Alternatives to Godot: Inducing Turnarounds in Failing States

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I. Introduction: Waiting for Godot

In this paper we discuss the policy implications of recent research on policy turnarounds in failing states (Chauvet and Collier, 2005). By failing states we mean countries that combine low income with bad economic policies and institutions. Such states are 'failing' in two senses. Most directly, they are failing their citizens: populations in most of the low-income world live in countries that are growing rapidly, these countries are stagnating. Yet more troubling, low-income countries that fail to grow are living dangerously: the statistical patterns suggest that they are at significant risk of falling into civil war, a condition which is both ruinous and highly persistent.

Whether at peace or at war, failing states are a hazard to others as well as to their own citizens. At war, the adverse spillovers are fairly evident: there is economic and social damage to neighbors. The existence of territory outside the control of a recognized government invites global problems such as drugs, crime and terrorism (Collier *at al*., 2003). As we discuss below, even at peace failing states damage their neighbours. Indeed, typically the cost to neighbors is greater in aggregate that the costs to citizens of the country itself. Hence, the wider world has a substantial interest in encouraging these countries to turn themselves around. As discussed below, we estimate that the total economic benefit from turning around the typical failing state is of the order of $100bn. This is before we factor in any of the global security benefits.

Failing states need change: in particular they need policy and institutional reform. The astonishing pace of globalization, and its encouraging impact on much of the developing world may suggest that time is on the side of such change. Our analysis suggests that this is wishful thinking. If change is left entirely to autonomous processes within the failing states, we are likely to have a long wait. Specifically, according to our analysis, the chances of substantial and sustained turnarounds are so low that the mathematical expectation of the duration of failing state status is 59 years. Hence, recent entrants to the failing state category, such as Cote d'Ivoire, would not be expected to turn around within the lifetimes of present policy actors. In effect, this is further support for the notion that the developing world is bifurcating into a large group that is broadly on track to develop, and a smaller group that is on track to stagnate in poverty and disorder. Inaction condemns us to waiting for Godot.

This is, of course, not by itself a case for international action. The payoff to turnaround can be very high, and the prospects of autonomous turnaround can be negligible yet the best course can indeed be to do nothing. This is because, a priori, it is entirely possible that international actors lack any effective non-military instruments to induce sustained change, and that military intervention is precluded for one reason or another.

The reality is that to the extent that policies and institutions in failing states are controlled by anybody, they are controlled by their own governments and elites. International actors have for years tried to advise, reward, and force change by non-military means, but to very little effect. Past efforts have basically failed and this must reflect the fact that the

task is enormously difficult, at least for external actors. The purpose of this paper is to discuss the evidence that despite these past failures, there has been sufficient success for us to learn what works and then to scale up the successful strategies. Success turns out to be feasible, and indeed, to be cost-effective. But if our analysis is correct, it will require interventions that are organized on a very different basis from the efforts of the past.

In Section II we describe more precisely what we mean by a sustained policy turnaround in a failing state. The concept is not controversial. Almost everyone agrees in practice on what a low-income failing state looks like. It is, for example, far less controversial than the desirable content of policy reform in a mid-performing, middle-income country. However, turning this concept of a failing state into a set of precise criteria that can be used empirically turns out to be replete with pitfalls. Having defined a turnaround, in Section III we then turn to explanation. We begin with the preconditions for turnaround, especially those that can be influenced by external actors. We then move on from the preconditions to the circumstances in which some signs of change are already present. An incipient reform has begun and the issue is what can be done when to encourage this reform to maintain momentum and achieve substantial sustained reform, and what can be done to prevent it from collapsing altogether. In Section IV we turn to the cost and benefits of policy interventions. We first estimate the benefits of turning around a failing state, and then compare it with the costs of doing so. This enables us both to rank interventions by their cost-effectiveness, and also determine, to an extent, the appropriate scale of an intervention, that is the point at which marginal costs and benefits are equated. In Section V we draw out the implications for strategies of external intervention, comparing aid to military and political interventions. Aid is potentially by far the most cost-effective of these strategies, but aid agencies would need to operate in a radically different manner were they to make a serious and large-scale attempt to induce policy turnarounds.

II. What is a Policy Turnaround in a Failing State?

To be a policy turnaround in a failing state, the state must start from failure. Our universe is thus going to be that of countries that are initially in such a condition.

Our entire analysis is confined to low-income countries. Some middle-income countries might reasonably be described as failing states, but they call for a different analysis. Just as families can famously be unhappy in many different ways, so are the paths to state failure various. Our routes all start with low per capita income.

Beyond this, failure can be defined in terms of choices or outcomes. In some sense if the state experiences economic decline, it is failing, if it grows rapidly, it is not failing. However, especially among the low-income countries, growth outcomes reflect both structural differences in opportunities, such as whether a country is landlocked, and shorter term matters of fortune such as movements in commodity prices and climatic shocks. Possibly because of these non-agency factors, there appears to be little rhyme or reason to growth turnarounds (Hausmann *et al.*, 2004). There are therefore advantages to

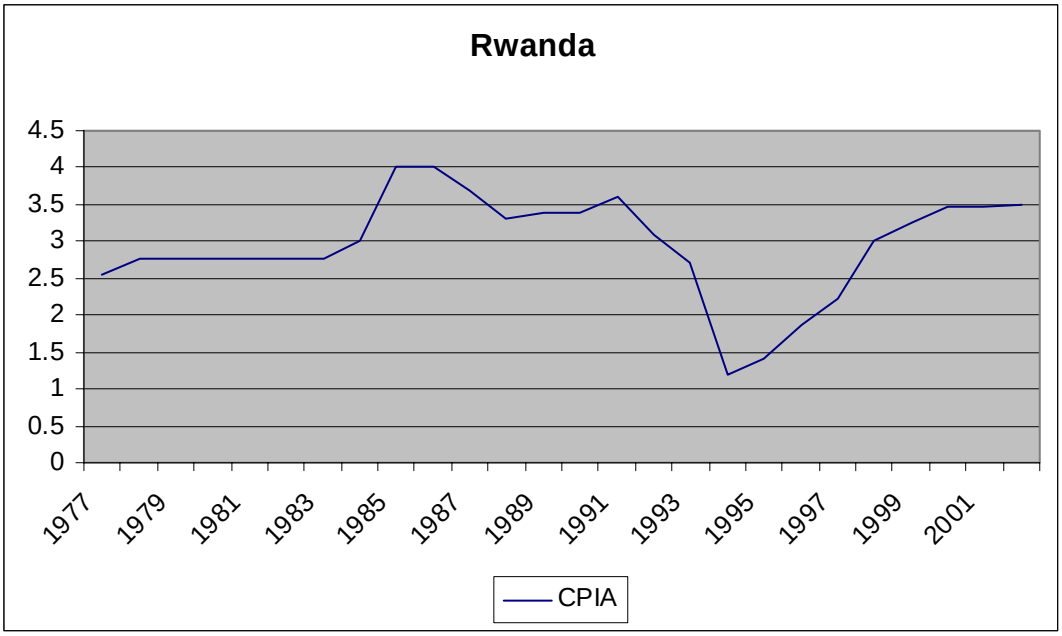
focusing on things that are more directly under the control of actors, namely policies and institutions. This is our approach. The main disadvantage of this approach is that economic policies and institutions are profoundly non-quantitative, and their assessment is heavily contested. There are indeed some quantitative measures, for example the fiscal deficit, but these are often better seen as outcomes than as being directly under the control of policy makers. Here we use the Country Policy and Institutional Assessment (CPIA) of the World Bank. This has several important advantages and some severe disadvantages. It is available for a long period, 1977-2002, and is intended to be comparable across countries and, with minor qualifications, over time. It is intended to assess the overall economic efficacy of government choices regarding policies and institutions. It is inevitably subjective, but this is implicit in the nature of the concept. Possibly its main drawback for our purposes is that it can be gamed by the staff who are making the judgment. A higher CPIA attracts a larger lending program, and this can be sometimes advantageous to the staff concerned. Both in principle and in practice, such manipulation is policed but it is unlikely to have been eliminated. A common objection to the CPIA is that it inadvertently incorporates growth outcomes: staff working on a rapidly growing economy will tend to assess policies and institutions more favourably than objectively similar policies and institutions in a slow-growing economy. While this is surely the case, for our purposes it is no bad thing if, in assessing policies, the assessors are influenced by what actually happens in the country concerned as well as by some textbook norm of what they imagine should usually happen as a result of the policies being adopted.

