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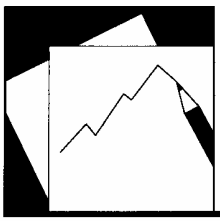

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Aid and Growth: What Does the Cross-Country Evidence Really Show?

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Aid and Growth: What Does the Cross-Country Evidence Really Show?

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Abstract

We examine the effects of aid on growth-- in cross-sectional and panel data--after correcting for the bias that aid typically goes to poorer countries, or to countries after poor performance. Even after this correction, we find little robust evidence of a positive (or negative) relationship between aid inflows into a country and its economic growth. We also find no evidence that aid works better in better policy or geographical environments, or that certain forms of aid work better than others. Our findings, which relate to the past, do not imply that aid cannot be beneficial in the future. But they do suggest that for aid to be effective in the future, the aid apparatus will have to be rethought. Our findings raise the question: what aspects of aid offset what ought to be the indisputable growth enhancing effects of resource transfers? Thus, our findings support efforts under way at national and international levels to understand and improve aid effectiveness.

JEL Classification Numbers: 01; 040

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I. INTRODUCTION

One of the most enduring and important questions in economics is whether foreign aid helps countries grow. There is a moral imperative to this question: it is a travesty for so many countries to remain poor if a relatively small transfer of resources from rich countries could set them on the path to growth. In fact, in the Millennium Declaration adopted in 2000, world leaders state, "We will spare no effort to free our fellow men, women and children from the abject and dehumanizing conditions of extreme poverty, to which more than a billion of them are currently subjected" and they resolve "to grant more generous development assistance, especially to countries that are genuinely making an effort to apply their resources to poverty reduction." As a result, the effort is on to mobilize billions of dollars of aid to help poor countries, especially those with good policies and institutions.

Yet, the question of whether aid helps poor countries grow in a sustained way is still mired in controversy. In this paper, we will re-examine (yet again!) *whether* aid leads to growth.[2] Motivated by the finding in this paper that there is little evidence of a robust impact of aid on growth, Rajan and Subramanian (2005) examine *why* it might be so difficult to find a significant positive impact. In other words, it searches for factors that might thwart aid from having a positive impact on long-run growth.

What does this paper add to the voluminous literature on aid effectiveness? Essentially two things. First, as is well recognized, aid flows are influenced by a country's situation. Aid may go to those countries in the midst of a natural disaster – which would explain a negative correlation between aid and growth. It may also go those who have used it well in the past – implying, if growth is persistent, there will be a positive correlation between aid and growth. Since neither of these relationships is causal, it is important to isolate the exogenous component of aid. While a number of prior studies have attempted to "instrument" aid, we believe, for reasons explained below, that our methodology adds some value, despite limitations, which we discuss below.

Second, the cross-country aid-growth literature typically focuses on one aspect of the relationship. Burnside and Dollar (2000), for example looked at the impact of aid conditional on policy. Hansen and Tarp (2001) examine the relationship in a panel framework, and most recently with a focus on aid's impact conditional on geography. Recently, Clemens et. al. (2004) disaggregate aid into so-called short- and long-impact aid. A second contribution of this paper is to test the general validity of the aid-growth relationship. That is, we test, under

[2] We do not refer here to the voluminous literature on aid effectiveness, which is very nicely surveyed in Clemens et. al. (2004). Some key papers, in addition to those cited below, include, Alesina and Weder (2000), Bauer (1971), Burnside and Dollar (2000), Collier and Dollar (2002), Dalgaard, Hansen, and Tarp (2004), Friedman (1958), Hansen and Tarp (2000), Roodman (2004), Svensson (2003), and World Bank (1998). Our reading of this literature, and hence the rationale for this paper, is that the existing evidence is mixed.

one framework, the robustness of the relationship across time horizons (medium and long run) and periods (1960s through 1990s), sources of aid (multilateral and bilateral), types of aid (economic, social, food, etc.), timing of impact of aid (short-term versus long-term), specifications (cross-section and panel), and samples.

Thus, despite lying squarely in the tradition of cross-country growth regressions with all its well-known shortcomings (see Rodrik, 2005), our objective is to lay out in a transparent and structured manner the different ways of looking at the aid-growth relationship so that particular claims about it can be evaluated. In some ways, therefore, this paper is an attempt at encompassing, or rather generalizing, past work on aid and growth. It seeks to answer the question, "even though the cross-country regression framework may be flawed, what does it *really* tell us about the impact of aid on growth?"

We would like to stress at the outset what the paper is not about. The literature has sometimes followed a cycle in which one paper finds a result, and is followed by another paper with a twist, either overturning or qualifying the previous result, followed by another, and so on. This has had some undesirable effects on policy with advocates selectively using results to bolster their preferred view on aid. Our aim is not to target any particular result or paper. Rather, our approach is to say that if one were starting de novo to examine the aid-growth relationship and attempting to do it in a comprehensive and transparent manner, based on a reasonable (but by no means perfect) specification and mindful of the pitfalls, what would one find.

Our findings are relatively easy to report. We find little evidence of a robust positive impact of aid on growth, and this despite the fact that our instrumentation strategy corrects the bias of conventional (ordinary least squares) estimation procedures against finding a positive impact of aid. To be more concrete, in the cross-sectional analysis, we find some evidence for a negative relationship in the long run (40 year horizon), though this is not significant and does not survive instrumentation. We find some evidence of a positive relationship for the period 1980-2000, but only when outliers are included. We find virtually no evidence that aid works better in better policy or institutional or geographical environments, or that certain kinds of aid work better than others.

In the panel context, we unearth both positive and negative significant relationships between aid and growth, but these are very fragile, and small changes in model specification are enough to dilute the relationships.

One explanation may simply be that the effects that even the theory would predict are too small to detect against the background noise, at least using the standard cross-sectional technique. Certainly, a simple theoretical model suggests that the predicted positive effects of aid inflows on growth are likely to be smaller than suggested by advocates, even if inflows are utilized well. If noise in the data plague all findings, then strong claims about aid effectiveness based on cross-country evidence are unwarranted, and aid policies that rely on such claims should be re-examined.

