The Challenge of the Lack of Education

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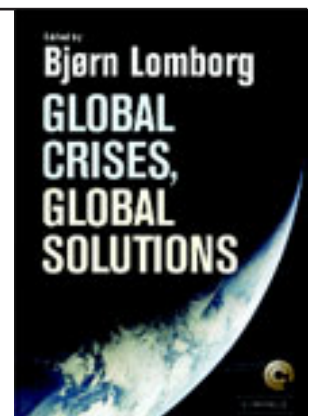
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Comments on the Opportunities to Address
the Global Challenge of Lack of Education

"Opponent Note" on the Challenge Paper on Education for the Copenhagen Consensus

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This note is meant to provide critical comments on Lant Pritchett's (2004) paper on the "Education" challenge for the Copenhagen Consensus. As I will argue in the first section, I think that the challenge paper does a wonderful job of defining the challenge and of discussing relevant opportunities to address it. I do not see major omissions, nor do I think that there are gross imbalances in the paper's assessments. What I would like to add is some additional details and cost-benefit discussions on the opportunity constituted by systemic school reform (section 2), as well as a more positive view on the cost-benefit assessment of the opportunity of increasing demand for education by eliminating primary-school fees (section 3). The note closes with some concluding comments (section 4). The appendix has two more specific comments for the author of the challenge paper, presumably less key to the Copenhagen Consensus assessment.

1. General Assessment

Pritchett (2004, p. 1) defines the challenge of lack of education as one of "creating competencies and learning achievement" rather than just formal school enrollment. It seems obvious that the chosen one is the preferable definition, which is only worth stressing because unfortunately, many policy discussions do not make the same choice. The definition is particularly appropriate because it places a focus on the quality as well as quantity of schooling, which has been shown to be of key relevance in much recent research.

Pritchett also provides a very useful framework for choosing and structuring the most important opportunities to alleviate the challenge of lack of education. I wholeheartedly agree with the challenge paper's main thrust of argument and general assessment of the costs and benefits of the different opportunities to tackle the education challenge. E.g., when Pritchett (2004, p. 28) states that there may be a divide of opinion in the literature as to whether policy actions will be sufficient to meet the challenge or whether this will require systemic reform, I think that evidence forces me to be firmly on his side of the divide, stressing the need for institutional reforms in addition to resource policies.

The paper discusses the opportunities most often considered in public debates, and shows that many of the "usual suspects" have been shown to give rise to only small benefits. Therefore, it has to be doubted that these would substantially alleviate the challenge of lack of education. E.g., on the supply side, overwhelming evidence has shown that simple physical expansion (opportunity 1) and increased spending per student (opportunity 2) generally do not seem to lead to substantial increases in children's competencies and learning achievement. On the demand side, Pritchett stresses that increased income and education of parents as well as increased returns to schooling (opportunity 3) would probably be very effective ways to alleviate the lack of education. However, these features are not subject to easy policy control, so that unfortunately, this again is not a clear practical "opportunity" to address the challenge. I agree with all these assessments.

The other opportunity on the demand side is to reduce the cost of schooling for households (opportunity 4). Much evidence shows that actions in this direction have the potential to alleviate the challenge of lack of education. However, Pritchett sounds a cautioning note on this opportunity

as well, stressing that the costs of such actions can easily be so large as to not warrant the benefits. While this cautioning note is clearly warranted, in section 3 below I will give a more optimistic assessment of one opportunity in this class, namely the eradication of fees for primary schooling.

Finally, Pritchett stresses that probably the most important opportunity to address the education challenge is systemic performance-focused school reform (opportunity 5, or Pritchett's "fifth element"). Such systemic reform would set clear objectives and hold producers accountable for achieving them, while at the same time giving producers the autonomy required to do so. I could not agree more.

The "bad news" from the point of view of the Copenhagen Consensus project is that systemic reform is not really a simple "project" where you can put money into and see what benefits this money brings about. This is not about having $50 billion and wondering where to spend it, because the very behavior of government is endogenized in this model of how to face the challenges of the world. This is somewhat disillusioning for the Copenhagen Consensus effort, as it is difficult to place this key opportunity in the framework of applied welfare economics, attaching a net present value of costs and benefits to it. Still, some sort of quantification does not seem impossible at least on the benefit side. Much research over the recent years has shown that institutional reforms that focus on altering the incentives of the people involved by increasing accountability, choice, and autonomy have very large benefits indeed. This leads into a discussion of feasibility, asking why it is that there is a lack of education in the first place. Arguably, it is the opposing interests of many actors in the field that have prevented an alleviation of the challenge so for – and, without systemic reform, will keep preventing it in the future, whatever budget-relevant policy action is implemented. Thus, systemic reform has to be considered the binding constraint on the global challenge of lack of education. Given that the discussion in the challenge paper gets a bit thin on this topic in the end, I will add some additional aspects and cost-benefit assessments on this opportunity in the following section.

