



CAN THE WEST SAVE AFRICA?

William Easterly



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AFRICA'S NEEDS AND WESTERN RESPONSE

Explosion of interest in "saving Africa"

The last few years have seen unprecedented attention to an attempt by Western governments to rapidly develop Africa.¹ British Prime Minister Tony Blair called at the World Economic Forum in Davos in January 2005 for "a big, big push forward" in Africa to end poverty, financed by an increase in foreign aid.² Tony Blair commissioned a Report on Africa, which released its findings in March 2005, likewise calling for a "big push." Gordon Brown and Tony Blair put the cause of ending poverty in Africa at the top of the agenda of the G-8 Summit in Gleneagles, Scotland in July 2005. In the 2005 summit at Gleneagles, Scotland, the G-8 agreed to double foreign aid to Africa, from \$25 billion a year to \$50 billion to finance the big push, as well as to forgive the African aid loans contracted during previous attempts at a "big push." Two years later, Germany again made Africa an important item on the agenda of the G-8 summit it hosted in Heiligendamm in June 2007. There, the G-8 again reiterated the promises made in 2005. Japan pledged to double its own aid to Africa in May 2008 over the next

five years.³ Most recently, the G8 Summit in Japan in July 2008 agreed: "We are firmly committed to working to fulfill our commitments on Official Development Assistance made at Gleneagles, and reaffirmed at Heiligendamm, including increasing... ODA to Africa by US\$ 25 billion a year by 2010."⁴

The goals of the Western effort are ambitious, not limited to promoting overall economic growth. A 2000 UN Summit agreed upon "Millennium Development Goals" (MDGs) for the year 2015 such as cutting poverty in half, reaching universal primary enrollment, sharply reducing mortality of infants and mothers, achieving gender equality, dramatically increasing access to clean water and other social indicators. Although this effort is worldwide, most of the MDG campaign focuses on Africa, where the shortfalls to the goals are the greatest.

The G8 also is making efforts to address civil war and "failed states" (also known as "fragile" and "post-conflict" states) in Africa, saying at the 2008 summit:

Peace and security are fundamental to states' ability to meet the needs of their people. Fragile and post-conflict states remain farthest from

reaching the MDGs. Overcoming fragility and successful recovery requires comprehensive, integrated and sustained international assistance, including peacekeeping and peacebuilding efforts where necessary, tailored to the particular context.⁵

This campaign places an emphasis on rapid transformation as opposed to gradual progress. As the Africa aid advocacy group DATA lectured the G8 in its 2008 report:

“Incrementalism will continue to help some people in Africa, but would be a disaster for most... it certainly won’t bring about the ultimate goal - help for Africa to ‘build the successful future all of us want to see’. [quote from 2005 G8 Summit Communiqué]” (DATA 2008, p. 5)

The previously obscure cause of African development has even burst into popular culture. Rock celebrity Bob Geldof assembled well-known bands for “Live 8” concerts on July 2, 2005 in nine cities around the world to lobby the G-8 leaders to “Make Poverty History” in Africa. Even movie stars got involved, with Angelina Jolie touring Kenya with Jeffrey Sachs to make an MTV video in 2005. *Vanity Fair* devoted its July 2007 celebrity-laden issue to saving Africa, with feature articles such as “Madonna’s Malawi.” In what might qualify as a surrealistic moment, the Administrator of USAID asked a staffer to summarize the policy conclusions of the *Vanity Fair* analysis for U.S. foreign aid.⁶ In the World Economic Forum in Davos in January 2008, a diverse panel of celebrities ranging from Bono to Bill Gates to Queen Rania of Jordan called for “emergency” action to drastically reduce poverty in Africa by the year 2015.

The debate on whether the West can “save Africa” revives a long-standing debate in development eco-

nomics. One side of this view sees very rapid and comprehensive social change as possible, emanating from an elite of political leaders and outside experts who can start from a blank slate in achieving development. The other side sees only gradual social change as possible (at least, gradual on average, since this side would concede there could be occasional rapid breakthroughs), emanating more from the emergent self-organizing order of many decentralized private entrepreneurs, creative inventors, and one-step-at-a-time political reformers, all constrained by existing traditions and social norms that have evolved for their own reasons over a long period. This debate has shown up in many forms over time, and with many different protagonists. In the 1950s, Albert Hirschman’s “unbalanced growth” was a partial version of the second view, in contrast to the first view: the “Big Push” arguments of Rosenstein-Rodan and Rostow that everything would need to change at once leading to “balanced growth.” P.T. Bauer in the 1960s was a forceful critic of the “Big Push” idea and argued that the payoff from outside aid was close to zero. In the 1980s, the advent of structural adjustment revived the debate about comprehensive versus partial reform. In the 1990s, the debate was about shock therapy vs. gradualism in the transition from Communism to capitalism. In the new millennium, the “Big Push” has regained favor in some aid policy circles, particularly with regard to Africa. This contrasts with the academic development literature, where there has been a turn away from such ambitious actions in favor of rigorously evaluating small interventions. Admittedly, this dichotomy is oversimplified and most scholars will fall somewhere in between the two extremes sketched out here. To give labels to the two extremes for the purposes of the Africa discussion, let us call the first approach “transformational” and the second approach “marginal.”⁷

Is this distinction artificial? Don’t both approaches recommend some of the same practical interven-

tions? The litmus test I propose to distinguish the two approaches is in the ambition or goal of the approach. If an approach has the goal of achieving a large permanent gain in an aggregate indicator like growth or level of GDP per capita or a package of aggregate social indicators, it will be called transformational. A program that aims at permanently raising the growth rate of the economy through a permanent increase in aid (often conditional on changes in aggregate policies or institutions) is clearly transformational by this test. If the approach has the goal of solving a much more specific problem for a target group of beneficiaries (much smaller than the entire population of a country), such as a program to administer deworming drugs to a specific group of schoolchildren, it is marginal by this test. The large goals of the transformational approach will inevitably lead to some differences in implementation, such as a greater emphasis on top down planning, as compared to decentralized provision by individual agents in the marginal approach. This makes clear why the two approaches are different even when they include the same interventions—after all, centrally planned economies and market economies also provided the same consumer goods, but this does not invalidate the distinction between the two.

Of course, mainstream economics has always had much to contribute to this debate, first as the source of one of the most successful models of the “emergent self-organizing order,” the “invisible hand” of markets, with nobody in charge, and hence doesn’t automatically require an effort by leaders and experts at the top to transform the economy. This might suggest an inclination towards the marginal approach in economics. However, economics has also contributed ideas such as general equilibrium, theory of the second best, multiple equilibria and poverty traps, and complementarities between policy interventions that might point towards a more comprehensive approach

to avoid unintended consequences of a single partial equilibrium intervention.

Although these debates touch on the fundamental determinants of development in Africa, their immediate preoccupation is with the question “what can ‘we’ do?” The “we” seems to be development economists, aid agencies, G-8 politicians, and any other outsiders—so what can this “we” do to lift Africa’s poor out of poverty? Answering this question is sometimes confused with answering the much broader question “how can Africa develop?” However, there is no reason to assume the two questions have the same answers. This article will only be about the first question, and not about the second.

We will see that both approaches to what outsiders can do have been studied in the academic literature on aid to Africa. The stronger the ambition of a transformational approach, the stronger the support it would seem to require from research findings, since the costs and consequences of success and failure are greater for large-scale programs than for small-scale ones. Unfortunately, the academic literature has stressed that the technology of research seems to go in the opposite direction—it is harder to test effects of transformational programs than marginal ones. The difficulties of testing the transformational approach are due to identification problems involving multiple endogenous variables and selection biases in aggregate data, uncertainty about what control variables should be included, the usual impossibility of natural experiments at the system level, and the difficulties of attribution of outcomes to interventions with a program that involves multiple interventions. Not only that, but the quality of macroeconomic data is poor in developing countries (even more so in Africa), with well known discrepancies between macro data and household data on aggregate trends in income and poverty, and startlingly large revisions to macro data

on these same indicators. The aid to Africa literature also suffers from theoretical shortcomings, as standard neoclassical and political economy theory—such as the central role of incentives for private individuals and public officials—often seems neglected in favor of mechanical models without firm theoretical basis or bold assertions about what the ideal policymaker could achieve. Of course, there are some macroeconomic studies that do better than others dealing with these problems. The bulk of such results, along with simple evidence comparing outcomes to intuitive notions of counterfactuals, suggest serious doubts that there have been positive results from transformational programs in Africa.

It is not clear to us that the best way to get growth is to do growth policy of any form. Perhaps making growth happen is ultimately beyond our control.

In contrast to identification problems in the aggregate literature, the boom in the academic literature in randomized evaluation (RE) of particular interventions is motivated by the claim that the problems with aggregate econometrics can be resolved using micro data (also directly collected by researchers and thus of higher quality than macro data) in a randomized framework. The randomization literature makes a claim to have solved the identification problem at least for the specific intervention at the place and time being evaluated. Critics of RE have questioned whether the results can be extrapolated to more general aid policy settings, and RE should not be viewed as the only or even the principal methodology available under the marginal approach. Hence, the marginal approach in this paper should not be equated to the RE approach.

However, RE does at the very least signify a remarkable shift by academic development economics

towards marginal ambitions away from transformational ambitions. Take one step at a time and make sure it is a positive step—this seems to be the agenda of the new literature. I believe this shift in focus and ambition, which may have been an accidental consequence of the commitment to the RE methodology, is actually a greater contribution to the development literature than the methodological one (for reasons to be discussed below).

This shift in ambition is not much reflected in aid policy discussions (as the quotes above verify), and the gap between aid practitioners and the academic development economists may now be even wider than it was in the past. There are some small signs of this gap closing, such as the recently adopted Development Impact Evaluation (DIME) program at the World Bank, but it is unlikely to disappear. The World Bank's motto is "Our dream is a world free of poverty." This motto is probably much more likely to attract political support and funding than a slogan like "our dream is a world full of rigorous evaluations of small development interventions." Yet academics have to be honest about what we can know, regardless of political consequences. One of the major proponents of RE (Banerjee, 2008) makes a (perhaps unusually strong) statement that shows the gulf between the transformational views of the aid agencies and the marginal views of the academics in the RE literature (whose doubts about knowing how to raise growth are shared by many macro economists, as we will see below):

It is not clear to us that the best way to get growth is to do growth policy of any form. Perhaps making growth happen is ultimately beyond our control. Maybe all that happens is that something goes right for once (privatized agriculture raises incomes in rural China) and then that sparks growth somewhere else in the economy, and so on. Perhaps, we will never learn where it will start or what will make

it continue. The best we can do in that world is to hold the fort till that initial spark arrives: make sure that there is not too much human misery, maintain the social equilibrium, try to make sure that there is enough human capital around to take advantage of the spark when it arrives.

Even if randomized experiments do not resolve all the issues (see below), or if they are not performed or even feasible, it is still easier to have some notion of the effectiveness of marginal programs. Indicators of inputs and outcomes are usually easier to measure, plus attribution of outcomes to inputs is usually more intuitive, so that even raw data on outcomes, along with case studies can give some partial verdict on marginal approaches. Another advantage of a marginal program is that if it doesn't work, then it is more obvious which specific intervention failed. In contrast, even when there is an indication of failure of a transformational approach, there is little guidance about how to adjust it to work better—there are too many things changing at once to know what caused the failure.

Hence, another consequence of the differential ease of testing for positive effects of marginal approaches compared to transformational approaches is that there is more possibility of learning from the former. We will see that, perhaps because the transformational approach has been dominant, aid ideas have often been cyclical, with the same ideas going out of fashion only to come back again many years later—a pattern that is suggestive of lack of learning. We will see other examples that show little or no learning over time.

Another suggestive symptom of lack of learning has been escalation. When one long list of transformational actions does not achieve satisfactory results, new (untested) actions are added—as opposed to de-

termining which of the first set of actions contributed to success or failure (very hard to do in the transformational approach). So aid to Africa has escalated over time from individual projects to structural adjustment to institutional transformation to ending civil wars and reconstructing failed states.

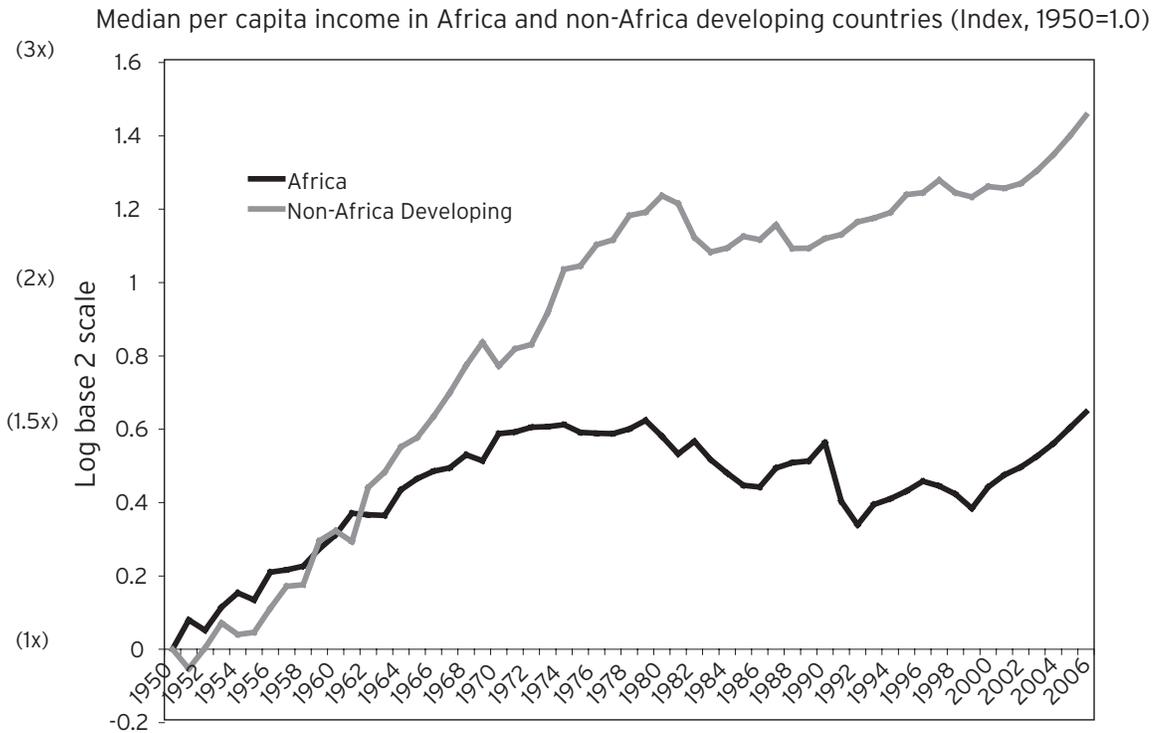
Poor growth and income levels

Why are calls to “save Africa” more common than calls to “save Latin America” or “save Asia”? The most obvious explanation is that Africa has a particularly unhappy combination of a low level of income and other social indicators, and low rate of progress on these indicators.⁸

First and foremost, Africa commands attention because it is the poorest region and has the worst per capita growth rates (which are obviously related facts if we measure poverty at the end of the period). As of 2005, 50.4 percent of Africa's population (380 million) live below the World Bank's international extreme poverty line—\$1.25 a day in Purchasing Power Parity terms—this proportion is about the same as it was in 1981. The mean consumption of this group was \$0.73 a day (Chen and Ravallion 2008).

Figure 1 shows an index on a log base 2 scale of an index of per capita income in the median African and non-African nation from 1950 to 2006, with the index=1.0 in 1950 (and thus $\log(\text{index})=0$ in 1950). The median country in Africa had positive growth 1950-1970, but was already falling behind the non-Africa median developing country as early as 1960.⁹ Divergence accelerated after 1970, when the median African country's growth was actually negative until the mid-1990s. There has been some recovery since, but the 2006 level in the median African country is barely above the previous peak in 1973.

Figure 1: Index of per capita income in Africa and other developing nations



Poor social indicators

Life expectancy is another indicator that highlights Africa’s tragedy, thanks to the double blow of high infant mortality and high adult mortality from AIDS. It is possible to pick a threshold for life expectancy (58 years) in which every African country is below that threshold and only a handful of societies elsewhere are (see Figure 2).

Table 1 highlights a fuller set of indicators on which Africa does very poorly in international comparisons. It dramatizes this by showing for every indicator in which there are N African observations, what percent of the N worst places in the world according to this indicator are occupied by African nations. For these indicators, Africa makes up 25-35 percent of

the worldwide sample, but occupies 70-80 percent of the worst rankings in the sample. Africa does very badly not only on per capita income, growth, and life expectancy, as already mentioned, but also on related social indicators such as infant mortality, AIDS prevalence, malnutrition, literacy, and the overall Human Development Index of the UN (which is a composite of the other indicators in this table). Deaton (2008) shows that life satisfaction (as measured by the Gallup World Poll) is strongly correlated with per capita income, so these measures suggest well-being in some broad sense is indeed significantly worse in Africa than elsewhere. (Deaton suggests the average Togolese man would be hospitalized for depression if he lived in Denmark).

Figure 2: Life expectancy in 2001

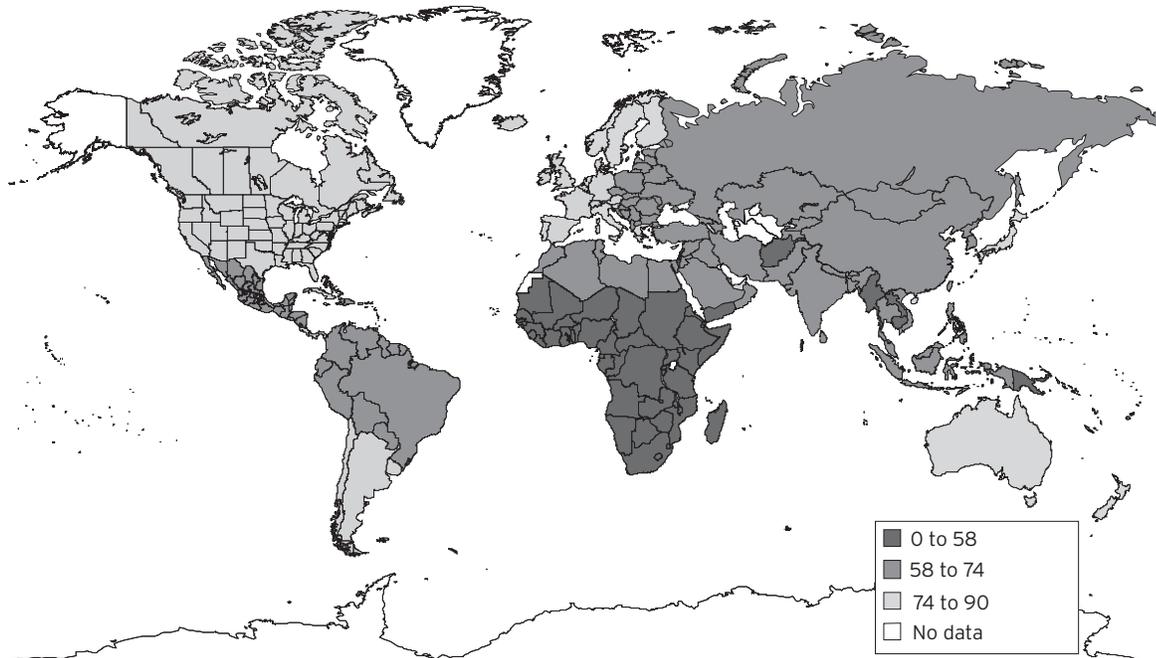


Table 1: Ranking of African countries by key international indicators where Africa does comparatively the worst

Variable	Number of observations (T)	Number of African observations (N)	Number of N worst places occupied by African countries (K)	Share of African observations in sample (N/T)	Percent of N worst places occupied by African countries (K/N)
Income per capita	130	44	35	34%	80%
Percent of Population Living on less than a \$1 a day	99	28	23	28%	82%
Per capita growth 1960-2003	113	44	34	39%	77%
Life expectancy	187	48	42	26%	88%
Infant mortality	195	48	36	25%	75%
Percent of Population 15-49 that is HIV positive	149	38	32	26%	84%
Prevalence of malnutrition, 2003	148	44	31	30%	70%
Literacy	122	34	21	28%	62%
Human Development Index	177	44	36	25%	82%

Not overdoing negative stereotypes

Although there is plenty of bad news on Africa, it is important to steer clear of stereotypical extremes. Some of those who want to save Africa justify their mission by painting a picture of Africa that is even grimmer than the not-so-happy reality. For example Collier (2007, p.3) portrays African societies that “co-exist with the twenty-first century, but their reality is the fourteenth century: civil war, plague, ignorance” (perhaps this statement is meant to be hyperbole in a book for general audiences). Celebrity activist Bob Geldof paints a similar picture: “War, Famine, Plague & Death are the Four Horsemen of the Apocalypse and these days they’re riding hard through the back roads of Africa.” The popular stereotype of Africans (reinforced by statements like these) seems to be as starving AIDS-stricken refugees being slaughtered by child soldiers, an image reinforced by the Western media following the “if it bleeds, it leads” rule of journalism. The reality of Africa contradicts the extremely negative stereotypes. While many of these disasters may be more likely in Africa than elsewhere, they are inherently rare occurrences. Table 2 shows that the Four Horsemen are the experience of a small minority of Africans—still far too many, but less than what seems to be implied by the stereotypes.

Although Africa is often portrayed as a place of uniquely bad government and civil war, its performance on quantitative measures of governance and war indicators is not as bad as that shown in Table 1. Using the same methodology as Table 1, African countries occupy 39 percent of the N worst places on democracy, 45 percent on corruption, and 35 percent on time spent in civil war since independence, as compared to Africa’s 24-27 percent of the cross-country sample. The world’s poorest region is still over-repre-

sented on these indicators, but to a much lesser extent than on the income, poverty, and social indicators shown above. (The average across countries for time spent in a serious civil war in Africa is 8.5 percent of the time since independence, which suggests war in Africa is a little more widespread than the fatality statistics in Table 2 might imply, but not much more so than in other very poor nations.) There are plenty of non-African countries sharing the bottom ranks for democracy, corruption, and war, highlighting again the need for a balanced rather than stereotypical view of Africa.¹⁰

There is an incentive for aid agencies and non-governmental organizations (NGOs) to exaggerate Africa’s negatives to facilitate fund-raising, even if most aid officials are professional enough to resist the temptation. Aid veteran Alex de Waal (1997) gives some (probably extreme) examples. He notes how some aid NGOs during the Christmas fund-raising season react to any current crisis such as a famine, drought, war, etc. with a “habitual inflation of estimates of expected deaths. ‘One million dead by Christmas’ ... has been heard every year since 1968 and has never been remotely close to the truth” (de Waal 1997, p. 144).¹¹ Journalists also sometimes adopt advocacy roles in disasters. De Waal (1997, p. 184) quotes a Somali doctor who describes a conversation he had with a television photojournalist in Somalia in 1992:

He just said to me, “Pick the children who are most severely malnourished.” I asked, “You go into a feeding centre with a thousand children. Two hundred are bad...why do you just select the two hundred-or the smaller number who are severely malnourished?” [The journalist] replied, “I am doing this to raise funds.”

Table 2: The four horsemen of the Apocalypse in Africa?

