CHAPTER TWO

THE LEGEND OF THE BIG PUSH

It is undesirable to believe a proposition when there is no ground whatsoever for supposing it is true.

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WHY DO INEFFECTUAL UTOPIAN plans dominate the debate on economic development? We have already seen that it is partly explained by the political appeal of utopian plans to rich-country politicians. In addition, the Planners' intellectual inspiration was an old legend about how Western efforts could achieve long-run development, which has come back with a vengeance.

The legend dates back to the 1950s. Many things have changed since the 1950s—we now have air-conditioning, the Internet, new life-saving drugs, and sex in movies. Yet one thing is unchanged: the legend that inspired foreign aid in the 1950s is the same legend that inspires foreign aid today.

The first chapter in this book presents parts of the legend. The full version goes like this: The poorest countries are in a *poverty trap* (they are poor *only* because they started poor) from which they cannot emerge without an aid-financed *Big Push,* involving investments and actions to address all constraints to development, after which they will have a *takeoff* into self-sustained growth, and aid will no longer be needed. This was exactly the legend that gave birth to foreign aid in the 1950s; it is exactly the legend that the advocates of a massive aid increase are telling today. This chapter will test this legend against the evidence that has accumulated over the past fifty years in between the original legend and its remake a half century later. I will tell you up front what you have already guessed: the evidence does not sup-

port the legend. This is a classic example of trying again something that didn't work before, one of the traits of Planners.

Let's examine each of the component parts of the legend of development.

LEGEND PART ONE: THE POOREST COUNTRIES ARE STUCK IN A POVERTY TRAP FROM WHICH THEY CANNOT EMERGE WITHOUT AN AID-FINANCED BIG PUSH.

The Big Push of massive aid flow was supposed to get poor countries out of what the UN Millennium Project calls a "poverty trap," which automatically prevents very poor countries from growing. As Jeffrey Sachs explains it in his 2005 book, *The End of Poverty*, "When people are . . . utterly destitute, they need their entire income, or more, just to survive. There is no margin of income above survival that can be invested for the future. This is the main reason why the poorest of the poor are most prone to becoming trapped with low or negative economic growth rates. They are too poor to save for the future and thereby accumulate the capital that could pull them out of their current misery" (pp. 56–57).

We can check this story out. As shown in table 1, we have data on per capita income from 1950 to 2001 for 137 countries, from a statistical compilation done by the economist Angus Maddison. (I exclude Communist economies and Persian Gulf oil producers as special cases.) We rank countries according to their per capita income in 1950. Did the poorest countries in 1950 remain stuck in poverty over the next half century? Well, no. The poorest fifth of countries in 1950 increased their income over the next five decades by a factor of 2.25. The other four fifths increased their incomes by a factor of 2.47. The difference in growth rates between the two groups is not statistically distinguishable from random fluctuation. We can statistically reject that the growth rate of the poorest countries as a group was zero. The only period that fits the legend is 1985–2001, to which I will return.

TABLE 1. TESTING THE POVERTY TRAP FOR LONG PERIODS

Average per capita growth per year for:	*1950–2001*	*1950–1975*	*1975–2001*	*1980–2001*	*1985–2001*
Poorest fifth at beginning of period indicated	1.6	1.9	0.8	0.5*	0.2*
All others	1.7	2.5†	1.1	0.9	1.3†
Reject stable income for poorest fifth	Yes	Yes	Yes	Yes	Yes
Fail to reject unstable income for poorest fifth	Yes	Yes	Yes	Yes	Yes

**Poorest fifth not statistically distinguishable from zero.*
†All others' growth statistically distinguishable from poorest fifth.
Sample: 137 countries. Statistical tests exclude 12 transition economies and Persian Gulf oil states.

There are further statistical tests we can do to assess the poverty trap legend. If the legend holds, then the poorest countries should have stagnant income at a very low level. Income will fluctuate randomly around this level, but will always tend to return to it. There are two ways we can test whether there is a cursed stability of low income (known as "stationarity" in statistics jargon). We can assume stagnation and see whether the data reject that assumption, or we can assume instability of income—positive per capita growth is a nice form of instability—and see whether the data are consistent with that assumption (the data fail to reject instability). When we do a test for the stagnation of income over the subsequent half century for the poorest fifth of countries in 1950, we decisively reject the hypothesis of stagnation. When we assume instability—such as positive growth—the data provide no evidence against that assumption.

Perhaps it was aid that enabled poor countries to break out of stagnant income? When I break the sample in half into those poor countries that had above-average foreign aid and those that had below-average foreign aid, I find identical results for 1950–2001 in both halves as with the above tests of stable income. Over 1950–2001, countries with below-average aid had the same growth rate as countries with above-average foreign aid. Poor countries without aid had no trouble having positive growth.

This is a critical finding—the poorest countries *can* grow and develop on their own. Since foreign aid received does not explain these successes, perhaps they happened for entirely homegrown reasons. The Searchers among the poor can find a way toward higher living standards; they do not have to wait for the West to save them.

