2009), along with West and Woessmann (2008)) and provides an additional instrument. In our sample, the share of private enrollment in a country is significantly positively associated with cognitive skills in the first stage of our IV model (specification (3) of Table 4).[22] The second-stage estimate of the growth model confirms our previous results – schooling-induced differences in cognitive skills are significantly related to economic growth. Again, the Sargan test does not reject the validity of the overidentification restrictions, and the Durbin-Wu-Hausman test presents no evidence of endogeneity of the cognitive-skill measure.[23]

A final institutional feature regularly shown to be positively associated with student achievement is the extent to which schools (or at least local decision-makers) are autonomous to make their own decisions about the organization of instruction (see Woessmann (2003b)).

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[21] Likewise, the Anderson-Rubin χ^2 statistic (3.06) of this just-identified model indicates significance at the 8% level. Note that the LIML estimators, around which the Moreira bands are centered, differ from the reported 2SLS estimates only in the third digit in all our models.

[22] The data on private enrollment as percentage of total enrollment in general secondary education are from UNESCO (1998) and refer to 1985, the earliest year with consistent data. For greater consistency of the time spans, the dependent variable in this specification is economic growth in 1980-2000; results are robust to using growth in 1960-2000. Given that the results from the educational production literature mostly refer to the sample of OECD countries, we restrict the analysis to the OECD sample, for which 19 observations are available.

[23] Results are very similar without years of schooling as a second instrument.

Specification (4) of Table 4 shows that the share of decisions on the organization of instruction that are made at the central government level is significantly negatively associated with our cognitive-skill measure. The second-stage estimators, robust to potentially weak instruments, confirm the significantly positive effect of cognitive skills on economic growth.[24]

One potential worry about the exogeneity of our instruments is that the institutional features of school systems may be correlated with economic institutions, which are themselves correlated with economic growth. To test whether this affects our identification, we add the two measures of differences in economic institutions that tend to enter most robustly in growth regressions – openness and security of property rights – to our IV models (remembering, however, our prior reservations about the distinct possibility that these economic institutions capture part of the human capital effect). Our basic result is unaffected. In fact, the measures of economic institutions do not enter significantly (individually or jointly) in any of the IV models except column (2), and the effect of cognitive skills remains significant in all specification except for the just-identified model (1). The point estimates are hardly affected except for model (2), where – in line with the OLS results of Table 1 – it is reduced to 1.1, similar to our OLS estimate of the lower bound for the effect. In the other three models, it remains between 2.6 and 3.7.

The results suggest that cognitive skills generated in the school system lead to higher long-run growth of economies. There are obvious limits of cross-country regressions with small data samples, and these are particularly salient in IV specifications. Nonetheless, the results using several institutional features of the school systems as instruments suggest a causal interpretation of the results previously presented. Caution is appropriate in interpreting IV results for our relatively small samples of countries and employing the aggregate nature of the institutional measures, but these make the statistical significance, reasonable precision, and quantitative robustness of the results based on various instruments even more striking.[25]

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[24] Data on the percentage of decisions on the organization of instruction in public lower secondary education taken at the central level of government are available in Organisation for Economic Co-operation and Development (1998), available only for 1998. The IV results are very similar without using years of schooling as a second instrument. In this specification, the estimated growth effect is even larger than the OLS estimate. Note, though, that the Fuller estimate is already closer to the OLS estimate, and the Moreira confidence bands include the OLS and other IV estimates.

[25] The IV results hold when employing two or all three of the institutional instruments jointly in the specification, but only one of them tends to capture statistical significance in the joint specifications.

VI. Comparing the Impacts of U.S. and Home-Country Education on the U.S. Labor Market

The IV estimates of the previous section could, nonetheless, fail if the favorable educational institutions were correlated with unmeasured cultural factors or economic institutions. While the common institutional measures did not affect the results, it is still not possible to rule out other unmeasured attributes of national economies that are correlated with the educational institutions.

An alternative approach for assessing the causal importance of our measured skill differences on economic outcomes relies on microdata on earnings differences within a single labor market – the U.S. labor market. Looking within a single labor market explicitly holds constant the quality of economic and cultural factors affecting the operations of the economy and allows focusing on whether measured cognitive skills directly relate to productivity. Following a difference-in-differences strategy, we can compare the returns to skills of immigrants schooled in their country of origin to those of immigrants from the same country schooled within the United States. If it is the measured differences in cognitive skills and not other economically relevant attributes of the families and economies that are important, the impact of skills can be derived from the different earnings of immigrants who received their schooling at home and in the United States.

The structure of the estimation is derived from a standard Mincer (1974) wage equation augmented by measured cognitive skills such as:

$$\ln y_{ic} = \alpha_0 + \alpha_1 S_{ic} + \alpha_2 PE_{ic} + \alpha_3 PE_{ic}^2 + \gamma_y H_{ic} + \upsilon_{ic} \tag{3}$$

where y is annual earnings for immigrant i from country c, S is years of school attainment, PE (=age-S-6) is potential experience, H is cognitive skills, and υ is a random error.[26]

We look at immigrants to the U.S. who were either educated entirely in their country of origin or entirely in the United States.[27] (This excludes any individuals partially educated in both

[26] Given that our growth specifications are most closely related to an endogenous-growth formulation, one cannot directly go from the estimated effects on individual productivity to the impact on growth rates. For that reason, in order to validate our micro estimates below, we concentrate on comparisons with other micro estimates of the impact of cognitive skills on productivity. While our analysis uses skill differences by country of origin to infer earnings differences among immigration in the U.S., Hendricks (2002) goes the opposite way of using earnings differences of U.S. immigrations to infer cross-country differences in human capital.

[27] Immigrants are individuals born in a foreign country. The sample includes all individuals age 25 or older currently in the labor force with wage and salary earnings of at least $1,000 and not enrolled in school. Included immigrants had to have been born in a country with international test data (see Appendix B). The number of included countries is larger than in the previous growth regressions because of the lack of need to have internationally comparable GDP data for country of origin. Descriptive statistics are found in Appendix Table C2.

the U.S. and their home countries in order to obtain a clear separation of treatment and control groups.) We assign the average cognitive-skill score of the home country ($\overline{T}_c$) for each immigrant and estimate the Mincer earnings equation (3) as:

$$\ln y_{ic} = \alpha_0 + \alpha_1 S_{ic} + \alpha_2 PE_{ic} + \alpha_3 PE_{ic}^2 + [\alpha_4 ORIGIN_i + \delta \overline{T}_c + \delta_O (\overline{T}_c \times ORIGIN_i)] + \upsilon_{ic} \tag{4}$$

where *ORIGIN* is an indicator that is one if immigrant *i* was educated entirely in schools in the country of origin and zero otherwise and the combined terms in brackets indicate the skills of individuals from country *c*. The parameter δ_O is the relevant contrast in skills between home-country schooling and U.S. schooling. We interpret δ_O as a difference-in-differences estimate of the effect of home-country test scores on earnings, where the first difference is between home-country educated immigrants (the "treatment group") and U.S.-educated immigrants (the "control group") from the same country, and the second difference is in the average cognitive-skill score of the home country.[28]

The first two columns of Table 5 report the estimates of the impact of cognitive skills from stratified samples for the two groups of immigrants. Test scores are normalized to mean zero and a standard deviation of one, so that the estimates indicate the proportionate increase in earnings from a one standard deviation increase in scores. Other things equal, there is essentially no relationship of U.S. earnings to scores of their country of origin, either quantitatively or statistically, for the 50,597 immigrants educated entirely in the U.S. On the other hand, one standard deviation greater performance translates into a statistically significant earnings increase of approximately 16 percent for the 258,977 immigrants educated in their country of origin.

This estimate is surprisingly close to recent estimates for cognitive skills of U.S. workers, which indicate 10-15 percent returns to a standard deviation of test scores for young workers and 19 percent across the full age range of workers.[29] The closeness to the various estimates is

[28] Immigrants educated in their home country necessarily come to the U.S. at an older age than comparable immigrants educated in the U.S., suggesting that there might be differential selectivity and motivation of these two groups. But the key issue for identifying the impact of cognitive skills is that any selectivity in migration is the same across countries (which would then be captured by α_4), or at least is not correlated with differences in home-country cognitive skills.

[29] Murnane, Willett, Duhaldeborde, and Tyler (2000) provide evidence from the High School and Beyond and the National Longitudinal Survey of the High School Class of 1972. Their estimates suggest some variation with males obtaining a 15 percent increase and females a 10 percent increase per standard deviation of test performance. Lazear (2003), relying on a somewhat younger sample from NELS88, provides a single estimate of 12 percent. These estimates are also very close to those in Mulligan (1999), who finds 11 percent for the normalized AFQT score in

surprising given that just average country scores as opposed to individual specific scores are used in the estimation here, although the averaging of scores does eliminate the measurement error found in individual test data.

Column 3 combines the samples and fully estimates equation (4). These estimates indicate a significant impact of test scores with schooling in country of origin (δ_O). The estimate of (home-country) test score for U.S.-educated immigrants is statistically insignificant, although the point estimate is noticeably greater than zero. Column 4 demonstrates that this latter effect comes entirely from the influence of immigrants from Mexico (who constitute 37 percent of all immigrants to the U.S.). The estimation for immigrants from Mexico is prone to classification error, because many Mexican families tend to move back and forth from Mexico – thus making assignment to U.S. or Mexican schooling prone to error.[30] Excluding Mexican immigrants, $\hat{\delta}_O$ is highly significant with a point estimate of 0.13, while the coefficient for U.S.-educated immigrants falls to -0.026 and remains statistically insignificant.

The prior estimates indicate that the estimation strategy might be sensitive to variations in immigration patterns across the 64 sampled countries. For example, in addition to the complications for Mexican immigrants, the immigrants from other countries might vary by where they come in the ability distribution of the home country and the like. For this reason, the remaining columns of Table 5 contain country-of-origin fixed effects. Thus, immigrants educated entirely abroad in their home country are compared directly to immigrants *from the same country* educated entirely in the U.S. This should eliminate any potential bias emanating from features specific to the country of origin, be it specific selectivity of the immigrant population or country-specific cultural traits. The only remaining assumption required for identification of our parameter of interest is that any potential difference between the early-immigrated U-S.-educated and the late-immigrated home-educated group of immigrants from each country (as captured by the *ORIGIN* indicator) does not vary across countries in a way associated with country-of-origin test scores.

the NLSY data. Hanushek and Zhang (2009) estimate a return of 19 percent from the International Adult Literacy Survey, which samples workers aged 16-65.