2. Additional Aspects on Institutions and Incentives

While Pritchett (2004) emphasizes institutional reform as the key opportunity to alleviate a lack of education, the final part of the challenge paper discussing this opportunity is a little flimsy, sometimes lacking concreteness, justification, and an assessment of costs and benefits. E.g., the reader is left wondering if and how measurement of different outputs can be achieved (p. 62), which concrete functions should or should not be decentralized (p. 63), where the recommendation list for a "well ordered system for schooling" is derived from (p. 61), which positive theory it is based on, and what the returns to specific reforms may be (p. 65). The following brief notes try to fill some of these gaps a little.

2.1 Some Additional Concrete Elements of Successful Systemic Reform

One concrete systemic element focusing on introducing accountability that has been shown to be strongly related to superior student learning are curriculum-based exit exams that are external to schools (e.g., Bishop 2004; Wößmann 2003a). Another means to increase accountability are explicit school-focused accountability systems, which have been shown to increase students' learning achievement in the US (Carnoy and Loeb 2003; Hanushek and Raymond 2004).

In terms of school autonomy, using data from international student achievement tests Wößmann (2003a) finds that students learn more in schools that have autonomy in process and personnel decisions and in schools whose teachers have both incentives and powers to select appropriate teaching methods. By contrast, in curricular and budgetary decision-making areas, centralized control mechanisms are related to better student learning.[1] In addition, there are important

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[1] For a positive model of why and how institutions such as central exams and school autonomy affect behavior and ultimately students' learning achievement, see Bishop and Wößmann (2004).

interaction effects between external exams and school autonomy, in that school autonomy is more beneficial in systems that have external exit exams (Wößmann 2003b). Thus, external exams are a precondition for decentralized education systems to function properly, just as centrally provided currencies are for decentralized economic systems. Systemic school reforms should thus combine central exams with school autonomy, setting and testing standards externally but leaving it up to schools how to pursue them.

Jimenez and Paqueo (1996) find that local financial contributions increased the productivity of public schools in the Philippines relative to central financing, and Jimenez and Sawada (1999) show that enhanced community and local involvement improved student learning in El Salvador. Another systemic feature that introduces performance orientation focuses on the incentives of teachers. Lavy (2002, 2003) shows that monetary incentives for teachers based on their students' performance immensely improved student learning in Israel. Finally, Colombia ran a program that provided vouchers for the attendance of private schools, which can be argued to be more subject to performance-focused incentives than public schools due to market forces. The benefits of this program have been found to clearly exceed its cost, which was similar to providing a place in public schools (Angrist et al. 2002). Similarly, James et al. (1996) and Bedi and Garg (2000) find that privately managed schools in Indonesia are more efficient and effective than public schools.

2.2 The Costs and Benefits of Systemic Reform

It seems hard to perform a meaningful cost-benefit analysis of systemic reforms. This is particularly true for the cost side. What are the costs of systemic reform? These are probably mainly not directly budget-related, but tend to lie in the political realm of breaking opposing interests. I am unaware of any study trying to quantify such costs in monetary terms. However, it seems fair to say that any such implementation costs are probably similar for meeting most other challenges and not restricted to education. In contrast to the political costs, the direct costs of most systemic reforms in education seem to be miniscule (see Hoxby 2002 for evidence in the case of accountability programs).

Likewise, quantifying the benefits of systemic reform is difficult, albeit not impossible. To get economic estimates of the large benefits found in recent research in terms of student learning, the easiest way would be to focus on effects on later earnings in the labor market. There are probably also significant non-market benefits to increased student learning (see also section 4 below), but these are much harder to measure. Thus, one can at least estimate the benefits of systemic reforms in terms of additional (quantity and/or quality of) education, and then try to link them to estimates of how they translate into additional earnings. In this sense, it seems somewhat unfortunate that Pritchett (2004, p. 5) chose not to review the benefits of quality basic education, as these should probably be the founding stone for any cost-benefit analysis of educational opportunities, as well as for their comparison to opportunities that address other global challenges.