A failing state must therefore be failing both in terms of having a low per capita income, and in having policies and institutions which are in some sense bad. A further area of controversy with the CPIA is that its assessment of what constitutes good policies from time to time incorporates intellectual fashions such as 'neo-liberalism' (in the 1980s) or 'good governance' (post-2000). While there is some truth in this critique, fortunately, we are interested only in the very bottom of the range of the CPIA. When, for example, one looks at the policies and institutions of the government of Angola in the 1980s, it really does not matter whether one's intellectual position is neo-liberal or social democrat: even some members of the then government regarded its choices with a degree of shame. The CPIA is ranged on a five-point scale. By convention anything below 3 is seen as problematic. We adopt the lower threshold of 2.5: a failing state must have a CPIA score below this level. Any such rule generates the potential for errors of two types: inclusion of countries that are not really failing, and exclusion of countries that really are. By adopting such a low threshold we have, we believe, eliminated the first type of error: few people would seriously dispute the countries included. We have most certainly opened ourselves to the second type of error: we have missed some failing states, perhaps quite a lot of them. However, our purpose is not to certify states, but rather to generate a clean sample of countries for reliable analysis.

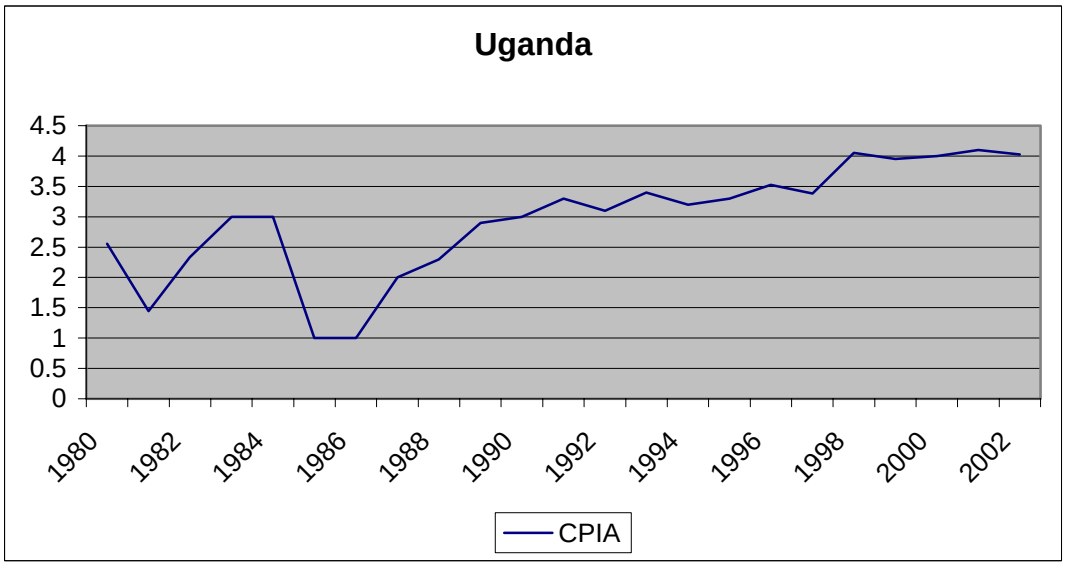
However, not all low-income states that have had a CPIA score below 2.5 have been failing states. For a number of countries the CPIA score briefly crashed below 2.5 and then rebounded. Such temporary crashes are not of interest to us. It is surely much easier to restore a country to reasonable policies if it has only just abandoned them than if it has

been stuck with bad policies for a long time. Rwanda, illustrated below, is surely such a case: better interpreted as a recovery than as a turnaround. Indeed, the temporary crashes we observe in the CPIA data may sometimes be spurious assessments of Bank staff that are subsequently reversed. We therefore require that the CPIA stay below 2.5 for a continuous period of four years. Even this rule generates some close calls. We illustrate the case of Uganda, a country treated in much of the literature as the best example of a successful African turnaround. On our criteria Uganda is a recovery, albeit with overshooting to a level of policy superior to that attained prior to the crash.

Recovery with no overshooting



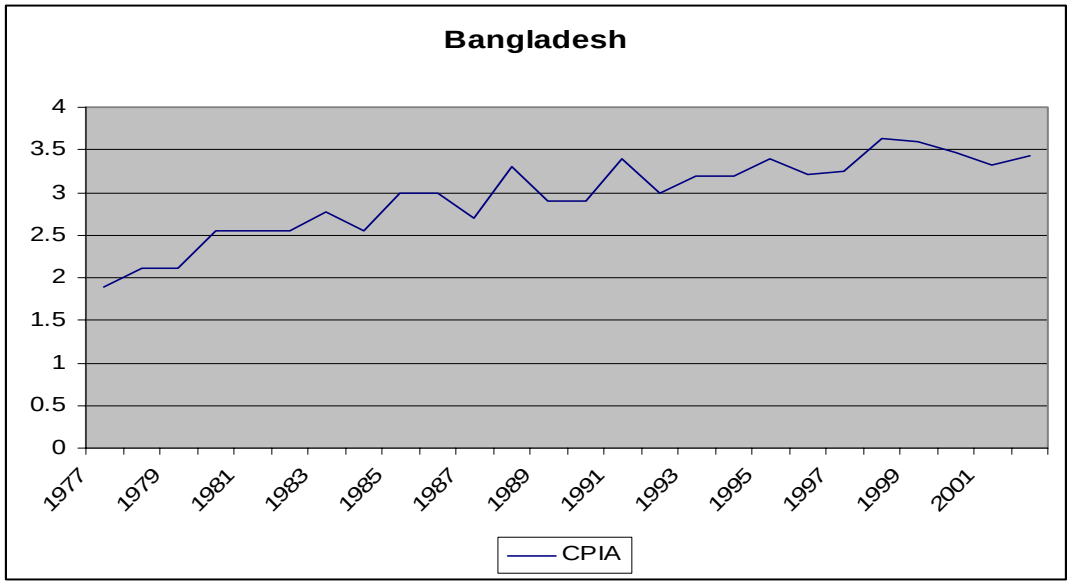
Recovery with overshooting



These criteria give us lists of states that can be classified as failing, year-by-year. As of 2002, our most recent data, there were only eight such states: Angola, the Central African Republic, Haiti, Liberia, Sudan, the Solomon Islands, Somalia, and Zimbabwe. It would surely be difficult to argue with any of these assessments. A further five states narrowly missed either because their CPIA scores had risen to slightly above 2.5, or because oil had raised per capita income above the least-developed country threshold. These were Guinea-Bissau, Turkmenistan, the Democratic Republic of the Congo, Tajikistan, and Sao Tome, Principe. Since there is some evidence for grade inflation in the CPIA, and since oil may create as many problems as it alleviates, these five might also reasonably be thought of as failing states.