To be sure, among the poorest countries, there were individual poor countries that failed to grow. Chad had zero growth from 1950 to 2001. Zaire/Democratic Republic of the Congo (DRC) actually had negative per capita growth over this period. Aid still has a role to play to help those unlucky enough to be born into a stagnant economy—even if it doesn't help the overall economy escape stagnation.

The stagnant economies were offset by such success stories as Botswana, which was the fourth poorest in 1950, but which increased its income by a factor of thirteen by 2001. Lesotho was the fifth poorest in 1950, but increased its income by a factor of five over the half century. Two other subsequent success stories who were among the poorest in 1950 are China and India.

Let us keep looking for confirmation of the two main predictions of the poverty trap legend: (1) that growth of the poorest countries is lower than other countries, and (2) that per capita growth of the poorest countries is zero or negative. The poorest did have lower growth than the others in an earlier period, 1950–1975. However, this was not a poverty trap, as average growth of the poorest during 1950–1975 was still a very healthy 1.9 percent per year (roughly the same as the long-run growth rate of the American economy, for example).

There is no evidence of lower growth for the poorest countries for recent periods, such as 1975–2001 or 1980–2001. Their growth was disappointing—much worse than in the previous period—but so was growth in middle-income countries. The poorest fifth of countries at the beginning of those periods had growth performance over the subsequent period that was statistically indistinguishable from the other four fifths of countries. Only when the starting point is put at 1985 does there finally appear evidence that the poorest did worse.

The evidence that Jeffrey Sachs adduces for the poverty trap in his book *The End of Poverty* is from this later period. So, from 1985 to the present, it

is true that the poorest fifth of countries have had significantly lower per capita growth than other countries, about 1.1 percentage points lower. The next section will consider further whether this period fits the classic legend of the poverty trap.

The numbers in table 1 don't seem to add up. The poorest countries did not have lower growth in the whole period 1950–2001, but they had slightly lower growth in 1950–1975, and much lower growth in more recent periods. The solution to the conundrum is that the identities of the poorest countries at the start of each period shown keeps changing. It doesn't help the poverty trap legend that eleven out of the twenty-eight poorest countries in 1985 were *not* in the poorest fifth back in 1950. They got into poverty by declining from above, rather than from being stuck in it from below, while others escaped. If the identity of who is in the poverty trap keeps changing, then it must not be much of a trap.

Other scholars have also failed to find any evidence for a "poverty trap."[1] One of the requirements for a poverty trap is the idea that saving is very low for poor people, increasing only at some intermediate level of income. Aart Kraay and Claudio Raddatz, in a January 2005 paper, studied the savings rate in all countries with data and found that that saving does *not* behave the way the poverty trap requires at low income. The reasons countries stay poor must lie elsewhere.

It is still possible that some countries are in a poverty trap; it is just that the average poor country is not. The theory of poverty traps is quite appealing: there are many ways in which we could think that countries are caught in traps. In a previous book, I give an example of how low average skills in the population could discourage new entrants to the labor force from getting skills, perpetuating a low-skill trap. Traps can also form at higher levels of income if there is some factor missing, such as high-quality formal institutions (which may itself be a consequence of insufficient income), keeping an economy stuck at a middle-income level.

With so many possible kinds of traps, it is not possible to definitively prove or refute the existence of traps in general. I can only test the specific form of the poverty trap discussed in the aid debates on the poorest countries, which predicts that being poor means a country will not grow without external assistance. This the data *can* reject.

LEGEND PART TWO: WHENEVER POOR COUNTRIES HAVE LOUSY GROWTH, IT IS BECAUSE OF A POVERTY TRAP RATHER THAN BAD GOVERNMENT.

What about the period of lower growth and stagnation in poor countries in 1985–2001 shown in table 1? The UN Millennium Project argues that it is the poverty trap rather than bad government that explains the poor growth of those countries and their failure to make progress toward the Millennium Development Goals (MDGs). Jeffrey Sachs says, "The claim that Africa's corruption is the basic source of the problem [the poverty trap] does not withstand practical experience or serious scrutiny."[2] Likewise the Millennium Project says, "Many reasonably well governed countries are too poor to make the investments to climb the first steps of the ladder."[3]

Why does it matter whether it is bad government or a technological poverty trap? The case for Planners is even weaker if they must deal with the complexities of bad government. (We will see in chapter 4 just how difficult it has been.) So aid advocates desperately want to disbelieve the bad government explanation for poverty, which is something akin to the church youth group minister who wants to believe that his charges are all virgins. Bad government is also bad for fund-raising for aid. Jeffrey Sachs worries in *The End of Poverty:* "If the poor are poor because . . . their governments are corrupt, how could global cooperation help?"[4]