	Proportion of African population
Average annual war deaths as proportion of population, 1965-2005	0.0001
Proportion of male children ages 10-17 who were child soldiers in 1999	0.0019
Average annual proportion affected by famine, 1990-2005	0.0029
Proportion of population who are refugees or internally displaced persons, 2005	0.0053
Proportion of population who died from AIDS in 2007	0.0020

Figure 3: Aid to Africa

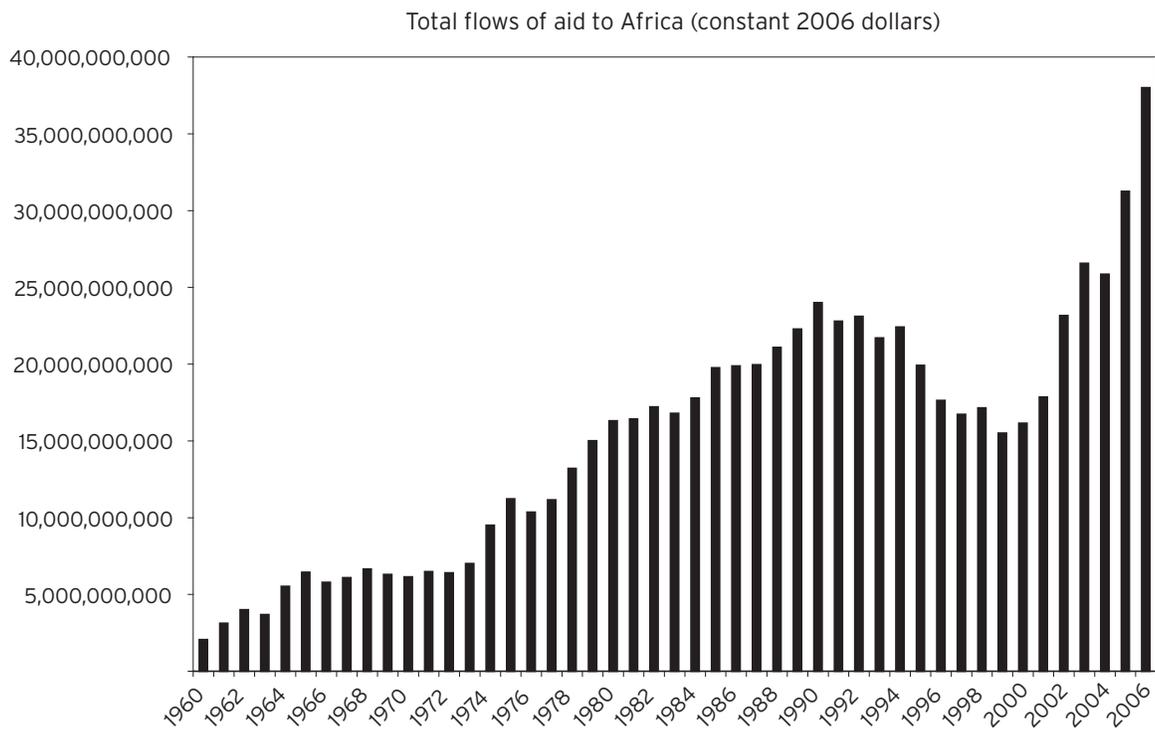
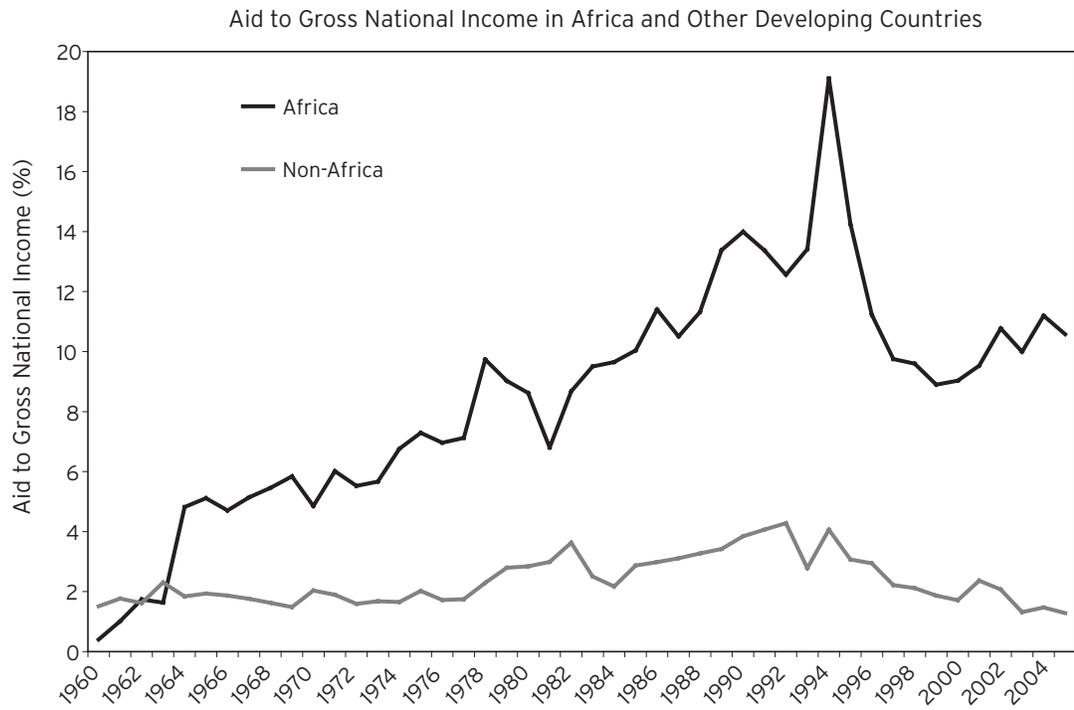


Figure 4: Aid to Africa in international perspective



Aid to Africa

Trends in aid to Africa

The recent high profile of Africa in international policy discussions is matched by a surge in aid to Africa (figure 3). The surge in aid came on top of a high base, so the cumulative total of aid to Africa in today's dollars from 1960 to 2006 is an impressive \$714 billion.

Aid compared to other regions

Some of those who advocate further aid increases to Africa point out that aid to Africa is not that large

measured in per capita recipient terms. However, this is misleading because there is a pronounced small country bias in aid. African nations with large populations get little aid as percent of GDP (notably Nigeria and South Africa), while many small African nations have large ratios of aid to national income. Hence, even prior to the recent surge in aid, the median African nation was already far more aid dependent than the median non-African developing nation (figure 4).

THEORIES AND EVIDENCE OF THE EFFECT OF WESTERN ASSISTANCE ON AFRICA

What would economic theory predict about the success of Western efforts to transform Africa? The models most often cited by those who predict large effects of Western efforts on Africa are models of poverty traps and multiple equilibria in which Africa's adverse initial conditions are both the explanation for African poverty and the potential lever by which Africa can be transformed, by making direct monetary transfers or by directly improving an input into development outcomes.

The alternative view is that of a unique equilibrium determined by adverse fundamentals. The latter view would require Western efforts to directly seek to improve the fundamentals, with a more modest payoff. Hence the "poverty trap" model goes with the "transformational" perspective, while the "fundamentals" approach goes with the "marginal" perspective.

The attempt to boost African growth with foreign aid

The simplest way that the "West could save Africa" would be if an injection of Western money (foreign aid) raised growth. Traditional development models of the 1950s and 1960s, which have now come back in favor in some policy circles, say that Africa is in a "poverty trap," in which a Big Push of aid to raise available funds for investment would permanently raise African growth (it is clear why this model is on the "transformational" side of the social change debate).

Theoretical model of poverty traps

A possible hypothesis of why Africa is poor is that it is in some version of a "poverty trap," which depends

purely on initial conditions. The competing explanation is that Africa's poverty is determined by fundamentals, regardless of initial conditions. To give a very general notion of a poverty trap, suppose there is some determinant X of per capita income y (we will call it "Factor X "), which is itself a function of per capita income y . The shapes of the two relationships, $y=f(\text{Factor } X)$ and $\text{Factor } X=g(y)$, will determine if poverty traps occur. Among the many possible candidates (not mutually exclusive) for Factor X in the aid and poverty trap literature, many of which will be considered below, are saving and investment, infrastructure, agricultural technology, education, health, policies, institutions, violent conflict, military coups, natural resource dependence, and "failed states."¹² The poverty trap view would hold if the situation depicted in Figure 5 holds. If the slopes are as in Figure 6, then a "fundamentals" explanation for Africa's poverty holds. In the first view, all countries have the same functional relationships, and only worse initial conditions have trapped Africa at the low equilibrium. In the fundamentals view, Africa has less of Factor X for every level of income, and it is this that determines its lower income.

As is obvious and already well known, although sometimes not always understood in aid policy circles, the simultaneity of factor X and income is not sufficient to generate "vicious circles" in which income and factor X get into a downward spiral on their way to the poverty trap. What is required is that BOTH Factor X and income have to be sufficiently sensitive to each other to generate the slopes shown in figure 5. For example, if $\log y = a + b \log X$ and $\log X = c + d \log y$, then a poverty trap will be generated if $bd > 1$. In other words, if the multiplicative average of elasticities of y wrt X and X wrt y is greater than 1, then there will be "vicious circles" and "poverty traps." Another simple prediction of the poverty trap model is that $-\log y$ (i.e.

the per capita growth rate) is increasing in the level of the initial $\log y$ (log per capita income).

What is so critical about the difference between the two figures, and what makes the poverty trap model so appealing, is that either Factor X or income just needs to have a one-time increase to escape the poverty trap. One only needs to increase one of the two, because Factor X would endogenously increase in response to higher income, and income would increase with Factor X. The escape from poverty through a one-time income increase makes for an appealing aid advocacy story—the need for aid is temporary, after which growth becomes self-sustaining. In the fundamentals view, in contrast, an exogenous, temporary increase in income through aid would have no effect. A temporary increase in Factor X would also be unavailing. Moving Africa to a higher level of income would require some kind of direct intervention that would permanently shift Factor X up for every level of income.

Hence, the “poverty trap” depicted in Figure 5 makes the solution to Africa’s poverty just one-shot cash transfers to whoever is the agent short of money to pay for X (the government for public goods, and private citizens for private goods and for saving/investment). Alternatively, outsiders could pay for directly or implement a technological fix to raise X, and this would get the economy out of the poverty trap. The difference from the fundamentals approach to Factor X is that the increase in X need only be a one-time temporary increase in the poverty trap story, and the effects of an increase in X are much larger in the poverty trap story (transforming the country from poor to rich) than in the fundamentals story (a marginal increase in income).

However, if some types of income increase Factor X, but aid receipts do not, then aid would not work to

escape the poverty trap even if it exists. For example, if X is institutions, we will see below that some studies argue that aid makes institutions worse (because aid increases the payoff to corruption, for example), even though we usually believe that higher income makes institutions better. Again, some poverty trap stories based on aid overlook the incentives faced by those who receive the aid when postulating that aid will have a positive effect on some particular Factor X.

The first and historically most oft cited mechanism for a poverty trap is that saving is very low for people who are very close to subsistence (as would be predicted by an intertemporal version of the Stone-Geary utility function). In a closed economy, saving is equal to investment, so investment is also low. In the Harrod-Domar model with the capital constraint binding, growth of GDP per capita is simply a linear function of the investment (=saving) rate minus the population growth rate and minus the depreciation rate. If saving is too low to keep up with population growth and the depreciation of capital, then per capita growth will be zero or negative. Early development economists in the 1950s and 1960s postulated a desirable per capita growth rate and calculated the “investment requirement” to meet this target—the distance between the low domestic saving rate and the “investment requirement” was called the “Financing Gap.” The role of aid was to fill the Financing Gap (Rostow, 1960; the “Two Gap Model” of Chenery & Strout, 1966). Thus, this model predicted a strong growth effect for foreign aid through its role in boosting domestic investment above what domestic saving would finance.

Although this model soon went out of favor in the academic literature on development (see Easterly 1999a for a discussion), it has come back strongly in the last few years in policy discussions, international organizations (where it always remained alive to some extent), and books for popular audiences. Current

Figure 5: Poverty trap for Africa

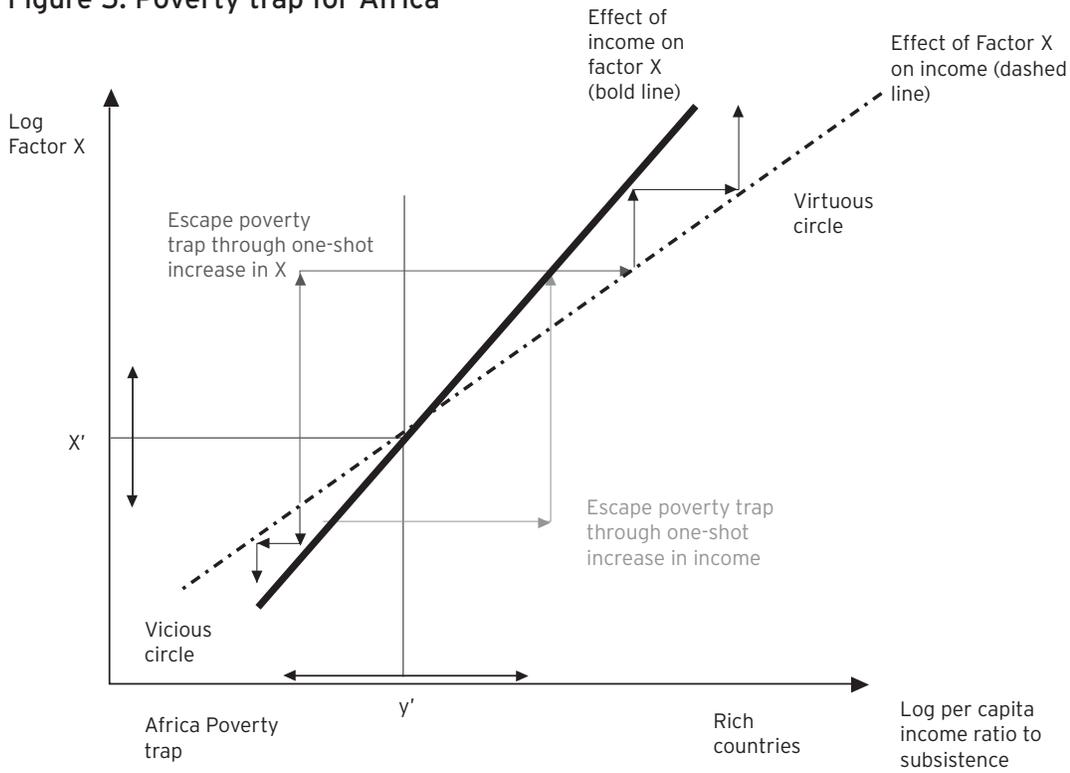
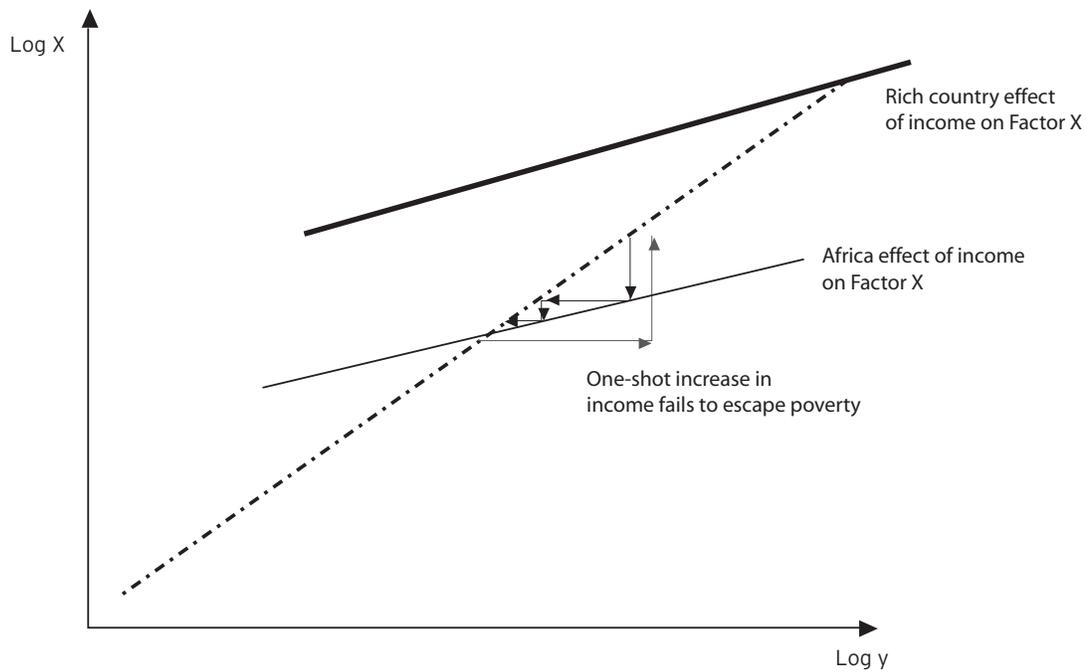


Figure 6: Fundamentals explanation for African poverty



policy advocates for an increase in foreign aid to Africa have cited this model explicitly (Devarajan et al. 2002 at the World Bank; Blair Commission on Africa, 2005; Sachs, 2005; Sachs, 2008; Collier, 2007). One attraction of this model is that it allows a mechanical calculation of “aid requirements” to achieve growth targets for Africa.

The “Financing Gap” approach shows the lack of attention to incentives (particularly local incentives) that has plagued the aid literature. Even in a closed economy, saving depends not only on the distance from subsistence but also on the local incentive to save depending on the rate of return to saving and investment. In an open economy, investment is not determined by domestic saving, but depends on the rate of return to investment. Private foreign investors and bank lenders will invest in the economy if returns are attractive enough.¹³ Domestic investors will also compare the returns to domestic and foreign investments, as shown by Africa’s extensive capital flight in which an estimated 39 percent of the stock of Africans’ capital is held outside the continent (Collier, Hoeffler, & Patillo, 2001).

In the Solow model of a closed economy, a strong relationship between income and saving rates could generate multiple equilibria at low and high levels of capital stock, reopening the possibility of a poverty trap. Kraay and Raddatz (2005) have shown that the relationship between initial capital and saving must follow an S-shaped curve to generate a poverty trap.

The other main mechanism to generate a poverty trap is some kind of nonconvexity in the production function in the Solow model. There may be strong external economies to investment, or there may be high fixed costs to investment projects such that a minimum threshold must be passed for investment to be pro-

ductive (“you can’t build half a bridge”). This idea was part of the inspiration for the original article that first proposed a Big Push (Paul Rosenstein-Rodan in 1943). This strand has had a longer shelf-life in the academic literature than the “Financing Gap” model because of the great interest of theorists in models with multiple equilibria (see for example the article by Murphy, Shleifer, and Vishny 1989).

Empirical evidence on poverty traps

General sample

It is not that easy to test for poverty traps in general, because they can take so many different forms and apply at so many different levels of aggregation. It is plausible that there WAS a poverty trap at the global level in the very long run (Galor 2005; Galor & Weil 2000), which may have inspired the idea of poverty traps in development.

It is somewhat easier to test some of the specific poverty trap mechanisms specified by early and recent development models. The savings- poverty trap model is testable by examining the shape of the savings function. Kraay and Raddatz (2007) failed to find evidence for the necessary S-shaped behavior of saving (they also failed to find technological nonconvexities in the production function, for good measure).

A more general test of the poverty trap depicted in Figure 5 is simply checking whether initially poor countries are more likely to have zero or lower growth than richer ones. The issue of growth differentials between rich and poor countries is the subject of a gigantic literature on convergence, the usual finding of which is that poor countries grow faster conditional on other fundamentals (“conditional convergence”). However, this is not the right test if the fundamentals are the

factor X's which may be responding to income in a way that creates a poverty trap. A simpler test is whether poor countries unconditionally grow more slowly or are more likely to have zero per capita growth (recall the prediction of the poverty trap model that growth is increasing in initial income). Easterly 2006 failed to find evidence of this type for poverty traps at low initial income—the poorest quintile at the beginning of each period did not subsequently have significantly lower growth rates than higher income strata.

Africa-specific poverty trap

Some of the literature argues that Africa is caught in a poverty trap even if other regions are not, or more generally, that countries in the “Bottom Billion” are still in a poverty trap which other initially poor countries have managed to escape. This latter story is close to making the poverty trap hypothesis non-falsifiable and tautological, in which any country still poor is in a poverty trap and any initially poor country that has grown richer is not. Collier 2007 shows that the Bottom Billion have had poor growth, but this finding suffers from selection bias. The Bottom Billion poorest countries were selected at the END of the period, thus biasing the sample towards countries that have had dismal growth performance over the preceding period.

An Africa-specific poverty trap seems to be ex-ante testable—the shapes of the Factor X and y curves could be different in Africa than elsewhere. For example, Africa's disease environment could be worse than other regions, and the health poverty trap could hold if African health is more sensitive to income than in other regions. However, if Africa's poor economic growth is the motive for singling out Africa for testing for a region-specific poverty trap, then a selection bias still renders the Africa-specific poverty trap test invalid. It is suggestive, moreover, that a number of

African members of the “Bottom Billion” were middle-income countries in earlier periods and then declined into the bottom (Cote d'Ivoire being the classic example: the “Ivorian miracle” of 1960-78 turned into one of the worst growth rates ever for the subsequent quarter-century.)

Of course, casual observation also influences priors about the Big Push and the Africa poverty trap stories. If the Big Push was already tried in Africa (as might be suggested by the aid/GNI numbers above), aid has further increased rather than being temporary, and yet Africa remains in poverty, then that seems inconsistent with the simplest stories of the Big Push and the poverty trap.

Empirical evidence on aid and growth

The literature has attempted more formal testing of the prediction of the poverty trap model that aid will have a sizeable effect on economic growth, as it enables countries to break out of poverty and move towards higher income (the “transformational” view again).

Most widely cited results

The aid and growth prediction has been the subject of a vast empirical literature. The literature only really became meaningful when the severe problem of reverse causality was addressed with the use of instrumental variables measuring political motivations for aid flows, as well as population size (a promising instrument, since as already noted, there is an exogenous small-country bias in aid such that smaller countries get higher aid per capita and higher aid as a ratio to their income). Boone (1996) was among the first to use such instruments and found zero effects of aid on investment and growth.

Boone provoked further testing of the claim that aid raised growth. By far the most cited aid and growth study in the entire literature was Burnside and Dollar (2000) in the *American Economic Review* (BD).¹⁴ BD also found that aid had no effect on growth. However, they also tested an interaction term between aid and government policy, which was significantly positive in some of their regressions. Hence, they concluded that raised growth when the recipient had good policies (measured by the Sachs-Warner openness index, low inflation, and low budget deficits). This finding offered an irresistible blend of plausibility and policy advice—reallocate aid to countries with good policies. Hence, it has been very influential in the policy debate about aid, and even contributed to the creation of a new US government aid agency (the Millennium Challenge Corporation) designed to give aid to countries with good policies.

What is notable given this strong policy influence is that the original results were both weak and fragile. BD used similar instruments as Boone for aid. Curiously, however, the significant positive effect of aid on growth (with “good policies”) held only in their OLS regressions, not in 2SLS (they argued this was not a problem because they failed to reject exogeneity of aid). And even for the OLS coefficients, the positive growth effect of aid was significant (under good policies) in just two out of the four regressions they presented. Even this was after they excluded some outliers that went against the hypothesis (as they made transparent). Furthermore, Easterly, Levine, and Roodman (2003) (ELR) subsequently showed that the significance of the Burnside-Dollar aid-policy interaction term even in the OLS regressions where it was significant was not robust to some basic checks, such as adding new data that had become available since the original study.¹⁵ The distinguished academic panel led by Angus Deaton that reviewed World Bank research singled out the Burnside and Dollar results

for criticism for lack of robustness and unconvincing identification strategy, and criticized the World Bank for overselling this particular result in its advocacy for more foreign aid (Deaton et al., 2006, pp. 52-57).

Identification, data mining, robustness checks, and magnitudes

This survey does not make more of an effort to survey all corners of this gigantic literature on aid and growth because the quality of most articles is poor. Most aid and growth articles fail to have a serious identification strategy.

While it was certainly progress to address identification in the articles cited above, that is not to say identification is easy to achieve. For example, does politically-motivated aid (such as aid to Egypt) have the same effects as altruistic aid? If not, the use of political motivations as instruments will address the effect of the first, but not the second.

Population size is another promising candidate for an instrument because of the exogenous and pronounced small country bias in aid. Of course, it may not satisfy the exclusion restriction as population size might directly affect growth. The growth regression literature has extensively looked for population scale effects and has generally failed to find them.¹⁶ This is not a valid test of the exclusion restriction, but it does give some important reduced form information. Given that aid received as a ratio to income is strongly affected by population size, then if aid affected growth, we would expect smaller population size would be associated with higher growth. This is not there in the data, which partly reflects the poor growth performance of many small Pacific and Caribbean islands and small African nations (all on average also very aid-intensive), so this is indirect evidence against a positive growth effect of aid.¹⁷

Werker et al 2008 is a recent paper that seems to have a believable and original identification strategy (aid from OPEC members to their poor Muslim allies, with the instrument being the price of oil interacted with a Muslim dummy). They also find a zero effect of aid on medium-term growth. There is still the doubt about this is whether it extrapolates to non-intra-Muslim aid.

There is no clear theory as to what other control variables should be included, which also weakens confidence in knowing what instruments satisfy the exclusion restriction. There is even doubt how the aid variable itself should be included (variants in the literature have included quadratic terms for aid/GDP, the log of aid, separating out aid loan repayments as a linear term combined with a log aid term, interacting aid with other variables, and many others), there is a serious data mining problem. Control variables in the literature have included such non-intuitive entries as Ethnic Fractionalization* Assassinations (BD). This is on top of the general data mining problem in growth regressions, in which Durlauf, Johnson, and Temple 2005 showed that 145 separate variables had been found to be significant in growth regressions with a typical sample of around 100 observations—and aid was not even one of the 145! The constructive thing that one can say is that data mining would manifest itself as a lack of robustness of results—changes in both the magnitude and significance of the aid coefficient. The failure of ELR to confirm BD is suggestive of this lack of robustness.