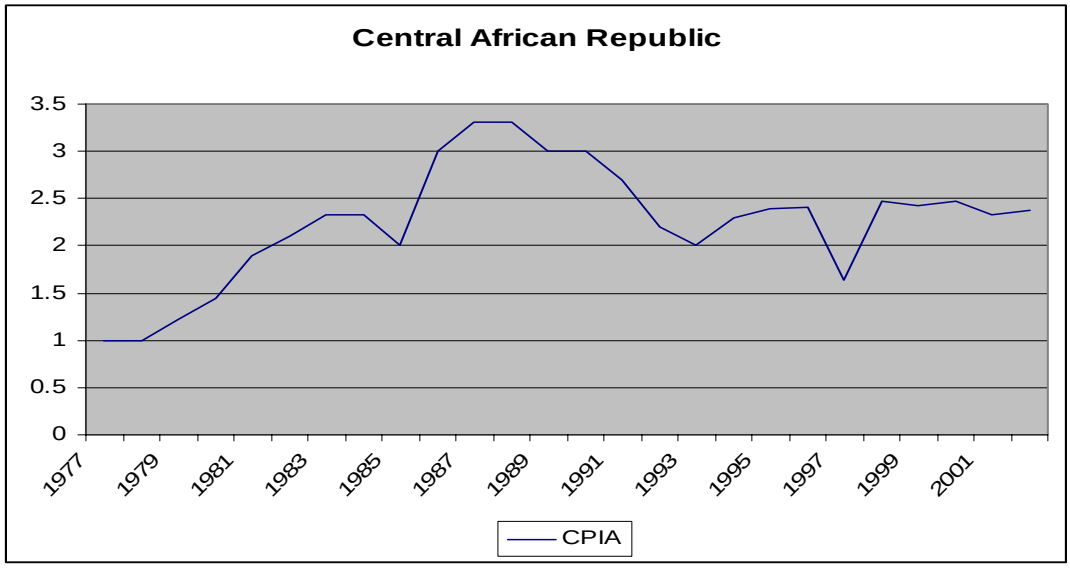
We next need to clarify what we mean by turnarounds. Of course the simplest criterion would be the converse of the conditions for state failure, but this would rapidly lead to absurdities. If the CPIA rises from a run of 2.4s to 2.6 we do not want to regard this as a turnaround. Given the subjective nature of the CPIA such small improvements might be entirely spurious, and even if they are genuine they hardly constitute a significant event. We therefore confine ourselves to turnarounds in the CPIA score that are so large as to be unmistakable. We work with an improvement of 1.4 or greater. We allow this to be achieved over any time period. Thus, for example, a country might start from a run of 2.4s and then increment by 0.1 each year for 14 years, and this would count as a turnaround. The case of Bangladesh, illustrated below, is an example of such a very gradual turnaround. There is no dramatic event' worthy of particular notice, but over a quarter century policy, institutions and governance improve from abysmal to adequate.

Slow and sustained turnaround



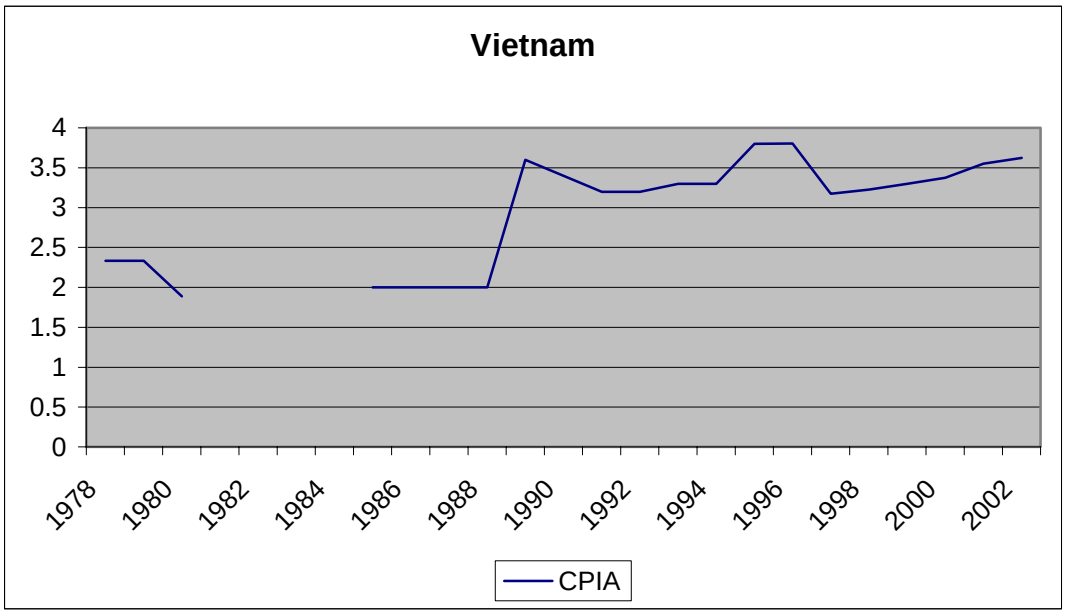
However, a large improvement in the CPIA is not enough, it must in some sense be sustained. We impose the requirement that it must remain above 3.0 for at least five years. In choosing five years we were balancing two considerations. A very short period captures as turnarounds improvements that rapidly collapse, and where the seeds of that collapse can therefore reasonably be seen to have been there in the original attempt at reform. By contrast, were we to require a very long period of sustained improvement we would exclude situations like Indonesia. The improvement in Indonesia began in 1967 and was broadly sustained until the collapse associated with the Asian financial crisis of 1998. It seems to us unreasonable to attribute that collapse to failures in the original reforms. It is surely better to see the reforms as being successful, but that other events occurred subsequently. The sort of series we classify as unsustained turnarounds are illustrated by the case of the Central African Republic.