However, the effects of other interventions (such as good policies) on growth are indeed discernible in the data and are robust. If noise in the data is not the entire explanation for the lack of a robust finding, the interesting question then is not "whether" but "why?" That is, what is it that offsets the transfers and subsidized credit inherent in aid and prevents it from having a robust positive effect on growth? Further research of this kind is essential to improve aid effectiveness. This is the focus of our companion paper, in which we have to move beyond the cross-country framework.

This paper is structured as follows. In Section I, we spell out in detail our strategy for constructing plausibly exogenous instruments for aid, which we use in the subsequent analyses. In Section II, we use these instruments to revisit the question of aid-effectiveness in a cross-sectional framework. In Section III, we examine the key issues in a panel context, using GMM estimation methods. Section IV compares the magnitude of the aid coefficients derived from theory with those obtained in the empirical literature. Section V offers some brief concluding remarks.

II. TACKLING ENDOGENEITY: AN INSTRUMENTATION STRATEGY

Before we go on to a discussion of our results, we make a few observations on methodology. It is well known that cross-sectional regressions have serious limitations. Apart from concerns about endogeneity, outliers, model uncertainty, and measurement error, a key drawback is the problem of unobservable heterogeneity or the omitted variables problem. In cross-country regressions, we can never be sure whether we are controlling for all possible ways in which countries might differ. Panel estimations have the virtue of addressing, albeit partially, the problem of unobservable heterogeneity by incorporating country fixed effects, which means that we essentially ask whether changes in aid over time for a country contemporaneously affect its growth (see Hansen and Tarp, 2000).

In this paper, we present results based on pure cross-sectional and panel estimations. Despite the enthusiasm in the aid literature for regressions explaining growth over the short run (e.g. three years), we believe that if estimations without country fixed effects are to be done at all, the appropriate horizon is long.[3] From a development perspective, we are interested in whether aid takes a country to its ultimate steady state potential (or to a higher steady state if it improves the country's potential growth) faster. Clearly, as we examine longer horizons, we will incorporate spillover effects and effects that take time to emerge. Since some of the

[3] Short-run growth regressions suffer from the problem of extra "noise" induced by cyclical, demand-related, factors (see Kraay, 2004). See Hauk and Wacziarg (2004) who argue, based on Monte Carlo simulations, that taking account of all the advantages and limitations of the different estimation procedures, the pure cross-section OLS estimator that averages data over long-periods might be the least inefficient.

criticisms of aid are precisely about these effects, it is hard to see how we can escape examining the long run. No doubt one could debate what "long run" means, which is why we examine different horizons for the cross-sectional regressions.

We also report results of panel estimations using the Arellano-Bond and Blundell-Blond estimators, which address the potential endogeneity of the regressors, and incorporate (implicitly) fixed effects. In order to have enough observations to estimate panel regressions, however, we will have to bow to fashion and examine five year growth horizons. Cross-sectional and panel estimations each have their virtues and downsides. Ultimately, this does not seem to affect our key finding that there is no discernible robust impact of aid on growth, positive or negative.

We present in Table 1 the basic descriptive statistics for the data we use in our analysis. For our cross-sectional specifications, we report results for the following five time periods: 1960-2000; 1960-80; 1970-2000; 1980-2000; and 1990-2000. We therefore cover the long run (40 and 30 year horizons) as well as the medium term (10 and 20 year-horizons). The differing time periods will also give a sense of changes in aid effectiveness over time.

We begin by reporting the simple OLS regressions in Table 2; that is, in these regressions we do not take into account the endogeneity of aid. In Table 2, columns (1-5), we report the results of running the standard cross-sectional growth regressions over the different time horizons described above. The dependent variable in all cases is the average annual growth rate of per capita GDP of a country over the period. The explanatory variable of interest is the average ratio of annual external aid to GDP over that period to that country.

The controls we include are the usual ones in the literature and they are: initial income; initial life expectancy; a measure of institutional quality (due to Hall and Jones (1999) and used in Bosworth and Collins (2003)); a variable indicating quality of policies (from Wacziarg and Welch (2003), which essentially updates the well-known Sachs and Warner (1995) measure); a measure of geography (which is a combination of frost days and area in the tropics again due to Bosworth and Collins (2004)); external shocks (average growth and the variability of a country's terms of trade); government consumption; and revolutions (from Banks (2004)). As is well known, there is a plethora of potential regressors to choose from.[4] We are guided by two recent papers: Bosworth and Collins, 2004; and Doppelhoffer et. al. 2004. The former selects regressors based on a comprehensive survey of the cross-country growth literature, while the latter identify the set of potential regressors based on a Bayesian approach.[5] In all

[4] In order to minimize endogeneity associated with the regressors in the cross-section regressions, the values of the potentially endogenous covariates are for the beginning of the relevant five-year time period, while exogenous variables (such as terms of trade changes) are averages over the relevant time horizon.

[5] Our choice of regressors (covariates) is not comprehensive, but short of running a few million regressions à la Sala-i-Martin (1997), few choices will be. But for the purposes of this paper, all we need is a set of plausible, even if restricted, set of regressors, that explain a

(continued…)

our cross-section specifications, we drop outliers, which we identify, for each time horizon, according to the Hadi (1992) procedure. Easterly (2004) makes the case that many of the cross-country results are driven by outlying observations, which argues in favor of dropping outliers.

The results are quite clear. In four out of the five cases, the estimate of the aid coefficient is negative, with the only significant estimate being the one for the longest period 1960-2000. The magnitude in this case suggests that an increase in aid of 1 percentage point of GDP would *lower* long-run growth by about 0.07 percentage points per year (the coefficients reported in the table should be divided by 100 to obtain the percentage point increase in annual growth).

One cannot take these estimates too seriously because of the problem of endogeneity. If donors are motivated by suffering in the recipient country, the lower the growth (and the more the suffering), the greater the desire to give aid to alleviate it. Thus there might be a negative correlation between aid and growth but this does not reflect causation from aid to growth. Conversely, if donors are motivated to give to successful recipients, one might see a positive correlation between aid and growth, and this again would not reflect causation from aid to growth.

The previous literature has recognized this problem (see, for example, Clemens et al. (2004), Easterly (2003), Easterly et al. (2004), Hansen and Tarp (2002), and Tavares (2003)) but we believe our instrumentation strategy is a useful contribution to the attempts to resolve it.

