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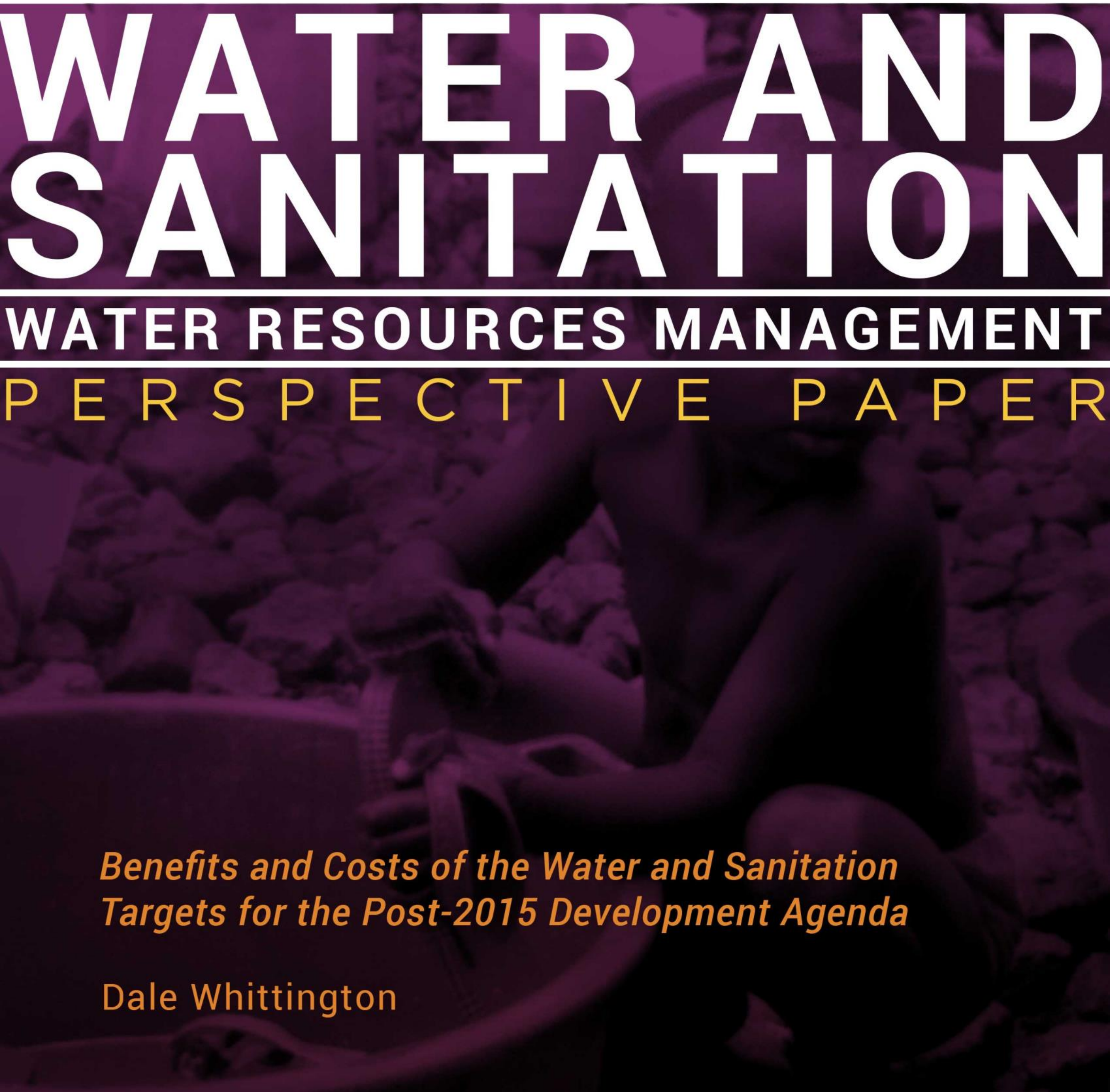

WATER AND SANITATION

WATER RESOURCES MANAGEMENT

PERSPECTIVE PAPER

Benefits and Costs of the Water and Sanitation Targets for the Post-2015 Development Agenda

Dale Whittington

POST-2015 CONSENSUS

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Post-2015 Consensus

Dale Whittington
University of North Carolina at Chapel Hill
Manchester Business School

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Introduction

The global community is struggling to find sustainable development goals for improved water resources management. Two recent efforts illustrate the nature of the challenge. Both the United Nations High-Level Panel of Eminent Persons and the Open Working Group for Sustainable Development Goals have proposed a broad goal and several subgoals for the water and sanitation sector (Table 1). Their broad goals are similar:

1) Achieve Universal Access to Water and Sanitation (United Nations Report of the High-Level Panel of Eminent Persons on the Post-2015 Development Agenda); and

2) Ensure availability and sustainable management of water and sanitation for all (Open Working Group for Sustainable Development Goals).

These two groups' subgoals for water, sanitation, and hygiene (WASH) coverage are also similar, and closely related to the Millennium Development Goals for the WASH sector that they are intended to replace. But both the UN Panel of Eminent Persons and the Open Working Group for Sustainable Development Goals struggled with how to frame goals for the broader water resources management challenge.