Unsustained turnaround

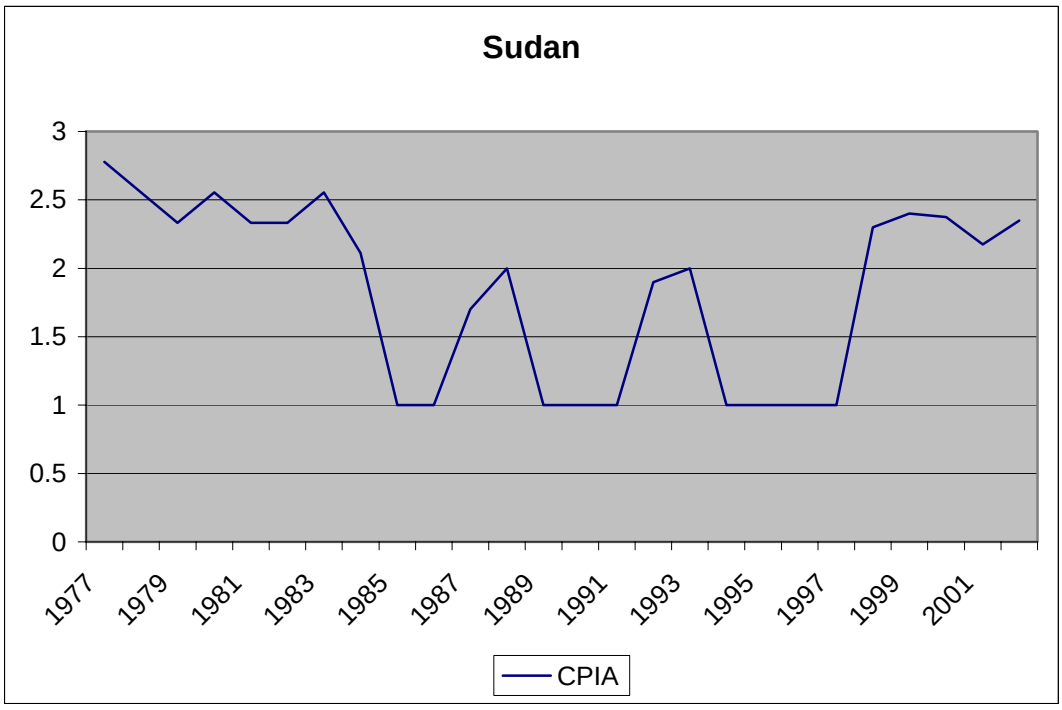


These marginal cases, where precise definitional rules matter, should not obscure the fact that most cases are pretty clear. There is no confusion between the case of Vietnam, a clear case of a sustained turnaround, and the Sudan, an equally clear case of a country mired in poor policies.

Rapid and sustained turnaround



No turnaround



Having defined both a failing state and a sustained turnaround, we next survey the implications of our criteria. In Table 1 we list the countries that have at some stage been failing states, and the various possibilities of turnaround. We should emphasize that the category of failing states without sustained turnarounds is decidedly not the same as that

of states that are currently failing. A country can exit the status of being a failed state even if to does not achieve a sustained policy turnaround. It can achieve improvements that are more limited, for example having an improvement in the CPIA from 2.4 to 2.6. Alternatively, a country can exit the status of failed state while retaining bad policies if it is lucky enough to grow beyond our income threshold. For example, Equatorial Guinea discovered large oil supplies and so per capita income has risen above the income threshold, even though in practice, given the policies pursued by the government, oil wealth has created a few multimillionaires and otherwise not significantly changed living conditions. It is in the process of passing from being a failing state on our criteria, to being a problem-stricken middle-income country.

Table 1: Failing States and Sustained Turnarounds

Failing States without Turnarounds	Sustained Turnarounds		Unsustained turnarounds		Recoveries	
No Turnaround	Slow		Slow		Slow	
Angola	Bangladesh	1977-1988	Central Afric. Rep.	1978-1986	Mauritania	1989-1995
Chad	Burkina Faso	1987-1992	Madagascar	1978-1988	Sri Lanka	1986-1991
Comoros	Cambodia	1991-2001				
Congo, Democratic Rep.	Egypt	1988-1998				
Congo, Rep.	Ethiopia	1991-1998				
Equatorial Guinea	Ghana	1978-1985				
Guinea-Bissau	Guinea	1977-1987				
Haiti	Indonesia	1977-1985				
Laos	Lesotho	1987-1996				
Liberia	Mozambique	1986-1999				
Myanmar	Nepal	1985-2001				
Nigeria	Niger	1993-2001				
Papua New Guinea	Pakistan	1979-1987				
Solomon Islands	Senegal	1993-2000				
Somalia	Tanzania	1985-1998				
Sudan	Ukraine	1997-2002				
Tajikistan	Rapid		Rapid		Rapid	
Turkmenistan	Albania	1991-1993	Burundi	1982-1986	Cameroon	1993-1998
Uzbekistan	Benin	1987-1991	Sao Tome & Princ.	1986-1990	Gambia	1985-1989
Yemen	Côte d'Ivoire	1992-1996	Sierra Leone	1989-1994	Mali	1987-1990
Zimbabwe	Guyana	1987-1991	Togo	1980-1985	Uganda	1986-1990
	Honduras	1989-1992			Rwanda	1994-1998
	Nicaragua	1989-1993			Zambia	1988-1992
	Vietnam	1988-1989				

III. Promoting Turnarounds

Having established what we mean by a turnaround in a failing state, we are now ready to discuss what generates them. The following discussion is based on previous econometric work (Chauvet and Collier, 2005). We first consider the preconditions for a turnaround, and then interventions that help an incipient turnaround to maintain momentum.

The Preconditions for Turnarounds

Our discussion of the preconditions is based upon the results of a statistical analysis that used the standard technique of logit regressions. We estimated the probability of a turnaround, year by year, among all potential turnaround countries. We perform specification tests to establish which among a wide range of potential characteristics are actually statistically significant in explaining the preconditions for turnaround. There are only three such characteristics. Starting from being a failing state, a country is more likely to achieve a sustained turnaround the larger is its population, the greater the proportion of its population that has secondary education, and if it has recently emerged from civil war. Among the many characteristics that are not significant, are democracy and political rights. Controlling for these three significant characteristics, we then introduce aid in the year in question. We are able to distinguish between two components of aid: technical assistance, and other aid.

There is one important potential problem with explaining turnarounds by means of aid: causality might run in the other direction, from the prospect of a turnaround to aid. Donors can be expected to seek out information so as to direct aid to those situations where it stands the best chance of being helpful. To the extent that donors get this right, aid will tend to be targeted to situations which are ripe for improvement, and hence which subsequently indeed improve. With such behaviour it is intrinsically difficult to determine the direction of causality. If we regress the probability of a turnaround on aid, it is indeed likely to appear that aid raises the chances of turnaround, but this is only because we have forced this version of causality on the regression. We might equally have regressed aid upon the probability of reform, in which case we would have found that a good prospect of reform causes aid to go up. The way to overcome this problem of causation is to 'instrument' for aid. That is, to find a component of aid that can be predicted country by country, year by year, and which is devoid of any influence of policy, institutions and governance. We adopt a set of instruments now common in the academic literature (Tavares, 2003). Essentially, the idea is that a substantial component of a country's aid receipts is determined not by its own current circumstances but by the characteristics of donors. For example, Ethiopia is likely to get relatively a lot of aid from Italy, and Cote d'Ivoire is likely to get relatively a lot of aid from France. If the Italian aid budget goes up and the French aid budget goes down, Ethiopia is likely to get an increase in its aid receipts relative to Cote d'Ivoire. Our 'instrumentation' enables us to investigate the effect on turnarounds of this component of aid that is unrelated to policy conditions in the recipient country. Thus, to continue our example, from time to time Ethiopia, or Cote d'Ivoire 'strike lucky' or unlucky with their configuration of aid donors, and we investigate whether these lucky and unlucky strikes have any effect on the chances of a sustained turnaround.

