Policy Turnarounds in Failing States

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1. Introduction

A 'failing state' can be defined in various ways. In political science it has come to mean a state which is not able to maintain internal security. We give the term an economic meaning: a failing state is a low-income country in which economic policies, institutions and governance are so poor that growth is highly unlikely. The state is failing its citizens because even if there is peace they are stuck in poverty. The failure may well, however, be wider. Empirically the combination of poverty and stagnation substantially increase proneness to civil war (Miguel et al, 2004; Collier and Hoeffler, 2004). Through various routes the state may become a hazard to its neighbours and conceivably to the world.

The international community recognizes that such states are a problem, but is unclear what to do about them. One strand of the aid effectiveness literature suggests that the absorptive capacity for aid might be limited in these environments (Burnside and Dollar, 2000; Collier and Dollar, 2002). The idea of more direct international replacement of the state, through trusteeships, is under active discussion. The primary purpose of the present paper is to investigate empirically the conditions under which failing states decisively turn round, achieving and sustaining radically improved policies, institutions and governance. We investigate both the preconditions for such turnarounds, and the conditions under which incipient turnarounds are most likely to continue to fully fledged transformations. We focus particularly upon aid, both money and technical assistance. Is aid of either form effective in promoting these transformations? Conditional upon aid being effective, a consequential is whether it is *cost*-effective. The secondary purpose of our paper is to estimate the benefits of turnarounds and compare them to the costs of those instruments that are effective in promoting them.

In Section 2 we define more precisely the concept of a sustained turnaround. In Section 3 we turn to the preconditions for such a turnaround. In Section 4 we investigate incipient turnarounds, and what determines whether they maintain momentum or, at the other extreme, relapse back to the initial condition. In Section 5 we develop a cost-benefit analysis of interventions. Section 6 concludes with a discussion of the implications for policy.

2. Defining Sustained Turnaround

In general terms the phenomenon we wish to analyze is clear enough. However, we need both a measure of economic policies, institutions and governance, and precise definitions of what is meant by a failing state and by a sustained turnaround. As a measure of policy, institutions and governance we will use the Country Policy and Institutional Assessment (CPIA) of the World Bank. This is an internal World Bank standardized subjective rating system done annually for all countries that are borrowers from the World Bank.[1] Twenty

[1] Because it has been used to allocate the Bank's concessional lending (IDA), the details of the ratings have been kept confidential from the countries concerned, although it has been published in quintiles for some years. We understand that the Bank's Board has decided to release the detailed information in 2006.

different aspects of policy, institutions and governance are rated using criteria that grade each of them on an ascending scale of 1-6 (see Appendix 4 for the definition of the CPIA). These are averaged by the Bank to form the rating which we use. The CPIA has the considerable advantage that it is the only such rating continuously available since 1977 which is explicitly designed to be comparable across countries. The major disadvantage is that it is a subjective index that incorporates judgments of World Bank economists. This has the potential for two types of error. Firs, some of the judgments reflect economic analysis which is professionally contentious. Fortunately for our purposes, the contentious judgments arise overwhelmingly in the higher reaches of the CPIA. To illustrate outside the range of rated countries, it is evident that there are significant differences between France, the USA, the UK and Japan, but there would be no professional consensus as to which of these countries had the 'better' policies, institutions and governance. Our focus on failing states takes us to the bottom end of the CPIA range. Here there is little controversy. There is an overwhelming professional consensus that countries such as Angola, Burundi and the Central African Republic have had bad policies, institutions and governance. A second potential for error is that despite the overview procedures, some changes in the CPIA may reflect nothing more than changes in staffing. We guard against this by devising criteria which are not dependent upon small changes in the score. Thus, for our purposes the CPIA is a reasonable index for the phenomenon we wish to classify. As with all continua, there will, however, be issues at the margin of what is, and what is not, a failing state. To specify a turnaround we need to answer the questions 'from what?' and 'to what?' It is to these issues that we now turn.

For our purposes, to be a failing state the country must have a low income. We use as an income cut-off that the country should have been classified as a LIC for at least one year by the World Bank (World Development Reports, 1977-2002). Evidently, not all low-income countries are failing states. We adopt as a cut-off for weakness in policy, institutions and governance, a level of the CPIA no better than 2.5, which is a very low rating.

However, many countries occasionally meet these criteria and yet are not appropriate for the category of turnaround. This is because their period of difficulty reflects only a temporary crisis into which they plunge and from which they rapidly bounce back. A failing state is not simply a country in a brief spell of crisis. In particular, a country facing a crash in its policies, institutions and governance may be able to recover rapidly because expectations are not set, and because those political groups that benefit from the new situation have not yet mustered the power to prevent change. The concept of a failing state includes some notion that the problem of weak policies, institutions and governance is not just of the moment, but has some persistence. We therefore add the requirement that the CPIA should have been under the threshold of 2.5 for at least four consecutive years. Countries that meet the other criteria but not this one we refer to as 'recoveries' rather than failing states.

Thus defined, we have the pool of all country episodes from which a turnaround from being a failing state might potentially happen. From within this pool we now define what we mean by a turnaround.

Evidently, to be a turnaround, policies, institutions and governance must improve above above the threshold for a failing state. However, this alone is clearly insufficient. A country whose CPIA improved from four consecutive years of being 2.49 to reach 2.51 would meet the letter of such a condition but would not have had a significant improvement. We introduce the additional requirement that the CPIA should not only rise we do not mean that the improvement should be abrupt, achieved within a single year. If any year subsequent to four years of consecutive CPIA scores low enough to qualify for the status of potential turnaround is at least 1.4 above the nadir, then that will satisfy our criterion.

However, momentary improvements, however large, are evidently not sustained turnarounds. We therefore need criteria for sustainability. It may seem obvious that the criterion for sustainability is simply that the reform should have persisted to the present. However, this would be a poor criterion. To quote a famous African saying, 'no condition is permanent'. Taking a specific example, Indonesia had a dramatic turnaround from the late 1960s, at which time it was a classic failing state, to become a star performer of the 1980s. Yet, during the late 1990s it suffered a collapse along with much of East Asia. It seems to us unreasonable to see this crisis of the late 1990s as being the result of failures in the design of the turnaround. It is surely more reasonable to think of the turnaround as having been successful in producing sustained improvement, with the crisis of the late 1990s being attributable to some intervening event or process subsequent to the turnaround. By 'unsustained', we wish to capture only those turnarounds where the subsequent relapse was sufficiently close to the turnaround that the reasons for it could sensibly be attributed in large part to weaknesses in the turnaround itself. We therefore deem a turnaround to have been sustained if the CPIA remains above 3.0 for at least five years after the turnaround is achieved. A further reason for setting such a limit to the requirement of sustainability is that otherwise relatively recent turnarounds look much more successful than earlier turnarounds which have simply had more time to be reversed.

3. Preconditions for Sustained Turnaround

A substantial case study and econometric literature on donor conditionality has concluded that it was largely ineffective in inducing policy reform. A good example of the case study literature is Devarajan (ed), 200?, which examined ten reform episodes to a common framework. The key econometric contribution is probably that of Dollar and Svensson (2000) who found that there was no significant causal relationship from aid programs onto policy and institutional change. The incentive effect of aid on reform is indeed highly doubtful. In economic terms any 'substitution effect', making reform more attractive for a government, is offset by an 'income effect' making it less necessary. Perhaps more important, in psychological terms, the infringement of freedom associated

with conditions induces 'reactance', whereby governments attempt to reestablish their freedom by doing the opposite of what the conditions require.

However, the incentive effect of aid on reform is by no means the only way in which aid can influence policy and institutions. Aid can build capacity, directly or indirectly. It can expose governments to new ideas. It can free up governments from crisis management, enabling them to think about longer term strategies. Such effects might be particularly important at the very bottom of the economic spectrum. Chauvet and Guillaumont (2004) estimate economic policy regressions and indeed find that when policies are initially very poor, aid has a positive impact on them. While the general result of Dollar and Svensson is surely robust, it is therefore worth investigating whether aid promotes policy and institutional reform within the range of failing states.

We now have precise criteria both for a country in a position potentially to have a turnaround, and for what constitutes a sustained turnaround. The issue we investigate in this Section is what preconditions significantly increase the prospects of such a turnaround. For this we will use logit regressions to estimate the probability of a turnaround, year by year, among all potential turnaround countries. First, however, we need to surmount the problem of the endogeneity of aid.

Aid is allocated purposively. Donors 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. We overcome this problem by instrumenting for aid. We adopt a set of instruments pioneered by Taveres (200?) and now common in the literature. 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. So we use as instruments the total aid budget of the main bilateral donors, as well as some measures of the geographical and cultural distance between these donors and the recipients : the inverse of the distance between capitals, dummy variables equal to one when recipient and donor countries share a common language, dummy variables equal to one if 30% or more of the population belong to a religious group in the donor and in the recipient country. The details of our instrumentation regressions are given in Appendix 5.

Our logit regression investigates the chances of a sustained turnaround from being a failing state. Our observations are annual: each year a failing state has some probability of starting an improvement in policies, institutions and governance that culminates in a sustained transformation. Once it has started on a sustained turnaround it drops from our sample since it cannot have a further turnaround. We control for three characteristics of the country: the proportion of its population with secondary education, its population and the fact that it might be in a post-conflict situation. We then introduce aid, instrumented

as discussed above, in the year in question. We are able to distinguish between two components of aid: technical assistance, and other aid. Because technical assistance and other aid are instrumented – *i.e.* their predicted values (from instrumentation regressions) are used instead of their real values – we report *t*-Student calculated from bootstrapped standard errors. The detailed results are reported in table 1.

Table 1 – *Determinants of the start of sustained turnarounds, annual data, 1973-1999.*

Start of sustained turnarounds	Logit	Predicted probability for different values of *X*									
		X at mean	*Xβ* at mean	*X*	*Xβ*	*X*	*Xβ*	*X*	*Xβ*	*X*	*Xβ*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Post-conflict, years 1 to 4	1.156 (1.64)* [1.67]*	0.034	0.039	**0.000**	**0.000**	**1.000**	**1.156**	0.034	0.039	0.034	0.039
Secondary education	0.339 (3.03)*** [2.57]***	2.186	0.741	2.186	0.741	2.186	0.741	**3.186**	**1.079**	2.186	0.741
Ln population	0.865 (2.09)** [1.88]*	8.645	7.478	8.645	7.478	8.645	7.478	8.645	7.478	8.645	7.478
(ODA-TA)/GNI, predicted	0.342 (1.93)* [1.76]*	6.836	2.335	6.836	2.335	6.836	2.335	6.836	2.335	**7.836**	**2.676**
TA/GNI, predicted	-0.163 (-0.33) [-0.29]	2.385	-0.389	2.385	-0.389	2.385	-0.389	2.385	-0.389	2.385	-0.389
Constant	-14.26 (-3.20)*** [-2.94]***	1.000	-14.26	1.000	-14.26	1.000	-14.26	1.000	-14.26	1.000	-14.26
Observations	412										
Countries	24										
Wald test (*p*-value)	0.003										
Pseudo-R^2	0.172										
Log-likelihood	-53.36										
Number of instruments	24										
Predicted probabilities			0.0170		0.0164		0.0503		0.0237		0.0238

Logit estimations. Dependent variable is the start of a sustained turnaround. *t*-Student robust to heteroskedasticity in (…). *t*-Student calculated from bootstrapped standard errors in […] (250 replications). *** : significant at 1% ; ** : significant at 5% ; * significant at 10%.