Both groups call for increased water use efficiency and increased wastewater treatment. But neither group defines what they mean by water use efficiency or specifies by how much water use efficiency should be increased. Nor does either group indicate how much wastewater treatment is needed (i.e., to what standard should wastewater be treated?).

The Open Working Group calls for integrated water resources management (IWRM) at all levels, which is consistent with the thinking and mission of the Global Water Partnership. But the practical usefulness of IWRM has been the subject of much controversy in the water resources field (Biswas, 2004; Giordano and Shah, 2014), and an IWRM goal is noticeably absent from the list proposed by the United Nations Report of the High-Level Panel of Eminent Persons. The Open Working Group also proposed an ecosystems services goal (Goal 6.6: "Protect and restore water-related ecosystems, including mountains, forests, wetlands, rivers, aquifers and lakes"), but the UN Report of the High-Level Panel does not. The Open Working Group proposed two goals that are closely related to water resources management, but are listed separately:

> Goal 9 - Build resilient infrastructure, promote inclusive and sustainable industrialization and foster innovation.
>
> Goal 15 -Protect, restore and promote sustainable use of terrestrial ecosystems, sustainably manage forests, combat desertification, and halt and reverse land degradation and halt biodiversity loss.

What do these water resources management goals imply in terms of global action? What can we reasonably say about the economic costs and benefits of reaching these types of water resource management targets?

Table 1 – Comparison of Proposed Water and Sanitation Goals from the UN High-level Panel on the Post-2015 Development Agenda and the Open Working Group of Sustainable Development Goals

Sub-sector	United Nations Report of the High-Level Panel of Eminent Persons on the Post-2015 Development Agenda: Goal 6: Achieve Universal Access to Water and Sanitation	Open Working Group for Sustainable Development Goals - Goal 6: Ensure availability and sustainable management of water and sanitation for all
Drinking water supply	Provide universal access to safe drinking water at home, and in schools, health centers, and refugee camps (6a)	6.1 By 2030, achieve universal and equitable access to safe and affordable drinking water for all
Household sanitation & hygiene	End open defecation and ensure universal access to sanitation at school and work, and increase access to sanitation at home by x% (6b)	6.2 By 2030, achieve access to adequate and equitable sanitation and hygiene for all and end open defecation, paying special attention to the needs of women and girls and those in vulnerable
Water Resources Management	Bring freshwater withdrawals in line with supply and increase water efficiency in agriculture by x%, industry by y% and urban areas by z% (6c)	By 2030, substantially increase water-use efficiency across all sectors and ensure sustainable withdrawals and supply of freshwater to address water scarcity and substantially reduce the number of people suffering from water scarcity (6.4)
		By 2030, implement integrated water resources management at all levels, including through transboundary cooperation as appropriate (6.5)
Wastewater treatment	Recycle or treat all municipal and industrial wastewater prior to discharge (6d)	By 2030, improve water quality by reducing pollution, eliminating dumping and minimizing release of hazardous chemicals and materials, halving the proportion of untreated wastewater and increasing recycling and safe reuse by [x] per cent globally (6.3)
Ecosystem protection		By 2020, protect and restore water-related ecosystems, including mountains, forests, wetlands, rivers, aquifers and lakes. (6.6)

It is easy to agree that water resources should be used more efficiently, both in consumption and production, but from an economic perspective this does not mean minimizing water use. For example, efficient production requires that the marginal rate of substitution between input factors (such as water) be the same in all sectors (industries)—or that the ratio of the marginal products of two factor inputs must be the same in all sectors. This condition for efficient production cannot hold if one focuses only on increasing the marginal product of one factor (e.g. water). A catchy phrase such as “more crop per drop” may lend itself to becoming a concrete target, but it is bad economics.

Because water is heavy and generally expensive to transport long distances, most water problems are local and solutions must be crafted to meet hydrological and financial realities at a local scale. In other words, the abundant water resources of Canada and Sweden cannot be used to alleviate water shortages in Ethiopia or Pakistan.

Countries with chronic water shortages must find their own local solutions. An important implication of the fact that most water problems are local is that economic solutions to water problems often come from outside the water sector. Although Canada cannot ship water to Ethiopia, it can ship wheat. This fact changes the economic problem Ethiopia faces from the variability associated with its water resources endowment.

An important implication for the economic analysis of water-related investments is that global averages and assessments yield few practical insights. Water-related infrastructure can be an extremely attractive economic investment – or not. Economists must roll up their sleeves and do the hard work to evaluate specific local water-related interventions. Global targets or development goals do not provide useful guidance for improving the management of water resources.