[30] The assignment of individuals to U.S. schooling is based on census data indicating immigration before age 6. The assignment of individuals to schooling all in country of origin is based on age of immigration greater than years of schooling plus six. A person who moves back and forth during the schooling years could be erroneously classified as all U.S. or no U.S. schooling, even though they are really in the partial treatment category (which is excluded from the difference-in-differences estimation).

Column 5 displays the primary estimation across all sampled countries with country-specific fixed effects. The estimated impact of cognitive skills is a 14 percent increase in earnings from each standard deviation increase in origin-country test scores (when educated there). This estimate is highly significant. Further, the point estimate is virtually unchanged by excluding the Mexican immigrants (col. 6). The standard error is reduced by clearer assignment to treatment category (when Mexicans excluded), even though the sample is substantially reduced.

The final two columns investigate the sensitivity of these estimates to sample definition. First, our estimation of growth models used the 50 countries for which we could obtain the relevant economic data for GDP growth. Restricting this analysis to that smaller sample yields a slight increase in the magnitude of $\hat{\delta}_O$ to 17 percent, while it remains statistically significant. Second, because immigrants from non-English speaking countries may have lower earnings because of language difficulties, the final column shows estimates that come entirely from countries where English is the primary or official language.[31] Again, even for this sample of just 12 countries, variations in cognitive skills across countries have a strongly significant impact on earnings of 16 percent.

The remaining rows of Table 5 provide estimates of the complete set of parameters. While there is some variance across samples in the estimate of the effect on earnings of being educated entirely in the country of origin, this appears to reduce average earnings by 6-13 percent with the exception of English-speaking immigrants, who appear to suffer no significant average earnings loss compared to people educated entirely in the U.S. The estimated "Mincer" parameters (α_1, α_2, and α_3) appear within the range of typical estimates for the general population (see Heckman, Lochner, and Todd (2008)). Results remain qualitatively the same when indicators for decade of immigration and for gender are added to the model.[32]

These difference-in-differences estimates provide support for two conclusions about the causal impacts of cognitive skills. First, they contrast individuals receiving the treatment of home-country schooling to immigrants from the same country, all within the same labor market. Thus, they cannot indicate differences in economic institutions around the globe that are

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[31] Data on English language come from the CIA World Factbook. Countries were coded as English speaking if the CIA World Factbook listed English as an official language or as the most widely spoken language in the country. See https://www.cia.gov/library/publications/the-world-factbook/.
[32] When analyzed separately by gender, the results hold strongly for males whereas results for females – while pointing in the same direction – mostly do not reach statistical significance, as is common in labor-market analyses.

correlated with differences in cognitive skills. Second, they pinpoint the impact of schooling differences across countries, as distinct from family or cultural differences in attitudes, motivation, child rearing, and the like. In sum, the estimates, which are highly stable across different estimation samples, provide evidence that the economic impact is a causal one, and not purely associational.

VII. Skill Improvement and Improved Growth

The prior analyses have relied upon the average test score for each nation in order to characterize differences in skills of their labor forces. An alternative difference-in-differences approach uses the time-series evidence on performance within each country to identify the impact of skills on growth. Specifically, countries that improve the skills of their population – no matter how it is done – should see commensurate improvements in their rate of growth. This estimation removes any country-specific fixed effects affecting growth rates – such as basic economic institutions, cultural factors, political environment, and the like – and focuses on whether a country that alters the cognitive skills of its population is observed to receive an economic return.

While others have investigated turning points in growth, our focus is low-frequency changes such as those that might result from evolutionary schooling policies and that alter the path of economic growth.[33] Policies affecting the skill composition of the labor force necessarily unfold over lengthy periods and are not seen as sharp changes in outcomes.

To characterize the longitudinal patterns of test scores, we regress separate test scores by year, age group, and subject on a time variable (as well as age-group and subject indicators) and use the time coefficient as the measure of change in cognitive skills for each nation (see Appendix B for details). The amount of noise in each test observation, particularly with our common scaling, implies that such trends are also estimated with considerable noise. We therefore trust the rough cross-country pattern more than the specific point estimates of changes

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[33] Relevant studies include Hausmann, Pritchett, and Rodrik (2005) that looks for episodes of "growth accelerations"; Jones and Olken (August 2008) that considers patterns of 10-year periods of acceleration and collapse; and Barro and Ursúa (2008) that identifies events of major declines in consumption that have potential implications for long-run growth. The identified periods are generally characterized by financial crisis, political instability, or war.

in each country. To put limits on the amount of noise affecting our analyses, we rely on the sample of OECD countries that have test observations both before 1985 and up to 2003.[34]

As is evident from Figure 2 (see also Appendix Figure B3), substantial changes in test performance – both positive and negative – have occurred for OECD countries. The rapid growth in performance of such countries as Canada, Finland, and the Netherlands contrasts sharply with the declining scores in Germany, Italy, and Norway. For our purposes, however, we are not interested in test scores for the school-aged population but instead in the skills of the relevant portions of the labor force. Thus, we need to assume that the currently observed trends in performance reflect long-run patterns of skill change and specifically those holding during the earlier time periods.

In a parallel manner, we estimate a time trend for annual growth rates in each country using the Penn World Tables data. The annual growth rate series for each country contains considerable noise, largely reflecting short-run business cycle phenomena or financial crises, and the trend estimation is designed to extract long-run changes in growth.[35]

The consistency of changes in test performance and changes in growth rates is evident in Figure 2. When we split countries by being above or below the median change in growth rates and above or below the median change in cognitive skills, all countries fall into either the positive or negative quadrants on both measures. The largest outliers from the trend line are precisely the countries that have less historical test score data (Canada, Korea, and Norway) and that thus have poorer trend data.

We provide estimates of simple models of the change in growth rates over the 1975-2000 period in Table 6. For the 15 OECD countries, 38 percent of the variance in growth-rate changes can be explained by test-score changes. If we add measures for the average growth rate in each country and the initial GDP per capita (col. 2-3), the change in achievement scores remains statistically significant at near the same level as found in the simple regressions of column 1.

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[34] In fact, all countries except Canada, Korea, and Norway have test scores dating back at least to 1971.

[35] Descriptive statistics are found in Appendix Table C3. We also tried alternative measures of growth-rate changes, including the difference between the average growth rate in the first five years and in the last five years; trend growth using IMF data in national currencies; and IMF national currency data for the period 1975-2004. Using IMF national currency data is consistent with Nuxoll (1994) and Hanousek, Hajkova, and Filer (2008) who argue that using national accounts data is superior to relying on the price and exchange-rate adjustments in the basic Penn World Tables data when looking at growth rates. In our investigation of these options, the estimates of the impact of changes in test scores remain statistically significant and quantitatively very similar across alternatives and compared to the estimates reported in Table 6.

The same is true when the change in quantitative educational attainment is added to the model (col. 4). Importantly, the change in educational attainment is orthogonal to the change in growth rates (either with controls for the test-score trend or without), reinforcing the introductory skepticism about the efficacy of past schooling policies. Likewise, results are hardly affected if we weigh each observation by the inverse of the standard error with which the trend in test scores was estimated, in order to downweigh those that are more noisily estimated (col. 5).

If, however, we restrict the analysis to those countries with test scores spanning a range of more than three decades, from at least 1971 to 2003, both the coefficient estimate and the explained variance grow in size (col. 5), as suggested in Figure 2. In the sample without these countries (Canada, Korea, and Norway), the test-score trend alone accounts for 64 percent of the variation in growth trends. Alternative specifications look simply at whether the test-score trend is above or below the OECD median (col. 6-7). In all cases, the impact of changes in test scores on changes in growth rates remains very stable and is always statistically significant.

This analysis requires extrapolation of the test-score data in order to capture changes for workers in the labor force. Thus, by itself it should be considered suggestive and not definitive. If we assumed that the observed trend in test scores had been going on since the oldest person in the current labor force went to school, an annual increase in test scores by 1 percent of a standard deviation would translate into an annual increase in the growth rate by 0.07-0.12 percentage points. However, if we more realistically thought that any change in test scores began at the beginning of our observation period, then the impact of student improvements on the average labor force is much less, and the projected change in growth rates would be commensurately reduced. Back-of-the-envelope calculations suggest that in such a setting, the estimates based on the trend analysis in Table 6 are close to the steady-state estimates in Table 1.

In conclusion, the positive relationship between improving cognitive skills and improving growth rates provides another set of consistent results based on a different approach to identifying the causal impact of cognitive skills – a focus on changes within each country that removes country-specific fixed effects.

VIII. Rocket Scientists or Basic Education for All?

While addressing the range of potential schooling policy options is clearly beyond the scope of this paper, our new data series allows us to extend the growth analysis to illuminate one important issue – whether to concentrate attention at the lowest or at the highest achievers. Some argue in favor of elitist school systems which focus on the top performers as potential future managers of the economy and drivers of innovation. Others favor more egalitarian school systems to ensure well-educated masses that will be capable of implementing established technologies. In other words, should education policy focus on forming a small group of "rocket scientists," or are approaches such as the Education for All initiative (UNESCO (2005)) more promising in spurring growth?

To capture these differences in the distributional patterns of the test-score performance in different countries, we use the microdata from each of the international assessments to calculate measures of the share of students in each country who reach at least basic skills as well as those who reach superior performance levels (see Appendix B). We use performance of at least 400 test-score points on our transformed international scale – one standard deviation below the OECD mean – as our threshold of basic literacy and numeracy.[36] The international median of this share of students is 86 percent in our sample, ranging from 18 percent in Peru to 97 percent in the Netherlands and Japan. As our threshold for superior performance, we take 600 points or one standard deviation above the OECD mean.[37] This level is reached by an international median of only 5 percent, although it ranges from below 0.1 percent in Colombia and Morocco to 18 percent in Singapore and Korea and 22 percent in Taiwan.[38] (As shown in Appendix Figure B2, these differences represent more than simple mean displacement.)

As seen in the first three columns of Table 7, both measures of the test-score distribution are significantly related to economic growth, either when entered individually or jointly.[39] Both the

[36] The PISA 2003 science test uses the threshold of 400 points as the lowest bound for a basic level of science literacy (Organisation for Economic Co-operation and Development (2004), p. 292), and on the math test this corresponds to the middle of the level 1 range (358 to 420 test-score points), which denotes that students can answer questions involving familiar contexts where all relevant information is present and the questions are clearly defined.

