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Breaking Out Of the Pocket:

Do Health Interventions Work? Which and In What Sense?

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Abstract

Despite likely strong medium-term growth prospects for many poor countries, there will remain serious pockets of poverty for the foreseeable future. People in these pockets are unhealthy, uneducated and low income. Interventions to improve health in these pockets, by targeting child mortality, are appealing as ways to increase welfare, and a package of health measures is likely to have more effect than single interventions. Increasing access to education should presumably be bundled with health improvements. Robust sustainable delivery mechanisms also need to be established. Research design should move towards testing the plausible set of ideas in this regard, sometimes implemented as packages of measures rather than single interventions, using the high standards of medical science.

Still, it remains unclear if improving health and education by themselves will substantially eliminate these pockets of poverty. We also need an appropriate experimental research design that will examine the effect of adding to the bundle of interventions a variety of more direct measures that could increase productivity.

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"The only things that matter in this fallen world are transportation and sanitation."
-- Rudyard Kipling, 1913[2]

I. Introduction

By the early 1960s, there was a great sense of progress and optimism among both epidemiologists and economists. Both felt that they had tied major breakthroughs in their respective fundamental sciences to the pressing problems of the contemporary world. And both saw strong evidence that they had created plausible policy "levers" that could be adjusted so as to improve people's lives and increase prosperity around the world.

Much of this positive view in both professions was justified. For example, the progress over the past 50 years in terms of lowering the burden of infectious disease is undeniable. And during the same time span, a substantial number of countries have experienced sustained growth, reaching previously unprecedented levels of GDP per capita.

One could argue that the applied public health progress was more as directly intended; new drugs, vaccines, and – most importantly – innovations in national and international delivery systems have had profound effects in terms of increasing life expectancy and reducing ill-health. Economics, without doubt, has experienced more mis-steps, particularly in terms of policy recommendations or "interventions" suggested and funded for low income countries. Bill Easterly's (2001) aptly titled book, *The Elusive Quest for Growth*, makes this point in compelling detail.[3] And Dani Rodrik is of

[2] Quoted in Anderson (2006), first page of introduction.
[3] Rosenstein-Rodan's (1961) forecasts of capital requirements and growth implications for "underdeveloped" countries might be seen as the high tide of incautious optimism about the preciseness of our knowledge about economics.

course right to worry that the evidence on policies that work in any measurable cross-country sense is still uncomfortably weak.[4]

Yet, seen in the bigger picture, the fall of communism and the closely associated intellectual defeat (or substantial retreat) of the idea that the state and its bureaucrats should run a large part of the economy, can reasonably be attributed in part to modern economic thinking.[5] The state, and the elite that controls it, is far from finished as an economic actor, but the idea that state-owned firms will predominate is long gone.[6]

And it is the consequent economic rise of both India and China that sets the stage for the next 20 years of global economic growth.[7] No doubt there will be (and perhaps even are today) bumps in the road, but a major transformation of the global division of labor started 20 years ago – even if we only now recognize the profound consequences.[8] This transformation was, of course, made possible by a revolution in "transportation," meaning the cost of moving goods and services, which began for goods with containerized shipping in the early 1960s and spread to services through the information technology innovations of the 1980s and 1990s. This transformation is as profound and

[4] For anyone feeling that there are easy answers or that outsiders can drop in with previously overlooked solutions, Rodrik's webpage and blog are appropriate antidotes: http://ksghome.harvard.edu/~drodrik/. His views remind us of the mid-19th century pessimism about the effectiveness of medicine, known as "therapeutic nihilism" (Shorter, 2006). Much of that pessimism, and its associated emphasis on the need for more careful study of the fundamentals, of course turned out to be justified.

[5] The debate over the Washington Consensus and the apparent concern that this was in some sense inappropriate, not applied or did not work (e.g., Spence et al 2008) seems to miss the point. The Consensus (and the underlying ideas that long predated it) prevailed in broad terms against the alternatives, and countries that figured out how to apply an appropriate version of this Consensus are now driving global growth (Johnson, Ostry and Subramanian 2007).

[6] The rise of Sovereign Wealth Funds suggests that the state may be "back" as a global creditor. More generally, of course, there are vast resources controlled by elites in many countries – whether these elites accept the label of "state-" anything on their activities is largely irrelevant. What matters is the nature, purpose and operation of political connections.

[7] Seen in terms of direct economic consequences for the global economy, the fall of the Soviet bloc was a side show; with the major issue, still unresolved, being exactly how this will affect the provision of oil and gas to the world market. The former Soviet empire is in demographic decline and was never more than 300m people to start with. India and China have close to 3bn people between them.

[8] On the consequences of globalization, see International Monetary Fund (2007a and 2007b). On the difficulty of anticipating precise bumps in the road, see Berg, Ostry and Zettelmeyer (2008).

as irreversible as any other major wave of intertwined technological and social innovations.

The rapid growth of India and China, both in terms of wealth and human capital, will pull along even the poorest regions. The rise of India and China has already reversed a decades long decline in commodity prices, opening up opportunities in Africa and poorer Asian countries for major investment. The same rise will also change labor markets, encouraging the movement of low-value added industries to lower wage countries, and offering Africa and other parts of Asia the opportunity to become low value added manufacturing and service bases in the future.[9]

Yet this transformation and the associated rise of prosperity in what were, until recently, relatively remote and low productivity parts of the world undoubtedly leaves some people behind. It is therefore reasonable to expect that countries previously known as "poor" will be host to regions of high income (close to industrial country averages or higher) and other, smaller pockets of extreme poverty.

This paper focuses on what could make a substantial difference to people living in these potential pockets. The market is unlikely to reach down and lift them up anytime soon. In part, this is because they do not have access to high quality public goods, particularly health care. "Interventions" that improve health could make a big difference, but only if (a) we understand more how about to package health *and* education interventions, and (b) there are ways to also provide potentially productivity enhancing

[9] The evidence already suggests strongly that there are no insurmountable obstacles to Africa's development (Johnson, Ostry, and Subramanian, 2006). The key is finding ways to integrate with the global economy that are robust to internal and external shocks. On this, substantial progress has been made, but the jury will remain out at least until we see what happens when there is a large negative shock to these countries' terms of trade (International Monetary Fund, 2008).

interventions. Point (a) is essential and also attainable. Point (b) may or may not be potentially essential, and it remains unclear whether it is attainable.

Section II explains the present and future for pockets of poverty in broad terms, based on what we observe in India. If we accept there should an agenda to reduce pockets of poverty, we need to think very clearly about how to channel help to the people that need it most. Section III reviews what we know about the political economy why pockets exist and persist.

Since what works and doesn't work in interventions is a vast topic, we concentrate here on evidence regarding a narrower topic: how to reduce child deaths in pockets of poverty? This is an area where some success has already been achieved. Clinicians often argue that we already know how to dramatically reduce child deaths; however, closer scrutiny reveals there is a great deal of uncertainty. Section IV discusses the methodological issues and suggests one way to make further progress; this is somewhat at odds with the relatively new standard approach in development economics.

We then consider the potential poverty impact of reducing child deaths and argue in Section V that this can be substantial and should be pursued. Section VI reviews the available evidence on what works and what doesn't, but also highlights what we don't know – and how we can address that. Section VII suggests that the key to delivery is to build private services that largely bypass the public sector. If designed appropriately, these should be sustainable.

At the same time, in Section VIII, we caution that the available macroeconomic evidence suggests improved child health may not necessarily raise per capita incomes and reduce poverty. While there may be some "end-runs" around this problem, including

through migration, we remain concerned that there is not yet an integrated agenda to define, test and implement packages that include direct measures that will raise productivity and reduce poverty.

Our conclusion is that we probably can dramatically improve health, and possibly speed poverty decline, in the pockets of poverty, but to get there we also need to implement new research strategies and evaluation methodologies. This is likely to be prove more effective and more sustainable that a strategy of "just do it." The best evidence today comes from well-designed randomised controlled trials, however, as we discuss below, the existing evidence derived from these has important methodological and strategic shortcomings when analysing how to reduce child deaths. Fortunately, the nature of pockets of poverty is such that they lend themselves to well-designed randomised controlled trials where broad packages of interventions could easily be tested against status-quo or alternatives; Section IX lays explains our work in progress in this regard. Section X concludes that the advent of more testing and experimentation with these packages may mean we will soon (within 20 years?) learn the best routes to rapidly end pockets of poverty.

II. Pockets Defined

During the next 20 years, we can expect the pattern of poverty and poor health around the world to change dramatically. The days where large nations suffer massive extreme poverty, such as the times of famine in Bangladesh and India, are certainly

gone.[10] The reality today is that pot-bellied malnourished children are very rare, and in twenty years they should be almost unheard of.[11]

This change in the pattern of poverty has important ramifications for development assistance. We believe the pattern of poverty observed in India today will be representative of many poor nations.[12] Despite rapid growth and improvements in health and education, many regions and communities in India still suffer poverty similar to the worst areas of sub-Saharan Africa. For example, the state of Bihar has a child mortality rate similar to Malawi, with 12% of children dying before the age of five, while in urban Andhra Pradesh child mortality is one third that level. If you drive three hours due west of wealthy Hyderabad neighborhoods, you find villages where 6% of children die at birth, and schools and public health services hardly function. Within these communities lower caste families and tribal areas are even worse off.

The education system has similar discrepancies. In urban areas children attend public or private schools and many young adults achieve standards equivalent to averages in wealthy nations. However, smaller towns and many villages have schools that hardly function. For example, today in India, despite high official enrolment rates, 33% of children in grades 3-5 are unable to read text at the level of a grade 1 student, and even more are unable to do simple subtraction (Pratham (2007), Sathe (2005)).

[10] We recognize that measured progress towards poverty reduction has been uneven (Chen and Ravallion 2007); in fact, this is part of our point. We are also aware that some countries may impose terrible burdens on their people, for example through creating agricultural production disasters or refusing to allow outside support in the case of catastrophe. Amartya Sen's insight on the incompatibility of democracy and famine continues to hold.

[11] The World Food Program, World Bank and others are no doubt correct to emphasize there is in 2008 a new food crisis and this may push back anti-poverty progress by 10 years. Our view is that a supply response will be forthcoming, as seen after every other surge in commodity prices. We also see gains for many low income countries coming exactly from higher commodity prices. But it is true that changing terms of trade, if persistent, may deepen (and perhaps move the location of) the pockets of poverty we focus on below. It is certainly unlikely to eliminate the issue.

[12] The pattern in China may be more similar than is commonly thought. But the data are much less good, so we confine ourselves to India as the plausible benchmark.

We believe this change in the map of poverty has critical ramifications for the design and use of aid programs that aim to improve people's lives. While we can reasonably anticipate that trickle down will eventually make most groups better off, it may take many generations to remove the pockets of extreme poverty. This delay has many costs. For some people it will simply mean they die early in childhood, where they otherwise might not have. For those children that survive, they will receive very poor education. Recent neurological and behavioural studies suggest human brains have a critical period of development during childhood. Children who miss out on early stimulation and education may ultimately be worse off mentally and socially, and these losses appear irreversible. So by ignoring the problems today, we condemn the current generation of children to some permanent neurological disabilities.