Overall, we find that the probability of a sustained turnaround starting in any year is very low, at 1.7%. Countries are therefore likely to stay as failing states for a long time. This is the basis for our estimate of the mathematical expectation of 59 years. This slightly exaggerates the duration because it considers only exits due to improvement in policies, institutions and governance. A country can also exit because of a rise in income that takes

it above the low-income threshold. While on average very poor policies, institutions and governance make such a rise in income unlikely, sometimes countries have good fortune.

Somewhat surprisingly, we find that country characteristics are not very important, notably neither the country's level of income, nor its level of democracy. Hence, the modest trend towards democratization, even if coupled with some growth in income, would not significantly shorten the expected duration. Countries with a higher proportion of their people who have secondary education, and to a lesser extent, countries with larger populations, are significantly more likely to achieve sustained reform. The apparently distinct effects of population size and the proportion of the population with secondary education may be pointing to the same thing: the need for a critical mass of educated people. The impetus for change must generally come from within the society, and the capacity to diagnose failure and chart a new course is likely to depend upon the capacities of the elite. For example, China under Mao and Tanzania under Nyerere both failed and indeed failed through somewhat similar strategies. The Chinese elite was able to rethink and adopt a radically different strategy; the Tanzanians also changed course but more belatedly, and half-heartedly.

We now turn to the effects of aid. Recall that here we are only investigating the effect of aid prior to turnaround. We distinguish between technical assistance and other forms of aid. Technical assistance prior to turnaround has no significant effect on the prospect of turnaround. This is surely a surprising result, since to the extent that there has been a conventional strategy for reforming failing states it has been to provide 'advice'. Our results suggest that on average such advice has not been well received. In contrast, other aid in aggregate significantly increases the chances of a turnaround. Quite why such aid should assist change we are unable to determine. Possibly aid projects build the capacity of the society. For whatever reason, precisely contrary to received wisdom, it is money rather than advice that seems to help create the conditions for change.

Assisting Incipient Turnarounds

We now look at interventions *during* the early stages of a reform. The objective that we analyze is again not enhanced growth or poverty reduction as such, but rather whether the turnaround is sustained as measured by the quality of policies, institutions and governance.

For this analysis we need a definition of 'incipient' reform. Our previous definition of sustained reform included the criterion of durability – the CPIA had to remain above 3 for at least five consecutive years. One possibility in distinguishing between sustained and unsustained reforms is therefore simply to determine whether a turnaround that meets our other criteria does, or does not meet this criterion of five-year persistence. However, this categorization of unsustained reforms turns out to be far too strict – only six of the 29 reforms that meet our other criteria fail to meet the five-year criterion. This is because one of our other criteria is an improvement in the CPIA of 1.4 points. Once a country has achieved such a large increase in its CPIA it seldom reverts to being a failing state. However, this is not very helpful for donors: agencies need to know whether it is a good

use of resources to back a reform program well before it has achieved such a major improvement in policies, institutions and governance. We therefore radically lower the threshold of CPIA improvement to define what constitutes an 'incipient turnaround'. At the same time, since the CPIA is a subjective judgment call, it would be unreasonable to consider *any* improvement in the CPIA, however, small, as something that should potentially trigger a rethink of aid policy. The reasonable position is surely to try to determine thresholds which trigger changes in aid policy and which are sufficiently sizable that they occur relatively infrequently. In this spirit, we investigate a range of thresholds. A low threshold of CPIA improvement will imply more false dawns: a higher proportion of 'incipient turnarounds' will revert back to failing states. Specifically, we consider threshold levels of CPIA improvement of 0.25, 0.5, 0.75, and 1.0. In Table 2 we report the frequency with which the threshold would have been triggered, and the proportion of 'incipient turnarounds' that become 'sustained turnarounds' on our previous definition.

Table 2: False Dawns under different Triggers for Aid Policy

Triggering threshold	Frequency of policy change (times per country)	Rate of false dawns {(incipient-sustained)/incipient}
0,25	1,76	0,78
0,5	1,53	0,75
0,75	1,28	0,70
1	0,97	0,61
1,4	0,62	0,39

Table 2 illustrates the fundamental donor problem in supporting signs of reform in failing states. Mostly, the donor will end up 'backing losers' in the sense that without donor intervention these incipient reforms collapse. In one sense this should be good news for donors because it implies that there efforts will not usually be superfluous. However, unless donor interventions are remarkably powerful (on which more below), most of the time they will not be sufficient to avert failure of the reform effort. Hence, most of the time donor efforts will be wasted. This sort of failure probably worries donors much more than the risk of being superfluous. Being superfluous to a successful reform is never apparent: the donor is participating in success and will doubtless claim a share in the credit. There is no information capable of contradicting such a claim, even when it is false. By contrast, an intervention which is insufficient to avert the collapse of the reform effort is a highly public failure on the part of the donor.

Even if the donor waits until the CPIA has already improved by a full point (1.0) before supporting the reform effort, six times out of ten the support risks being a public failure, and only four times out of ten risks being superfluous. In principle, it would be possible to estimate the optimal decision rule for donor intervention, balancing the costs of errors of commission against the costs of errors of omission. Here, we simply consider the consequences of adopting one particular threshold for triggering intervention, namely an improvement in the CPIA of 0.5. That is, we explore the consequences of a strategy in which the donor waits until the CPIA has improved from its low-point by 0.5 points and then intervenes with supporting aid.

Our sample is thus all those failing states that embarked upon 'incipient turnarounds', defined as having at least a 0.5 improvement in the CPIA. We then attempt to explain which of these incipient turnarounds either matured into a sustained turnaround or collapsed back into failure. The technique we used to answer this question was hazard functions. A hazard function models the period-by-period 'hazard' that a process will end. For example, it is commonly used to analyze the duration of unemployment, and has also been used to analyze the duration of civil war.

Again we perform specification tests to establish which among the many possible characteristics are statistically significant. Six characteristics appear to matter. An incipient reform is more likely to progress to a sustained turnaround the higher is income, the larger is population, and the greater the proportion of the population with primary education. It is less likely to progress if the country has recently emerged from civil war, if it is experiencing a positive terms of trade shock, and if the leader has been in office a long time. Compared with the pre-conditions for a sustained turnaround there are some striking similarities and differences. Countries with large and educated populations are doubly blessed, although the actual level of education that is significant differs. By contrast, post-conflict countries are more likely to achieve a sustained turnaround, but an incipient reform is less likely to progress. How can these two seemingly contradictory results be reconciled? A possible answer is that post-conflict situations are highly fluid. Some sort of reform is much more likely to be initiated than in other failing states, but many of these incipient reforms will fail because it is harder to sustain a continuous course of change.

Controlling for these characteristics, we investigate the effects of aid, again distinguishing between technical assistance and other aid. Both forms of aid are again 'instrumented'. This is particularly important because presumably donors tend to respond most strongly to the most promising of the incipient reforms: the scale of actual aid is thus likely to be endogenous to the prospects of its success. By 'instrumenting', we control for this effect, considering only that component of aid which is truly exogenous.

Time is continuous and the 'units' into which it is divided – months, years or whatever – are a matter of choice in research design. When sample sizes are relatively small, as in analysis of failing states, time periods have to be chosen with regard for their effects on sample size. Our core analysis pools across periods in order to give the largest sample size relative to explanatory variables. For this, a short time period is helpful since it increases the number of observations. We therefore pool on two-year observations of the reform experience. Thus, our first two-year period starts as soon as the CPIA has risen by 0.5 points. Thus, the largest sample size is for results which 'pool' all the different periods during the turnaround. If there is a type of intervention which is generally helpful during turnarounds, this pooling approach is the most likely to pick it up. However, pooling time periods precludes the possibility of investigating when during an incipient reform interventions are most effective. For this, distinct time periods must become explanatory variables and now a short time period creates the problem of requiring many such variables. We therefore aggregate up to four-year periods.