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 very long time. Indeed, given the probability, the mathematical expectation of the duration is 59 years. Note that this may slightly exaggerate the duration because it considers only exits due to improvement in policies, institutions and governance. A country can also exit being a failing state because of a rise in income that takes it above the low-income threshold. While on average, very poor policies, institutions and governance makes such a rise in income unlikely, sometimes countries have good fortune. For example, Equatorial Guinea is not, on our criteria, currently a failing state because its income level is too high due to an oil discovery. While our estimate of persistence abstracts from such occurrences, growth in the presence of failing state-type policies, institutions and governance is unlikely to be significantly poverty-reducing, as indeed is indicated by Equatorial Guinea.

We find that the probability of achieving a sustained turnaround is dramatically increased in post-conflict periods. A country which is not in a post-conflict situation has 1.64% chance of starting a sustained turnaround, while in post-conflict situation it has 5.03% chance. This result is consistent with the analysis by Collier and Hoeffler (2004) of post-conflict situations. 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.

We now turn to the effects of aid. Recall that here we are only investigating the effect of aid prior to turnaround. We investigate the effect of aid post-turnaround in the next section. We are able to distinguish between aid provided as technical assistance and other forms of aid. Our source for these data is the reporting system of the Development Assistance Committee (DAC) of the OECD. This reporting system leaves much to be desired. However, while it most certainly contains errors and inconsistencies, it may nevertheless have some informational content. We find that technical assistance aid prior to turnaround has no significant effect on the prospect of turnaround. This is surely a surprising result, since to the extent that there was a conventional donor strategy for failing states it was to provide ‘advice’. Our results suggest that on average such advice has not been well received. The effect of aid other than technical assistance is quite different. We find that this component of aid has significantly favourable effects on the chances of a sustained turnaround. This result is robust across our variants in specification.

Robustness checks : alternative methodologies and specifications

We now proceed to robustness checks of our logit estimations of the preconditions for sustained turnarounds. We investigate alternative estimation methodologies (table 2), and alternative specifications (table 3).

The first column of table 2 presents the same regression as that of table 1, but estimated *via* Probit. All the coefficients are strongly decreased (divided by two), but their sign and

significance remain unchanged, as is the probability estimated at the mean values of the variables ($\hat{p} = 0.019$).

Our probit and logit estimations of the start of sustained turnarounds, with aid and technical assistance instrumented, consist in two-stage estimations : first we estimate instrumentation regressions, and then we replace the endogenous explanatory variables by their predicted values and bootstrap the standard errors. Columns (2) to (6) of table 2 explore two alternative instrumental variables methodologies for probit and logit estimations (see Alvarez and Glasgow (1999) and Keshk (2003) for a review of these methodologies). Both are also two-stage methodologies. First, we implement the two-stage conditional maximum-likelihood (2SCML) proposed by Rivers and Vuong (1988). Instead of using the predicted values of TA and other aid as we previously did, their real values are introduced in the probit and logit regressions, along with the residuals from the first stage instrumentation regressions. Rivers and Vuong show that 2SCML produces consistent and asymptotically efficient estimates and that it provides a practical means of testing the hypothesis of exogeneity (Alvarez and Glasgow 1999 : 151).[2] 2SCML are implemented in columns (2) and (3) of table 2, and lead to very similar results as those of our logit and probit, with the exception of population which is no longer significant.

[2] If the residuals of the instrumentation regressions are significant in the probit estimations, then the corresponding variable is endogenous. The residual of the instrumentation regression for technical assistance is not significant in columns (2) and (3), suggesting that this variable is exogenous, whereas the residual of the instrumentation regression of aid excluding technical assistance is significant suggesting that this variable is indeed endogenous. Rivers and Vuong also provide a likelihood-ratio test for joint exogeneity : $\mathrm{LR} = -2(\ln \hat{L}_R - \ln \hat{L}_U)$, where $\hat{L}_R$ is the log-likelihood estimated without the residuals and $\hat{L}_U$ is the log-likelihood estimated with the residuals. This test has a chi-square distribution with degrees of freedom equal to the number of endogenous variables. Since LR=2.133 (in reg. 2) and LR=1.344 (in reg. 3) are both inferior to χ^2_2 , this test suggests that we cannot reject H_0 of joint exogeneity.

Table 2 – *Robustness checks : alternative estimation methods, annual data, 1973-1999.*

Start of sustained turnarounds	Probit	Rivers & Vuong Logit	Rivers & Vuong Probit	Probit {one instrument dropped}	Keshk 2SLS Probit	Keshk 2SLS Probit	Random effects Logit	King & Zeng Rare events
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-conflict. years 1 to 4	0.554	1.368*	0.655	0.557	0.562	0.601	1.156	1.277
	(1.36) [1.45]	(1.83) [1.75]	(1.58) [1.58]	(1.38) [1.41]	(1.38) {0.89}	(1.40) {0.96}	(1.27)	(1.84)
Secondary education	0.162***	0.378**	0.177*	0.155**	0.160*	0.155	0.339***	0.334***
	(3.11) [2.59]	(2.98) [2.03]	(3.16) [1.93]	(3.00) [1.99]	(2.78) {1.80}	(3.19) {1.58}	(2.45)	(3.03)
Ln population	0.371*	0.993	0.400	0.350*	0.365***	0.362*	0.865***	0.802**
	(2.14) [1.69]	(1.81) [1.14]	(2.07) [0.89]	(2.11) [1.65]	(2.65) {2.20}	(2.21) {1.94}	(2.72)	(1.97)
(ODA-TA)/GNI. Predicted	0.143*			0.134	0.122*		0.342***	0.318*
	(2.00) [1.74]			(1.86) [1.25]	(2.59) {1.95}		(2.33)	(1.82)
TA/GNI. predicted	-0.052			-0.045		0.223	-0.163	-0.137
	(-0.26) [-0.21]			(-0.23) [-0.17]		(1.68) {1.12}	(-0.41)	(-0.28)
(ODA-TA)/GNI		0.294*	0.117					
		(2.12) [1.75]	(1.80) [1.47]					
Res. instrumentation reg. (ODA-TA)/GNI		-0.238	-0.085					
		(-1.50) [-1.24]	(-1.19) [-0.95]					
TA/GNI		0.055	0.017					
		(0.14) [0.09]	(0.10) [0.05]					
Res. instrumentation reg. TA/GNI		0.417	0.195					
		(1.02) [0.73]	(1.01) [0.68]					
Constant	-6.52***	-15.98	-6.91	-6.25*	-6.41***	-5.99**	-14.26***	-13.37***
	(-3.52) [-2.82]	(-2.60) [-1.56]	(-3.27) [-1.29]	(-3.61) [-2.73]	(-4.31) {-3.35}	(-3.44) {-2.72}	(-3.87)	(-3.04)
Observations (countries)	412	412	412	412	412	412	412	412
Countries	24	24	24	24	24	24	24	24
Wald test (*p*-value)	0.002	0.002	0.003	0.001	0.001	0.004	0.004	
Pseudo-R^2	0.166	0.244	0.233	0.157	0.156	0.121		
Log-Likelihood	-53.72	-48.72	-49.39	-54.32	-54.35	-56.61	-53.36	
Instruments	24	24	24	23	23	23	24	24
Rho = 0 (*p*-value)							1.000	
Pred[d] proba. at mean	0.019						0.017	0.021

Dependent variable is the start of a sustained turnaround. *t*-Student robust to heteroskedasticity in ( ). *t*-student calculated from bootstrapped standard errors in [ ] (250 replications). *t*-student calculated from corrected standard errors in { } (Keshk 2003). *** : significant at 1% ; ** : significant at 5% ; * significant at 10%.

Second, we explore the two-stage least squares probit (2SLSP) methodology (Amemiya, 1978 ; Maddala, 1983). 2SLSP procedure is identical to our logit and probit with the predicted values of reduced forms equations (*i.e.* instrumentation regressions) substituted for endogenous right-hand side variables. Then, the estimates of the second stage are efficient, but the standard errors are likely to be biased because they are based on the predicted values of the explanatory endogenous variables instead of their real values. When the second stage equation is a probit estimation, there is no simple correction of the standard errors. The asymptotic covariance matrix of the probit estimates derived by Amemiya (1978) is, as noted by Alvarez and Glasgow (1999 : 150), "exceptionally complex and computationally difficult". Fortunately, Keshk (2003) has recently developed a Stata program that implements 2SLSP and corrects the standard errors. This program can only be run on a simultaneous model with two equations (we have three, since we instrument for both technical assistance and other aid). So, using this method, we estimate 2SLSP for the start of a turnaround with technical assistance and other aid instrumented separately (regressions 5 and 6). To run this program we also had to drop one instrument (income *per capita* squared). In order to compare regressions (5) and (6) with our previous results, we therefore re-estimate regression (1) of table 6 on the reduced set of instruments (in column 4). 2SLSP estimations of the start of a sustained turnaround confirm our previous results: aid is significantly positive while technical is not significant.

Our last robustness checks in columns (7) and (8) consist in estimating random effects and rare event logits (on rare even logistic regressions, see King and Zeng, 1999). Once again, the results are very close to our first logit and probit estimations. Note that we cannot reject the hypothesis that the panel-level variance component is not significant, suggesting that the pooled logit estimator is not significantly different from the panel estimator.

In table 3 we explore alternative specifications and include various country characteristics, such as income *per capita*, democracy, political rights, the quality of institutions, the number of years the leader has spent in office, external price shocks and alternative measures of education. Table 7 suggests that these country characteristics are not very important. The chances of initiating a sustained turnaround are not significantly different depending upon the country's level of income, nor upon its level of democracy or political rights. Hence, the modest trend towards democratization, even if coupled with some growth in income, would not significantly shorten the expected duration of being a failing state. We should note, however, that all failing states are by definition low-years the leader spent in office is not significant in table 7. The quality of institutions as measured by the ICRG (rule of law, bureaucratic quality, risk of expropriation, risk of repudiation of contracts, corruption) also has no significant impact on the probability of turnaround. Similarly, external positive price shocks and higher and primary education measures are insignificant.

Regarding the robustness of our results as analyzed in table 3, all the variables of the initial model remain significant with reasonably stable coefficients whatever new explanatory variables are added (and their induced changes in the sample). The only low

p-values are the one for aid in regression (4) [p = 0.151] and for post-conflict in regression (5) [p = 0.102].