Given limited concrete developing-country evidence on effects of institutional variation on student learning, as well as of learning achievement on earnings, much of this evidence will have to come from cross-country and developed-country evidence. Therefore, the presented estimates have to be faced with considerable caution. Still, for example in the case of curriculum-based external exit exams, cross-country evidence from at least five different international student achievement tests and cross-regional evidence from Canada, Germany, and the US all show that such exam systems are a powerful accountability device that yields benefits in terms of students' learning achievement that dwarf any effect found for resource-related policies (cf. Bishop 2004). The effects found in these studies are often as large as a whole grade-level equivalent and more, or roughly some 40% of an international standard deviation in test scores (Wößmann 2003b). The different systemic features

analyzed in Wößmann (2003a) combine to a total effect that equals about two international standard deviations in test scores.[2]

Unfortunately, not much is known about how exactly such gains in educational achievement translate into economically quantifiable benefits, particularly in developing countries. However, to give a rough assessment, one might refer to Krueger's (2003) estimate that a plausible assessment of the existing evidence might be that an improvement in test-score performance by one standard deviation is associated with an 8% improvement in annual earnings in Britain and the United States. Thus, external exit exams might increase annual earnings by more than 3%, and combined with autonomy effects, the total effect may be as large as 16%. Given that these increases in annual earnings accrue throughout lifetime for each individual who is taught in the reformed system, the total benefits of the reform would simply be immense. Whether taking the narrow external-exam estimate or the broader institutional-reform estimate, this rough back-of-the-envelope calculation suggests that the total benefit of these systemic reforms would equal 3-16% of the total net present value of all lifetime earnings of all students.[3] This is astoundingly large, compared to whatever the cost of such a reform may be in the end, and it does not even consider additional promising systemic reforms such as performance-related teacher incentives and choice-based reforms.

3. Effects and Cost Effectiveness of Fee Waivers

3.1 Dropping Out of School Versus Never Enrolling in School

Before getting to the fee issue concretely, I would like to briefly downplay the emphasis that Pritchett (2004, pp. 7-8 and 16-17) lays on the difference between insufficient education due to dropping out of school and insufficient education due to never enrolling in school. Pritchett stresses that the problem of dropping out is much more important than the problem of never enrolling. First, based on the evidence that he presents, my feeling is that this assertion is only valid for middle-income countries at best. In those regions of the world where the challenge of lack of education is strongest (sub-Saharan Africa and South Asia), the problem of not enrolling actually seems to be more important than the problem of dropping out. According to Pritchett's Table 2, only 15% (16%) of the deficit from universal completion of grade 5 in West and Central Africa (South Asia) is due to dropping out, and as many as 44% (28%) of children never enroll in school. Furthermore, the lack of education is most severe for those who never enroll, so this latter group presents a very important margin for action.

Second, the difference between dropping out and never enrolling is probably not an utterly crucial one anyways. This is because both problems are probably mainly due to similar reasons (high costs relative to household income, low quality of schools, low rate of return) and susceptible to similar policy remedies. Thus, if a main constraint is high school fees, then this will probably lead some households to decide that they cannot afford to enroll (some of) their children at all, and at the same time lead other households to decide that they can afford to enroll (some of) their children for, say, three years, after which they will have to drop out of school.

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[2] One caveat is that all these estimates are based on secondary rather than primary education, so that they can only serve as a rough estimate of how large the benefits of systemic reforms in primary education may be.

[3] To give a general idea of how large this is in monetary terms, one could make the following assumptions. First, assume that the benefits apply to all children of primary-school age (6-11) in the developing world, which are 623 million (Delamonica et al. 2001). Second, assume that the relevant average income is the simple mean of low and lower-middle income countries, which is 770 US$ (gross national income per capita in purchasing power parity, World Bank 2002a). Third, assume that the benefits accrue for an average working life of 25 years and that the relevant discount rate is 5%. Then, the total net present value of a 3% increase in earnings would equal 213 billion US$, and 1.1 trillion US$ for a 16% increase.