[37] A score of 600 points is near the threshold of the level 5 range of performance on the PISA 2003 math test, which denotes that students can develop and work with models for complex situations, identifying constraints and specifying assumptions; they can reflect on their answers and can formulate and communicate their interpretations and reasoning.

[38] The distributions depicted in Figure B2 reveal that such distributional measures capture much more of the overall distribution than a simple measure such as the standard deviation in national test scores. The standard deviation in test scores does not enter our basic model significantly (see Castelló and Doménech (2002) for related analyses using measures of educational inequality based on years of schooling).

[39] In the joint model, the two measures are separately significant even though they are highly correlated across countries with a simple correlation of 0.73. The mean test score used in previous models is more highly correlated with the basic literacy share (r=0.96) than with the top-performing share (r=0.85). If the mean test score is added to

basic-skill and the top-performing dimensions of educational performance appear separately important for growth. From the estimates in column 3, a ten percentage point increase in the share of students reaching basic literacy is associated with 0.3 percentage points higher annual growth, and a ten percentage point increase in the share of top-performing students is associated with 1.3 percentage points higher annual growth. However, it may be much more feasible to increase the basic-literacy share than to increase the top-performing share by the same amount, as suggested by the fact that the international standard deviations of these two shares are 0.215 and 0.054, respectively. Thus, increasing each share by roughly half a standard deviation (10 percentage points basic-literacy share and 2.5 percentage points top-performing share) yields a similar growth effect of roughly 0.3 percentage points.

The impact of having more top performers is only slightly reduced by introducing the measures of economic institutions, fertility, and tropical geography (col. 4). On the other hand, the separate influence of basic literacy levels falls quantitatively and becomes statistically insignificant in the expanded model (for the 45 countries with complete data), in line with an interpretation where part of the effect of basic literacy comes through improved institutions (Glaeser, La Porta, Lopez-de-Silanes, and Shleifer (2004)).

The effect of the basic-literacy share does not vary significantly with the initial level of development, but the effect of the top-performing share is significantly larger in countries that have more scope to catch up to the initially most productive countries (col. 5). These results appear consistent with a mixture of the basic models of human capital and growth mentioned earlier. The accumulation of skills as a standard production factor, emphasized by augmented neoclassical growth models (e.g., Mankiw, Romer, and Weil (1992)), is probably best captured by the basic-literacy term, which has positive effects that are similar in size across all countries. But, the larger growth effect of high-level skills in countries farther from the technological frontier is most consistent with technological diffusion models (e.g., Nelson and Phelps (1966)). From this perspective, countries need high-skilled human capital for an imitation strategy, and the process of economic convergence is accelerated in countries with larger shares of high-performing students.[40] Obvious cases are East Asian countries such as Taiwan, Singapore, and

column 3, the basic-literacy share becomes insignificant, but in a specification with just the mean, mean and top-performing shares both remain significant.

[40] For an alternative model of imitation and innovation that emphasizes the innovation margin, see Vandenbussche, Aghion, and Meghir (2006) and Aghion, Boustan, Hoxby, and Vandenbussche (2005).

Korea that all have particularly large shares of high-performers, started from relatively low levels, and have shown outstanding growth performances, but the results of column 5 are nonetheless robust to the inclusion of an East Asian dummy, or a full set of regional dummies.

A particularly informative extension considers the interaction of the top-performing and basic-literacy shares (col. 6 and 7). This complementarity between basic skills and top-level skills suggests that in order to be able to implement the imitation and innovation strategies developed by scientists, countries need a workforce with at least basic skills.[41]

Many countries have focused on either basic skills or engineers and scientists. In terms of growth, our estimates suggest that developing basic skills and highly talented people reinforce each other. Moreover, achieving basic literacy for all may well be a precondition for identifying those who can reach "rocket scientist" status. In other words, tournaments among a large pool of students with basic skills may be an efficient way to obtain a large share of high-performers.

Finally, our emphasis has been on growth and aggregate economic outcomes, and our results suggest a balanced investment in skills. This focus, of course, does not capture the range of policy objectives. In particular, initiatives may be aimed at basic literacy for equity and income-distribution reasons, and the optimal investment choices between basic and high-level skills will depend on the relative costs of improving at the different margins. In any event, however, the economic returns come only from policies that effectively improve student achievement and that thus add to the skills of the labor force – and not from ones that increase the length of schooling without improving achievement.

IX. Conclusions

A myriad of empirical estimates of cross-country growth models exist. The general criticism of these is that they provide little confidence that the models satisfactorily identify the causal impact of their included determinants of growth. And, a related criticism is that they then cannot provide any real policy guidance.

We have focused on the role of cognitive skills in determining economic growth and have taken the quest for policy guidance seriously. We have investigated a set of models that

[41] The issue of skill complementarity in production has been addressed in explaining the pattern of earnings inequality. The U.S. analysis of Autor, Katz, and Kearney (2006, 2008) suggests that high-skilled workers and low-skilled workers are complements, a result that helps explain income variations across the educational spectrum.

approach identification from different vantage points. While each of the approaches is subject to some questions, the key is that each is subject to *different* questions. As a result, each potentially fails for very different reasons.

The consistency of the alternative estimates – both in terms of quantitative impacts and statistical significance – provides considerable support for a causal interpretation of the impact of cognitive skills produced in schools.

First, there is a remarkable stability of the models in the face of alternative specifications, varying samples, and alternative measures of cognitive skills, a robustness uncommon to most cross-country growth modeling. Second, the instrumental variable estimation using institutional characteristics of each country's school system yield results close to those of the OLS regressions, while also supporting the conclusion that schooling policies can have direct economic returns. Third, immigrants to the U.S. who have been educated in their home countries receive labor-market returns reflecting the cognitive skills of the home country – but the control group of immigrants from the same home country schooled in the U.S. receive no return to home-country quality. This difference-in-differences approach rules out the possibility that test scores simply reflect cultural factors or economic institutions of the home country. It also provides further support to the potential role of schools to change the cognitive skills of citizens in economically meaningful ways. Finally, perhaps the toughest test of causality is reliance on how *changes* in test scores over time lead to *changes* in growth rates. By eliminating the level effects which may be interrelated with country-specific institutions and cultures, this investigation provides more evidence of the causal influence of cognitive skills.

The simple conclusion from the combined evidence is that differences in cognitive skills lead to economically significant differences in economic growth. Moreover, since the tests concentrate on the impact of schools, the evidence suggests that school policy can, if effective in raising cognitive skills, be an important force in economic development.

By itself, finding a potential role for schools does not point to any clear policies. Indeed, that discussion would enter into a variety of controversial areas and would lead us far afield. Nonetheless, our aggregate data provide direct evidence on where to focus attention. We find evidence that both providing broad basic education – education for all – *and* pushing significant numbers to very high achievement levels have economic payoffs.

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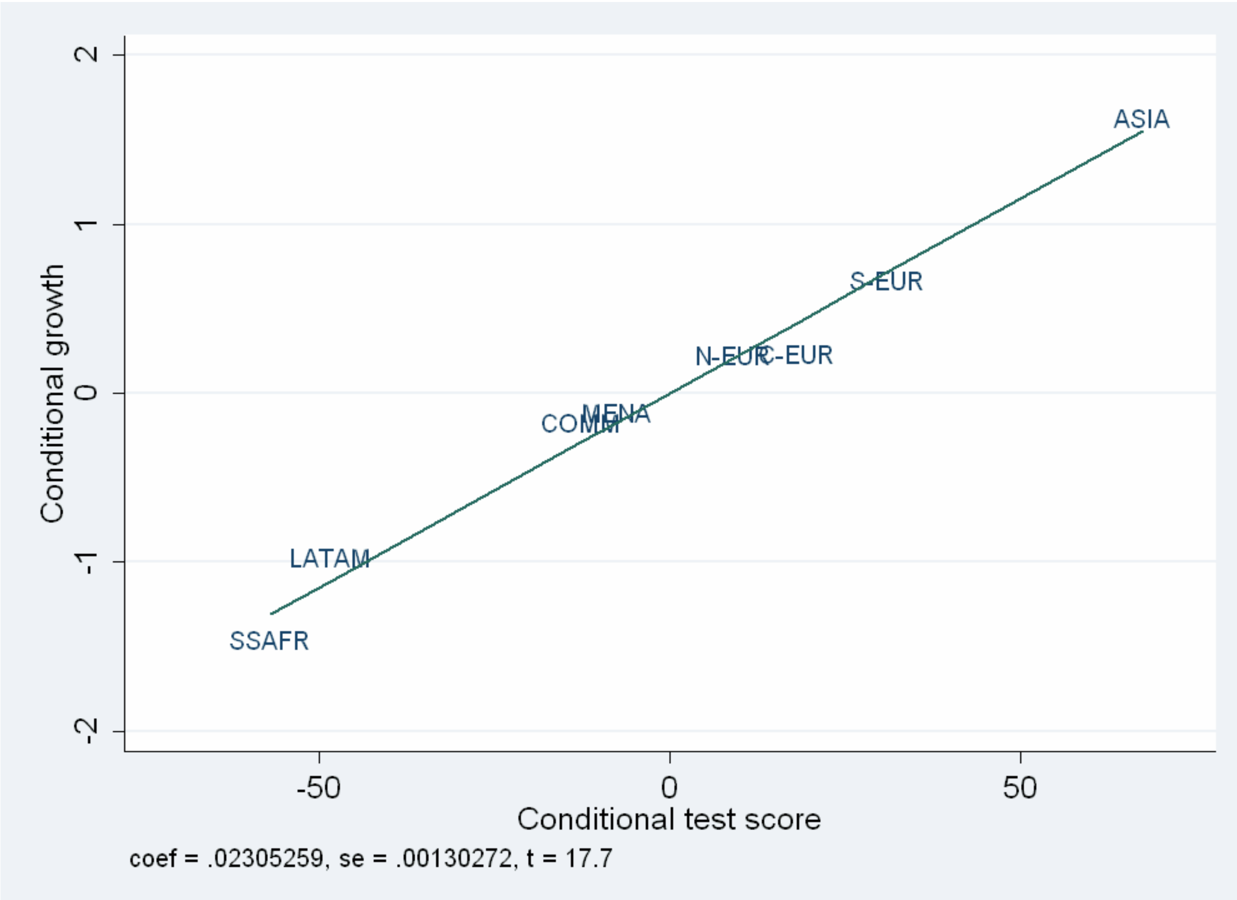
Figure 1: Cognitive Skills and Growth across World Regions



Notes: Added-variable plot of a regression of the average annual rate of growth (in percent) of real GDP per capita in 1960-2000 on the initial level of real GDP per capita in 1960 and average test scores on international student achievement tests. Authors' calculations. See Table A1 for a list of the countries contained in each world region.