One can recommend that countries should “implement integrated water resources management at all levels, including through transboundary cooperation as appropriate.” (Open Working Group for Sustainable Development Goals, Goal 6.5) But in Africa there are 28 international rivers shared by three or more countries. Ten river basins—the Congo, Limpopo, Niger, Nile, Ogooué, Okavango, Orange, Senegal, Volta, and the Zambezi—are shared by four or more African nations. Guinea has 14 international rivers, Côte d’Ivoire 9, and Mozambique 8. Each river and each country faces its own set of risks and opportunities in managing its surface water resources. The Nile riparians need to find a path to transboundary cooperation, and the World Bank and international donors have invested perhaps US$100 million over the past two decades trying to facilitate this, but the target has still not been achieved, and the path forward remains unclear (Whittington, Waterbury, and Jeuland, 2014).

Finding local solutions to water resource management problems involves investments in water infrastructure, institutions, and policy reform. It involves recognizing that water is a renewable resource. Human beings use water, but they do not “consume” it. After it is used, a molecule of water continues on its travels through the hydrological cycle. The economic analysis of renewal resources is thus fundamentally different from the economic analysis of nonrenewal resources. When evaluating water-related infrastructure, economists do not need to “charge” a water-related investment for the reduction in the stock of the natural resource. But human use of water resources can shift the spatial and temporal scales of the renewable stock of water resources, as well as the quality of the resource.

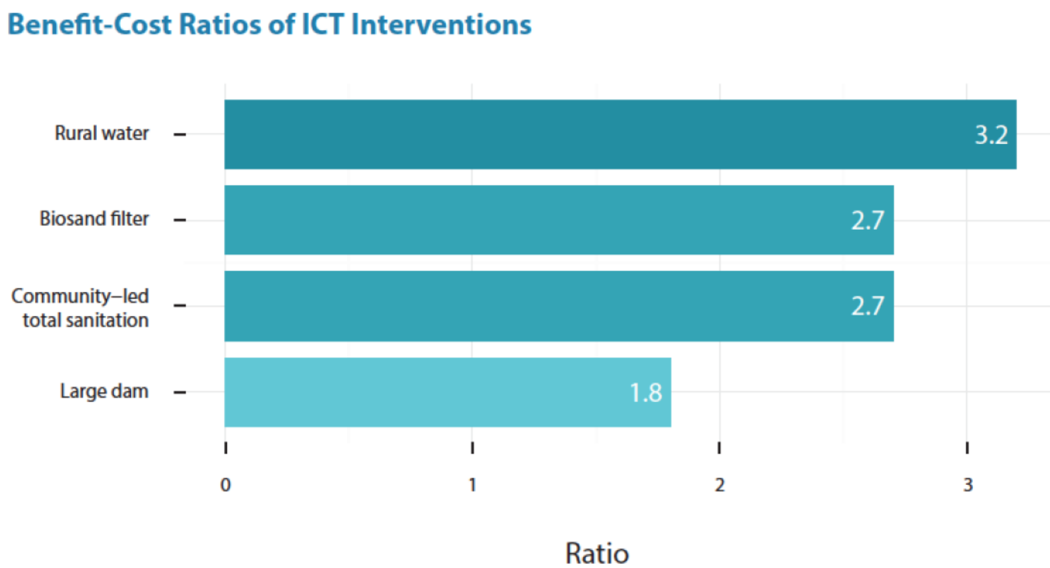
Because improved management of water resources requires local solutions, the economic analysis of water-related investments infrastructure, institutions, and policy reform must be done at the appropriate spatial and temporal scales. It does not make economic sense to present an average benefit-cost ratio for all possible dams that could be built around the world because all of these dams do not need to be built. There are investments in water resources management with high returns and investments with low returns, just as in every other sector of the economy, and good management demands selecting the best investments and bypassing the others. There is no universal rule that constructing water infrastructure is either good or bad in cost-benefits terms. The challenge will always to determine the timing and sequencing of investments in a particular location that will yield the highest returns.

Illustrating what would be required to present a global estimate of benefits and costs of increased water storage serves to emphasize this point. One approach to presenting a global estimate would be to select a set of proposed water storage infrastructure from every country in the world, and collect cost-benefit studies for each of these proposed facilities. One could then construct a frequency distribution of cost-benefit ratios from all dams in the set, and calculate descriptive statistics for the distribution (e.g., mean, median benefit-cost ratios). This approach is, however, not feasible because cost-benefit analyses have not been done for most potential sites, and for those that have been done, they have not been kept up to date.

A second approach would to try to estimate an average unit cost of storage and an associated economic benefit per unit of storage. Global averages of unit costs of storage are available, even if they may be very different than the costs at a particular site. But unfortunately there are no associated benefit estimates to compare with these unit costs.

A third alternative would be to look at the cost-benefit ratios of some of the most economically attractive dams in the world, and to assume that the benefit-cost ratios for other dams would likely be less than these. Figure 1 (cited by the UN Panel) shows the benefit-cost ratio for a large multipurpose dam in the Blue Nile gorge in Ethiopia, compared to three interventions in the WASH sector. As shown, even though this multipurpose dam easily passes a benefit-cost test, the dam's benefit-cost ratio is much lower than for the three WASH interventions. Given the fact that this dam is located at one of the most attractive places in the world for hydropower production, it raises the question of whether other large water resources infrastructure projects will ever compare favorably with smaller scale WASH interventions or other health policy interventions such as HIV-AIDS control, malaria drugs, or immunization campaigns.

