Despite the many failures of the past, foreign aid is once again seen as a way to 'make poverty history'. **Peter Boone** argues that to have a real impact on extreme poverty, aid needs to be much more carefully targeted, allocated on the basis of good scientific evidence of its effectiveness and delivered through well-designed institutions.

Effective intervention: making aid work

Large foreign aid flows are making a comeback. In the past year, the members of the G-8 have promised to increase aid by $50 billion annually by 2010, the European Union has promised to raise aid to 0.7% of GDP by 2015, while Live8, Tony Blair's Commission for Africa and Bill Clinton's Global Initiative have brought greater public awareness to the pressing problem of extreme poverty.

Jeffrey Sachs' book, *The End of Poverty*, is a brutally compelling document outlining the case for more funds. The situation he describes is dire: over 8,000 people die daily from AIDS having never received adequate antiretroviral therapies; and a further 27,000 children die each day from preventable infectious diseases and birthing problems. While in most parts of the world, extreme poverty is on the decline, in sub-Saharan Africa, the number of extremely poor has doubled, to 300 million, in the last 20 years. Sachs' book focuses on the need for much larger funding to end this 'poverty trap', calling for an increase of funding from $65 billion now to $135 billion in 2006.

Will more aid work?

Sachs is making an enormous contribution to the goal of poverty reduction by outlining poverty's terrible human impact. Indeed, given the scale of the problem and the relatively small effort that western countries make to help solve it, it seems cruel, bordering on immoral, to question whether more aid will work.

But critical analysis combined with action is essential to make sure we really do solve the problem. Unfortunately for Sachs, there is one very large problem with his plan: the history of large aid flows is, to date, a major failure.

In research I completed with CEP colleagues over a decade ago, we examined the relative performance of 96 countries to see whether increased aid flows led to higher growth or more rapid improvement in health indicators such as child mortality. The answer was clear: between 1970 and 1993, countries that received large aid flows fared no better than countries that received small aid flows either in terms of growth or measures of extreme poverty such as child mortality.

Subsequent research with more recent data has confirmed this finding. For example, in well-publicised studies, Craig Burnside and David Dollar at the World Bank used the same data I used to modify the argument. After dividing countries into categories according to quality of economic policies, they concluded that countries with 'good economic policies' did benefit from aid though for most countries, the benefit was small.

This research was used by the World Bank to justify more targeted aid. But the conclusions were later shown to be a statistical fluke. When William Easterly and others extended the dataset by an additional five years, Burnside and Dollar's results disappeared, with the conclusion again being that cross-country data suggest larger aid flows don't raise growth or improve health, better economic policies notwithstanding.

Focusing aid on what works

The aid successes with which we are all familiar – the eradication of smallpox, vaccination programmes, antibiotics and emergency disaster aid to relieve famines – are important, but they've never been part of a case for large aid flows.

Indeed, these policies are cheap to implement and make up a small portion of all aid flows. In 2004, 4% of bilateral aid went to health, 12% to education and

The history of large aid flows is, to date, a major failure

6% to emergencies. The largest category is 'economic infrastructure', which receives 23% of total funds. The remaining aid is divided among a large number of small projects including civil society, trade promotion and administration.

In my view, the failures from the past are too often glossed over by aid advocates. Careful after-the-fact evaluations of aid projects by donors are rare, and when they are done, they are usually flawed by the standards of scientific analysis. This reluctance to make careful assessments may actually be counterproductive.

Sachs criticises former US Treasury Secretary Paul O'Neill for stating: 'We've spent trillions of dollars on these programs and we have damn near nothing to show for it.' In a recent survey, the UK public appeared to side with O'Neill: 83% of respondents thought aid would be wasted by recipient governments. But as the favourable public reaction to Live8 seems to show, the problem is not that the many critics don't believe in the moral agenda: rather they don't believe we've found a means to solve that agenda through large aid flows.

I've recently helped found an organisation, Effective Intervention, which sponsors programmes in Africa and Asia aimed at reducing extreme poverty. We've spent the last year examining alternative sectors and projects to decide where interventions can be most effective. We are presently helping design several projects in India and Africa that target large, inexpensive improvements in child health. We hope this research will contribute to a better understanding of how to reduce extreme poverty cheaply, and potentially improve allocation of aid budgets.