Region codes: East Asia and India (ASIA), Central Europe (C-EUR), Commonwealth OECD members (COMM), Latin America (LATAM), Middle East and North Africa (MENA), Northern Europe (N-EUR), Southern Europe (S-EUR), Sub-Saharan Africa (SSAFR).

Figure 2: Trends in Growth Rates vs. Trends in Test Scores

Notes: Scatter plot of trend in the growth rate of GDP per capita from 1975 to 2000 against trend in test scores. Equivalent of first column of Table 6. Three countries without test scores before 1972 in light gray; regression line refers to the remaining twelve countries. See Appendix B for details.

Table 1: Years of Schooling vs. Cognitive Skills in Growth Regressions

	(1)	(2)	(3)	(4)[a]	(5)[b]	(6)[c]	(7)[d]	(8)[e]
Cognitive skills		2.015	1.980	1.975	1.933	1.666	1.265	1.239
		(10.68)	*(9.12)*	*(8.28)*	*(8.29)*	*(5.09)*	*(4.06)*	*(4.12)*
Years of schooling 1960	0.369		0.026	0.024	0.025	0.047	0.004	-0.049
	(3.23)		*(0.34)*	*(0.78)*	*(0.29)*	*(0.54)*	*(0.05)*	*(0.66)*
GDP per capita 1960	-0.379	-0.287	-0.302	-0.298	-0.298	-0.255	-0.351	-0.310
	(4.24)	*(9.15)*	*(5.54)*	*(6.02)*	*(5.04)*	*(3.12)*	*(6.01)*	*(5.73)*
No. of countries	50	50	50	50	52	50	47	45
R^2 (adj.)	0.252	0.733	0.728	0.728		0.706	0.784	0.797

Notes: Dependent variable: average annual growth rate in GDP per capita, 1960-2000. Regressions include a constant. Test scores are average of math and science, primary through end of secondary school, all years. *t*-statistics in parentheses.

a. Measure of years of schooling refers to the average between 1960 and 2000.

b. Robust regression including two outliers (using *rreg* robust estimation command implemented in Stata).

c. Specification includes dummies for the eight world regions depicted in Figure 1.

d. Specification includes additional controls for openness and property rights.

e. Specification includes additional controls for openness, property rights, fertility, and tropical location.

Table 2: Sensitivity of Estimated Effects of Cognitive Skills to the Sample of Countries and Time Periods

Country/year sample ► ▼ Test-score specification	(1) Full	(2) OECD	(3) Non-OECD	(4) High-income[a]	(5) Low-income[a]	(6) W/o East Asia	(7) 1960-1980	(8) 1980-2000	(9) 1980-2000[b]	(10) Score-schooling outliers[c]	(11) Score-schooling core[c]
All math and science	1.980	1.736	2.056	1.287	2.286	1.301	1.522	2.996	3.523	1.888	2.175
	(9.12)	*(4.17)*	*(6.10)*	*(5.37)*	*(6.98)*	*(4.90)*	*(4.29)*	*(9.42)*	*(2.89)*	*(7.81)*	*(3.47)*
Only lower secondary	1.759	1.646	1.792	1.040	2.083	1.137	1.407	2.580	3.703	1.673	1.887
	(9.22)	*(4.02)*	*(6.19)*	*(4.70)*	*(7.44)*	*(4.82)*	*(4.56)*	*(8.88)*	*(3.49)*	*(7.83)*	*(3.45)*
No. of countries	50	23	27	25	25	40	50	50	22	25	25

Notes: Reported numbers are the coefficient on test scores in each model specification. Dependent variable: Unless noted otherwise, average annual growth rate in GDP per capita, 1960-2000. Control variables: Initial GDP per capita, initial years of schooling, and a constant. Test scores: Unless noted otherwise, average of math and science, primary through end of secondary school, all years. *t*-statistics in parentheses.

a. Countries above/below sample median of GDP per capita 1960.

b. Test scores refer only to tests performed until 1984.

c. Countries with largest (outliers)/smallest (core) residuals when regressing years of schooling on test scores.

Table 3: Sensitivity of Estimated Effects of Cognitive Skills to the Measurement of Skills

Country sample ►	(1) Full	(2) OECD	(3) Non-OECD	
▼ Test-score specification				No. of countries
(A) Only since 1995	1.814	1.473	1.850	47
	(9.91)	*(3.80)*	*(6.74)*	
(B) Only lower secondary since 1995	1.644	1.379	1.657	47
	(9.57)	*(3.49)*	*(6.48)*	
(C) Only until 1995	3.568	1.193	4.696	34
	(8.31)	*(1.80)*	*(7.48)*	
(D) Early as instrument for average[a]	1.988	1.187	2.472	34
	(7.49)	*(1.85)*	*(4.75)*	
(E) Only math	2.009	1.529	2.063	47
	(8.98)	*(4.62)*	*(5.81)*	
(F) Only science	1.576	1.769	1.556	50
	(7.00)	*(3.28)*	*(4.48)*	
(G) Only reading	2.351	1.616	2.529	46
	(6.21)	*(3.33)*	*(3.68)*	
(H) All subjects entered jointly				41
Math	1.662	2.270	1.882	
	(3.69)	*(2.97)*	*(1.97)*	
Science	1.007	-2.414	1.270	
	(2.34)	*(1.62)*	*(1.92)*	
Reading	-0.793	1.333	-1.457	
	(1.15)	*(1.44)*	*(0.94)*	

Notes: Reported numbers are the coefficient on test scores in each model specification. Dependent variable: Average annual growth rate in GDP per capita, 1960-2000. Control variables: GDP per capita 1960, years of schooling 1960, and a constant. Test scores: Unless noted otherwise, average of math and science, primary through end of secondary school, all years. *t*-statistics in parentheses.

a. 2SLS with average of test scores until 1995 as instrument for average of all test scores.

Table 4: From Schooling Institutions to Cognitive Skills to Economic Growth: Instrumental Variable Estimates

	(1)	(2)	(3)[a]	(4)[a]
Second stage:				
2SLS:				
Cognitive skills	2.151	2.023	3.050	4.091
	(2.73)	*(5.81)*	*(5.32)*	*(3.20)*
Fuller (1) modification of LIML:				
Cognitive skills	2.121	2.022	3.036	3.871
	(3.01)	*(5.94)*	*(5.42)*	*(3.21)*
Moreira 95% confidence band:				
Cognitive skills	[-3.888, 19.871]	[1.190, 2.868]	[1.601, 4.689]	[1.626, 9.499]
p-value	*(0.100)*	*(0.001)*	*(0.002)*	*(0.001)*
First stage (dependent variable: cognitive skills):				
External exit exam system	0.286	0.286		
	(2.01)	*(2.01)*		
Initial years of schooling		0.176	0.132	0.065
		(4.11)	*(3.83)*	*(2.00)*
Private enrollment share			0.493	
			(2.15)	
Centralization (share) of decisions on organization of instruction				-0.993
				(-2.49)
No. of countries	43	43	19	17
Centered R^2	0.752	0.753	0.791	0.575
First-stage F-statistic	4.04	10.28	9.65	7.37
Sargan statistic		0.033	0.136	0.001
p-value		*(0.856)*	*(0.712)*	*(0.973)*
Durbin-Wu-Hausman χ^2 test	0.034	0.003	0.131	4.209
p-value	*(0.855)*	*(0.957)*	*(0.718)*	*(0.040)*

Notes: Dependent variable (of the second stage): average annual growth rate in GDP per capita, 1960-2000. Control variables: Initial GDP per capita, initial years of schooling, and a constant. Test scores are average of math and science, primary through end of secondary school, all years. *t*-statistics in parentheses unless otherwise noted.

a. Dependent variable: average annual growth rate in GDP per capita, 1980-2000. Sample of OECD countries.

Table 5: Difference-in-Differences Estimates of Returns to Country-of-Origin Cognitive Skills for U.S. Immigrants

Sample:	U.S. educated[a] (1)	Educated in country of origin[b] (2)	All immigrants (3)	W/o Mexico (4)	All immigrants (5)	W/o Mexico (6)	Growth sample[c] (7)	Only English speaking countries (8)
Cognitive skills x Educated in country of origin			0.0873	0.1324	0.1375	0.1398	0.1670	0.1616
			(2.02)	*(3.31)*	*(3.16)*	*(4.13)*	*(3.77)*	*(3.57)*
Cognitive skills	0.0050	0.1582	0.0634	-0.0258	not	not	not	not
	(0.14)	*(2.37)*	*(1.06)*	*(1.42)*	identified	identified	identified	identified
Educated in country of origin			-0.1385	-0.1011	-0.1298	-0.0626	-0.1309	-0.0206
			(3.95)	*(3.03)*	*(2.98)*	*(2.07)*	*(2.58)*	*(0.83)*
Years of schooling	0.1155	0.0673	0.0700	0.0863	0.0579	0.0856	0.0553	0.0992
	(14.08)	*(7.35)*	*(7.43)*	*(13.47)*	*(4.14)*	*(17.40)*	*(4.06)*	*(15.41)*
Potential experience	0.0372	0.0235	0.0243	0.0215	0.0241	0.0227	0.0233	0.0205
	(19.71)	*(4.12)*	*(5.67)*	*(4.75)*	*(7.68)*	*(5.57)*	*(6.50)*	*(2.80)*
Potential experience squared	-0.00064	-0.00035	-0.00036	-0.0004	-0.00039	-0.0004	-0.0004	-0.0004
	(13.02)	*(4.79)*	*(6.10)*	*(4.90)*	*(7.40)*	*(5.87)*	*(6.46)*	*(3.25)*
Fixed effects for country of origin	no	no	no	no	yes	yes	yes	yes
Observations	50,597	258,977	309,574	187,506	309,574	187,506	273,213	72,091
No. of countries	64	64	64	63	64	63	47	12
R^2	0.157	0.170	0.180	0.132	0.196	0.150	0.202	0.156

Notes: Dependent variable: log(annual earnings). Cognitive skills refer to average test score of country of origin (centered at zero). Sample: All immigrants identified by country of birth not in school whose age is greater than 25, who are employed, and who earned more than $1,000 in 1999. Immigrants who had obtained some but not all of their education in the U.S. were excluded from the sample. Immigrants from all countries of origin for which there are cognitive-skill scores, except for the following countries (areas) which could not be identified because of census restrictions on release of data for small cells: Swaziland, Slovenia, Macau-China, Luxembourg, Liechtenstein, Estonia, Botswana, Bahrain, Tunisia, and Iceland. Israel could not be identified separately from Palestine; both were assigned the Israeli score. Robust absolute values of *t*-statistics in parentheses with clustering by country of origin. Source: Authors' calculations from 2000 Census IPUMS data.

a. U.S. educated immigrants are identified as immigrating to the U.S. before the beginning year of schooling.

b. Immigrants educated in their country of origin are identified as immigrating to the U.S. after the final year of schooling.

c. The economic growth sample relies on the data for immigrants from the 50 countries in the basic growth regressions.

