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*Benefits and Costs of the Education
Targets for the Post-2015 Development Agenda*

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Post-2015 Consensus

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Introduction

This paper assesses George Psacharopoulos's paper on the post-2015 challenge of education for the Copenhagen Consensus. Psacharopoulos identifies the targets within education that, in his judgment, have the highest benefit to cost ratios, as well as targets that may be valuable investments and targets that definitively are not worthwhile investments. Specifically, he identifies increasing pre-primary and primary education attendance in sub-Saharan Africa and improving school quality as the highest value targets. Raising secondary school completion rates is identified as another valuable target, while vocational education and training for older workers are identified as poor investments.

Overall, we generally agree on the value of improving school quality, and we share his skepticism of vocational and technical education. Yet we would also argue that the evidence for expanding pre-primary and primary education in Sub-Saharan Africa is not as strong as Psacharopoulos claims. Moreover, the vague goal of improving school quality is not very helpful.

In this paper, we begin with a section that critically reviews the methods, reasoning, and findings of the Psacharopoulos challenge paper. The section also discusses the inherent difficulties of prioritizing education interventions. We then supply additional evidence on the likely costs and benefits of various interventions and goals. We close with some suggestions for how to prioritize future investments in education more effectively.

Goals, Interventions, and Cost-Benefit Analysis

The challenge set before Psacharopoulos, to identify post-2015 goals in the area of education and to produce cost-benefit analyses to rank goals, is an enormous and difficult undertaking. While we applaud the idea of prioritizing goals within education and across other sectors using cost-benefit analysis, we question whether the right sorts of goals are being considered. Particularly in education, the types of goals that have been set in the past and are being considered by Psacharopoulos are primarily enrollment measures, rather than outcomes in terms of human capital, learning or skills. For example, a goal of universal primary or secondary education essentially measures children being present at school, but it is a poor measure of how much they learn from being in school. In contrast, goals such as literacy and numeracy embody valuable skills and learning. Setting goals that focus on proxies for learning, such as enrollment rates, creates an incentive to attain those specific goals, but will not necessarily correspond to the essential goals or benefits of education, such as the acquisition of human capital, knowledge, and skills.

Other sectors' development goals have been defined in terms of underlying outcomes. For instance, the Millennium Development Goals for health included improving maternal health, not time spent in health care facilities, as a goal. Yet the Millennium Development Goals for education focus on proxy measures, such as achieving universal primary education, as opposed to essential outcomes such as acquiring basic skills and knowledge. Perhaps this is due to the fundamental difficulty of measuring the knowledge, human capital, and skills that are the ultimate goal of education; while health organizations have established clear,

quantifiable measures (on everything from mortality to blood pressure), the fundamental outcome measures of education are not as clearly established, making it difficult to set measurable goals for key learning outcomes. A regrettable consequence of this is that goals are often set in terms of proxy measures such as enrollment rates.

An additional problem with applying cost-benefit analysis to the proposed goals is that the costs and benefits of achieving any given goal depend on which type of intervention is used. For example, ensuring that all children complete primary school is a goal; building primary classrooms or providing cash transfers that are conditioned on primary school attendance are interventions, the means by which a goal might be attained. Each of these interventions has a different cost, and these costs could vary widely. They could also have different benefits; if the new primary classrooms decrease crowding, children may learn more. Thus it is not possible to calculate a benefit-cost ratio for a given goal without specifying the means by which that goal is to be achieved. Attempting to rank standard education goals and targets in terms of benefit-cost ratios misses the key question of how to reach those goals.

It is this 'how to proceed' that is crucially absent from Psacharopoulos's paper. For instance, improving school quality by increasing student test scores is stated to have a benefit-cost ratio between 3.0 and 5.0, but there is no discussion of how to improve test scores and school quality. Indeed, Psacharopoulos himself points out that there are wide variations in the cost-effectiveness of different quality interventions (see, *inter alia*, Kremer, Brannen, and Glennerster 2013; McEwan 2014). Likewise, the relative returns to different levels of education are discussed in some detail, and the returns are then translated into benefit-cost ratios. But the costs and benefits of different methods of expanding education remain unaddressed; it is not simply a case of "build schools and they will come." Standard estimates of the returns to an additional year of education are, in principle, guides to individuals as to whether to obtain additional schooling; they contain no information on how a government can induce (or prevent) individuals to complete additional schooling.