Figure 1 –Benefit-cost ratio of a large dam compared to 3 WASH interventions (from the report of the UN High Level Panel



Why do water infrastructure investments have low benefit-cost ratios compared to health interventions?

This question is of interest to both water resources and international development professionals, and its answer has important implications for the Copenhagen Consensus Project's priority ranking protocol. Every four years since 2004, the Copenhagen Consensus Project has asked a distinguished group of economists (many Nobel Laureates) the following, seemingly simple question:

"Suppose that you had a very large sum of "extra" money to spend on international aid for poor countries (e.g. US$75 billion). How should these funds be allocated to different possible interventions (e.g., projects, programs, policy reforms) to create the largest net economic benefits?"

A second group of economists with expertise in different sectors (or policy areas) are each asked to propose the most economically attractive interventions in their sector and to estimate the benefit-cost ratio of each policy or program they recommend. The first group of economists (the "Nobel panel") then assesses the work of the second group of sector economists, and proposes a cross-sectoral ranking of the most economically attractive interventions based in part on their estimated benefit-cost ratios. The results of the rankings for the 2004, 2008, and 2012 Copenhagen Consensus exercises are shown in Table 2.

Health policy interventions (e.g., vaccination, malaria, HIV-AIDS, hospital care) consistently dominate the Copenhagen Consensus rankings in Table 2. Large-scale infrastructure investments are notably missing. This is a puzzle because states often put large-scale infrastructure investment at the top of their economic development agenda. For example, sustained economic development in China is the single most important reason that hundreds of millions of people globally have escaped severe poverty over the last two decades, and China has invested heavily in major large-scale infrastructure projects, building thousands of new dams and urban water supply systems. Chinese development banks now provide major financing for such large-scale infrastructure projects in other countries (Brautigan, 2010; Sanderson, H., and M. Forsythe, 2013).

Several years ago, the World Bank commissioned a survey of national government officials in low-income countries and asked them about their development priorities (Briscoe & Malik, 2008). Health interventions were at the bottom of their lists—just the opposite of the Copenhagen Consensus rankings. The officials' top priorities were infrastructure and education. Similarly, development and environmental economists have found that improved health services are not the top priorities of many households in developing countries (Whittington, 2010). One interpretation of this difference is that the Copenhagen Consensus Project has done precisely what it set out to do, i.e., identify economically attractive interventions that have been overlooked by governments in developing countries and deserve higher priority (i.e., that targeted health interventions really do have much higher benefit-cost ratios than large-scale infrastructure projects).

In theory cost-benefit analysis should be the appropriate tool for making sound comparisons of the economic attractiveness of investments in different sectors. But in practice this requires careful attention to the details of the implementation of the method in the various cost-benefit studies, i.e. to the principles and standards for conducting such cost-benefit studies and the subsequent cross-sectoral comparisons.

In this section I reflect on why large-scale infrastructure investments in general--and water resources and water supply and sewerage network infrastructure in particular – have low benefit-cost ratios compared to targeted health interventions.[1] I argue that there are five systemic reasons why the Copenhagen Consensus priority rankings have repeatedly overlooked the economic importance of large-scale infrastructure investments.
I also discuss what this implies when comparing large-scale infrastructure projects with other development priorities.

Table 2: Comparison of Copenhagen Consensus Projects "Top 10" Interventions: 2004, 2008, and 2012)

Ranking	2004	2008	2012
1	Control of HIV/AIDS (BCR = 50)	Micronutrient supplements for children, vitamin A & zinc (BCR = 17)	Fighting Malnutrition (BCR = 59)
2	Providing micronutrients (BCR = 43)	Doha development agenda (BCR = 913)	Malaria medicines (BCR = 35)
3	Trade liberalization (BCR = 24)	Micronutrient fortification (iron and salt iodization) (BCR = 10)	Expanded childhood immunization (BCR = 20)
4	Control of malaria (BCR = 27)	Expanded immunization coverage for children (BCR = 13)	Deworming treatment for Children (BCR = 10)
5	Development of new agricultural technologies (BCR = 13)	Biofortification (BCR = 17)	Expand tuberculosis treatment (BCR = 15)

[1] Note that some investments in water resources management and water supply are best characterized as large-scale infrastructure (e.g., dams, piped water supply and sewer networks), while others are targeted heath interventions (e.g. point-of-use water treatment, community-led total sanitation (CLTS), handwashing promotion).

6	Small-scale water technology for livelihoods (BCR = 3)	Deworming and other nutrition programs at school (BCR = 40)	Increase agricultural output/yield enhancements (BCR = 16)
7	Community managed water supply and sanitation (BCR = 5)	Lowering the price of schooling (BCR = 20)	Early warning system for natural disasters (BCR = 35)
8	Research on water productivity in food production (BCR = 18)	Increase and improve girls' schooling (BCR = 10)	Strengthening surgical capacity (BCR = 10)
9	Lowering the costs of starting a new business (BCR = very large)	Community-based nutrition promotion (BCR = 13)	Hepatitis B Vaccine (BCR = 10)
10	Lowering barriers to migration for skilled workers (BCR = large & uncertain)	Provide support for women's reproductive role (BCR = 30)	Low-cost heart attack drugs (BCR = 25)

Five reasons why large-scale infrastructure investments are missing from the Copenhagen Consensus rankings

Reason #1 – Cost-benefit analysts underestimate the costs of targeted health and social interventions, in part because they implicitly assume that infrastructure is in place to enable the efficient delivery of health services.