Reducing child mortality

Let's start by looking at what really can be achieved, and without too much money. Figure 1 illustrates the percentage of children that die before the age of 5 in low- and middle-income countries. In Niger, Malawi and Ethiopia, more than 10% of children die before the age of 5. This contrasts with Cuba and Sri Lanka, where fewer than 2% die.

Figure 2 shows child mortality rates across states in India and makes a similar point. Despite having the same national political and legal system, and similar

Figure 1:
Comparing child mortality rates (deaths per 1,000 children aged 0 to 5) with incomes across countries

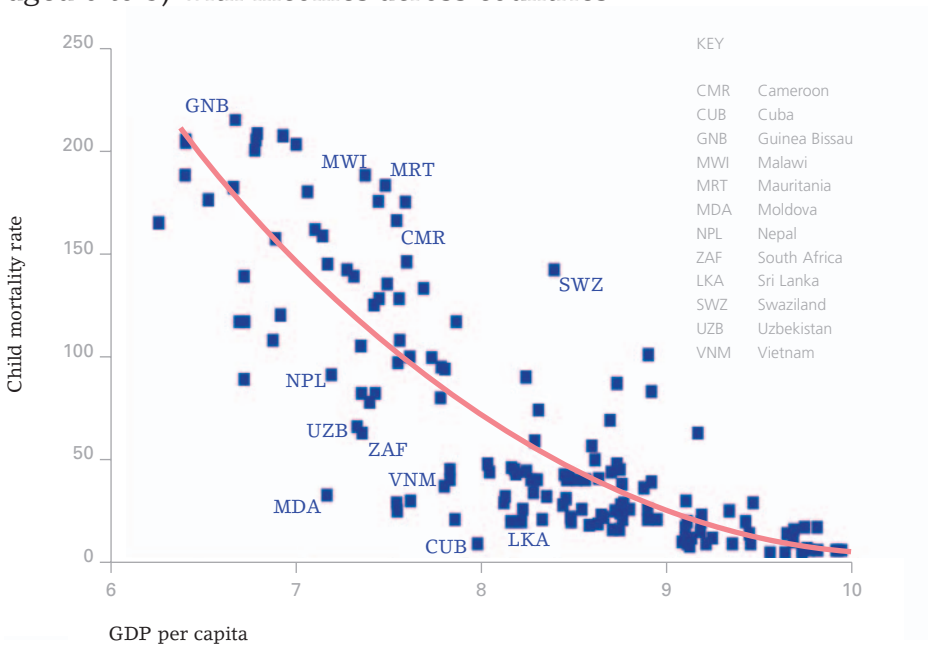
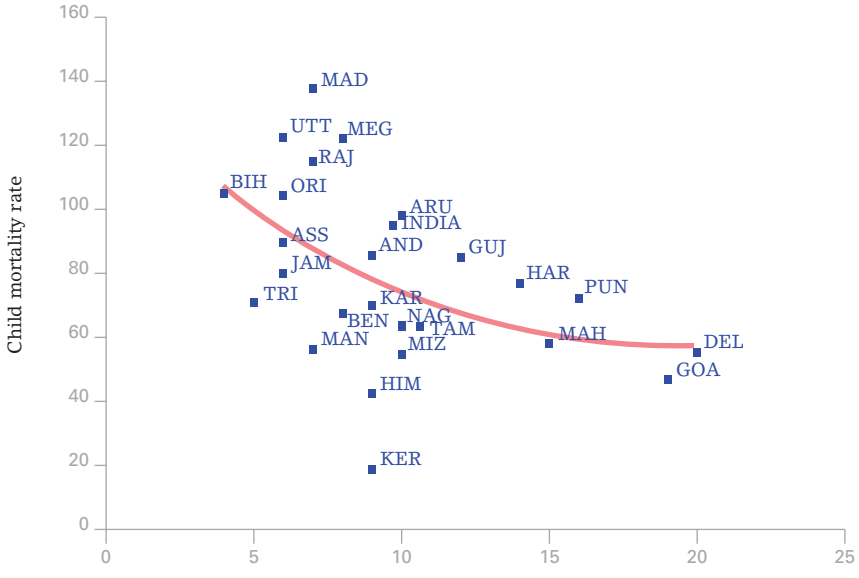