Moreover, even if one could determine how to induce (or prevent) individuals to obtain additional years of schooling, using standard Mincer equation estimates to calculate returns to education, as it is done in Psacharopoulos's paper, can be very misleading, especially when applied to developing countries. First, as acknowledged by Psacharopoulos, Mincer estimates of returns to years of education do not take into account externalities, which can be substantial in education; for example, Temple and Reynolds (2007) find that the public benefits to preschool in the U.S are 2.5-4.5 times greater than the direct, private benefits to attendees. Second, measurement error in years of schooling could lead to underestimates of rates of return, and such measurement errors are particularly likely to occur in data gathered from developing countries. Third, unobserved ability could lead to overestimates in rates of return to schooling. Fourth, the estimates from developing countries are for wage earners only, not for the self-employed, and there is substantial evidence that the return to education among the self-employed is lower than the return to

wage earners,¹ so estimates based only on wage earners are likely to be overestimates. Fifth, even among wage earners, estimates should exclude government workers. The pay received by government workers with different levels of education mainly reflects government salary policies, not the productivity of different types of workers.

Overall, the estimates of the rates of return in Tables 3 and 4 of Psacharopoulos's paper, and thus the benefit-cost ratios in Tables 12 and 13, are likely to be very inaccurate. Indeed, comparing the private rates in Tables 3 and 4 shows how different estimates can be. In Table 3 the world average private return to an additional year of primary education is 26.6%, but in Table 4 it has fallen by more than half, to 10.3%. In addition, in Table 3 the private rate of return for an additional year of higher/tertiary education (19.0%) is much lower than the primary rate (26.6%), but in Table 4 the rate for higher/tertiary education (16.8%) is much greater than the primary rate (10.3%). The changes for Sub-Saharan Africa are even more striking; for example, the average return for primary education falls by two thirds, and the average return for secondary education falls by more than one half. Presumably the social rate calculations would also change dramatically, but they are not shown in Table 4.

So should one use the results in Table 3 or the results in Table 4? The studies from Table 3 are all pre-2000 (Psacharopoulos and Patrinos 2004), while 73% of the surveys used to calculate the results in Table 4 are post-2000 (Montenegro and Patrinos 2012). If the results in these two tables are to be taken seriously for policy choices, one should use those in Table 4, since they are more up to date. Yet Psacharopoulos uses the presumably outdated estimates of Table 3 to assess the post-2015 goals because he wants to use so-called social returns instead of private returns, and Table 4 does not have information on social returns.² The more recent private returns for primary and secondary in Table 4 are lower than the social returns in Table 3. Thus, if his social returns, which are essentially private returns net of government schooling costs, had been calculated in Table 4 for those two levels of education they would necessarily be lower than the private returns, and therefore much lower than the social returns for those levels in Table 3.

The quality of the evidence presented (and perhaps the quality of evidence available) is also concerning. Consider the evidence on preschool. The Egyptian study cited is not a cost benefit analysis of preschool in Egypt, but rather a World Bank simulation of the benefits and costs Egypt might face based on international evidence (Janssens, van der Gaag, and Tananka 2001). The single, key piece of evidence Psacharopoulos uses to support his contention that the benefit-cost ratio for increasing preschool in sub-Saharan Africa is between 28-39 is, in fact, the addition of a school meals program to preschools in Kenya, not the provision of preschool itself (Vermeersch and Kremer 2004). Likewise, the Gertler et al. (2013) study is miscast as a preschool intervention; it targeted stunted children of

¹ For instance, compare the meta-analyses of van der Sluis, van Praag and Vijverbeg (2005) on returns to self employment to those for wage workers such as Psacharopoulos and Patrinos (2004).

² In theory, social returns are the correct return to use but, as discussed below, the estimates of social returns in Table 3 have serious problems.

preschool age and provided weekly home visits from a community health worker, who encouraged mothers to engage with their children, and had nothing to do with preschools. These are not appropriate studies to use to assess the costs and benefits of providing pre-primary education.