Most of the papers in the literature use the instruments from Hansen and Tarp (2002). These are: dummy for Egypt, arms imports (t-1), policy (t-1), policy-squared (t-1), policy*ln population, policy *initial GDP per capita, policy, initial gdp per capita-squared, policy*aid (t-1), policy*aid-squared (t-1), aid-squared (t-1). Clearly, variables like policy, even if they are lagged, are not really exogenous to growth.[6]

We construct instruments for aid that are more likely to be exogenous and satisfy the exclusion restrictions. We exploit the fact that aid is often extended for non-economic reasons. Our main identification assumption is that non-economically-motivated aid is unaffected by economic outcomes. This notion is far from new. A number of papers have used this to explain aid flows (Alesina and Dollar, 2001; and Barro and Lee, 2004). But we

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substantial variation in the data because we are interested in seeing whether the aid-growth relationship is robust in such a standard specification.

[6] Assessing the validity of the instrumentation strategy in many of these papers is rendered difficult by the fact that first-stage results are seldom reported, nor are the exclusion restrictions discussed. In Roodman's (2004) excellent testing of the robustness of the recent results, endogeneity and instrumentation issues are not addressed.

are not aware of papers that have taken the obvious next step of exploiting it to systematically develop instruments for aid which could be used in aid-growth analyses.

We derive our aid instruments along the lines of Frankel and Romer (1999). Our basic model is as follows. Once a donor d decides on a total quantum of aid, it allocates it to a recipient r using the following equation:

$$\begin{aligned}\theta_{drt} &= \beta' Y_{drt} + \upsilon_{drt} \\ &= \beta_0 + \beta_1 STRAT_{drt} + \beta_2 USISEG_{drt} + \beta_3 COMCOL_{dr} + \beta_4 COMCOLUKdr + \beta_5 COMCOLFRA_{dr} \\ &+ \beta_6 COMCOLSPA_{dr} + \beta_7 COMCOLPOR_{dr} + \beta_8 CURCOL_{drt} + \beta_9 COMLANG_{dr} + \upsilon_{drt}\end{aligned} \qquad \text{--(1)}$$

where θ_{drt} is the share of donor country d's aid allocated to recipient r in year t, and Y is the vector of explanatory variables that capture different (non-economic) aspects of donor-recipient relationships.[7] The variables include: STRAT takes on a value of 1 if the donor and recipient are common members of, or signatories to, an Entente or Alliance in any given time period;[8] USISEG takes on a value of 1 for US-Egypt and US-Israel observations after the Camp David agreement; COMCOL a value of one if the recipient was ever a colony of the donor, COMCOLUK, COMCOLFRA, COMCOLSPA, and COMCOLPOR refer in turn to colonial relationships involving respectively the U.K. France, Spain and Portugal); CURCOL a value of one if there is a contemporaneous colonial relationship between donor and recipient; and COMLANG is a dummy that takes a value of one if the donor and recipient share a common language. A key identifying assumption is that none of the right hand side variables directly relate to growth in the recipient country. The data to estimate these equations are discussed in Appendix 1.[9]

The predicted share $\hat{\theta}_{drt} = \beta' Y_{drt}$ (where Y are the regressors in matrix notation) is then used to calculate the (instrumented) aid to GDP ratio received by country r in year t as follows:

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[7] In order to estimate equation 1, we need to compute the share of a country's total (i.e. bilateral and multilateral) aid that goes to any particular recipient. To do this, we obtain a decomposition of multilateral aid into its underlying bilateral constituents. The OECD DAC database contains a series called "imputed" bilateral aid, which does precisely this.

[8] In the Correlates of War database from which these data are obtained, there are 4 types of alliances: a common alliance; a defense alliance; a neutrality or non-aggression alliance; and an entente alliance. We use the last as it seems the most consistent with the economic relationships we are interested in.

[9] So, our construction of instruments starts from the bilateral (donor-recipient) relationship and aggregates up. This is in contrast to the literature that pick instruments directly at the level of the recipient country.

$$\hat{A}_{rt} = \frac{\sum_{d} GDP_{dt} A_{dt} . \hat{\theta}_{drt}}{GDP_{rt}} \tag{2}$$

where GDP_{dt} is the GDP of the donor country d in dollars in year t and A_{dt} is the Aid to GDP ratio for that donor country in that year. $\hat{A}_{rt}$ averaged over the relevant period will be the instrument we use in much of the paper for aid.

In Table 3, we present estimates for the model represented by equation 1.[10] Virtually all the instrumenting variables are significant for all the time horizons, and between them the variables account for a reasonable share (between 20 and 23 percent) of the variation in the donor allocation decision.

How much information about aid is contained in our instrument? While the simple correlation between actual and fitted aid is good, this may be due to the fact that other variables such as GDP per capita could be driving the correlation, given the well-known bias of aid going to poorer countries. In Chart 1, we depict the first-stage relationship between actual and fitted aid. The first stage controls for all the variables that are used in explaining growth (in the second-stage), including per capita GDP. The chart shows that even after controlling for a number of relevant covariates, the relationship between actual and fitted aid is strong, with a coefficient of about 0.44 and a t-statistic that is greater than 5. Our instrument appears to contain a lot of, hopefully exogenous, information about actual aid.

While the strong positive correlation between actual aid and our instrument is encouraging, it does not validate our instrumentation strategy, which can be questioned on a number of counts. In Appendix 2, we elaborate on these and spell out how we address them in the paper. No instrumentation strategy is perfect because of inherent difficulties in instrumentation, but we believe that our strategy works reasonably well. We also check the robustness of our instrumentation by considering alternatives in the aid-growth regressions (see below).

