Perspective Paper

The Challenge of Hunger and Malnutrition

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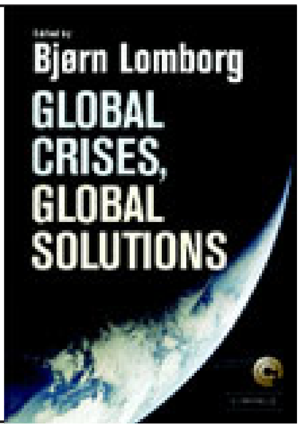

Copenhagen Consensus – Challenges and Opportunities

HUNGER AND MALNUTRITION - COMMENTS

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1. Introduction

The Challenge Paper “Hunger and Malnutrition” by Jere Behrman, Harold Alderman and John Hoddinott provides a valuable survey of academic literature on the subject as well as a thought-provoking evaluation of the economic returns to public investments in that field. The range and volume of literature surveyed – much of it original research by the Challenge Paper authors themselves- is a testament to the paper’s scholarly contribution while the exceptionally high benefit-cost ratios produced show its interest for policy. Consequently, the paper is one beneficial outcome of the Copenhagen Consensus exercise. The literature surveyed spans a variety of fields, ranging from econometric to medical journals, and is very up to date. The paper’s review of the literature is characterised by an acute awareness of the empirical problems involved and, although often circumscribed by the quality of the evidence available, at times shows a formidable degree of rigour in interpreting data. The four Opportunities evaluated are all major ones, of great importance and concern to policy-makers, although – perhaps because they are so fundamental – difficult subjects for cost-benefit analysis.

The Challenge paper is wide-ranging and comprehensive, so this comment does not propose any new Opportunity. Nonetheless, it is noteworthy that the paper does not address the more general issue of poverty reduction, which is intimately linked to malnutrition, nor does it formally evaluate any interventions to deal with the most extreme case of hunger, namely famine. The omission of the former is understandable, as the topic of poverty reduction may be too broad, intractable and less subject to high return interventions. The topic of famine may be already be partly addressed by the Opportunities for reducing civil conflict considered in a companion Challenge paper, since famine appears increasingly to be a corollary to conflict, or even a weapon of war. Nonetheless, interventions to reduce the risk of famine - such as contingencies for emergency food-for-work schemes - could well have been the basis for a fifth Opportunity within this Challenge.

However, rather than try to broaden the discussion, this note addresses three aspects of the paper. First, it questions some of the assumptions that underlie the Challenge paper’s estimates of benefit-cost ratios. Here, the general thrust of the comments is that the authors’ assumptions may lead them to *under-estimate* the benefits to the Opportunities they evaluate. Second, this note provides a “second opinion” on the relevant empirical evidence. It is argued that the evidence base for much of the paper is rather fragmentary and partial, often drawing on developed country data rather than developing country data and frequently subject to serious methodological limitations. Moreover, the derivation of figures for benefits and costs in the paper from the evidence is sometimes opaque or merely reflects the authors’ judgements. The implication is not that the Challenge paper necessarily overstates the returns to the Opportunities it presents, but that the uncertainty surrounding the evidence implies that they should be subject to a discount for uncertainty. Of course, these two sets of comments are to a degree offsetting. The fact the authors’ assumptions lead them to tend to under-estimate the returns to the Opportunities is mitigated by the fact that these returns should be discounted due to the uncertainty over the empirical estimates. It is not clear whether, on balance, the paper under or over-estimates the returns to interventions, and for this reason, it would seem unwarranted to try to “second guess” it. Consequently, the final part of this note takes the paper's estimates at face value and addresses the question of aggregation that arises. The Challenge paper provides a range of cost-benefit ratios for 3-4 interventions per Opportunity rather than a single median cost-benefit estimate for each Opportunity. Consequently, we conclude by discussing the problem of aggregation the Consensus experts will face in assigning an overall rate-of-return to each Opportunity.

2. Assumptions used in return calculations

As required by the Copenhagen Consensus procedure, the Challenge paper is focussed around the estimates of the benefit and costs for various Opportunities. Some of the assumptions underlying these estimates can be questioned, although some criticisms are dependent on one's value judgements.