The existence of these large pockets of poverty has potential costs for rich countries, including the United States and European nations, and for local neighbors. In order to limit the development and spread of disease, such as Asian bird flu or AIDS, we need good monitoring systems and effective national programs. Since poor regions are generally "dysfunctional" in these areas, they can become long term incubating grounds for disease. They will certainly host and maintain a human reservoir of existing infectious diseases such as malaria, making it necessary to develop new treatments as resistance builds, and preventing any closely connected regions from permanently resolving disease.

This change in the map of poverty also has important ramifications for how aid agencies with a moral agenda to reduce poverty should function. We need to recognize that inhabitants of these pockets of poverty will often be outcasts and weak groups of the

political system, so funding and programs directed through governments to improve the poor's lot will probably fail. If we cannot turn these "outcast" regions into insiders, we should not expect central governments to truly use resources to make these regions rise up to be more nearly equal to more privileged areas.

III. Why do Pockets of Poverty Exist?

We define pockets of poverty as regions in extreme poverty, where the basic services needed to preserve and build human capital are poor or non-existent, so that child mortality is high and literacy rates are low. Unfortunately, there are no reliable, comprehensive surveys that allow us to calculate the population in such regions, but the numbers would certainly be in the hundreds of millions.[13]

Figure 1 shows the international pattern of child deaths, plotting child mortality against log GDP per capita. As we discuss below, child deaths are highly correlated with education and income, along with provision of public services, so they are probably good proxies for general failure to promote human capital. These national statistics hide very large variation within countries. Northern states of India, such as Bihar, have child mortality rates similar to western Africa. The rural rates of child deaths in India average 30-50% higher than urban rates. Even in the better off states, services to tribal populations and other "outsiders" are generally extremely poor.[14]

Despite India's rapidly growing wealth, such pockets of poverty are widespread. There are many reasons why they persist in India, and are likely to become the standard

[13] According to World Bank (2008a and b) estimates 2.7bn people live with less than $2 income per day (in 2001). Most of these people probably live in regions where health and education function poorly.
[14] In a baseline survey of 144,000 women ahead of a randomized controlled trial in tribal regions of Andhra Pradesh, not far from Hyderabad, we found 6% of children died before one month of age in 2007.

in poor African nations and other poor nations as they too become wealthier. One reason is the interaction of family environment and geography in any economic system. Children with illiterate parents who have not benefited from economic change need to break with their parent's generation. Their ability and desire to do this will depend on many factors, but it is natural to imagine that isolated regions, or specific ethnic and religious groups, will naturally integrate less quickly with the changes around them. This may due to choice, but often, especially for the children, it is not.[15]

A second reason is the inherent nature of capitalist politics and interests. While rapid economic growth probably trickles down to all eventually, people with wealth concentrate in urban areas. They tend to ultimately control political power, and they demand good private services for themselves, or they demand the government make services available to them. This naturally leads to a concentration of the best public and private services in wealthy areas.[16] Migration to urban areas may be a solution, but if remote villages have not provided decent health and education to children, the migrants may not be well-placed to succeed in urban areas, and this could slow the migration process.

Third, in some instances we observe a tyranny of some groups over other groups. In the tribal regions of India, low caste groups, Muslims, and others have historically faced prejudice from all levels of authority. It will take decades to truly change the ability of these peoples to fully benefit from rapid growth, and to pull some communities out of their current extreme poverty. If political power is based in part on oppression

[15] Sachs (2005) stresses the importance of geography. People living near water, rail links and good roads will be better able to benefit from the coming growth than those people living in more remote regions with poor transport.

[16] This is a version of "urban bias," but not exactly the versions in Lipton (1977) or Bates (1981).

through ill-health, then there will be powerful opposition to sensible interventions by outsiders.[17] Our impression is that this is a relatively small consideration in most areas, although there can definitely be a backlash if local notables feel that outsiders are organizing relatively poor people for political action.

Finally, even when there is public or private demand for health and education services, there can be other obstacles to regular provision of such services. Collier et al (2003) calculate that on average, nations in sub-Saharan Africa experienced civil conflict once every six years during the last half century. This makes long term investment and regular supply of health, education, infrastructure, private business, etc., difficult to maintain. The problems that stem from communities with different ethnic, religious, tribal, and other backgrounds living close together means that such problems will persist and those regions may be poor for a very long time.

We cannot quantify the relative importance of these rival explanations in general or how they are balanced across different locations. In general, our qualitative assessment from practical experience is that pockets of poverty exist primarily because they are neglected by the elites with power and by the groups that have effective voice. It is neglect, rather than a sinister conspiracy of oppression, that means relatively isolated people do not have access to decent health care or education. As a result, it is possible for outsiders to offer such services without generating adverse political reaction or the development of some other means of oppression.

[17] The value to the elite of continued ill-health and denial of health care for the poor is one element of "structural violence" emphasized by Paul Farmer (2004). No doubt he is right for many situations and this may provide a convincing mechanism supporting the finding that "inequality" is bad for the poor, e.g., Ravallion 2005, but we don't (yet at least) see compelling evidence that this implies a general first order constraint on effective health interventions – again, Farmer's own medical work suggests these are possible even in the most difficult circumstances.

IV. What Constitutes Good Evidence?

There is a remarkable difference between the evidence considered acceptable to justify development policies and the evidence needed to gain regulatory approval for simple drugs. Since one of our goals is to examine the existing evidence, we first start with a framework analysing the quality of evidence.

The gold standard for evidence in the medical literature is a well-designed randomised controlled trial. Table 1 illustrates the phases of development and associated types of evidence necessary to get a drug approved by the US Food and Drug Administration (FDA). Phase 1 and 2 are early stages in which initial evidence on safety and efficacy is gathered. The key necessary step for approval is Phase 3. Here drug companies must pre-specify the design and analysis in one or more randomised controlled trials, which are sufficient to demonstrate efficacy of the drug in a setting similar to public use post-approval. The rules for design of randomised trials have evolved over many decades, and they are structured to limit the scope for investigator and selection bias.

In general, the published evidence today in the economics literature – as well as much of the public health literature – would be classified as phase 2 information in medical science. Creative secondary analysis of randomised experiments, e.g., Miguel and Kremer (2004), of course can provide valuable insights into optimal project design, and add substantially to understanding of the impact of interventions outside of the primary analysis.

But the view in medical science is that retrospective analysis of surveys and other data is subject to substantial investigator and selection bias, and so would never satisfy

strict criteria needed for demonstrating efficacy of a drug. The advent of randomised evaluations in economics without doubt greatly improves the quality of available evidence, but these would not be considered well-designed randomised controlled trials in the medical community, due to the lack of pre-specification of primary and secondary endpoints, no pre-specified statistical analysis plan, multiple endpoints, use of non-testable hypotheses to analyse trial results, emphasis on somewhat complicated regression analysis rather than simpler trial designs, etc.[18]

The statistical importance of the quality of information can be readily seen from success in drug trials. Approximately 70% of drugs that make it to human trials advance to Phase 2 trials. Of those that succeed in phase 2, approximately 60% ultimately succeed during phase 3 trials; across therapeutic categories, this conditional success rate ranges from 39% to 83%. Hence approximately 40% of drugs which appear likely to be effective after phase 2 are later rejected during phase 3 (Danzon, Nicholson, and Pereira 2005). These numbers probably understate the true failure rate of phase 2 information since many drugs that enter phase 3 are based on previously approved products, such as different dosing regimes for pain relief ingredients already approved.

Unfortunately, when designing programs to reduce child deaths, there is limited evidence available from well-designed randomised controlled trials (RCTs). Such trials

[18] See, for example, the Consolidated Statement of Reporting Trials (CONSORT) regarding transparent reporting of trials and trial design (www.consort-statement.org); this has been endorsed by most leading medical journals. These rules reflect accrued consensus in medical science over the last several decades reflecting a long history of failures and successes in trial design. The rules continue to evolve but in general are becoming increasingly strict. Randomised evaluations in the economics literature typically fail two key tests: pre-specification of the primary endpoint, and pre-specification of the analysis plan. In medical trials, any secondary findings, i.e. all those that are not the pre-specified primary analysis of the primary endpoint, are judged to be exploratory and must be confirmed by at least one further dedicated trial. This strict focus on pre-specification reduces the risk of investigator bias, since investigators can otherwise pick and choose analysis strategies and endpoints to find significant results which in reality may be spurious. See Altman, Schulz et al (2001).

are primarily limited to clinical settings where existing and new drugs are tested or community settings where very specific interventions, such as hand-washing, are introduced. As discussed below, these trials help us understand which ingredients should be used in health packages to limit child deaths, but they do not tell us much about the best means to ensure services are received by children that need them. Another flaw with these trials is that they rarely have all-cause mortality as a primary endpoint, and instead target specific diseases or conditions. So, for example, although we may learn that hand washing reduces diarrhea, we cannot be sure what impact it ultimately has on mortality unless we know the exact relation between changes in diarrhea incidence and changes in child deaths.[19]

The second available body of evidence derives from household surveys. These are far less costly than trials to implement, and they can collect substantial information for analysis. This gives them the advantage that they can be large enough to examine mortality outcomes with sufficient statistical power to capture important relationships. However, since we can never be confident that these relationships are causal rather than spurious or biased, the results need to be treated with caution.

Given the limited evidence base, as discussed in the next two sections below, there will always be major uncertainties surrounding the impact of any potential package of measures. Should we be willing to risk embarking on policies that have only phase 2 levels of evidence, and arguably, there might well be a 20-60% chance that we are

[19] For example, Luby et al (2005) found hand-washing with soap reduced diarrhea incidence in Karachi slums. Despite this finding, there was no clear trend towards lower mortality in the intervention arm of the trial. The trial was not powered to measure mortality, and this was not a primary endpoint. If we are designing packages to reduce child deaths, and not just child morbidity, we need to assume that the reduced diarrhea will actually lead to lower deaths. However, there is no evidence which would permit us to define the relation between the lowered diarrhea incidence and mortality. The trials examining the impact of impregnated bednets were more convincing since they were designed with all-cause mortality as the primary endpoint (Keiser et. al. (2005)).

wrongly concluding they are effective when they are not? Poole (1970) argues that, when the impact of alternative policies is uncertain, the optimal response is to prefer policies for which we have greatest certainty. This argument suggests we should generally focus active policy on areas where we have evidence from well-designed RCTs wherever possible.

We believe the available evidence points us in a clear direction, which we explain below, but it would be dangerous to assume we can succeed without more experimentation and rigorous analysis. Well designed trials which examine the impact of packages of health interventions, in combination with creative secondary analysis based on techniques used in randomised experiments, would be an important addition to the evidence base. We stress: there really is not yet sufficient evidence to give us sufficient confidence regarding how to sharply reduce child deaths in pockets of poverty.