Sometimes the critics of aid-causes-growth models have been alleged to confuse “absence of evidence” with “evidence of absence” of a growth effect of aid. The predicted value for the aid coefficient under the “Two Gap Model” of the 1960s that expanded on the “Big Push” model of the 1950s was around 0.2 to 0.5,

so this model is strongly rejected by any estimates with an upper confidence bound below such a range.¹⁸ In the end, despite vast effort, the literature has failed to produce such a large (or any) positive causal effect of aid on growth that survives robustness checks, failing to confirm the prediction of the Big Push/Two Gap model. This resonates again with the stylized fact that African growth outcomes have been uniquely poor, and yet Africa is the most aid-intensive continent. To believe in a positive growth effect of aid, one needs to believe in the counterfactual that African growth would have been even worse in the absence of aid (not impossible, but harder to believe than if growth had been respectable). Given the figures shown above where the median aid received since independence (around 1965) was around 10 percent of GDP (Figure 4) and the per capita growth outcome was roughly zero percent (Figure 1), the implausible counterfactual implied by the “Big Push” coefficient of [0.2,0.5] is that the median African growth would have been -2 to -5 percent per capita in the absence of the aid “Big Push” since independence. As far as the better performance in the rest of the world, even proponents of more rigorous randomized evaluation methods (to be discussed below) like Banerjee (2007) have some intuition about the limited role of aid in successes outside of Africa: “my sense is that [the dramatic reduction in world poverty between 1981 and 2001] was driven largely by events in India and China, where donors had very little impact.”¹⁹

Project interventions

Another approach to “saving Africa” is to try to deal directly with some of the root causes of Africa’s poverty (in other words, directly attack some Factor Xs). At first blush, it would seem to be easy for donors to finance some productive public goods—just pave the roads! Just drill some boreholes! Just give farmers

fertilizer! In terms of the poverty trap and fundamentals model, the intervention to increase Factor X could either be motivated by an attempt to escape the poverty trap (the “transformational” case that the development impact of the increase in X is very large) or by an attempt to improve the fundamentals so as to shift income higher in Africa (with a more “marginal” payoff).

Indeed because the results are so tangible and visible, this survey will argue that aid to Africa has probably been more successful at achieving some project successes than it has been at other approaches to aid. However, the aid industry still felt that the results of the project approach were sufficiently disappointing (from a “transformational” viewpoint) that it shifted away from it strongly. We will see an interesting escalation in the literature and in policy, with the West first trying to fix those project-specific Xs that are more amenable to outside fixes, with at least some success but still a disappointing growth payoff (i.e. the results seemed to be marginal rather than transformational), followed by “transformational” attempts at more systemic changes to be discussed in the following section.

Most of the emphasis in project-specific efforts has been in addressing problems of illiteracy, disease, low agricultural productivity (possibly linked to land tenure practices, to be discussed more in the “institutions” section below), and poor social and physical infrastructure. These efforts have a long history. In an extreme example of the recycling of aid ideas across generations, a 1938 survey of colonial Africa commissioned by the British (the “Hailey report”) covered some of the same problems and even proposed some of the same solutions as the 2005 UN Millennium Project that comprehensively surveyed aid interventions, as shown in Table 5. It would be hard to argue that Africa’s development problem is missing techni-

cal knowledge, as some transformational approaches claim, when some of that knowledge has already been around for 70 years. For example, why is there still malnutrition in Africa due to lack of vitamin A, when this problem and its solution has been well known for 70 years?

Overall record of projects

Old evidence from project rates of return

Before turning to a discussion of the details of Western efforts in each sector, it is useful to survey the overall record of the project approach. The first kind of evidence is ex-post rates of return to aid projects, usually calculated by the aid agency or even the individual doing the project (and so probably biased upwards). In the first few decades of foreign aid, these rates of return were in the positive double-digit range. The literature discussed the “micro-macro” paradox, in which project returns to aid were high and yet as we have seen, the literature often failed to find an overall growth payoff to aid (see discussion in Doucouliagos and Paldam 2008). Later evidence on projects was not as favorable. The World Bank commissioned a study (known as “the Wapenhans report,” World Bank 1992) of World Bank project performance, as measured by the percent of projects classified as successful (again done by project managers and thus probably biased upward). Even with the probable upward bias, only 59 percent of projects in Africa were classified as “successful,” compared to 74 percent worldwide for World Bank projects.

New evidence of randomized controlled trials

The calculation of project rates of return had a number of problems. The estimation of the benefits of the project were done in an ad-hoc way that left a lot of

Table 5: The similarity of old and new recommendations for technical interventions in Africa

African problem to be addressed	Committee of the African Research Survey, 1938 (headed by Lord Hailey)	UN Millennium Project, 2005 (headed by Jeffrey Sachs)
Malaria	"[Steps to control malaria in European homes include] mosquito screening, mosquito bed-nets, and the use of insecticidal sprays...in certain native areas...malaria control by the spraying of native huts with a preparation of pyrethrum" (p. 1126)	"the public good will best be served by the free provision of insecticide-treated nets, application of residual insecticides, and provision of effective antimalarial medicines and diagnostics... insecticides for indoor residual spraying (mainly DDT and pyrethroids)" (p. xii, p. 6, Malaria task force report)
Hunger and nutrition	"Whether the African eats enough food and, if he does whether it is of the right kind, and whether the attack on poor nutrition may not be the most important factor in reducing disease...the African suffers from deficiency of Vitamin A" (pp. 1122-1123)	"Chronic undernourishment is caused by a ... lack of access to food of sufficient quality and quantity... It results in ... high child mortality brought about by associated diseases...Malnutrition [is also] caused by inadequate intake of ...[micronutrients such as] vitamin A" (p.3 Hunger Task Force summary report, p. 128, Hunger Task Force full report)
Soil fertility	"methods of improving soil fertility [such as] green manuring" (p. 962)	"using green manure to improve soil fertility" (p. 107 Hunger Task force main report)
Soil erosion and deforestation	"soil erosion has become recognized as one of the major problems ..." (p. 1056) "Since the destruction of vegetal cover is the prime cause, the restoration of such cover is the obvious remedy." (p. 1063) "The most ancient, universal and effective method of increasing absorption and reducing runoff on cultivated land is the use of terraces." (p. 1064)	"severely degraded soils...often suffer from unchecked erosion... (p. 107, Hunger Task force main report) "the overharvesting of vegetation, stripping landscapes of their forest and plant cover and destroying riparian vegetation... increases the risks of ... erosion. (pp. 172-173) Contour terraces, necessary on sloping lands... when furnished with grasses and trees...[to avoid] soil erosion" (p. 113)
Land tenure	"all discussions on the subject agree as to the value of giving security to the occupier of land... legal security against attack or disturbance can most effectively be guaranteed by registration." (pp. 868, 876)	"The rule of law involves security in private property and tenure rights ... upholding the rule of law requires institutions for government accountability... this requires a well functioning and adequately paid civil service and judiciary, proper information technology (for registration of property ...)" (pp. 31, 111)
Clean drinking water	Description of sinking boreholes in various African countries (pp. 1033-1052)	"Increase the share of boreholes to half the share of improved dug wells" (Water and Sanitation Task Force, p. 105)

room for subjective judgments. This was particularly problematic because the aid agency (and sometimes the specific individual who had led the project effort) were the ones calculating rates of return, implying a possible conflict of interest that would bias rates of return upwards. Even if the evaluators were completely objective, there was no mechanism to regulate their subjective judgments so that hypothesized benefits corresponded to real improvements enjoyed by the beneficiaries.

A much more rigorous way to assess aid-financed interventions has blossomed in the literature in recent years—the use of randomized evaluations. These measure the impact on some measure of well-being of an intervention in a randomly selected treatment group, as compared to the randomly selected control group. This literature has found many aid project interventions to have positive benefits and to be cost-effective (Banerjee 2008; Duflo & Kremer, 2008).

Based on this encouraging evidence, Banerjee has written positively about the potential of such (marginal) aid in his book *Making Aid Work* (2007). This literature offers its methodology as an improvement not only on subjective rate of return calculations, but even more as an improvement over aggregate cross-country regressions, such as those described above estimating the effect of aid on growth.

The REs became a popular methodology because of the great vacuum of evidence on development projects. As Pritchett (2008) says eloquently:

...nearly all World Bank discussion of policies or project design had the character “ignorant armies clashing by night.” There was heated debate among advocates of various activities but very rarely any firm evidence presented and con-

sidered about the likely impact of the proposed actions. Certainly in my experience, there was never any definitive evidence that would inform decisions of ... funding one instrument versus another (e.g., vaccinations versus public education about hygiene to improve health, textbook reform versus teacher training to improve educational quality.)

As even a World Bank handbook said “Despite the billions of dollars spent on development assistance each year, there is still very little known about the actual impact of projects on the poor” (Baker, 2000). At the very least, the RE literature successfully dramatized the case for basing aid policy on evidence rather than on prejudice and special interests.

The case for REs being a major advance over cross-country empirics rests on several strong claims. First, and most importantly, the RE literature claims to have solved the identification problem. The random assignment to a treatment group is an instrument for the treatment, and one can then calculate the causal effect of the treatment on the chosen outcome. This does qualify as a major advance on identification in empirical development work.²⁰

Second, the RE literature claims to be free from the data mining problem we have discussed above for cross-country regressions. One is simply doing one pre-specified regression of outcome on treatment, so even researchers with the same “searching for significance” motivation as those doing aggregate regressions will have their hands tied. Unfortunately, this claim is a little overblown for several reasons. First, there will often be more than one outcome measure, and researchers often emphasize those outcome regressions that show significant treatment effects, without adequately taking into account that such a

result may be random if there are many outcomes to choose from. Second, researchers often report results from an ex-post slice of the sample, and they will naturally report mainly those ex-post slices that are significant. Third, researchers often include as other covariates some individual characteristics that affect outcomes, as a way to reduce the standard error on the treatment dummy. However, choosing which covariates to include is something like choosing which RHS variables to include in a growth regression—it is not obvious ex-ante. Hence, researchers could be (unconsciously) searching among covariates until one achieves a significant effect of treatment. Duflo, Glennester, and Kremer 2008 acknowledge these problems and recommend full disclosure, which is commendable, but this is hard to enforce. The scope for data mining still may be less than in cross country regressions.

Duflo (2004) has argued that REs present a simple form of unambiguous evidence that is more likely to influence policy than other kinds of empirical development work. Here, the great success story is Progresa in Mexico, which was scaled up and continued under two different administrations due in part to the positive results from REs evaluating Progresa (Levy, 2006). Of course, there were also political factors. Green (2005) found that, despite the attempt to depoliticize Progresa, municipalities that had previously voted for the party in power were more likely to have their localities enrolled in the program. Diaz-Cayeros et al. (2008) dispute that finding, but found that even a non-discretionary Progresa/ OPORTUNIDADES program paid off at the polls for the incumbent in both the 2000 and 2006 elections. They also point out that President Vicente Fox's decision to expand OPORTUNIDADES from rural areas to the cities made political sense since his party's political base was urban.

There are also clear failures of REs to translate into program adoption, such as the Colombia private school vouchers that received accolades from one of the most famous REs of all (Angrist et al., 2002) and yet was discontinued and never revived in Colombia (the cancellation of the program goes curiously unmentioned in the large literature citing Angrist et al.). Moreover, most governments are unwilling to even do REs, so most results in the literature are based on NGO projects, not government projects. Pritchett (2008) argues that a model of government behavior as driven by economists' normative recommendations performs very poorly as a positive model (using education as an example). As Pritchett says:

the randomization agenda as a methodological approach inherits an enormous internal contradiction—that all empirical claims should only be believed when backed by evidence from randomization excepting of course those enormous (and completely unsupported) empirical claims about the impact of randomization on policy.

Perhaps most importantly, critics such as Deaton (2007; 2008) and Rodrik (2008) point out that while the strong claim to identification of RE may hold for internal validity, they don't necessarily extrapolate to other settings than the experimental situation. (The same problem appears in aggregate econometrics, as we saw above, where the variation (in e.g. aid) associated with an instrument's variation may have different effects than other variation in aid. It is also very possible that different regions have different coefficients on some RHS variable—such as aid—in aggregate regressions.) Cartwright (2007—quoted in Deaton, 2008) points out that REs do “not tell us what the overall outcome on the effect in question would be from introducing the treatment in some particular way in an uncontrolled situation, even if we consider

introducing it only in the very population sampled. For that we need a causal model.”

RE proponents (e.g. Duflo, Glennester, & Kremer, 2008) respond that REs can be replicated in many different settings to confirm a general result. However, as they acknowledge, the incentives for researchers to do replications fall off very rapidly with number of replications already performed, and it is unclear how many you need or how to choose the right sample of environments (with what factors varying?) to validate a result from the original study. This survey will report RE results from any environments that have seen studies (including outside of Africa), just as with aggregate evidence, the presumption will be that evidence from outside of Africa applies also to Africa, unless we have a good reason to think otherwise.

The biggest problem is the absence of a model to clarify why, when, and where the treatment is expected to work (Deaton, 2008). An RE is most useful when it sheds light on some behavioral response (e.g. the price elasticity of demand for health inputs, to be discussed below), although even then it may not extrapolate to other settings; it is less useful when it makes a blanket claim that “X works but not Y” based on one very small sample in a particular context, without any clear intuition as to why X is more likely to work than Y. Rodrik 2008 points out that to go from RE results to policy often involves the same kinds of appeals to theoretical priors, common sense, casual empirics about similarity of the new policy setting to the original research setting in some (but not all) aspects, and other more casual sources of evidence that are not much different from using aggregate econometric results and stylized facts to influence policy.

This methodology could also work as an evaluation of whether THAT NGO or aid agency’s project worked on THAT occasion, which could be useful for holding aid

agencies accountable for results. However, RE proponents like Duflo and Kremer (2008) have voiced opposition to any scheme that would reward or penalize particular aid actors for positive or negative results of evaluations.²¹ They object in part because they need the cooperation of the implementing agency to do an RE. If the agency felt threatened by a negative result or perceived great rewards to a positive result, they might fake the results. The problem with this argument is that either the existing RE system already contains considerable rewards for positive REs (as debated above) or the RE proponents want to redesign the aid system to do so. Duflo (2004) says: “Positive results, on the other hand, can help build a consensus for the project, which has the potential to be extended far beyond the scale that was initially envisioned.” It is hard to imagine that an implementing agency or its staff would be indifferent to a large increase in its budget from scaling up, not to mention kudos for having found a very successful intervention. Ravallion 2008 argues with such motives in mind that agencies selectively agree to REs where they are already confident a program is working, so the probability of a positive evaluation is biased upwards. The cost of such aid evaluation may also be prohibitive, but if costs can be low enough relative to the benefits of the project perhaps the use of REs for accountability should be explored further (and, in any case, more attention should be paid to incentives of agencies to manipulate results).

Lastly, this methodology does not address the general equilibrium effects of a marginal aid project, to be discussed next.

These many criticisms and caveats make clear that REs are far from being a panacea in development, or even just to “make aid work,” and the RE proponents overstate their potential. REs are neither necessary nor sufficient to verify that a development inter-

vention is working in general. The proponents have been overly dogmatic in dismissing other forms of evidence, which has hampered mutual learning from practitioners of different methodologies in development empirics.

The REs do represent progress in having added to the kit of empirical researchers a tool that alters priors of both other academics and policymakers when there is a strong result (particularly if it helps test a behavioral model). The effect on priors is perhaps the real acid test that this methodology has something to contribute, even if not as much as its proponents claim.

The debate on REs vs. other forms of econometric or case study evidence has perhaps obscured a far more important divide in the world of aid practice—that between those who feel bound by objective evidence and those who do not. The RE methodology has had a positive demonstration effect showing the scientific method can be applied with marginal interventions, in an aid world that too often ignores ANY existing evidence (or any need to find such evidence). Aggregate econometric work suffers from many problems, but the best examples of such work try to resolve problems such as identification and data mining, showing that they also take the scientific method seriously. In contrast, too much aid practice doesn't bother with seeking objective evidence, or ignores evidence that does exist. Banerjee (2007) gives the example of a computer kiosk program for the poor in India that often didn't work because of unreliable electricity supply and bad telephone connectivity that failed to connect to the Internet (interestingly, a few descriptive sentences on this convinced Banerjee, not an RE). Yet the World Bank's "Empowerment Sourcebook" said: "Following the success of the initiative...". Even more incredibly, another long-time aid official still defended the World Bank Sourcebook after hearing Banerjee's example by saying the World Bank only

intended to help achieve "greater empowerment." Banerjee responds: "Helped to achieve greater empowerment? Through non-working computers?" (see Banerjee 2007, pp. 77, 112)

Is the RE literature clearly marginal rather than transformational? RE proponents have some of the same difficulty resisting the siren song of transformation as anyone else. Duflo and Kremer (2008) close an article with these words:

[RE is] credibly establishing which programs work and which do not, [so] the international agencies can counteract skepticism about the possibility of spending aid effectively and build long-term support for development. Just as randomized trials revolutionized medicine in the twentieth century, they have the possibility to revolutionize social policy during the twenty-first.

Similarly, Banerjee (2007) said right after his skeptical remarks about "growth policy" quoted in the introduction: "Social policy may be the best thing that we can do for growth to happen and micro-evidence on how to do it well, may turn out to be the key to growth success." It is ironic that testing these large claims for the RE methodology cannot be done with RE methodology and would instead require the very big-picture kinds of evidence that the RE proponents disparage. Even the most casual empiricism would detect the lack of any obvious examples of country-wide escapes from poverty using policies determined by REs. So despite the rhetoric of some RE proponents, REs mainly seem useful as a way to sometimes (especially when sufficiently tied to a behavioral model) influence outside donor decisions on marginal interventions that have previously operated in a vacuum of evidence. I will discuss particular REs relevant to each of the sectors I discuss below.

General equilibrium effects

The aid literature has worried about whether the evidence of positive project impacts is enough to suggest a significant positive impact of aid. Rajan and Subramanian 2008 pointed out that the micro-macro paradox still holds with the new randomized evaluation literature, with positive returns to micro projects yet apparently still zero macro growth payoff. I will consider more systemic approaches to aid below, but here I stay within the confines of the project approach to discuss two issues that are often raised in the literature: fungibility and implementation. Note that these arguments are often used to justify more sweeping transformational approaches themselves, but whether they are valid concerns is a separate question than whether the transformational approach is the right one.

Problem of fungibility

The fungibility concern recognizes that if the government receives an aid transfer for good purpose A, that transfer frees up the government's own money previously spent on A for some other (possibly bad) purpose B. In this case, the true effect of the aid is to finance the other increased spending B that would not have happened without aid to the donor-favored purpose A. As Paul Rosenstein-Rodan said colorfully way back in 1953, you might think you are financing a power plant when in fact you are financing a brothel. Fungibility has been explicitly tested in the aid to Africa literature. Swaroop and Devarajan (2000) and Feyzioglu, Swaroop, and Zhu (1998) both find significant but less than 100 percent aid fungibility across sectors. Even with partial fungibility, unfortunately, the rate of return to an aid-financed project is not the same as the general equilibrium rate of return to aid spending.

Interaction with incentives on implementation

The second problem with evaluating the benefits of aid spending is one of implementation. If an RE shows positive results from a particular project or intervention that is executed, it does not follow that giving aid for that purpose will automatically result in project execution. As Reinikka and Svensson (2005) argue:

When scaling-up a specific program found to work in a controlled experiment run by a specific organization (often an NGO with substantial assistance from the research team), it is crucial also to have an understanding of the whole delivery chain; from the institutional constraints that affect central government policy decisions, through the incentive constraints that influence different layers of government agencies and officials implementing a given policy, to the actions and incentives of the end-producers (schools) and beneficiaries (students and parents). Lack of attention to the service delivery system, and adjustment of policy accordingly, may imply effects very different from what a simple extrapolation of the estimates of the controlled experiment produces.

Incentive problems have been a major theme of the literature on health and education in Africa (often called "systems issues," as in you cannot expect good health outcomes if the public health *system* is dysfunctional). While educational enrollments have expanded rapidly, the quality of education is hampered by missing inputs like textbooks and other school materials, weak incentives for teachers to show up or teach effectively, corruption in education bureaucracies, appointment of unqualified teachers for patronage reasons, and disruption of schooling by political events (Filmer & Pritchett, 1999). Donors have long

recognized the quality problems in education (for example, World Bank WDR, 1980), but these problems are remarkably persistent (World Bank WDR, 2007 again stressed quality problems in education).

In health, corruption in the health system (studies in Guinea, Cameroon, Uganda, and Tanzania estimated that 30 to 70 percent of government drugs disappeared before reaching the patients), absenteeism of health workers, and sheer bureaucratic inefficiency are chronic problems. Some widely-cited regressions find no impact of health spending on health outcomes (Filmer, Hammer, & Pritchett, 2000; Pritchett & Woolcock, 2004).²²

The RE literature has itself documented the weak incentives facing public servants to provide services. Chaudhury, Hammer, Kremer, Muralidharan, and Rogers (2006) surveyed studies in Bangladesh, Ecuador, India, Indonesia, Peru, and Uganda where unannounced visits to schools and health clinics found teachers absent 19 percent of the time on average and health workers absent 35 percent of the time. Even this was an understatement as some who were present were not working. We can understand this as reflecting weak sanctions for absence: in a sample of 3000 Indian schools, there was only one report of a teacher fired for repeated absences. The problem of teacher and health absence is worse in poorer countries or states within countries (e.g. the one African country, Uganda, has a worse problem than richer countries in the study). There is some evidence of response to incentives. Teachers in an NGO program of non-formal schools in India that were required to take a date-stamped picture of themselves with students everyday, with a pay bonus for each additional day of attendance, had a much lower absence rate—22 percent compared to 42 percent in the control group (Duflo & Hanna, 2005).

With doubts about implementation, a research project studying a health or education intervention whose execution is guaranteed by the design of the research project tells us little about how effective will be health or education aid in achieving that execution in the existing system of public services. The RE literature does not seem to have a good answer to this conundrum, although to be fair these studies often seem to envision NGOs doing the intervention rather than the government. However, since many interventions can only be brought to a large scale by the government, the larger policy interpretation of many RE claims that “Intervention X works” are in doubt.

We see similar implementation problems in infrastructure. Since independence, there has been much road building and expansion of electric generating capacity and water supply, supporting the idea that aid is more productive when directed to specific, piecemeal interventions. However, there has been a chronic underinvestment in maintenance of infrastructure. For example, donors (and the recipient governments) have the incentive to build highly visible new roads, but less incentive to provide invisible maintenance.

The bias against operations and maintenance in infrastructure has been known for decades—highlighted for example in World Bank (1981, 1988, 1994), with each succeeding report bemoaning the failure to make progress since the previous report—and it remains a problem today.²³ The results are chronically potholed and cratered donor-financed roads, for example, always being reconstructed and then deteriorating again. This is another example of inability to learn from past mistakes.

Kremer and Miguel (2007) suggest the problem is the donors’ obsession with “sustainability,” in which they envision the recipient government or local communi-

ties providing the financing of recurrent costs (operations and maintenance) after donors finance the capital costs of infrastructure, so that the project will be “sustained” once donor financing ends. This hope has turned out to be an illusion, as the failure to cover recurrent costs has been nearly universal. Kremer and Miguel suggest donors should be willing to permanently bear the recurrent costs of their projects if they really want those projects to be effective. This is again the conflict between the “transformational” view of projects, in which a project will lead to a permanent systemic improvement with “sustainability,” versus the “marginal” view of Kremer and Miguel that the project should just be assured of having lasting positive benefits.

Of course, if Kremer and Miguel’s analysis is extended into a proposal for the donors to take over completely any and all aspects of any public service that yields positive benefits, then once again one would have to worry about the fungibility question—wouldn’t the resulting equilibrium be that domestic government spending would be completely redirected to unproductive uses? Still, the fungibility question does not completely destroy the information content of finding positive project returns to aid projects. Fungibility is generally significantly less than 100 percent in empirical studies, so its effect is to scale down the positive effect of a project rather than to reverse or eliminate it.²⁴

Fungibility and implementation problems are often used to justify a movement towards another kind of transformational approach to project aid: namely the aid donor should review all public expenditure and reform the civil service and do “capacity-building” so as to create civil servants who had the capacity to spend money on the right things and implement things effectively. This kind of approach increasingly got

bundled together with major economic policy reforms in the “structural adjustment” era after 1980 to be discussed below. The World Bank did 70 civil service reforms in Africa during 1987-97, and over a quarter of World Bank lending to Africa is currently devoted to “capacity-building.” Yet political scientists specializing in analyzing African states see little sign of effect of these Herculean efforts at making civil servants perform better, even seeing some signs of decline (Moss et al., 2008). The transformational response to fungibility and implementation problems was not so constructive.

International collective action on outcomes affected by project aid

An alternative transformational approach to most of the social indicators affected by project aid was for the United Nations to announce targets for social indicators like primary enrollment and child mortality for some date 10-15 years in the future, such as the Millennium Development Goals (MDGs) set for 2015 in a UN Summit in 2000. This was supposed to induce greater effort by international aid agencies and poor country governments to improve these indicators, and the MDGs have been remarkably successful in capturing the attention of official agencies. The goal-setting approach is in the “transformational” camp because the goals imply a very large improvement in development outcomes, and the intention in improving the social indicators is to launch the country as a whole into self-sustained growth.

There is also an analogue to the Big Push/Two Gap approach to aid and growth in the MDG discussion, as increased aid is predicted to mechanically increase social indicators such that MDGs are attained, given minimum good government: “aid ‘will ensure that no country genuinely committed to poverty reduction,

good governance and economic reform, will be denied the chance to achieve the Millennium Goals through lack of finance.”²⁵ The same mechanical approach shows up in exercises that calculate the “costs” of achieving the MDGs, and then leaps to the non sequitur that raising aid by an amount equal to those “costs” will in fact achieve the MDGs.