First, too narrow a view of what constitutes costs and benefits is taken[1]. The focus is on productivity effects and resource costs of malnutrition. No value is placed on the intrinsic importance of nutrition or indeed health. For example, when considering the effect of low birth weight on the illnesses of infants, only out of pocket costs of illness are computed - the suffering of the infants (and their parents) is given no weight. Ascribing some monetary value to instances of illness – for example, analogous to the "severity index" used in computations of QALYs and DALYs – would appear warranted. Other things being equal, this point is likely to mean that the returns to the nutritional interventions presented in the paper are underestimates.

Second, the benefits of Opportunities that do not operate via nutritional pathways are often not valued (the main exception being for Opportunity four, agricultural research and development, which is valued in terms of general economic returns). For example, the interventions to raise birth weight operate by increasing the welfare of the mother (e.g. by treating presumptive STD). However, only the benefits for the child are quantified. This comment implies that the returns to Opportunity 1, in particular, are underestimated.

Third, distributional weights are not used in evaluating the benefits of interventions. The implicit assumption is that "*a dollar is a dollar*" and dollar gains are equally valuable whoever receives them. The implicit social welfare function appears to be a simple "utilitarian" one, but with no allowance for the utilitarian argument for equality (diminishing marginal utility of income). However, this assumption appears particularly questionable given the intimate link between hunger and poverty. If, as is widely agreed among those concerned with global poverty reduction, greater weight should be assigned to benefits accruing to the poor then this may have a number of implications. Within this particular challenge, it may increase the importance of the third Opportunity – if micronutrient deficiencies are concentrated amongst the poorest – and reduce the importance of the fourth – if better-off farmers benefit more from agricultural research and development. Perhaps more importantly, distributional weighting will increase the importance of the nutritional interventions compared to some of the other Challenges (e.g. financial or trade liberalisation) which are less intimately connected with poverty.

Fourth, the approach take to the value of life is curious. It is based on a figure, \$800, given by Summers (1992) as the *cost* of saving a statistical life (specifically, the estimated cost of saving a life through measles innoculation in the early 1990s). The first observation here is that this appears to make life extremely cheap (even after the authors' upward revaluation to \$1250). For example, it can be compared with \$4515, the present discounted value of the lifetime stream of \$500 annual earnings taken in the paper as the benchmark earnings of people in developing countries. The implicit assumption is that we are indifferent between an intervention that saves a statistical life and one that increases a person's productivity by 28%. Noting this feature helps understand why the productive benefits of the nutritional Opportunities are often estimated to be larger than the benefits in terms of mortality reduction. Further, the rationale for the Summers' approach to valuing life seems curious – wishing to avoid the thorny issue of putting a value on life, it merely calculates the opportunity cost. This approach seems of questionable use in evaluating some interventions – presumably, it would imply that the benchmark intervention of measles innoculation had zero net benefit. Moreover, to be morally persuasive it would have to be the case that the life being valued would in fact be saved (through measles innoculation), when obviously this is a fiction. Like "compensation tests" in welfare economics, it seeks to avoid making valuations but only does this by positing hypothetical conditions that are arguably morally irrelevant. A preferable approach would be to confront the issue of valuing life more directly, for example, by looking at what people appear willing to pay to avoid risks of death.

If, understandably, one wishes to avoid the difficulties with such direct approaches to valuing statistical life, the admittedly arbitrary approach used in the Challenge paper on communicable diseases (Mills and Shillcutt, 2004) may have wider appeal than that proposed by Summers. This simply values a year of life lost at per capita Gross National Income[2]. Using the mean GNI per capita for low and middle income countries (PPP\$3830 or US\$1160) might have the widest appeal, since it would be unlikely to be internationally acceptable to assign a lower value to the lives of those in poorer countries. Under this approach, the present discounted (at 5%) value of a life expectancy of 65 years is US\$22,227 or PPP\$73,387, exceeding the Summers' valuation by factor of 18 (or 59, using the PPP figure). This implies that the estimated benefits of Opportunities 1-3 in the paper are substantially under-estimated.