We start from the overall picture provided by pooling. Both technical assistance and other aid have significant effects on whether an incipient turnaround becomes a sustained turnaround. For both the relationship is non-linear. Technical assistance is subject to diminishing returns: an implication is that there is an optimal amount. This is around 5% of GDP. Much above this and the marginal effects of technical assistance become adverse. Actual technical assistance during incipient reforms is substantially less than this at around 3% of GDP. The non-linearity for other aid is, however, dramatically different. Small amounts of aid actually slow down the process of turnaround: only big aid starts to be helpful but it must first offset the intramarginal effect. The threshold above which the net effect of aid becomes positive, accelerating the attainment of a sustained turnaround is very high – around 30% of GDP.

A related but distinct issue is whether interventions help to reduce the chance that an incipient reform collapses completely, with the country reverting to failure. The results from the analysis of pooled periods is in most respects the mirror-image of the previous results. In particular, the effect of technical assistance on this risk of reversion to failure is precisely complementary to its effect on continuing to success: just as it accelerates progress to sustained turnaround, it significantly reduces the risk of reform collapse. Indeed, even the optimal amount of technical assistance is the same at 5%. Other aid has no significant effect on whether an incipient reform collapses. This suggests that we can be somewhat more confident in the deployment of technical assistance during the incipient stages of a turnaround than of other forms of aid.

We now take the analysis further by disaggregating the reform period into four-year sub-periods. As noted, this has the potential to show when during an incipient turnaround support is most useful. In the *first* four-year sub-period, technical assistance has large and favorable effects on the likelihood that a reform will progress to a sustained turnaround. Beyond that the effects are either insignificant or much smaller depending upon specification. These results are partly complemented by the effect of technical assistance on the risk that the reform will collapse. Technical assistance is significant in reducing the risk of relapse, but this occurs in the *second* four-year sub-period, rather than the first.

Turning to other forms of aid, during the *first* four-year sub-period, the effects of aid on the prospects of reform progress are consistently significantly adverse and this is not offset by any favorable effect on the risk of reform relapse: there is no significant effect on this risk. However, in the *second* four-year sub-period, aid significantly increases the prospects of reform progress, and in the *third* sub-period it significantly reduces the risk that the reforms will relapse. In neither case is this favorable effect offset by an effect on the other risk.

These results have implications for the sequencing and packaging of aid interventions that are taken up in Section V. An appropriately sequenced package can be effective in reducing the global incidence of failed states. This, however, is not enough to guide policy. Interventions should not just be effective, they should be *cost*-effective. For this we need to estimate the benefits to interventions and compare them against their costs.

IV. The Benefits of Policy Interventions relative to their Costs

To know that some interventions are effective and that others are not is the first, rudimentary step in guiding policy. The second step is to estimate the benefits of these interventions relative to their costs.

The benefits of intervention

The benefits of intervention are the reduced risk that a country will continue to fail. Hence, we need to know the typical cost of state failure, and the increase in the chance of a sustained turnaround. The benefits of intervention are then analogous to the value of a lottery ticket that gives us a relatively small chance of a large gain.

The core of the cost of a failing state is what policy and institutional failure does to the economy of the country itself. Our approach estimates a lower-bound to these costs: it omits a lot of the consequences of state failure that are legitimate objects of concern. For example, we omit the security costs implied by an increased risk of civil war, and the human costs implied by avoidably high infant mortality. To give some sense of how much we are thereby omitting, related work has made a lower-bound estimate of the cost of the typical civil war in a low-income country at around $50bn (Collier and Hoeffler, 2004). Here, we confine our analysis to the cost of a failing state *that is and remains at peace.*

We estimate the cost of the policies and institutions that define a failing state by introducing a 'failing state' dummy variable into a standard growth regression. To reduce the problems associated with endogeneity, we use the standard 'system GMM' technique. We find that even at peace, a failing state grows significantly more slowly that a normal state: growth is reduced by 2.9 percentage points. The costs of lost growth mount with persistence. We assume that once a turnaround has been achieved, the country gradually catches up to where it would have been without a phase of state failure. The issue of catch-up after turnaround has not yet been researched, but the related issue of recovery after civil war has been researched and displays this pattern. The cost of failure is thus the discounted present value of the income forgone during the failure and recovery phases. If the country is lucky to turn around after only a few years of failure this cost will be small. However, recall that the expected duration of being a failed state is 59 years. We estimate the expected cost of state failure by taking each possible future scenario of reform, and weighting it by its probability of occurrence.

The other layer of costs that we include is spillover effects on the growth rates of neighbours. Previous work has established that civil wars reduce the growth rates of the whole sub-region in which they occur (Murdoch and Sandler, 2002) and there is good reason to expect that there is an analogous effect from the economic failure of the state. We indeed find such a spillover effect, at least for direct neighbours.

The discounted present value of these costs of the typical state failure is around $100bn. This is the lower-bound estimate of what a sustained turnaround is worth. It is a mesmerizingly large number, but then, the phenomenon we are considering is indeed dramatic: a world without failing states would be a transformed world. The US-led military intervention in Iraq provides us with a rare opportunity to infer what a key international actor regarded as the benefits of one sustained turnaround. The purpose of the intervention was clearly stated as being regime change. The regime in Iraq was a classic example of a failing but politically secure state, so that the costs of failure, both to Iraqis and to their neighbours could reasonably be seen as likely to be highly persistent. The military intervention in Iraq has cost of the order of $100bn. Costs of at least this level could readily be forecast, since the actual military deployment has been towards the lower end of envisaged scenarios. Thus, as a matter of revealed preference, the decision to intervene in Iraq implies expected benefits of intervention in excess of these costs. The expected benefits of intervention to promote turnaround in general depend upon the value of the turnaround and the probability that it will take place, so we know that this expectation must have been valued at more than $100bn. We can tease out the minimum value of the turnaround itself by assuming that the US Administration regarded the prospects of a sustained turnaround as a certainty. In this case, the turnaround would have been valued at more than $100bn. If the Administration had applied any discount for the prospects that a new Iraqi regime would revert to state failure, the valuation placed on a successful turnaround would necessarily be higher than this. Clearly, the US-led intervention in Iraq is unusual in many respects. Usually, interventions to achieve turnarounds in failing states are economic rather than military. Their expected prospects of success are radically lower but not specified, and so little can be inferred from these economic interventions about how international actors value a turnaround. Despite its other complexities, Iraq does offer a unique opportunity to infer a clear lower-bound estimate of the benefits of turnaround to the single most important international actor. The most unusual thing about Iraq was that, with extraordinary folly, the regime opened itself to the risk of external military intervention. Amin made the same mistake in Uganda, exposing his regime to successful invasion from Tanzania. More usually, failing states avoid such an evident mistake so that the scope for external actors to induce state turnaround is more limited. Nevertheless, the objective is basically the same, and hence, so are the expected payoffs. Our lower-bound estimate of the benefits from a sustained turnaround of the typical failing state of around $100bn is thus not inconsistent with the revealed valuation of the key international actor.