Even as skeptical and rigorous an economist as Banerjee (2007) cannot resist the appeal of a mechanical calculation of scaling up to show that aid money will help achieve worthy goals if directed to the right things. Banerjee first chooses programs that have been verified to “work” by RE, second saying “the way we calculate costs is to take a point estimate of the per person cost for each program,” and third, multiplies this per person cost by the number of eligible beneficiaries.

A much quoted study by Devarajan et al. (2002) of such a costing exercise came up with a price tag of \$40-60 billion. Devarajan et al. themselves are too good as economists to take their own estimates seriously—for example, they note about their cost calculation for the health and education MDGs that “empirical evidence from developing countries suggests only a weak link between public spending on education and school enrollments, or between health expenditures and mortality or disease.”

Taken literally, this approach was not successful as the goals were very seldom met, and the same goal was postponed to a later date for another international campaign. Education was a good example of this, with one of the goals of the Millennium Development Goal campaign to achieve universal primary enrollment by 2015, a goal that is most relevant to Africa since it is the main region still lagging behind on this indicator (despite rapid progress to be noted below).

Clemens (2004) notes about the education goal that “Roughly once every two decades since the Second World War, an international gathering of policymakers has solemnly promised to achieve universal primary education in developing countries by about twenty years thereafter.” A series of UNESCO conferences in the early 1960s set Universal Primary Enrollment as a goal for 1980. When that was not met, a series of new UN summits reset the goal for 2000. As 2000 came without such achievement, the UN’s Millennium Development Goals summit in that year made another promise to achieve universal enrollment by 2015. Similarly for infrastructure, a previous summit in 1977 set the goal of universal access to water and sanitation—2015 targets for the Millennium Development Goals—for 1990.²⁶ So missing the goals did not seem to induce any change in behavior for those who favored this approach, since they simply repeated the exercise for a future date.

The international goals approach has some obvious theoretical flaws. It sets up an international collective action problem, with multiple agents (many official aid agencies plus many aid recipient governments) who face a serious free rider problem, with the result that no one actor faced any consequences for failing to meet the goal. For a single agent, having multiple goals is like having multiple principals, which is well known to weaken incentives for the agent because principals’ incentives for the agent to work on their goal cancel out each other. Finally, to make things even worse, even if there were only one agent and one goal, the Millennium Development Goals are broad outcome measures where it is very difficult to attribute social outcomes to aid efforts, since the outcomes also depend on many other things, including the important but often-overlooked incentives of local public and private actors to make progress in the areas covered by the Goals. Hence, the incentives

for action created by international targets seem to be very weak indeed (and even then the action seems to be more oriented toward increasing total aid dollars rather than improving effectiveness of that spending to produce better outcomes). Pande (2006) says it perhaps most clearly: "if we are to succeed in designing and implementing policies which bring about development then we need to be both more modest in what we expect to achieve solely through the setting of appropriate goals, and much more ambitious in trying to understand the incentives facing individuals, institutions and governments in developing countries."

Defenders of these goal-setting exercises suggest they increase the aspirations of some or all of the aid agents, leading to positive results. They do seem to have recently been successful in contributing to the international advocacy for aid to Africa (as discussed in the introduction). However, given the repeated lack of success in attaining goals, the goals approach

seems like another example of cyclical fashions, i.e. of failure to learn in the African aid effort. It is also another example of exaggerating the potential impact of outside actions.

In line with the theme of this paper, the Millennium Development Goals were very much a "transformational" exercise, in that they implicitly committed aid agencies to "do everything at once," to fix ALL the problems of poverty in one fell swoop. This reflects a shift in aid thinking towards more comprehensive approaches that began in the 1990s (reflected at the time in World Bank President James Wolfensohn's "Comprehensive Development Framework".) This was a shift towards an even more ambitious agenda (including almost every possible dimension of development such as "faith and development" and "women's empowerment") than even the more modest transformational idea of aid creating economic growth, again reflecting the escalation theme.

AID AND SOCIAL INDICATORS

Education

Trends in education and micro evidence

Despite the implementation problems stated above, education is a relative success story in Africa since independence. Primary enrollment started off very low and then rapidly caught up to other developing countries (figure 7). There was a lot of donor involvement in education—is this an area where aid helped shift a fundamental determinant of development in a way that helped “save Africa”? A pattern we will see recur is a global trend towards improvement of social indicators, which includes Africa (as pointed out by Kenny, 2006). Of course, aid to the poorest countries could have played a role in this improvement. There is an obvious long run global trend towards increasing enrollments (Kenny, 2008c); developing countries since 1960 have raised enrollments faster than today’s rich countries did in their history (Clemens, 2004). The brute stylized fact is that donors intended to increase education with aid, they spent money building schools, and enrollment did increase—such evidence is suggestive even if far from definitive.

The randomization literature has found a number of aid interventions (both inside and outside Africa) to be effective in education. Kremer, Miguel, and Thornton (KMT) (2007) found that a merit scholarship for high school girls in Kenya seemed to induce greater study effort and increased the girls’ test scores, and even had some externalities to boys’ performance in the same classroom. In contrast, a program to give textbooks to students in Kenya did *not* increase test scores on average (Glewwe, Kremer, & Moulin, 2007), a result that contrasts sharply with the previous literature (see e.g. Lockheed & Hanushek, 1988; even

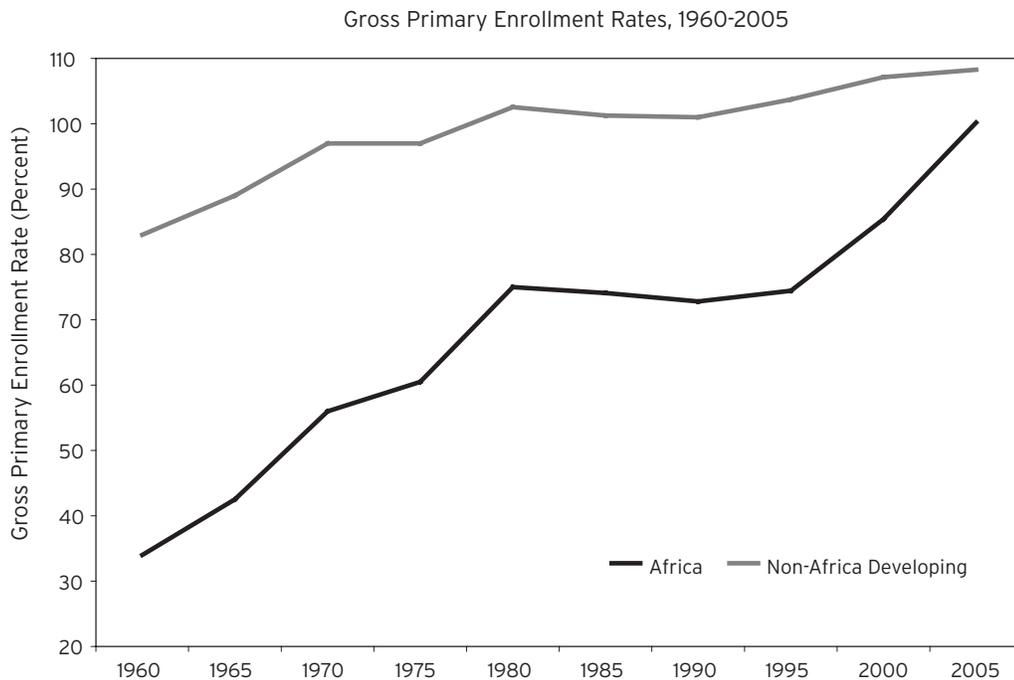
education skeptics like Filmer and Pritchett, 1999b argued there was a high payoff from textbooks). The authors argue that Kenyan schools were oriented towards the strongest students (whose test scores did improve), while the weaker students suffered from lack of English skills (textbooks were in English) and greater absenteeism of both pupils and teachers. Vermeesch (2003) found that a school meals program in pre-schools in Kenya raised attendance rates from 21 percent to 29 percent. It did not raise test scores on average, but did raise scores in schools with better-trained teachers. Note that the conditional conclusions in this paragraph are examples of ex-post slicing of the sample that this article discussed above as a sacrifice of econometric rigor in REs (including KMT 2007, where the merit scholarship worked in one sample site and not in the other).

As noted earlier, Angrist et al. (2002) studied the effect of vouchers for private school distributed via a lottery in Colombia. The lottery winners had 0.12-0.16 additional years of schooling, test scores higher by 0.2 standard deviations, and higher secondary school completion (the latter confirmed in a follow-up study by Angrist, Bettinger, & Kremer, 2006).

This only scratches the surface of randomized studies on education inputs as shown by a statement like Kremer and Holla (2008):

Evidence is also now accumulating on the effectiveness of certain school inputs like extra teachers and textbooks (Banerjee et al, 2005; Duflo, Dupas and Kremer, 2007; and Glewwe et al, 2007), and provider incentives (Glewwe et al, 2008; and Muralidharan and Sundaramanan, 2007), remedial education (Banerjee et al, 2007; Duflo et al, 2007; He et al, 2007), citizens’ report cards, the hiring of contract teachers, or increased oversight of local

Figure 7: Relative education performance in Africa



school committees (Bjorkman and Svensson, 2007; and Duflo, Dupas, & Kremer, 2007), school choice programs (Angrist et al, 2002, 2006; Bettinger et al, 2007).

It looks like the RE literature has offered a lot of particular aid interventions that “work” in education. Yet there is an air of randomness about which interventions work and which don’t, since the intuition distinguishing the two is not compelling. One has the worry stated earlier that such laundry lists of results tend to select out significant coefficients without enough information about how many different outcomes were tested, what results were based on ex post slices of the sample, and how many results depended on inclusion of covariates. Also the worry about how RE findings are very sensitive to context remains relevant.

Deaton (2007) is articulate on this issue concerning one of the interventions cited here:

The effectiveness of flip charts clearly depends on many things, of which the skill of the teacher and the age, background, and previous training of the children are only the most obvious. So a trial from a group of Kenyan schools gives us the average effectiveness of flip charts in the experimental schools relative to the control schools for an area in western Kenya, at a specific time, for specific teachers, and for specific pupils. It is far from clear that this evidence is useful outside of that situation.

Other RE studies seem more convincing and more robust. The famous Progres program in Mexico to give

cash grants to poor families in return for them keeping their children in school (subsequently known as conditional cash transfers, or CCT) has led to several influential studies using a randomized design. Schultz (2004) found that schooling among the beneficiaries did increase significantly, estimating the long run effect as 0.66 additional years of schooling on top of a baseline of 6.8 years of schooling. Behrman et al. (2005) came up with a similar estimate of 0.7 additional years of schooling using different methods (including effects on dropout, re-entry, and grade repetition rates). There are a few pilots of CCTs underway in Africa that are being evaluated with a randomized design, but none of the evaluations are available yet as of September 2008.²⁷ These studies seem more persuasive because they align well with theory—a sufficiently large incentive to keep kids in school, created by Progres, trumps the incentive for families to use children as workers to earn income.

Another famous RE finding on education offers some claims to robustness. The Kremer and Miguel 2004 study on treatment of children for worms in Kenya found that it reduced school absenteeism by one-quarter (although it did not improve test scores). An interesting historical confirmation of this result is Bleakley (2007), who discusses the Rockefeller Foundation campaign against hookworm in the American South in the early 20th century. Bleakley also found strong effects on school attendance from decreasing worm infection. Bobonis, Miguel, and Puri-Sharma 2006 found that treatment of children with iron supplements, Vitamin A, and deworming for anemia reduced student absenteeism in pre-school by one-fifth in a district in India. This finding is thus an example of one that was successfully replicated in different settings.

So one is left with the conclusion that some things work, but only under the right conditions, and only if

they are actually implemented as opposed to falling prey to dysfunctional education systems. Even then, the REs are not directly relevant to the question of whether aid explained the relative success of education in Africa 1960-2005, since we have no information on whether the donors did the interventions that REs evaluate. At best, the multiplication of “interventions that work” shifts priors that “donor efforts can pay off in education.” If so, then together with the stylized fact that enrollments rose in Africa at the same time as there was extensive donor involvement in African education, perhaps does shift priors that “aid works in education.” Otherwise, one is left with the feeling that aid *could* improve education, but the literature is not always that clear on when, why, or how.

Results from education

Despite Africa’s success on raising primary enrollment, there has been disappointment that growth in education has not paid off in higher economic growth, as stressed in Pritchett (2001). Education has its own micro-macro paradox, as Mincer regressions usually show a positive impact of an individual’s educational attainment on their wages, but results from growth regressions and growth accounting suggest little or no aggregate payoff to society-wide education. Africa plays a large role in Pritchett’s results, as it contributes several dozen observations with low economic growth and rapid percent growth in schooling attainment. Krueger and Lindahl 2001 contradicted Pritchett with much more positive results showing a positive association between the absolute change in years of schooling and economic growth—this put the African low growth observations more in the middle of the sample compared to being at the top of the sample on percent growth in years of schooling, since initial schooling in Africa was so low. However, Pritchett (2006) lets micro and macro data arbitrate the functional form and

finds that the best fit is closer to percent change than to absolute change. The poor outcome of educational improvements in Africa is consistent with the stylized fact that there is little job creation in the African formal private sector, which would normally be the employer of skilled labor (Pritchett 2006). Poor institutions could explain such an outcome, and poor institutions could also divert skilled labor into rent-seeking rather than productive activities. Even those who argue strongly for a positive effect of education on growth concede that poor institutions and policies, as in Africa, prevent education from paying off (Hanushek & Wößmann, 2008).²⁸

Another well-known and long-standing finding in the growth regression literature is between initial schooling (usually the primary enrollment rate) and subsequent growth rate, controlling for per capita income (Barro & Sala-i-Martin, 2003). Doppelhoffer et al. 2004 find that initial primary enrollment is the single best performing variables in a Bayesian exercise to decide what variables belong in the growth regression.²⁹ Hanushek and Kim (2000) and Hanushek and Wößmann (2008) stress initial quality (as measured by test scores) of education, and get stronger results with their test score variable than those for initial enrollment. Low primary enrollment quantity could have also been proxying for low schooling quality, since a dysfunctional education bureaucracy would plausibly produce both low quantity and low quality.

However, Bils and Klenow (2000) had already raised some doubts about whether the relationship between education level and growth was causal, noting that the coefficient magnitude could be explained entirely by individuals' investing in education in anticipation of high growth (which obviously raises future returns to skills). Easterly (2001) and Pritchett (2006) pointed out that a causal relationship between ini-

tial schooling and growth would predict accelerating growth with rising education in all developing regions, whereas the sample mean growth rates actually fell instead from the 60s through the 90s. Even if we accept as credible worldwide evidence on some growth payoff to initial level of schooling, there is considerable disappointment for Africa that this payoff has not materialized despite successful efforts at expanding schooling, which again could reflect poor quality of schooling and/or low demand for skills related to poor institutions.³⁰

We are left with little reason from the aggregate empirical literature to believe that rising education in Africa has paid off in higher per capita income or growth. This disappointment weakened the arguments of advocates of "marginal" project interventions and strengthened the case for "transformational" systemic changes, as we will see in the next section.

Health

Trends on health in Africa

Health is an even more clear success story than education in Africa, as child mortality has improved dramatically over time (Figure 8). There are well known and striking donor success stories, like the elimination of smallpox, the near-eradication of river blindness and Guinea worm, the spread of oral rehydration therapy for treating infant diarrheal diseases, DDT campaigns against malarial mosquitoes (although later halted for environmental reasons), and the success of WHO vaccination programs against measles and other childhood diseases. The aid campaign against diseases in Africa (known as vertical health programs, see discussion below) is likely the single biggest success story in the history of aid to Africa (see Levine, 2007).

In this case, the clear verdict of the case studies is probably a lot more helpful than the aggregate stylized facts, aggregate econometrics, or REs. Under-five mortality fell dramatically in Africa, but it fell by somewhat less than in other developing countries (figure 8 again). We ideally need to parcel out factors such as Africa's lower growth (although the effect of growth on health is controversial), different disease ecology (for example, malaria is much more of a problem in Africa than any other region), other factors, and aid, not to mention finding an identification strategy to assess causal effects of aid; no such aggregate econometric efforts have been notably successful. Even with econometric support unavailable, perhaps Africa's health performance is impressive after all given its lower growth and its more difficult disease ecology, which is consistent with the important role for aid shown by the case studies.

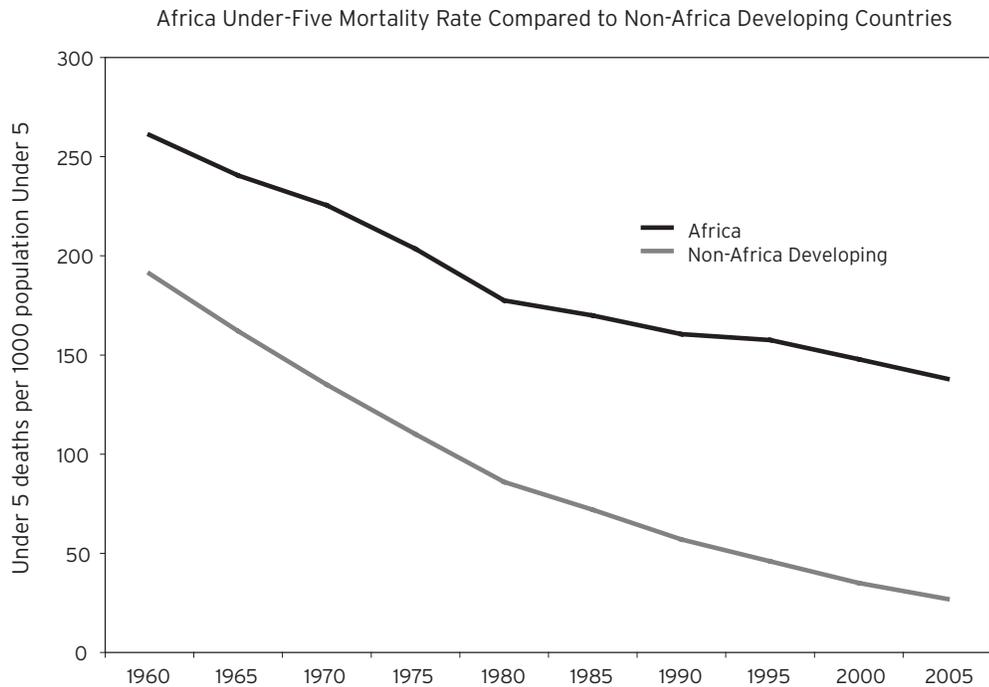
There is another sense in which the West had a major effect on health in Africa. The major technological breakthroughs in health—e.g. antibiotics, vaccines, the germ theory of disease, the identification of mosquito transmission of malaria, later the discovery of the AIDS virus—originated in the science of the West (see discussion in Cutler, Deaton, & Lleras-Muney, 2006). The health improvements in Africa would have been impossible without Western science; this is one important way in which Western outsiders did indeed “save Africa,” at least in one specific area. Acemoglu and Johnson (2007) show empirically the strong effect of the international epidemiological transition on changes in life expectancy after 1940.

Finally, randomized evaluations of also found positive impacts of a number of health interventions adopted by aid agencies or NGOs. First, many of the education interventions discussed above also had a health component. Gertler (2004) checked whether the Progresca cash-for-schooling program also had a major health

impact, since the cash rewards were also conditional on families receiving micronutrients and protein supplements, and bringing their children to clinics for regular health and nutritional checkups. For children covered by the program compared to the randomized control group, Gertler found significant effects of a 22-25 percent decrease in probability of illness in the four weeks preceding the checkup, an impact on child height of 1 centimeter (although, puzzlingly, not a significant decrease in probability of stunting), and a 25 percent decrease in probability of anemia. The nutritional success is notable when we remember that knowledge of the large payoff to cheap nutritional supplements has been around for decades (like Vitamin A in Table 5), and yet these still remain underutilized. These findings comprised another large part of the “Progresca Success Story” discussed above. The Bobonis et al. 2008 study on anemia and school participation also found that iron supplements and deworming drugs were effective in increasing children's weight-for-height and weight-for-age scores. This might seem to be obvious, except the impacts on direct measures of anemia and worm infection were surprisingly insignificant. (Again, one worries about a pattern of some outcomes being significant—hence the intervention “works”—but other equally plausible ones are not. This makes it more difficult to interpret the significance level of a conclusion that an intervention “works.”)

In contrast, the well-known Kremer and Miguel paper showed a strong effect of deworming on worm infection rates in a district in Kenya, which reflected not only direct effects on children receiving the drugs but also surprisingly strong externalities to others in the same school or nearby schools. Bleakley 2007 also noted the strong and immediate effects of the Rockefeller deworming campaign in the American South.

Figure 8: Relative health performance in Africa



Another area where REs point to success is in preventing or treating infant diarrhea (Zwane & Kremer, 2007). Breastfeeding, immunization against diarrheal diseases, micronutrient supplementation and oral rehydration therapy (ORT) have all been found to work in randomized trials in the fight against diarrhea. Unlike the education interventions, we know from case studies that these interventions were pursued by donors. Case studies suggest ORT is another health aid success story, accounting for a substantial drop in diarrheal mortality since 1980. REs seem to be more persuasive in health, but for reasons that also make them less necessary. The link between medicines and health is often so obvious that it doesn't require an RE to verify it. Still to be as generous as possible, taken together, the various kinds of evidence support some positive effect of aid on health.

Approaches to improving health through foreign aid

Horizontal vs. vertical

Despite this success, there are huge health problems in Africa that aid agencies are still trying to solve. There has been throughout the history of foreign aid a tension between two alternative approaches to health. The "vertical" approach focuses on one disease at a time, marshalling a top-down mass campaign against the disease through targeted prevention measures, vaccination if applicable, and medicines for treatment. As just mentioned, it was extraordinarily effective in taking the initial strides against the target disease. However, the vertical programs were not sufficient to resolve Africa's health crisis, because each program eventually reached some point of diminishing returns

where there remained a segment of the population beyond its reach. In some sense, the health aid field has never figured out what to do next after diminishing returns to vertical programs set in. Table 6 shows the gaps that still remain in health coverage in Sub-Saharan Africa (as well as the average for the comparator group of all low income countries, which does not appear to be significantly different). There is a good news/bad news character of this table—coverage rates of 70-80 percent are reached in immunization and nutritional supplements, which is considerable progress compared to zero, but one still wonders why 20-30 percent of all African children fail to receive such well-known cheap and easy remedies for life-threatening conditions (such as our 70-year-old standby from Table 5, Vitamin A).

The “horizontal” approach focused on making the health system work well to administer prevention and treatment to patients rather than diseases, whatever the patient’s disease may be. Horizontal advocates criticize the vertical programs for ignoring implementation problems with health projects in general, and for potentially crowding out less costly treatment for more widespread illnesses with more costly treatment for less common diseases. Defenders of the vertical programs can point to many of the health successes mentioned above; horizontal critics of vertical programs point to their severe diminishing returns, namely the continuation of high mortality rates in Africa from preventable and treatable diseases and the health gaps shown above.

The history of health aid is a cycling between these two alternatives. After the early “vertical” health successes described above ran into diminishing returns, there was a switch to the “horizontal” approach. By 1980, the World Bank had shifted towards recommending an “integrated approach” in health (i.e.

horizontal), which continued for the next two decades. The 1993 World Bank WDR on health, for example, stressed the health system problems described above as a critical bottleneck in improving health.

By the new millennium, however, the prominent health crisis of AIDS in Africa induced a shift back towards vertical, disease-specific programs, such as the creation of the Global Fund to fight AIDS, TB, and Malaria (GFATM) in 2002, the US President’s Emergency Plan for AIDS Relief (PEPFAR) in 2003, the President’s Malaria Initiative (PMI) in 2005, and the Gates Foundation’s well-publicized efforts on these same diseases, which implied large increases in health aid but mainly in these vertical programs. There have been some successes from these programs, such as the life-saving treatment of more than 1 million HIV positive Africans (Sachs, 2008). However, critics have complained that the concentration of foreign aid on AIDS, in particular, has crowded out more cost-effective approaches to more common diseases, not least because the AIDS initiatives may have overwhelmed the still dysfunctional public health systems. For example, a group of health experts wrote in the prestigious medical journal the *Lancet* in July 2003 about how 5.5 million child deaths could have been prevented in 2003, lamenting that “child survival has lost its focus.” They blamed in part the “levels of attention and effort directed at preventing the small proportion of child deaths due to AIDS with a new, complex, and expensive intervention” (Gareth Jones et al., 2003). England (2008) points out that while AIDS causes 3.7 percent of mortality, it gets 25 percent of international healthcare aid. Even within AIDS programs, prevention is neglected relative to treatment, even though the former has far better cost/benefit ratios (Canning, 2006). Moreover, AIDS funding is increasing even further—President George W. Bush signed a bill in July 2008 giving an extension of his original 2003

Table 6: Most recent health indicators for Africa compared to all low income countries

	Africa	Low Income
Acute Respiratory Infection treatment (% of children under 5 taken to a health provider)*	43	44
Children with fever receiving antimalarial drugs (% of children under age 5 with fever)**	38	23
Diarrhea treatment (% of children under 5 with diarrhea receiving oral rehydration and continued feeding)*	38	40
Immunization, Diphtheria/Pertussis/Tetanus (% of children ages 12-23 months)**	71	67
Immunization, Measles (% of children ages 12-23 months)**	69	67
Vitamin A supplementation coverage rate (% of children ages 6-59 months)**	79	76

*median of all countries with data for 2000-2006

**regional or income group average provided by World Development Indicators for 2005

five-year \$15 billion PEPFAR program for another five years at \$30 billion (still heavily skewed towards treatment). AIDS is a good example of how the vertical approach is vulnerable to capture by lobbies for particular diseases that are “fashionable” causes in rich countries but don’t necessarily match the aid recipient’s priorities.