Table 3 – *Robustness checks : alternative specifications, Logit estimations, 1973-1999.*

Start of sustained turnarounds	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Post-conflict, years 1 to 4	1.197*	1.231*	1.343*	1.910**	1.240	1.254*	1.546**	1.380*
	(1.71)	(1.71)	(1.81)	(2.12)	(1.63)	(1.78)	(2.06)	(1.68)
Secondary education	0.328***	0.330***	0.319***	0.330**	0.351***			0.392***
	(3.17)	(2.95)	(3.08)	(2.02)	(3.11)			(3.02)
Ln population	0.944*	0.884*	0.892**	0.882*	0.887**	0.862**	0.966***	0.889**
	(1.73)	(1.89)	(2.06)	(1.79)	(2.15)	(2.04)	(3.21)	(2.43)
(ODA-TA)/GNI, predicted	0.349*	0.334*	0.310*	0.222	0.334*	0.402**	0.508**	0.361*
	(1.95)	(1.83)	(1.78)	(1.44)	(1.80)	(2.22)	(2.30)	(1.94)
TA/GNI, predicted	-0.127	-0.150	-0.092	0.350	-0.127	-0.295	-0.432	-0.111
	(-0.24)	(-0.26)	(-0.18)	(0.78)	(-0.26)	(-0.58)	(-1.01)	(-0.24)
Ln income *per capita*, t-1	0.269							
	(0.43)							
Democracy		-0.005						
		(-0.04)						
Political rights			0.081					
			(0.45)					
Quality of institutions				0.123				
				(0.39)				
Years spent in office					-0.015			
					(-0.37)			
Higher education						0.202		
						(0.87)		
Primary education							0.151	
							(1.60)	
Dummy positive price shocks								1.074
								(0.86)
Constant	-17.0*	-14.4***	-14.6***	-15.2**	-14.4***	-13.8***	-16.0***	-14.9***
	(-1.69)	(-2.89)	(-2.97)	(-2.49)	(-3.28)	(-3.01)	(-4.18)	(-4.01)
Observations	412	410	409	331	406	412	412	402
Predicted pobabilities	0.0167	0.0170	0.0169	0.0154	0.0166	0.0174	0.0151	0.0157
Wald test (*p*-values)	0.000	0.002	0.003	0.007	0.002	0.017	0.019	0.000
Pseudo-R^2	0.173	0.173	0.172	0.208	0.180	0.166	0.186	0.194
Log-Likelihood	-53.29	-53.22	-53.24	-40.85	-52.63	-53.76	-52.44	-51.62

Logit estimations. Dependent variable is the start of a sustained turnaround. *t*-Student robust to heteroskedasticity in brackets. *** : significant at 1% ; ** : significant at 5% ; * significant at 10%. Each tested variable was also introduced in the instrumentation regressions. When education variables are tested, secondary education is dropped from the instrumentation regression. All education variables are "completed education" (same results with "total education attained"). Democray is from Polity IV (high values : high level of democracy). Political rights is from Freedom House (high values : high level of political rights). Institutions is from International Country Risk Guide (high values : high quality of institutions). Years spent in office is from the Center of International Studies of Princeton University and the State Failure Task Force. The dummy for positive price shocks is from Collier and Dehn (2001). The dummy for negative shock was dropped due to colinearity.

4. Aid to Sustain Incipient Turnarounds

In Section 3 we considered the preconditions for sustained turnaround in failing states. We now turn to the question of conditions and interventions *during* the early stages of a reform. The objective that we analyze is again 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: that route would lead into the problem of aid volatility. We investigate a range of thresholds. A low threshold of CPIA improvement will imply not only more volatility but also more false dawns: a higher proportion of 'incipient turnarounds' will revert. Specifically, we consider threshold levels of CPIA improvement of 0.25, 0.5, 0.75, and 1.0. In Table 4 we report for the typical failing state 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 4 – *Volatility and Disappointment 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 4 illustrates the fundamental donor problem in supporting signs of reform. Mostly, the donor will end up backing reforms that would have failed without support, but which as we will see, are still likely to fail even with support. 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 will be for such cases. To avoid being superfluous, donors

should indeed target situations which would fail without support. However, public scrutiny of performance will make donors cautious of precisely these situations, whereas being superfluous cannot be detected. Here 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.

Thus, we take as our sample 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 matured into a sustained turnaround (on our previous definition). Various methodologies are available for such a question. The one we use involves a switch from the logit regressions to hazard functions. We track the reform, investigating in turn the factors which lead to success – the attainment of a 'sustained turnaround' – or complete failure – the collapse of the incipient reform and reversion to the initial state. We use a Weibull distribution to model our data. It is suitable for modeling data with monotone hazard rates that either increase or decrease exponentially with time, according to a shape parameter, p. When p is greater (smaller) than one, the hazard of failure increases (decreases) with time, while if p equals one, the Weibull model reduces to the exponential model. The Weibull model is fitted in the proportional hazard metric, which implies that an explanatory variable with a positive coefficient increases the hazard that the process will end – *i.e.* decreases the duration for a turnaround to be sustained (in the first case) or the duration for the incipient reform to collapse (in the second case).[3]

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, there is a trade-off between a high frequency of observation – choosing short time periods – and statistical significance. Our highest frequency analysis considers two-year periods. Thus, our first observation is the first two years of an incipient turnaround – that is, the first two years after the CPIA has crossed the threshold of a 0.5 point improvement. The second observation is the third and fourth years after the threshold has been crossed and so forth. We supplement this with four-year periodization.

We use specification tests to establish the explanatory variables to be included. However, there is a trade-off between sample size, variables, and time units. The largest sample is for pooled two-year time periods. With this approach specification tests find several variables to be significant that lose significance when sample sizes are reduced once the time periods are not pooled. Our most reliable results are thus for the pooled two-year time periods. However, with such pooling we are unable to investigate issues of timing during the incipient reform. We therefore supplement the pooled results with those in which time periods are distinguished. Since the shorter the time period the more variables are introduced, we restrict the analysis to four-year periods. Some of the explanatory variables that are significant in the pooled regression are seriously sample-constraining, and when they are included, the sample size is small for the number of variables. Since our core focus for interventions during incipient reform is on the timing of aid, we

[3] The Weibull model can also be fitted in the accelerated failure-time metric, in which case a positive value for a coefficient implies that the explanatory variable increases the duration of the process.

therefore supplement these results by dropping those variables that are most data-constraining.

With respect to aid, we again distinguish 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.

Hazard function : two-year periods, pooled observations

Our first analysis focuses on the factors which make the incipient turnaround more or less likely to achieve the criteria for a sustained turnaround (Table 5, Model 1). Specification tests establish several variables other than aid as significant. An incipient reform is more likely to continue to sustained transformation the higher is income, the larger is the population, and the greater is the proportion of the population with primary education. Whereas on our previous results post-conflict countries are much more likely to launch a sustained turnaround, they are also more likely to have false dawns. In effect, post-conflict situations are highly fluid: it is easier to achieve a sustained turnaround, but often incipient changes occur that lead nowhere. Echoing a result of Dollar and Svensson (2000), the longer the leader has been in office, the less likely is an incipient turnaround to maintain momentum. However, our most surprising result concerns terms of trade shocks. Favorable terms of trade shocks appear to derail incipient reforms. We return to this in our discussion of the results concerning aid.

Both technical assistance and other aid have significant effects on the time which an incipient turnaround takes to become a sustained turnaround. For both the relationship is non-linear (the quadratic term is significant). 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. The non-linearity for other aid is, however, dramatically different. Small amounts of aid (excluding TA) actually slow down the process of turnaround: taken literally, the results imply that very big aid works. The threshold above which the net effect of aid becomes positive, accelerating the attainment of a sustained turnaround is, however, very high – around 30% of GDP. Indeed, figures above this are so infrequent in the sample that the quadratic is being driven by observations in which the net effect of aid is negative. The somewhat disturbing results on financial aid are at least consistent with those on the effect of favorable terms of trade shocks. It appears that a swift increase in public resources chills the pace of reform.

Table 5 – *Model 1 : econometric estimates of hazard function parameters.*

	Model 1 : Time until sustainability is achieved					
			Observation level heterogeneity		Country level heterogeneity	
	1	2	3	4	5	6
Ln income p.c., t-1	1.536	1.707**	2.096	2.091	1.536	1.707
	(1.61)	(1.73)	(1.14)	(1.03)	(1.32)	(1.29)
Ln population	1.647**	1.632***	1.793*	2.158*	1.647***	1.632***
	(2.47)	(3.14)	(1.74)	(1.73)	(2.71)	(2.76)
Post-conflict, years 1 to 4	-2.614**	-4.784**	-3.258	-6.210	-2.614	-4.784*
	(-2.23)	(-2.60)	(-1.06)	(-1.61)	(-1.53)	(-1.90)
Primary education	0.140*	0.193**	0.243	0.268	0.140	0.193*
	(1.90)	(2.31)	(1.47)	(1.55)	(1.55)	(1.74)
Dummy positive price shocks	-25.9***	-22.4***	-39.2	-29.7	-69.2	-50.3
	(-15.10)	(-11.64)	(0.00)	(0.00)	(0.00)	(0.00)
Years the leader spent in office	-0.081*	-0.118**	-0.163	-0.184	-0.081	-0.118
	(-1.82)	(-2.18)	(-1.01)	(-1.18)	(-1.24)	(-1.46)
(ODA-TA)/GNI, predicted	-0.059	-1.282**	-0.132	-1.813**	-0.059	-1.282**
	(-0.49)	(-2.82)	(-0.51)	(-1.83)	(-0.39)	(-2.28)
(ODA-TA)/GNI squared, pred.		0.042**		0.059*		0.042**
		(2.65)		(1.90)		(2.28)
TA/GNI, predicted	1.504**	5.765***	1.943	8.780*	1.504*	5.766***
	(2.44)	(3.68)	(1.20)	(1.67)	(1.95)	(2.70)
TA/GNI squared, predicted		-0.561***		-0.901		-0.561**
		(-3.34)		(-1.59)		(-2.25)
Constant	-34.892**	-35.975***	-42.295*	-46.741	-34.892**	-35.977**
	(-2.55)	(-3.09)	(-1.78)	(-1.63)	(-2.52)	(-2.42)
Observations at risk	214	214	214	214	214	214
Countries	43	43	43	43	43	43
Exit from the risk category	10	10	10	10	10	10
Wald test (*p*-value)	0.000	0.000	0.689	0.313	0.723	0.338
p for Weibull distribution	1.624	1.744	2.628	2.486	1.624	1.744
Log-Likelihood	-20.269	-17.302	-19.929	-16.943	-20.269	-17.302
Test $\theta = 0$ (*p*-value)			0.205	0.198	1.000	0.500

Weibull regressions with duration in years as dependent variable. Z-statistics in parentheses. *** : significant at 1% ; ** : significant at 5% ; * significant at 10%. (ODA-TA)/GNI and TA/GNI are predicted from instrumentation regressions including the same instruments as in Appendix 5, along with the exogenous explanatory variables of the model. These estimations are not shown but lead to results very similar to those of Appendix 5.

In columns (3) to (6) of table 5, we investigate whether controlling for unobserved heterogeneity – both at the observations and country levels – modify our results. Frailty models (observation-level heterogeneity) and shared frailty models (country-level heterogeneity) are estimated assuming a Gamma distribution for frailty. First, we find no evidence of unobservable heterogeneity ($\theta = 0$). Second, coefficients are overall pretty stable, one major difference with columns (1) and (2) of table 5 being that the dummy for positive price shocks is no longer significant. The calculated optimum for technical assistance remains at around 5%.

In table 6 below, we present specification tests for model 1. We introduce variables for democracy, political rights, the quality of institutions, a dummy for negative price shocks and alternative measured of education (secondary and higher education). None of them are significant, and the previous results are maintained.