Cost-benefit analysis (CBA) is a partial equilibrium method that compares two states of the world: one with the project (policy), and one without. By design, a CBA focuses on the intervention in question; other attributes of the economy (including infrastructure) are held constant in both the new state of the world with the project and the baseline (status quo) state of the world without the project.

Targeted health and social policy interventions often implicitly assume that large-scale infrastructure is already in place. For example, food, malaria, vaccination interventions assume that transportation infrastructure (ports, railroads, and roads) is available to move food and medical supplies to children. The costs of delivering such supplies are rarely based on the assumption that infrastructure is not available. Similarly, improvements in education typically assume that school facilities have improved water supply and sanitation facilities, and that children are not missing school because they must collect water for their families. Nor do the cost calculations for targeted health and social policy interventions reflect the consequences of unmitigated hydrological variability. Both floods and droughts disrupt the delivery of health policy interventions in numerous ways, but typically this is not considered. The result of implicitly assuming that an infrastructure platform is in place to deliver targeted health and social policy interventions is that the costs of these top-ranked interventions will be systematically underestimated.

Ex-post cost-benefit analysis of infrastructure projects often reveals cost overruns, and analysts responsible for the appraisal of large water resources projects have long been aware of this issue (Merewitz, 1972). More ex post cost-benefit analysis of targeted health and social policy interventions is needed to temper overly optimistic cost estimates. This problem of ex-ante analysis overstating the attractiveness of investments compared to the ex-post results is solved in the profit sector by the calculation of profits and losses. Without careful ex-post analyses of public sector investments, it is impossible to determine the accuracy of ex-ante cost-benefit analyses such as those commissioned by the Copenhagen Consensus.

Reason #2 – Cost-benefit analysts overestimate the benefits of targeted health interventions, in part because arbitrary assumptions are made about the value of mortality risk reductions.

Many of the top-ranked interventions in Table 2 share the characteristic that the majority of their economic benefits are based on a monetary valuation of the mortality risk reduction they provide. Micronutrient fortification, expanded immunization coverage of children, control of malaria and HIV/AIDS all save lives by reducing mortality risk. How the monetary valuation of these mortality risk reductions is handled in the cost-benefit calculations is thus central to the final results.

The theoretically correct approach is to determine how much individuals would be willing to pay for mortality risk reductions.[2] However, the use of both revealed and stated preference techniques to estimate poor households' willingness to pay for mortality risk reductions has revealed surprisingly low values (Kremer et al., 2011), which translate into low values of statistical lives (VSLs) in low-income countries.[3] Noneconomists often find the use of VSLs in global cost-benefit comparisons objectionable because they appear to suggest that the cost-benefit analyst values saving lives in high-income countries more than lives in low-income countries. But all the VSL really says is that households in low-income countries are willing to pay less for mortality risk reductions than households in high-income countries, a rather uncontroversial observation.

In the case of the Copenhagen Consensus rankings, the focus of the cross-sectoral comparison of benefit-cost ratios of different interventions is not between low- and high-income countries, but between different interventions in low-income countries. So it would seem that the use of VSLs would be less objectionable. But to avoid the controversy around the use of VSLs, health economists often use a human capital approach to value mortality risk reductions, which assumes that the value of a life saved is worth the discounted value of the person's income stream. Rather than bothering with the discounting calculation, sometimes analysts simply assume that the value of a life saved is some multiple of the individual's annual income. These are not credible ways to estimate the economic (welfare-theoretic) benefits of mortality risk reductions because they are not based on individuals' preferences but rather on the essentially arbitrary judgment of the cost-benefit analyst (Mishan, 1971; Johannesson, 1996).

Another limitation of the way that VSLs are used in the cost-benefit analyses of targeted health interventions is that they do not account for the disamenties of the using the technologies that reduce mortality risks. When these disamenties are significant from the individuals' perspective (e.g., the discomfort associated with sleeping under mosquito nets, the taste of residual chlorine in drinking water, or the side effects of vaccines), both the usage of the targeted intervention and its economic benefits will be overestimated.

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[2] It is possible that willingness to accept (WTA) compensation is the correct welfare measure if the welfare gain is perceived as a reduction in losses (Knetsch, Riyanto, and Zong, 2012; Zong and Knetsch, 2013).

[3] Kremer et al. report that household willingness to pay to avoid a child's death from diarrhea to be US$769 in rural Kenya (with the 95% confidence level ranging from US$555-1281).