3.2 The Impact of Primary-School Fees

In situations of insufficient public budgets, user fees have been cautiously advocated as a means to increase enrollment (e.g., Mingat and Tan 1986), and most developing countries today impose some type of user fees in primary education (World Bank 2002b, p. 12).[4] However, school fees that might seem rather low to outside observers prove to be prohibitive for people living in extreme poverty. Even though parents might be well aware of potentially substantial future benefits of education for their children, the mere necessity to obtain the means to survive inhibits them from financing school fees for their children. If poor people in developing countries are credit or even cash constrained, fees may lead to suboptimally low education. In such a situation, a feasible positive model might indeed predict, "if you make schooling free, they will come!" (compare to Pritchett 2004, p. 24). This has certainly proved to be the case in Malawi and Uganda, where direct primary-school fees have been abolished in the mid-1990s and school enrollment has shot up immediately and substantially (cf. Al-Samarrai and Zaman 2002; Deininger 2003).[5] Kremer (2003) presents additional evidence that school participation is quite elastic to cost, and Spohr (2003) reports noteworthy effects of an extension of tuition-free education from 6 to 9 years in Taiwan. Still, the potential importance of school fees seems to be largely neglected in current policy initiatives, such as Education for All.

In the challenge paper, Pritchett (2004, p. 57) raises three points of caution against a policy of primary-school fee waivers. First, the revenue forgone might be substantial. However, I will argue below that it might still be easily dwarfed by the potential benefits. Second, evidence from Malawi seems to suggest that the attainment and persistence response to the fee waiver was not nearly as large as the enrollment response. To some extent, this point is well taken, as it only shows that complementary, mainly systemic, reforms as well as a commitment to replacing the fees by public funds are needed for a sustained effect, both of which are largely missing in Malawi. Pritchett shows that attainment of 15-year-olds in grade 6 to 8 in Malawi was not substantially higher in 2000 than in 1992, arguing that this shows a lack of sustained attainment effect. This is quite misleading, however, because the fee waiver was only introduced in 1994, and the children entering school in that year would have only reached grade 6 at best in 2000. Given that attainment in grades 1 to 5 has increased substantially, one may be more optimistic on this example. Third, the revenue forgone by waiving fees will have to be replaced by public funds, as otherwise the learning achievement of all students may suffer, a point which is certainly correct.

3.3 Cost-Benefit Analysis of Fee Waivers

Ultimately, the decisive question runs down to one of comparing costs and benefits of waiving fees. At first sight, this does not seem to be promising for fee waivers, because they are untargeted and thus particularly prone to Pritchett's (2004, p. 55-59) cost-effectiveness caveat with respect to infra-marginal transfers. For the costs and benefits of a targeted poverty-reduction program in Mexico that made transfers contingent on children attending school, Schultz (2004) estimates an internal rate of return of 8% in terms of the educational outcomes, which are in addition to the main poverty-alleviation objective pursued by the program. Unfortunately, no explicit cost-benefit analysis of fee waivers is available in the literature. Still, using available information from the Uganda case, we might get a rough idea of which ballpark the costs and benefits lie in. In 1997,

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[4] While the discussion here focuses only on primary-school fees, it should be noted that due to significant cross-price elasticities (cf. Lavy 1996), fees in secondary school will also have negative effects on decisions to enroll in primary school.

[5] Note that public school fees generally do not vary within countries at a given point in time, so that cross-sectional studies of student enrollment in individual countries, such as Nielsen (2001) and Handa (2002), cannot analyze the impact of public school fees because there is no variation. Those schooling costs which they can analyze are not necessarily exogenous to educational enrollment in a cross-sectional setting, as they are mainly the outcome of endogenous parental choices to pay voluntarily.

Uganda eliminated primary-school fees, which beforehand had been a major contribution to school financing, for up to four children per household. Using data from two household surveys, Deininger (2003) shows that the primary-school attendance rate increased dramatically, from 62.1% (in 1992) to 83.6% (in 1999) on average.[6] According to World Bank (2002c) data, total primary-school enrollment increased from 3.4 million children in 1996 to 6.9 million in 2001.

The costs of this fee waiver policy are both the forgone fees of those students who would have attended school anyways and the full additional cost of schooling for the new students. Direct data on the fees are not available, but we have estimates of the total costs of primary education per student per year. These might over-estimate the fee by a wide margin. Delamonica et al. (2001) report that the costs of primary education per student per year in Uganda are 13 US$. Based on this estimate, paying for the total costs of primary education for all students in school after the fee waiver costs 89.7 million US$ per year. However, the cost figure that Delamonica et al. (2001) report for Uganda is relatively low even among sub-Saharan African countries. The costs of primary education per student per year in the median sub-Saharan African country are more than three times as large, at 45 US$. Using this figure, the total costs of primary education for all students in school after the fee waiver would be estimated at 310.5 million US$ per year.