V. Clinical Evidence: Twenty-three Measures that Could Dramatically Reduce Child Deaths

The Bellagio Child Survival Group, a team of leading medical experts, was assembled by *The Lancet* in 2003 in order to assemble and judge current evidence regarding how to reduce the 10 million annual child deaths in low income countries. Figure 2 shows their main findings (Bryce et al (2003 and 2005), Darmstadt et. al. (2005)). They concluded that 90% coverage of 23 simple measures could reduce child deaths by 63%, saving six million lives per year. The three single largest measures (oral rehydration therapy, breast-feeding, and insecticide treated bed nets) can all be conducted at home, with little or no cost, and they would prevent 35% of child deaths. They

recommended a public outreach program be created, with frequent home visits by nurses, to ensure that children and pregnant mothers received the remaining interventions. At first glance it would seem reasonable to simply take these measures and implement them in "pockets of poverty" to improve child health. However, before doing so several issues need to be considered.

First, while the clinical evidence derives from single experiments, bringing down child deaths needs the implementation of a package of measures. Trials with single interventions cannot tell us how the same interventions would work in a package. For example, consider the relative importance of bednets and anti-malarials in Figure 2. There is very good evidence that use of insecticide treated bednets reduces all-cause mortality for children in moderate and high malaria incidence regions of Africa.[20] There is also very good evidence, e.g. see Mutabingwa et al (2005), that timely use of artemisinin-based therapies resolve virtually all malaria in Africa, and with second line treatments nearly all malaria deaths could be prevented. Therefore bed nets and drug therapy are potential substitutes, and if you could manage to cover the population with good clinical services and early malaria recognition, you may find that bed nets have little additional impact.[21] You would also find that the reservoir of malaria declines, so that eventually malaria disappears from the population if it is treated quickly and fully.

Since there are few well-designed trials that examine the impact of a package of measures in order to understand such interactions, the Bellagio team had to make guesses

[20] There is also a strong case for distributing bed nets free of charge (e.g., Cohen and Dupas 2007).
[21] The issue is of course far more complicated. The WHO recommends all cases of fever be treated as presumptive malaria. This may increase resistance and can be costly in regions where non-malarial fevers are common. The use of impregnated bed nets could reduce the frequency and cost of treatments and may also reduce development of resistance if the reduced incidence of disease slows resistance. Some of these complications could be better understood or modelled if we ran trials with packages of interventions and compared outcomes of alternative packages where we excluded specific ingredients.

as to the relative importance of bednets and antimalarials when deriving Figure 2. They had to also do this for many other interventions that could be complements or substitutes. In fact, there is very little reliable evidence to guide them, apart from personal experience and guesswork.

A second issue that needs to be addressed ahead of practical implementation is cost effectiveness. The Bellagio group did not explicitly consider cost effectiveness, but rather limited themselves to choosing major interventions for which there was evidence that a substantial number of lives could be saved.[22] If we could estimate costs, and the interactions between different interventions, we could theoretically solve an optimization problem to determine the best package of measures subject to a budget constraint. This could well lead to measures other than those discussed by the Bellagio group – for example we might remove substitutes but aim for greater coverage of complements.

In practice, the cost effectiveness of these interventions is a relatively minor issue. Each of these interventions is fairly inexpensive on its own, while the bulk of any health budget will be made up of quasi-fixed costs such as personnel and transportation costs for outreach and general implementation.[23] The only expensive intervention would be water and sanitation infrastructure. The Bellagio group estimated that water and sanitation would reduce child deaths by just 3% and yet cost 16% of the overall budget for the program (Bryce et. al 2005). If we took into account cost effectiveness, we may prefer to

[22] Bryce et. al. (2005) calculated the cost of implementing the 23 recommended measures with universal coverage in 42 countries responsible for 90% of deaths at $5.1bn per year in 2002 dollars.

[23] For example, the spring 2008 cost of a package of Oral Rehydration Salts (ORS) is $0.10. A single child's course of the anti-bacterial co-trimoxazole is $0.50, and a child's course of the anti-malarial coartim is $0.90. If chloroquine can be used instead of coartem, the cost falls to $0.20. These are the three main treatments that children would need from age one month to five years. They are likely to have 3-4 bouts of symptoms of each disease during one year. The need for treatment however will depend on the course of the disease. If we assume two treatments for each disease per year, the cost of providing drugs to all children in the low income countries studied by the Bellagio group would be just $0.6bn per year.

remove water and sanitation infrastructure in order to increase spending on areas related to hygiene and behaviour change which could be cheaper and as effective (Luby et. al (2005), Curtis et. al. (2003)).

The final issue is the one where we have the greatest concern. The Bellagio group recommended achieving 90% coverage of these interventions through public outreach programs in communities. They called for $5.1bn support from national governments to hire nurses, provide them with transportation, and arrange frequent community visits to households with children. This is surprising because outreach programs have been tried in many regions of the world, including many extremely poor regions, and very often failed. The Bellagio group made this recommendation without referral to well-designed studies that would back up their assertion that this could work. Nor did they discuss the constraints that today prevent such programs from being successful.

VI. Survey Evidence on How to Reduce Child Deaths

The measures listed in Figure 2 are not breakthrough science, and nor are they a hard and fast recipe. There are many nations and communities in very poor regions which managed to substantially reduce child mortality long before the Bellagio study was completed, and before the clinical measures listed there were ever tested.[24] The Bellagio study group confirms there are no "missing drugs", and we can dramatically reduce child mortality at low cost with existing technologies. However, there is one major piece

[24] The former Soviet Union, Cuba, the Indian state of Kerala, Costa Rica, Vietnam and China are all well-known cases of good child health despite relatively low incomes (and pockets of poverty). There are also smaller community-based projects, as in Arole and Arole (1994), which appear to have achieved impressive results despite extreme poverty, although no well-designed studies have verified this achievement.

missing: we still do not know how to ensure populations get these interventions in the sustained manner (over a decade or more) necessary to dramatically reduce child deaths.

In this section we focus on two key issues. First, what should be the mechanism for ensuring that children get the treatments listed above? Second, since it is frequently argued we need to build up the public health sector to lower mortality, is there evidence suggesting we should favour either the public or private sector?

Figure 3 outlines the general stages that children pass through when they eventually die from disease. The first stage is to be exposed to a risk, such as a virus or bacterial infection, or possibly an unsafe practice during birth. If the risk manifests itself in disease, some action by people close to the child will be taken. This is usually the parents, and it could mean the child is given a medicine or taken to a clinic; or perhaps nothing at all happens. If the child is taken to a clinic, then he/she can either be treated effectively or not effectively. In order to die, a child generally passes through each of these steps, so a child death can be thought of as the conditional probability of passing through each step without resolving the disease. To reduce the probability of death we can target each stage.

There have been numerous studies using survey data that have attempted to examine the relative importance of different interventions, household characteristics, and other factors that impact child deaths. One of the richest sources of information is the Demographic Health Survey Database (DHS). This survey includes answers to questionnaires from women of fertile age, household heads, and village leaders, and records birth and child survival histories, education levels, wealth measures, and other household and village indicators. The DHS also records recent disease symptoms of

children that match WHO criteria for suspected malaria, pneumonia and diarrhea. These are the three main causes of post-neonatal child deaths. The combined survey data covers 278,000 child survival records and 20,000 clusters.

Boone and Zhan (2006) constructed indexes to proxy for treatment seeking behaviour, morbidity levels, wealth, education, distance to clinics and access to safe water and sanitation. Their "treatment seeking" indicator was constructed using principal component analysis across a range of measures, which demonstrate whether a mother tended to seek modern treatments for her child's health but which were not directly correlated with survival of the child. These measures included the mother's antenatal and delivery care, and child vaccinations near birth that protect against diseases from which there is very little or no child mortality. Boone and Zhan then specified household and community level logistic regression equations to examine if post-neonatal child deaths could be predicted by this treatment indicator and other right hand side variables. The main results from this survey are pictured in Figure 4. The figure shows the predicted impact of alternative interventions on all-cause child mortality in the 45 countries where DHS data is available. For example, if all households had access to improved sanitation and water, child mortality is predicted to fall by 3%. The small predicted decline in mortality reflects the fact that approximately half the households in the survey already had improved water and sanitation, and the general finding that this variable was insignificant with a small coefficient in the estimated regression equations.

The remaining exercises calculated here were to: (i) raise the estimated treatment indicator in each households to the average level in Egypt (Egypt had the median level of the 45 countries in the sample), (ii) raise schooling to the average level in Egypt (eight

years for boys and six years for girls), and (iii) reduce by half the morbidity levels in each survey cluster of roughly 50 households.

The findings from these survey data should be treated with caution. As opposed to well-designed randomised controlled trials, we cannot assume these relations reflect causality, and there may be confounding factors which were not adequately controlled for that could bias results. There are also problems with measurement error and other potential biases. However, the data show potentially informative correlations, and the DHS has the advantage of covering a large number of households across most of the countries and regions where we would be likely to implement policies to reduce extreme poverty.

With these caveats, the results in Figure 4 show similar findings to the Bellagio child survival group and other surveys. In particular, the impact of improved water and sanitation appears empirically small. Clasen et al (2007) completed a meta-study analysing impact from water and sanitation trials and found that while there was evidence that they reduced disease, there was no credible evidence that they reduced all-cause mortality. This evidence contrasts with Cutler and Miller (2005) who argue the advent of public water and sanitation services in the United States can explain a large reduction in population death rates around 100 years ago. It seems plausible that the much wider coverage of water and sanitation today, along with the advent of vaccines and treatments for the main causes of death from infectious disease, mean that further improvements in water and sanitation are no longer necessary or very significant to eliminate remaining deaths.

There was a very weak correlation between reported morbidity and child mortality. On average, in pockets of poverty, children suffer 1-4 bouts each of diarrhea, fever, and cough with rapid breathing each year.[25] If a large difference in morbidity rates did explain the differences in mortality across countries, then we would expect these morbidity indicators to play a more important role explaining cross-country differences in deaths. We take this weak correlation as suggesting appropriate treatment for sick children is more important than preventing children from becoming ill in low income countries. However, since the data on morbidity may be biased due to mother's recall (Manesh et al, 2008), and the prevalence of disease reflects only the previous two weeks from the day of the survey, rather than the history over which the mother's child survival data is taken, measurement error and other sources of bias could be driving this relationship.[26]

Figure 4 also illustrates that the "treatment" indicator used in Boone and Zhan proved highly significant, implying that children living in households where mothers sought out antenatal care and early childhood vaccines were far less likely to die in the post-neonatal stage, despite the fact that these factors have little direct impact on post neonatal survival odds. This finding is not surprising – it is consistent with the Bellagio study since both imply that increased coverage of modern treatments will substantially reduce child deaths.[27]

[25] These three sets of symptoms are considered sufficient for presumptive treatment for diarrhea (using ORS), malaria (coartem or other anti-malarial) and pneumonia (co-trimoxazole) according to World Health Organization (2006) Integrated Management of Childhood Illness (IMCI) guidelines.
[26] For example, the strong role of parent's education may reflect their ability to reduce disease prevalence, so the impact of lower morbidity may be better captured in the regressions through the parent's education.
[27] The treatment indicator was calculated using the first principle component from indicators of antenatal care, place of delivery and BCG vaccinations. None of these should directly impact whether a child survives if they survived to one month of age. The dependent variable in these regressions is survival from one month of age to five years of age.