Table 6 – *Specification tests for Model 1.*

	Model 1 : Time until sustainability is achieved											
	1	2	3	4	5	6	7	8	9	10	11	12
Ln income p.c., t-1	1.667*	1.605	1.101	1.368	1.785**	2.435**	2.061**	1.830*	0.525	1.746*	2.221**	3.266**
	(1.74)	(1.58)	(1.07)	(1.33)	(2.10)	(2.16)	(1.99)	(1.71)	(0.49)	(1.63)	(2.40)	(2.41)
Ln population	1.797**	1.700**	1.603***	1.577**	1.213**	1.608**	1.788***	1.684***	0.975***	1.597***	1.209**	1.628***
	(2.43)	(2.40)	(3.18)	(2.38)	(2.01)	(2.41)	(3.24)	(2.66)	(2.75)	(2.87)	(2.16)	(2.81)
Post-conflict, years 1 to 4	-2.924**	-2.876**	-2.598**	-2.718**	-1.404	-2.221**	-4.386	-4.641**	-5.31***	-4.550**	-2.976*	-3.265**
	(-2.31)	(-2.19)	(-2.49)	(-2.42)	(-1.36)	(-2.11)	(-2.86)	(-2.58)	(-2.77)	(-2.44)	(-1.69)	(-2.13)
Primary education	0.127	0.129*	0.230*	0.139*			0.183*	0.185**	0.254**	0.185**		
	(1.56)	(1.84)	(1.95)	(1.89)			(1.74)	(2.25)	(2.31)	(2.24)		
Dummy positive price shocks	-24.1***	-25.6***	-26.2***	-24.6***	-27.1***	-26.6***	-22.8***	-22.3***	-26.6***	-22.6***	-23.2***	-23.7***
	(-13.0)	(-14.2)	(-13.7)	(-14.7)	(-17.1)	(-15.8)	(-13.9)	(-12.0)	(-11.1)	(-11.9)	(-11.8)	(-13.4)
Years the leader spent in office	-0.076*	-0.066	-0.075	-0.089**	-0.078	-0.063	-0.120*	-0.107*	-0.074	-0.120**	-0.092	-0.065
	(-1.85)	(-1.42)	(-1.48)	(-2.16)	(-1.17)	(-1.11)	(-1.82)	(-1.66)	(-1.48)	(-2.21)	(-1.20)	(-1.01)
(ODA-TA)/GNI, predicted	-0.046	-0.068	-0.142	-0.042	0.073	0.043	-0.969**	-1.108**	-0.628	-1.23***	-1.19***	-0.906**
	(-0.39)	(-0.62)	(-0.95)	(-0.38)	(0.48)	(0.29)	(-2.33)	(-2.59)	(-1.41)	(-2.71)	(-2.86)	(-2.29)
(ODA-TA)/GNI squared, pred.							0.031**	0.035**	0.020*	0.041**	0.048***	0.034**
							(2.11)	(2.36)	(1.64)	(2.57)	(2.95)	(2.38)
TA/GNI, predicted	1.541**	1.569**	1.913**	1.394**	0.522	1.100	5.186***	5.282***	1.120	5.631***	4.952**	5.021***
	(2.36)	(2.50)	(2.19)	(2.51)	(0.74)	(1.50)	(3.07)	(3.29)	(0.69)	(3.56)	(2.52)	(2.69)
TA/GNI squared, predicted							-0.476**	-0.476**	0.079	-0.55***	-0.684**	-0.567**
							(-2.50)	(-2.63)	(0.50)	(-3.28)	(-2.52)	(-2.46)
Democracy	0.012						-0.056					
	(0.08)						(-0.30)					
Political rights		0.133						-0.006				
		(0.44)						(-0.02)				
ICRG			-0.744						-1.001			
			(-1.26)						(-1.41)			
Dummy negative price shocks				1.041						-1.623		
				(0.56)						(-0.76)		
Secondary education					-0.038						-0.071	
					(-0.40)						(-0.78)	
Higher education						-0.082						-0.163
						(-0.38)						(-0.68)
Constant	-37.48**	-36.46**	-29.60**	-32.86**	-29.82**	-39.7***	-40.6***	-37.6***	-15.79*	-35.8***	-32.17**	-46.3***
	(-2.62)	(-2.60)	(-2.62)	(-2.35)	(-2.52)	(-2.74)	(-3.29)	(-2.98)	(-1.75)	(-2.84)	(-2.68)	(-3.00)
Observations at risk	214	214	165	214	214	214	214	214	165	214	214	214
Countries	43	43	34	43	43	43	43	43	34	43	43	43
Exit from the risk category	10	10	8	10	10	10	10	10	8	10	10	10
p for Weibull distribution	1.640	1.617	1.609	1.632	1.525	1.552	1.722	1.732	1.635	1.721	1.639	1.655
Log-Likelihood	-19.674	-19.395	-15.254	-20.177	-22.385	-21.308	-17.64	-17.274	-12.139	-17.31	-19.17	-19.024
Wald test (*p*-value)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

We next turn to the complementary issue of preventing the collapse of an incipient reform. That is, we switch the focus from whether aid helps to press an incipient reform on to the attainment of 'sustained turnaround', to whether it helps to reduce the risk that the incipient reform will fall apart altogether, taking the country back to where it had started. These results are shown in Table 7. Although the issue is distinct from the previous one, the political economy processes should evidently be closely related. If the analysis is robust we should therefore expect to find results that are approximately the mirror image of our previous results. Both to facilitate comparison, and because there seems no good reason for relapses to be driven by fundamentally different processes from those that determine successes, we therefore maintain the same specification. The results reassuringly complement our previous results. Almost everything is indeed the mirror image. Incipient reforms are significantly more likely to relapse if income and population is low, if the country is post-conflict, if there is a positive terms of trade shock, and if the leader has been in power a long time. The results on technical assistance are particularly striking, the quadratic implies precisely the same optimum of 5% of GDP. This level of technical assistance, which is substantial, not only enhances the prospects of success, but averts relapse. The only results that are not the mirror image of our previous results concern education and aid other than technical assistance, neither of which are significant in averting relapse.

Table 7 – Model 2 : *econometric estimates of hazard function parameters*.

	Model 2 : Time until the turnaorund collapses					
			Observation level heterogeneity		Country level heterogeneity	
	1	2	3	4	5	6
Ln income p.c., t-1	-1.855**	-2.370*	-3.457*	-6.024	-10.674***	-2.370**
	(-2.00)	(-1.80)	(-1.71)	(-1.50)	(-2.79)	(-2.14)
Ln population	-1.779***	-1.957***	-4.614**	-6.260*	-11.110***	-1.957***
	(-5.36)	(-3.51)	(-2.55)	(-1.93)	(-4.04)	(-3.56)
Post-conflict, years 1 to 4	2.404**	2.502**	4.272	4.361	12.829***	2.502*
	(2.17)	(2.03)	(1.45)	(1.41)	(4.33)	(1.91)
Primary education	-0.063	-0.044	-0.126	-0.188	-0.280	-0.044
	(-0.40)	(-0.33)	(-0.73)	(-0.72)	(-1.05)	(-0.36)
Dummy positive price shocks	3.143***	3.574***	3.975	4.597**	3.555	3.574**
	(2.81)	(2.77)	(1.85)	(2.04)	(0.97)	(2.52)
Years the leader spent in office	0.060*	0.050*	0.136*	0.090	0.132	0.050
	(1.89)	(1.67)	(1.92)	(1.21)	(0.84)	(1.33)
(ODA-TA)/GNI, predicted	-0.115	0.156	0.039	0.785	0.091	0.156
	(-0.57)	(0.36)	(0.20)	(0.89)	(0.21)	(0.34)
(ODA-TA)/GNI squared, pred.		-0.015		-0.035		-0.015
		(-0.95)		(-0.90)		(-0.71)
TA/GNI, predicted	-0.858	-2.444**	-3.247**	-8.024*	-6.739**	-2.444*
	(-1.26)	(-2.25)	(-2.02)	(-1.69)	(-2.50)	(-1.82)
TA/GNI squared, predicted		0.259		0.580		0.260
		(1.62)		(1.50)		(1.34)
Constant	27.9***	34.0**	66.4**	103.6*	178.3***	34.0**
	(3.30)	(2.29)	(2.13)	(1.66)	(3.44)	(2.55)
Observations at risk	154	154	154	154	154	154
Countries	42	42	42	42	42	42
Exit from the risk category	19	19	19	19	19	19
Wald test (*p*-value)	0.000	0.000	0.000	0.000	0.000	0.000
p for Weibull distribution	2.754	2.798	5.563	6.532	14.582	2.798
Log-Likelihood	-20.22	-19.25	-15.36	-13.26	-19.26	-19.25
Test $\theta = 0$ (*p*-value)			0.001	0.000	0.083	1.000

Weibull regressions with duration in years as dependent variable. Z-statistics in parentheses. *** : significant at 1% ; ** : significant at 5% ; * significant at 10%. (ODA-TA)/GNI and TA/GNI are predicted from instrumentation regressions including the same instruments as in Appendix 5, along with the exogenous explanatory variables of the model. These estimations are not shown but lead to results very similar to those of Appendix 5.

Four-year periodization

We now take the analysis further by distinguishing four-year periods separately (Table 8). As noted, this has the potential to show when during an incipient turnaround support is most useful. However, due to constraints of sample size we drop the quadratic terms. We investigate four variants which represent different choices in the trade-offs between sample size, explanatory variables and degrees of freedom. The ideal specification in the absence of considerations of sample size is that of the first column. All the explanatory variables that were found to be significant in the previous specification tests are retained, and we investigate the effects of technical assistance and other aid over twenty-years of reform, divided into five sub-periods. The second specification economizes on degrees of freedom by reducing the period analyzed from twenty years to twelve: the first three sub-periods. The third specification reverts to the full twenty years but expands the sample by dropping four sample-constraining explanatory variables. The final specification combines this reduction in the explanatory variables with the narrower focus on the first twelve years.

We consider the prospects of attaining a sustained turnaround (Model 1) and focus initially on technical assistance. All four specifications tell a consistent story with regard to technical assistance: it has significantly positive effects, but these effects are concentrated in the first four years of the incipient reform. Two of the four specifications find weakly significant positive effects in the second four-year period, but with smaller coefficients than during the first period. All four specifications find that by the third four-year period technical assistance is insignificant. Thus, all of our regressions on technical assistance during incipient reform find it to be significantly useful. The scale should be of the order of 5% of GDP, and the timing should be during the first four years after the 0.5 threshold improvement in the CPIA has been triggered. Our results do not preclude that technical assistance is useful at other times, but they do suggest that it is particularly useful at this time.

We next investigate the periodization of the effect of aid other than technical assistance. The results are weaker but they do suggest a pattern. All four specifications find that during the first four years the effects of this aid are negative, although the coefficients are only significant in two of the four regressions. Conversely, during the second four-year period all four specifications find the effects to be positive, although the coefficients are again only significant in two of the four specifications. Hence, it appears that to the extent that such aid is useful it is needed somewhat later in the reform program. Indeed, there is some evidence for a sequence of aid interventions during incipient reform: technical assistance during the first four years, followed by money in the next four years.

Table 8 – *Decomposition of the impact of TA and other aid by period : Model 1.*

	Model 1 : Time until sustainability is achieved			
	1	2	3	4
Ln income p.c.	0.390	1.246	0.241	1.013*
	(0.31)	(0.98)	(0.35)	(1.71)
Ln population	1.521*	2.642***	0.585	1.223***
	(1.86)	(2.96)	(1.44)	(4.09)
Post-conflict, years 1 to 4	-3.830	-6.723***		
	(-1.55)	(-2.86)		
Primary education	0.241**	0.184*		
	(2.23)	(1.76)		
Dummy positive price shocks	-76.73***	-48.99***		
	(-14.68)	(-8.57)		
Years the leader spent in office	-0.045	-0.046		
	(-0.57)	(-0.65)		
(ODA-TA)/GNI pred, years 1 to 4	-1.229***	-1.102***	-0.155	-0.212*
	(-3.84)	(-3.44)	(-0.84)	(-1.81)
(ODA-TA)/GNI pred, years 5 to 8	0.060	0.054	0.248**	0.210*
	(0.31)	(0.21)	(2.11)	(1.67)
(ODA-TA)/GNI pred, years 9 to 12	-0.274	0.030	-0.133	0.188
	(-0.67)	(0.19)	(-0.47)	(1.07)
(ODA-TA)/GNI pred, years 13 to 16	-5.020***		-0.721**	
	(-11.86)		(-1.96)	
(ODA-TA)/GNI pred, years 17 to 20	-6.302***		-4.876***	
	(-7.49)		(-7.18)	
(ODA-TA)/GNI pred, years 21 and 22			-6.868***	
			(-11.09)	
TA/GNI pred, years 1 to 4	6.951***	6.628***	1.485**	1.692***
	(3.91)	(4.18)	(1.98)	(2.94)
TA/GNI pred, years 5 to 8	1.562*	2.339*	-0.295	0.218
	(1.66)	(1.66)	(-0.67)	(0.44)
TA/GNI pred, years 9 to 12	1.173	1.258	-0.022	-0.468
	(0.80)	(1.42)	(-0.03)	(-0.81)
TA/GNI pred, years 13 to 16	8.600***		1.271**	
	(7.06)		(2.00)	
TA/GNI pred, years 17 to 20	10.989***		7.377***	
	(4.88)		(3.99)	
TA/GNI pred, years 21 and 22			13.610***	
			(8.92)	
Constant	-34.535*	-49.062**	-15.370*	-25.868***
	(-1.69)	(-2.40)	(-1.86)	(-3.85)
Observations	119	119	181	181
Countries	43	43	64	64
Exit from risk category	10	10	17	17
Log likelihood	-2.38	-7.39	-19.88	-26.30
Wald test (*p*-value)	0.000	0.000	0.000	
for Weibull distribution	11.89	6.69	6.53	3.48

Weibull regressions with duration in years as dependent variable. Z-statistics in parentheses. *** : significant at 1% ; ** : significant at 5% ; * significant at 10%. (ODA-TA)/GNI and TA/GNI are predicted from instrumentation regressions including the same instruments as in Appendix 5, along with the exogenous explanatory variables of the model. These estimations are not shown but lead to results very similar to those of Appendix 5.