III. Aid and Growth: Revisiting The Cross-Section Evidence

Armed with our instruments, in this section we revisit the cross-country evidence with two aims. First, we examine whether instrumenting for aid affects the results on aid

[10] Throughout the paper, instruments vary according to the time horizon of the analysis. For example, in growth regressions for 1960-2000, we estimate equations 1 and 2 for the period 1960-2000; for 1970-2000, the equations are re-estimated for the period 1970-2000; and so on.

effectiveness. Second, we explore if the aid-growth relationship varies across time horizons and periods, sources of aid, types of aid, episodes of growth, and specifications.[11]

A. The Basic IV Results

We now present estimates for the cross-sections presented earlier in Table 2, with the difference that we instrument for aid using $\hat{A}_{rt}$.[12] In Table 4A, we present the core instrumental variable (IV) specification, which is representative of the results we obtain more broadly. A substantial fraction of the variation in growth is explained by our core specifications, with R-squares typically being greater in the longer horizons (73 and 70 percent, respectively in the 1960-2000 and 1970-2000 horizons). The coefficient on the aid term is negative in three out of five cases, and significant in none. When we retain outliers in the sample, the coefficient on aid is significant and positive in the 1980-00 horizon (available from the authors upon request): more specifically, just two observations—Cape Verde and Guinea-Bissau—are enough to increase the aid coefficient five-fold and render it statistically significant, underlining the fragility of the result.[13]

We would note that compared with the OLS regressions reported in Table 2, our IV results consistently (except for the 1990-00 horizon) tend to make the impact of aid less negative or more positive (compare the aid coefficients columns 1-4 in Table 2 with those in Table 4A). In other words, the IV strategy tempers the tendency of the OLS to magnify the negative impact of aid. The magnitudes of the coefficient are all quite small, suggesting that aid has a very small effect—positive or negative—on growth.

If donors give aid to countries that are doing well (i.e. growing faster), the OLS estimate would be biased upwards (that is, it would be reflected in a more positive coefficient on aid). The "true" (IV) estimate would correct for this bias and hence result in a coefficient that is lower than the OLS coefficient. However, our IV estimates are consistently greater than the OLS estimates, suggesting that our instrumentation is correcting for a negative endogeneity

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[11] Our sample comprises all developing countries which have received aid during the post-war period and for which data are available (see Appendix 3). We do not have any sample selection biases due to countries dropping out of the sample in later time periods because they have graduated from aid-recipient status.

[12] In all the IV estimations, we checked to see if the standard errors are affected by the fact that the instruments are estimated. The standard error correction we used to check this was the same as in Frankel and Romer (1999). The results were virtually unaffected by this correction, so we report the uncorrected standard errors in all the tables.

[13] But the fact that significant results can be obtained shows that a possible problem with the IV procedure, namely its low power to reject null hypotheses, is minimized.

bias, resulting from the tendency of donors, on average, to give aid to countries that are faring poorly.[14]

Could our results have occurred because our instruments are invalid or "weak."? Table 4B, which presents the first-stage equations corresponding to each of the four cross-section specifications, sheds evidence on this. In every equation, the coefficient on the instrument is significant at the 1 percent significance level with high F-values that exceed, in four out of five cases, the weak instrumentation threshold of ten suggested by Staiger and Stock (1997).[15]

In what follows, we subject our results to a number of other robustness checks.[16] In all cases, we find the aid coefficient to be statistically insignificant and small in magnitude.

B. Robustness to Instrumentation

Our instruments are subject to a number of concerns that we try and address in Appendix 2. But we can check for their robustness by trying alternative ways of addressing endogeneity. The simplest, and one that has been used extensively, is to use initial or lagged values of aid instead of instrumenting for contemporaneous values. Table 5 shows that using initial values of aid does not change the core result of small and insignificant effects of aid on growth.

We construct an alternative instrument, which exploits an additional source of variation in aid, namely, the exogenous variation in the donor decision on the aggregate amount of aid to give. This instrument is described in Appendix 2. Appendix Table 1 presents the results when using this alternative instrument. Comparing Appendix Table 1 with Table 4A, we see that the aid coefficients are remarkably similar.

C. Categories of Aid

In Table 6, we try to distinguish the impact of different types of aid in the spirit of Clemens et. al. (2004). We disaggregate aid in three different ways: by sector (social, economic, and

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[14] There could be a possible downward bias in the aid coefficient because aid-to-GDP ratios are dominated by movements in GDP, the denominator. To address this possible bias, we measured the ratio as average aid divided by the initial period level of GDP. In this variant too, the aid coefficient was always insignificant (available from the authors).

[15] We do not report the first-stage regressions in the subsequent cross-section results, but they are consistently similar to those reported in Table 4B, with fitted aid always being positive and statistically significant, typically at the 1 percent confidence level.

[16] Our results are robust to the inclusion of inflation and budget balance as covariates (available from the authors upon request).

food);[17] by timing of impact (short and long impact);[18] and by type of donor (multilateral and bilateral). The results are shown in three panels in Table 6. Various arguments can be made as to why some categories but not others should affect long-run growth. For example, food aid should typically not be expected to affect long-run growth while economic and social sector aid should because they lead to an increase in physical and human capital. Clemens et. al (2004) argue that the effect of short-impact aid will be easier to detect in the data than long-impact aid. Similarly, one can make the argument that multilateral aid is less explicitly "political" than bilateral aid and should therefore have a different impact. The argument for a possible differential impact between multilateral and bilateral aid could also relate to the type of aid given or to the nature and effectiveness of conditionality. What we find, however, is that no sub-categories have any significant impact—positive or negative--on growth.[19]

D. Non-Linear and Conditional Effects

In columns (1-5) of Table 7, we ask whether there are diminishing returns to aid. To test this, we include a squared aid term in the regression. This has the effect of rendering all the aid terms positive (but insignificant), and the squared terms themselves have negative signs, suggesting that there might be diminishing returns to aid. However, in no case is either the level or squared aid term significant.

Burnside and Dollar (2000) and Collier and Dollar (2003) suggest that aid, even if it does not unconditionally help growth, is helpful in those countries that have good policies and institutions. In columns 6-10, we ask whether aid is more effective in better policy environments. To answer this, we interact aid with two measures of policy: the Sachs-Warner measure updated by Warcziarg and Welch (2003) which is reported in the table, and the World Bank's CPIA ratings which is reported in Appendix 2. In both cases, we find that the

[17] The categories come from the OECD's DEC database that provide data on ODA commitments by purpose (CRS). Social sector aid includes education, health and population, and water supply and sanitation; economic aid includes energy and transport and communications; and multi-sector includes support for projects which straddle several sectors. We do not report results for multi-sector aid as they are very similar to those for the other sectors.

[18] We need to instrument for short impact and long impact aid. In the Clemens et. al. (2004) categorization, the former contains many of the economic aid categories and the latter the social aid categories. So in our specifications that use the Clemens et. al. (2004) variables, we instrument for short impact aid with our instrument for economic aid and for long impact aid with the instrument for social aid. The first-stage results (available from the authors upon request) show that these instruments work well.