Figure 2:
Comparing child mortality rates (deaths per 1,000 children aged 0 to 5) with incomes across states in India



KEY

AND	Andhra Pradesh	HIM	Himachal Pradesh	NAG	Nagaland
ARU	Arunachal Pradesh	IND	India	ORI	Orissa
ASS	Assam	JAM	Jammu	PUN	Punjab
BIH	Bihar	KAR	Karnataka	RAJ	Rajasthan
DEL	Delhi	KER	Kerala	TAM	Tamil Nadu
GOA	Goa	MAD	Madhya Pradesh	TRI	Tripura
GUJ	Gujarat	MAH	Maharashtra	UTT	Uttar Pradesh
HAR	Haryana	MAN	Manipur	BEN	West Bengal
		MEG	Meghalaya		
		MIZ	Mizoram		

Donors have never adopted the scientific discipline needed to measure carefully how well their projects work

levels of income across states, there are striking differences in child mortality. The state of Rajasthan has child mortality rates similar to the poorest sub-Saharan African countries, while Kerala has achieved levels that are not far from Western Europe.

The success of Kerala, Cuba, Sri Lanka, Costa Rica and many socialist countries is even more striking because it has been achieved in very different political and economic systems. The Cuban health system benefited from a socialist revolution that was instigated by paediatricians. They built a state-funded health system largely from scratch. The success in Kerala is more complex, but today is based on a private healthcare system. Roughly three-quarters of medical care in Kerala comes from the private sector. In all these cases, the total healthcare spending of these states is roughly equal to the average for low-income countries. Good healthcare can be cheap.

I have no doubt that we can fairly rapidly achieve the success of Kerala in other regions of India and in sub-Saharan Africa. The Bellagio Child Survival Papers, a series of reports by leading global medical and healthcare experts published in *The Lancet*, concluded that 70% of child mortality in low-income countries could be eliminated through universal access to 23 basic health interventions and treatments. The measures these studies specified are not expensive, but they require enormous institutional change in many countries. The conclusions are not surprising once the cause of death is understood: nearly all child deaths in low-income countries are from treatable causes, such as diarrhoea, pneumonia and infections acquired during birth.

At Effective Intervention, our furthest advanced programme is in tribal regions of Andhra Pradesh in India. In this extremely poor region, 6% of children die before they reach one month of age. Roughly 70% of these deaths are attributed to infections (mostly due to unclean procedures used when cutting the umbilical cord and subsequent care of the belly button) and poor procedures at delivery that lead to birth asphyxiation.

Together with the Naandi Foundation and colleagues at the London School of Hygiene and Tropical Medicine, we are planning a multi-year trial that aims to reduce neonatal mortality by 50%. The programme focuses on improving antenatal care services, and raising education for village health workers and mothers. Since most neonatal deaths are caused by simple problems related to hygiene and delivery, there is good reason to believe that better education and techniques will go a long way to reducing mortality rates.

Large-scale reductions in child mortality rates can be achieved at relatively low cost

We've designed the intervention on the model of a pharmaceutical drug trial that would meet the highest standards of credibility set by the US Food and Drug Administration and comparable European regulators. We'll implement the programme in a region with a population of roughly 300,000, randomising villages and including a control group that initially receives no interventions.

Once our project has achieved a large reduction in neonatal mortality in the intervention area, as assessed by an independent data monitoring committee, we will then expand it to cover the control region. In this manner, we will be able to measure our success carefully. If we can't achieve a large mortality reduction relative to the region where implementation is delayed, there's little purpose in expanding it. If we can achieve it, we can make a case for expanding the programme in similar regions elsewhere in India.

What does all this cost? The surprising answer is very little. The recurring costs of the project will be around $80,000 a year. We could expand such a programme to all of Africa for under $500 million a year. Of course we need to prove such a programme could work in different regions, and it would have to be modified, but the point is that large-scale reduction in child mortality can be achieved. Costs are not the issue: the much bigger problem is designing projects with specified, verifiable results, and creating the institutional structures to achieve such results.