Table 6: Changes in Cognitive Skills and Changes in Growth Paths

	(1)	(2)	(3)	(4)	(5)[a]	(6)[b]	(7)	(8)
Trend in cognitive skills	0.084	0.073	0.074	0.074	0.080	0.117		
	(3.10)	*(3.21)*	*(3.07)*	*(3.04)*	*(3.34)*	*(6.90)*		
Dummy for cognitive-skill trend above median							0.117	0.103
							(5.98)	*(4.87)*
Average annual growth rate in GDP per capita 1975-2000		-0.030	-0.035	-0.028	-0.039	-0.085		-0.004
		(2.73)	*(1.61)*	*(1.69)*	*(-2.32)*	*(5.26)*		*(0.21)*
Initial GDP per capita			-0.002					0.005
			(0.27)					*(0.80)*
Change in years of schooling 1975-2000				-0.004				
				(0.21)				
No. of countries	15	15	15	15	15	12	15	15
R^2 (adj.)	0.380	0.586	0.551	0.550	0.582	0.891	0.713	0.735

Notes: Dependent variable: trend in the growth rate of GDP per capita from 1975 to 2000. Regressions include a constant. Sample: OECD countries with test-score data both before 1985 and up to 2003. Test scores are average of math and science, primary through end of secondary school. *t*-statistics in parentheses.

a. WLS with inverse of standard error with which the trend in test scores was estimated as weights.

b. Excluding countries without test scores before 1972 (Canada, Korea, and Norway).

Table 7: Rocket Scientists or Basic Education for All?

	(1)	(2)	(3)	(4)[a]	(5)[b]	(6)[b]	(7)[b]
Share of students reaching basic literacy	4.717		2.732	1.002	3.460	5.150	5.869
	(6.64)		*(3.61)*	*(1.33)*	*(3.81)*	*(2.87)*	*(3.33)*
Share of top-performing students		19.347	12.880	11.735	8.460	4.226	-1.530
		(2.653)	*(4.35)*	*(4.18)*	*(2.37)*	*(0.65)*	*(0.22)*
Share of students reaching basic literacy					0.376		
x Initial GDP per capita					*(1.25)*		
Share of top-performing students					-2.148		-1.649
x Initial GDP per capita					*(2.11)*		*(2.07)*
Share of students reaching basic literacy						42.357	53.538
x Share of top-performing students						*(1.48)*	*(1.91)*
No. of countries	50	50	50	45	50	50	50
R^2 (adj.)	0.610	0.646	0.719	0.823	0.734	0.727	0.746

Notes: Dependent variable: average annual growth rate in GDP per capita, 1960-2000. Control variables: GDP per capita 1960, years of schooling 1960, and a constant. Shares are based on average test scores in math and science, primary through end of secondary school, all years. *t*-statistics in parentheses.

a. Specification includes additional controls for openness, property rights, fertility, and tropical location.

b. All interacted variables are centered on zero.

APPENDIX A. Regional Data

Table A1: Income, Education, and Growth across World Regions

Region[a]	No. countries[b]	GDP per capita 1960 (US$)	Growth of GDP per capita 1960-2000 (%)	GDP per capita 2000 (US$)	Years of schooling 1960	Test score	All Penn World Tables Countries	
							No. countries[c]	GDP per capita 1960 (US$)
Asia	11	1,891	4.5	13,571	4.0	479.8	15	1,642
Sub-Saharan Africa	3	2,304	1.4	3,792	3.3	360.0	40	1,482
Middle East and North Africa	8	2,599	2.7	8,415	2.7	412.4	10	2,487
Southern Europe	5	4,030	3.4	14,943	5.6	466.4	5	4,030
Latin America	7	4,152	1.8	8,063	4.7	388.3	24	3,276
Central Europe	7	8,859	2.6	24,163	8.3	505.3	7	8,859
Northern Europe	5	8,962	2.6	25,185	8.0	497.3	5	8,962
Commonwealth OECD	4	11,251	2.1	26,147	9.5	500.3	4	11,251
Note: Asia w/o Japan	*10*	*1,614*	*4.5*	*12,460*	*3.5*	*474.7*	*14*	*1,427*

Notes:

a. The country observations contained in the eight regions are: Asia (11): China, Hong Kong, India, Indonesia, Japan, Rep. of Korea, Malaysia, Philippines, Singapore, Taiwan, Thailand; Sub-Saharan Africa (3): Ghana, South Africa, Zimbabwe; Middle East and North Africa (8): Cyprus, Egypt, Iran, Israel, Jordan, Morocco, Tunisia, Turkey; Southern Europe (5): Greece, Italy, Portugal, Romania, Spain; Latin America (7): Argentina, Brazil, Chile, Colombia, Mexico, Peru, Uruguay; Central Europe (7): Austria, Belgium, France, Ireland, Netherlands, Switzerland, United Kingdom; Northern Europe (5): Denmark, Finland, Iceland, Norway, Sweden; Commonwealth OECD members (4): Australia, Canada, New Zealand, USA.

b. Sample of all countries by region with internationally comparable data on GDP that ever participated in an international student achievement test; see Appendix B for details.

c. Sample of all countries in Penn World Tables with data on GDP per capita in 1960 by region.

Sources: GDP: own calculations based on Penn World Tables (Heston, Summers, and Aten (2002)); years of schooling: own calculations based on Cohen and Soto (2007); test score: own calculations based on international student achievement tests; see Appendix B for details.

APPENDIX B. Measures of Cognitive Skills

A key element of our work is developing a measure that can equate knowledge of people across countries. In many ways this is an extension of notions of human capital that have been developed over the past half century. But it is a specific refinement that, while important in a variety of applications within nations, becomes a necessity when comparing different countries. Within a country, human capital is often proxied by quantity of schooling. This is partly necessitated by commonly available data but partly justified on the idea that differences in knowledge between levels of schooling are greater than those within levels of schooling.

Until recent publicity, most people were unaware of the international student testing that could provide direct comparisons of student knowledge across countries. In fact, international assessments of student achievement, aimed largely at math and science, were begun over four decades ago. Although national participation has been voluntary, recent expansions to all OECD countries and more have led to increasingly valid and reliable indicators of cognitive skills.

Internationally comparable student achievement tests have been conducted since the First International Mathematics Study (FIMS), which tested in 1964. The latest international studies used in our analyses are the 2003 cycles of the Trends in International Mathematics and Science Study (TIMSS) and the Programme for International Student Assessment (PISA). From FIMS to the latest TIMSS and PISA, a total of 12 international student achievement tests (ISATs) were conducted.[42] Although varying across the individual assessments, testing covers math, science, and reading for three age/grade groups: primary education (age 9/10), lower secondary education (age 13 to 15), and the final year of secondary education (generally grade 12 or 13).

Given this 3x3 grade-by-subject matrix, Table B1 summarizes the specific ISATs that have been conducted in three periods of time: late 1960s/early 1970s (1964-72), 1980s (1982-91), and late 1990s/early 2000s (1995-2003). Several features of the emerging pattern are worth noting. First, math and science have been tested at all three grade levels, while reading has not been tested in the final grade of secondary school. Second, all subjects are available in all periods,

[42] In this study, we do not include the two tests conducted by the International Assessment of Educational Progress (IAEP) in 1988 and 1991, because they used the U.S. NAEP test as their testing instrument, which is geared to the U.S. curriculum and may thus introduce bias to the international testing. By contrast, the tests included here are not associated with the curriculum in any particular country, but have been devised in an international cooperative process between all participating countries.

although coverage is more extensive in math and science than in reading. Third, in each period, the lower secondary level has been tested in all three subjects; thus, there is no primary or final-secondary study that would add a subject not already tested at lower secondary in the period. Fourth, each cell available in 1964-91 has at least one counterpart in 1995-2003.

Table B2 provides additional detail on each ISAT. A total of 77 countries have participated in at least one of the ISATs in math or science, but several of the countries have participated at only one or a few points in time. Even within the same assessment, countries do not always participate at all grade levels. The largest number of countries tends to have participated at the lower secondary level.

To obtain a common measure of cognitive skills, we want to draw on as much internationally comparable information as possible. This raises the issue whether the different ISATs with their different participating countries, student samples, and perspectives on what should be tested (see Neidorf, Binkley, Gattis, and Nohara (2006)) are measuring a common dimension of cognitive skills. For example, the TIMSS tests are related to elements of the school curricula common to participating countries, while the PISA tests are designed to be applied assessments of real-world problems, irrespective of specific curricula. However, the fact is that the TIMSS tests with their curricular focus and the PISA tests with their applied focus are highly correlated at the country level. For example, the correlation between the TIMSS 2003 tests of 8th graders and the PISA 2003 tests of 15-year-olds across the 19 countries participating in both is as high as 0.87 in math and 0.97 in science. It is also 0.86 in both math and science across the 21 countries participating both in the TIMSS 1999 tests and the PISA 2000/02 tests. Thus, ISATs with very different foci and perspectives tend, nonetheless, to be highly related, lending support to our approach of aggregating different ISATs for each country.

The general idea behind our approach to aggregation is that of empirical calibration. We rely upon information about the overall distribution of scores on each ISAT to compare national responses. This contrasts with the psychometric approach to scaling that calibrates tests through common elements on each test. In reality, the international testing situations are separate events with no general attempt to provide common scaling across tests and across the full time period.

The fact that the scales of their test-score results are not directly equated across tests is a major drawback in comparative uses of the various ISATs. They do not use the same test questions; nor do they even use the same technique and scale of mapping answers into test scores.[43] The early tests mainly used aggregate scores in "percent correct" format, but with questions of varying difficulty in the different tests, these scores will not be comparable across tests. The later tests use a more sophisticated scale, constructed using Item Response Theory (IRT). Among other things, IRT weights different questions by their revealed difficulty and then maps answers onto a pre-set scale set to yield a given international mean and standard deviation among the participating countries. However, the questions on which the mapping is based are not the same in the different tests. Even more, the set of participating countries varies considerably across tests, making the separately developed scales incomparable across ISATs.