[19] We stress here that we have not replicated the Clemens et. al. (2004) results using their specification and covariates. We have used their measure of short-impact aid in our framework and find little evidence of any impact.

coefficient on the aid-policy interaction terms is never positive and significant.[20] The underlying policy variable is significant in most cases, while the coefficient of the interaction term is not stable, turning negative in some of the regressions.

E. Effectiveness Conditional on Geography

Recently, Dalgaard et. al. (2004) have argued that aid's effectiveness depends on geographic location. In fact, Roodman (2004) concludes after testing the robustness of a number of prior results on aid effectiveness that "if there is one strong conclusion from this literature, it is that on average aid works well outside the tropics but not in them." Though there are plausible stories for why growth may be higher outside the tropics, the rationale for the effectiveness of aid outside the tropics (or its ineffectiveness within) is unclear – suggesting that this result is simply a way of separating countries where aid has worked from countries where it has not, rather than an explanation. The underlying policy conclusion is also a little bleak because aid and aid effectiveness are especially important inside the tropics, where most of the poorest countries are situated.

But does aid conditional on geography show up in the cross-section? In Table 8, in addition to the standard covariates, we introduced a term, interacting aid with a measure of geography (due to Bosworth and Collins, 2003)). While geography itself usually has a significant impact on growth, the aid-geography coefficient is significant in only one of the five time horizons.

IV. Aid and Growth: Revisiting The Panel Evidence

In this section, we revisit the aid-growth evidence based on panel estimations. Much of the literature with the exception of Hansen and Tarp (2002) and Dalgaard, Hansen and Tarp (2004) employs either OLS or 2-stage least squares estimations procedures without fixed effects.

An alternative, and arguably superior approach, that addresses the potential endogeneity of the other regressors and also incorporates fixed effects is to use panel GMM regressions. These come in two flavors. There is the difference-GMM estimator due to Arellano and Bond (AB)(1991) and the system-GMM estimator due to Blundell and Bond (BB) (1998). In both, identification relies on first-differencing and using lagged values of the endogenous variables as instruments.[21]

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[20] We obtained similar results when the aid-policy and aid-square terms were included simultaneously. In Appendix Table 1, we check whether the results change when the World Bank's CPIA ratings are substituted for the Sachs-Warner policy measure, but find no significant difference.

[21] In the AB estimator, lagged levels are used to instrument for the differenced right hand side variables, whereas in the BB estimator the estimated system comprises the difference

(continued…)

There is a problem though. GMM estimation confers on the researcher considerably more degrees of freedom than simple OLS estimation: for example, the freedom to specify the variables whose lags will be used as instruments; the variables whose contemporaneous values will be used as instruments; how many lags to be used for instrumentation etc. This freedom means that claims of robustness need to be evaluated with special care to ensure that results are not driven by the manner in which this freedom is exercised.

We consider three recent findings: that certain types of aid (short-impact/economic) aid have a positive impact on growth (Clemens et. al. 2004); that aid given to countries with good policies have a positive impact on growth (Burnside and Dollar (2000); and that aid is effective outside the tropics (Dalgaard, Hansen and Tarp, 2004, and Roodman, 2004).

We show simply and transparently (by detailing the choices made on estimation procedure) that these findings are fragile. Of course, there is equal fragility to findings of a negative impact of aid on growth.

In Table 9, we introduce three different measures of aid—total aid, short-impact aid, and economic aid and estimate them using the system and difference GMM estimator. [22] Total aid is negative and significant in one variant (column 2), short-impact aid is also negative and significant in the difference-GMM variant (column 4) and economic aid is positive and significant in both variants.[23] Apart from the difficulty of understanding these divergent results, there is the question of whether they are robust.

It turns out that both the positive and negative results are fragile. The results on the positive effect of economic aid in columns 5 and 6 break down with just one change: if they are re-estimated but with only three lags of the predetermined variables used as instruments, the coefficient is no longer significant (columns 7 and 8), and in one case the (column 7), the magnitude drops by three-quarters, showing how fragile the result is. Similarly, the negative effect of short-term aid in column 4 breaks down if government consumption is excluded as a regressor (column 9).

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equation instrumented with lagged levels as in the AB estimator as well as the level equation, which is estimated using lagged differences as instruments. Each estimator has its limitations. The AB estimator often leads to a weak instruments problem because lagged levels are typically not highly correlated with their differenced counterparts. On the other hand, the BB estimator generates large upward biases in the right-hand side variables (see Hauk and Wacziarg, 2004).

[22] We include the covariates from the cross-section. The list is quite close to that in Dalgaard et. al. (2004) with one difference that we use a measure a time-invariant measure of institutional quality, in part on the grounds that institutions tend to have long-run effects. The budget balance variable was never significant, so we dropped it from the analysis.

[23] These results remain unchanged even when the variable for revolutions is treated as endogenous.

In Table 10, we introduce the aid-square variable, to see if there are diminishing or increasing returns to aid (we do this for the economic aid variable as it appears to have the best shot as showing up as having a positive impact). The squared term is insignificant (column 1).[24] In columns 2 and 3, we check whether the aid-policy interaction term is significant. Again, we chose the aid variable that had the best chance of conforming to the existing results. This interaction term is indeed positive and significant for the short-impact aid term (column 2), appearing to confirm the Burnside and Dollar (2000) result. When this specification is estimated in the difference GMM variant, however, the conditional aid term loses significance (column 3). Indeed, if we estimate the specification in column 3, replacing short-impact aid with economic aid, and using three lags for instrumentation, the aid-policy interaction term is nearly significant, but with the "wrong" (negative) sign, signifying that economic aid works better in worse policy environments (column 4).

We then test the Dalgaard et. al. (2004) result on the impact of aid conditional on geography. For this we chose the same geography variable—fraction of land area in the tropics—as theirs. We indeed find in one specification that the aid-geography interaction term is significant, with the coefficient suggesting that aid is more effective outside the tropics (column 5). To obtain this result, we had to express inflation simply as the inflation rate, unlike in the other specifications (and in most of the literature), where it is expressed as log(1+ inflation rate). However, like before, just one change in the specification—using the difference GMM estimator (column 6) or using the normal specification of the inflation variable (column 7) renders the aid-geography interaction term insignificant.