A final criticism is that greater consideration needs to be taken of uncertainty around estimates and potential outliers. When alternative estimates are available, the selection of one is sometimes made without adequate justification. For instance, the benefit-cost ratio for the Kenya preschool program, which is 77, is used and presented prominently. Benefit-cost ratios for the other preschool programs presented by Psacharopoulos are 2-4 in developing countries and 3-7 in developed countries. A benefit-cost ratio of around 3 seems much more probable on the balance than one ten times that large, and a likely range of 2-7 better represents uncertainty.

Additional Evidence and Analysis

Justifying Public Investments

In order to justify public expenditures in education, economic theory states that there need to be substantial market failures that cause the private demand for education to be lower than the socially optimal amount. We have already mentioned one key argument for public expenditure, namely that the presence of substantial externalities (public benefits) would lead to the under-provision of pre-primary education in the private market (Temple and Reynolds 2007). That parents, the decision makers for many, if not most, education decisions, may not capture the returns to their children's education could also contribute to the need for public investments (Brown and Park 2002). Information problems and credit constraints may also generate a sub-optimal provision of education. For instance, eighth graders in the Dominican Republic reported estimates of returns to school that were only one fourth their likely actual size (as estimated by Jensen 2010). Given this array of problems, public investment in education is quite likely to be justified, but Psacharopoulos does not address this issue. Indeed, his calculations of social rates of return are guaranteed to be lower than the private rates of return, since they add a cost (government spending on education) but do not change the benefits.³ Thus from the point of view of economic theory, governments should not subsidize education at all since the social rates are lower than the private rates; indeed those rates will be equal only when all government spending on education ceases.

Pre-primary

If countries want to expand the quantity of education they provide, the best investment they can make is likely to be in pre-primary education, particularly for disadvantaged children (Carneiro and Heckman 2003; Heckman and Masterov 2007; Heckman 2006).

³ He makes a distinction between "narrow social" and "wide social" returns, but this terminology is, in general, not used by economists. For example, this terminology appears in none of the chapters in the four volumes of *Handbook of the Economics of Education* (Hanushek and Welch 2006a; b; Hanushek, Machin, and Wößmann 2011a; b).

Research from developed countries has shown that this is the most cost-effective stage of education in which those countries can invest. The opportunity cost of children's time is lowest in the pre-primary years. Early investments are also more effective, given what is known about cognitive skill development; investments have larger effects on cognitive skills at earlier ages (Cunha and Heckman 2008). The best evidence on pre-primary education is that it consistently improves child development (Engle et al. 2011). Early childhood programs, such as preschools, can also raise enrollment in later grades in developing countries (Berlinski, Galiani, and Manacorda 2008; Hazarika and Viren 2013; Martinez, Naudeau, and Pereira 2012). Investing in early education can enhance attainment throughout students' time in school, while raising primary and secondary school enrollments can spill over only onto later stages. The impact of a combined preschool and nutrition program in Bolivia, based on earnings gains from observed increases in height, cognitive skills and educational attainment, generated benefit/cost ratios in the 1.7 to 3.7 range (Behrman, Cheng, and Todd 2004). This is in line with benefit/cost ratios to preschool in Brazil of approximately 2 (World Bank 2001). In our opinion, it is likely that benefit/cost ratios are as high in developing countries as in developed countries, given that developing country studies primarily look only at increases in wages, while developed country studies that account for a wider range of benefits find benefit-cost ratios from 4-10 (Temple and Reynolds 2007). Quality definitely matters for pre-primary, with higher quality programs having higher impacts (Engle et al. 2011). For instance, comparing a higher quality preschool program to regular preschool in Bangladesh, children in the higher quality program experienced greater improvements for a number of social and cognitive outcomes (Moore, Akhter, and Aboud 2008).

Early childhood programs tend to have the greatest benefits for disadvantaged children, creating a rare opportunity to increase both equity and efficiency. For instance, an early childhood education program in Indonesia was particularly beneficial to poor children, decreasing the achievement gap between rich and poor children (Jung and Hasan 2014). Therefore, expanding pre-primary first to poorer areas is likely to generate higher returns. It will also be unlikely to crowd out private investment in pre-primary, as there are large wealth gaps in pre-primary enrollment between the rich and poor (UNESCO 2014). These gaps, before children even enter primary education, place disadvantaged children on unequal footing when (if) they attend primary school, making it difficult for such children to equally benefit from or succeed in school.