More generally, given the subjective and controversial judgements involved in valuing life, it might be useful if the outcomes of interventions in terms of lives saved and other benefits were presented separately. Aggregation into a single monetary value – as required by the Copenhagen Challenge – appears to be a rather old fashioned application of welfare economics and does not seem to take seriously the multidimensionality of welfare and poverty now widely acknowledged (Sen, 1999; World Bank, 2000). While lives saved may be the dominant benefit from some Opportunities (e.g. communicable diseases), they may be negligible for others (e.g. financial or trade reform). Prioritisation between these different kinds of Opportunities will depend heavily on the valuation of life and aggregation into single benefit-cost indicators will mask this. The argument here parallels the criticism of composite welfare indicators (e.g. the Human Development Index) for masking information and applying arbitrary weights.

Fifth, the approach taken in the paper might be criticised as being "Top-Down" and technocratic, rather than "Bottom-Up". It represents experts evaluating technical interventions that are delivered to people by public servants (via fortification, supplements, injections etc). However, such criticism would seem to be misplaced. Primary health care and nutrition seem to be one area where simple mass technical interventions (e.g. immunisations) can yield enormous benefits. Moreover the delivery of many interventions is likely to require participation of many primary health workers working at the "grass-roots", as, for example, was done with the Chinese "barefoot doctors". Mobilising political support for the interventions, and involving local communities in their delivery, may be required to overcome some of the problems of implementation discussed in the Challenge Paper.

3. Empirical evidence on benefits and costs of Interventions

Perhaps the main strength of the paper is the wealth of empirical literature it has uncovered and draws upon. There are sometimes slips in the presentation of the evidence[3], but the authors' mastery of the technical issues in empirical analysis is apparent in the paper. However, a careful reading of the paper and some of the sources it reviews shows the often fragmentary and partial nature of the evidence base upon which it rests. This section illustrates this limitation, focussing on the evaluation of Opportunity 1, which the Challenge Paper also devotes the most space to and which raises a number of key issues common to some of the other Opportunities.

Opportunity 1: Reducing prevalence of Low Birth Weights

The paper covers in most detail the benefits and costs of the first Opportunity considered, interventions to avoid low birth weight. It devotes most attention to quantifying the benefits of avoiding an instance of low birth weight *per se* rather than the benefits of specific interventions to reduce low birth weight. In particular, there is relatively little critical examination of the *effectiveness* of interventions to reduce low birth weight[4]. Instead, various alternative kinds of intervention are reviewed and assigned rough costs per low birth weight avoided. It should be noted that this approach will under-estimate even the nutritional benefits of some interventions to the extent that the interventions benefit children who would not anyway have had low birth weight[5].

Considerable uncertainty surrounds the estimates of the two major sources of benefits to reducing low birth weight – lower mortality and higher productivity. The mortality benefits are questionable because they are based on Ashworth's (1998) summary of studies of bivariate associations, which in turn were largely driven by studies of the US. These studies made no control for confounders and as such are likely to over-estimate the beneficial effect on mortality[6]. The authors' reliance on such evidence contrasts with the rigour espoused in some of their own studies where they try to factor out the effect of genetic factors unobserved by the researchers as well as observable ones (e.g. Behrman and Rosenzweig, 2004). The reliance on developed country data introduces additional uncertainty because it is not clear that results from areas where there are not serious problems of malnutrition, can be generalised to developing countries, where there are. It is possible that low birth weight in an affluent society often reflects more serious medical conditions than it commonly does in poor countries. However, this is hard to test for precisely the reason that the authors were forced to rely on developed country evidence – because the developing country studies available appear so few and rely on small, not nationally representative, samples[7].