We next investigate the obverse of the hazard that an incipient reform will continue to sustained transformation, namely the hazard that it will collapse altogether. Table 9 replicates the analysis of Table 8 for this different hazard. Perhaps because incipient reforms usually take longer to relapse than to succeed, the deletion of aid in the later periods, nearly all of which are significant, eliminates the significance of the remaining aid variables (columns 2 and 4). Hence, we focus on the results for the full period (columns 1 and 3). We first consider technical assistance. For the first 16 years of the reform, technical assistance consistently reduces the risk of relapse, and mostly this is statistically significant. Thus, whereas its ability to impart momentum to an incipient reform appears to be relatively short lived, it may have some more durable role in staving off reversion. Aid other than technical assistance also appears to have this more durable role. Recall that in assisting incipient reforms to progress, such aid was only significant in years 5-8. In staving off relapse it is significant in all four specifications in the subsequent period, years 9-12.

Table 9 – *Decomposition of the impact of TA and other aid by period : Model 2.*

	Model 2 : Time until the turnaorund collapses			
	1	2	3	4
Ln income p.c.	-3.230***	-1.977*	-2.683***	-0.804*
	(-2.88)	(-1.80)	(-4.37)	(-1.72)
Ln population	-2.454***	-1.428***	-2.203***	-0.869***
	(-3.75)	(-3.86)	(-4.14)	(-3.25)
Post-conflict, years 1 to 4	4.285**	2.118		
	(2.03)	(1.04)		
Primary education	0.010	0.104		
	(0.11)	(0.97)		
Dummy positive price shocks	6.536***	6.087***		
	(3.71)	(3.38)		
Years the leader spent in office	0.030	-0.0002		
	(0.96)	(-0.01)		
(ODA-TA)/GNI pred, years 1 to 4	-0.026	-0.169	0.077	0.048
	(-0.17)	(-1.20)	(0.70)	(0.42)
(ODA-TA)/GNI pred, years 5 to 8	-0.156	-0.133	0.001	0.083
	(-0.68)	(-0.68)	(0.00)	(0.60)
(ODA-TA)/GNI pred, years 9 to 12	-0.706***	-0.363**	-0.562**	-0.226*
	(-2.69)	(-2.26)	(-2.53)	(-1.64)
(ODA-TA)/GNI pred, years 13 to 16	18.01***		1.758***	
	(32.51)		(2.93)	
(ODA-TA)/GNI pred, years 17 to 20	-18.14***		-18.64***	
	(-29.34)		(-22.04)	
TA/GNI pred, years 1 to 4	-0.495	0.587	-0.881**	-0.031
	(-0.64)	(0.97)	(-2.04)	(-0.07)
TA/GNI pred, years 5 to 8	-1.837**	-0.180	-1.764***	-0.642
	(-2.01)	(-0.24)	(-3.12)	(-1.18)
TA/GNI pred, years 9 to 12	-0.965	0.379	-0.362	0.320
	(-1.45)	(0.80)	(-0.77)	(0.84)
TA/GNI pred, years 13 to 16	-84.25***		-14.436***	
	(-32.36)		(-4.27)	
TA/GNI pred, years 17 to 20	61.01***		54.08***	
	(28.6)		(24.17)	
Constant	43.14***	23.43**	37.91***	10.78**
	(3.31)	(2.50)	(4.12)	(2.14)
Observations	87	87	136	136
Countries	42	42	63	63
Exit from risk category	22	22	29	29
Log likelihood	-3.24	-20.71	-20.22	-40.63
Wald test (*p*-value)	0.000	0.000	0.000	0.000
p for Weibull distribution	9.441	3.971	6.178	2.846

Weibull regressions with duration in years as dependent variable. Z-statistics in parentheses. *** : significant at 1% ; ** : significant at 5% ; * significant at 10%. (ODA-TA)/GNI and TA/GNI are predicted from instrumentation regressions including the same instruments as in Appendix 5, along with the exogenous explanatory variables of the model. These estimations are not shown but lead to results very similar to those of Appendix 5.

5. The Benefits and Costs of Interventions

In the two preceding sections we have found that aid, both as technical assistance and as finance sometimes significantly increases the prospects of a failing state achieving a sustained turnaround. In certain contexts, aid is effective. The purpose of the section is to investigate whether, in those circumstances in which it is effective, it is also *cost*-effective. Evidently, the main task here is to estimate the benefits. These in turn decompose into the typical payoff should a failed state achieve a sustained turnaround, and the increase in the probability that this will come about due to the intervention.

The cost of being a failing state to the country itself

Obviously, the core of the costs consequent upon a state failing are what this does to the country's own population. Poor policies, institutions and governance increase poverty. One simple indicator is the loss of growth associated with being a failing state. Although all failing states are beset by poor policies, institutions and governance, an important distinction is between those beset by violent conflict – civil war – and those that are not. That civil war damages economic performance is unsurprising. Typical estimates are that growth is reduced during war by between two and three percentage points (Collier, 1998). Here, our focus is on the economic damage done by failing state policies, institutions and governance, even if the country is at peace.

We estimate this cost by introducing a failing state dummy into a standard growth regression. In table 10 the growth regressions are estimated by ordinary least squares. Regression (2) suggests that Failing State status typically reduces the annual growth rate of peacetime economies by 2.5 percentage points relative to other developing economies. This is clearly a very large effect, implying that turnaround, where it is possible, is cumulatively highly beneficial.

Table 10 – *Growth spillover effects of Failing States: 4-years sub-periods, 1974-2001*

Annual average income growth, OLS estimations	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Ln income *p.c.* *t*-1	-0.004*	-0.004**	-0.004*	-0.004*	-0.005**	-0.005**	-0.008***	-0.008***
	(-1.94)	(-2.09)	(-1.72)	(-1.74)	(-2.39)	(-2.48)	(-3.35)	(-3.58)
CPIA			0.021***	0.021***	0.017***	0.017***		0.016***
			(8.52)	(8.51)	(6.87)	(6.79)		(6.69)
Failing State	-0.027***				-0.017***			
	(-7.18)				(-4.33)			
Failing State at war		-0.040***				-0.026***	-0.040***	-0.026***
		(-4.60)				(-3.21)	(-4.69)	(-3.27)
Failing State at peace		-0.025***				-0.015***	-0.024***	-0.014***
		(-6.74)				(-3.96)	(-6.67)	(-3.89)
Neighbours of FS			-0.017***					
			(-3.43)					
Neighbours of FS at war				-0.019**			-0.017**	-0.018**
				(-2.34)			(-2.04)	(-2.30)
Neighbours of FS at peace				-0.016***			-0.017***	-0.016***
				(-3.17)			(-3.58)	(-3.17)
Dummy 1978-1981	0.0004	0.001	-0.0005	-0.0004	0.00003	0.0005	0.001	0.001
	(0.07)	(0.20)	(-0.09)	(-0.07)	(0.01)	(0.10)	(0.23)	(0.15)
Dummy 1982-1985	-0.016***	-0.016***	-0.020***	-0.020***	-0.019***	-0.018***	-0.015***	-0.018***
	(-3.15)	(-3.00)	(-4.02)	(-3.99)	(-3.73)	(-3.60)	(-3.01)	(-3.58)
Dummy 1986-1989	-0.007*	-0.007	-0.012***	-0.012***	-0.010**	-0.010**	-0.007	-0.010**
	(-1.68)	(-1.49)	(-2.76)	(-2.71)	(-2.43)	(-2.26)	(-1.61)	(-2.34)
Dummy 1990-1993	-0.016***	-0.015***	-0.022***	-0.022***	-0.021***	-0.020***	-0.016***	-0.020***
	(-3.41)	(-3.26)	(-4.83)	(-4.77)	(-4.55)	(-4.43)	(-3.44)	(-4.57)
Dummy 1994-1997	-0.006	-0.005	-0.013**	-0.013**	-0.011**	-0.011**	-0.007	-0.012**
	(-1.12)	(-1.01)	(-2.48)	(-2.44)	(-2.13)	(-2.02)	(-1.35)	(-2.31)
Dummy 1998-2001	-0.016***	-0.016***	-0.029***	-0.029***	-0.027***	-0.026***	-0.018***	-0.028***
	(-3.87)	(-3.80)	(-6.59)	(-6.56)	(-6.15)	(-6.07)	(-4.32)	(-6.52)
Constant	0.060***	0.062***	-0.003	-0.002	0.015	0.017	0.098***	0.052***
	(3.64)	(3.77)	(-0.14)	(-0.13)	(0.85)	(1.00)	(4.81)	(2.39)
Observations	605	605	605	605	605	605	605	605
Countries	106	106	106	106	106	106	106	106
R^2	0.128	0.138	0.191	0.191	0.196	0.201	0.159	0.220
F-test (*p*-values)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

OLS estimations. Dependent variable is the annual growth rate of income *per capita*. *t*-Student robust to heteroskedasticity in parentheses. *** : significant at 1% ; ** : significant at 5% ; * significant at 10%.

The cost of being a failing state evidently mounts with its persistence. As we have seen, the condition is highly persistent: the annual chance of commencing a sustained turnaround being only 1.7%. To quantitative the cost we consider a long term horizon and estimate the expected loss as a percentage of initial GDP. The country that starts out as a failing state sees its GDP decline, relative to a comparator country, by 2.5% for each year that it stays a failing state, while having a chance of sustained turnaround of only 1.7% per year. We assume that once a country does have a turnaround, it gradually recovers

onto the normal state growth path, the time taken to regain the growth path being the same as the number of years that the country has been a failing state. Thus, for example, a country that turns around after only two years, will lose 2.5% of initial GDP in the first year and 5% in the second year. In the third year it starts to recover, so that the fourth year it regains the level of income that it would have had. But meanwhile, the country still incurs a loss relative to the counterfactual of not having been a failing state.[4] We then discount these four losses back to the opening period, using a discount rate of 5%. We make such a calculation for each possible future scenario and sum them, weighted by the probability that each scenario will occur. Details of the calculations are reported in appendix 2. The result is the expected cost to a country of starting out as a failing state, given the likely prospects of turnaround. We estimate a cost of 4.9 times the initial GDP.