What should we conclude from these GMM results? That economic aid has a robust positive impact, that short-impact aid has a negative one, and that overall aid can be negative for growth? That short-impact aid works better in better policy and institutional environments while economic aid works better in worse environments and that sometimes aid is more effective in more advanced temperate zone countries? Or simply that these results are fragile?[25] Note that the fragility is not simply a result of the degrees of freedom conferred by the GMM estimation procedure because some of the other covariates—the policy variable for example—can be robust across specifications. The degrees of freedom essentially mean that claims to robustness need to be examined carefully. In our view, all that one can conclude is that it is difficult to discern robust effects of aid in the data, either positive or negative.

[24] While the level term is positive and significant, it is as fragile to small changes as noted in the previous paragraph.

[25] Cordella et. al. (2005) obtain similar results on the impact of aid using GMM estimation methods.

V. QUANTITATIVE IMPACT OF AID: THEORY AND EMPIRICS

What should one expect? Suppose the primary channel through which aid worked was by increasing public investment.[26] What then would be the quantitative impact on growth? A theoretical estimate of this impact can be obtained from a simple growth model. This model yields the conclusion that, even under the most optimistic assumptions about the use of aid (optimistic in the sense that all aid is invested and none of it is wasted or consumed), the impact of aid should be positive but relatively small in magnitude.

Specifically, the relationship between aid inflows and growth is captured by the following equation (see Appendix 4 for details):

$$\frac{\delta\gamma_y}{\delta(\frac{Aid}{Y})} = \alpha\beta\frac{Y}{K}$$

where γ_y is the rate of growth of output per worker, Aid/Y is the ratio of aid to GDP, α is capital share in income, β is the fraction of aid that is invested, and Y/K is the output capital ratio (Y/K). Assuming that all aid is invested (β =1), and using a value of capital share in income of 0.35 computed by Bosworth and Collins (2003), and a value of 0.45 for the output-capital ratio for the average developing country in our regression sample, the magnitude of the regression coefficient suggested by theory is 0.16; that is, a 1 percentage point increase in the ratio of aid to GDP should at most raise the *long-run growth* rate by 0.16 percent, even on the most optimistic assumption that all aid is usefully invested. More realistically, if half of all aid is wasted or consumed, the coefficient value should be close to 0.1.

How does this theoretical estimate compare with the magnitudes in the empirical literature? This comparison also serves as a robustness check on the results of this paper as well as others in the literature. Sampling a few of the more influential papers that find a positive impact of aid on growth, the pattern that one discerns is that OLS and two-stage least squares estimations yield lower magnitudes than GMM estimations, with the exception of Clemens et. al. (2004). The latter find a coefficient of gross aid on growth of about 0.9. The GMM estimations of Hansen and Tarp (2000) and Dalgaard et. al. (2004) yield a coefficient on net aid that varies between 0.6 and 1.3. The theory suggests that these values are high, about 6-10 times greater than what the theory predicts. In the estimation reported in this paper, many of the coefficients on aid are negatively signed, but when they are positively signed (as in Table 4A, columns 2 and 4, and Table 10, columns 5-7), the magnitudes are in the range of about 0.01 and 0.2 (recall the coefficient estimate has to be divided by 100 to get the effects of a percentage point increase in aid to GDP on annual growth) , which are much closer to

[26] Of course, aid by financing schooling and increasing human capital accumulation could also lead to total factor productivity growth. We discuss this channel below.

what theory would predict.[27] It is worth noting here that the coefficients on aid should be close to those on investment. Barro and Martin (1995, Chapter 12), in summarizing the cross-section growth estimates, suggest that a plausible coefficient on the investment to GDP ratio is about 0.03, that is a 1 percentage point increase in the ratio of investment to GDP should increase per capita growth rate by 0.03 percent, even less than the theoretical estimate of 0.1 that we derive.

The only way one can reconcile the theory (and the estimates on investment from the cross-country growth literature) with the high estimates reported by Clemens et. al. (2004) and Dalgaard et. al. (2004) is if aid has a substantial impact on total factor productivity growth (for example via human capital accumulation or through the promotion of better policies): specifically, a one percentage point increase in the aid to GDP ratio should increase annual average TFP growth to the extent of between 0.5 percent and 1 percent. In other words, for the empirical estimates in these papers to be plausible, it would have to be the case that in the post-war period (encompassing the cold-war period), an additional 1 percentage point of aid to GDP had an impact on productivity growth in developing countries roughly equivalent to that of the IT-revolution in the United States in the1990s.

VI. Concluding Remarks

This paper had a simple objective: to present in one place and using one framework results on the different aspects of the aid-growth relationship and to do so both in cross-section and panel contexts.

Our central conclusion is there is no robust positive relationship between aid and growth in the cross-section, and this despite the fact that our instrumenting strategy corrects for the bias in conventional (ordinary least squares) estimation procedures of finding a negative impact of aid on growth. This conclusion holds across:

- time horizons;
- time periods;
- types of aid distinguished by:
    - what they are used for (economic, social, food, etc.);
    - who gives it (multilateral donors, bilateral donors etc.);
    - who it is given to (those with good policies and institutions and others);
    - who it is given to (those in the tropics and outside); and
    - how long it takes to impact (short and long impact).

[27] The high coefficient values on aid in GMM regressions suggest that these regressions should be viewed with some degree of caution.

One mildly interesting finding is that the results (whatever their sign) are reasonably uniform across different sub-categories of aid suggesting a high degree of fungibility. Economic, social and food aid seem to have similar effects on growth, as do bilateral and multilateral aid. Evidently, it is not easy to compartmentalize aid because governments seem to view all forms of aid as going to a common pot and act accordingly.

Our central conclusion from our GMM estimations of the panel data is that one can find significant impacts of aid on growth—positive and negative—but these do not hold up when we check their robustness to the considerable choice of specifications at the researcher's disposal. In short, it is difficult to discern any systematic effect of aid on growth in the panel context also.