The World Bank (2007) responded by again fervently advocating the horizontal approach. The large new vertical programs would not work unless there was an “urgent effort ... made to strengthen health systems” (p. 15). But the G8 Summit in July 2008 in its discussion of health in Africa stubbornly stuck with vertical: “G8 members are determined to honor in full their specific commitments to fight infectious diseases, namely malaria, tuberculosis, polio and working towards the goal of universal access to HIV/AIDS prevention, treatment and care by 2010.”³¹

The cycling between vertical and horizontal approaches could be another example of inability to learn characteristic of the transformational ap-

proaches in foreign aid to Africa. There were both vertical and horizontal advocates who hoped for “transformational” results. The rhetoric of vertical programs often implied absolute and improbable goals without regards to cost-benefit analysis, such as wiping out a disease altogether (such as malaria) or providing universal access to treatment for that disease (such as AIDS). For its part, the unrealistic ambition of the horizontal approach is similar to that of “capacity-building” discussed above—changing the health civil service is no easier than changing the rest of the civil service.

User fees in health

Another long-standing debate in health is whether to charge patients user fees for health services. The World Bank orthodoxy in the structural adjustment era of the 1980s and 1990s was that user fees in health (as in other sectors) were desirable, since they avoided subsidization of wealthy patients and allowed programs to collect more revenue and reach more beneficiaries. The World Bank retreated from this po-

sition under attack in the late 1990s from NGOs who found the idea of charging for life-saving services morally offensive.

The debate shifted in the new millennium towards a pragmatic and evidence-based debate about whether user fees were successful in screening out people who did not value the health service, may have had an additional behavioral effect on patients actually using the health input (through the sunk cost effect documented in behavioral studies), and/or the fees may have allowed the health service to reward distributors for making sure the inputs were available to patients. On the other side, there was a good public economics argument for subsidizing health inputs that had major external effects, such as prevention and treatment for infectious diseases.

This is the kind of debate where the RE literature claims to deliver a clearer message to policymakers and does not allow them to cherry-pick studies for support for their favorite policies or interventions. It is also supposed to deliver a clearer verdict than the sometimes inconclusive debates between academics about empirical findings. Unfortunately, things did not work out so cleanly, as pointed out by Rodrik (2008) and others. For example, advocates of providing development goods for free often quote the Cohen and Dupas (2007) study that finds that going from free provision of bed nets to charging 75 cents per net (still heavily subsidized) reduced uptake by 75 percent. Supporters of charging for development goods have cited the Ashraf et al 2007 finding that charging for water purification tablets was successful in screening out those less likely to use them.

The general conclusion that demand for health inputs is very sensitive to price in Africa seems on firmer ground. Kremer and Miguel (2007) found that mod-

est user fees for deworming drugs reduced take-up rates by 80 percent in Kenya. Kremer and Holla 2008 argue that the pattern of take-up being very sensitive to price is consistent with a number of RCT studies of interventions: not only deworming and bed nets, but also learning the results of HIV tests. They point out that the evidence from Progresa of significant health, nutrition, and education responses to relatively small subsidies is also consistent with the same high price elasticity hypothesis. Even the Ashraf et al. 2007 study did show a high price elasticity for water purification tablets (even if price is successful at screening out those less likely to use them, there remains the question of why there are so many who don't want to use water purification tablets). This is also an area where RE studies are likely to be more productive since they focus on behavioral parameters like the price elasticity of demand, as opposed to the average response to a seemingly random list of development interventions that could generate many different behavioral responses.

The REs were useful in that they focused research on a new puzzle: what explains what seem to be irrationally high price elasticities in health? The first explanation is from behavioral economics, emphasized by Kremer and Holla (2008), where irrational responses to the availability of relatively cheap life-saving treatment can be changed by a "nudge" in the right direction. The second possibility, as Kremer and Miguel (2004) discuss, is that this extreme sensitivity to price may reflect the local state of knowledge about health, in which disease is viewed through the lens of traditional cultural beliefs and little value is placed on modern scientific medicine. If this second explanation is true, then the high price elasticity is not good news for cost-sharing programs, but the news is not so good for free provision either, since modern medicine will be heavily under-utilized even if it is free (especially

considering it is not really free when getting and administering medicine is time-consuming). There have long been anecdotes about malaria bed nets being used as wedding veils and fishing nets, for example; Minakawa et al 2008 give more systematic evidence of free insecticide-treated bed nets donated by an NGO in Western Kenya on Lake Victoria being diverted to uses such as drying fish and fishing nets. Given the drastically different implications of the behavioral versus health knowledge explanations, the literature needs even more discussion and testing of them.

Water and sanitation infrastructure

I use water and sanitation as an example of aid's approach to financing infrastructure, since in this sector we have a clear welfare indicator linked to aid-financed infrastructure projects.

Trends on water and sanitation indicators

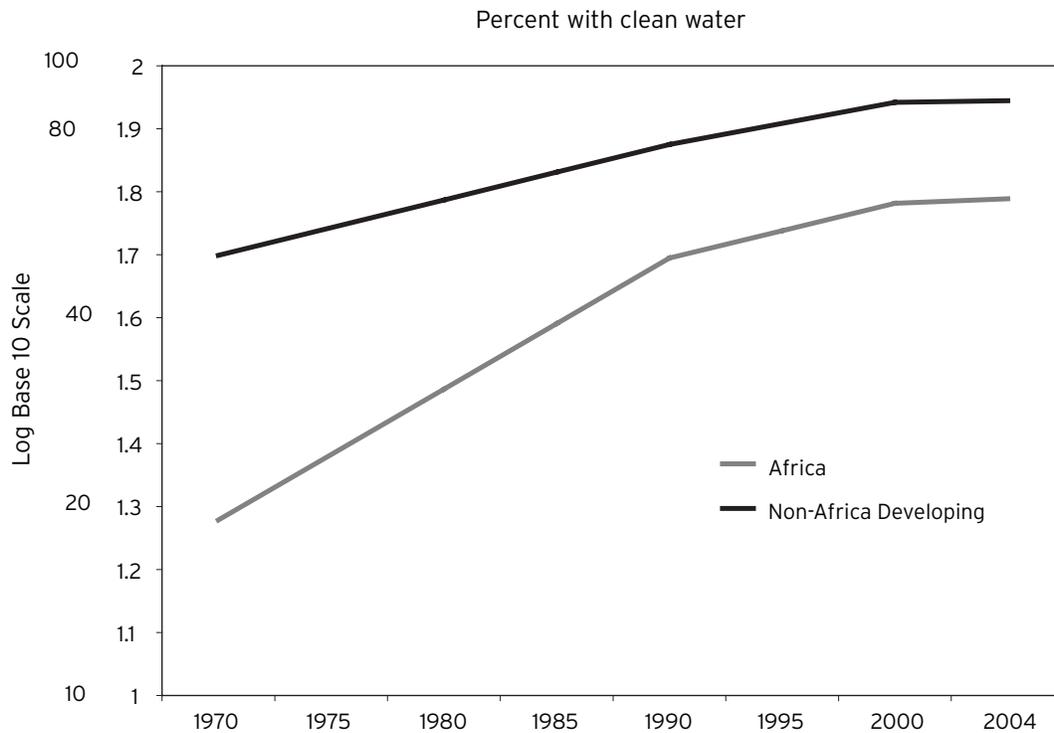
The trends on water and sanitation in Africa are similar to those of the other social indicators. There has been success in increasing the percent of Africans with access to clean water, as in other developing countries.³² This could suggest some success of aid-financed water projects, or it could be consistent with a worldwide tendency for improvement in access to clean water in poor countries unrelated to amount of aid received in each country. Again, it is informative to review stylized facts but they fall well short of the kind of detailed attribution evidence that would make it possible to evaluate aid agencies' efforts.

The randomization literature has contributed some insights into the area of clean water provision. If the goal of clean water provision is to prevent water-borne disease, there may be smaller-scale programs that are more cost-effective under some circum-

stances than the infrastructure traditionally favored by donors—large scale distribution systems with water pipes or massive efforts to sink boreholes.

REs identified some smaller-scale programs that have strong effects on clean water provision. As already noted, Ashraf et al. (2007) noted that water purification tablets in Lusaka, Zambia were an inexpensive way of avoiding water-borne illness. Zwane and Kremer (2007) suggest behavioral changes such as hand-washing and disinfecting the household's own water were more effective in rural areas than formal infrastructure. Hand-washing and other hygiene behaviors may be necessary even if there is clean water infrastructure, to avoid re-contaminating the water, although evidence on this is ambiguous (Kremer & Zwane, 2007). However, how to induce such behavior change is still unclear. Kremer et al. 2008 showed that investments in protecting naturally occurring springs from contamination led to dramatic improvements in water quality in rural Kenya (as measured by the fecal indicator *E. Coli*). However, the higher communal water quality at the springs did not seem to pay off at the household level, perhaps because of recontamination through household behavior, as there was no effect on diarrhea, or child height and weight. We have already seen that there was high price elasticity for water purification tablets, and Zwane and Kremer describe how behavior changes such as hand-washing and purifying water were surprisingly difficult to achieve in poor households. Another study by Kremer et al. 2008 in rural Kenya estimated household willingness to pay for clean water (estimated through transportation costs to protected springs that were clean compared to those that were not) as coming out only to \$0.86-\$1.72 per case of diarrhea avoided (surprisingly low when diarrhea from water-borne diseases is a life-threatening condition for infants.) As in the health area, there could be lack of knowledge among

Figure 9: Relative infrastructure performance in Africa



the poor of the scientific mechanisms that make clean water desirable. This anomaly is again perhaps the most interesting result from the RE literature on clean water, pointing to a new area where researchers and aid workers could search for solutions which may not have been so compelling without these RE studies.

Changing fashions in infrastructure aid

In the early days of aid, the emphasis in infrastructure aid was simply on increasing the quantity of physical infrastructure. By the time of the 1994 World Bank WDR on infrastructure, the emphasis had changed to emphasize problems like inadequate maintenance

and allocation of scarce funds to “white elephants.” Despite this change in emphasis, there has not been much progress on improving maintenance, as described earlier. On “white elephants,” the Bank has used its Public Expenditure Reviews as its traditional tool to redirect aid away from unproductive boondoggles towards productive infrastructure. The problem of fungibility has meant that cutting off aid financing to a particular project does not necessarily succeed in killing the project, since the government can turn to other donors or use its own funds (famous white elephants include the new national capitals built in Cote d’Ivoire and Nigeria, the state-owned \$5 billion Ajaokuta steel mill in Nigeria begun in 1979 that has yet to produce a bar of steel, and the building of an

international airport in Eldoret, Kenya—the hometown of Kenya’s long-time autocrat Daniel Arap Moi). The white elephant problem has probably become worse because of the influx of aid from China into Africa, much of it directed to infrastructure and with even fewer restrictions on allocation of spending.

Recently, the focus on quantity of infrastructure spending has returned, with advocates of an increase in aid justifying it in part by the need to pay for better infrastructure for Africa (UN Millennium Project, 2005; Blair Commission, 2005; Sachs, 2005; Sachs, 2008; Collier, 2007). The Blair Commission (2005), for example, said that Africa needed \$10-\$20 billion a year in additional Western aid for its infrastructure quantity needs (p. 49).³³ The World Bank web site on clean water stressed quantity of funds and investment in 2008:

Finding new sources of finance will be critical to expanding access to urban water supply and sanitation (WSS). Present investment towards the WSS Millennium Development Goals is only half what is needed, and all sources of investment finance for the sector have been declining.³⁴

The return to emphasizing infrastructure quantity does not seem to acknowledge the micro empirical research cited above. The quantity emphasis also seems to disregard common sense principles of cost-benefit analysis in favor of opaque calculations of “what is needed.” The recent micro empirical literature also emphasizes the chronic and seemingly insoluble maintenance problems, like those mentioned above—a third of all South Asian water infrastructure and half of all boreholes in Western Kenya were found to be not functional in recent reports. New fads such as community-based maintenance schemes have little evidence to support their effectiveness. Obviously, inability to solve the maintenance problem sharply lowers the

payoff to quantity of physical infrastructure (Zwane & Kremer, 2007). Low tech solutions like those discussed above (hand washing, water purification tablets, spring protection) are accordingly even more attractive if the behavioral changes could be achieved. In this area, the RE results were useful because they were intuitive, suggested new problems to solve, and questioned an aid agency bias towards financing physical infrastructure that was not based on evidence.

Another cycle with infrastructure (like in health) was that between free public provision, public provision with user fees, and private provision. In infrastructure, free public provision had been the default assumption in the beginning of foreign aid. The World Bank (and to some extent the IMF) began to point after 1980 to the advantages of user fees in having non-poor users of public services help raise revenue for these goods (which could then be used to subsidize the poor). About the same time, the potential role of the private sector began to seem more promising and there were privatizations of public utilities like water and electricity (and there was also gradually growing awareness that the private sector played an important role in providing health and education despite the existence of public services). All of this was sharply reversed after the late 1990s under pressure from NGO critics and globalization protesters who were scandalized that anybody should have to pay for basic necessities like water. Hence, the cycle has swung back to free or heavily subsidized public provision in infrastructure. This is an unfortunate triumph of demagogic rhetoric over evidence, since both case studies and rigorous micro studies show positive results from privatization of some utilities. For example, the privatization of water services in Argentina was associated with a 5 to 7 percent drop in infant mortality in Argentina according to one recent study (Galiani, Gertler, & Schargrodsky, 2005).

Agriculture

Agriculture is an area that has long attracted attention from those who want to help Africa (see the quotes from Lord Hailey, 1938 above). The success of the "green revolution" in Asia in the 1970s was tantalizing to aid donors, who hoped for similar results in Africa. Yet African agricultural aid is also unusual in that virtually all those involved agree that it has been a failure, amidst much recrimination and finger-pointing. The stylized facts on food production per capita certainly influence this pessimism, with a decline in Africa contrasting with the general Asian rise (Figure 10).³⁵ Of course, there are the same problems with negative outcomes as with positive outcomes, that it is hard to resolve attribution of outcomes to aid vis-à-vis other factors such as policies followed by African governments, world market conditions, climate, etc.

Eicher and Baker (1982) noted a quarter century ago that Africa was the only region that experienced declining food production per capita over the preceding two decades, a situation they labeled "Africa's food crisis." Soil fertility, erosion, and deforestation continue to be problems, although some technical solutions have been known for at least 70 years (Table 5 on the 1938 Hailey Report again.) Periodic World Bank task forces tried to remedy the situation. World Bank (1997) called for movement "From Vision to Action." World Bank (2003) is the report of another task force called "Reaching the Rural Poor," which noted

...the agricultural development portfolio has not yet met the 80% satisfactory development outcome rating at completion, as targeted by "From Vision to Action." The quality of the poverty focus, and the sustainability and quality of the institutional development still leave much to be desired. Reaching the Rural Poor will address these concerns.

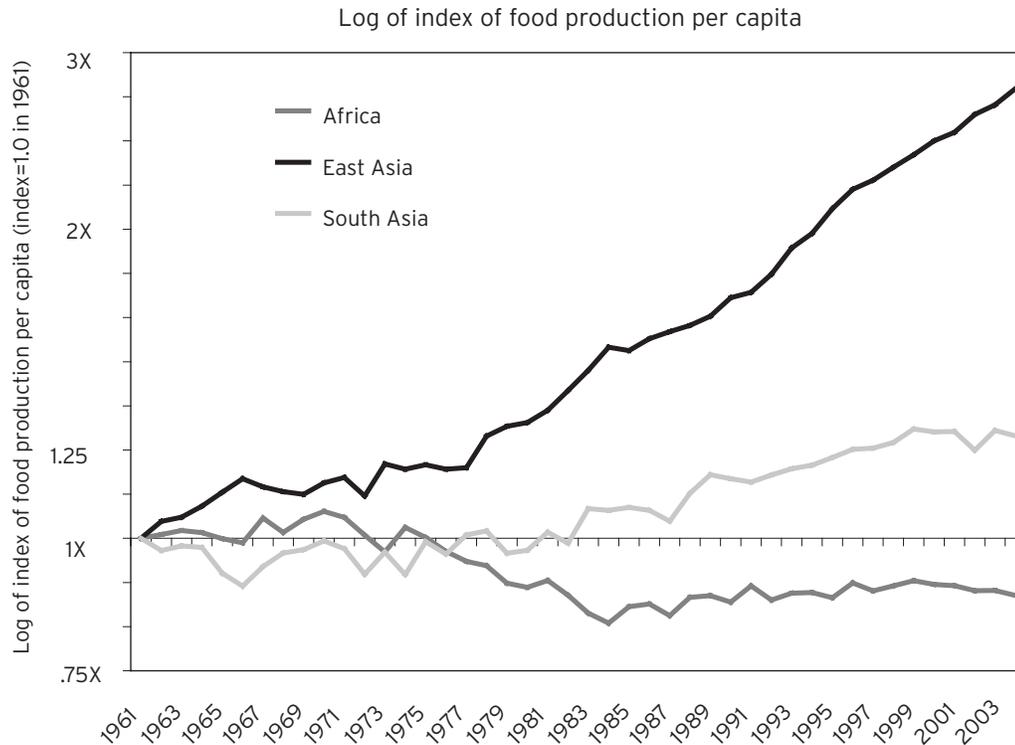
The latest report, an internal evaluation of all World Bank work in agriculture over the period 1991-2006 (World Bank Independent Evaluation Group, 2008), was again scathing about failure. The 2008 World Bank WDR (p. 15) in turn noted the stagnant cereal yields in Africa in contrast to rising yields in all other regions.³⁶ WDR 2008 noted the existence of "'agroskepticism' of many donors" which "may well be related to their experience with past unsuccessful interventions in agriculture." Similarly Eicher and Kane (2004) noted "The failure of past initiatives in agriculture led to a reduced confidence among donors in agriculture in the 1980s ...and many donors have since turned to other sectors."

The UN system has followed a similar progression, with a World Food Summit in 1996 another installment in a long line of efforts to make progress on hunger in Africa through agricultural development. The FAO (2006) passed judgment on that effort: "Ten years later, we are confronted with the sad reality that virtually no progress has been made towards that objective."

As far as the "green revolution" specifically, Eicher (1999) had already noted that:

Much energy has also been wasted in trying to replicate Asia's Green Revolution model in Africa before the completion of pilot studies. Over the past decade, many instant experts on Africa have talked glibly about the ease of replicating Asia's Green Revolution model in Africa. Many of these experts have overlooked Africa's early stage of scientific development, falsely assuming that Africa had the requisite infrastructure, irrigated land, trained scientists, technology, and national and local institutions to replicate the Asian model.

Figure 10: Failure of food production in Africa relative to Green Revolution in Asia



The Wapenhans report (World Bank, 1992) confirmed this picture, with only 40 percent of World Bank agriculture projects in Africa judged as successful (compared to 59 percent for all projects in Africa, and 72 percent for African education projects).

The attempt to jump-start African agriculture has involved many different interventions from subsidized fertilizer and heavy investment in agricultural R&D and extension services in the 1960s to “integrated rural development” (an attempt to deal with all the complementary inputs at once) in the 1970s, to a shift away from public support for farmers towards market forces in the 1980s and 1990s with “structural adjustment,” to renewed interest in the new millennium in

more agricultural R&D (again) and fixing “market failures” (again) in inputs such as fertilizer and improved seeds (the cyclical nature of aid ideas is again evident).

The pattern of actual aid to African agriculture has followed the Bank’s “agro-skepticism” description, with a sharply falling share in total aid to Africa (with the winner appearing to be the social sectors whose share has risen sharply). Agricultural scholars have severely criticized donors like the World Bank and USAID for the diminished attention to agriculture in Africa, and have blamed international NGOs for lobbying for special causes (most of which imply more social spending in aid); these criticisms gained traction with the current world food crisis.

A more positive spin on the changing sectoral shares of aid is that the aid agencies were responding appropriately to areas of relative success and failure. We have seen that there is more ground for seeing some success in the social sectors, so the reallocation of aid to social sectors from the failing agricultural aid sector could be seen as a constructive move to maximize returns from aid.

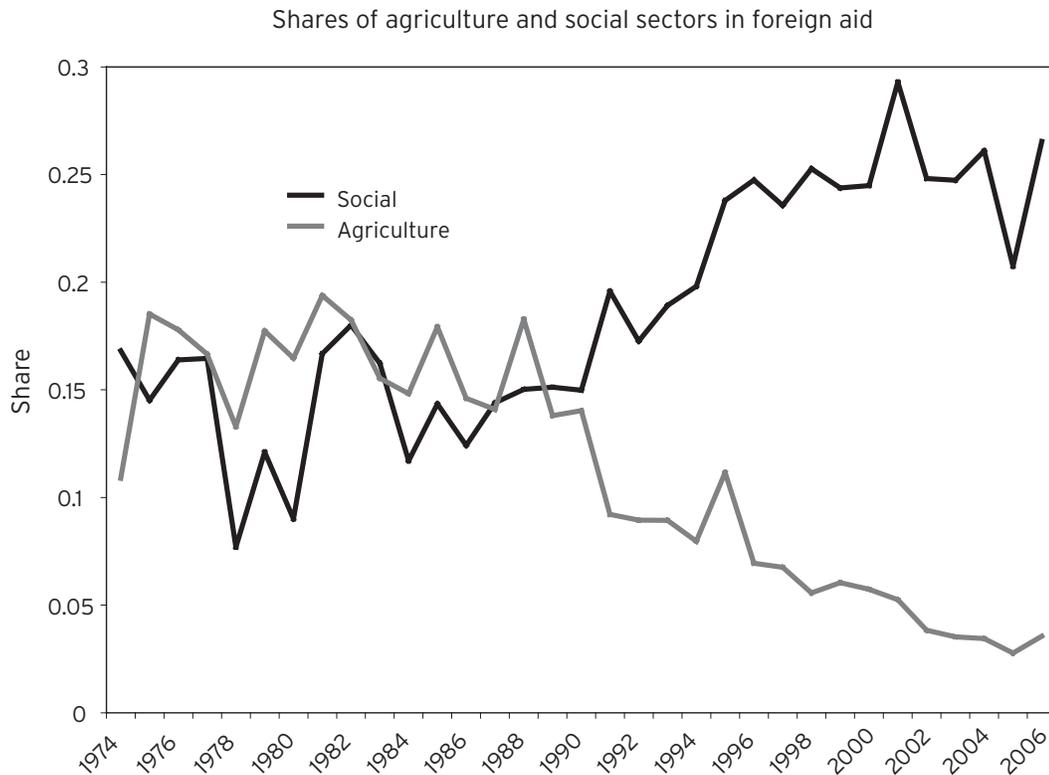
However, neglect of any high-profile problem like African agriculture sooner or later results in countervailing pressures, so aid agencies and private foundations are now making renewed efforts to treat the ills of African agriculture. Reports from the UN Millennium Commission and Blair Commission on Africa in 2005, not to mention the World Bank's 2008 WDR on agriculture, put a lot of stress on solving problems of African agriculture. The Bill and Melinda Gates Foundation (2006) recently announced a "Green Revolution" initiative towards that end. The crisis of sharply rising food prices in 2008 put even more pressure on donors to restart old agricultural development programs. International summits are again a preferred vehicle for action, despite their ineffectual track record. At another World Food Summit sponsored by the FAO in response to the food crisis of 2008, donor agencies and 180 governments said in their joint statement "We reaffirm the conclusions of the World Food Summit in 1996."³⁷ The G-8 Summit in July 2008 said "we will reverse the overall decline of aid and investment in the agricultural sector" in Africa, as well as "promote more agricultural research and development," and a "Green Revolution."³⁸ The recycling of failed ideas in the transformational approaches is very stark in African agriculture.