Primary and Secondary Education

In considering the potential goal of increasing or even universalizing primary education, an important consideration is the nature of the countries that currently have shortfalls in primary education. Psacharopoulos argues that investing in universal primary education in sub-Saharan Africa should be a priority investment, because this region has the greatest shortfalls in enrollment, with a large share of the 57 million primary school age children who remain out of school (UNESCO 2014). This argument rests on the assumption that there are greater returns to education where human capital is scarcer. However, half of the 57 million children out of school are in conflict zones (UNESCO 2014). It is problematic to provide primary education at all, much less at a normal cost, in such situations.

Additionally, the returns to human capital are likely to be both uncertain and low in conflict zones and other issues, such as security, are very likely to be higher priorities than education.

Outside of conflict contexts, primary education is likely to be a worthwhile investment, or more accurately, the learning and literacy that individuals should achieve upon completing primary education (Orazem, Glewwe, and Patrinos 2009) is likely to be worthwhile. However, the reasons that children do not complete primary education need to be addressed. For instance, while pull factors, such as marriage or economic constraints, lead to dropping out, push factors, such as disliking schools or failing exams, have an equal role (Pritchett 2013). As well as facing economic constraints, many parents (and children) perceive schools as not being worthwhile, especially when the quality of those schools is often low, so that children learn little.

Turning to secondary education, universal secondary enrollment may not be an essential goal for all developing countries. Ensuring universal enrollment in pre-primary and primary education should be a higher priority, since those investments are likely to be more cost-effective than aiming to increase secondary enrollment. Particularly in countries with less developed economies, the demand for secondary-educated labor may be limited and increases in secondary may dampen returns. For instance, almost half of male wage workers in Egypt with secondary degrees are employed in jobs that they report require no formal education whatsoever (Krafft 2013). Secondary may be an appropriate investment in other country contexts, where there is unmet need for more educated labor, but it should not be a high priority in all countries.

School Quality

School quality was a long neglected goal in the push for quantity goals such as universal primary education. Yet children are often not learning much from school (Pritchett 2013), suggesting that goals for education quantity, such as universal primary education, may not actually deliver much learning. The reasons that school quality has historically taken a back seat to school quantity are two-fold. First, there have to be schools (and students) before one can address the issue of school quality. To a certain extent, quantity, or at least school entry, is a pre-requisite of school quality. Secondly, quality is very difficult to measure and quantify, especially when compared to readily measured quantities such as years of schooling. Quality has many dimensions, and these dimensions have a greater element of contextuality than increasing education quantity. For example, providing textbooks may work well in contexts where students have acquired sufficient skills to read the textbook, but not where most students are unable to read the textbooks (Glewwe, Kremer, and Moulin 2009).

Given the complexity and contextuality of school quality, it is not surprising that there is wide variation in the cost-effectiveness of different school quality interventions. Fortunately, there is now an increasing number of studies that have evaluated the impact of such interventions, at least in the short run. Kremer et al. (2013) examine a number of interventions designed to increase learning in developing countries, and for each of these

they calculate the impact on test scores per \$100 spent. A number of interventions have near-zero or insignificant effects, and a number of the findings are, on the surface, contradictory. Should governments deploy technology in education in developing countries? The evidence from One Laptop per Child in Peru and adding computers to classrooms in Colombia indicates that such investments will be ineffective, but individually paced computer assisted learning in India increased test scores by about 1 standard deviation (SD) of the distribution of students' test scores per \$100 spent. Glewwe et al. (2013) review the literature on school resources and find that the only consistently promising interventions are those that ensure that teachers are present (teacher absence is a substantial problem in developing countries) and that teachers are knowledgeable about the subjects they teach. Additional resources are effective only when they change how teachers instruct and thus the experiences of children in school (Murnane and Ganimian 2014). So while school quality is an important investment, more research is needed to identify priority and cost-effective interventions. In particular, there is a great need for benefit-cost analysis of school quality interventions. Benefit-cost ratios for some relatively simple interventions may be quite high; for instance, providing eyeglasses in China had a benefit-cost ratio in the 3-10 range (Glewwe, Park, and Zhao 2014).