One implication may simply be that the entire enterprise of running cross-country growth regressions may be plagued by noise in the data, which makes it hard to establish any relationship even if they actually exist. This possibility is strengthened by a simple theoretical exercise, which suggests that the effects of aid on growth are likely to be positive but much smaller than suggested by previous studies. If noise in the data plague all findings, then strong claims about aid effectiveness (or equally, on aid ineffectiveness) based on cross-country evidence are unwarranted, and aid policies that rely on such claims should be re-examined.

If noise is not the entire explanation (there are robust findings in the cross-country growth literature, such as the importance of institutions and policies for growth), one has to ask what aspects of aid offset what must be the indisputable growth enhancing effects of resource transfers. We then have to move away from the traditional cross-sectional analysis, and focus on more direct evidence of the channels through which aid might help or hinder growth. Such further research is essential to improve aid effectiveness. We attempt some answers in Rajan and Subramanian (2005).

In sum, there are two important implications of our findings. First, it should be stressed that our findings, which relate to the past, do not imply that aid cannot be beneficial in the future. But they do suggest that for aid to be effective in the future, the aid apparatus (in terms of how aid should be delivered, to whom, in what form, and under what conditions) will have to be rethought. Second, our findings force us to ask what aspects of aid offset what ought to be the indisputable growth enhancing effects of resource transfers. Understanding the hindrances is essential to any effort to making aid more effective. Thus, our findings support efforts under way at national and international levels to improve aid effectiveness.

Table 1: Summary Statistics

Variable	*Mean*	*Std. Dev.*	*Min*	*Max*
	1960-00 ***(77 Obs.)***			
Real annual average per capita GDP growth (PPP)	1.435	1.756	-3.373432	6.794
Aid to GDP	0.069	0.081	0.000872	0.423
Fitted aid to GDP	0.095	0.171	0.000523	0.927
Initial level of per capita (PPP) GDP	7.375	0.696	5.944191	8.967
Initial level of life expectancy at birth	47.443	9.664	32.380000	71.680
Institutional quality	0.525	0.119	0.225000	0.859
Geography	-0.599	0.724	-1.040000	1.528
Initial level of government consumption to GDP	13.595	9.510	1.376561	65.041
Average no. of revolutions	0.233	0.201	0.000000	0.829
Average level of the terms of trade	113.332	22.922	66.665820	176.213
Standard deviation of the terms of trade	26.237	18.430	1.681126	94.323
Initial level of trade policy	0.013	0.114	0.000000	1.000
	1980-00 ***(83 Obs.)***			
Real annual average per capita GDP growth (PPP)	0.781	2.151	-5.557046	6.273
Aid to GDP	0.063	0.078	0.000485	0.439
Fitted aid to GDP	0.086	0.166	0.000391	1.047
Initial level of per capita (PPP) GDP	7.819	0.822	6.093689	9.347
Initial level of life expectancy at birth	56.349	9.843	35.400000	72.850
Institutional quality	0.526	0.124	0.225000	0.859
Geography	-0.535	0.782	-1.040000	1.528
Initial level of government consumption to GDP	21.293	11.117	3.310046	63.680
Average no. of revolutions	0.252	0.264	0.000000	1.286
Average level of the terms of trade	106.270	18.611	79.873570	171.998
Standard deviation of the terms of trade	18.389	17.250	1.800865	81.759
Initial level of trade policy	0.108	0.313	0.000000	1.000
	Panel ***(581 Obs.)***			
Real annual average per capita GDP growth (PPP)	1.203	3.477	-14.279790	22.934
Aid to GDP	0.063	0.082	0.000113	0.539
Fitted aid to GDP	0.160	0.339	0.000991	3.484
Initial level of per capita (PPP) GDP	7.772	0.819	5.804100	9.794
Initial level of life expectancy at birth	56.141	10.819	32.880000	77.350
Institutional quality	n.a.	n.a.	n.a.	n.a.
Geography	n.a.	n.a.	n.a.	n.a.
Initial level of government consumption to GDP	21.408	10.886	3.061446	73.452
Average no. of revolutions	0.210	0.346	0.000	2.600
Average level of the terms of trade	112.916	35.672	26.452500	353.196
Standard deviation of the terms of trade	11.827	12.090	0.000000	86.830
Initial level of trade policy	0.231	0.422	0.000000	1.000

For description of the variables, see Appendix 1. The summary statistics are presented for the main variables and for three time periods.

Table 2: Impact of Total Aid on Growth, OLS Estimations
(dependent variable is average annual growth rate of per capita GDP)

	(1) **1960_00**	**(2)** **1960_80**	**(3)** **1970_00**	**(4)** **1980_00**	**(5)** **1990_00**
Aid/GDP	-7.276	-5.900	-5.852	-4.159	9.446
	(2.79)***	(1.36)	(1.42)	(0.75)	(0.87)
Initial per cap. GDP	-1.228	-1.159	-1.591	-1.400	-0.576
	(5.47)***	(2.01)**	(5.49)***	(3.37)***	(0.84)
Initial level of life expectancy	0.028	0.054	0.031	0.045	0.067
	(1.71)*	(1.25)	(1.34)	(1.14)	(0.87)
Institutional quality	3.982	7.713	3.542	3.036	3.748
	(2.22)**	(3.02)***	(1.51)	(1.03)	(0.98)
Geography	0.371	-0.154	0.368	0.545	0.459
	(2.86)***	(0.58)	(2.44)**	(3.04)***	(1.26)
Initial level of government consumption	0.000	0.006	-0.014	-0.039	-0.048
	(0.02)	(0.21)	(0.67)	(2.01)**	(1.92)*
Revolutions	-1.321	0.560	-1.402	-0.369	-1.617
	(3.02)***	(0.57)	(3.01)***	(0.61)	(2.18)**
Terms of trade growth	0.016	0.010	0.029	0.008	-0.028
	(1.98)*	(1.62)	(2.54)**	(0.47)	(0.66)
St. deviation of TOT growth	-0.015	-0.000	-0.033	-0.011	-0.121
	(1.48)	(0.01)	(2.37)**	(0.52)	(2.22)**
Initial level of policy (Sachs-Warner)	2.046	1.527	2.278	2.251	0.485
	(3.93)***	(1.49)	(4.10)***	(3.06)***	(0.84)
Observations	74	61	80	81	79
R-square	0.77	0.57	0.72	0.59	0.52
Outliers	Cape Verde Mauritania Thailand	Mauritania Thailand	Cape Verde	Cape Verde Guinea Bissau	Cape Verde Guinea Bissau Mozambique Nicaragua Yemen, Rep.