However, there is one major difference between the survey findings and the Bellagio study. A common finding in most studies using survey data is that parental education is an important predictor for child survival. In Boone and Zhan, similar to other studies, combined maternal and paternal years of schooling was a highly significant and empirically important predictor for survival outcomes. Since Boone and Zhan controlled for wealth in their regressions, this probably reflects the greater health knowledge of educated parents. More educated parents are probably better able to treat disease at home, and more likely to seek out care in clinics and hospitals as needed.

The Bellagio Study group made no explicit mention of improving parents' education or their health knowledge as an important intervention. One reason they left this out is that, at the time of their study, they did not find credible evidence that raising a parent's health knowledge, or providing general education to parents, impacted child survival. However, a further reason is that health professionals generally prefer to separate delivery from interventions, with the implicit or explicit assumption that public health services will find means to provide interventions. This is reasonable in certain cases, but often the delivery method is closely bundled with the type of treatment or intervention. For example, when choosing between a one-time vaccine and treatment for an infectious disease, there are good reasons to think we would be far more successful with vaccines. Vaccination campaigns have been carried out regularly in regions which otherwise receive virtually no health care. Medical trials cannot indicate whether your implementation method is feasible unless they are conducted with that purpose in mind in the regions which are relevant.

The risk that the Bellagio team takes by not including parent's education in a package of measures may actually be very large. Recent neurological studies have confirmed the very intuitive understanding that a love for one's child is probably a highly potent mechanism for ensuring child survival. In a functional MRI scanning study, mothers shown pictures of their own children demonstrated greater neurological activity in reward zones of the brain compared to mothers shown pictures of children that they knew, but were not their own (Bartels and Zeki, 2004). This natural biological link arguably makes parents the single best agent to ensure their child survives during her first few risky years of life. Since parents make the crucial first decisions regarding antenatal care, location and type of delivery, recognition of symptoms of disease, and the choice of treatment, they are potentially a major agent for change, or a factor that can prevent change, when targeting child mortality reduction. Indeed, if we embarked on a program without sufficiently working with parents to change behaviour, we might not succeed despite making available all 23 interventions highlighted in the Bellagio study.

Despite the logical importance of this channel and the potential for it to be a necessary ingredient in a package to reduce child deaths, there is surprisingly little evidence from randomised controlled trials measuring the potential for better educating parents to limit child deaths. One recent study does suggest the potential is large. Manandhar et al. (2005) compared the impact of organizing women's participatory discussion groups in rural regions of Nepal, with control regions where no discussion groups were introduced. The groups discussed safe delivery, pregnancy and care of the neonate. They found that neonatal deaths in the intervention regions were 28% lower than in the control region after two years.

There is good evidence that parents are not very knowledgeable about health. For example, there is a great deal of variation in maternal knowledge regarding the use of fluids to prevent diarrhea. Children suffering from diarrhea can quickly die from dehydration, so it is important to provide additional fluids in order to prevent death. Figure 5 shows that approximately 1/3 of Indian mothers believe fluids should be withdrawn from a child with diarrhea. This indicator is highly correlated with actual child mortality rates in each state.

These problems are similar throughout Africa although they vary by disease. In a recent health knowledge survey we conducted in rural Guinea-Bissau, only 16% of 600 parents interviewed had ever heard of pneumonia, although pneumonia is probably responsible for 1/3 of postneonatal child deaths in rural Guinea-Bissau. This contrasts sharply with a survey at a clinic in Nigeria where 61% of women attending the clinic knew that difficulty breathing is a symptom of pneumonia (Uwaezuoke 2002).

It may be unlikely, but it is plausible parental education could be both necessary and sufficient to dramatically reduce child deaths. If we examine Figure 2, approximately 35% of the gains are due to factors which can be implemented at home, such as breastfeeding, using impregnated bed nets, and treating diarrhea with ORS. Such measures have no financial burden on the public sector and no need for clinics or pharmacies. Behavior change alone should permit these gains.

The remaining interventions can be divided into those that can be implemented with simple drugs in the community or at home, and those which require a visit to a clinic or a health professional. In reality, only a small fraction of the lives saved actually require a clinic, and these clinics do not need to be in the public sector. Our next

question is therefore whether there is clear evidence favoring public or private clinics when trying to improve health in pockets of poverty.

VII. Public versus Private Delivery of Health Services

There is a general bias in the health profession to promote public care over private systems in low income countries. The WHO's *Commission on Macroeconomics and Health* argued that a large public sector build-out was needed because the private sector could not be relied on to provide equitable, affordable services sufficient to reduce mortality. However, they did not present empirical evidence to justify this argument.[28]

The public sector today provides inadequate services, suffers from corruption, and better serves wealthier political elites than the poor. It is an empirical question as to whether public or private services will ultimately perform better to help pockets of poverty, and this undoubtedly also depends upon the specific local circumstances, so there may be no right solution for any region (Bustreo, F., A. Harding, et al. (2003)).

There is substantial variation in the size of the private sector in the 45 countries covered by DHS, so it is possible to use DHS data to compare basic performance measures of public and private systems. In Figure 6 we have plotted post-neonatal child mortality rates against the share of the private sector in child health services.[29] The data

[28] World Health Organization (2001). The Bellagio Child Survival Study Group was less decisive, although their general assumption was that the public sector would be responsible for most services, while the private sector "should be involved whenever possible, especially in monitoring and ensuring quality and equity" (Bryce, J., S. el Arifeen, et al. 2003).

[29] DHS reports where mothers sought out care when there child was most recently sick with symptoms of diarrhea, fever, or cough with rapid breathing. We have used these reports to calculate the percentage of visits that were in the public or private sector. We only include visits to clinics in these calculations. Many parents seek advice at pharmacies in low income countries, and these are usually private. If we included pharmacies in the calculations, the share of the private sector would rise substantially for each country, although the qualitative relationship between mortality and size of the public sector would not change.

show there is no clear relationship between public or non-public management of the health system, and mortality outcomes for these children.[30]

In fact, the real conclusion from Figure 6 is that both public and private systems are consistent with very low child mortality. For example, today most child care is sought in the private sector in Kerala, Egypt, Indonesia and Vietnam, and each of these countries and states have achieved very low child mortality. On the other hand Cuba, many states in the former Soviet Union, and regions in Latin America generally rely on the public sector and have achieved low mortality. In China, low child mortality has been achieved in a system where parents pay user fees equal to 75% of total costs.

These findings suggest there are multiple systemic routes to low child deaths. In regions with stable government and political elites that are willing to spend on the poor, it may make sense to channel interventions through the public sector. In other regions it may be preferable to develop private supply, and hope that parental education and experience will lead to substantial increases in demand for health services. In order to reduce risks of supply disruptions we should probably aim to buttress both public and private services. Loevinsohn and Harding (2005) provide impressive evidence that public sector contracting of services can be a highly successful means to deliver services to children while avoiding the pitfalls of public sector implementation – although these programs do require public sector willingness and ability to provide financing.

[30] BZ use similar data and show that inequality of healthcare services is unrelated to the public or non-public management of care.

VIII. The Macroeconomic Consequences of Improved Health

While the evidence is not yet fully satisfactory, there is good reason to believe dramatic reductions in child deaths are achievable if an appropriate package of measures is introduced in "pockets of poverty". The measures discussed in Figure 1 can be rapidly introduced, so over a period of five years we could well see much greater than 50% reductions in child deaths. In our view, this is attainable even in the poorest parts of Asia or Africa today.

However, if our goal is to design a package of measures to reduce extreme poverty, or at least make sure we do not exacerbate poverty, then we need to consider implications of rapid improvements in health on poverty. Here the evidence is mixed. There is obviously a considerable amount of microeconomic evidence that suggests better health leads to better economic outcomes for individuals, for example in the form of higher wages.[31] So there is no doubt that improving health for people born into pockets of poverty would make them better off relative to those not in "pockets" in the same country.[32] For example, Soares (2007) finds that health improvements across regions Brazil had a welfare value roughly one-third that of the growth in income per capita, 1970-2000, and contributed about a quarter of the improvement in welfare over this period.

Strong claims for the effects of health on income have been made, based on cross-country correlations (e.g., Bloom and Canning 2005, Bloom and Sachs, 1998, World Health Organization, 2001, Lorentzon, McMillan and Wacziarg 2008). Plausible

[31] Strauss and Thomas (1998) survey the research through the late 1990s. Important recent papers include Behrman and Rosenzweig (2004), Bleakley (2003 and 2007), Miguel and Kremer (2004), and Schultz (2002).

[32] In addition, of course, we should value health improvements for their own sake and this may outweigh any income considerations (Becker, Philipson, and Soares 2005).

mechanisms include positive effects from reducing ill-health on productivity directly, as well as through increasing education and savings.[33] Weil (2007) finds that when the microeconomic evidence is aggregated, health can explain about 25% of the variation in the cross-sectional log income per worker across countries (about the same as education and more than physical capital).

None of this speaks to causation, of course, and it is hard to find well identified experiments with plausibly exogenous increases in collective health (of the kind and scale that would happen if we could improve health in pockets of poverty). Nevertheless, some insight may be drawn from the from the post-World War II "international epidemiological transition", in which new drugs, chemicals and delivery mechanisms spread quickly from a few industrial countries around the world (Acemoglu and Johnson, 2007). This country-level evidence indicates that there were quick and positive effects on life expectancy and on ill-health.[34] But the effects on GDP per capita or per working age population were much slower to emerge – taking at least 40 years and, in some cases, even longer. The proximate cause of this delayed effect was the increase in population that followed the health improvements; there was a fertility transition, but only over time. To put this phenomenon into perspective, Figures 7 and 8 show that while there has been considerable convergence in life expectancy among countries that were, circa 1940, initially poor, middle income and rich, there has been much less convergence in income per capita.[35]

[33] Case and Paxson (2008) find that height is associated with cognitive ability. This suggests, although it does not prove, that health improvements which increase height could have direct effects on cognition, and presumably therefore on productivity.
[34] Data limitations mean that we do not (yet) have a clear picture of what happened to health in Africa during this period.
[35] As Deaton (2004) points out, the convergence in life expectancy in broad terms suggests there are more gains to be had if globalization can bring better health care to world's least healthy.