Therefore, to compare performance on the ISATs across tests and thus over time, we have to project the performance of different countries on different tests onto a common metric. For that, we have to develop a common metric both for the *level* and for the *variation* of test performance.

Comparable level. To make the level of ISATs comparable, we need information on test performance that is comparable over time. Such information is available in the United States in the form of the National Assessment of Educational Progress (NAEP), which tests the math, science, and reading performance of nationally representative samples of 9-, 13-, and 17-year-old U.S. students in an intertemporally comparable way since 1969. This is the only available information on educational performance that is consistently available for comparisons over time. The United States is also the only country that participated in every ISAT. Given the time-series evidence on the performance of U.S. students, we can thus scale the level of each ISAT relative to the known intertemporally comparable test performance of the United States. Figure B1 shows the available NAEP results in the three subjects and age groups. Despite some notable changes, the performance of U.S. students has been relatively flat over the period 1969-1999.

We start by calculating the U.S. performance difference between 1999 and any earlier point in time and express it in standard deviations (s.d.) of the international PISA 2000 study:

$$U_{a,s,t}^{US} = \left(NAEP_{a,s,t}^{US} - NAEP_{a,s,1999}^{US}\right)\frac{SD_{s}^{US,PISA}}{SD_{a,s}^{US,NAEP}} \tag{A1}$$

where U is the standardized performance difference of U.S. students at age a in subject s at time t relative to 1999, $NAEP$ is the age-, subject-, and time-specific NAEP test score, $SD^{US,PISA}$ is the

[43] Recent testing in both TIMSS and PISA has involved overlapping test items that permit test calibration, but these do not provide any benchmarks across the two testing regimes or linked with earlier testing.

subject-specific s.d. of U.S. students on the PISA test, and $SD^{US,NAEP}$ is the age- and subject-specific s.d. of the U.S. NAEP test.[44] NAEP scores are available at 2-4 year intervals over the period; values for non-NAEP years are obtained by linear interpolation between available years.

This alone does not yet yield a common scale for all the countries on the different tests. While we know for each participating country whether it performed above or below the respective U.S. performance on each specific test, we need to make the international variation in test scores comparable across the different ISATs to determine “how much” above or below.

Comparable variation. Developing a common metric for the variation of test scores in the different ISATs is harder to achieve than for the level. There is no explicit external information available on trends in the cross-country performance variation, and the diversity of original tests and participating countries precludes a direct comparison across tests. One way to achieve comparability, though, would be to have a group of countries across which it is reasonable to assume relative constancy in the size of the cross-country variation in test scores and whose members participated in sufficient number in the different tests. This group could only include relatively stable countries with relatively stable education systems over time, which should not have experienced major changes in overall enrollment across the ISATs.

Thus, we suggest two criteria for a group of countries to serve as a standardization benchmark for performance variation over time. First, the countries have been member states of the relatively homogenous and economically advanced group of OECD countries in the whole period of ISAT observations, that is, since 1964. Second, the countries should have had a substantial enrollment in secondary education already in 1964. Given data constraints, we implement this by dropping all countries where more than half of the 2001 population aged 45-54 (the cohort roughly in secondary school in the first ISAT) did not attain upper secondary education (OECD 2003a). There are 13 countries that meet both of these measures of stability, which we term the “OECD Standardization Group” (OSG) of countries.[45]

[44] The s.d. of the NAEP tests in reading for 1984-1996 and in math and science in 1977/78-1996 are reported in U.S. Department of Education, Institute of Education Sciences (2008). Since no s.d. information is available for the earlier and the 1999 NAEP tests, and since the available s.d. are relatively stable over time, we take a simple mean of the available s.d. in each subject at each age level over time. PISA tested only 15-year-olds, but has the same three subjects as the NAEP test.

[45] The OSG countries are: Austria, Belgium, Canada, Denmark, France, Germany, Iceland, Japan, Norway, Sweden, Switzerland, the United Kingdom, and the United States. The Netherlands also meets both criteria, but does not have internationally comparable PISA 2000 data which we require for our standardization.

Under the assumption that the cross-country variation among the OSG countries did not vary substantially since 1964, we can use the OSG countries to develop a comparable scale for the variation on the different ISATs. We do so by projecting the s.d. among those of the OSG countries that participated in any particular ISAT from the subject-specific PISA test onto the particular ISAT. That is, we transform the original test score *O* of country *i* (specific for each age *a* and subject *s*) at time *t* into a transformed test score *X* according to:

$$X^{i}_{a,s,t} = \left(O^{i}_{a,s,t} - \overline{O^{OSG}_{a,s,t}}\right)\frac{SD^{OSG}_{s,PISA}}{SD^{OSG}_{a,s,t}} \tag{A2}$$

The test score *X* has the following distributional characteristics for each ISAT. First, it has a mean of zero among the OSG (attained by subtracting the OSG mean $\overline{O^{OSG}}$ from each country's original test score). Second, it has a between-country s.d. among the OSG that is the same as the s.d. of the very same countries on the PISA test in the specific subject (attained by dividing through the s.d. among the OSG countries in the specific test and multiplying by the s.d. of these same countries in the relevant PISA test). In effect, this rescaled test score now has a metric whose variation is comparable across tests.

Performance on a common metric. Finally, we use the time-series evidence on educational performance in the U.S. derived above to put a common level to the intertemporally comparable metric for the different ISATs. This is achieved in the standardized test score *I*:

$$I^{i}_{a,s,t} = X^{i}_{a,s,t} - X^{US}_{a,s,t} + O^{US}_{s,PISA} + U^{US}_{a,s,t} \tag{A3}$$

which adjusts the variation-adjusted test score *X* so that the U.S. performance level on each test equals the U.S. performance on the PISA test in the specific subject plus the age- and subject-specific adjustment factor *U* based on NAEP as derived in equation (A1) above.

Equation (A3) yields measures of the performance of the participating countries in each ISAT on a common scale that is comparable across ISATs. In effect, the internationally and intertemporally standardized test score *I* projects the PISA scale onto all other tests.

While we are reasonably confident about the comparisons of the standardized scores within the OECD countries which are fully tested in recent years, we are less certain about countries that are far from the measured OECD performance. In particular, countries far off the scale of the original test scores – e.g., two s.d. below the mean – may not be well represented because the tests may be too hard and thus not very informative for them. Our linear transformations are susceptible to considerable noise for these countries.

Our main measure of cognitive skills is a simple average of all standardized math and science test scores of the ISATs in which a country participated. Table B3 reports the basic combined measure for the 77 countries that have ever participated in any of the math and science tests.[46] The sample for our growth regressions contains 50 of these countries.[47]

Distributional measures. Apart from the mean scores, we also analyze the distribution of test scores in each country by accessing the microdata of all ISATs.[48] The kernel density plots for math achievement on the 2003 PISA in Figure B2 show that countries vary significantly in their patterns of test-score distributions. The depicted selected examples of developed countries reveals that it is possible to achieve relatively high median performance both with a relatively equitable spread (Finland) and with a relatively unequal spread (Belgium) in the test scores at the student level. The same is true for countries with low average performance such as the depicted developing countries, where Brazil has a long right tail in contrast to Indonesia which shows a much greater density around its median.

To depict both ends of the distribution, we aim to calculate both the share of students reaching a basic level of literacy in the different subjects equivalent to 400 test-score points on the PISA scale (one student-level s.d. below the OECD mean) and the share of students reaching a top performance level equivalent to 600 test-score points on the PISA scale.

To do so, we use the above transformation to translate these two thresholds into the specific metric of each ISAT. Using the microdata of each ISAT, we then calculate the share of students in each country reaching the thresholds in the overall distribution of the ISAT. The information from the different ISATs is again combined by taking a simple average of the shares across tests.

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[46] The sources of the underlying international test data are*:* Beaton et al. (1996a, 1996b), Lee and Barro (2001), Martin et al. (1997, 2000, 2004), Mullis et al. (1997, 1998, 2000, 2003, 2004), OECD (2001, 2003b, 2004), and own calculations based on the microdata of the early tests.

[47] Twenty-five of the total of 77 countries with cognitive-skill data are not included in the growth database due to lack of data on economic output or because they drop out of the sample for a standard exclusion criterion in growth analyses (15 former communist countries, 3 countries for which oil production is the dominant industry, 2 small countries, 3 newly created countries, 2 further countries lacking early output data). In addition, two strong outliers are excluded in most models (see above). There are four countries with cognitive-skill data which have a few years of economic data missing at the beginning or end of the 1960-2000 period. Data for Tunisia start in 1961, and data for Cyprus (1996), Singapore (1996), and Taiwan (1998) end slightly earlier than in 2000. These countries were included in the growth regressions by estimating average annual growth over the available 36-to-39-year period.

[48] Unfortunately, the microdata from the FIMS test do not seem to be available in an accessible way any more, so that the distributional measures only draw on the remaining ISATs.

Trends over time. The standardized performance information over a long period of time also allows us to derive longitudinal patterns of test scores for countries that participated both in early and recent ISATs. Given the amplification of noise in first-differenced data and the limitations of our rescaling method for poorly performing countries mentioned above, we perform the trend estimation only for the sample of 15 OECD countries that participated both in an ISAT before 1985 (i.e., on FIMS, FISS, FIRS, SIMS, or SISS) and up to 2003, spanning a period of nearly 20 years. (Twelve countries participated in a test before 1971, spanning a period of over 30 years.)

To estimate the trend in test performance, for each country we regress performance on the different ISATs, expressed on the standardized test metric developed above, on dummies for the subjects, dummies for the age groups, and on the year the test was conducted. The unit of observation in these country-specific regressions is each subject-by-age-by-year occasion of an ISAT, using all available tests, subjects, and age groups (see Appendix Table B2). To account for heteroscedasticity and for the fact that the signal-to-noise ratio will be larger the smaller the number of OSG countries that participated in a test, we weight the regression by the square root of the number of OSG countries participating in each test. The coefficient on the year variable provides us with the time trend that we are interested in. The patterns captured by these country-specific regressions are shown in Figure B3 that simply extrapolates scores for the range of 1975-2000 with scores anchored by the PISA 2000 score.