Let us test bad government against the poverty trap as an explanation for poor economic growth. The earliest rating we have on corruption is from 1984, from the International Country Risk Guide. We have a rating on democracy for the same year from a research project at the University of Maryland called Polity IV. Let's take countries that have the worst ratings on both corruption and democracy, and call these countries "bad governments." While poor countries did worse, it's also true that the twenty-four countries with bad governments in 1984 had significantly lower growth from 1985 to the present, 1.3 percentage points slower than the rest. There is some overlap between these two stories, as poor countries are much more likely to have bad government. So

which is it, bad government or the poverty trap? When we control both for initial poverty and for bad government, it is bad government that explains the slower growth. We cannot statistically discern any effect of initial poverty on subsequent growth once we control for bad government. This is still true if we limit the definition of bad government to corruption alone. The recent stagnation of the poorest countries appears to have more to do with awful government than with a poverty trap, contrary to the UN/Sachs hypothesis. Actually if those preparing the UN Millennium Project report about escaping the well-governed poverty trap had looked at the Millennium Project's own country studies, they would have found interesting clues to this result, such as the following vignette on Cambodian schoolteachers: "Many supplement their income by soliciting bribes from students, including the sale of examination questions and answers. . . . [T]he end result is a high dropout rate."[5]

There is another piece of evidence that we have to consider that looks like it *does* support the poverty trap story. Over the last two centuries, there *has* been a widening gap between rich and poor nations. This is what World Bank economist Lant Pritchett calls in a famous article "Divergence, Big Time." There is historical data on about fifty countries from the economist Angus Maddison. The gap between the richest and poorest countries has widened drastically over the last two centuries, with the ratio of the max to the min going from about six to one two hundred years ago to about seventy to one today. There *is* a positive correlation between per capita growth from, say, 1820 to 2001 and the initial level of income in 1820.

Was this because the poor countries were stuck in a poverty trap? Well, first of all, the data do not fit our definition of a poverty trap—per capita growth of the poorest countries was not zero. The predicted level of annual per capita growth for the poorest countries in the sample in 1820 was 1.05 percent, with a margin of error of .25 percent. One limitation may be that African countries are not in the sample. However, Maddison gives an estimate for per capita income in the continent as a whole in 1820—per capita growth in Africa from 1820 to 2001 is 0.7 percent per annum, a 3.5-fold increase, not a poverty trap.

Still, let us consider the slower growth of the poorest countries as suggestive of a poverty trap. The alternative explanation to the "poverty trap" is that Europe and its offshoots had better government than the Rest. Good government could be correlated with per capita income in 1820, and that

could explain why countries that were richer in 1820 subsequently grew faster. The poor countries were stuck with authoritarian governments (or another form of authoritarian rule: colonial occupation). This could imply a bad-government poverty trap, but not the savings-and-technology poverty trap favored by the UN/Sachs story.

I test this story by using again the data from the Polity IV research project, which covers democracy since 1820. I average whatever Polity data are available on each country over the period 1820–2001.[6] It turns out that average democracy is significantly correlated with long-term growth in most specifications, and the positive relationship of growth with initial per capita income declines or even turns negative once you control for quality of government. The latter results would suggest that poor countries grow faster than rich countries if they have a good government (using democracy as a proxy for good government)—contrary to the Millennium Project idea that "many reasonably well governed countries are too poor to make the investments to climb the first steps of the ladder." These results hold up when you control for possible reverse causality going from economic growth to bad government. There is no evidence that initially poor countries are at a disadvantage once you control for good government. The Big Push is not going to work if the problem is bad government rather than a poverty trap. We will see in chapter 4 what tortured conundrums foreign aid encounters when dealing with bad governments.

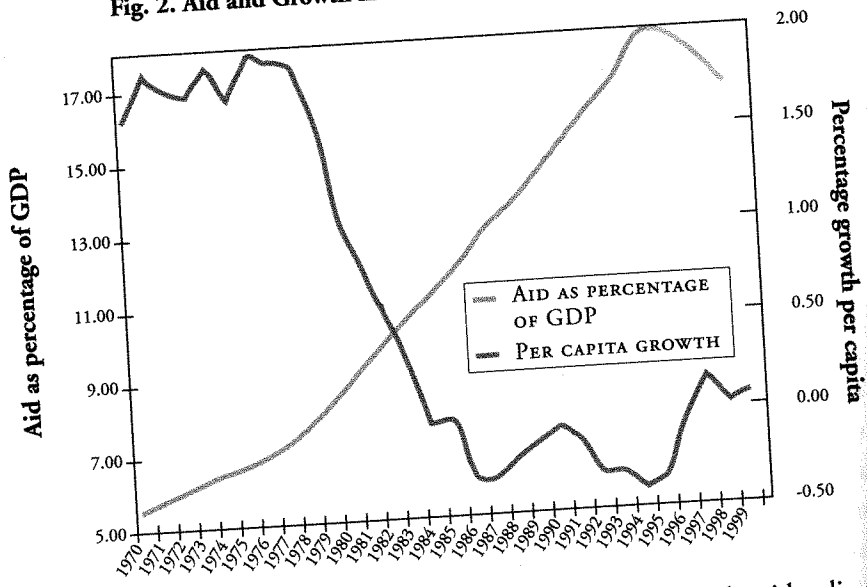
LEGEND PART THREE: FOREIGN AID GIVES A BIG PUSH TO COUNTRIES TO ACHIEVE A TAKEOFF INTO SELF-SUSTAINED GROWTH.