The murky side of water infrastructure

So what's going wrong? If there are cheap means to reduce extreme poverty by addressing neonatal deaths and child mortality, why are we not focusing on those? Part of the problem is that the donor agencies have not adequately attempted to allocate aid where we know it works, and to complicate matters, they have never adopted the scientific discipline needed to measure carefully how well their projects work.

One of the first areas we looked into at Effective Intervention was investing in water infrastructure. This includes drilling wells, providing pumps and possibly pipes, so that households can have improved water sources. The potential benefits seem large: infectious diseases cause the bulk of child mortality in extremely poor regions, and these diseases can be prevented through better hygiene.

Water infrastructure varies sharply throughout India, and there are good data from national health surveys on household disease and mortality. So in regions with improved water quality (after controlling for incomes and education), do we see a

large reduction in child deaths or infectious disease? Based on surveys of 90,000 households across India, we've found that access to improved water supply has very little impact on the incidence of disease: you get sick as often whether you have improved water supplies or not.

The reason water supply fails to reduce the incidence of disease probably relates to multiple causes, including failure to service infrastructure properly, and contamination in storage containers at home, but also to the importance of hygiene. A systematic review by Val Curtis and Sandy Cairncross concludes that

Simple hygiene like washing hands with soap is more important than water infrastructure in reducing disease

washing hands with soap (and presumably hygiene in general) is more important than infrastructure at reducing morbidity. You can wash your hands with dirty water, but as long as you use soap, you will avoid much disease – and it is much less costly. It seems that clean water is not necessary to reduce disease substantially.

Despite the weak evidence linking water infrastructure to mortality and disease, there are large projects in progress and being planned. For example, in Uganda, a US consulting firm estimated the country needed $2 billion in aid to modernise its water infrastructure. It may be easier to build a water system than to build sustainable rural healthcare and related education, but the evidence indicates it would not be wiser.

Getting aid to the right projects

The difficulty with water infrastructure highlights a key problem with aid programmes. To do them well, we need to be far more rigorous in deciding where to allocate money, and also ensuring that results are achieved. This requires a scientific approach to projects: we need to estimate returns in advance, monitor outcomes and design our projects so that we learn as we go.

The Millennium Challenge Corporation (MCC) is a good example of an attempt to allocate aid better. The MCC selects countries that have good records on 'ruling justly, encouraging economic freedom, and investing in people' using independent rankings derived from 16 indicators. They offer the selected countries large grants to finance programmes that are 'transformational'. The programmes are selected by the national government, but they must be based on a nationwide consultative process. Each individual project needs to demonstrate that it will generate large positive economic returns before it can be agreed.

The actual proposals by each country are readily available on the MCC website (http://www.mca.gov) and make interesting reading. The bulk of the projects are for infrastructure and generally in areas where it is hard to assess the benefits. But the MCC is making a valiant effort to measure potential returns rigorously, and then monitor implementation and outcomes. They have rejected many projects because, after careful analysis, they found them to be uneconomical. This is a big step forward.

One weakness of the MCC is that they only provide funds to a select group of countries: the extremely poor live in many countries that do not satisfy MCC criteria. What's more, the organisation limits funds to five-year allocations, so, for example, long-term projects aimed at improving healthcare and education could not be funded beyond five years. To solve the problem of the extremely poor, we need to select effective projects, and target funds to reach them also.

Ensuring it is the poor who benefit

Sachs' book is more concerned with total spending than the allocation of spending across sectors, and there is not much on how to ensure that the poor directly benefit. But one of his most contentious comments is that 'development economics is like eighteenth century medicine, when doctors used leeches to draw blood from their patients, often killing them in the process,' meaning that, in their crusade against profligacy, the IMF and the World Bank advise poor countries to raise taxes and cut spending, thus actually bleeding those countries of the funds they need to fight poverty.

Sachs' solution is to change the aid allocation process radically: national governments should design multi-sector programmes that aim to reduce extreme poverty, the United Nations should coordinate donors, and multilateral and bilateral agencies should find as much funds as needed to back all worthy programmes. To buttress his arguments, he mentions five poverty reduction programmes completed by Ethiopia, Ghana, Kenya, Senegal and Uganda, which he believes are of high quality and demonstrate how a revamped aid allocation system could work.