One possible worry with combining the different tests into a single measure and with estimating performance changes over time is that enrollment shares have changed to different extents over time, especially at the secondary level. To test the extent to which this affects our cognitive-skill measures, we calculated the correlation between our measure of trend in test scores and changes in enrollment rates. It turns out that the two are orthogonal to each other, diluting worries that differential changes in enrollment bias the results reported in this paper.

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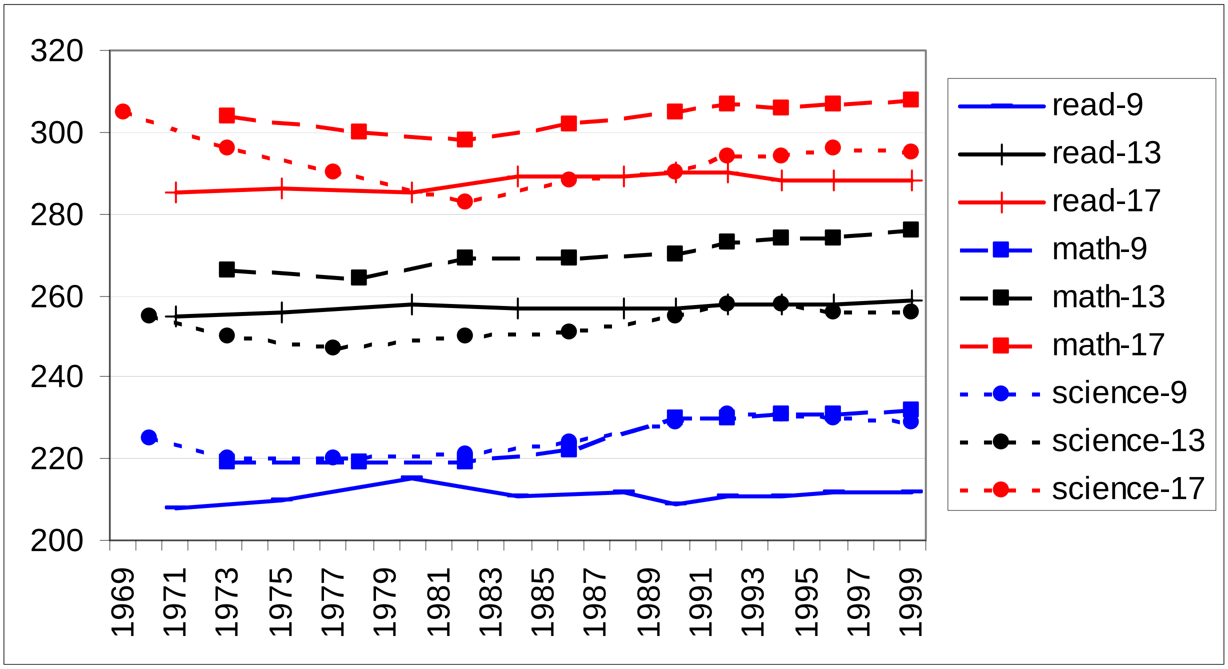
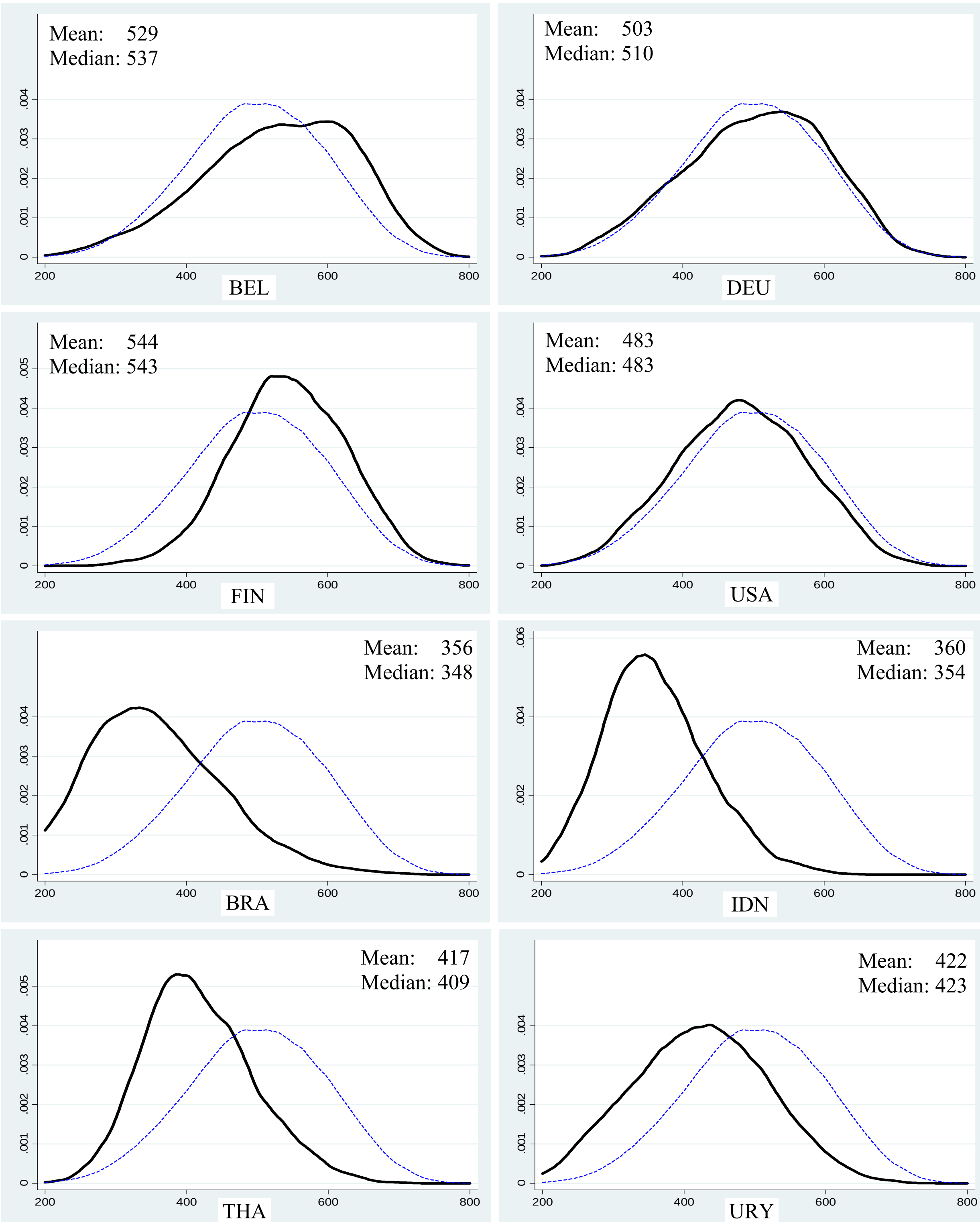
Figure B1: Student Achievement in the United States over Time: The National Assessment of Educational Progress (NAEP)



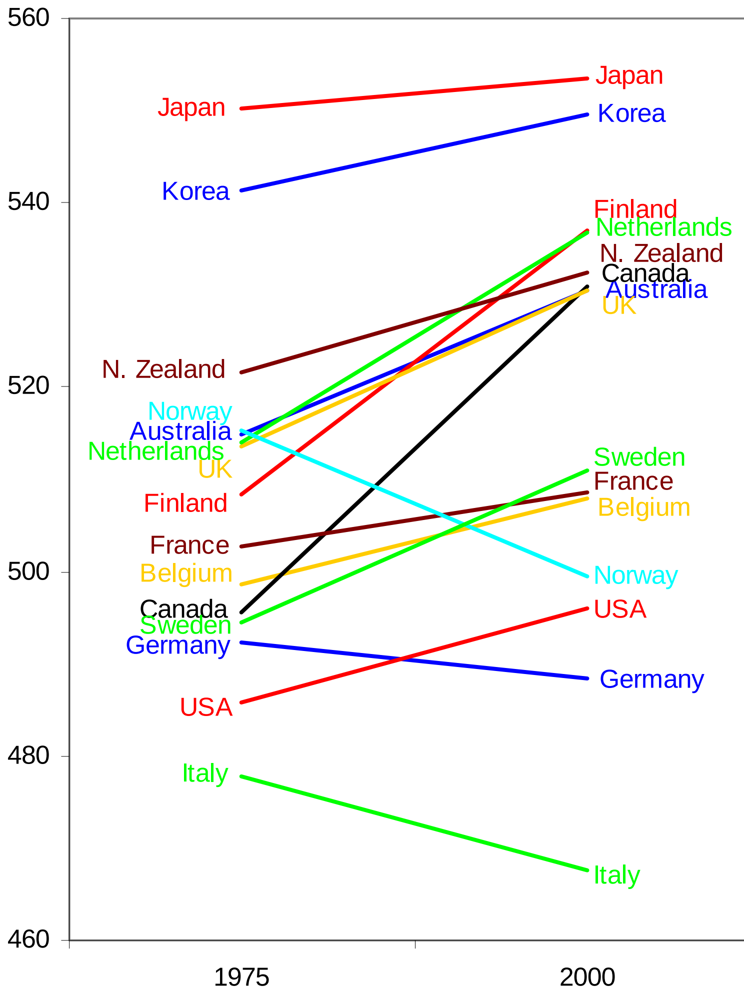
Source: U.S. Department of Education, Institute of Education Sciences (2008).

Figure B2: Selected Examples of the Distribution of Student Performance



Notes: Kernel densities of student performance on the PISA 2003 math test. Bold solid line: specified country; thin dotted line: OECD countries. Codes: Belgium (BEL), Germany (DEU), Finland (FIN), United States of America (USA), Brazil (BRA), Indonesia (IDN), Thailand (THA), Uruguay (URY).

Figure B3: Trends in Test Scores



Notes: Depiction based on PISA 2000 performance and a backward induction based on the coefficient on a time variable from a regression of all available international test scores (by year, age group, and subject) on the time variable and dummies for age group and subject. See Appendix B for details.

Table B1: International Tests by Period, Subject, and Age Group

1964-72

	Math	Science	Reading
Primary		FISS	
Lower secondary	FIMS	FISS	FIRS
Final secondary	FIMS	FISS	

1982-91

	Math	Science	Reading
Primary		SISS	SIRS
Lower secondary	SIMS	SISS	SIRS
Final secondary	SIMS	SISS	

1995-2003

	Math	Science	Reading
Primary	TIMSS TIMSS 2003	TIMSS TIMSS 2003	PIRLS
Lower secondary	TIMSS TIMSS-Repeat PISA 2000/02 TIMSS 2003 PISA 2003	TIMSS TI-Repeat PISA 2000/02 TIMSS 2003 PISA 2003	PISA 2000/02 PISA 2003
Final secondary	TIMSS	TIMSS	

Notes: For abbreviations and details, see Table B2.