The use of a human capital approach or an arbitrarily high VSL can greatly increase the benefit estimates of targeted health interventions compared to estimates based on empirical evidence of individuals' valuation of mortality risk reductions in low-income countries.[4] To the extent the benefit-cost ratios of interventions in the Copenhagen Consensus rankings utilize such arbitrary valuations for mortality risk reductions, the resulting benefit-cost ratios are artifacts of these assumptions, and should not be compared to the benefit-cost ratios of interventions in other sectors (such as large-scale infrastructure) where mortality risk reductions are a less important component of the economic benefits.[5]

Another common reason that the benefits of targeted health interventions are over estimated is that is that publicly provided health services are rarely rationed by price (Murry and Hanke, 1975; Hanke, 1981; Loomis and Hof, 1985). This problem arises with large infrastructure projects as well, but arguably not to the same extent as in targeted health interventions, which are often provided free of charge.

Reason #3- Differences in the planning horizon

Cost-benefit analysts who evaluate large-scale infrastructure projects and targeted health and social policy interventions have adopted different conventions for handling the planning horizon of the investments. The analysis of large-scale infrastructure projects such as multipurpose dams requires that the analyst be explicit about the time profile of costs and benefits. Large capital costs are incurred early in the project, and the benefit stream may not materialize for 5 or even 10 years after the initial capital outlays. Inevitably the choice of the discount rate used to calculate the discount factors required to weight future benefits is a major determinant on the final results. It is thus common for analysts evaluating large-scale infrastructure projects to carry out systematic sensitivity analyses on the discount rate and other parameters (Whittington et al., 2009; Jeuland and Whittington, 2014)

Cost-benefit analysts evaluating targeted health and social policy interventions often frame their analysis differently: they calculate annual benefits and costs once the program is up and running. Their benefit-cost ratios are typically the steady-state annual benefits divided by steady-state annual program costs. There are two reasons this approach makes the comparison with infrastructure projects problematic.

First, although the lag between starting up most health and social policy interventions and receiving the benefits is not as long as for large-scale infrastructure projects, health and social policy interventions do in fact take time to scale up. These lags are assumed away when steady-state annual benefits are compared to steady-state annual costs, making health and social policy interventions appear more attractive relative to infrastructure projects, where the difference between the timing of the initial costs and the receipt of benefits is made explicit.

Second, a long planning horizon (e.g., 50 years) is typically assumed for the analysis of large-scale infrastructure projects. The planning horizon for health and social policy interventions is often undefined, i.e., it is not discussed how long the steady-state annual benefits and costs will continue. This lack of specificity in the planning horizon for the analysis of health and social policy

[4] The use of an arbitrary, high VSL also favors health interventions that focus on reducing mortality instead of morbidity.

[5] Some large infrastructure projects do save lives (e.g. dams may provide flood protection; piped water and sewer networks reduce infectious diseases). Others may increase mortality (e.g. roads that increase miles traveled and traffic fatalities). But the positive and negative effects of large infrastructure projects are often difficult to include in a cost-benefit analysis, especially when they are a relatively small component of the total economic benefits, and funds of benefit estimation are allocated elsewhere.

interventions is problematic because it is common for the magnitude of the periodic benefits from large-scale infrastructure to increase over time, while the opposite is often the case for many health and social policy interventions.

For example, the annual benefits and costs of a cholera vaccination program in a low-income country will likely decline over time if economic growth proceeds and housing, health care, nutrition, and water and sanitation conditions gradually improve. One would generally not contemplate continuing cholera vaccination for decades. On the other hand, large multipurpose dams will continue to provide hydropower and protection against floods and droughts for decades, and these benefits will likely grow in economic value over time. If the cholera vaccination program were to "phase out" before the benefits from the multipurpose dam ceased, the proper comparison of the cholera vaccination program and multipurpose dam would require that the two alternatives be "normalized" to include a common planning horizon and budget outlay. This would necessitate that the analyst of the vaccination program make assumptions about what to do with budget allocation funds after the cholera vaccination program is phased out. This is rarely attempted.

The consequences of the focus only on steady-steady conditions and the lack of normalization of alternatives is that the cholera vaccination program looks better relative to the large-infrastructure project than it actually is. The intuitive explanation for this result is that the large-scale infrastructure may provide an internal rate of return of, say, 10%, over the entire 50-year planning horizon. The cholera vaccination program may provide a 20% rate of return for 5 years, but the question is what options are available in year 6? Perhaps the large-scale infrastructure project can be delayed until year 6 and then initiated, but perhaps not. In this example a key valued attribute of the large-scale infrastructure project is that it provides a "good" return for a very long time (i.e., over a long planning horizon). In contrast, cost-benefit analyses of health and social policy interventions are often not explicit about how long they will last, and what investment alternatives will be available as they are phased out.

Reason #4 – Feasibility of Private Sector Financing

The question posed by the Copenhagen Consensus Project to the Nobel panel –how should additional donor funds be allocated to maximize economic benefits--could be interpreted to mean that these additional donor funds should be allocated to projects or programs that would not get done otherwise. Investments with attractive benefit-cost ratios may have the potential to be self-financing, at least in part, and thus not require funding from donors.