The effects of health improvements in this major post-war episode can arguably be identified by measuring the initial burden of disease in terms of deaths from specific infectious diseases and measuring changes in "predicted mortality," i.e., how much mortality should have fallen if the new medical technologies were adopted.[36] Figure 9 shows that over 1940-1980 there was a strong negative correlation between changes in predicted mortality and changes in log life expectancy, i.e., countries that had a larger initial burden of infectious disease also had a larger increase in life expectancy. Figure 10 shows a similar negative relationship between predicted mortality and log population – places with more to gain (and presumably more gains) from the reduction in infectious diseases experienced larger increases in population. And Figure 11 illustrates the problem: while the change in GDP was also higher for countries experiencing bigger declines in predicted mortality, the effect on GDP was not large enough – over this time period – to make up for the increase in population. In short, there is no evidence that these dramatic and rapid improvements in health led to higher income per capita (or even per worker).

These empirical findings are consistent with neoclassical theory, if there is an important fixed factor of production (e.g., land). Ashraf, Lester and Weil (2008) calibrate a model with exogenous health improvements on income, and the available evidence on

[36] Acemoglu and Johnson (2007) have data on 15 diseases, but most of the action comes from reductions in deaths from tuberculosis, pneumonia, and – for some countries – malaria. The increase in life expectancy for many countries, from around 40 to around 60, is similar to what could be achieved in pockets of poverty today. However, the pattern of disease burden in today's developing countries is of course somewhat different, with child mortality being higher relative to adult mortality than was the case generally in 1940. We would also stress that the effect of reducing HIV-AIDS in particular may have quite different (and more positive) consequences for income per capita.

key parameters suggests health improvements are likely to produce a population increase that can substantially delay the positive effects on income at the macro level.[37]

None of this recent work suggests that health improvements should not be pursued – they are surely important enough in their own right, irrespective of the effect on income. But the question is whether health (and education) interventions will, by themselves, lift people out of poverty. What are the other plausible interventions that could address this issue? Is it about the availability of (micro) finance? Is it about the availability of subsidized fertilizer? Is it about the dissemination of new and (hopefully) "appropriate" technologies – perhaps the arrival of cell phones? Is it roads? All of these, and more, have their proponents. Not only is this an open issue, but we seem to be some distance from even designing interventions that would provide meaningful tests and guidance on how to move forward.[38]

In a sense, the broad "Millennium Village" package advocated by Jeff Sachs is a sensible approach.[39] This is an integrated set of interventions that attempt to address many issues, including health, education and agricultural productivity directly. But from the design of this project, it is hard to know what exactly we will learn – other than (presumably) large inflows of foreign money and attention have positive effects for recipients, at least while the flows continue. Still, a broad-based approach along these

[37] Still, their calibration of the fertility effect is less than what Acemoglu and Johnson find in the data. Either something is missing in the theory or the postwar experience in developing countries had some features that are not in the model. Acemoglu, Fergusson, and Johnson (2008) suggest that part of the "missing link" may be that population increases led to more social conflict and violence as there was greater competition for resources. In turn, this is linked to who gets access to health care services in general and in the face of shocks (e.g., the arrival of HIV-AIDS).

[38] One approach would be to encourage birth control, i.e., to try to accelerate the fertility transition. There is obviously a great deal of activity in this area (e.g., see http://www.hewlett.org/Programs/Population/). But relatively little is yet known about how to make this effective in pockets of poverty, other than through improving the female education. Lant Pritchett (2008) argues for a different approach focussing on letting labor move more freely across borders, and encouraging international migration would remove the fixed (local) factors of production problem. But it remains to be seen if this is politically feasible.

[39] See http://www.unmillenniumproject.org/, and associated press coverage.

lines, within the "experimental" framework laid out above, could have considerable appeal.

IX. Designing programs to end pockets of poverty

The Millennium Villages project has another important flaw – it will be impossible in the future to confidently conclude whether it was a success. The project could have been designed similar to a drug trial, with villages being randomly selected from a list to decide which received support, and the outcomes of those villages compared to the non-selected villages in a pre-specified analysis plan. Such rigorous analysis would be an extremely valuable tool to understand how well it works, and to learn how to improve outcomes. If we combined the best aspects of medical trial design listed in section 2, with creative techniques for conducting secondary analysis (e.g. Miguel and Kremer's (2004)), we could potentially learn a lot about how best to reduce poverty in rural African Villages.

The authors of this paper, through Effective Intervention (http://www.effint.org/), a UK based charity, are involved in several large trials aimed at improving health and education in extremely poor regions. We have partnered with medical statisticians at the London School of Hygiene and Tropical Medicine along with local health professionals, to design and implement projects to reduce child mortality in rural regions of Africa and India. While these are aid projects, we have designed them as randomised controlled trials identical to a drug approval trial, and we are testing whether a comprehensive package of interventions, including intensive provision of community health education

and contracted out clinical services, will be sufficient to rapidly reduce child deaths.[40] The trials are being implemented in 600 villages in India and Africa, with a total population of 500,000. It will take three years to accumulate sufficient events (i.e., child deaths) in order to credibly determine the impact on overall child mortality for each trial. At the end of each trial, we will learn whether the system we have put in place is truly capable of rapidly reducing deaths within three years, and we will also learn the costs.

We are also in the planning stages for a project that will combine improved child survival with education in pockets of poverty. One endpoint for the trial will the percentage of children at age ten who are literate and numerate at a level consistent with middle income counties. We will track these children, and compare them to controls, for at least ten years after the trial. This trial will help us learn whether children living in remote impoverished regions, who learn by age ten to be literate and numerate, gain benefits from this additional education. We will also learn how to implement such projects and what can be achieved in these communities at modest cost.

Our aim is to ultimately build up a rigorously evaluated and validated package of measures which can help lift current and future generations of children out of pockets of poverty. However, much more knowledge is needed before we can understand whether this is possible, and how to achieve it.

X. Conclusion

There is enough evidence to say that we likely can rapidly reduce child deaths in pockets of poverty. Doing this effectively requires designing an appropriate package of measures and ensuring that they can be sustainably delivered to the children and pregnant

[40] The details of the study protocol are described in Boone and others (2007).

mothers that need them. This is both feasible and compelling, quite aside from any economic considerations.

A dramatic improvement in health conditions has the potential to reduce poverty. But it may also lower per capita income in these regions by raising total births or fertility or both, particularly if fixed factors of production mean that additional population creates negative "crowding" effects. Presumably, encouraging investments in human capital will help make poverty reduction more likely. But we should also be cautious regarding whether combining education with health improvements would be sufficient for poverty eradication.

Since survey data tells us health, education and poverty are very closely linked, to really end pockets of poverty we probably need to have an even broader package of measures that addresses all these areas directly. Without doubt, we need more evidence to know what can be achieved on this "grander" scale. The standard approach of medical research, in which single interventions are tested in trials, may not be ideal for gaining evidence here. Rather, packages of measures should be tested, preferably as randomised controlled trials, with clearly defined endpoints and pre-specified goals. Over the next decade, this would provide us with much better evidence regarding what works, and what can be achieved.

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Table 1

Types of Evidence		
	Goals	**Methods**
Phase 1 and pre-clinical	Safety	Very small trials
Phase 2	Preliminary evidence of efficacy Dosing Optimal procedures for implementation Secondary analysis of well-designed randomised controlled trials	Survey data Small randomised controlled trials Randomised experiments Secondary analysis of well-designed randomised controlled trials
Phase 3	Substantial evidence of efficacy and safety	Several randomised controlled trials with registered protocols which state: • Pre-specified primary and secondary endpoints • Powered appropriately • Pre-specified statistical analysis plan • Blinding and limited ability for investigators to learn results before the trial is completed

FIGURE 1

Child mortality and income

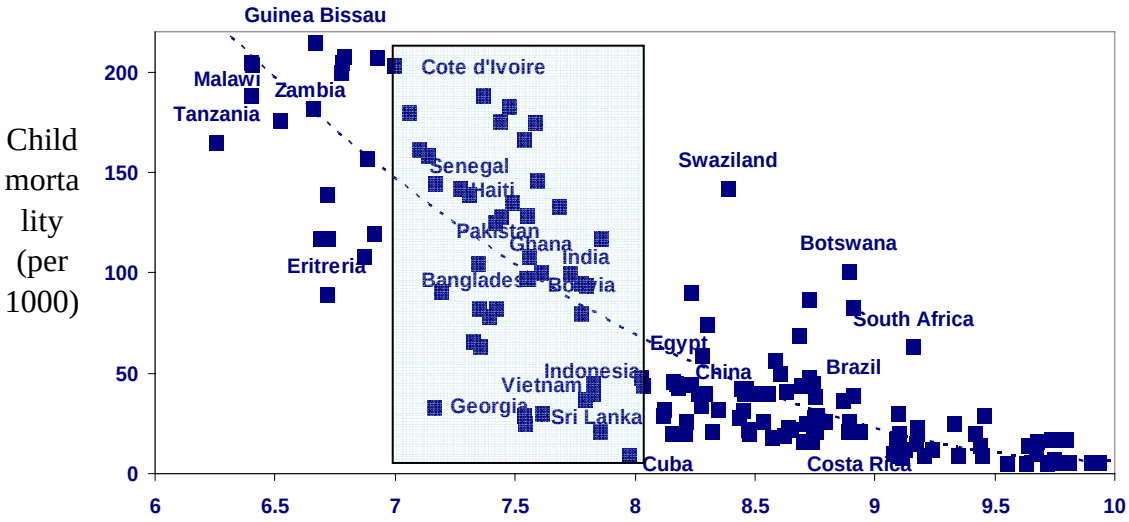


FIGURE 2
Bellagio Child Survival Group: 23 Interventions, share of total feasible mortality reduction from each intervention if implemented separately