Table B2: The International Student Achievement Tests

	Abbr.	Study	Year	Subject	Age[a,b]	Countries[c]	Organization[d]	Scale[e]
1	FIMS	First International Mathematics Study	1964	Math	13,FS	11	IEA	PC
2	FISS	First International Science Study	1970-71	Science	10,14,FS	14,16,16	IEA	PC
3	FIRS	First International Reading Study	1970-72	Reading	13	12	IEA	PC
4	SIMS	Second International Mathematics Study	1980-82	Math	13,FS	17,12	IEA	PC
5	SISS	Second International Science Study	1983-84	Science	10,13,FS	15,17,13	IEA	PC
6	SIRS	Second International Reading Study	1990-91	Reading	9,13	26,30	IEA	IRT
7	TIMSS	Third International Mathematics and Science Study	1994-95	Math/Science	9(3+4), 13(7+8),FS	25,39,21	IEA	IRT
8	TIMSS-Repeat	TIMSS-Repeat	1999	Math/Science	13(8)	38	IEA	IRT
9	PISA 2000/02	Programme for International Student Assessment	2000+02	Reading/ Math/Science	15	31+10	OECD	IRT
10	PIRLS	Progress in International Reading Literacy Study	2001	Reading	9(4)	34	IEA	IRT
11	TIMSS 2003	Trends in International Mathematics and Science Study	2003	Math/Science	9(4),13(8)	24,45	IEA	IRT
12	PISA 2003	Programme for International Student Assessment	2003	Reading/ Math/Science	15	40	OECD	IRT

Notes:

a. Grade in parentheses where grade level was target population.

b. FS = final year of secondary education (differs across countries).

c. Number of participating countries that yielded internationally comparable performance data.

d. Conducting organization: International Association for the Evaluation of Educational Achievement (IEA); Organisation for Economic Co-operation and Development (OECD).

e. Test scale: percent-correct formal (PC); item-response-theory proficiency scale (IRT).

Table B3: International Data on Cognitive Skills

Code	Country	Growth sample[a]	cognitive[b]	lowsec[c]	basic[d]	top[e]
ALB	Albania	0	3.785	3.785	0.424	0.013
ARG	Argentina	1	3.920	3.920	0.492	0.027
ARM	Armenia	0	4.429	4.490	0.745	0.008
AUS	Australia	1	5.094	5.138	0.938	0.112
AUT	Austria	1	5.089	5.090	0.931	0.097
BEL	Belgium	1	5.041	5.072	0.931	0.094
BGR	Bulgaria	0	4.789	4.789	0.765	0.083
BHR	Bahrain	0	4.114	4.114	0.608	0.003
BRA	Brazil	1	3.638	3.638	0.338	0.011
BWA	Botswana	0	3.575	3.575	0.374	0.000
CAN	Canada	1	5.038	5.125	0.948	0.083
CHE	Switzerland	1	5.142	5.102	0.919	0.134
CHL	Chile	1	4.049	3.945	0.625	0.013
CHN	China	1	4.939	4.939	0.935	0.083
COL	Colombia	1	4.152	4.152	0.644	0.000
CYP	Cyprus	1	4.542	4.413	0.825	0.011
CZE	Czech Rep.	0	5.108	5.177	0.931	0.122
DNK	Denmark	1	4.962	4.869	0.888	0.088
EGY	Egypt	1	4.030	4.030	0.577	0.010
ESP	Spain	1	4.829	4.829	0.859	0.079
EST	Estonia	0	5.192	5.192	0.973	0.095
FIN	Finland	1	5.126	5.173	0.958	0.124
FRA	France	1	5.040	4.972	0.926	0.085
GBR	United Kingdom	1	4.950	4.995	0.929	0.088
GER	Germany	0	4.956	4.959	0.906	0.105
GHA	Ghana	1	3.603	3.252	0.403	0.010
GRC	Greece	1	4.608	4.618	0.798	0.042
HKG	Hong Kong	1	5.195	5.265	0.944	0.123
HUN	Hungary	0	5.045	5.134	0.941	0.103
IDN	Indonesia	1	3.880	3.880	0.467	0.008
IND	India	1	4.281	4.165	0.922	0.013
IRL	Ireland	1	4.995	5.040	0.914	0.094
IRN	Iran	1	4.219	4.262	0.727	0.006

Table B3 (continued)

Code	Country	Growth sample[a]	cognitive[b]	lowsec[c]	basic[d]	top[e]
ISL	Iceland	1	4.936	4.945	0.908	0.074
ISR	Israel	1	4.686	4.660	0.826	0.053
ITA	Italy	1	4.758	4.693	0.875	0.054
JOR	Jordan	1	4.264	4.264	0.662	0.044
JPN	Japan	1	5.310	5.398	0.967	0.168
KOR	Korea, Rep.	1	5.338	5.401	0.962	0.178
KWT	Kuwait	0	4.046	4.223	0.575	0.000
LBN	Lebanon	0	3.950	3.950	0.595	0.002
LIE	Liechtenstein	0	5.128	5.128	0.860	0.198
LTU	Lithuania	0	4.779	4.694	0.891	0.030
LUX	Luxembourg	0	4.641	4.641	0.776	0.067
LVA	Latvia	0	4.803	4.779	0.869	0.050
MAC	Macao-China	0	5.260	5.260	0.919	0.204
MAR	Morocco	1	3.327	3.243	0.344	0.001
MDA	Moldova	0	4.530	4.419	0.787	0.029
MEX	Mexico	1	3.998	3.998	0.489	0.009
MKD	Macedonia	0	4.151	4.151	0.609	0.028
MYS	Malaysia	1	4.838	4.838	0.864	0.065
NGA	Nigeria	0	4.154	4.163	0.671	0.001
NLD	Netherlands	1	5.115	5.149	0.965	0.092
NOR	Norway	1	4.830	4.855	0.894	0.056
NZL	New Zealand	1	4.978	5.009	0.910	0.106
PER	Peru	1	3.125	3.125	0.182	0.002
PHL	Philippines	1	3.647	3.502	0.485	0.006
POL	Poland	0	4.846	4.861	0.838	0.099
PRT	Portugal	1	4.564	4.592	0.803	0.032
PSE	Palestine	0	4.062	4.062	0.571	0.008
ROM	Romania	1	4.562	4.562	0.780	0.046
RUS	Russian Fed.	0	4.922	4.906	0.884	0.081
SAU	Saudi Arabia	0	3.663	3.663	0.331	0.000
SGP	Singapore	1	5.330	5.512	0.945	0.177
SRB	Serbia	0	4.447	4.447	0.718	0.024
SVK	Slovak Rep.	0	5.052	5.052	0.906	0.112

Table B3 (continued)

Code	Country	Growth sample[a]	cognitive[b]	lowsec[c]	basic[d]	top[e]
SVN	Slovenia	0	4.993	5.076	0.939	0.061
SWE	Sweden	1	5.013	4.948	0.939	0.088
SWZ	Swaziland	0	4.398	4.398	0.801	0.004
THA	Thailand	1	4.565	4.556	0.851	0.019
TUN	Tunisia	1	3.795	3.889	0.458	0.003
TUR	Turkey	1	4.128	4.128	0.582	0.039
TWN	Taiwan (Chinese Taipei)	1	5.452	5.599	0.958	0.219
URY	Uruguay	1	4.300	4.300	0.615	0.049
USA	United States	1	4.903	4.911	0.918	0.073
ZAF	South Africa	1	3.089	2.683	0.353	0.005
ZWE	Zimbabwe	1	4.107	4.107	0.684	0.010

Notes: A data file is available at www.cesifo.de/woessmann#data.

a. Indicator of whether country is in the main sample of 50 countries contained in the growth regressions, for which internationally comparable GDP data are available.

b. Average test score in math and science, primary through end of secondary school, all years (scaled to PISA scale divided by 100).

c. Average test score in math and science, only lower secondary, all years (scaled to PISA scale divided by 100).

d. Share of students reaching basic literacy (based on average test scores in math and science, primary through end of secondary school, all years).

e. Share of top-performing students (based on average test scores in math and science, primary through end of secondary school, all years).

APPENDIX C. Descriptive Statistics

Table C1: Descriptive Statistics for the Growth Models

	Observations	Mean	Std. Dev.	Min	Max
Average annual growth rate in GDP per capita 1960-2000	50	2.903	1.387	0.967	6.871
Cognitive skills (all grades)	50	4.546	0.611	3.089	5.452
Cognitive skills (lower secondary)	50	4.535	0.671	2.683	5.599
Share of students reaching basic literacy	50	0.761	0.215	0.182	0.967
Share of top-performing students	50	0.062	0.054	0.000	0.219
GDP per capita 1960	50	4,991	3,676	685	14,877
Years of schooling 1960	50	5.447	2.877	0.611	10.963
Years of schooling, average 1960-2000	50	7.425	2.654	2.098	11.845
External exit exam system	43	0.661	0.467	0	1
Private enrollment share	19	0.186	0.206	0	0.720
Centralization (share) of decisions on organization of instruction	17	0.104	0.117	0	0.380

Notes: Descriptive statistics for variables used in Tables 1-4 and 7. See main text for data sources.

Table C2: Descriptive Statistics for the U.S.-Immigrant Models

	Observations	Mean	Std. Dev.	Min	Max
Annual earnings	309,574	33,243	40,983	1,000	385,000
Cognitive skills	309,574	4.334	0.535	3.089	5.452
Educated in country of origin	309,574	0.837	0.370	0	1
Years of schooling	309,574	11.558	5.006	0	20
Potential experience	309,574	24.841	11.966	0	87

Notes: Descriptive statistics for variables used in Table 5. See main text for data sources.

Table C3: Descriptive Statistics for the Changes-in-Growth-Paths Models

	Observations	Mean	Std. Dev.	Min	Max
Trend in growth rate of GDP per capita 1975-2000	15	-0.007	0.071	-0.118	0.106
Trend in cognitive skills	15	0.409	0.546	-0.630	1.420
Average annual growth rate in GDP per capita 1975-2000	15	2.318	1.106	0.855	5.978
GDP per capita 1975	15	13,884	3,217	3,720	18,175
Change in years of schooling 1975-2000	15	1.994	0.895	0.899	4.376

Notes: Descriptive statistics for variables used in Table 6. See main text for data sources.