There is now a regular cycle in the literature on foreign aid and growth. Someone will survey the evidence and find that foreign aid does not produce growth. There will be some to-and-fro in the literature, in the course of which a few studies will find a positive effect of aid on growth. Foreign aid agencies will then seize upon the positive effect, usually focusing on only one study, and will publicize it widely. Researchers will examine the one positive result more carefully and find that it is spurious. Then there will be more to-and-fro in the literature, and a new twist will be discovered under which aid has a positive effect on growth. Aid agencies will seize on this result again, and the cycle will begin all over again.

We have already had a test of old and new theories of the Big Push in Africa. For a region as poor as Africa, aid receipts have *already* been large enough to constitute a Big Push. The typical African country received more than 15 percent of its income from foreign donors in the 1990s. Figure 2 shows the overall outcome for aid and growth in Africa. Aid accelerated as growth fell. Note that African growth over the previous ten years had been a respectable 2 percent up to about 1975 (with modest aid), contradicting the idea that Africa is always and everywhere condemned to low growth without aid. There is a negative association, but I don't think the increase in aid *caused* the fall in growth. Rather, the fall in growth probably caused the increase in aid. But the surge of aid was not successful in reversing or halting the slide in growth of income per capita toward zero.

Let us do more formal statistical testing. Long and inconclusive literature on aid and economic growth was produced in the 1960s, 1970s, and 1980s, which was hampered by the limited data availability and inconclusive debate about the mechanisms by which aid would affect growth. The possible reverse causality made conclusions difficult: if donors gave greater aid in response to slower growth, then interpreting how aid flow affected growth could be difficult. The literature got new life in 1996 with a paper by London School of Economics economist Peter Boone, who found that aid financed consumption rather than investment. (Financing consumption of a few poor people is not so bad, but the Big Push hoped for the society-wide transformation that would come from aid financing investment and growth.) Boone addressed the problem of reverse causality by using political factors to predict which countries got aid—usually rich countries gave aid to poor countries that were their political allies, or with which they had a colonial association. When aid is predicted by political factors that are themselves unrelated to growth outcomes, you can examine whether the predicted values of aid caused higher growth. Even controlling for possible reverse causality, Boone found aid to have zero effect on investment. Similarly, controlling for reverse causality, he found aid to have zero effect on growth. *The Econo-*

Fig. 2. Aid and Growth in Africa (ten-year moving averages)



mist publicized Boone's research, and it was widely known in the aid policy-making community.

Boone's research created a terrible disjunction: aid policy was based on the premise that aid raises growth, but now the best study of the question was saying that this premise was false. Soon a study appeared to fill the vacuum between policy and research,[7] an academic study by World Bank economists Craig Burnside and David Dollar.[8] I am not saying that Burnside and Dollar consciously set out to reach a predetermined conclusion, which would obviously have been bad science. Rather, theirs was a serious scientific study; there were also other equally serious studies that found different results. The point is, the policy community chose to believe the finding that was most favorable to the aid policies it wanted to implement.

Burnside and Dollar related growth rates in developing countries to foreign aid received, as figure 2 does for Africa. However, their new twist distinguished between aid recipients who had "good" policies (measured by things such as low budget deficits, low inflation, and free trade) and those with "bad" policies. Their hypothesis was that good policy increased the payoff to aid, so growth should be related to aid among countries with good policy. This was intuitively appealing, because it recognized that bad governments

could be the problem, as discussed in the previous section. If poor countries had good governments, then perhaps aid would increase growth after all.

Their sample consisted of six four-year time periods running from 1970–1973 to 1990–1993. In many of their tests, they found that when a country both got more foreign aid and had good policy, growth went up. They summarized: "We find that aid has a positive impact on growth in developing countries with good fiscal, monetary, and trade policies but has little effect in the presence of poor policies" (p. 847).

Their paper reinforced the hope that aid could accomplish great deeds, which fed a policy recommendation to increase foreign aid to a country only if that country's policies were good. In early 2002, *The Economist* rebuked then U.S. Treasury secretary Paul O'Neill for his skepticism about foreign aid on the grounds that "there is now a strong body of evidence, led by the research of David Dollar, Craig Burnside[,] . . . economists at the World Bank, that aid does boost growth when countries have reasonable economic policies." An article in the *New Yorker* in 2002 chimed in that "aid can be effective in any country where it is accompanied by sensible economic policies," based on the Dollar and Burnside study.