Spillovers

However, even this seriously underestimates the extent of the development problem posed by failing states because it ignores the spillovers across the neighbourhood. There are already quantitative estimates of such spillovers for the case of civil wars. Both Murdoch and Sandler (2002) and Collier and Hoeffler (2004) find significant effects. Here we focus on the spillovers of a failing state *at peace*. Using the same econometric approach as previously, we find that the typical neighbour loses 1.6 percentage points of its growth rate if its neighbour is a failing state (see table 10, regression 4). In regression (4) the specification is slightly different from that of regressions (1) and (2). Here, the CPIA is introduced along with the dummies for neighbouring countries of a failing state in order to capture countries' own economic and institutional performance. The CPIA was not introduced in the first two regressions because the LICUS dummy is based on this variable, so they are highly correlated. This is confirmed by regressions (5) and (6) in which failing state dummies are introduced along with the CPIA. Compared to regressions (1) and (2), the coefficients of the failing state dummies are much smaller (diminished by one third).

An alternative specification is to estimate simultaneously the failing state dummies and the dummies for their neighbouring countries (regressions 7 and 8), with and without the CPIA. Results from regression (7) lead to very similar conclusions as those of regressions (2) and (4) : the failing state itself loses 2.4 percentage points off the growth rate, while neighbouring countries lose 1.7 percentage points. Regression (8) confirms the colinearity between the failing state dummies and the CPIA : while the coefficients of the dummies for neighbouring countries are almost unchanged when the CPIA is introduced, the coefficients of the failing state dummies are greatly decreased compared to that of regression (7).[5]

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[4] We assume that during the third year, the country still loses 5% of its initial GDP, while losing 2.5% during the fourth year.

[5] We should note that although we find surprisingly large spillover effect, we have not taken into account the non-economic consequences of such potential spillovers as violence, organised crime, communicable diseases and refugees.

The loss for the typical neighbour is then calculated in the same way as the loss to the country itself, with the growth rate loss of 1.6% (regression 4) replacing the previous loss of 2.5%. The rest of the calculation follows precisely the same approach. Following this procedure, we estimate the cost to the typical neighbour as 3.47 times its initial GDP. While this is unsurprisingly a smaller loss than that for the country itself, we have yet to allow for the fact that the typical failing state has around three neighbours and so inflicts spillover effects on three countries. The loss-to-neighbours is therefore triple 347%.

Suppose, for purposes of illustration, that the neighbours have economies of a similar size to that of the failing state itself. Then the total loss of income to neighbours is more than double the loss to the country itself: two thirds of the economic damage done by failing states are externalities accruing to neighbours rather than costs to the country itself. These large externalities imply that the internal pressure for reform will be considerably less than is warranted by the costs of the failure to reform. They therefore warrant a substantial donor effort to promote reform, conditional, of course, upon such an effort having a reasonable prospect of success.

Based on the above numbers, it is possible to make a lower-bound estimate of the 'cost' of a failing state. This is the NPV of the growth losses to the country itself and to its neighbours. Taking the conservative assumption that the GDP of neighbours is on average the same as that of the typical failing state, the total cost is the sum of the two costs estimated above, multiplied by the initial GDP of the average failing state. That is:

[(4.89 + 3x3.47)].AVERAGE LICUS GDP ($5.56bn) = $85.2bn.

Although this figure of the expected economic cost of a failing state is very approximate, it can provide some guidance when we come to the potential benefits from aid interventions. In effect, if there were some intervention that overnight transformed a single failing state into an ordinary low-income country, the benefit would be of the order of $85.2bn. This sum is the foundation for a cost-benefit analysis of aid intervention to promote sustainable turnarounds.

Robustness checks

The calculation of the total cost of a failing state is based on the coefficients of growth regressions, so robustness checks are provided in table 11. Coefficients of table 10 are likely to suffer from two sources of endogeneity bias. A first endogeneity bias comes from unobservable country heterogeneity which, if correlated with the variables of the model, creates a correlation between the independent variables and the residuals. A second cause of endogeneity is the simultaneity relationship between growth and some of the right-hand side variables. The CPIA has often been criticized on the ground that it reflects the assessment of World Bank officials regarding growth of developing economies. As such, increases in the CPIA might be caused by growth, as much as growth might be caused by the CPIA. Of course, the failing state dummies, which are based on the values of the CPIA (see section 2) fall under the same criticism.

In table 11 unobservable heterogeneity is tackled by the introduction of fixed effects. Both the *within* estimator and the *generalized method of moment* (first-difference or system-GMM) take into account country specific heterogeneity. The second source of endogeneity (double causation) is taken care of through instrumental variables (difference and system-GMM) where all the right-hand side variables are instrumented (except time dummies). The difference-GMM estimator (Arellano and Bond, 1991) is performed on first-difference growth equations, where all the endogenous explanatory variables are instrumented by their lag values in level (from lag $t-2$ for endogenous variables and $t-1$ for predetermined variables).[6] The system-GMM estimator (Blundell and Bond, 1998) is performed on a system of both difference and level equations, where difference equations are instrumented with lagged variables in level and level equations are instrumented with lagged variables in difference. Both GMM estimators proceed in two-steps. The first-step estimator assumes homoskedasticity of the residuals, while the second-step estimator is robust to heteroskedasticity. While the coefficients from first and second-step estimations are very close, the standard errors are likely to be smaller in the latter because of a small-sample bias. Windmeijer (2000) proposes a correction for this finite-sample bias. Thus, in table 2, the two-step difference and system-GMM estimators are reported, with the Windmeijer correction of the standard errors.

The dummies for failing states and for neighbouring countries do not vary much over time (see the table in appendix 3, where standard errors of dummies in difference are much lower than those of dummies in levels). Therefore, the *within* and first-difference transformations of the variables are unlikely to be as significant as in the OLS and system-GMM estimations. This is confirmed by regressions (1) to (8) of table 11 where only in regressions (1) and (3) are the failing state dummies significant (with coefficients greatly reduced). In difference-GMM, none of the dummies are significant. However, the system-GMM estimations confirm our OLS results. Both dummies for failing states and for neighbours are significant (whatever the specification), with coefficients close to those of table 10.

[6] Here all the instrumented explanatory variables are supposed to be endogenous rather than predetermined. A drawback of GMM (either difference or system GMM) is that the number of instruments increases rapidly with the number of periods and endogenous explanatory variables. Thus, the number of lags allowed as instruments has been constained to 6, and the number of instruments is reported at the end of each tables. The assumption of no residual autocorrelation is essential to use the lagged variables as instruments. If the error terms of equations in level are not auto-correlated, the first-order autocorrelation of first-differenced residuals should be significant, whereas their second-order autocorrelation should not be significant. The validity of instruments is also tested with a Sargan test of over-identification. Autocorrelation and Sargan tests are reported in table 2.

Table 11 – *Robustness checks : within, difference-GMM and system-GMM estimations*.

	WITHIN				DIFF-GMM				SYS-GMM			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Ln income *p.c.* *t*-1	-0.060***	-0.063***	-0.064***	-0.063***	-0.051**	-0.046***	-0.056***	-0.061***	0.004	-0.007*	-0.006	-0.011**
	(-8.11)	(-8.48)	(-8.31)	(-8.49)	(-2.55)	(-2.71)	(-3.28)	(-3.60)	(1.01)	(-1.72)	(-1.51)	(-2.55)
CPIA		0.018***		0.016***		0.024***		0.019***		0.025***		0.022***
		(6.06)		(5.14)		(3.10)		(2.88)		(4.20)		(4.52)
Failing State at war	-0.030***		-0.029***	-0.017	-0.007		-0.022	-0.007	-0.041**		-0.036**	-0.018
	(-3.97)		(-3.86)	(-2.18)	(-0.37)		(-1.31)	(-0.52)	(-2.60)		(-2.46)	(-1.47)
Failing State at peace	-0.011*		-0.012**	-0.003	0.004		-0.015	-0.003	-0.027***		-0.029***	-0.016**
	(-1.86)		(-2.05)	(-0.56)	(0.25)		(-0.96)	(-0.29)	(-3.82)		(-4.49)	(-2.26)
Neighbours of FS at war		-0.010	-0.007	-0.011		-0.024	-0.014	-0.028		-0.054***	-0.052***	-0.049***
		(-0.83)	(-0.54)	(-0.87)		(-1.34)	(-0.67)	(-1.30)		(-3.26)	(-2.71)	(-2.89)
Neighbours of FS at peace		0.012	0.015	0.010		0.012	0.023	0.016		-0.029***	-0.021*	-0.023**
		(1.15)	(1.36)	(0.96)		(0.70)	(1.10)	(0.70)		(-2.85)	(-1.77)	(-2.09)
Dummy 1978-1981	0.003	0.002	0.003	0.002	-0.003	0.0004	-0.001	0.003	0.001	-0.001	0.001	0.001
	(0.53)	(0.32)	(0.65)	(0.47)	(-0.47)	(0.08)	(-0.12)	(0.54)	(0.25)	(-0.09)	(0.11)	(0.20)
Dummy 1982-1985	-0.011**	-0.014***	-0.010**	-0.013***	-0.017***	-0.015	-0.014***	-0.013**	-0.016***	-0.020***	-0.016***	-0.018***
	(-2.36)	(-2.90)	(-2.09)	(-2.71)	(-3.38)	(-2.50)	(-2.82)	(-2.44)	(-2.85)	(-4.49)	(-3.24)	(-4.09)
Dummy 1986-1989	-0.001	-0.004	0.001	-0.003	-0.005	-0.006	-0.003	-0.002	-0.008	-0.013**	-0.008	-0.011**
	(-0.20)	(-0.74)	(0.19)	(-0.64)	(-1.01)	(-0.92)	(-0.46)	(-0.34)	(-1.59)	(-2.49)	(-1.63)	(-2.47)
Dummy 1990-1993	-0.007	-0.011**	-0.005	-0.011**	-0.012	-0.015**	-0.009	-0.009	-0.017***	-0.024***	-0.017***	-0.022***
	(-1.49)	(-2.22)	(-0.93)	(-2.09)	(-1.45)	(-2.12)	(-1.19)	(-1.42)	(-3.20)	(-4.65)	(-3.46)	(-4.56)
Dummy 1994-1997	0.005	0.001	0.008	0.001	0.001	-0.003	0.004	0.002	-0.009	-0.016***	-0.009*	-0.015***
	(0.92)	(0.24)	(1.44)	(0.26)	(0.10)	(-0.40)	(0.50)	(0.25)	(-1.80)*	(-2.91)	(-1.85)	(-2.73)
Dummy 1998-2001	-0.001	-0.009	0.003	-0.009	-0.002	-0.016	0.0002	-0.008	-0.019***	-0.034***	-0.020***	-0.032***
	(-0.11)	(-1.40)	(0.50)	(-1.44)	(-0.26)	(-1.47)	(0.03)	(-0.82)	(-3.72)	(-4.88)	(-3.80)	(-5.78)
Constant	0.484***	0.455***	0.510***	0.463***					-0.002	0.023	0.091**	0.066
	(8.56)	(7.93)	(8.80)	(8.07)					-0.07	(0.60)	(2.51)	(1.64)
Observations	605	605	605	605	498	498	498	498	605	605	605	605
Countries	106	106	106	106	98	98	98	98	106	106	106	106
R^2	0.188	0.228	0.195	0.236								
F-test (*p*-values)	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
F-test FE (*p*-values)	0.000	0.000	0.000	0.000								
Hansen (*p*-value) [instruments]					0.380 [60]	0.278 [75]	0.623 [90]	0.52 [105]	0.323 [78]	0.256 [98]	0.78 [118]	0.98 [138]
AR(1) (*p*-value)					0.004	0.001	0.003	0.001	0.002	0.001	0.001	0.001
AR(2) (*p*-value)					0.442	0.433	0.505	0.483	0.387	0.575	0.556	0.589

Dependent variable is the annual growth rate of income *per capita*. *t*-Student robust to heteroskedasticity in parentheses. *** : significant at 1% ; ** : significant at 5% ; * significant at 10%.