For example, hydropower from a multipurpose dam can yield revenues that can be used to pay back debt used to finance the infrastructure. To the extent that project outcomes can be montarized and sold, the project may impose less of a burden on the government's or donors' budget because the revenue stream can be securitized. It is thus possible that a project may be very attractive from an economic perspective, and can be privately financed (at least partially). Thus, even if the ability to privately finance an infrastructure project does not affect its benefit-cost ratio, it may affect the priority the Nobel Panel will give the infrastructure investment for a share of a fixed government budget.[6] A cursory review of Table 2 shows that the Nobel panel considered other factors in their priority rankings than the benefit-cost ratios.

[6] In fact, one member of the 2008 Nobel Panel told me that large-scale water resources projects with hydropower were given low priority for precisely this reason.

Of course, cost recovery for some services derived from health and social policy investments is also possible. But because household demand for such services is often so low, the efficiency losses associated with attempts to price such services to recover costs may be large (Cohen and Dupas, 2010). For example, the 7th priority in the 2008 ranking is an explicit pricing policy: to "lower the price of schooling" (Table 2). This intervention would increase economic benefits by expanding enrollment, but costs would increase and revenues would decrease, with increased demands on the government budget.

Reasons #5 – Nobel Panel's risk preferences and demand for high-quality evidence

The Nobel Panel's rankings appear to reflect a strong preference for very high-quality evidence. Their high-priority interventions (deworming, micronutrients, biofortification, low-cost heart attack drugs) are mostly supported by evidence on causal linkages provided by randomized controlled trials (RCTs). Such evidence is not available for large-scale infrastructure projects. Large-scale infrastructure projects lead to a concatenation of outcomes that are harder to capture in a cost-benefit analysis than targeted health interventions. This is not to say that rigorous program evaluation is impossible for large-scale infrastructure, but the internality validity of the evidence will never be as convincing as that provided by RCTs of targeted health interventions.

RCTs provide evidence with high internal validity, but not necessarily high external validity. The ability of state institutions to replicate the results of small experimental programs on a large scale and the applicability of the experimental results in different locations are both open to question and increase the uncertainty about the benefit-cost ratios of the interventions and thus the Novel Panel's priority ranking.

Another aspect of the Nobel Panel's priority ranking merits attention. The 2004 ranking was a mix of targeted health interventions and broader policy reforms (reduce trade barriers, lower business costs, reform immigration). By 2012 these broad policy reforms have disappeared from the ranking, replaced by targeted health interventions (and agricultural yield enhancement). By 2012 not only is large-scale infrastructure gone, but so are broad policy reforms. The estimates of the costs and benefits of large-scale policy reforms and large-scale infrastructure are both more uncertain and the beneficiaries more diffuse than for targeted health and social interventions. The Nobel Panel appears to have a preference not only for high quality evidence, but also for small-scale interventions that have easily identified beneficiaries.

Evaluating Investments in Large-Scale Water Resources infrastructure: Five Challenges

All five of the reasons enumerated above provide a partial explanation for why large-scale infrastructure projects are never prioritized in the Copenhagen Consensus rankings. But cost-benefit analysts confront several additional challenges in applying the method to the evaluation of investments in large-scale water resources projects.

First, water is a productive input into so many human activities that it is rarely possible to include all of the channels between improved water services and economic benefits in a rigorous analysis. Not only is water a pervasive factor of production throughout the economy, but also the main technologies for providing improved water services are networks. These technologies are characterized by economies of scale, joint products and positive externalities. These effects take economists into a world of increasing returns from investments and path dependency, and are thus especially hard to quantify and value.

Also, because water–related investments are often large and extremely capital intensive, economic analysis must include a careful assessment of where this capital would have gone if it had not been invested in the water sector. There is no small irony here. Because water services are so important and so pervasive through the economy, it is extremely difficult to identify the appropriate counterfactual against which to assess the costs and benefits of large investment.

Second, investments in water resources infrastructure yield two conceptually different types of economic benefits. The same investment can reduce the losses experienced from the hydrological volatility, and at the same time produce valued goods and services. Both the reduced losses and the benefits of the valued outcomes enter the cost-benefit analysis on the benefit side of the equation. For example, the benefits of dams include both flood control benefits (reduced flood losses) and hydropower generation (a valued good). Similarly, municipal piped water supplies yield both health improvements (reduced losses), and quality-of-life and aesthetic benefits from in-house plumbing (a valued service). It is easy for a cost-benefit analyst to overlook the full spectrum of economic benefits from water-related investments.

Third, large-scale water resources infrastructure can reduce hydrological variability, but this can be both good and bad. For example, the inter-year variation in the flows of the Nile famously brought seven years of plenty followed by seven years of famine. Clearly it would be better if water-related investment could have delivered 14 years of plenty. But for millennia (until the 19th century) the annual Nile flood also washed salts from the soil, and was essential to the long-term productivity of Egyptian agriculture. Without this intra-annual hydrological variability, basin irrigation in the Nile Valley would have been unsustainable. As another example, typhoons destroy homes and lives, but they also bring heavy rains that replenish man-made reservoirs and groundwater aquifers.