President George W. Bush was apparently reading the *American Economic Review* as well. On March 14, 2002 (any coincidence in timing with the war on terror is purely intentional), he announced a five-billion-dollar increase in U.S. foreign assistance, about a 50 percent increase.[9]

The White House followed up on November 26, 2002, with the creation of the Millennium Challenge Corporation (MCC), whose job is to administer the five-billion-dollar increment in foreign aid. Arguing that aid works only with good policies, the administration announced sixteen indicators of country performance to guide the selection of countries to receive MCC aid—three of the indicators were versions of the Burnside and Dollar policy measures (most of the rest were measures of quality of institutions). On its Web site, the White House said that the new aid was motivated by the idea that "economic development assistance can be successful only if it is linked to sound policies in developing countries."[10]

In May 2004, the Millennium Challenge Corporation announced the selection of sixteen "good policy" countries eligible to apply for its aid grants from fiscal year 2004 funds.[11] In March 2005, the MCC reached its first

agreement with a "good policy" country, a Millennium Challenge Compact with Madagascar.

How much can we rely on the original study that sent this freight train down the tracks? A study I did with Ross Levine (Brown University), and David Roodman (Center for Global Development) used the exact same techniques and specifications as Burnside and Dollar, but added new data that had become available since Burnside and Dollar did their study. We also hunted for more data in their original sample period (1970–1993). We found more data even over their sample period by consulting the original sources rather than secondary sources. Using updated data, we did the same statistical exercise with four-year averages with the same control variables, including terms for aid/GDP, and their policy index (a weighted average of budget deficits/GDP, inflation, and an index of openness to trade). We found no evidence that aid raised growth among countries with good policies, indicating no support for the conclusion that "aid works in a good policy environment." Our study was published as a comment on Burnside and Dollar in the *American Economic Review.*

The original researchers and other researchers may have tried many different statistical exercises, but the aid policy community is tempted to select the study that confirms its prior beliefs (known as "confirmation bias")—even though other statistical exercises may have found no evidence for it. Applying new data to the old statistical exercise is a good test of whether the original result really holds and is not just confirmation bias. The statistical exercise with the new data is constrained by the old statistical exercise, so you are not searching among many different exercises for the one confirming prior beliefs. Even good first-round research can suffer from confirmation bias.[12]

The cycle is now starting all over again. After my co-authors and I found no evidence for the "aid works in a good policy environment" conclusion, a new study came out by Michael Clemens, Steven Radelet, and Rikhil Bhavnani (hereafter denoted CRB) of the Center for Global Development. I respect these authors a lot and think they were following high academic standards. Their new twist on the statistical exercise was to separate aid that could be expected to have an impact on growth in the short run from aid that had either a humanitarian purpose or could work only in the long run,

such as health or education aid. They found a strong growth effect for their preferred category of aid ("short-impact aid")—and not only when there was good policy in the recipient country.

Again, the original research was scientific; the use of it was less so. Aid advocates once again regarded the new finding as supporting their recommendations. The UN Millennium Project Report in January 2005 cited the CRB study as providing support for the project's proposal of massive increases in aid.[13] The Blair Commission for Africa, in March 2005, recommended an immediate doubling of aid to Africa, and cited the CRB findings as support for its recommendations.[14]

Unfortunately for these recommendations, researchers again subjected the positive aid findings to further scrutiny and found them wanting. The chief economist of the International Monetary Fund, Raghuram Rajan, and IMF researcher Arvind Subramanian subjected the CRB finding to statistical testing. They used the simplest specification to control for possible reverse causality from adverse country characteristics to aid receipts, and a standard specification for the determinants of growth. In their May 2005 study, Rajan and Subramanian found no evidence that either "short-impact aid" or any other type of aid had a positive effect on growth.[15] For good measure, they also tested the Burnside-Dollar hypothesis yet again, and found no evidence that "aid works in a good policy environment."

They also considered some alternative explanations as to why foreign aid does *not* raise growth. One well-justified complaint about aid is that it is often tied to the purchase of goods and consultants from the donor country, which may prevent the aid from bringing much growth to the recipient country. Another possibility is that the donor country gives the aid for political reasons, which again may limit the aid's effectiveness. There is one simple test of these explanations—only aid from national aid agencies (bilateral aid) is tied, while aid from the World Bank and regional development banks (multilateral aid) is not. Similarly, bilateral aid is far more politicized than multilateral aid. Rajan and Subramanian found, however, that there was no difference between the effects of bilateral and multilateral aid on growth. Another test they did was to see if having a high share of aid coming from Scandinavian countries (which are less motivated by political alliances and do less aid tying) was associated with faster growth—they found it was not.

With so little light shed by statistical studies of growth, the big picture is perhaps still useful in evaluating the aid and growth relationship. Do we believe that African growth would have declined even more sharply from the mid-seventies to the present but for the tripling of aid as a percentage of income?