In contrast, RE studies have shed light on some of the marginal steps that could pay off in agriculture. One problem that is being studied is the chronically low

use of fertilizer by African farmers, compared to other regions. Duflo, Kremer and Robinson 2008 study the hypothesis that the return to fertilizer on real world maize farms in Kenya is lower than the high returns on pilot farms, using REs of actual farms at different dosages of fertilizer. They found high returns also on real farms, although it required a kind of Goldilocks conclusion—too little or too much fertilizer makes the return unfavorable, but using just the right amount yields a large positive return. The official Kenyan government recommendation is NOT at the right amount and would yield poor returns. These results could suggest the problem with fertilizer under-utilization is due to missing technical knowledge on how much fertilizer to apply, but this is puzzling given the high private returns to acquiring such knowledge. In contrast, Conley and Udry 2007 document farmers learning how much fertilizer to apply from their successful neighbors in a new technology, pineapple growing, in Ghana (using spatial econometric techniques on a unique dataset of social connections among farmers).

An earlier study by Duflo, Kremer, and Robinson (2007) tests for behavioral explanations of low fertilizer use. They find what seems to be a savings commitment problem—farmers do not set aside money for fertilizer for the next season when they are flush with funds from the harvest in the current season. Selling a voucher earmarked for fertilizer purchases to the farmers right after the harvest seems to correct the problem. These two RE studies are notable for shedding light on behavioral models of farmers' fertilizer use, again in contrast to the frequent complaint about RE studies that they are not well-connected to good behavioral models. There is still the tension between irrational behavior explanations of low fertilizer use and the missing knowledge explanation, similar to the rival hypotheses we saw above in health. REs have usefully provided some evidence to make both viable

Figure 11: The shift out of agricultural aid into social sector aid



candidates, but more research is required to distinguish between the two hypotheses with such different policy implications.

Other micro studies (not REs) also show some potential in tackling some of the chronic problems of agriculture one at a time. A common concern about aid and agriculture is that food aid—giving food in kind for free—could harm local agriculture by driving down producer prices. If the poor are disproportionately small farmers, the longstanding fear in aid policy circles was that this kind of aid would perversely hurt the poor. Levinsohn and McMillan (2007) address this concern by analyzing a dataset with tens of thousands of household in Ethiopia, one of the chronic recipients

of food aid. They find that the poor are disproportionately likely to be net buyers of wheat, and buyers outnumber sellers at all levels of income. Hence, the food aid fears are misplaced at least in Ethiopia—food aid is helping more households than it is hurting, and even more so among the poorest households. However, they note that the same objectives could be attained and local farmers also benefited if food were purchased locally and then distributed to the poor. This has been a frequent recommendation of aid analysts and there has been some movement in that direction, but there is still considerable pressure to source food purchases from the donor economies because of rich country agricultural lobbies.

Conclusions on project interventions

The project record on aid is mixed, with some suggestive evidence of success in social sectors, and nearly universal agreement on failure in agriculture. Micro evidence is also consistent with success of project interventions, at least in social sectors. RE studies shed some light on some of these interventions, even if not as much as their proponents promised. This picture suggests that the marginal approach to fix one problem at a time or to assist individual Africans to get better health and education has a suggestive track record of success, as well as indications of future potential.

If this picture is accurate, an important research question that I cannot resolve here is why the results were so poor in agriculture compared to social sectors. I can't resist throwing out some suggestive hypotheses however: Perhaps the different types of problems in different sectors led to the application of the marginal approach in social sectors and the transformational approach in agriculture. The marginal approach was more feasible in social sectors because the easier attribution of observable success or failure in individual social sector projects compared to agriculture made it more feasible to monitor aid agencies in the social sector, strengthening incentives for good performance and inducing a resort to marginal approaches in some areas.

A related idea by Pritchett and Woolcock 2004 is that government services (and aid) perform the worst in areas that are both transaction-intensive and discretionary. So for example, vaccination programs worked fairly well because they were not discretionary (even though they were transaction intensive), because the implementing agents were performing a routine action. Similarly, a massive school-building program to raise enrollment was transaction-intensive but not

discretionary—the same school blueprint could be built everywhere. Agricultural extension, on the other hand, is both discretionary and transaction-intensive. The extension agent must deal with each individual farmer, and each farmer's problems are different, precluding a routine response. It was thus extremely difficult to monitor extension agent's performance, and incentives for good performance were weaker. The same logic could explain the areas of relative failure within health. The inability to make much project on strengthening health systems could be related to how highly discretionary and transaction-intensive are health systems. In terms of this paper, aid agencies are more likely to resort to transformational approaches in areas (or combinations of areas) that are both discretionary and transaction-intensive. The agencies will be rewarded for "big efforts" in these areas but it will not be feasible to assess the impact of aid agency actions (so that the transformational approach will persist whether it works or not).

The RE literature also can more readily assess payoff when it studies routine actions in aid—administering deworming drugs, conditional cash transfers, ORT, etc.—rather than discretionary actions, like agricultural extension. This hints at the probability that the RE literature is addressing the areas where aid was already working the best. REs can study incentives for teachers to show up to class, but not how well the teachers are doing the discretionary, transaction-intensive job of helping their students learn.

Another contentious issue on projects is how do you define "success"? This again reflects the tension between the marginal and the transformational view of Western aid. If success is defined as improving the well-being of a significant number of poor individuals, the project evidence is suggestive that there have been achievements on this score in Africa. If success is defined as improving the "Factor X's" in such

a strong fashion as to lift Africa out of poverty into steady growth towards prosperity, i.e. to “save Africa,” then the record is not so encouraging. The aid agencies seemed to have the transformational definition of success in mind by the late 1970s, since disappointment with project interventions led them to engage in attempts to induce more systemic changes in African countries, beginning in the 1980s, as we will see now in the next section.

The beginning of systemic interventions: structural adjustment

The disappointment with the apparently low growth payoff to project aid to Africa led the Western aid policymakers to get ever more ambitious, with attempted interventions in remaking the economic, political, and social system in Africa. The disappointing results on growth made more compelling the general equilibrium argument that it does little good to get an individual project working when overall systemic incentives for growth and development are very negative. For example, the designer of the Progres program, Santiago Levy (2008), is pessimistic about the long run results of Progres for its beneficiaries, because there would only be low-productivity informal sector jobs for them due to Mexico’s policy-induced labor market distortions. Similar conclusions could be drawn about any health or education intervention when the economy does not create opportunities even for those with higher human capital thanks to the intervention. There was also positive inspiration from the success of the Gang of Four (Hong Kong, Singapore, South Korea, Taiwan), where aid agencies gave credit for success to outward-oriented economy-wide policies. The targets for interventions began with economic policies, then institutions, and finally fixing failed states and resolving civil wars.

The arguments for systemic approaches were plausible and the movement from one target to the next could have reflected learning. Moreover, there definitely was some learning in the literature on determinants of development, such as the increased dismissal of mechanical models in favor of awareness of incentives created by first, economic policies and second, institutions. However, this development learning did not necessarily pay off in aid practice, as stylized facts and some of the more well-executed aggregate econometric studies (even if identification was a little shaky, as noted earlier) led to disappointment with each successive systemic approach among both academics and aid officials. Yet escalation continued anyway, consistent with an adherence of aid agencies to the transformational view.

Record on structural adjustment

Structural adjustment loans (SALs) were created in 1980 by the International Monetary Fund and the World Bank. These were loans whose funds disbursed rapidly, conditional on the recipient “adjusting” their economic (“structural”) policies. In Africa, the conditions came to focus on correcting a number of distortions that were prevalent in Africa: (1) artificial official exchange rates that implied real overvaluation of the domestic currency and; (2) foreign exchange controls that led to a high black market premium on foreign currency given the artificial exchange rate in (1); (3) controls on interest rates that led to negative real interest rates; (4) restrictive tariffs and quotas that gave very high protection to domestic firms and/or led to consumer goods rationing; (5) prevalence of inefficient state enterprises that required government subsidies and delivered few benefits for the economy; and, (6) high budget and current account deficits. Correction of these distortions implied devaluation, liberalization, fiscal austerity, and privatization, a

combination that became known in developing country policymaking as the “Washington Consensus” (a term coined by John Williamson). The argument for policy changes to supplement the project approach became stronger with a famous result shown by the 1991 World Development Report of the World Bank (1991), that rates of returns to projects were lower with bad policies (specifically, high trade restrictions, high foreign exchange premiums, and high fiscal deficits) compared to good policies (low values of the above). This result was eventually published as Isham and Kaufmann (1999).

However, the poor growth outcomes in Africa in the 1980s and 1990s caused much blame to be heaped on structural adjustment. The attempt to attain East Asia’s growth (or even respectable per capita growth) did not succeed, not only in Africa but also in Latin America and the Middle East. With this failure, the “imitate the stars” approach fell into disrepute with many academic observers, such as Dixit (2007):

At any time, some country is doing well, and academic as well as practical observers are tempted to generalize from its choices and recommend the same to all countries. After a decade or two, this country ceases to do so well, some other country using some other policies starts to do well, and becomes the new star that all countries are supposed to follow.

Hence, the backlash against structural adjustment coincided with a loss of confidence in the academic literature that researchers could identify policy actions that would raise growth. The early hope that growth regressions would identify growth-promoting policies ran afoul of concerns about data mining (see discussion above). So many of the world’s leading macroeconomists concluded in a conference called the Barcelona Development Agenda (2004): “there

is no single set of policies that can be guaranteed to ignite sustained growth.”³⁹ The World Bank (2005) itself accepted this agnosticism: “different policies can yield the same result, and the same policy can yield different results.” The World Bank followed this up by sponsoring a Growth Commission, whose final report appeared in May 2008, and contained a similarly agnostic conclusion: “It is hard to know how the economy will respond to a policy, and the right answer in the present moment may not apply in the future” (Commission on Growth and Development 2008, p. 29). This is not to say that these cited reports embraced development nihilism—they all contain plenty of useful insights about development—but the confidence of the SAL era that economists can say what policies will reliably pay off in growth rates is gone.

The controversy over SALs following the growth disappointments became so intense, involving both academic economists and NGO advocates (the latter concerned about the effect of fiscal austerity on social spending), that the IMF and World Bank retreated. In the new millennium, there was at the very least a re-naming of the controversial SALs, and perhaps some change in policy, to *Poverty Reduction and Growth Facilities* in the IMF, and *Poverty Reduction Support Credits* in the World Bank.

Effect on policies

What actually happened on macroeconomic policy reform in Africa after the introduction of SALs, and how and why did it happen?

The literature’s take on the effect of SALs on policies seems at first blush contradictory: (1) SALs were ineffective at changing economic policies, and (2) economic policies improved in Africa during the era of structural adjustment and afterwards. The resolution of the apparent contradiction is simply that there is a

lot of variation within Africa as to who received SALs, and this variation was unrelated to the improvement in policies.

The variation consists of whether countries received SALs at all, and for those who did, how many they received. The biggest surprise in the way that SALs evolved was that many countries received an awful lot of them, topped by the 26 in Cote d'Ivoire over 1980-1999. There are two different ways to interpret the frequent repetition of adjustment lending to the same country: (1) policy dysfunction requires a gradual, multi-stage treatment, so each additional SAL was taking a salutary step in the right direction, or (2) previous SALs were ineffective at changing policies (or raising growth, to be discussed below), so new SALs tried again, which also helped repay the previous ineffective SALs. In the view according to (2), over time a serious problem of moral hazard in adjustment lending developed.

The evidence seems inconsistent with (1), since Easterly (2005) found no evidence of policy improvements from one SAL to the next within countries. Collier et al. (1997) also pointed out the lack of evidence that SAL conditions were kept. World Bank (1998, p. 51) pointed out that the same agricultural policy reform in Kenya was the subject of a condition in five different SALs, violated each time. The IMF's own evaluation office (Independent Evaluation Office (IEO), 2002) harshly criticized the IMF's repeat lending as counterproductive, apparently finding no evidence that it was part of a salutary multi-step process. The IEO 2007 confirmed the general failure of IMF SALs to change economic policies over 1995-2004 (a period including the successor instrument to SALs after 1999). IEO 2007 found that about half of structural conditions were not kept.

Yet Easterly (2005) found an exogenous trend in improvement in most of the policies described above, unrelated to the number of SALs received. It could be that the intellectual influence of the IMF and World Bank was important in convincing countries to improve their policies, but if so, this was not mediated through SALs.

Why were SALs ineffective at inducing policy change? Svensson (2003) argues that there was a problem of time inconsistency in conditionality. Since aid recipients knew that each country department in the World Bank, for example, was under pressure ex-post to fully disburse its budget, the threat of withholding disbursements if conditions were unmet was not credible ex-ante.⁴⁰ The donors also seemed to lack appreciation for internal political incentives to sometimes keep pursuing policies that benefited local elites. The weak incentives to change from donor conditions paled by comparison.

Even though the evidence is against SALs as an inducement to change, African governments did indeed correct some major distortions during the era of structural adjustment, consistent with the exogenous improvement orthogonal to SALs. Some of the worse distortions were the overvalued exchange rates. According to one estimate, the median African currency was 82 percent overvalued in PPP terms in 1980.⁴¹ Over 1980-2001, there was a steady trend towards real devaluation (including a major devaluation of the French-supervised CFA Franc for most francophone countries in 1994), so that by the early 1990s, the currency in the median African country was at PPP parity, or even undervalued. The devaluation of the official exchange rate also sharply reduced the high black market premiums on foreign exchange that had previously been prevalent in Africa.

The overall movement towards correcting other distortions is positive but not quite as impressive as with the exchange rate. Easterly (2005) defines a country as having a major macroeconomic distortion if any of the following hold: inflation is above 40 percent, the black market premium is above 40 percent, real overvaluation is more than 40 percent, or the real interest is less than -5 percent. Then the percent of SAL-intensive countries with major macroeconomic distortions declined steadily during the structural adjustment period 1980-1999, albeit still nearly half at the end.

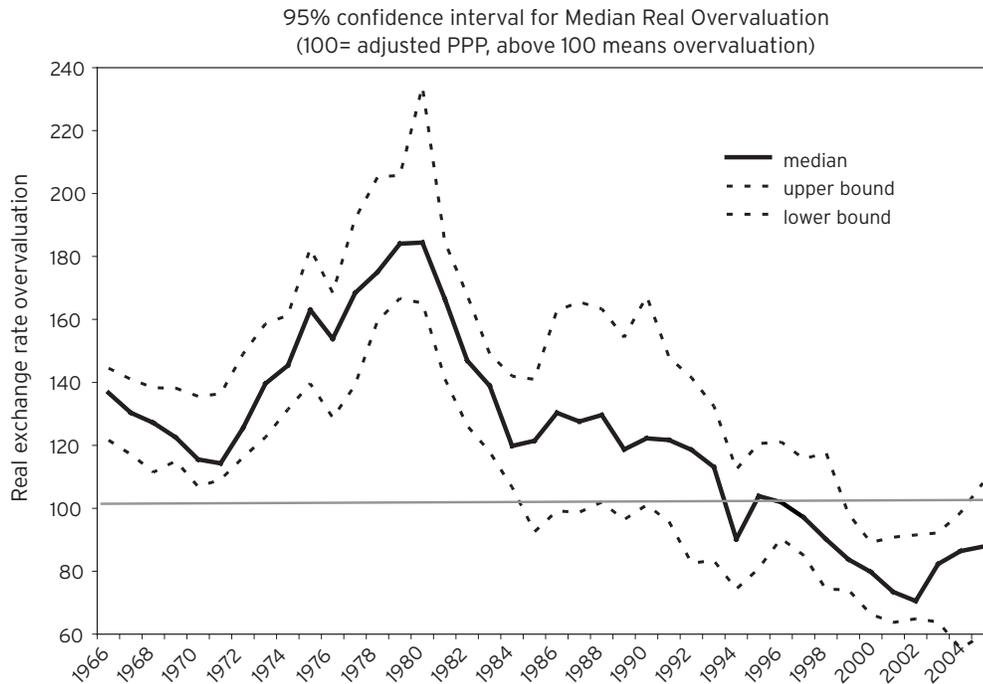
Effect on growth

The empirical literature on IMF/World Bank structural adjustment lending and growth outcomes faces many of the same issues as the aid and growth literature. There is an obvious selection bias in whom the World Bank/IMF treats with adjustment lending, just as there is a selection bias in whom an emergency room treats. Some of the complaints by NGOs about SALs are based on correlations between SALs and outcomes that are the equivalent of the negative correlation between admission to an emergency room and a person's health, with the implication that the emergency room is bad for your health. Przeworski and Vreeland 2000 address this problem by doing a selection equation for entering an IMF program involving when a country is under pressure from low foreign exchange reserves, high budget deficits, and high debt service. Variables affecting the IMF's side of the loan decision are also significant, such as the balance of payments deficit, whether the government is a dictatorship (favorable for getting the IMF to give you a loan), and the number of loans the IMF is making to other countries. Controlling for selection bias, they find that an IMF program lowers growth by 1.5 percentage points.

Barro and Lee (2005) find that IMF "loans tend to be larger and more frequent when a country has a bigger quota and more professional staff at the IMF and when a country is more connected politically and economically to the United States and other major shareholding countries of the IMF." Using these variables as instruments, Barro and Lee find that IMF loans have a negative effect on growth. Easterly 2005 does an IV growth regression for the number of World Bank and IMF adjustment loans using as instruments strategic variables like a dummy for former French colonies, US military assistance, and log of population size; this regression finds a positive but insignificant effect of SALs on growth. Of course, the same concerns about identification assumptions (do SALs to Francophone countries have the same effects as others, for example?), unclear specifications, and data mining could be leveled against this literature as much as the aid and growth literature. This literature differs from the aid literature, however, in that there are very few academic claims of positive effects of SALs on growth (although there are such claims in non-academic publications of the IMF and World Bank itself).

The repetition of the loans to the same country alleviates, but does not eliminate, the selection bias problem. If the same patient is re-admitted on a daily basis to the emergency room and fails to improve, one is inclined to think the emergency room is ineffective or the wrong form of treatment has been given. SALs were supposed to be "emergency" loans that enabled countries to correct problems over the life of the original loan—their repetition was not envisioned in their design. It could be designers of SALs did not realize that they needed to be a multi-stage process in which different loans would address different problems. However, as we have seen, the macroeconomic policies did not improve from one loan to the next.

Figure 11: Real currency devaluation in Africa



Another indirect piece of evidence on the outcomes from SALs is that the loans ultimately were forgiven (the Heavily Indebted Poor Countries–HIPC– initiative in 1996 partially forgave the SAL debt, then the Multilateral Debt Relief Initiative created in 2005 forgave virtually all of it, which was intended to end over 20 years of incremental debt relief). Since SALs to Africa were heavily concessional (zero interest and 40 year maturity), the payoffs to the loans were not good enough to avoid a crippling debt crisis even with debt that was mostly a grant.⁴² Only countries borrowing at the World Bank’s International Development Association (IDA) concessional rates were eligible to become HIPCs receiving debt forgiveness (there were 41 HIPCs in the end). Of the 18 IDA countries that received above average number of SALs, 17 became HIPCs. Today, debt ratios are rising once again in these same countries and the UN (2008, p. x) notes that 21 HIPC countries “are considered to be at mod-

erate-to-high risk of falling back into debt distress.” The World Bank and IMF do not seem to have learned sufficient lessons about the dangers of lending to the poorest countries, with the history of debt forgiveness now creating an obvious moral hazard problem.

Aid, institutions, and development

The disappointment with structural adjustment in Africa brought another escalation in attempted systemic reform. Under the plausible argument that returns to economic policy reform were low if property rights were weak and corrupt autocrats a perpetual threat to the private sector, the West shifted emphasis in the 1990s to institutions like corruption, democracy, and property rights. This occurred at the same time as the literature increasingly stressed institutions as being the fundamental determinant of development (see Acemoglu, Johnson & Robinson, 2005). Hence, the es-

calation from policies to institutions made some sense, and once again may have reflected some learning in the aid agencies about determinants of development.

However, this and future escalations also made possible a continual evasion of aid agencies for failed reform strategies. As Rodrik (2006) pointed out, the response of aid agencies to the failure of previous recommended reforms was to say that they had been “necessary but not sufficient,” and make ever longer the list of “necessary” reforms. As Rodrik (2006) also pointed out, this makes the hypothesis that the Western-recommended reforms were the right ones almost non-falsifiable, since there is always some missing “necessary condition” like “good institutions” that can be invoked to explain the failure. In the end, however, even this nearly tautological defense is unconvincing to those with the “marginal” view—they would point out that no real world reform has ever encompassed a nearly infinite list of “necessary reforms,” and so a choice always has to be made as to which reforms to include.

Moreover, the goal of even transformational reformers was just to get social change started along each of these dimensions: social indicators, policies, institutions, and ending civil war and state failure (to be discussed below). The idea that a society must have already attained good policies, good social indicators, good institutions, good law and order, etc. in order to develop is like saying “you must be developed in order to develop.” Once it is recognized that any reform package faces limits on scale and scope, there is hope for actually altering priors as to whether a particular package of reforms worked.

Those with the “marginal” view would also worry about to what extent the more systemic features of African societies are really amenable to fixes by outsiders. If even the attempt to change economic poli-

cies that could be changed by a few technocrats was a big disappointment, how much can one hope for outsiders to change more deep-rooted phenomena like corruption, democracy, and property rights? The cross country literature gives some useful insights as to the deep historical roots of poor institutions in Africa, such as the relative lack of strong pre-colonial states (Bockstette et al., 2002; Gennaioli & Rainer, 2007),⁴³ the slave trade (Nunn 2007; 2008), ethnic divisions (Easterly & Levine 1997; La Porta et al., 1999), colonial interaction with local elites (Mamdani, 1996), and artificial borders left behind by colonizers (Alesina et al., 2008). Moreover, the cross-section association between good institutions and development gives no information on what transitional dynamics of institutions and income are likely or optimal within a society over time.

Finally, such institutions depend not only on top-down legal rules, but also on bottom-up social norms and conventions that may have evolved over a very long period (see Easterly, 2008b for a discussion). Fisman and Miguel (2008) have a clever experiment on the effect of norms by showing large differences in unpaid UN parking tickets in Manhattan by national origin, which are correlated with corruption outcomes in the home countries. African UN diplomats get a lot of parking tickets compared with Scandinavians. Formal rules to implement institutions could be either a complement or a substitute for social norms. For example, Djankov, La Porta, Lopez de Silanes, and Shleifer (2008) devise a measure of rules on disclosure of assets by public officials, meant to be a tool for controlling corruption. This measure turns out to be *negatively* correlated with polling data averages from countries in the World Values Survey on whether citizens believe it is wrong to take a bribe, which is suggestive that the rules are introduced when the social sanctions on corruption are not sufficiently strong.

Corruption

Corruption used to be an unmentionable word in aid discourse, but that changed in the 1990s, which in itself is a sign of some progress. One benchmark turning point was a high profile speech condemning corruption that World Bank president James Wolfensohn gave at the 1996 Annual Meetings of the Bank and IMF. The aid community had two levers available to try to induce decreases in corruption. It could withhold aid from corrupt governments, and it could use its technical advice to control corruption. However, using aid money as leverage was subject to the same time inconsistency problem as conditions on SALs, and did not turn out to be conspicuously effective. In fact, there is no evidence of increasing responsiveness of aid allocation to corruption (or to democracy), as demonstrated earlier in Alesina and Weder (2002) and reaffirmed and updated in Easterly (2007).

The second “technical” remedy could be useful if there was a domestic political shift in favor of cleaning up corruption, but local actors lacked knowledge of techniques to control corruption. However, in practice, aid agencies pushed “anti-corruption strategies” on countries almost universally, as if all corruption was a technical problem. The alternative to the “technical” view is that corruption is a political economy phenomenon where under some circumstances public officials have a particularly strong incentive to favor their private interests rather than the public interest (again we see the lack of attention to incentives in the transformational approach, in this case political incentives –see Pande (2008) for an articulate treatment). Perhaps the ultimate example of the technical approach was the suggestion by Sachs (2005) that corrupt governments should be given more aid money to implement anti-corruption strategies. At first blush, this seems analogous to giving grants to burglars in the hope that they will install alarm systems in homes before

burgling them. In fairness to Sachs, he probably had in mind some incorruptible reformer within the corrupt government, who will get his corrupt colleagues under control with a well-financed anti-corruption effort (including such technical fixes as computers on which to track government spending). The question remains as to how to identify these incorruptible reformers, and the technical approach glosses over the political battle between corrupt insiders and anti-corruption reformers that will be determined by many factors besides aid and technology.

Trends on corruption

There is no trend in corruption in Africa relative to the rest of the world over 1996-2006. This conclusion is derived from the measure of Kaufmann, Kraay, and Mastruzzi 2007 (KKM), who do a sophisticated averaging over all available corruption indicators, correcting for selection bias and other problems. The KKM measure is relative each year, standardized as a Normal (0,1). African countries on average are a little over 0.6 standard deviations worse than the world average on corruption, a measure that showed little change over 1996-2006 (especially considering the wide confidence intervals). The trend on the relative measure seem most relevant to the aid-corruption question when comparing a region above average in aid-intensity to the rest of the world in making progress against corruption.