What are the benefits of the Ugandan fee waiver, which created an additional 3.5 million individual years of schooling each year, in monetary terms? Taking the mean of the available estimates across sub-Saharan African countries surveyed in Psacharopoulos and Patrinos (2004), each additional year of schooling increases individuals' earnings by 11.7% (based on the coefficient on years of schooling in standard Mincer equations).[7] Using the mean sub-Saharan African per capita income and years of schooling to which this estimate applies, this means that increasing a person's years of education from none at all to five years would raise her or his annual income by 321 US$.[8] If we simply divide this by the 5 years, this gives a rough estimate of an additional annual income per additional year of schooling of 64.2 US$. Assuming that on average, people might get this additional annual income for maybe 25 years, and using a discount rate of 5%, this is equivalent to a net present value of additional lifetime earnings of 950 US$ for each additional year of schooling. Multiplying this by the 3.5 million additional years of schooling created each year by the fee waiver, a rough estimate of the total benefits created by the fee waiver in Uganda stands at 3.3 billion US$.[9]

Comparing this to the cost estimate of 89.7 million US$, we get a stunning benefit-to-cost ratio of 37! Even with the overrated cost estimate of 310.5 million US$, the benefit-to-cost ratio would be larger than 10. That is, even though infra-marginal transfers may be high, they seem to be dwarfed by the generated benefits. Clearly, these estimates have to be taken with extreme caution, as a lot of very imprecise measures entered the calculations. Furthermore, increasing enrollment so quickly and dramatically will probably have a negative effect on the quality of schooling, because, e.g., there may simply not be enough trained teachers around, which may reduce the total benefits of the policy. Also, with the ensuing substantial increase in the supply of workers who have primary education, the rate of return might be lowered (although evidence from more developed countries

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[6] These household-survey-based figures may be more reliable than official enrollment statistics, which in 1999 have the gross enrollment rate at 140.9 and the net enrollment rate at 108.9 (UNESCO 2002a, 2002b).

[7] No Mincer estimate is available for Uganda. However, the social rate of return to investment in primary education in Uganda was estimated as high as 66% in 1965 (cf. Psacharopoulos and Patrinos 2004), which is the highest among all reported estimates. Compared to this, using the mean sub-Saharan African Mincer estimate of 11.7% seems extremely conservative.

[8] Using Ugandan per capita income (gross national income per capita in purchasing power parity in 2000, from World Bank 2002a) and average years of schooling (from Barro and Lee 2001) instead of sub-Saharan African means, the estimate would be 89% higher at 607 $.

[9] Using alternative discount rates, the total benefits would stand at 4.5 billion US$ for a discount rate of 2% and at 2.2 billion US$ for a discount rate of 10%.

does not really suggest so). Thus, the presented cost-benefit estimates may surely be wide off the true value. However, in choosing the values entering the estimation, I tended to choose to over-estimate the costs and under-estimate the benefits. Also, while the infra-marginal transfers do not directly cause additional education, they relieve constraints on the infra-marginal households. At least part of the relieved resources may well be spent on more advanced educational goals. Thus, there are also good reasons to consider the presented benefit-to-cost ratios as rather conservative estimates. If they roughly fall in the right ballpark, then the fee-waiver policy seems like an incredibly attractive opportunity to address the challenge of a lack of education, exactly in those countries and for those people where the challenge is greatest.

4. Concluding Comments

In concluding, it should be noted that there are also many non-market benefits of increased education that have not been considered here and that might tilt the cost-benefit analyses even more to the positive side (cf. Haveman and Wolfe 1984; Glewwe 2002). As one example, Lochner and Moretti (2004) show that education substantially lowers criminal behavior, which adds 14-26% to the private return to high-school graduation in the US. Furthermore, there are substantial complementarities between addressing the education challenge and addressing other major global challenges. Education has been shown to be positively related to political awareness, stability of democratic systems, and social cohesion. Achieving better education will no doubt have alleviating effects on such challenges as malnutrition and hunger, health, and armed conflicts. Bettering the education of one generation has also been shown to have strong intergenerational effects on the education and well-being of the next generation. Similarly, alleviating challenges such as bad governance and corruption, armed conflicts, and poor health will no doubt help alleviating the education challenge, as will general improvements in the economic well-being of the population (cf. Pritchett's opportunity 3; Glewwe and Jacoby 2004).