There is another aspect to both the Burnside-Dollar and CRB studies that aid agencies and advocates have chosen to emphasize much less. To the extent that they found any growth effect at all, both Burnside-Dollar and CRB found that the larger the aid already was, the smaller was the additional growth benefit from that additional injection of aid. In the CRB study, their category of aid had a *zero* effect on growth when it reached 8 percent of the recipient's GDP, and after that the additional aid had a *negative* effect on growth. This feature of their results directly contradicts the Big Push reasoning, which is that small sums don't help because you need a sufficiently *large* mobilization of aid to fix all the big problems simultaneously (that's why it had to be a *Big* Push). This theory implies that the larger the aid is already, the larger the additional growth benefit from an additional injection of aid. This is contrary to CRB. There are already twenty-seven countries with aid receipts over the 8 percent of GDP at which the CRB-estimated effect of additional aid turned negative; if the donors adopt the current Big Push proposals, virtually all low-income countries (forty-seven of them) will be far above that level.[16] Unfortunately, the Blair report and the Millennium Project report select research results to support a Big Push idea that is contradicted even by the selected studies themselves.

We can also check on some of the intermediate steps in the aid and growth story. Jeffrey Sachs and co-authors previously predicted that large aid increases would finance "a 'big push' in public investments to produce a rapid 'step' increase in Africa's underlying productivity, both rural and urban."[17] Alas, we have already seen this movie, and it doesn't have a happy ending. There is good data on public investment for twenty-two African countries over the 1970–1994 period. These countries' governments spent $342 billion on public investment. The donors gave these same countries' governments $187 billion in aid over that period. Unfortunately, the corresponding "step" increase in productivity, measured as production per person, was *zero*. Perhaps part of the reason for this was such disasters as the five billion dollars spent on the publicly owned Ajaokuta steel mill in Nigeria, begun in 1979, which has yet to produce a bar of steel.[18]

What about the elusive "takeoff" into self-sustained growth? If we define "takeoff" as a one-time shift from zero growth to sustained positive growth, there are surprisingly few countries whose development experiences fit this description. Most countries that escaped from extreme poverty did so with gradually accelerating growth, sometimes punctuated by crises of zero or negative growth. Japan is the only rich country that became rich by means of a takeoff. In more recent data, there are only eight countries (all in South and East Asia) that had a takeoff in the period 1950–1975: China, Hong Kong, India, Indonesia, Singapore, South Korea, Taiwan, and Thailand. Three of the eight countries had aid-to-GDP ratios above the norm: Indonesia, South Korea, and Taiwan; in the others, aid did not play an important role in their takeoff. Moreover, other countries got high foreign aid over this period and did not take off. Statistically, countries with high aid are no more likely to take off than are those with low aid—contrary to the Big Push idea.

So the aid Planners keep pouring in aid resources with the fixed objective of stimulating higher growth, although evidence does not support an effect of aid on growth.

The Problem of Evaluating the White Man's Burden

One thing that makes the aid debate so contentious is that it is not easy to evaluate the effect of Big Pushes. Actually, one argument *against* Big Push programs is that they are so hard to evaluate. All of the major interventions of the White Man's Burden have similar evaluation difficulties.

My daughter Grace asked me several years ago as we were driving on the Washington Beltway, "Daddy, why do ambulances make so many accidents?" Of course, now that she is nine, Grace knows that the presence of an ambulance at every accident is a consequence rather than a cause of the accident. The presence of the IMF and World Bank and aid agencies at country crises is surely a consequence rather than as a cause of the accident. This is the selection effect—ambulances show up at car wrecks, not at tailgate parties.

This is the same as the reverse causality problem just mentioned about foreign aid. Once you control for the selection effect, you find that things could have been even worse without the aid. This is what is called the counterfactual question: How does what happened with the White Man's Burden compare to what would have happened *without* the White Man's Burden?

There are several approaches that can partially (but not completely) resolve the selection problem and address the counterfactual question for the big programs of the White Man's Burden. One is to find factors that are *not* themselves determined by an economic crisis and to ask if the variation in the White Man's Burden programs associated with those factors had positive or negative effects. If some ambulances just patrolled a neighborhood because the mayor lived there, we could evaluate the effect of the ambulance patrol on survival from heart attacks by comparing what happened to the heart attack victims who lived next door to the mayor with what happened to victims elsewhere. All the statements I make earlier about "controlling for reverse causality" are based on some method like this. The method is never perfect. For example, it won't work if being the mayor's neighbor has a direct effect on your survival fitness that has nothing to do with the ambulance patrol—that would contaminate the comparison between the mayor's neighbors and others.

Another is to analyze cases where there were repeated White Man's Burden efforts. If ambulances keep showing up at the accident, but the injured still do not get any help for their injuries, you would question how good the ambulance service is. Unfortunately, these methods are not always available, but we still need some way of judging real-world programs that are going ahead anyway. The last resort, which is far from perfect but still provides insight, is simply to describe the results of a program or intervention. If a program is associated with a disastrous outcome, you need to believe that things would have been even more disastrous without the program. If all the ambulance patients are always DOA at the hospital, it's hard to believe that the ambulances are doing any good. This book will use all of these methods.