Empirical evidence on aid and corruption

What about more formal empirics on the relationship between aid and corruption? One of the most well-known regression studies finds that aid worsens corruption for ethnically diverse countries—which includes most African countries (Svensson, 2000). This finding resonates with Banerjee and Pande’s (2007)

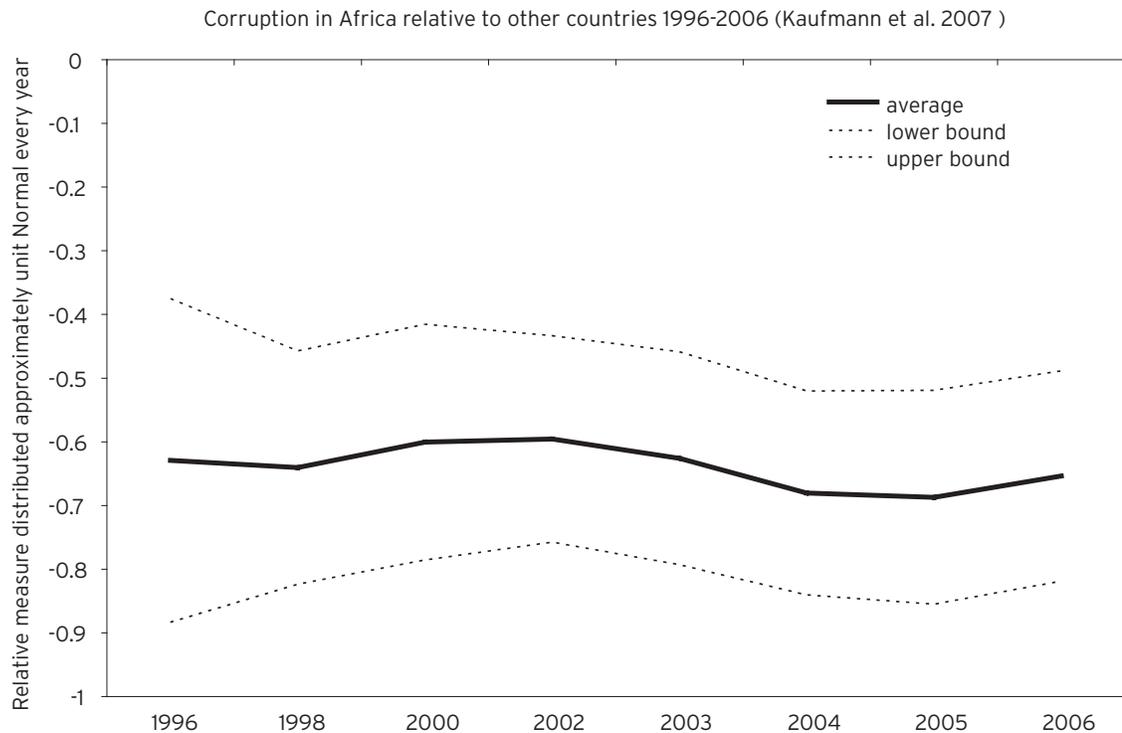
story that voters that are more polarized along ethnic lines are more likely to elect corrupt candidates, because they care more about electing a candidate from their ethnic group than electing an honest candidate. Banerjee and Pande showed in the state of Uttar Pradesh in India that a rise in ethnic politics went together with increasingly corrupt politicians (as measured by voter perceptions—and by the criminal record of the candidate!) Other studies find that ethnic diversity (which is the highest in the world in Africa according to standard measures) tends to increase the demand for redistribution towards one's own ethnic group using channels like public employment (Alesina, Baqir & Easterly, 2000) and public transfers (Besley, Pande, & Rao, 2004). A related literature shows that less resources are allocated to public goods in ethnically diverse environments (see Alesina et. al, 1999; Luttmer, 2001 for the US; Banerjee, Iyer, & Somanathan, 2005 for India; and Miguel & Gugerty, 2005 for Kenya). Introducing more additional resources such as aid into such a political equilibrium is likely to raise misappropriation of public funds.

Knack (2001) found that aid unconditionally worsened "governance," an average of ratings of corruption, bureaucratic quality, and rule of law. Svensson and Knack instrumented for aid with the usual population size, initial need, and strategic variables. However, Tavares (2003) finds that aid decreases corruption, using a similar IV specification for aid. It is rather frustrating that later authors did not themselves try to relate disparate results to previous studies. Of course, these aggregate studies are subject to the same critiques as with the aid and growth literature, with unclear specification of other control variables and identifying assumptions that are always somewhat problematic for aggregate outcomes in which most factors are endogenous.

The micro randomization literature has also addressed corruption and possibly points the way to more successful marginal aid interventions. One influential study is Olken (2007), who found that official audits reduced corruption in Indonesian village road projects. When the villagers were told in advance that they would be subject to an audit by the central government audit agency (increasing the probability of an audit from 4 to 100 percent), an estimate of "missing expenditures" decreased from 28 percent of expenditures to 19 percent. The effect was statistically significant, but the magnitude is modest. Olken suggests the probability of punishment did not increase anywhere near as dramatically as the probability of an audit. Olken also tested whether grassroots monitoring (such as distributing anonymous complaint sheets to villagers) decreased corruption, and found no effect.

Other micro empirical studies (not using randomization) also shed light on the effectiveness of some kind of transparency or auditing. In a famous paper, Reinikka and Svensson 2004 found from a tracking survey that only 13 percent of central government transfers to local primary schools in Uganda arrived at their destination. This research was itself a form of transparency, as the release of the study by itself prompted improvement in the transfer ratio. The Ugandan central government took the bold measure of publishing the intended transfers by school in the local newspapers where they could be monitored by parents and local officials. Reinikka and Svensson 2005 showed that the newspaper campaign successfully increased the proportion of transfers that arrived at the schools, one of the factors behind an increase in this proportion to 80 percent. The newspaper campaign also showed up in the increased enrollment and test scores in these schools. Also possibly supportive of the monitoring and transparency approach to re-

Figure 12: Corruption in Africa in international perspective



ducing corruption is the finding by Besley, Pande, and Rao (2005) that higher education among the voter population is associated with less corruption, using a natural experiment of elections to village councils in South India.

Micro empirics has also found novel ways to document corruption, such as Fisman’s (2001) clever study linking Indonesian stock market movements of individual companies to fluctuations in the health of the dictator Suharto, implicitly capturing which firms relied on connections to Suharto’s network of corruption and patronage. Similar political connections have been documented in many other studies covering other countries (see survey in Pande 2008, pp. 3168-3169). Bertrand, Djankov, Hanna, and Mullainathan (2007) documented costly corruption in obtaining drivers’

licenses in India in an experimental setting, where they found that a treatment group given a bonus for obtaining a license quickly was more likely than the control group to pay bribes and to obtain a license without knowing how to drive! This latter is an example of the well known story that any government regulation creates opportunities for corruption, which has the obvious “marginal” recommendation that governments should reduce red tape as much as possible. Red tape could also help explain what makes political connections so valuable.

Democracy

Aid to promote democracy became fashionable about the same time as aid to combat corruption, with democracy promotion linked especially to the end of the

Cold War and the early 1990s “end of history” view that the whole world was in transition to democratic capitalism (see Carothers 1999; 2004 for an extended discussion). The levers for democracy are the same as those for corruption—donors being selective on degree of democracy as an incentive to follow democratic practices, and technical advice (how to hold an election, etc.) Again, the outside actors seemed to assume an exaggerated sense of their own importance, not recognizing the dependence of democracy on many bottom-up social norms and associations not amenable to outside manipulation.

Trends on democratization

Africa had more of a democratic transition than other developing countries, at about the same time (but not quite to the same degree) as ex-Communist countries moved away from autocracy. However, Africa’s democratic transition preceded the heyday of democracy promotion efforts by donors, so it would be hard to attribute the former to the latter (although general aid may still have played some role).

The KKM governance indicator on African democracy relative to the rest of the world is available only for the decade 1996-2006 (which would correspond more closely to the timing of democracy promotion efforts). There has been little sign of Africa converging to the rest of the world on the KKM democracy measure.

This is an area where case studies may be useful. Donors were certainly involved in internationally-supervised elections in formerly war-torn societies like Liberia and Democratic Republic of the Congo. Donors also applied pressure to Kenya to conform to democratic principles after the long-time autocrat Daniel Arap Moi left office, and again in 2007-2008 when there was a seriously flawed election. However, other flawed elections happened with little donor complaint

(such as Nigeria in 2007), or there were forceful complaints by donors that were ineffective (Zimbabwe 2008). Conversely, some democratic transitions in Africa were based on indigenous mass movements that forced autocrats to hold fair elections, with little donor involvement, such as Zambia (Ottaway, 2000).

Case studies can also assess some of the tools donors have used to try to promote democracy. A widespread aid fashion in the 1990s was for donors to try to strengthen “civil society,” voluntary citizens’ associations which were thought to be a way to promote political participation and holding governments accountable. Unfortunately, it was very unclear what qualified as civil society, or whether all civil society was such a good thing, since voluntary membership groups in society could include anything from gangs to the Mafia to terrorist organizations (Carothers, 2004). In Africa, the uncomfortable reality was that many voluntary groups formed along ethnic lines, which politicians often exploited at election time in a way that increased ethnic animosity. Even aside from the ethnic issue, other voluntary groups in Africa were economic self-help organizations that were relatively apolitical. Donors’ attempts to fund Western-style NGOs that promoted political participation and issue lobbying often created artificial NGOs with few roots in the community, which would immediately collapse if donor support was withdrawn (Ottaway, 2000). We see again the theme of the good intentions of donors’ top-down “transformational” schemes being frustrated by messy bottom-up realities.

Olken (2008) uses the randomization methodology to assess the impact of introducing more democracy into the aid process itself. A random sample of Indonesian villages was given the right to democratically choose which aid projects would be implemented. Aid democracy did not have much effect on which projects were actually chosen, but it did dramatically improve villag-

Figure 13: Democratization in Africa in international perspective

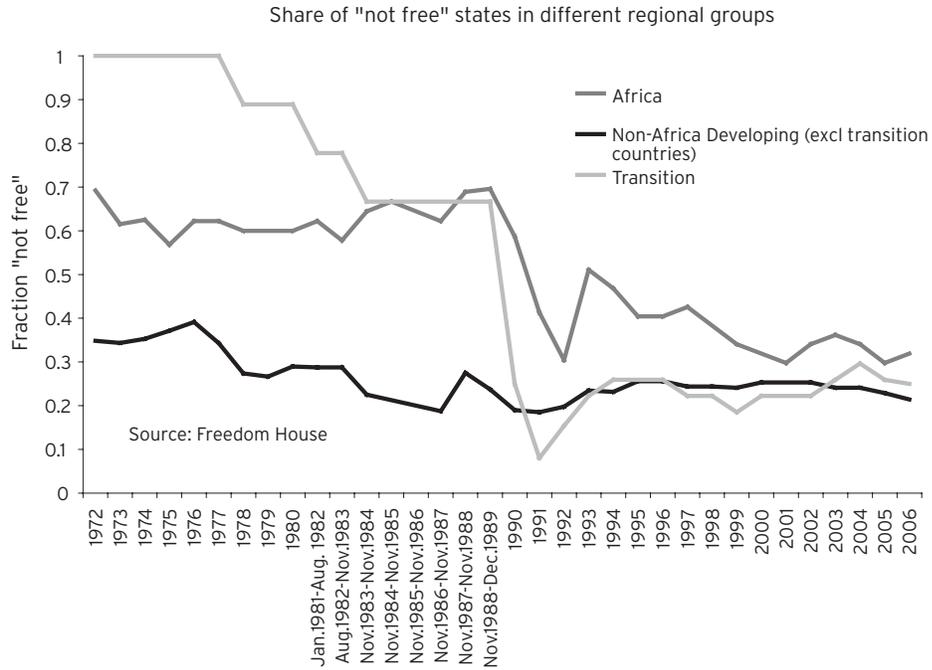
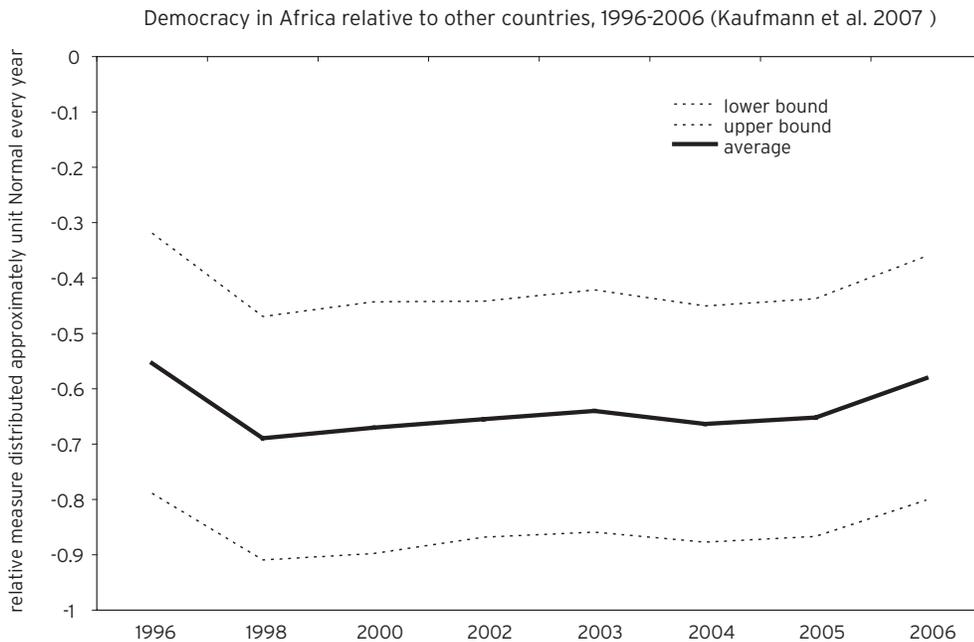


Figure 14: Recent democracy trends in Africa relative to other countries



ers' satisfaction with the projects and their willingness to contribute. Again, a micro study points to a small step whereby donors could directly introduce a modest level of democracy and it would have some effect on outcomes. It is pure guesswork to assess how such steps affect the larger agenda of making a country more democratic, but at least it makes a village a little more democratic! Again marginal steps towards more democracy seem more feasible (and testable) than grandiose democratic ambitions.

Empirical evidence on aid and democracy

A small cross-country regression literature has analyzed the effect of aid on democracy. Knack 2004 finds no association between aid and the change in democracy from 1975 to 2000, including when he instruments for aid. Djankov, Montalvo and Reynal-Querol (2008; 2006) found a causal negative relationship from aid to the change in democracy, using the usual suspects as instruments for aid. They labeled this the "aid curse," in which aid is as bad for democracy as oil is in the well-known "oil curse." The intuition is similar: more money available to those who control the state will make them less likely to permit any democratic threat to their stay in power. Moss et al. 2008 provide further intuition for such results when they argue that states beholden to donors for most of their revenues have less incentive to be accountable to their own citizens compared to states dependent on domestic tax revenue. Their argument is most relevant for Africa, since the median African country got aid equal to 37 percent of government expenditures over 1990-2006 (compared to 4 percent for non-African aid recipients). They also argue that goods provided by donors such as four-wheel drive vehicles or "sitting fees" for attending donor seminars (which can exceed monthly salaries of civil servants) can become objects of political patronage, reinforcing the "patrimonial state" and further undermining the

prospects for democracy. Although the catch-all nature of aggregate data sheds little light on how to make aid more consistent with promoting democracy, and identification is still problematic, these results did reinforce the picture from case studies and aggregate trends that the attempt of aid to transform African governments into democratic ones was not a success.

Property rights: land titling

The third institutional area suggested by research and that has attracted much interest from donors is property rights. In Africa's agricultural economies, the main asset is land, so there has been much focus on reforming land titling so as to implement individual property rights in land. De Soto (2000) made an influential statement about the potentially large payoffs from converting land with insecure rights ("dead capital" in De Soto's language) into formal title. This has not only the obvious benefit of improving incentives for farmers to invest in land quality (recall that crop yields and soil quality are comparatively poor in Africa, as discussed in the agriculture section above), but also unlocking access to formal credit using titled land as collateral. The simplest view of how aid donors could improve property rights in land would be to give money and advice to implement an effective system of formal paper titles for land, which seems akin to the "transformational" view of what outsiders can accomplish.

The theme of land titles improving incentives is an old one in Africa, as apparent from the 1938 statement cited in Table 5 (Lord Hailey, 1938, pp. 868, 876): "legal security ... can most effectively be guaranteed by registration." The World Bank (2003b) expressed pretty much the same viewpoint, as if very little had changed in 65 years:

[Land] arrangements found in many countries are often not optimal from either an economic or a social perspective. For example, in Africa, the vast majority of the land area is operated under customary tenure arrangements that, until very recently, were not even recognized by the state and therefore remained outside the realm of the law. (p. xviii)

Despite decades of attempts to register land titles, during both the colonial and independence eras, today only about 1 percent of land in Africa is registered under the formal system (Blair Commission for Africa 2005, p. 231). In Africa, there has been a long historical evolution of customary rights to land. As Pande and Udry (2005) say in a study of the Democratic Republic of the Congo, Cote d'Ivoire, Gambia, and Ghana, such rights can be complex:

...what matters for rural land rights is the country's community-based mechanisms as exemplified by customary law. The use of almost all land in these four countries is governed by customary tenure arrangements, not formal sector rules... The same piece of land can be subject to multiple claims which relate to the ways in which it is used by separate groups and individuals at different levels. For example, one individual may have the right to cultivate annual crops on a plot, while another retains rights to the tree crops that exist on the same land. An elder might have the right to allocate a plot to a family member for temporary use, but not the right to rent the plot to an outsider on a commercial basis.

Outside donors paid little heed to the pre-existing local arrangements. Under these circumstances, issuing a land title to yet another party can increase rather than decrease uncertainty about who has what rights to the land.

Indeed, a number of empirical studies show little effect of outsider-directed formal land titles on the incentive to invest in Africa. Jacoby and Minten (2007) found no effect of land titles on plot-specific investment in rice fields in Madagascar. Deininger and Jin (2006) have recently summarized the literature on land titles in Africa as showing little or no effect of titles on investment or access to credit, although they found evidence that a more general measure of "tenure security" in Ethiopia (not dependent on titles, which did not exist) fostered land investments.⁴⁴

Why was the payoff to formal land titling in Africa so hard to find? Migot-Adholla et al. 1991 long ago presented evidence that indigenous property systems in Africa, far from being static, have themselves spontaneously evolved towards more individualized land rights in response to increased population pressure. They argued therefore that the indigenous systems do not constrain investments in increased land productivity. Platteau 1996 also argued that there is little evidence of any benefit of formal land rights compared to indigenous systems. Cotula 2007 and Boudreaux and Aligica (2007) provide more recent statements of this same view, albeit with some variations and cautionary notes that indigenous evolution of land rights is not a panacea for optimal outcomes. Udry and Pande (2007) have a more recent empirical result reinforcing the picture of population pressure-induced evolution of customary land systems; they found that commercial land transactions were more likely in matrilineages in Ghana that had higher population pressure.

RE studies from other contexts give a somewhat more positive picture for the effect of formal titling. Erica Field has performed a number of studies based on a natural experiment of the semi-randomized timing of a government program giving formal titles to urban squatters in Peru. Field (2005) found that these

titles increased title-owners investment in their urban shantytown dwellings (a 68 percent increase in the rate of home renovation in the four years after receiving a title). Field (2007) also found that title holders were able to reallocate work away from that performed at home and to increase total work hours, plausibly because greater tenure security reduced the need to have someone always at home to protect the property.

Field and Torero (2006) have a more ambiguous result on the De Soto prediction that formal titles would unlock access to credit. They found newly entitled households got more credit from government banks, but not from private banks. If the household did manage to get a loan, the interest rate for households with titles was 9 percentage points lower. Dower and Potamites (2007) find that an Indonesian titling program had a positive effect on credit access, but not necessarily through collateral for the loans (which is how De Soto thought titles would increase credit access). Their data on rural Indonesian households showed when a title was used as collateral, compared to having a title but not using it as collateral on a loan. The latter had an effect on access to credit, but the first did not. Their interpretation was that title was more useful as a signal of creditworthiness to banks, not through collateral. Sixty percent of titled households with formal loans do not use the title as collateral. Further evidence against the "title as collateral" story was that the banks also accepted non-formal claims to land as collateral (and they accounted for 58 percent of the land collateral cases).

Udry and Pande's (2007) world wide survey of the land titling literature concludes "Land titling and registration typically increase agricultural productivity and farm investment," but their list of studies in Africa mirror the same non-results described above.

These rather ambiguous results and the clearly different contexts of the Peru and Indonesia and other non-Africa studies do little to restore faith in the utility of formal titles in Africa. Yet despite all the research and experience in Africa, the aid donors today remain stuck on a transformational government reform of land rights in Africa. The United Nations Millennium Project 2005 said for example: "The rule of law involves security in private property and tenure rights ... upholding the rule of law requires institutions for government accountability... this requires a well functioning and adequately paid civil service and judiciary, proper information technology (for registration of property ...)" (pp. 31, 111).

This is another apparent failure of the "transformational" approach by donors, and a key example of how outsiders exaggerated their own importance. Perhaps further research can find a way to gradually build formal institutions on top of indigenous institutions in a way that preserves their benefits while adding some advantages of formality.

Civil war and failed states

Some African states collapsed altogether over the last two decades, and the societies descended into civil war, regional warlords, and the nearly complete breakdown of public services (e.g. Somalia, Sierra Leone, Liberia, Democratic Republic of the Congo). Other societies experienced bursts of violence against civilians or outright genocide (e.g. Rwanda in 1994, Darfur in the new millennium). Preventing or resolving civil wars, and halting attacks on civilians and genocide understandably became part of international aid advocacy on Africa in the new millennium. Rich country governments and international organizations responded with plans to combine outside military intervention and traditional aid work to take on tasks like ex-ante preven-

tion of civil war and genocide or ex-post “fixing failed states” or “post-conflict reconstruction.”

The British aid arm, the Department for International Development (DFID, 2006), said that “the growing awareness of the linkages between conflict prevention and poverty reduction ... and the importance attached to helping rebuild countries emerging from conflict all serve to emphasize the need for DFID to work effectively with the military.”

Here once again, we see the theme of escalation, since now the list of aid tools has grown to include Western or UN armies and the task list now includes “reconstructing” a war-torn society practically from scratch, which is far beyond what the aid industry would have previously contemplated. I briefly survey this area mainly to examine whether here we have reached the ultimate *reductio al absurdum* of the West’s transformational approach.

The World Bank’s economists suggested in a prominent 2003 report called “Breaking the Conflict Trap” how “international action” including military intervention and foreign aid could achieve both peace and economic development:

Our new understanding of the causes and consequences of civil wars provides a compelling basis for international action. ...Increased [foreign] aid and changes in allocation and administration could make such assistance more effective in preventing conflict... International action ... could avert untold suffering, spur poverty reduction, and help to protect people around the world from ... drug-trafficking, disease, and terrorism.

The report estimates that a specific package of international military peacekeeping forces, reforms, and foreign aid halves the probability of a civil war break-

ing out in a poor country from 44 percent to 22 percent.⁴⁵ A large-scale World Bank research project lay behind the 2003 report, and continued afterward. The research made valuable contributions in bringing to economists’ attention the possible economic dimensions of civil war, but also represented an escalation of ambition.

In his book for general audiences summarizing this research, Collier (2007) gives precise recommendations for donor agencies (as well as military agencies):

So what seems to show up is a sequence. Aid is not very effective in inducing a turnaround in a failing state; you have to wait for a political opportunity. When it arises, pour in the technical assistance as quickly as possible to help implement reform. Then, after a few years, start pouring in the money for the government to spend.”(p. 116)

I want to persuade you that external military intervention has an important place in helping societies of the bottom billion, and that these countries’ own military forces are more often part of the problem than a substitute for external military forces. (p. 124)

Coups such as the one that destabilized Cote d’Ivoire are still a problem for the bottom billion. Remember, they are driven by much the same factors as rebellions are: poverty and stagnation. And yet it would be relatively easy to make coups history. We just need a credible military guarantee of external intervention. (p. 131)

Security in postconflict societies will normally require an external military presence, both sending and recipient governments should expect this presence to last for around a decade, and must commit to it. Much less than a decade and do-

mestic politicians are likely to play a waiting game rather than building the peace... Much more than a decade and citizens are likely to get restive for foreign troops to leave the country. (p. 177)

Where does this precision come from? A look at the underlying papers listed by Collier (2007)—the same ones that emanated from the World Bank project—shows that they are based on cross-country regressions, where the list of variables to be explained now includes civil war onset, peace onset, civil war duration, economic growth, military spending, and commodity export dependence, and the right hand side variables often include some of the other LHS variables on the RHS side of any equation not explaining that particular LHS variables, plus other endogenous variables such as aid, UN peacekeeping expenditures, and timing of elections (see paper citations below). Unfortunately, even though the list of endogenous variables is even longer and more ambitious than in other cross-country literatures, there is either no attempt or a seriously inadequate attempt to find instruments or establish causal effects. Endogenous variables swap places between LHS and RHS in different articles or different parts of the same article. The Deaton et al. (2006) report on the World Bank's research efforts severely criticized the civil war research on these grounds (while praising the research for raising interesting issues). Daron Acemoglu (2006) contributed these comments to the Deaton report:

The econometric framework is very deficient. It has a number of serious conceptual and methodological problems. First of all, at the end the regression is one of endogenous variables on endogenous variables. But all of the results are interpreted as causal effects... Contrary to the claims in the paper, the regression evidence does not test any well-specified hypothesis, and the correlations that are interpreted as causal ef-

fects are really no more than correlations... It is too early to jump to policy conclusions.⁴⁶

When the World Bank project authors do recognize causality problems, they usually address it by lagging the endogenous RHS variable in a panel regression. The reasons why this is inadequate are well known—the dubious exclusion restriction, serial correlation, permanent country factors, and so on. Some starkly endogenous variables such as UN peacekeeping expenditures are simply used without instruments, although the endogeneity problem is acknowledged. In fairness to the authors, it would be difficult to imagine a successful identification strategy for some of the big aid policy questions involved in civil war and state failure in Africa.