One cautioning note should be raised on Pritchett's (2004, pp. 63-64) positive assessment of the current international policy initiative, the Education for All/Fast Track Initiative (EFA/FTI). While this initiative clearly has several laudable features, and while I do not doubt that "the people behind EFA/FTI understand the depth of the problem", it is less clear whether the same can be said for the responsible people in the countries. More importantly, EFA/FTI is likely to suffer from the general problem of foreign aid, well-documented in Pritchett's challenge paper, that most initiatives are undermined by divergent interests of the relevant actors in the countries. The crucial task is probably not one of providing "plans", but one of how to change interests to overcome dysfunctional systems. Thus, it is not too clear whether in the end the EFA/FTI initiative will be substantially more effective than the many previous national and foreign-aid initiatives with their disappointing results (cf. Pritchett 2004, p. 44, for one example).

To conclude on the economic costs and benefits of the opportunities to address the global challenge of lack of education, all estimates suggest that the potential benefits would be immense, but also that there is no easy way to get improvements implemented in practice. The most promising opportunity by far seems to be systemic reforms that introduce performance-oriented incentives for all people involved. The benefits of such institutional changes have been shown to be very large, and the direct costs low – the problem being that the "costs" of breaking interfering interests might be high. In addition to systemic reform, I am also cautiously optimistic on demand-focused policies in many circumstances, particularly in the most severely affected situations. There will always be shortcomings to initiatives like universal waivers of primary-school fees, but the potential benefits are very large in all but the most pathologic circumstances. Most of these benefits will only occur in the relatively long run, raising future productivity and having additional positive effects on subsequent generations. But they appear to be more than worth the cost.

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Appendix

This appendix makes two further comments which may help the author in finalizing the challenge paper, but which are not central to the assessments of the Copenhagen Consensus and may thus not be of specific interest to the panelists.

As one example of the possibility of effect heterogeneity of class size, Pritchett (2004, p. 36) suggests that class sizes might matter much with high-capability teachers, but not with low-capability teachers. Evidence in Wößmann and West (2002) suggests that the effect of class size on student performance indeed depends on teacher capability, but exactly in the opposite way: Smaller classes have an observable beneficial effect on student achievement only in countries where the average capability of the teaching force, as measured by average teacher pay and teachers' education levels, appears to be low. This suggests a positive theory of educational production where capable teachers are able to promote student learning equally well regardless of class size, i.e., they are capable enough to teach well in large classes. Less capable teachers, however, while perhaps doing reasonably well when faced with small classes, do not seem to be up to the job of teaching large classes.

In calculating the share of students completing grade 9 but with "inadequate" learning achievement in his Table 4, Pritchett (2004, p. 17) uses the somewhat arbitrary assumptions that (1) "students more than one standard deviation below the performance in the median OECD country 'lack education'" and that (2) in calculating the fraction of students in a country falling below this threshold, we can use the country mean and standard deviation and assume a normal distribution. Both are actually quite reasonable assumptions, but I think that there would be preferable choices and justifications in both cases. (1) For the threshold value, it would seem advisable to use the "International Benchmarks of Student Achievement" developed in the TIMSS-R study "in order to provide meaningful descriptions of what performance on the scale could mean in terms of the mathematics that students know and can do" (Mullis et al. 2000, p. 38). Preferably, to depict students who lack education, it would seem sensible to use the "Lower Quarter Benchmark" for lower-achieving students, because these generally could not even demonstrate "computational facility with whole numbers" (Mullis et al. 2000, p. 39, cf. p. 42). This benchmark is at 396 test-score points, which – incidentally – is very close to Pritchett's (2004) choice of 400, but has a preferable justification to it. (2) To derive the share of students not achieving this threshold, it would seem straightforward to go for the direct student information to compute the correct share, rather than basing it on the mentioned assumptions. Actually, the share of students not meeting the "Lower Quarter Benchmark" are also reported for each country in Mullis et al. (2000, p. 43): Indonesia 52%, Morocco 73%, Philippines 69%, and Turkey 35%.[10] These numbers are close to Pritchett's, thus showing the sensibility of his assumptions, but maybe they carry a bit more rigor.[11]

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[10] Colombia did not participate in TIMSS-R in 1999, but only in TIMSS 1995, where the comparable benchmark is not directly available.

[11] It would also help interpretability of the results in Table 4, as well as in Table 2, if the numbers were presented in a way so that they sum to 100% across each row. I.e., each number would report which share of the total cohort would lack education because of each specific reason.