Alternative to the Legend of Development

Fortunately, there are some people who work on aid and poverty who do have a more neutral, modest mind-set. These are mainly academic economists, who are woefully short of a plan to eradicate poverty or achieve world peace. They are not good visionaries and are terrible at public relations. They experiment and come up with smaller but more useful things that outsiders can do to help the poor, which they subject to ruthless testing to see if they really work.

With smaller interventions, more rigorous evaluation is available to address the counterfactual question. One scientific method used is the controlled experiment. The control group represents what would have happened to the treatment group without the treatment. The difference between the two groups is the effect of the treatment.

The researcher must choose both groups randomly—say, a lottery determines who is in the treatment group and who is in the control group. If you assign people based on some other criteria, then the difference between the treatment and the control groups could reflect the selection criteria rather than the treatment. For example, if you assigned people with more severe problems to the treatment group, then you could get a spurious negative effect of treatment. (You don't want to test the effect of ambulances on health by comparing the health of ambulance patients to that of the man on the street.) Conversely, if you assigned those with the most potential to benefit from the treatment to the treatment group, then you would get an overestimate of the treatment effect.

The U.S. Food and Drug Administration (FDA) follows this approach when it decides if new drugs work. It first does randomized treatment and control groups. If the drug works for the treatment group compared with the control group, then everyone gets the drug.[19] The FDA may feel a stronger incentive to use scientific methods than aid agencies because it is democratically accountable to voters, who are the same group that will be using FDA-approved drugs. If the drugs do not actually work among the general population, or if they generate side effects that kill off the patients, the new

drug users (or their survivors) will complain to the politicians. The politicians will put the heat on the FDA, which will then take more care to test scientifically what really works but does not include bad side effects. The intended beneficiaries of the aid agencies—the poorest people in the poor countries—don't have a similar way to put heat on the agencies.

The Dutch aid organization International Christian Support Fund (ICS) distributed deworming drugs to schoolchildren in southern Busia district, Kenya, where 92 percent of children were infected with intestinal worms that caused listlessness, malnutrition, and pain. Economists Michael Kremer of Harvard and Edward Miguel of Berkeley took the randomized approach in assessing the effects of deworming drugs. Kremer and Miguel studied programs that administered drugs and that conducted worm-prevention education for schools in Busia district, Kenya. The ICS project phased in the programs over three years, so there were three groups for Kremer and Miguel to study. In the first phase, phase I schools could be compared to phase II and III schools. In the second phase, phase I and II schools could be compared to phase III schools. Kremer and Miguel were able to identify a positive effect of deworming drugs on school attendance and a zero effect of deworming education on worm infection rates. The deworming drugs decreased school absenteeism by one quarter. "Pupils who had been miserable now became active and lifeful," said schoolteacher Wiafred Mujema.[20]

Kremer and Miguel's practical scientific approach identified a way to help children stay in school (give them deworming drugs) and also identified other methods that didn't work (educate children on behavior to prevent worm infection). After the results came in, ICS expanded its deworming program; it now covers all of Busia district plus neighboring Teso district. Other aid organizations have imitated the deworming program around the world. If this practical, critical approach spreads, much more of the foreign aid dollars available could actually reach the poor! And then maybe aid advocates could make the case for more foreign aid.

Not all scientific work is on randomized trials of individual interventions; some is on statistical analysis of aggregate data. And not all findings are positive; some tell policymakers and aid officials what *not* to do. Researchers Thorsten Beck, Asli Demirgüç-Kunt (both at the World Bank) and Ross Levine (Brown University) studied whether small and medium enterprises (SMEs) were catalysts for poverty reduction. The aid community believes in SMEs' catalytic role, with the World Bank having lent $10 billion to support SMEs over the last five years.[21] USAID spends about $170 million a year on microenterprise promotion.[22]

Unfortunately, in a thorough review of both firm-level and macroeconomic data, Beck, Demirgüç-Kunt, and Levine found no evidence that SME promotion created economic growth or poverty reduction. They sensibly point out that there is nothing sacred about small firms. Firm size reflects many things, such as whether it is more efficient to handle transactions in the marketplace or inside the firm or whether a given technology is more productive at a large or small scale. Some countries and sectors may be more competitive with small firms; others with large firms. There is no reason that aid Planners should try to artificially promote one size firm versus a different size firm.

This skeptical paper caused panic in the pro-SME aid community. I myself got an e-mail from a contractor for an aid agency asking me to write a paper refuting Beck, Demirgüç-Kunt, and Levine. I declined, explaining that academic researchers usually don't first find the defendant guilty, and then afterward hold a trial.