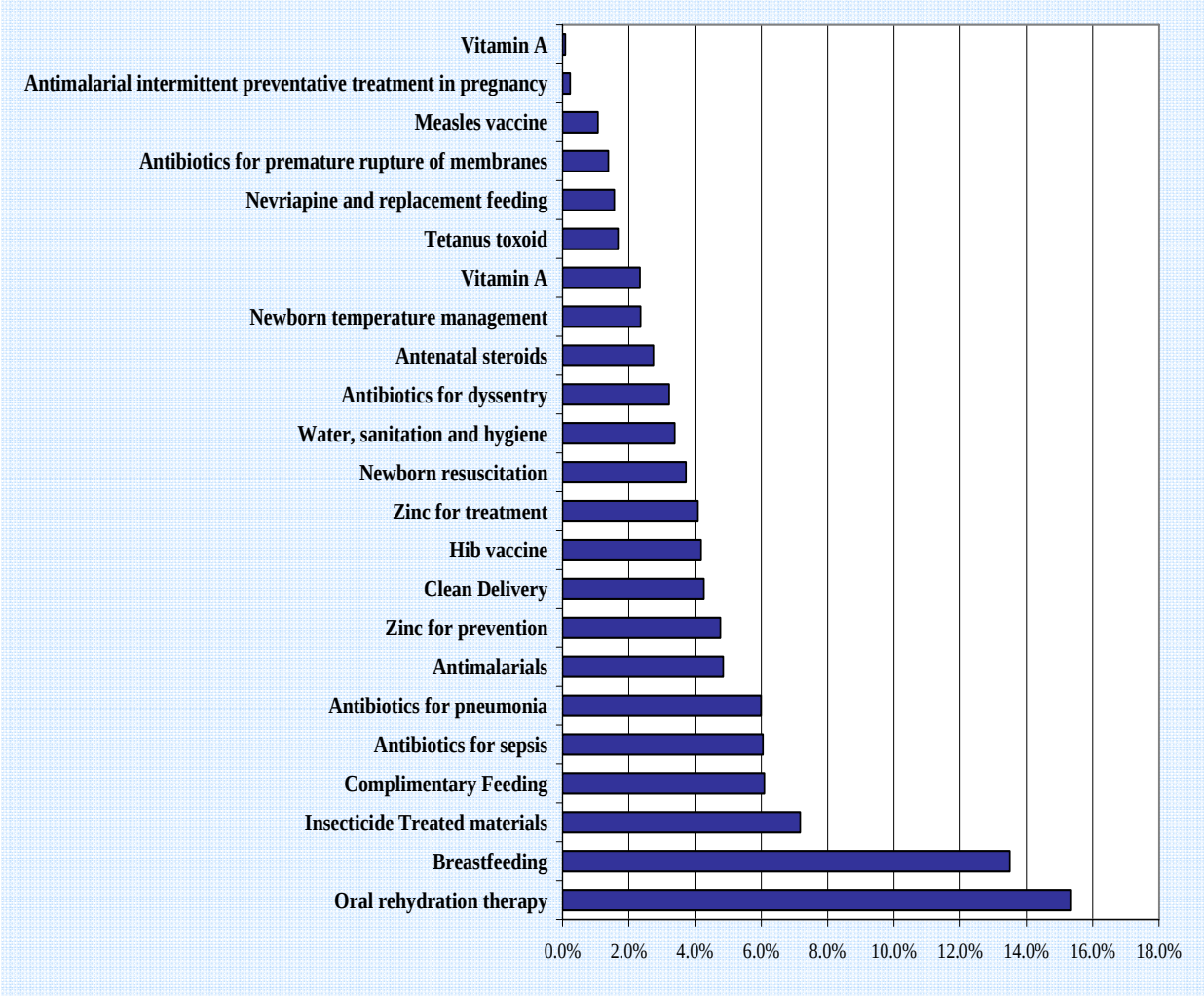


FIGURE 3

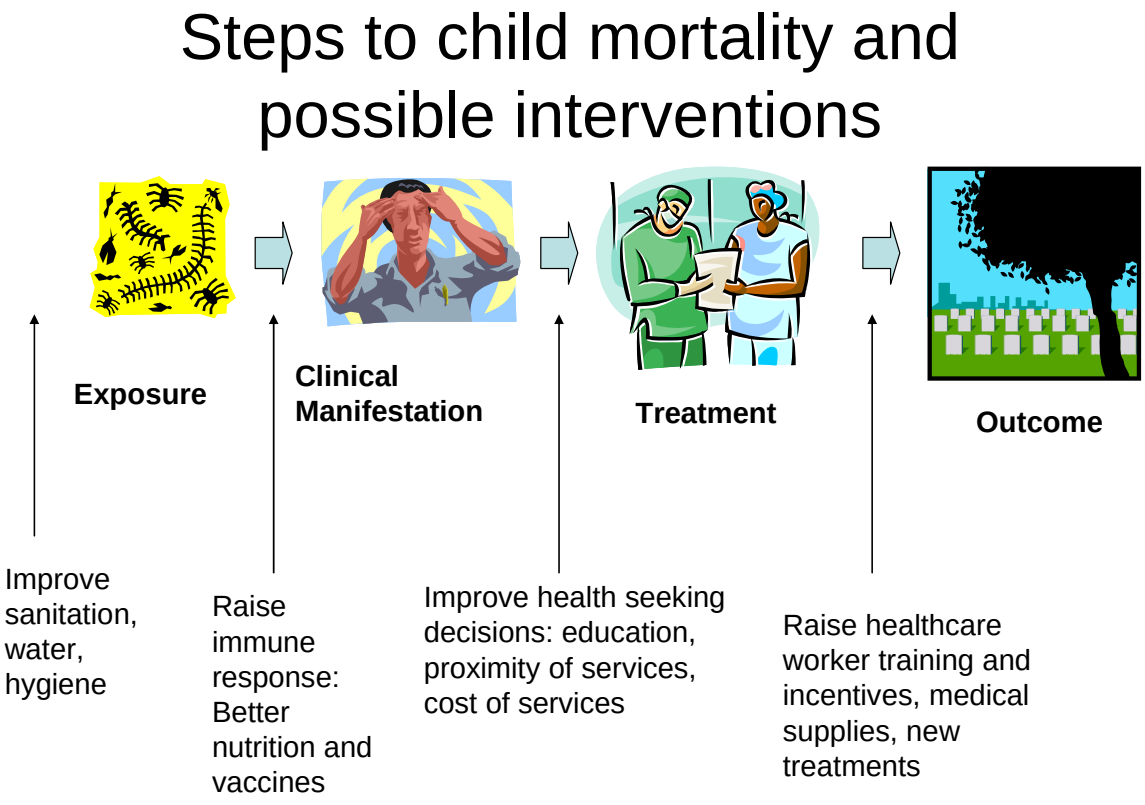

Steps to child mortality and possible interventions
Exposure
Clinical Manifestation
Treatment
Outcome
Improve sanitation, water, hygiene
Raise immune response: Better nutrition and vaccines
Improve health seeking decisions: education, proximity of services, cost of services
Raise healthcare worker training and incentives, medical supplies, new treatments

FIGURE 4
The Impact of Four Alternative Packages Aimed at Reducing Child Mortality in the Global Data

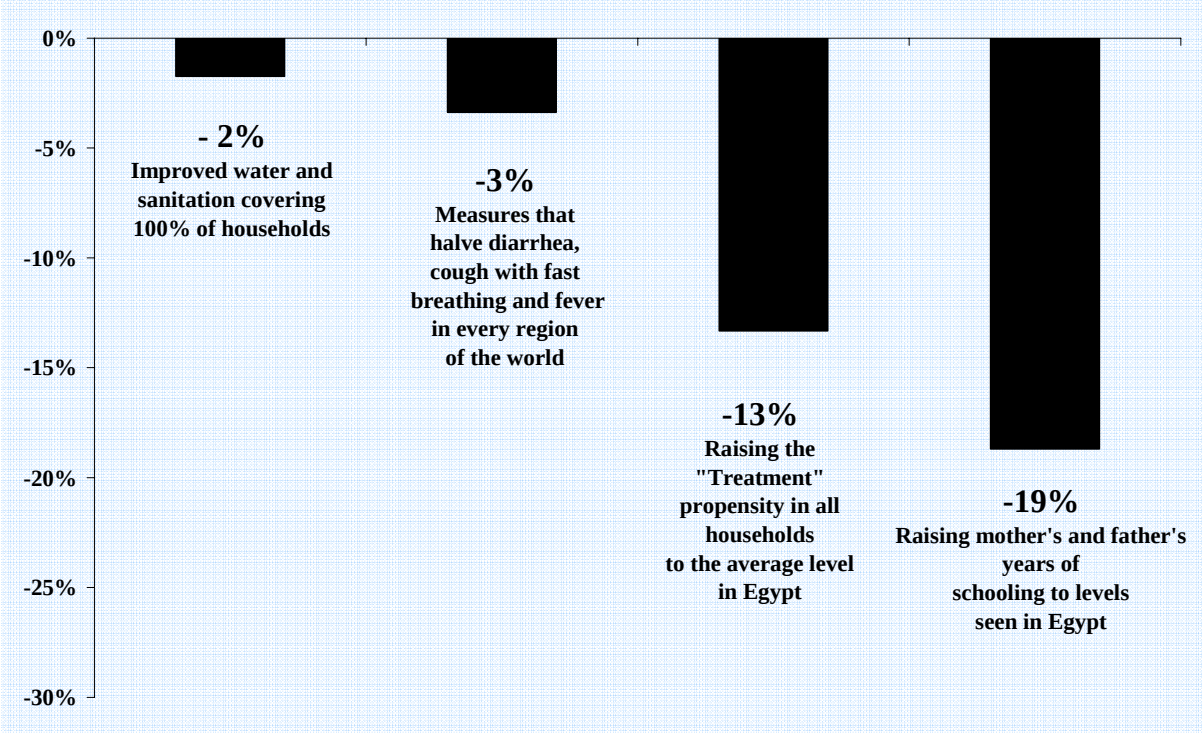


FIGURE 5

Should You Increase, Maintain, or Reduce Fluids, (Or Don't Know) For a Child With Diarrhea

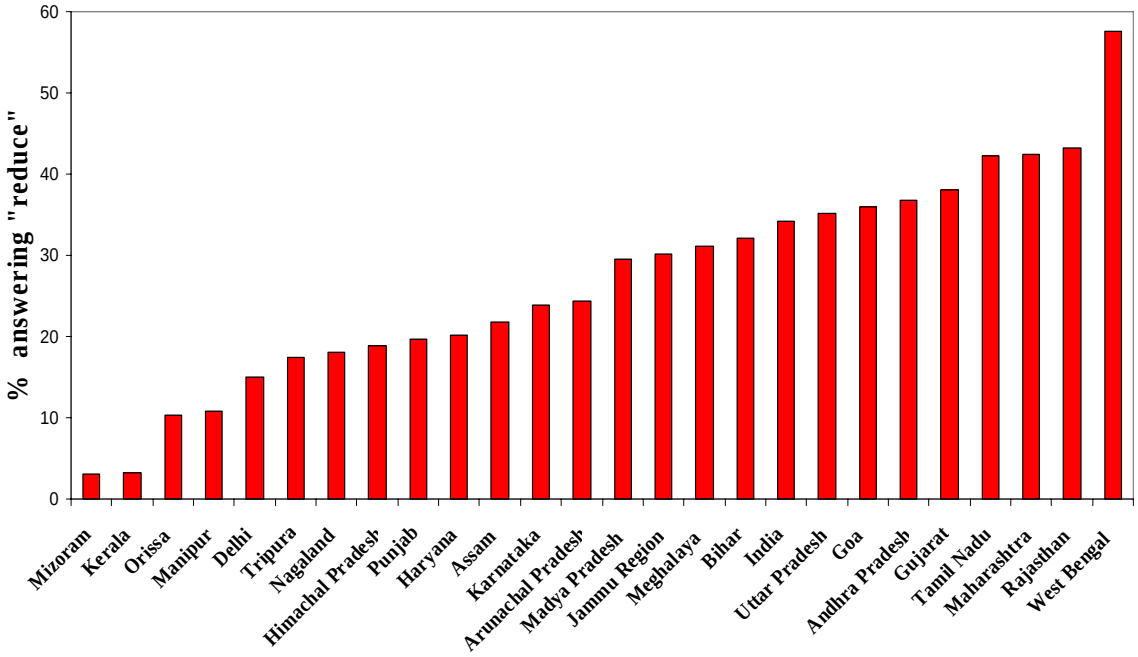


FIGURE 6
The Relation Between Post-neonatal Child Mortality and the Share of Child Health Services Sought in the Private Sector In 45 Low Income Countries