Table 12 summarizes the results of tables 10 and 11. The cost of failing state is calculated only for the specifications where dummies for failing states and for neighbours are both significant. The overall cost of a failing state lies between $85bn and $120bn. The extreme value of $120bn can be discounted because it comes from a regression, estimated by system-GMM, in which the dummy for neighbours has a bigger coefficient than that of the dummy for the failing state itself. This result only arises because the dummies are entered separately. When the dummies are entered simultaneously (column 8), the total cost of a failing state is $102bn, which is close to our initial estimate. We will use this figure in our subsequent analysis of interventions.

Table 12 – *Spillovers and costs arising from a Failing State : Summary table*.

	OLS[1]	OLS[2]	FE[1]	FE[2]	DIFF-GMM[1]	DIFF-GMM[2]	SYS-GMM[1]	SYS-GMM[2]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LICUS at peace	-0.025***	-0.024***	-0.011*	-0.012**	0.004	-0.015	-0.027***	-0.029***
Cost of being a Failing State [3]	489.9	475.2					525.0	545.9
Neighbour of Failing State at peace	-0.016***	-0.017***	0.012	0.015	0.012	0.023	-0.029***	-0.021*
Cost of having a FS as neighbour [3]	347.2	367.8					541.6	433.2
Total cost of a FS (billion $)	85.2	87.8					119.5	102.6

(1) coefficients from regressions where dummies for LICUS at peace and neighbouring countries of LICUS at peace are entered separately.
(2) coefficients from regressions where dummies for LICUS at peace and neighbouring countries of LICUS at peace are entered simultaneously.
(3) in percentage of initial GDP.

Evaluating Interventions

Having established an estimated payoff to a sustained turnaround, we now combine this benefit with the enhanced probability of its occurrence achieved by various interventions.

1. Extra Aid Prior to Reform

We first consider a small increase in aid (excluding technical assistance) delivered prior to an incipient turnaround. On average, failing states have received aid other than technical assistance equal to 6.8 percent of their GPD. We therefore simulate the effect of increasing this by one percentage point to 7.8% and maintaining this increase for a period of five years before reverting to initial levels. 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 it to raise the annual probability of achieving a sustained turnaround from 1.7% per year to 2.38% per year. 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 2.38% for the first five years of our long-term horizon. The cost of starting out as a failing state now falls from $102.6bn to $93.3bn. This difference of $3.3bn 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. Note that again we will 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.[7] 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.

2. An Expansion in Secondary Education prior to Reform

We next simulate the effect of increasing the proportion of the population with secondary education from the mean found in failing states, 2.186%, to 3.186% and then maintained at this higher level. This would raise the probability of reform from 1.7% per year to 2.37% per year. The expected cost of starting as a failing state now falls from $102.6bn to $90.6bn. Hence, the pay-off of expanded secondary education is $12bn. This would have to be compared against the costs of such an expansion, but 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.

3. A technical assistance package early in incipient reform

We next simulate the effect of a technical assistance package, set at the apparently optimal level of 5% of GDP, for each of the first four years of an incipient reform. We compare this with an absence of such technical assistance in this period, with all other

[7] A quadratic relationship between aid and the probability of turnaround has been explicitely tested in the regressions but it proved to be not significant (as for technical assistance).

characteristics being set at the mean for our sample. To keep the calculation as straightforward as possible, we assume that at the end of this four-year period, if the reform has not progressed to success there is a relapse, so that in subsequent years the changes of turnaround revert to the underlying level. Since we wish to target the assistance by time period, we use the results from Table 8. Recall that technical assistance is significant in all four of the variants in that table. We use regression (3) since it has the lowest coefficient on technical assistance, giving greater confidence that this is a conservative estimate. The pay-off to this package is considerable because cumulatively over the four years it raises the chances of turnaround by around 15 percentage points, which is worth around $16bn. The cost of the package is just under $1bn.

4. A Financial Aid Package later during reform

Our final intervention is a package of financial aid delivered in the second four-year period of the reform. To maintain comparability, we again consider a package worth 5% of GDP maintained for each of the four years, and evaluate it against a scenario in which there is no financial assistance during this period. We again use regression (3) of Table 8. All other characteristics are maintained at the mean for our sample. The cost of the package is again just under $1bn. The effect of the package is to raise the cumulative probability of turnaround during the second period by 2.3 percentage points. The expected payoff is thus around $2.3bn.

The cost-benefit analysis of these four interventions is summarized in Table 13, together with some robustness checks. The overall implication is that each of these interventions appears to be highly cost-effective. Appropriately designed aid strategies are thus useful in tackling the problem of failing states. As an approximation, prior to any sign of reform aid should concentrate on pre-conditions such as education. Once reform has started, there is initially a need for large technical assistance packages – much larger than have typically been provided. After a few years this should be supplemented by financial aid. These implications, and their consequences for the organization and evaluation of aid programs, are considered more fully in a companion paper (Chauvet and Collier, 2005).

Table 13 – *Cost-benefit analysis: robustness checks.*

	OLS Reg (2) and (4)		OLS Reg (7)		System-GMM Reg. (9) and (10)		System-GMM Reg. (11)	
	Failing State	Neighb.	FS	Neighb.	FS	Neighb.	FS	Neighb.
Growth loss	-0.025	-0.016	-0.024	-0.017	-0.027	-0.029	-0.029	-0.021
Initial loss from FS ($\hat{p}$ = 0.017)	85.2		87.8		119.5		102.6	
Loss after a permanent rise in education ($\hat{p}$ = 0.0237)	74.8		77.2		105.9		90.6	
Δ cost	10.4		10.6		13.6		12	
Loss after a 5 years rise in aid ($\hat{p}$ = 0.0238)	82.4		84.9		115.7		99.3	
Δ cost	2.8		2.9		3.8		3.3	
Loss when TA=0% during years 1 to 4 of an incipient turnaround, which is not sustained afterwards : $\hat{p}$ = 0.000023 years 1 to 4 $\hat{p}$ = 0.017 afterwards	90.9		93.5		128.3		109.6	
Loss when TA=5% during years 1 to 4 of an incipient turnaround, which is not sustained afterwards : $\hat{p}$ = 0.041 years 1 to 4 $\hat{p}$ = 0.017 afterwards	77.3		79.5		109.1		93.2	
Δ cost	13.6		14		19.2		16.4	
Loss when ODA=0% during years 5 to 8 of an incipient turnaround, all variables being at the mean during years 1 to 4: $\hat{p}$ = 0.00011 years 1 to 4 $\hat{p}$ = 0.0024 years 5 to 8 $\hat{p}$ = 0.017 afterwards	95.6		98.3		134.7		115.2	
Loss when ODA=5% during years 5 to 8 of an incipient turnaround, all variables being at the mean during years 1 to 4: $\hat{p}$ = 0.00011 years 1 to 4 $\hat{p}$ = 0.0083 years 5 to 8 $\hat{p}$ = 0.017 afterwards	93.7		96.3		132.1		112.9	
Δ cost	1.9		2		2.6		2.3	

All numbers are in billion dollars.

5. Conclusion

In this paper we have proposed an economic definition of a failing state: a low-income country with very weak economic policies, institutions and governance. We have found such conditions to be highly persistent: with mean characteristics, the mathematical expectation of the time until a decisive change is well-established is nearly 60 years. We have estimated the cost that such a newly failing state will eventually inflict upon its citizens and its neighbors at a present value of around $100bn, much of this arising from spillovers onto neighbors and future generations. This estimate, though large, is consistent with the current popular recognition that failing states are a serious global concern.

Although failing states around concern, there is a degree of pessimism as to what can be done about them. The high costs and controversy surrounding US military intervention in Iraq and Afghanistan raise doubts as to the future viability of military solutions. A recent political literature is flirting with the revival of international trusteeship, or colonialism mark 2. Aid tends to be dismissed as being not up to the task, or even as part of the problem. In part, this reflects current thinking that aid is most effective where policies and institutions are already reasonable, while past attempts to improve policies through donor conditionality are judged to have failed. However, aid can be effective in promoting policy turnarounds through mechanisms other than conditionality. Financial aid may strengthen the preconditions for reform, and technical assistance may substitute for missing capacities during reform. These effects are likely to be more important in the unusual circumstances of failing states, where the capacity for reform is extremely limited.

We find that aid is cost-effective both in inducing and assisting policy turnarounds in failing states. However, aid effectiveness depends upon the composition and the timing of the assistance. Technical assistance has no discernable effect prior to reform, but then becomes highly cost-effective in accelerating an incipient reform in its early years. Financial assistance strengthens the preconditions for reform, but then becomes counter-productive in the first years of incipient reform. The credibility of this disturbing result is somewhat strengthened by analogous results for favorable terms of trade shocks: such windfalls reduce the chances that incipient reforms progress, and increase the chances that they collapse altogether. However, later in the reform period financial assistance probably becomes useful again.

Although these aid interventions are cost-effective, they are high risk. The increasing emphasis upon judging aid by results – 'results orientation'- is leading in practice to bureaucratic conservatism in which interventions with a high risk of failure are avoided. An example is the new Millennium Challenge Account of the US government which is directing all its resources to those developing countries that are the antithesis of failing states. Aid for failing states would have a high return overall, but it could only be correctly evaluated were interventions assessed in aggregate analogous to a venture capital fund.

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Appendix 1 – *Countries in the sample for growth regressions, 1974-2001.*

Countries	Years in the sample	Years as FS	FS at least 1 year	FS at war	Neighbour of a FS
Algeria	28	0			1
Angola	8	8	1	1	1
Argentina	28	0			
Azerbaijan	4	0			
Bangladesh	28	16	1		1
Barbados	20	0			
Belize	20	0			
Benin	28	20	1		1
Bolivia	28	0			
Botswana	24	0			
Brazil	28	0			
Bulgaria	8	0			
Burkina Faso	28	20	1		1
Burundi	28	20	1	1	1
Cambodia	4	4	1		
Cameroon	28	8	1		1
Cap Verde	24	0			
Central African Rep.	24	24	1		1
Chad	28	28	1	1	1
Chile	28	0			
China	24	0			1
Colombia	28	0			
Comoros	24	24	1		
Congo, Dem. Rep.	24	24	1	1	1
Congo, Rep	28	28	1	1	1
Costa Rica	28	0			1
Côte d'Ivoire	28	16	1		1
Cyprus	20	0			
Czech Republic	8	0			
Dominican Rep.	28	0			1
Ecuador	28	0			
Egypt	28	24	1		1
El Salvador	28	0			1
Ethiopia	28	24	1	1	1
Fiji	24	0			
Gabon	28	0			1
Gambia	28	12	1		1
Georgia	4	0			
Ghana	28	12	1		1
Grenada	24	0			
Guatemala	28	0			
Guinea	28	16	1		1
Guinea-Bissau	24	24	1		1
Guyana	24	20	1		
Haiti	24	24	1		
Honduras	28	20	1		
Hungary	20	0			
India	28	0			1
Indonesia	28	12	1	1	1
Iran	8	0			
Jamaica	28	0			
Jordania	28	0			
Kenya	28	0			1
Lebanon	8	0			
Lesotho	28	20	1		
Madagascar	28	28	1		
Malawi	28	0			1
Malaysia	28	0			1
Mali	28	16	1		1
Mauritania	24	24	1		1
Mauritius	28	0			
Mexico	28	0			
Moldova	4	0			
Morocco	28	0			
Mozambique	20	20	1	1	1
Namibia	4	0			
Nepal	28	28	1		
Nicaragua	28	20	1	1	1
Niger	28	28	1		1
Nigeria	28	28	1	1	1
Pakistan	28	20	1	1	
Panama	28	0			
Papua N^{w} Guinea	24	24	1		1
Paraguay	28	0			
Peru	28	0			
Philippines	28	0			
Poland	16	0			
Portugal	20	0			
Romania	28	0			
Russia	8	0			
Rwanda	28	20	1	1	1
Senegal	28	28	1		1
Seychelles	24	0			
Sierra Leone	20	20	1	1	1
Singapore	8	0			
South Africa	8	0			1
Sri Lanka	28	16	1	1	
St. Kitts & Nevis	20	0			
St. Lucia	20	0			
St. Vincent Gren.	20	0			
Swaziland	4	0			1
Syria	20	0			
Tajikistan	4	4	1		
Tanzania	28	24	1		1
Thailand	28	0			1
Togo	28	24	1		1
Trinidad & Tob.	24	0			
Tunisia	28	0			
Turkey	28	0			
Uganda	24	12	1	1	1
Uruguay	28	0			
Venezuela	16	0			
Vietnam	8	0			1
Yemen	8	8	1	1	
Zambia	28	20	1		1
Zimbabwe	24	24	1	1	1
			45	17	47

Appendix 2 – *Calculation of the total cost of being a Failing State*

g : annual growth rate
r : discount rate

If a country starts out as a failing state from an initial GDP of 1, its GDP at the end of year one will be $[1 – (1 – g)]$. If this country has a turnaround at the end of year one, then it will recorver its initial level of GDP during the second year, meanwhile losing again $(1 – g)$ of its initial GDP.