Other development researchers study many aspects of economic policy, institutions, and politics of poor countries to identify things that seem to contribute to development, based on statistical evidence from household-level, firm-level, and country-level data. These studies point to piecemeal ways to move toward prosperity, such as keeping roads in good condition or pursuing good monetary policies to keep inflation low—not big answers or comprehensive reforms.

Unfortunately, the stubborn survival of the legend of the Big Push, despite evidence of its failure, has continued to foster the planning approach to development. The Planners' response to failure of previous interventions was to do even more intensive and comprehensive interventions. The next two chapters examine some more of the economic and political complexity that makes these top-down plans fail.

SNAPSHOT: TEENAGE PARAMEDIC

THE DEATH OF MOTHERS during childbirth is virtually unknown in rich countries, but it is tragically common in poor countries. Instead of the new life with childbirth that many of us in rich countries count as the most supreme moment in a lifetime, a family in a poor country must too frequently confront the death of the wife and mother (and often of the newborn baby as well). The woman herself dies in agony due to such causes as the seizures and severe agitation of eclampsia. Eclampsia (and other causes of death in childbirth) can be prevented with prenatal care that recognizes the warning signs and gets the woman to the hospital once she displays those signs. Providing such prenatal care is a major challenge in poor countries.

Feroza Yasmin Shahida is a nineteen-year-old Bangladeshi girl from a poor peasant family. She got a scholarship from a program run by USAID and the World Bank to finish secondary school. Now she is a bicycle paramedic responsible for 515 families in the countryside around Savar, Bangladesh. She is the only health worker these 515 families have. She earns twenty-five dollars a month working for Gonoshasthaya Kendra (GK), the "People's Health Center."

GK is the brainchild of Dr. Zafrullah Chowdhury (affectionately called Dr. Zaf), a Bangladeshi doctor who returned from Britain after Bangladesh won its independence in 1971. Dr. Zaf trained teenage girls to treat common ailments, deliver prenatal and postnatal care to pregnant women, and refer any emergencies to the hospital that he built. Foreign donors and the Bangladeshi government gave Dr. Zaf money, but he also charged his poor patients modest fees to expand services further. He found that even the poor were willing to pay for good service. Charging the poor modest fees for health care—a notion that outrages Planners and anti-globalization activists—is a way to increase accountability for delivering health services. If the villagers don't get good service after they have sacrificed to pay for it, they loudly complain. Dr. Zaf says, "If a woman dies, the worker has to face the village. Accountability is here." GK has been successful in lowering maternal deaths in childbirth, infant mortality, and also the number of children that women choose to have. Maternal mortality in the area covered by GK is one fourth of the national average.

If Feroza continues to be one of Dr. Zaf's best paramedics, she will be promoted to supervisor, with a raise to one hundred dollars a month and a scooter instead of a bicycle. Dr. Zaf searched for and found a piecemeal way to improve the lot of the Bangladeshi poor.

SNAPSHOT: THE SECRET HISTORY OF GRAMEEN BANK

Mohammad Yunus of Bangladesh, the founder of the Grameen Bank and the main inventor of microcredit schemes, didn't start off with the goal of giving poor people credit. As Columbia University Business School professor Bill Duggan tells the story in a great book about people who find things that work, *Napoleon's Glance,* Yunus started off with the conviction that the Green Revolution, and irrigation, was the answer to poverty in Bangladesh. His doctoral dissertation at Vanderbilt University was titled "Optimal Allocation of Multi-Purpose Reservoir Water: A Dynamic Programming Model." His first attempt to help the poor was to sponsor tube wells for irrigation during the dry season so farmers could grow two crops a year. Yunus gave the farmers a loan out of his own money to finance the scheme. The farmers reaped a good harvest. Ironically for the founder of the idea that the poor can be a good credit risk, the farmers didn't fully repay Yunus, and he lost money. But he persisted, with the city boy visiting as many rural villages as possible to try to understand how to help. He encountered a woman named Sufiya Begum making a bamboo stool. Begum made a pitiful two cents on every stool, mainly because a moneylender charged her a very high interest rate (around 120 percent per year) to advance her the bamboo. Yunus realized that very small loans to very poor people could make a big difference in their lives. Contrary to conventional wisdom at the time, he realized that the poor had a huge untapped demand for credit. He experimented, and found that microcredit borrowers would repay the loan in order to get access to future loans and also because of peer pressure from other microcredit borrowers. His first loan was to Sufiya Begum, who started a successful peddling business with the money, instead of making more bamboo stools. There was a huge demand for such loans, and Grameen Bank became the legend that it is today, with imitators from all over the world. Yunus was a Searcher.

Microcredit is not a panacea for poverty reduction that some made it out to be after Yunus's discovery. Some disillusionment with microcredit has already come in response to these blown-up expectations. Microcredit didn't solve everything; it just solved one particular problem under one particular set of circumstances—the poor's lack of access to credit except at usorious rates from moneylenders.