Having got an estimate of the cost of state failure, we then estimate the payoff to each policy intervention from its estimated impact on the probability of a turnaround multiplied by this cost.

A cost-benefit analysis of effective interventions

We now combine our analysis of the effects of interventions on the probability of achieving a sustained turnaround with our estimate of the benefits of such a turnaround. We consider four interventions. We take two interventions that build the preconditions for turnaround: financial aid, and an increase in secondary education. We then take two

interventions that increase the changes that an incipient turnaround will progress: technical assistance in the first sub-period, and financial aid in the second sub-period.

We start by simulating a small increase in aid other than technical assistance, in a typical failing state that is not showing signs of a turnaround. We then maintain this small increase for five years before reverting to normal levels of aid. On average, failing states have received aid other than technical assistance equal to 6.8 percent of their GDP. We therefore simulate the effect of increasing this by one percentage point to 7.8%. One advantage of considering a small increase of this kind is that it is well within the observed range of the data, thereby increasing the reliability of our results. The effect on the chance of reform is to raise it from 1.7% per year to 2.4% per year. In proportionate terms this is a large effect: we are increasing the chance of escape by nearly a half. To estimate the benefit relative to the cost we return to our calculation of the costs of starting out as a failing state, but now raise the probability of sustained turnaround to 2.4% for the first five years of our long-term horizon. The cost of starting out as a failing state now falls by $3.3bn. This reduction in costs is the present value of the benefits from the investment in temporarily enhanced aid. The costs of the enhanced aid are simply one percent of GDP – $55.6 millions – each year for five years, discounted back to the present at the 5% discount rate. The resulting present value of the cost is $240.8 millions. The investment of an incremental aid program to a typical failing state of $240.8 millions thus results in a payoff of around $3.3bn. Since, as noted, this considers only one component of the payoff to aid, namely the enhanced prospect of sustained reform, it suggests that expanded aid would be well worth while.

If, starting from present levels, a marginal increase in aid is highly beneficial, this raises the further question of how far could an expansion usefully go? At what point would the marginal benefits in terms of enhanced prospects of turnaround broadly equal their marginal cost. We again abstract from any effects of aid on income or social outcomes prior to turnaround: the only effect of aid we allow for is the effect on the chance of turnaround. This is conservative, but as such it provides a lower-bound, and acknowledges the difficulty of using aid to promote growth in failing state conditions. However, we should further qualify the subsequent analysis by noting that any really large increase in aid will take us outside the range of the observed data. Our logit analysis ensures that we will eventually encounter diminishing returns to aid, simply because the probability of reform must asymptote, implying diminishing returns to everything; but we are not using an explicitly observed diminishing return to aid[1]. We use the same long-term horizon as in our previous calculations. With these caveats, the 'optimal' amount of aid, at which the marginal benefits in terms of enhanced prospects of reform equal the marginal costs, occurs when aid is around 22% of GDP. The typical failing state would thus get an increase in aid of around $800m per year.

The other potentially effective intervention in the preconditions for turnaround is to provide aid specifically for secondary education. Consider an increase of one percentage point in the proportion of the population with secondary education, from the mean for

[1] A quadratic relationship between aid and the probability of turnaround has been explicitly tested in the regressions but it proved to be insignificant (as for technical assistance).

failing states of 2.2% to 3.2%. This would also raise the probability of reform from 1.7% per year to 2.4% per year. If this enhanced educational provision was maintained permanently, this would reduce the expected future cost of currently being a failing state by around $10bn. We should note that we have not taken into account any of the normal returns to education in terms of raising incomes. The reform effect is purely additional to conventional calculations. Evidently, on this estimate, donor investment in what might be thought of as the social preconditions for turnaround has a high expected pay-off.

We then turn to interventions once reform has started. We first consider a technical assistance program, set at the maximum effective level of 5% of GDP, which is run for the first four years after the CPIA has improved by 0.5, the threshold we used in Section 4. The total cost of a technical assistance package of this level and duration is around $1bn. The pay-off, however, is dramatic. Taking a representative failing state, the probability that the package will achieve its objective is around 15%, implying a discounted pay-off of nearly $15bn. Big technical assistance packages delivered early in reform attempts appear to be very cost-effective interventions.

Finally, we consider a package of financial aid of the same size, (5% of GDP), delivered during the second four-year period of the incipient turnaround. The cost is evidently again around $1bn. The payoff is less substantial, but still in excess of its cost, at around $2.3bn.

V. Implications for Strategies of External Intervention

Although ordinary citizens of developed countries imagine that aid programs are designed to assist failing states to turn around this is far from being the current reality. A good example is the new Millennium Challenge Account of the US government, the design of which reflects recent thinking on aid effectiveness. The criteria of eligibility for the MCA completely exclude failing states: the money is targeted on states that are already effective. There are good reasons for this design: the contribution of aid to poverty reduction in these environments is radically less uncertain than in other environments. The design of the MCA makes a lot of sense, but it leaves a hole: it simply does not address the problem of failing states.

In practice, the strategies of external intervention that have addressed failing states have been military and political. Iraq and Afghanistan are dramatic examples of military intervention. The political strategy is manifested in the recent wave of democratization in countries such as Nigeria and Ukraine. Typically, the military strategy rapidly evolves into the democratic: Afghanistan and Iraq hold elections and the external military intervention is scaled down. Unfortunately, on our analysis, democracy is not significant as a precondition for turnaround, and nor is it significant in raising the chances that an incipient reform will progress to a sustained turnaround. This is not to denigrate democracy: the case for freedom is basic, rather than being dependent upon its economic consequences. However, it does suggest that states that are failing in economic terms need something beyond democratization.

Our results suggest that aid interventions, properly designed, are much more effective than democratization. Compared with regime change in Iraq, aid is radically more cost-effective than military intervention. However, the emphasis has to be on design. A striking feature of our results is that even with the very limited distinction between technical assistance and other forms of aid, the issue of what form of aid is appropriate at what time is important. The appropriate model for aid to failing states is a cross between a venture capital fund and an emergency relief fund: it needs to accept high risks and it needs to be capable of rapid response. These features are so diametrically opposed to the normal organization of development assistance that they would require distinct, ring-fenced financing, though not another aid institution.

Consider, first, technical assistance. Such aid has no discernable effect until after a turnaround has clearly begun. Before that it appears to be a waste: it is not a precondition for reform. However, once there are clear signs that the government has itself embarked upon a turnaround, rapid technical assistance is a highly effective form of aid, dramatically increasing the chances that the incipient reform will progress to a substantial and sustained improvement, and reducing the chances of relapse. But technical assistance can be excessive and it can continue for too long. Around 5% of GDP appears to be the right amount of technical assistance in early reform environments, although this will obviously have to be nuanced by the particular circumstances. If the incipient reform does not progress towards a sustained turnaround, the case for continued technical assistance is weakened, although it still plays some role in averting complete relapse.

This pattern has several important implications for the provision of technical assistance. An obvious interpretation of our results is that technical assistance is only really useful when it is provided to governments that *want* and *need* to use it. An indication of intention is that the government has already made significant reforms – improving the CPIA by at least 0.5. An indication of need is that there is continuing progress. Analogous to the research on the circumstances in which education is productive, knowledge is only really useful when a system is trying to adapt. Without continuing change there is no need for technical assistance. *Technical assistance can help a government get change right, it cannot make change happen*.