Water resources infrastructure can help reduce the effects of hydrological variability, and, depending on the costs and management of the infrastructure, this can increase result in some types of economic benefits (Brown et al., 2011). But ecological systems have adapted to the existing hydrological (and climatic) variability, and species have evolved to handle these conditions. Water-related investments designed to reduce the effects of hydrological variability alter ecosystems and the populations and evolution of species. Such ecosystem changes may impose costs (as well as benefits) on the economy and on the livelihoods of poor households.

Fourth, water easily crosses administrative and political boundaries. In addition many groundwater aquifers cross administrative and national boundaries. Thus, the evaluation of large-scale water resources infrastructure often requires that the analyst make assumptions about the cooperation (or lack of cooperation) of more than one country or state. The assessment of such infrastructure is best tackled at a regional scale and poses complex challenges, in part because states must choose whether to cooperate with their neighbors or attempt to solve their water problems alone. If states do not consider the consequences of their actions on their neighbors, water-related investments will not achieve their full economic potential, and political tensions increase.

Fifth, the causal link between water-related investments, economic benefits of improved services, and economic growth runs in both directions (Barbier, 2004). One can easily document that water-related investments and economic growth are associated with each other. For example, throughout the hunting and gathering stage of our ancestors' history (e.g. 200,000 years to 10,000 years ago), there were essentially no water-related technologies, innovations, or investments – and there was no economic growth in per capita incomes or significant increases in human population. After our ancestors settled down in permanent communities, they began to innovate and invest in water management. They developed pottery, cisterns, dug wells, and dams--and population and total income began to increase (although per capita income changed very little, if at all).

Fast-forward to the Industrial Revolution, and capital investments in water-related infrastructure (dams, canals, and municipal piped water and sewer networks) and institutions (e.g. the Tennessee Valley Authority in the USA) skyrocketed, as have national and per capita incomes in all high and middle-income countries. But untangling the causal links is hard because large-scale infrastructure investments lead to economic benefits and economic growth, and at the same time, economic growth is needed to provide the resources to finance the capital-intensive, water-related investments (Kline and Moretti, 2014).

These challenges in the application of cost-benefit analysis in the water resources sector and the relationship between water resources investments and economic growth have been debated in the water resources literature for decades (Howe, 1968; Cox P.T., C.W. Grover, B. Siskin, 1971; Cicchetti et al., 1973; Cicchetti C.J., V.K. Smith, J Carson, 1975; Bhatia, R. et al, 2009). These debates have increased the awareness of analysts working in the sector of the uncertainties associated with point estimates of benefit-cost results and the susceptibility of benefit-cost findings to political bias and manipulation (Hanke and Walker, 1974). In my opinion, this is less true in the health and social policy sectors, where there is more use of cost-benefit analysis for policy advocacy, and fewer analysts are willing to assert that a specific health or social policy intervention does not make economic sense. The political economy of the use of cost-benefit analysis in the water resources and health sectors is thus different, and this too makes cross-sectoral comparisons problematic.

Concluding Remarks

To summarize, I have made two broad arguments in this paper. First, global average benefit-cost ratios for large-scale water resource management investments are not useful because investments must be analyzed at the local level. It does not make sense to discuss the costs and benefits of large-scale infrastructure such as multipurpose dams as an abstract category of policy intervention. Some dams are economically attractive and others are not. The average benefit-cost ratio of a global set of possible dams is not of interest because it is not necessary to invest in them all. One must be specific about the timing, sequencing, and location of large-scale infrastructure investments. The same is, in fact, true of health and social policy interventions, but cost-benefit analysts in these sectors are less reluctant to present global averages of benefit-cost ratios to advance their case for more sector funding.

Second, the sectoral comparison between infrastructure projects and targeted health and social interventions is misleading for a number of reasons discussed. I believe it would be a tragic mistake for low-income countries to prioritize targeted health interventions at the expense of large-scale infrastructure based on the analyses underpinning the Copenhagen Consensus rankings.

This discussion raises the question of what can be done to improve the Copenhagen Consensus Project's approach. I have two suggestions. First, sector specialists should be asked to provide more evidence from ex-post cost-benefit analyses to support the findings from their ex-ante studies.

The second is to divide sector policy proposals into short and long-range interventions, using some necessarily arbitrary threshold (e.g. 10 years) that would separate large-scale infrastructure projects from the "simple" delivery of health and social services. The Nobel Panel could rank proposals in each category separately. Then the Nobel Panel could try to reconcile the ranking in the two categories, perhaps by making a judgment as to what fraction of the total funding should be allocated to each of the two categories. This would require careful deliberation of the differences inherent in these two types of interventions.

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This paper was written by Dale Whittington, Professor of Economics, Departments of Environmental Sciences & Engineering, and City & Regional Planning, University of North Carolina at Chapel Hill and Manchester Business School.. The project brings together 60 teams of economists with NGOs, international agencies and businesses to identify the targets with the greatest benefit-to-cost ratio for the UN's post-2015 development goals.

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