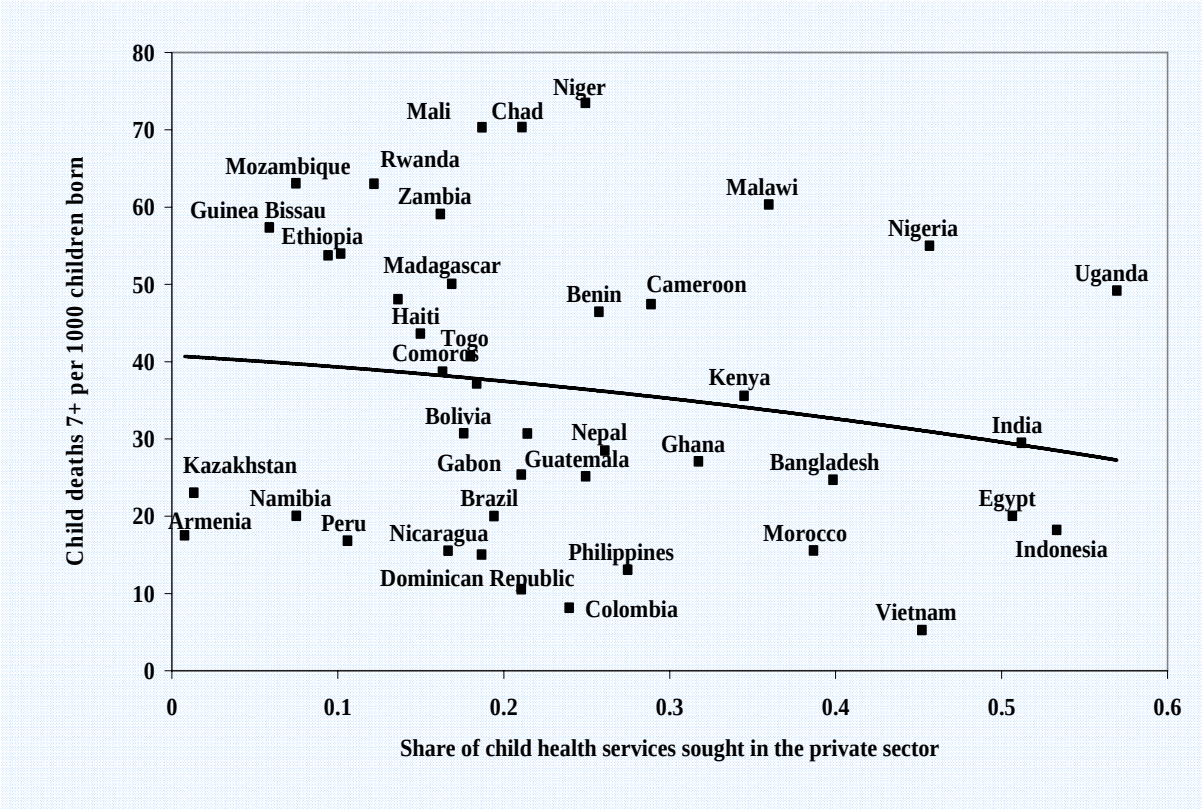


FIGURE 7

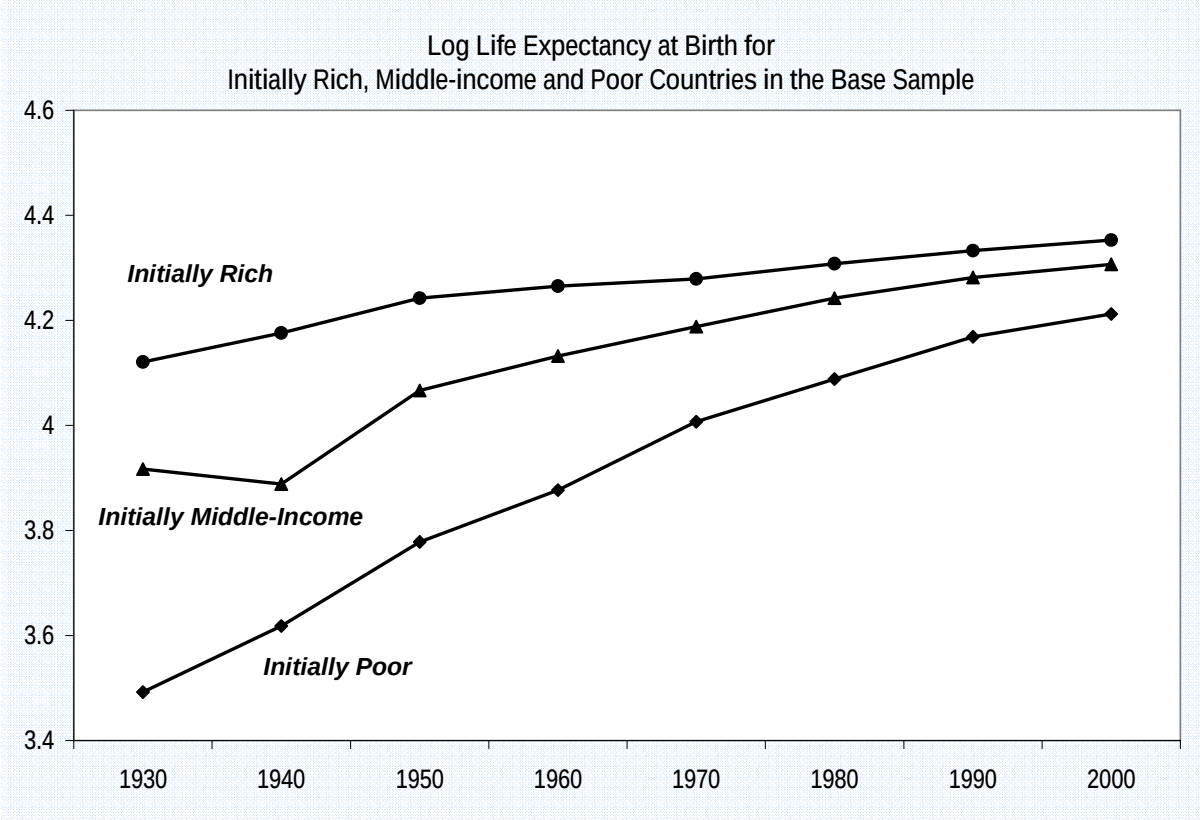

Log Life Expectancy at Birth for
Initially Rich, Middle-income and Poor Countries in the Base Sample
4.6
4.4
4.2
4
3.8
3.6
3.4
1930
1940
1950
1960
1970
1980
1990
2000
Initially Rich
Initially Middle-Income
Initially Poor

FIGURE 8

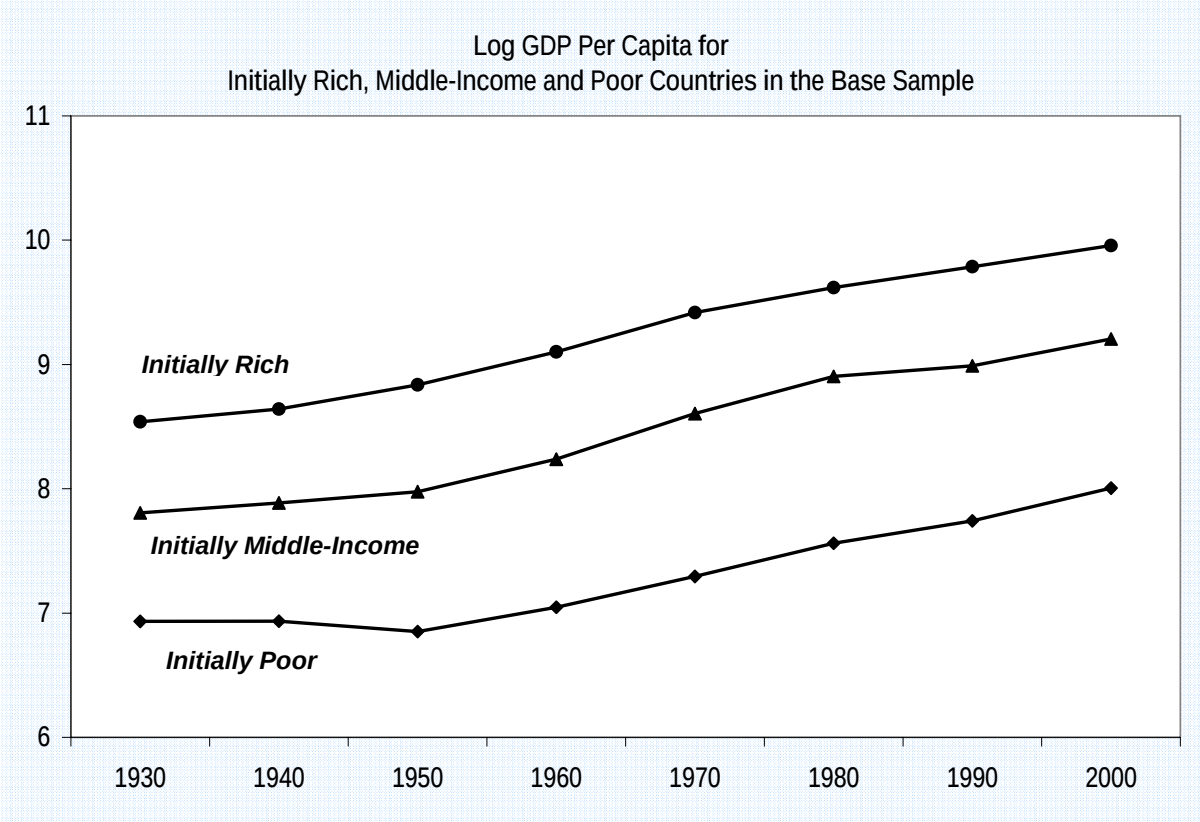

Log GDP Per Capita for
Initially Rich, Middle-Income and Poor Countries in the Base Sample
11
10
9
8
7
6
1930
1940
1950
1960
1970
1980
1990
2000
Initially Rich
Initially Middle-Income
Initially Poor