If technical assistance is only productive when governments both want it and need it, in such circumstances the government itself should be the best judge of its needs. A practical way of implementing the implied approach would be for newly reforming governments to be given a 'Technical Assistance Account' that they can draw down at their own initiative for international expertise. A second implication is that the supply of technical assistance should be organized by donor agencies in such a way as to be highly responsive to changes in development circumstances. In effect, *it needs to be organized analogous to emergency relief* rather than being handled as if it were simply an ordinary development project. The allocation of TA between countries should be 'volatile': if it does not change much from year to year then it is definitely being misallocated. A final implication is that the evaluation of technical assistance needs to take into account that it is intrinsically a high risk investment. Even when properly allocated in response to

opportunities, much of the time it will fail. This is more than compensated by the times when it pays off.

At present, the key donor agencies simply lack the instruments for providing technical assistance in this fashion. The agency which is specialized in providing technical assistance is UNDP, but the supply is essentially determined by an allocation which reflects each country's right to a share in the overall UNDP budget. UNDP simply lacks the authorizing environment to allocate technical assistance purposively. The only major agency with the authorizing environment to supply technical assistance purposively is the World Bank. At present the Bank simply lacks a financing instrument that would enable technical assistance to be supplied with the speed required. This is surely a prime candidate for financing through an earmarked grant facility. Because of the high risk of such assistance, evaluation should be of the entire facility rather than of each intervention separately. The typical technical assistance help during an incipient turnaround might well fail, and yet the overall strategy of supplying such assistance be highly cost-effective. The practice of evaluating each technical assistance project in isolation discourages precisely the sort of high-risk intervention that is required to achieve sustained turnarounds.

Recall that as of 2002 there were around 13 failing states. Suppose, not unrealistically, that over the next decade each of them has some incipient reform effort, a brief window of opportunity that typically without external assistance will abort. Now suppose that technical assistance was so organized that it delivered a package of the appropriate scale – 5% of GDP – at the appropriate time. The cumulative cost of such an intervention would be around $13bn. The mathematical expectation is that this would buy two sustained turnarounds that otherwise would not have happened. For $13bn the world gets two Iraqs! Such technical assistance has to wait its opportunity, but then so does external military intervention. Over the coming decade, the world may well not provide more than two opportunities for successful military intervention in failing states. Hence, big technical assistance is not only radically cheaper than military intervention, it may well be the more important strategy in terms of overall contribution to turning failing states around.

Other effective forms of aid for failing states do not need to have such capability for rapid reaction. One reasonable use appears to be to improve the pre-conditions for a turnaround. Among these uses, secondary education appears to be really productive. The typical failing state not only has a small share of its population with above-primary education, it has a small population. Hence, the pool of educated people may well be too small either to generate or to manage the radical adaptations that are necessary in the transformation from failure. Beyond its use to build the preconditions for reform, aid (other than technical assistance) also has a role once reform is underway. On our analysis there is no urgency: early finance discourages reform, analogous to a favorable terms of trade shock. Aid can, however, be useful during the middle of the first decade of turnaround.

Although appropriately chosen aid for turnarounds in failed states has a high expected pay-off, this pay-off is very risky: mostly such aid will fail. Were donors completely risk neutral, or able to pool their risks across many investments like a venture capital fund, then the optimal amount of 'precondition-enhancing-aid' would be quite high. On our analysis the point at which marginal costs equal marginal benefits is when aid is 22% of GDP. Donors are not risk neutral, and they cannot pool their risks except to a limited extent. Hence, 22% is surely better seen as an upper bound rather than as a target. Nevertheless, it is strikingly high. Donors can justify having some of their portfolio at this high-risk end of the spectrum, even though it cannot be a major component because of the difficulty of showing proper accountability for expenditures with this level of risk. The right model for evaluation of aid interventions to turn around failing states is that of venture capital. Evaluation project-by-project chills risk-taking. The increased emphasis upon 'results orientation' in aid, inadvertently encourages staff in aid agencies to take less risk. Because both the risk profile and the appropriate evaluation procedures for aid to failing states are so different from normal aid, the two should be considered as separate pools of aid finance. Unless aid for failing states is ring-fenced, risk-averse aid bureaucracies will systematically under-finance it.

We summarize our results by the following implied sequencing of aid instruments:

Prior to any improvement:	avoid technical assistance, but use other aid, especially for post-primary education, to build the conditions for reform.
Early support for reform:	Once a government has raised the CPIA by 0.5 donors should very rapidly provide substantial technical assistance, of the order of 5% of GDP during the first four years. Other forms of aid at this early stage are probably counterproductive.
The peak stage of reform:	During the middle of the first decade of reform (around the 5^{th}-8^{th} years), technical assistance should be supplemented and possibly to an extent replaced by financial aid.
Sustained turnaround:	If sustained turnaround is attained, then aid can be determined on the more conventional criterion of 'poverty efficiency'.
Reform collapse:	If the incipient reform completely collapses, donor involvement should revert to the first stage, as if there had been no improvement.

Finally, we should note two important caveats. First, we are not able to differentiate technical assistance according to its destination. It is possible that technical assistance to government might have quite different effects from technical assistance to non-

government organisations such as the media. In particular, we are not able to test the proposition that technical assistance for such non-governmental organisations is a useful precondition for turnarounds. Second, we cannot differentiate between different types of non-technical assistance aid, notably between project assistance and budget support, nor between different sectors. Presumably just as with the broad distinction between technical assistance and other aid, such choices matter and would therefore change and deepen the results.

VI. Conclusion

Our analysis has generated both disturbing and encouraging results.

The most disturbing results concern the longevity and cost of failing states. Typically, once a country starts to fail it continues failing for a very long time: the expected duration being 59 years. In conjunction with this problem of longevity is the cost: typically around $100bn. Much of this cost accrues to neighbors rather than to the country itself. This is the case even when the failing state is entirely peaceful: very weak policies, institutions and governance have adverse neighborhood spillovers. However, we should note that such failing states are unlikely to remain at peace: they live dangerously. The typical civil war in a low-income country costs around $50bn over-and-above the costs of a failing state at peace.

The encouraging aspect of our analysis is that even with the limited traditional instruments of a development agency – various types of aid – something can be done to enhance the chances that countries will turn themselves around. Properly designed, aid is more effective than the political strategy of democratization, and more cost-effective than some military interventions. To reap these effects, the organization of development assistance would need a redesign so as to permit some aid to be directed to high risk environments, and to be capable of rapid response to incipient turnarounds. This does not need new agencies, but it probably needs new, ring-fenced money within the existing agencies.

Finally, we should make it clear that our paper has not provided a comprehensive analysis of instruments for inducing turnarounds in failing states. For example, we have not considered international initiatives that provide public benchmarks of adequate governance, thereby generating domestic pressure within failing states for reform. Such initiatives are extremely cheap and there is some evidence that they are effective. They have been neglected, perhaps because no agency regards itself as having such actions within its mandate. One recent example is the Extractive Industries Transparency Initiative. Its take-up among failing states, and those in danger of failing, has been encouraging. Given the magnitude of the problem of failing states, and the relative weakness of traditional instruments, the emergence of public benchmarks as standard tools of development is surely a priority.

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