The authors find the largest source of main benefit from avoiding low birth weight to be higher future productivity. However, unlike the clearly derived mortality benefit, the 7.5% productivity benefit assumed reflects a judgement rather than being something mathematically computed from estimates in the literature. Only two studies that directly relate birth weight to wages are reviewed – Strauss, 2000 and Behrman and Rosenzweig, 2004. These imply around 9% and 16% productivity gains from avoiding low birth weight respectively. The studies are of the UK and US, not developing countries, but do control for some confounders and even, in the US study, for genetic differences[8]. In addition to this rather limited direct evidence, the paper also reviews indirect evidence, looking at the effects of birth weight on height and cognitive ability, and then the effects of height and cognitive ability on wages. The non-economic evidence reviewed on the links between low birth weight, height and cognitive ability is largely drawn from studies of developed countries. The economic evidence of a link between height and wages is somewhat less voluminous than the authors assert[9]. The evidence on the link between cognitive ability and wages is stronger, both in terms of volume and the implied quantitative effects. Nonetheless, the economic studies typically take height and cognitive achievement as exogenous and so estimates of their effects may be contaminated by the impact of factors unobserved to the researcher. One may also question the extent to which evidence on wage differentials applies to productivity outside the wage sector, particularly to the majority of adults in low income countries who are engaged in agricultural self-employment or other work for their households. Furthermore, since no studies simultaneously control for both height and cognitive ability, it would not be valid to simply add together estimated returns to LBW via these two pathways. Taken together these issues imply considerable uncertainty around the assumed 7.5% productivity benefit, although the figure does not appear unreasonable in view of the available evidence.

There also appears to be considerable uncertainty around the costs of each LBW averted: the authors consider a range of costs from $28 per LBW to $1000. Part of the variation is driven by differences between studies of specific interventions. However, since the studies tend only to cost the medicines, the authors also consider a range of assumptions about the ratio of medicine costs to total costs – specifically various multiplicative costs of 2, 5 and 10. No guidance is given as to what ratio is more plausible, which is disappointing given that in this context determining costs seems in principle more straightforward than determining benefits.

What this short discussion of the evidence on Opportunity 1 implies is, not that the Challenge Paper necessarily over-estimates the returns to interventions to reduce low birth weight, but that considerable uncertainty prevails over the particular figures presented.

Opportunity 2: Improving infant and child nutrition

This Opportunity is discussed by reviewing the costs and benefits of four different interventions. It is assumed that such interventions will have comparable productivity benefits similar to those posited for Opportunity 1. However, the calculations here sometimes have what the authors concede to be a "back-of-the-envelope" nature. While it is plausible that qualitatively similar benefits may be expected, it is not established that magnitudes of these effects will be similar. Furthermore the derivation of some of the results is often opaque. For example, consider the information in Table 9 on intervention 2a (breast-feeding promotion in hospitals). Here, it is hard to reconcile the range of benefits ($131-134) and costs ($133-1064) with that for the benefit-cost ratio (4.8-7.35).

Opportunity 3: Reducing micro nutrient deficiencies

The evidence for the returns to interventions to reduce micro nutrient deficiencies is arguably the strongest of all four Opportunities. This is partly on account of the persuasiveness of the evidence on the benefits of the interventions – often based on experimental trials. However, it is also a reflection of the extremely low cost of some interventions, such as fortification. Nonetheless, the quantification of the benefits and costs in the Challenge Paper is sometimes unclear. Consider, for example, the case of the intervention to offset iron deficiency – the highest return intervention discussed in the paper (according to Table 9). It is not clear how the per capita cost of $0.25 was arrived at, nor why the benefit-cost ratios are in the range 176-200 when the highest benefit ratio reported in the text was 84 (Levin, et al, 1993) cited on page 32.

Opportunity 4: Agricultural research and development

This Opportunity is discussed partly in terms of the general return to agricultural research and development, highlighting the 44% average return found in meta-analysis by Alston et al., (2000). This figure is not straightforwardly comparable with the returns estimated for the other opportunities, since it implicitly incorporates a range of benefits wider than the purely nutritional impacts evaluated for other interventions. Moreover these benefits are likely to be much more widely spread in developing countries and less focussed on the disadvantaged (especially when disadvantage is assessed in nutritional terms). The benefits do not include large effects on mortality and this may reduce the relative priority assigned to this Opportunity if life is valued more highly than is done in the Challenge Paper.

However, the discussion of opportunity 4 also includes estimates of *ex ante* returns to disseminating staples rich in micronutrients. Arguably, these interventions should be included within Opportunity 3 as they are essentially concerned with reducing micro nutrient deficiencies rather than the general benefits to agricultural research *per se*.