I took a close look at Ethiopia's three-year Sustainable Development and Poverty Reduction Programme published in 2002. Ethiopia is one of the poorest countries in the world. Roughly 10% of children die before one year of age, and only 30% of the rural population is literate. The country suffers a major AIDS epidemic. Their programme is described in 225 pages, including significant sections on AIDS, schooling and child health and a plan for very large spending on agriculture. There are also a few specific targets but these are goals rather than well-defined endpoints linked to projects. Despite the broad nature, a careful read raises questions as to whether this sufficiently addresses our goal of reducing extreme poverty.

In the document, Ethiopia's fiscal programme specifies how expenditures will be allocated. In 2005, already three years into the planned programme, the government expects to raise 22% of GDP in revenues, but spend 33% of GDP, leaving a budget deficit of 11% of GDP to be financed mostly by external debt and grants.

Of this spending, 3.8% of GDP is allocated to education and 1.1% to health. The GDP of the country is $8 billion, so roughly $90m goes to health or a little over $1 per capita. While the document claims that the priorities and goals of the programmes were the result of widespread grassroots discussion and meetings, I find it hard to believe that the extremely poor had much say in the process. In a country with one of the highest child mortality rates in the world and a major AIDS epidemic, can we really believe the population is satisfied to have one of the lowest health budgets (in absolute terms and as a percentage of national expenditures) in the world?

This raises one of the most difficult issues in aid allocation: given the nature of national elites and the ambitions of the nation-state, it is unlikely that poverty reduction will trump other priorities anywhere, even in desperately poor countries. When giving aid, we need to recognise that we are actually setting different goals from those of the recipient's political system, so working through a national development plan designed by the central government may simply be the wrong way to start.

Targeting aid to reach people in extreme poverty

The Global Fund to fight AIDS, malaria and tuberculosis, which Sachs deserves credit for helping create, is a good example. The Fund finances AIDS prevention and antiretroviral therapies in recipient countries. To be eligible for funding, recipients need to come up with a credible programme agreed in a broad cross-section of the country. The implementation is monitored carefully, and the Fund has teeth: work in Uganda and Burma was suspended recently when it looked like local administrations were preventing success.

The advantage of the Fund structure is that it takes some of the politics out of aid allocation: recipients know there is money available for a specific project that alleviates extreme poverty, and the donor agency has a clear guideline as to what should be achieved. There is a good scientific basis for believing that AIDS prevention strategies are cost effective and highly important for reducing extreme poverty. It seems this model, which can be applied to all countries, provides a good blueprint for more expanded targeting of aid.

While Sachs criticises the existing development economics paradigm practised by aid agencies, he could easily have extended the criticism to the broad array of untested projects that we currently implement as aid. While programmes have improved in recent years, we still have far to go if we are truly to target funds to the problems we believe they can best address.

I've argued that we need to revamp our aid allocation process if we are to achieve our goal to reduce extreme poverty. Specifically, we should allocate far more aid to areas where we have good scientific evidence that it works, and we should do this through well-designed institutions like the MCC and the Global Fund, which have a mandate to measure and monitor outcomes carefully. We also need to take much more care to evaluate and monitor the impact of large infrastructure projects, such as roads, water supply and electricity, given their poor track record and relatively high costs.

Through such mechanisms, we could dramatically improve child health and related education, along with greatly reducing the burden of AIDS. But we need to focus this aid where it is needed. Africa has seen major improvements in child healthcare, literacy and education over the last 20 years, and most African countries are richer than they ever have been. The continent is now the fastest growing market in the world for mobile telephones, and it looks set to benefit from a long-term recovery in commodity prices, along with demand for West African offshore oil, as India and China grow.

The problem is not that Africa will be mired in poverty without aid, but rather that there is a large population of extremely poor households who are being left behind. This makes it all the more imperative that we target aid at these groups, and make sure it works.

Peter Boone is a research associate in CEP's globalisation programme and the chair of Effective Intervention (http://www.effint.org).

Further reading

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