↳ loss of GDP if a turnaround occurs at the end of year 1 :

$$t = 1 : C(1) = \frac{1-(1-g)}{r} + \frac{1-(1-g)}{r^2} = \frac{g}{r} + \frac{g}{r^2} = g\left(\frac{1}{r} + \frac{1}{r^2}\right)$$

If, instead of having a turnaround in year one, the country has a turnaround in year 2, then it will lose $[1 – (1 – g)]$ in year one and $[1 – (1 – g)^2]$ in year 2. If the turnaround occurs at the end of year two, the country will start to recover, but meanwhile continues to lose $[1 – (1 – g)^2]$ in year 3 and $[1 – (1 – g)]$ in year 4.

↳ loss of GDP if a turnaround occurs at the end of year 2 :

$$t = 2 : C(2) = \frac{1-(1-g)}{r} + \frac{1-(1-g)^2}{r^2} + \frac{1-(1-g)^2}{r^3} + \frac{1-(1-g)}{r^4}$$

$$= \frac{g}{r} + \frac{1-(1-g)^2}{r^2} + \frac{1-(1-g)^2}{r^3} + \frac{g}{r^4}$$

$$= g\left(\frac{1}{r} + \frac{1}{r^4}\right) + \left(\frac{1}{r^2} + \frac{1}{r^3}\right) - (1-g)^2\left(\frac{1}{r^2} + \frac{1}{r^3}\right)$$

The same reasoning applies for year 3 :

↳ loss of GDP if a turnaround occurs at the end of year 3 :

$$t = 3 : C(3) = \frac{g}{r} + \frac{1-(1-g)^2}{r^2} + \frac{1-(1-g)^3}{r^3} + \frac{1-(1-g)^3}{r^4} + \frac{1-(1-g)^2}{r^5} + \frac{g}{r^6}$$

$$= g\left(\frac{1}{r} + \frac{1}{r^6}\right) + \left(\frac{1}{r^2} + \frac{1}{r^3} + \frac{1}{r^4} + \frac{1}{r^5}\right) - (1-g)^2\left(\frac{1}{r^2} + \frac{1}{r^5}\right) - (1-g)^3\left(\frac{1}{r^3} + \frac{1}{r^4}\right)$$

…….

We end up with the following general formula :

↳ loss of GDP if a turnaround occurs at the end of year *t* :

$$C(t) = g\left(\frac{1}{r} + \frac{1}{r^{2t}}\right) + \sum_{k=2}^{k=2t-1} \frac{1}{r^k} - \sum_{k=2}^{k=t} (1-g)^k \left(\frac{1}{r^k} + \frac{1}{r^{2t-k+1}}\right)$$

So taking into account the probability that each of these scenarios will occur, we end up with the following total cost from being a failing state :

Total loss from being a failing state = $\sum_t p(t).C(t)$

where $p(t)$ is the probability of turnaround in year *t*. It is calculated the following way :

X : date of exit from the failing state category (random variable)

$P(X = 1) = a$
$P(X = 2) = (1 - a)\, a$
$P(X = 3) = (1 - a)^2\, a$
…
$P(X=t) = p(t) = (1 - a)^{t-1}\, a$

Appendix 3 – ***Standard errors of dummies for failing states and for neighbouring countries, in levels and in differences.***

Standard errors	Level	Difference
Failing state	0.481	0.262
Failing state at war	0.246	0.221
Failing state at peace	0.458	0.297
Neighbour	0.371	0.123
Neighbour of FS at war	0.169	0.115
Neighbour of FS at peace	0.329	0.135

Appendix 4 – *Definition of the Country Policy and Institutional Assessment (CPIA)*.

A. Macroeconomic management and sustainability of reforms
   1. General macroeconomic performance
   2. Fiscal policy
   3. Management of external debt
   4. Macroeconomic management capacity
   5. Sustainability of structural reforms

B. Structural policies for sustainable and equitable growth
   1. Trade policy
   2. Foreign exchange regime
   3. Financial stability and depth
   4. Banking sector efficiency and resource mobilization
   5. Property rights and rule-based governance
   6. Competitive environment for the private sector
   7. Factor and product markets
   8. Environmental policies and regulations

C. Policies for social inclusion
   1. Poverty monitoring and analysis
   2. Pro-poor targeting and programs
   3. Safety nets

D. Public sector management
   1. Quality of budget and public investment process
   2. Efficiency and equity of revenue mobilization
   3. Efficiency and equity of public expenditures
   4. Accountability of the public service

Each of the twenty components of the CPIA is rated on a scale of 1–6.

Appendix 5 – *Instrumentation regressions, annual data, 1973-1999.*

OLS estimations	(ODA-TA)/GNI		TA/GNI	
Post-conflict years 1 to 4	-0.288	(-0.32)	-0.522**	(-1.97)
Secondary education	-0.675***	(-5.09)	-0.200***	(-4.45)
Ln population	-1.585***	(-5.64)	-0.745***	(-10.49)
Ln income *p.c.*, *t*-1	42.850***	(4.07)	6.448**	(1.96)
Ln income *p.c.* squared, *t*-1	-3.007***	(-4.12)	-0.433*	(-1.89)
Same language as UK	-3.299	(-1.26)	0.062	(0.08)
Same language as France	-1.581	(-0.77)	0.948**	(2.28)
Same religion as UK	6.497**	(2.39)	-0.825	(-0.92)
Same religion as France	1.722*	(1.67)	0.477*	(1.72)
Distance from Washington	0.00003	(0.15)	0.0002***	(3.85)
Distance from Tokyo	-0.001***	(-4.31)	-0.00002	(-0.40)
Distance from Brussels	-0.001*	(-1.67)	0.0001	(0.51)
Total ODA budget of France	0.003**	(2.10)	0.001	(1.29)
Total ODA budget of Germany	-0.0002	(-0.89)	-0.0001*	(-1.67)
Total ODA budget of Japan	-0.001	(-1.54)	0.001**	(2.43)
Total ODA budget of USA	0.0001	(0.34)	0.0003**	(2.39)
Total ODA budget of UK	-0.007**	(-2.35)	-0.001	(-0.80)
ODA budg. of France x Same religion as France	0.0001	(0.32)	-0.00005	(-0.38)
ODA budg. of France x Same language as France	-0.0003	(-0.45)	-0.00004	(-0.20)
ODA budg. of UK x Same religion as UK	-0.006**	(-2.00)	-0.0002	(-0.14)
ODA budg. of UK x Same language as UK	0.002	(0.63)	0.0002	(0.16)
ODA budg. of USA x Same religion as USA	-0.001	(-1.47)	-0.0004**	(-2.05)
ODA budg. of USA x Same language as USA	0.0004	(0.72)	0.0003	(1.46)
ODA budg. of USA x Distance from Washington	0.000000001	(0.03)	-0.00000002	(-1.47)
ODA budget of Japan x Distance from Tokyo	0.0000001	(1.49)	-0.00000005**	(-2.18)
ODA budget of UK x Distance from Brussels	0.000001**	(2.15)	0.00000005	(0.30)
ODA budget of France x Distance from Brussels	-0.0000001	(-0.74)	-0.00000003	(-0.86)
Constant	-119.34***	(-3.13)	-17.92	(-1.50)
Observations	412		412	
Wald test (*p*-value)	0.000		0.000	
R^2	0.409		0.552	

OLS estimations. Dependent variables are technical assistance and other aid. t-Student robust to heteroskedasticity in brackets. *** : significant at 1% ; ** : significant at 5% ; * significant at 10%.

Data and variables

Technical assistance and other aid

Aid is defined as net disbursements of official development assistance, minus technical assistance. ODA disbursements and technical assistance data is from OECD, and was divided by the Gross National Product (GNI) from World Development Indicators of the World Bank (2001).

Instruments for aid

Same language as donor i : dummy taking the value of one if the donor country and the recipient country share a common language [from Collier, Hoeffler and Pattillo (2004), source : CIA factbook (2003)].

Same religion as donor i : dummy variable taking the value of one if 30 percent or more of the population belong to one religious group in the donor as well as in the recipient country [from Collier, Hoeffler and Pattillo (2004), source : Barrett (1982)].

Distance from capitals : it is measured as the inverse of the distance in kilometres between the capitals of the recipients and Washington D.C., Tokyo and Brussels [from Collier, Hoeffler and Pattillo (2004), source : data made available by the World Bank]

Total aid budget of donor i : total net disbursements of ODA by donors *i*, in constant prices 2001 (OECD).

CPIA

Country Policy and Institutional Assessment (World Bank). It has 20 equally weighted components, divided into four categories : (1) Macroeconomic management and sustainability of reforms ; (2) Structural policies for sustainable and equitable growth ; (3) Policies for social inclusion ; (4) Public sector management.

Income per capita

Real gross domestic product *per capita* ($ in 1996 constant prices), Penn World Tables 6.1.

Post-conflict years 1 to 4

Dummy variable equals to one in years one to four after the ending year of a civil war.

Secondary education

Barro R. and J.W. Lee (2000) dataset. Percentage of the population who completed secondary education (population aged 25 and above).

Population

World Development Indicators of the World Bank (2001).

Democracy
Polity IV score for democracy. Ranges from 0 to 10 (0 = low; 10 = high). Measures the general openness of political institutions. The 11-point Democracy scale is constructed additively. The operational indicator is derived from coding of different authority characteristics.

Political rights
Freedom House score for political rights (1 to 7) Higher score implies higher political rights.

Institutions
ICRG score for the quality of institutions. It is a simple sum of the 5 following components. Risk of repudiation of contracts by government (0-10): 10 = high quality institutions. Risk of expropriation (0-10): 10 = high quality institutions. Corruption (0-6): 6 = high quality institutions. Rule of Law (0-6): 6 = high quality institutions. Bureaucratic quality (0-6): 6 = high quality institutions

Years spent in office
Years the national leader had been in office. '0' indicates transition year. Source : State Failure Task Force (2002) and Bienen and van de Walle ("Of Time and Power : Leadership Duration in the Modern World", Center of International Studies, Princeton University).

Higher education
Barro R. and J.W. Lee (2000) dataset. Percentage of the population who completed higher education (population aged 25 and above).

Primary education
Barro R. and J.W. Lee (2000) dataset. Percentage of the population who completed primary education (population aged 25 and above).

Dummy positive price shocks
From Paul Collier and Jan Dehn (2001). "Aid, Shocks and Growth". World Bank Working Paper, Washington DC.