One does not have to start out with the presumption of a “transformational” aid policy agenda on civil war to determine the research questions, however. For example, Miguel, Satyanath, and Sergenti (2004) showed in a widely cited paper that negative growth shocks cause increased likelihood of civil war, using rainfall shocks as a clever instrument for growth shocks (although as usual the excludability assumption, that rainfall does not directly affect war, is a little problematic). This is very useful to know, even it does not lead to any obvious aid policy (aid agencies presumably already wanted to prevent negative growth shocks before this finding, and aid agencies have even less effect on rainfall than they do on other variables). For the effects going from war to development, Miguel and Roland (2006) examined the long-run impact on development of intensity of bombing during the Vietnam War across districts in Vietnam, using geographic determinants of bombing patterns as an instrument. These studies suggest that it is possible to address causality between such aggregates as output and war in a more rigorous way than was done in the World Bank project.

The other defense of aggregate work like the World Bank's could be that, even if they cannot be used to justify confident policy interventions, correlations can be a useful guide to thinking: it does force one to think about which direction of causality is more likely to explain a given correlation, or if there is a third factor that makes two variables move together, which points toward some theories and rules out others.

However, even establishing partial correlations is not so easy in multivariate analysis. The data mining problem seems even worse with civil war and state failure regressions than in growth regressions, since there is not enough political economy theory available to guide the specification of control variables (and existing theory is little utilized in any case). Indeed, Collier, Hoeffler, and Soderblom (2004) seem to embrace data mining as a methodology:

Table 3 presents our preferred "baseline" model of conflict duration, reached after a series of iterations in which insignificant variables are deleted and variants of the economic, social, geographic and historical explanatory variables are then tested in turn.

As usual, data mining will show up as a failure to pass robustness tests. Take the civil war literature's most famous conclusion: that "greed" trumps "grievance," because of a correlation between dependence on primary commodity revenues and likelihood of civil war (interpreted as commodity revenue motivating rebels to try to capture the loot). This had a major effect on aid policy, such as the international attempt to regulate trade in "conflict diamonds." Fearon 2005 found that this partial correlation was not robust to very small and plausible changes in specification. Hegre and Sambanis (2006) performed a sensitivity analysis of civil war regressions similar to Sala-i-Martin's (1997) Bayesian model averaging exercise for growth

regressions. They found only a few civil war correlates to be robust, and primary commodity dependence was not among them.

There are other areas where it is not clear what research forms the basis of policy recommendations. Collier 2007 also passes judgment on the likely military success of foreign intervention in African civil wars (and thus recommends such intervention as quoted above). He points to the ease with which British military intervention halted Sierra Leone's civil war as "the future of military intervention," dismissing counter-examples such as Somalia or Iraq, and argues that a modest military intervention could have prevented the Rwanda genocide (a common view, but not without strongly contrarian views like Kuperman, 2001). The real question is on what basis do economists make judgments on such strictly military topics as ease of pacification or stopping genocide?⁴⁷

Intervention to rescue civilians from war and genocide is certainly appealing. But to the extent that economics has anything to say on such an issue, it would seem the current aid policy literature on military intervention overlooks basic incentive problems, even more egregiously than in other parts of the aid literature. The hope of international intervention may embolden rebels to undertake military action that will inevitably catch many civilians in the cross-fire between the rebels and the government before the interveners arrive (Kuperman, 2005; Crawford & Kuperman, 2008). To make things worse, the government in turn has the incentive to speed up atrocities to achieve its goals before the interveners can come (which is always with a lag because of political inertia and the constraints of military logistics). Exactly this scenario played out with the Kosovo Liberation Army (KLA), who said in interviews with Kuperman that their violence against Serbs starting in 1997 was motivated by hopes of foreign intervention. The Serbian government responded

to the NATO bombing campaign in 1999 with massive deportations, killings, and rapes of Kosovars, before finally withdrawing from Kosovo under the threat of a NATO ground invasion (Rieff, 2002). In general, University of Chicago Law Professor Eric Posner (2006) points out that a smart tyrant or warlord can foil a humanitarian invasion by using civilians as human shields, forcing the invaders to kill those they are trying to save (as arguably occurred in Somalia). There is a generic moral hazard problem, which is very relevant in Africa—if outside mediators and peacekeeping forces are expected to buy off the most powerful warlords to achieve peace, this creates an ex-ante incentive to use violence to become a powerful warlord (and to accelerate violence when intervention is anticipated).

These examples are meant only to suggest there has not been enough attention to likely strategies of the players in humanitarian military interventions. It's rather embarrassing that key theoretical insights such as moral hazard and the complexities of game-theoretic behavior were apparently understood more by non-economists than by the economists influencing aid policy in this area.

The related area where policy seems to be running ahead of research is in "re-building failed states" (most "failed states" are in Africa). Western concern about "failed states" surged because of the example of Afghanistan providing a haven for the terrorist attacks of 9/11, and aid agencies have responded accordingly. Aid agencies did reports on this (DFID, 2005; World Bank, 2002; USAID, 2005), and are mounting major efforts for these "failed states" (which are also known in aid jargon as "low income countries under stress (LICUS)," "fragile states," and "post-conflict societies"). There has also been a slew of books and reports on "failed states" from Washington think tanks

and in international relations magazines like *Foreign Policy* and *Foreign Affairs*, too numerous to be listed here. However, I have not been able to find much in the way of academic research on how or whether aid agencies can move a state out of "failure." Part of the difficulty for doing any research seems to be the vagueness of defining which states "failed," when they did so, and if and when they "un-failed." Although there are a small number of cases where everyone agrees there has been state failure (Somalia), aid agencies have typically applied "rebuilding failed states" policy to a much larger group.⁴⁸ For this larger group, aid agencies employ a fuzzy set of criteria for defining state failures, including the last three years per capita growth (USAID, 2005, p. 20) to the state's "ability to protect and support the ways in which the poorest people sustain themselves" (DFID, 2005, p. 7) to "an unfriendly environment for private sector activity" (World Bank, 2002, p.4). Since all of these variables have long been studied in their own right in development economics, it is not clear to what extent "state failure" is just "very low development" by another name. Such fuzziness makes the "state failure" phenomenon even more difficult to research.

There are only the beginnings of serious research and or even common-sense economic thinking in the "state failure" area. Coyne (2008) is a rare but refreshing example of analyzing nation-building with an economist's toolkit. He is very pessimistic about nation-building cum military intervention from the standpoint of both the economics of institutions and political economy. On the first, formal institutions must be supported by informal social norms and individual values (see discussion above), and where these are not present (surely the most likely scenario in a "failed state") a top down military intervention to impose an institution (like democracy) is unlikely to succeed. On the second, both the external actors

and internal actors have their own political incentives and interest groups to satisfy and whether these align with the goals of nation-building is anyone's guess. For example, foreign military nation-builders often have a strong incentive to minimize their own casualties but this tends to increase local casualties, which may increase violent resistance to outside nation-builders. For example, Coyne's case study of Somalia shows the repeated attempts by the UN, the US Army, and most recently the Ethiopians to create an internationally recognized formal state seem to have increased rather than reduced violence. Coyne argues that this happens both because the outside "peacekeepers" are seen as an enemy by some factions, and possibly because the prospect of payoffs emanating from an aid-receiving formal state increases violent competition by factions to capture this state (Coyne, 2008). In contrast, regions in Somalia where there has been no foreign intervention have been more successful at building their own indigenous state, such as Somaliland and Puntland. Weinstein (2005) similarly questions both the assumption that outside intervention is helpful in rebuilding failed states and also questions the fundamental assumption that "failed states" cannot recover on their own (giving examples of "autonomous recovery" like Uganda, Eritrea, and Somaliland).

These snippets of analysis are meant to be illustrative rather than a complete treatment of an enormously complex issue, in order to make two points. First, in contrast to rare examples such as Coyne and Weinstein, it is remarkable that the aid agencies have gone into the nation-building business without using

the most elementary economic and political economy analysis. Second, the hubris of the outsiders that they can cleanly resolve complex internal conflicts and fix failed states is perhaps the single strongest example of the over-reaching and escalation of the donors' transformational approach in Africa.

One might think that a marginal approach is not even possible with civil war and post-conflict questions, but some good recent research suggests otherwise. I give three examples of useful empirical research that makes much more progress than aggregate studies on identification. Blattman and Annan (2007) tested the effect of soldiering on children in Uganda using the quasi-randomized variation across children induced by kidnappings of children into the Lord's Resistance Army. One could use such research to ask what aid programs might help rehabilitate former child soldiers. Bellows and Miguel (2008) found a positive effect of wartime victimization on subsequent political participation in Sierra Leone, an unexpectedly hopeful result for post-conflict recovery. Paluck (2007) found that a radio program promoting tolerance in post-conflict Rwanda had some effect on perceived social norms about behaviors affecting ethnic conflict, compared to a randomly selected group listening to a radio program with no content on post-conflict issues. Although each study of this kind addresses only a narrow issue, a large collection of such studies could be useful to guide aid donors in taking many useful "marginal" steps to facilitate recovery from civil war and state failure.

CONCLUSIONS

There are several themes that emerge from this survey of Western efforts to “save Africa.” There is little evidence of learning over time within the aid to Africa effort. Instead, within each area of effort, there has been a cycling of aid ideas, with a particular approach going out of fashion to be replaced by a new fashion, only to have the old fashion come back and once again replace the new fashion. The paper argues that this reflects the difficulty of learning when pursuing transformational programs.

The surge of literature using the RE methodology has arguably been a step forward in several important ways: taking identification more seriously, stressing the importance of evidence, and above all focusing on taking one step at a time and checking to see if that works (the “marginal” approach personified). Unfortunately, this literature now seems more like a flawed beginning to a constructive marginal approach than a satisfying end. The RE studies have suffered from over-promising and dogmatism from their proponents, heroic extrapolation from results in small samples in particular contexts to general conclusions, and lack of a link to behavioral models. A more constructive approach might target REs more to shed light on behavioral parameters, perhaps use them more to hold aid accountable for results, and to be more open to using diverse types of evidence from case studies, other micro empirical research, and micro and macro stylized facts and some of the more well-executed macro regressions (with appropriate cautions on the severe limitations of the latter).

The conflict will likely continue between the “marginal” and the “transformational” approaches to the overall enterprise of African development. Occasional swings to the more modest “marginal” approach seem to quickly result in a countervailing swing to the more

ambitious “transformational” approach, which has particularly dominated the aid policymaking community in aid in recent years. I have argued at the same time that it is difficult to resolve conclusively what the effects of the more ambitious programs are and that the better attempts at doing so give multiple signs of failure of these programs.

Although the evidence has not been (and perhaps can never be) completely definitive on transformational approaches, there has in practice been widespread disappointment with each successive transformational approach. The current state of knowledge thus argues even more for caution in applying large scale outside interventions that could have unintended negative effects. Unfortunately, far from retreating from the transformational approach, each successive disappointment has led to an escalation of outside intervention, from the project approach to improve sectoral outcomes, to filling the financing gap with aid, to structural adjustment conditions on economic policies, to attempts to modify institutions such as corruption, democracy, and property rights, and finally, most ambitiously to prevent civil war and reconstruct failed states, including outside military intervention.

The dangers of the transformational approach, such as the one that wishes to “save Africa”, are captured well by a famous quote from Adam Smith’s *Theory of Moral Sentiments*:

The man of system, on the contrary, is apt to be very wise in his own conceit; and is often so enamoured with the supposed beauty of his own ideal plan of government, that he cannot suffer the smallest deviation from any part of it. He goes on to establish it completely and in all its parts, without any regard either to the great interests, or to the strong prejudices which may

oppose it. He seems to imagine that he can arrange the different members of a great society with as much ease as the hand arranges the different pieces upon a chess-board. He does not consider that the pieces upon the chess-board have no other principle of motion besides that which the hand impresses upon them; but that, in the great chess-board of human society, every single piece has a principle of motion of its own, altogether different from that which the legislature might chuse to impress upon it. If those two principles coincide and act in the same direction, the game of human society will go on easily and harmoniously, and is very likely to be happy and successful. If they are opposite or different, the game will go on miserably, and the society must be at all times in the highest degree of disorder.

“The game going on miserably” with the “highest degree of disorder” may be an apt description of the current disarray in aid to Africa. One can only hope that the record of the past will chasten outsiders to be more modest and humble about what they can do for Africa. Far from a counsel of despair, such a correction of expectations may make possible a sizeable expansion of programs that deliver substantial benefits to poor Africans under the “marginal” approach.

As far as the “transformational” approach, its ambitions are certainly understandable given the realities of poverty and suffering in Africa. But these

understandable ambitions seem to have created an intellectual bias that exaggerates the importance and potential for benevolent action of outside actors, as well as exaggerating Africa's negatives and inability to fix itself.

Even if the evidence fails to support the hypothesis that outside aid can create economic development, it does not follow that development is a hopeless cause. On the contrary, developing countries worldwide have grown at about 2 percent per capita since 1960, almost tripling per capita income. It is too soon to tell whether Africa's respectable growth since the mid-1990s means that it is joining the worldwide growth club, but there is no reason to think that it will be forever excluded.

As far as the role of outsiders in such growth, it is suggestive that most sustained and largest surges in GDP per capita (notably Botswana and Mauritius in Africa, as well as the East Asian tigers elsewhere, and more recently, India and China) have been largely homegrown rather than the result of ambitious outside aid and intervention.⁴⁹ It would be worth testing and exploring more the hypothesis that most successful development is homegrown. And if so, research should concentrate more on homegrown determinants of development rather than spend so much time on outsiders' actions. Perhaps then we might find that the ones most likely to “save Africa” are Africans themselves.

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ENDNOTES

1. Following a common convention, this paper means sub-Saharan Africa whenever it uses the name "Africa."
2. International Herald Tribune, Friday, January 28, 2005, p. 1
3. http://www.kantei.go.jp/foreign/hukudaspeech/2008/05/28speech_e.html
4. 2008 G8 Summit Declaration, "Development and Africa," July 8, 2008
5. 2008 G8 Summit Declaration, "Development and Africa," July 8, 2008
6. I verified this by getting an actual copy of the memo.
7. Although some may see this divide as corresponding to left vs. right, there are many trenchant critiques of the "transformational" view from the left, such as Scott (1998) and Ferguson (1994). Easterly 2006 pointed out that free market reforms under structural adjustment and shock therapy (usually associated with the right) were very much "transformational" attempts.
8. I do not have space to discuss the important issue of data quality, which is generally very poor for many of the indicators to be considered in this paper. Failure to invest more in data collection is one of the less noticed failures of the Western aid effort. For the purposes of description and analysis in this paper, I can only hope that the signal outweighs the noise, and I resort frequently to averages and medians to remove some of the noise.
9. Some might argue for a population-weighted index of African performance, which would give heavy weight to Nigeria and South Africa. If we take the West's effort to save Africa as operating at the level of national governments (which is certainly how it was conceived), then the median country outcome seems like the right metric to discuss the outcome of Western efforts.
10. There are numerous other examples of exaggeration of Africa's negatives in the aid policy discussion. Suhrke and Samset 2007 document how the likelihood of African civil wars starting up again after ending was overstated by a factor of two even in academic journals. Easterly 2008 shows how the choice of indicators in the Millennium Development Goals exercise consistently made Africa look worse than other equally plausible indicators.
11. De Waal's (1997) list includes Biafra 1968, the Sahel 1973, Cambodia 1979, Ethiopia 1984, Sudan 1985, Ethiopia 1987, Sudan 1990 (and many years since), Somalia 1992, Rwandese refugees 1994, and eastern Zaire 1996
12. See Sachs (2005) and Collier (2007).
13. The development economists of the 50s and 60s can be excused for neglecting this possibility given the underdeveloped international capital market of that era. There is much less excuse today, when many African countries have had some access to international capital markets beginning in the 1980s, and when those who today continue to lack access probably do more because of the investment risk than any market imperfection.
14. A search on Google Scholar for key words "aid" and "growth" gave 1384 cites for BD. Another paper by Collier and Dollar 2004 had essentially the same finding "aid works when policies are good" and had 509 cites. The sum of these two (1893) is about four times more than any other set of aid and growth results.
15. This set of results was the second most cited in the Google Scholar search on "aid" and "growth." In various forms it was cited 474 times as of August 2008.
16. Easterly and Kraay (2000) found no evidence that small population size affected growth performance on average.

17. Unfortunately for deriving unambiguous interpretations, there could be positive scale effects that small countries miss, offsetting the negative scale effects of getting more aid in small countries.
18. The Two Gap model assumed that all aid went into investment, and that the coefficient on investment for predicting growth was 0.2 to 0.5 (reflecting what was called the Incremental Capital Output Ratio of between 2 and 5). In a simple exercise for this paper, I went to the extreme of a simple bivariate regression of per capita growth 1961-2005 on the aid to Gross National Income ratio, 1961-2005, using the log of population in 1960 as an instrument for aid (as noted above, probably the best, albeit highly imperfect instrument for aid). There is a problem of omitted variables in the growth regression, but under the admittedly wildly heroic assumption that population does not affect the omitted variables, the IV procedure also corrects for omitted variable bias (the saving grace may also be that nothing much seems to be robust in growth regressions anyway). The first stage shows the initial log of population to be free of weak instrument problems. The second stage regression shows a slightly negative coefficient on aid in the growth regression. The confidence interval for the coefficient of aid on growth is [-.126, 0.047], hence 0.2 is strongly rejected.
19. Banerjee contrasts his interpretation to that of Goldin, Rogers, and Stern 2007, who attribute global poverty reduction to foreign aid, as an example of how stylized facts fail to induce consensus. However, Goldin, Stern, and Rogers 2002 was the original source of their conclusion, and this was not a research study but a World Bank advocacy effort ("The Case for Aid") to increase foreign aid in the run-up to the UN Monterrey Conference in 2002.
20. However, even this is disputed by Deaton (2008) who discusses problematic assumptions, what parameter is really being estimated, and just what "identification" really means.
21. See the discussion on the Creative Capitalism web site: "Holding aid agencies accountable," An email exchange between William Easterly, Esther Duflo and Michael Kremer, July 31, 2008, http://creativecapitalism.typepad.com/creative_capitalism/2008/07/exchange.html
22. Sachs (2005, 2008) has argued that Africa's health is particularly disfavored by an ecology favorable to the most lethal kind of malaria. Skeptics wonder why donors and governments cannot respond by adopting fairly low-cost treatment and prevention of malaria. The colonial authorities controlled malaria successfully in some places and periods where there were strong incentives to do so. Utzinger, Tozan, and Singer 2002 discuss successful malaria control in the Zambian copper mining belt during the colonial period. Caldas de Castro et al. 2004 discuss a successful program to control malaria in Dar es Salaam before World War I.
23. A nice survey on road maintenance is in Peterson (2008).
24. The exception would be if fungibility allows an increase in government spending elsewhere that is positively harmful, such as spending on armed forces that will harm the local population or neighbors (for example, Uganda was a donor favorite during the period its army were accused of atrocities in the civil war in the Congo).
25. From a report by the Africa Progress Panel (2008), a taskforce headed by Kofi Annan and made up of prominent African and non-African leaders to follow up on the Blair Commission for Africa (2005). The statement was originally made at a G-8 summit in 2002.
26. United Nations Habitat, Water and Sanitation in the World's Cities, 2003, <http://www.earthscan.co.uk/samplechapters/1844070042Intro.htm> and <http://portal.unesco.org/education/en/ev.php->

URL_ID=37612&URL_DO=DO_TOPIC&URL_SECTION=201.html

27. Thanks to Berk Ozler at the World Bank for getting me up to date on CCTs in Africa.
28. Another line of attack on the Pritchett results was that educational data was mismeasured, a problem that was amplified when considering the effect of *changes* in education on other outcomes. De La Fuente and Domenech 2006 found that higher quality OECD data led to a positive association between human capital growth and output growth. Unfortunately, the OECD countries also have better institutions and so don't really help resolve the issue of the effect of education in Africa. Cohen and Soto 2007 also stress data quality problems and get a positive effect of growth on the change in years of schooling with an improved data set for developed and developing countries; unfortunately, we don't know whether this is because of better quality data or whether they are just reproducing the Krueger and Lindahl finding that absolute changes work better than percent change in schooling to predict growth. Also Pritchett sometimes finds *negative* and significant coefficients of education growth on output growth, which could not be explained by poor quality data that would normally lead to attenuation bias.
29. Technically, primary enrollment was second to the East Asian dummy, but the latter seems like a meaningless ex-post creation based on knowledge of East Asia's high growth.
30. Of course, an association more supportive of strong education effects on development is the strong correlation in levels between years of schooling and per capita income. However, Acemoglu and Angrist 2000 point out that the coefficient of income regressed on schooling across countries is far too large to be explained by private returns to education estimated from micro data (and the differences in schooling quantity are far too small to explain cross-country income differences); hence it requires externalities to education at the country level. These authors fail to find evidence of education externalities across US states, using a convincing identification strategy with state compulsory schooling and child labor laws as instruments.
31. 2008 G8 Summit Declaration, Development and Africa, July 8, 2008
32. The definition of "clean water" is unfortunately rather fuzzy. While "clean water" is used as an easily recognizable shorthand, the data actually refer to "percent with access to an improved water source." The degree of improvement could fall short of producing what the reader might think of as "clean" water. These ambiguities contribute to the weak data situation on this indicator, where comparability over time and other sources of noise are more problematic than with other social indicators (not to imply the data are so good on the others either).
33. Although the Commission did suggest avoiding white elephants.
34. <http://go.worldbank.org/R62P1EEJD0>, accessed on August 1, 2008
35. One exception to the general gloom on African agriculture was the success of commercial maize production in southern Africa.
36. Incentives to increase yields per hectare may be weaker in Africa than in other regions because of its greater land/population ratios.
37. Declaration of the High-level Conference on World Food Security: The Challenges of Climate Change and Bioenergy, Food and Agriculture Organization, Rome, 5 June 2008.
38. G8 Leaders Statement on Global Food Security, July 8, 2008.
39. Authors of the Barcelona Development Agenda included Olivier Blanchard, Guillermo Calvo, Stanley Fischer, Jeffrey Frankel, Paul Krugman, Dani

Rodrik, Jeffrey Sachs, and Joseph Stiglitz.

40. Van de Walle 2001 also emphasizes this point in a rich political economy discussion of structural adjustment in Africa.
41. The measure of the benchmark real overvaluation is based on Dollar (1992), then extrapolated to other years with the usual real exchange rate index using domestic and US CPI and nominal exchange rate.
42. Another possibility was unwillingness to repay, as opposed to inability to repay. However, the World Bank and IMF would have been less likely to forgive the loans if it had been too obviously the first.
43. Iliffe (1995) attributes in turn the lack of pre-colonial strong states in Africa to very low population density, which meant that prospective citizens of a prospective state could simply move elsewhere to escape attempted state taxation, military drafts, or any other kind of control.
44. Earlier studies throughout Africa bear out the picture of ineffective land titling. Migot-Adholla and Place (1998), which showed a weak effect of land titles in Kenya on perceived land rights of farmers, credit use, and land yields (a measure of investment in the land). A study of land titles in Burkina Faso (Brasselle, Gaspart, and Platteau 2002) found no effect of land titles on incentives to invest in the land. Firmin-Sellers and Sellers (1999) found that a land titling program in Cameroon was not successful in consolidating individual property rights, although it had some other benefits.
45. World Bank (2003c), p. 168.
46. Acemoglu was commenting upon Collier and Sambanis 2003, a 2 volume publication that contained the above-mentioned findings.
47. This section places more emphasis on the research by Collier and co-authors than by other authors only because the former have been far more influential in aid policy discussions. For a more general scholarly review of the civil war literature, see Blattman and Miguel (2008).
48. There is also little discussion of what role aid played in the original "state failure". Somalia was one of the largest recipients of aid per capita in Africa prior to the fall of Siad Barre in 1991 (de Waal 1997). Easterly 2006 points out there is a correlation between number of IMF programs and subsequent state failure (according to the more restrictive definitions), although interpreting this correlation is obviously tricky.
49. See discussion in Easterly (2006).



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