In summary, considering all four Opportunities, although the Challenge Paper reviews a substantial and varied amount of literature, one is struck by the fragmentary and partial nature of the evidence-base used to assign benefit-cost ratios to various interventions. The derivation of some of the key figures is often unclear and sometimes reflects assumptions based on judgement. It is not that these judgements appear flawed or biased in a particular direction. However, the tentative nature of the empirical evidence and consequent uncertainty over key magnitudes may warrant discounting the estimates by some arbitrary "risk premium". The general implication that the Challenge Paper *overstates* the returns to the Opportunities considered must, however, be set against the argument of the previous section that it tends to *understate* the returns by virtue of some of its key assumptions.

4. Aggregation

Although the Challenge Paper does provide estimates of costs and benefits for interventions within Opportunities, it stops well short of assigning an overall rate of return to each Opportunity. Costs and benefits are given in Table 9, but it is stated that these cannot be understood without reference to the text and it is not clear that they meant to be taken as strictly comparable. Moreover a range of estimates are given for 3-4 interventions in each Opportunity with no median value provided or attempt to aggregate to provide a single benefit-cost ratio per Opportunity. Furthermore there is no discussion of the scale of the possible interventions – for example, if one had $50bn to spend, could it all be spent iron fortification and still achieve the extraordinarily high benefit-cost ratios presented? However, none of the interventions are likely to be one-off opportunities, but instead warrant recurrent expenditures each year for the foreseeable future. Consequently, even a very large sum of money could be usefully allocated to each intervention, provided that it did not all have to be disbursed in a single year (some could, instead, be set aside in a trust fund for used in future years).

This authors' reluctance to provide overall benefit-cost estimates for each Opportunity is understandable given the varied nature of possible interventions and the fragmentary quality of the evidence on their returns. The authors may well have concluded that assigning an overall rate of return to each Opportunity is going further than the available data will support and left such assignment to the panel of ten experts chosen by the Copenhagen Challenge. However, as a spur to discussion and aid to the deliberation of the expert panel, it may be useful to conclude this note by providing a tentative assignment based on the evidence in the Challenge Paper.

Looking at the summary Table 9, it is hard to avoid the conclusion that the third Opportunity – reducing micro nutrient deficiencies – offers the highest rate of return. [As stated earlier, it seems more coherent to bundle interventions 4b and 4c into this Opportunity – as dissemination of new varieties of staples appears an attractive way of dealing with micro nutrient deficiencies.] For almost all interventions considered, the lower bounds on the benefit cost ratios presented are higher for this Opportunity than for others while the upper bounds (and hence mid-points) are often incredibly high. Furthermore, interventions within this category are likely to have benefits in terms of reduced mortality – which this comment has argued may be undervalued in the paper and should be presented as distinct from benefits which can be more easily be given a monetary value. Given the extreme range of values presented, assigning a single benefit-cost ratio for Opportunity 3 seems an impossible task. However, the ratio of 36 put forward for iron fortification by Horton and Ross (2003) does not seem atypical of the estimates considered in the paper.

Of the other Opportunities, the returns to agricultural research and development appear to have the highest benefit-cost ratio (a median of 15 when the discount rate is 3%, if the meta-analysis of Alston et al, 2000, is believed). However, given the much weaker likely linkages with poverty and mortality, those who assign higher values to life and/or have more progressive distributional weights may wish to prioritise the other two remaining Opportunities. Of these, the impression gained from the paper is that returns from the two Opportunities are comparable, with perhaps interventions to avoid low birth weights having slightly higher benefits (particularly, if benefits for the mother were to be incorporated into the calculations). For Opportunity one, it would seem churlish not to accept the estimate of benefits put forward by Alderman and Behrman (1993). The benchmark to be taken for costs is less obvious, so arbitrarily one could take the middle of the range used by Alderman and Behrman, leading to a benefit-cost ratio of 4.3. For Opportunity two, one might take as representative one of the more in-depth published studies for a program in Bolivia by Behrman, Cheng and Todd (2004), which estimated a benefit-cost ratio of 2.9. Consequently, this note concludes by tentatively proposing the following summary benefit-cost ratios for each Opportunity:

Opportunity	**Benefit-cost ratio**
1: reducing prevalence of low birth weights	4
2: improving infant and child nutrition	3
3: reducing in micro nutrient deficiencies	36
4: agricultural research and development	15

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Notes

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[1] This is signalled on page 1 by the statement *"While reducing hunger is often justified on intrinsic grounds, it is these potential gains in productivity and reduction in economic costs that provide the focus of our paper."*

[2] The authors assert in footnote 16 that this has the "pitfall" of implicitly valuing life in terms of wages. However, the appeal of valuing a year of life at income per capita is *not* that this measures the economic contribution of the life, as it would indeed be bizarre to value people just as if they were income generating machines. One could say that since income consumed has value, it provides a minimum bound on the value of the consumer's life (minimum since one should also include the value of leisure and non-marketed goods, services and activities). On such grounds, there would be no reason to "net out consumption from … earnings" as suggested in footnote 16. More generally, using income per capita may appeal on egalitarian grounds, as it could be taken as implying that each person has a claim on – or entitlement to - an equal share of total income.

[3] For example, on page 13 it is stated that "*LBW infants are 40% more likely to die in the neonatal period than their normal weight counterparts…"*. Presumably, what is meant is 400% rather than 40%, since the benefit calculations follow Ashworth's (1998) summary that for LBW *"the risk of neonatal death is four times as high"*. Furthermore, the calculation in footnote 19 does not recognise the conditional (on neonatal survival) nature of the probability of post-natal death, so that LBW should reduce mortality by 0.075 rather than 0.078.

[4] Rouse (2003) provides a cautionary note here: *"even for the archetypal and nearly universally recommended pregnancy nutrition supplements folate and iron, compelling evidence of effectiveness in reducing the occurrence of adverse maternal or fetal and neo-natal outcomes is not available"*.

[5] Many interventions may raise the adult height and cognitive ability of those children who would not have low birth weight without the intervention. In such a case looking at only the benefits for child who would have low birth weight would seriously underestimate the total benefits of the intervention.

[6] This likely upwards bias is explicitly acknowledged by Ashworth (1998). "*If families with LBW (or IUGR) infants are more disadvantaged in terms of income, housing, parental education etc. than families of ABW infants, then they may be more exposed to infection postnatally and have inferior medical care when ill. Consequently higher mortality in LBW or IUGR infants could be due, at least in part, to their environment, rather than birth weight per se."* Only two studies reviewed by Ashworth included confounders. In the one for a developing country (Lira in Brazil), controlling for confounders reduced relative risk of death aged 0-6 months by around a third.

[7] In this context, Ashworth (1998) makes the interesting observation that the LBW appears to be less associated with adverse outcomes among black Americans, rather than their more affluent white countrymen. Of the 10 studies that are used to generate the median fourfold risk of neo-natal death due to low birth weight, only three are from developing countries – the rest are from the US. The three developing country studies show very varied relative risks (1.4, 3.4 and 4.5); none are nationally representative and two are for urban areas only. The two-hold risk of post-natal death due to LBW appears particularly questionable given that the three developing country studies show risk factors of 0.8, 1.1 and 2.3 (the two available American studies show a two-fold risk). Ashworth's evidence also appears rather dated – the developing country studies all relate to the end of the 1960s and beginning of the 1970s.

[8] Berhman and Rosenzweig (2004) control for genetic factors by using a sample of identical twins and perhaps surprisingly find that this *increases* the apparent returns to birth weight. Although the authors consider whether twin studies are generalisable, one possibility they do not consider is whether birth weight may be particularly important for twins as it affects which one is dominant and hence confidence, which may be important in future lifetime success.

[9] Although nine studies are reported on page 8 as giving evidence of the impact of height on earnings, the first four cited do not appear to provide original research on this question. The Glick and Sahn (1997) reference in the list is also incorrect and should be to Glick and Sahn (1998), who find no return to height for women or those in public employment in Conakry (Guinea). The Schultz (1996) paper on the list was not listed in the references.