FIGURE 9

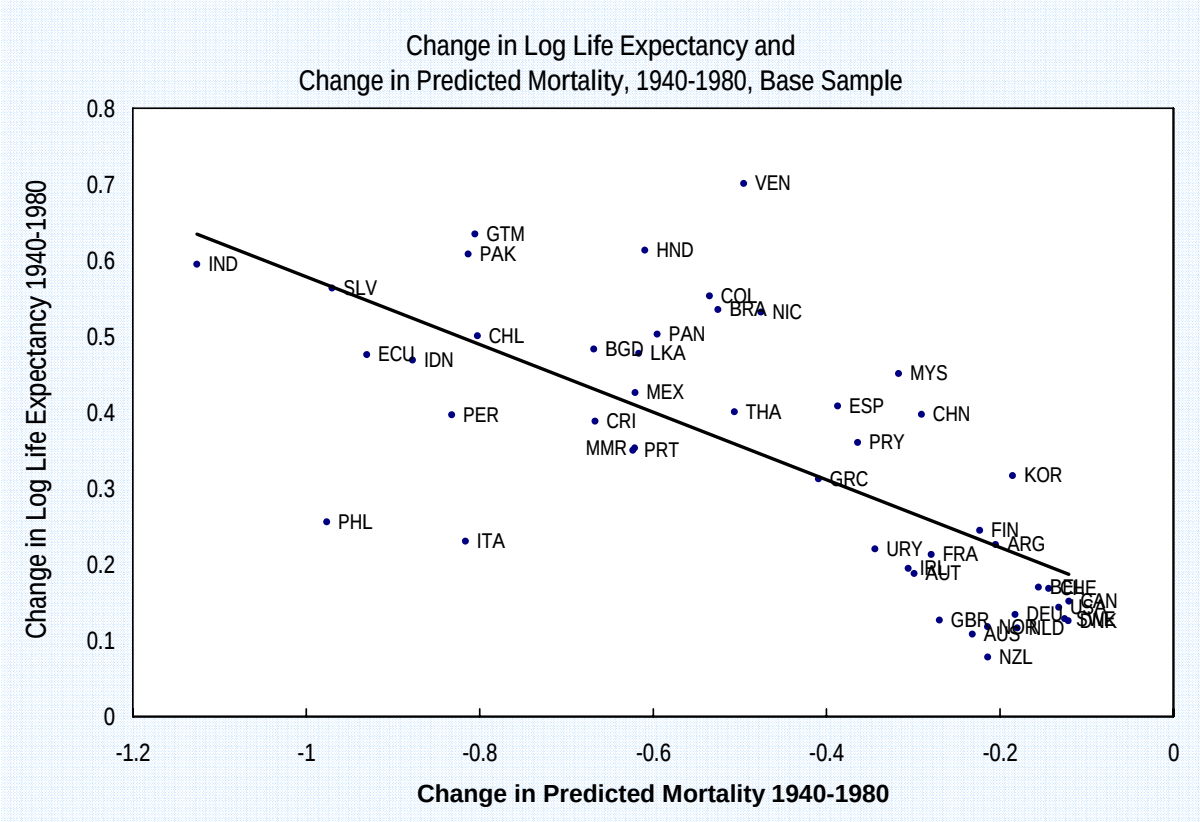
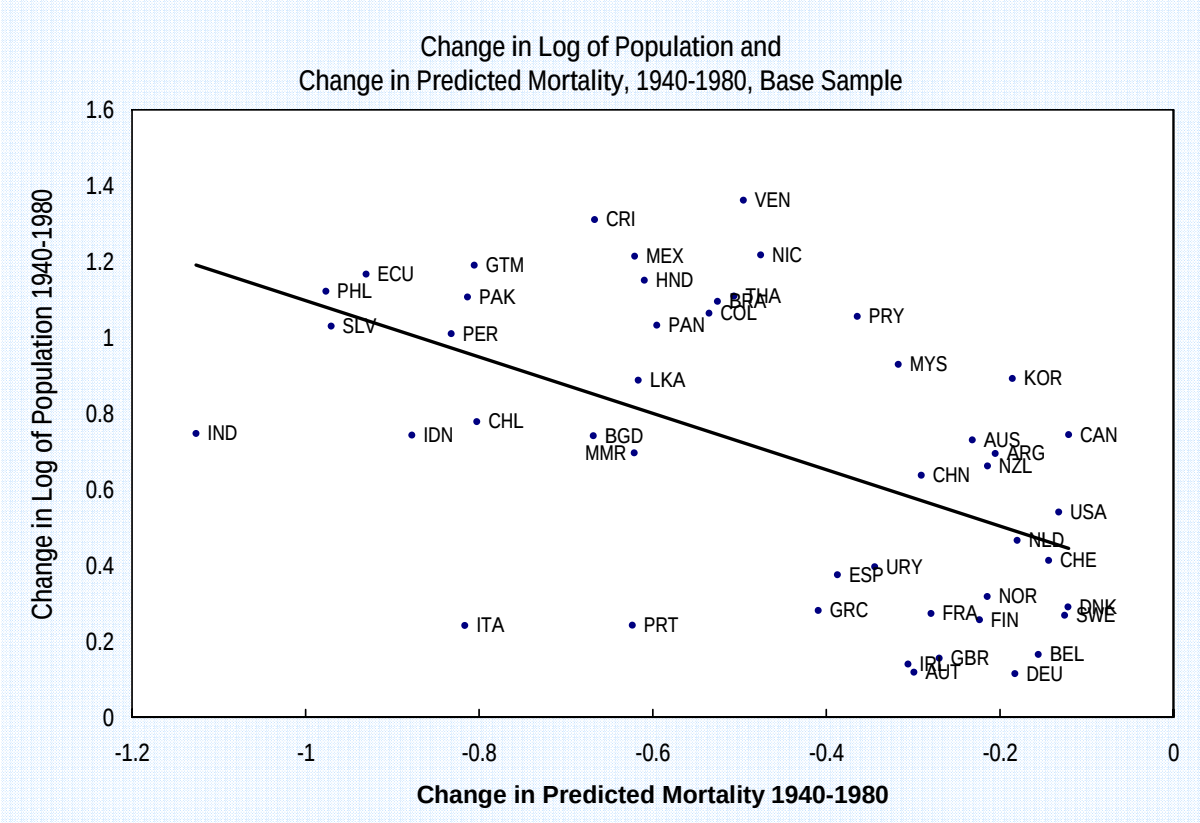

Change in Log Life Expectancy and
Change in Predicted Mortality, 1940-1980, Base Sample
Change in Log Life Expectancy 1940-1980
Change in Predicted Mortality 1940-1980
0.8
0.7
0.6
0.5
0.4
0.3
0.2
0.1
0
-1.2
-1
-0.8
-0.6
-0.4
-0.2
0
VEN
GTM
PAK
HND
IND
SLV
COL
BRA
NIC
CHL
PAN
ECU
IDN
BGD
LKA
MYS
MEX
PER
CRI
THA
ESP
CHN
MMR
PRT
PRY
GRC
KOR
PHL
ITA
FIN
ARG
URY
FRA
IRL
AUT
GBR
DEU
AUS
NLD
NZL

FIGURE 10


Change in Log of Population and
Change in Predicted Mortality, 1940-1980, Base Sample
Change in Log of Population 1940-1980
Change in Predicted Mortality 1940-1980
1.6
1.4
1.2
1
0.8
0.6
0.4
0.2
0
-1.2
-1
-0.8
-0.6
-0.4
-0.2
0
VEN
CRI
MEX
NIC
HND
GTM
ECU
PHL
PAK
SLV
PER
PAN
COL
PRY
MYS
KOR
LKA
IND
IDN
CHL
BGD
MMR
AUS
CAN
ARG
NZL
CHN
USA
NLD
CHE
URY
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NOR
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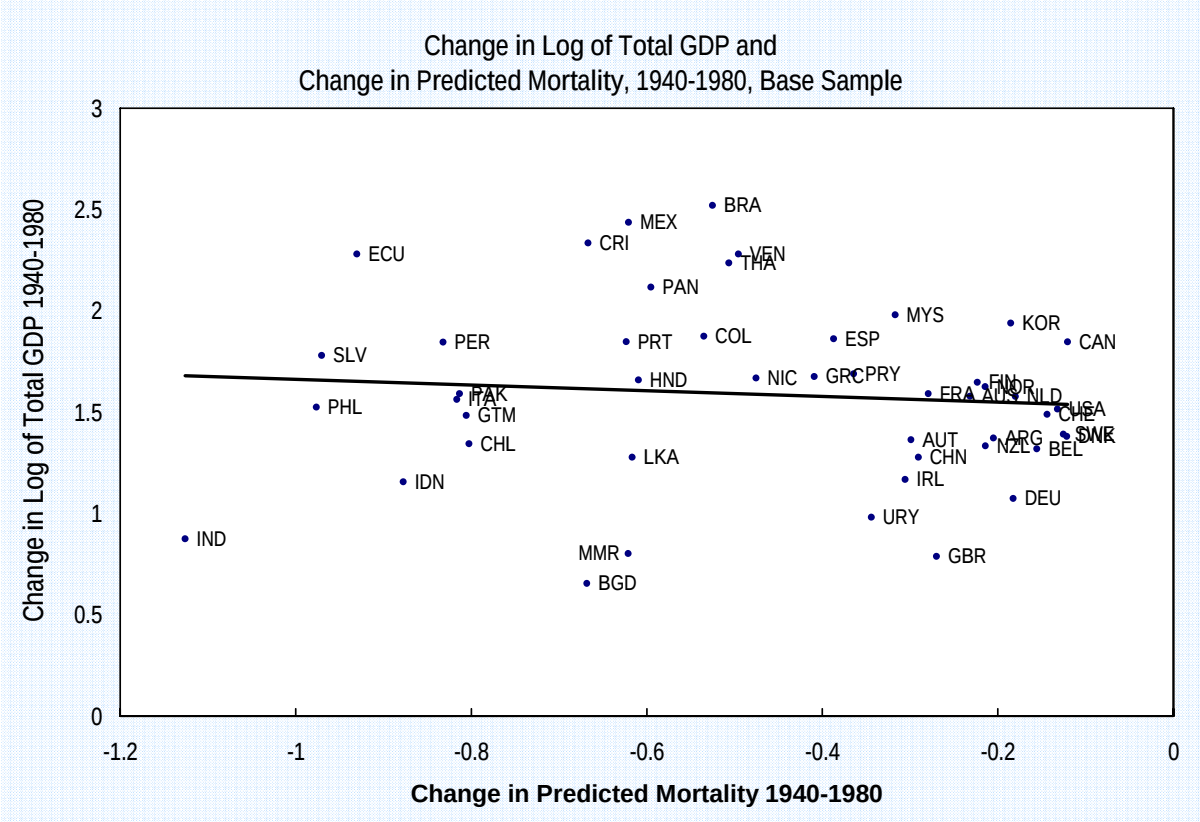
FIN
SWE
ITA
PRT
GBR
BEL
AUT
DEU

FIGURE 11


Change in Log of Total GDP and
Change in Predicted Mortality, 1940-1980, Base Sample
Change in Log of Total GDP 1940-1980
Change in Predicted Mortality 1940-1980
3
2.5
2
1.5
1
0.5
0
-1.2
-1
-0.8
-0.6
-0.4
-0.2
0
BRA
MEX
CRI
ECU
VEN
THA
PAN
MYS
KOR
CAN
SLV
PER
PRT
COL
ESP
HND
NIC
GRC
PRY
FIN
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