Although it may seem that there must be a quality/quantity tradeoff in education (if a country trains its teachers for longer, it has less money to build schools), an overlooked intervention in improving the quantity of education is improving school quality. For instance, one way to increase the quantity of education is to reduce the dropout rate (Orazem, Glewwe, and Patrinos 2009). Low-quality schooling increases dropping out (Hanushek, Lavy, and Hitomi 2008), while improving school quality can—in addition to its other benefits in terms of learning—reduce dropout. For instance, in Egypt, having a one-shift rather than a multiple-shift school, better facilities, teachers who have had in-service training, and more full-time regular teachers was associated with decreased dropout (Lloyd et al. 2003).

Attempting to increase education quantitatively in the absence of quality may in fact dampen the benefits of additional years of schooling. Countries in the Middle East and North Africa saw some of the fastest growth in educational attainment from 1980 to 2010. Despite the rapid growth in educational attainment, learning (educational achievement) remains low; these countries continue to have very low scores on international tests (Assaad 2014). Additionally, the rapid increase in the quantity of education in the region is likely to be one of the factors contributing to the declining and now low returns to education in that region (Dhillon and Yousef 2009; World Bank 2008). Not surprisingly, as quantity rose, the return paid for education fell.

Conclusion

The challenge of identifying post-2015 goals for education and providing cost-benefit analyses is extremely difficult, especially since the goals being considered do not have a single set of costs or benefits. Education goals are usually framed in terms of proxies, such as enrollment, rather than in terms of the key outcomes—learning, human capital, or skills—that are the true goal of education. While we have pointed out a number of

difficulties with the task of assessing these targets and with Psacharopoulos's approach, ultimately we come to similar priority goals. Pre-primary education and school quality investments should receive priority among the education targets being considered as post-2015 development goals. Outside of conflict zones, primary education—or the basic skills for which it is a proxy—is likely to be a good investment as well, although 100% primary completion is a goal that the vast majority of countries not in conflict have already achieved.

One of the greatest difficulties in assessing and prioritizing education goals and interventions is the very long run over which education pays off. For instance, when thinking about the benefits of pre-primary education, individuals who attend pre-primary at age 4 may have higher wages, but these benefits will not be obtained until decades later. While increasingly there are high-quality studies of the short-run impacts of education interventions, even in developed countries truly long-run studies are rare, and seldom generalizable. For instance, the best studied evidence on long-run preschool outcomes is perhaps the Perry Preschool Program, which had an original sample size of 123 low IQ, low-income urban African-American children in Ypsilanti, Michigan (Temple and Reynolds 2007). There is a desperate need for additional, high-quality, long-run evidence and research on what works in education, especially in developing country contexts. Additionally, research that helps identify and measure the most important learning outcomes can help policymakers move away from proxy goals in education, such as enrollment, and move towards targets in terms of human capital, learning, or skills. We therefore would suggest an additional goal within education—undertaking more research on what works, especially in terms of school quality, and on the long-run impacts of interventions in developing country contexts.

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This paper was written by Paul Glewwe and Caroline Krafft of University of Minnesota. The project brings together more than 50 top economists, NGOs, international agencies and businesses to identify the goals with the greatest benefit-to-cost ratio for the next set of UN development goals.

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C O P E N H A G E N C O N S E N S U S C E N T E R

Copenhagen Consensus Center is a think tank that investigates and publishes the best policies and investment opportunities based on how much social good (measured in dollars, but also incorporating e.g. welfare, health and environmental protection) for every dollar spent. The Copenhagen Consensus was conceived to address a fundamental, but overlooked topic in international development: In a world with limited budgets and attention spans, we need to find effective ways to do the most good for the most people. The Copenhagen Consensus works with 100+ of the world's top economists including 7 Nobel Laureates to prioritize solutions to the world's biggest problems, on the basis of data and cost-benefit analysis.