All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. All regressions exclude outliers, which are identified according to the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. For descriptions of the variables and their sources, see Appendix 1.

Table 3: Estimation of Exogenous Variation in the Allocation of Aid by Donors Across Recipients
(dependent variable is share of donor i's aid to recipient j)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Dummy for common membership in Entente Alliance (Alliance Dummy)	0.017	0.036	0.179	0.137
	(2.34)**	(2.70)***	(5.03)***	(6.23)***
Dummy for Egypt and Israel after Camp David (Egypt Israel Dummy)	0.081	0.104	0.122	0.120
	(8.68)***	(11.16)***	(13.44)***	(12.85)***
Dummy for pairs that ever had a colonial relationship	0.056	0.061	0.055	0.046
	(14.51)***	(15.84)***	(14.87)***	(11.50)***
Dummy for pairs currently in a colonial relationship	0.037	0.012	-0.010	-0.009
	(4.61)***	(1.28)	(0.97)	(0.78)
Dummy for pairs that have common language (Language Dummy)	0.001	0.001	0.001	0.001
	(1.02)	(1.33)	(1.98)**	(1.96)*
Dummy for country that ever had a colonial relationship with UK	-0.057	-0.058	-0.052	-0.043
	(13.42)***	(13.76)***	(12.59)***	(9.75)***
Dummy for country that ever had a colonial relationship with France	-0.047	-0.051	-0.045	-0.035
	(10.24)***	(11.03)***	(9.98)***	(7.37)***
Dummy for country that ever had a colonial relationship with Spain	-0.043	-0.048	-0.043	-0.033
	(8.38)***	(9.38)***	(8.65)***	(6.21)***
Dummy for country that ever had a colonial relationship with Portugal	0.049	0.056	0.062	0.072
	(7.33)***	(8.46)***	(9.73)***	(10.70)***
Observations	3328	3328	3328	3249
R-squared	0.20	0.22	0.23	0.21

Estimation is by ordinary least squares. The estimated equation corresponds to equation 1 in Section II of the paper. All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. For descriptions of the variables and their sources, see Appendix 1.

Table 4A: Impact of Total Aid on Growth, IV Estimations
(dependent variable is average annual growth rate of per capita GDP)

	(1) 1960_00	**(2) 1960_80**	**(3) 1970_00**	**(4) 1980_00**	**(5) 1990_00**
Aid/GDP	-2.551	1.353	-0.829	1.313	-13.514
	(1.08)	(0.34)	(0.19)	(0.22)	(0.83)
Initial per cap. GDP	-1.124	-1.074	-1.472	-1.288	-1.138
	(4.40)***	(1.76)*	(4.60)***	(2.90)***	(1.51)
Initial level of life expectancy	0.038	0.065	0.045	0.061	-0.006
	(2.13)**	(1.41)	(1.88)*	(1.44)	(0.06)
Institutional quality	4.035	7.923	3.354	2.409	5.280
	(2.32)**	(3.15)***	(1.48)	(0.78)	(1.24)
Geography	0.430	-0.180	0.407	0.601	0.315
	(3.26)***	(0.76)	(2.57)**	(3.14)***	(0.88)
Initial level of government consumption	-0.007	-0.015	-0.020	-0.046	-0.031
	(0.39)	(0.43)	(1.01)	(2.30)**	(0.98)
Revolutions	-1.258	1.082	-1.357	-0.344	-1.767
	(2.52)**	(1.02)	(2.74)***	(0.55)	(2.21)**
Terms of trade growth	0.015	0.011	0.030	0.011	-0.042
	(1.79)*	(1.81)*	(2.46)**	(0.59)	(0.99)
St. deviation of TOT growth	-0.014	0.002	-0.033	-0.013	-0.120
	(1.36)	(0.20)	(2.22)**	(0.53)	(1.97)*
Initial level of policy (Sachs-Warner)	1.934	1.549	2.215	2.339	0.764
	(3.48)***	(1.57)	(4.00)***	(3.11)***	(1.30)
Observations	74	61	80	81	79
R-square	0.73	0.55	0.70	0.59	0.52

Table 4B: Impact of Total Aid on Growth, First-Stage Regression, 1960-00
(dependent variable is average aid-to- GDP ratio)

	(1) 1960_00	**(2) 1960_80**	**(3) 1970_00**	**(4) 1980_00**	**(5) 1990_00**
Fitted aid to GDP	0.270	0.176	0.264	0.211	0.164
	(5.01)***	(2.99)***	(5.24)***	(4.15)***	(2.81)***
Observations	74	61	80	81	79
R-square	0.74	0.69	0.78	0.74	0.80
F-value	13.44	8.47	23.62	21.45	23.43

All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid in Table 4A corresponds to equation 2 in Section II of the paper. All regressions exclude outliers, which are identified according to the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates in the first-stage regression are omitted for presentational convenience. For descriptions of the variables and their sources, see Appendix 1.

Table 5: Impact of Total Aid on Growth, OLS Estimations using Lagged Aid
(dependent variable is average annual growth rate of per capita GDP)

	(1) 1960_00	**(2) 1960_80**	**(3) 1970_00**	**(4) 1980_00**	**(5) 1990_00**
Lag Aid/GDP	-0.811	-2.035	2.439	-1.116	3.880
	(0.20)	(0.52)	(0.50)	(0.19)	(0.59)
Initial per cap. GDP	-1.209	-1.159	-1.456	-1.335	-0.562
	(4.24)***	(1.88)*	(4.73)***	(3.05)***	(0.87)
Initial level of life expectancy	0.049	0.049	0.066	0.058	0.046
	(2.08)**	(1.07)	(2.66)***	(1.33)	(0.52)
Institutional quality	4.385	8.091	2.807	3.060	3.840
	(2.50)**	(3.02)***	(1.31)	(0.94)	(0.95)
Geography	0.444	-0.067	0.570	0.749	0.465
	(2.94)***	(0.26)	(3.83)***	(3.95)***	(1.29)
Initial level of government consumption	-0.011	-0.013	-0.028	-0.050	-0.044
	(0.52)	(0.36)	(1.34)	(2.43)**	(1.78)*
Revolutions	-1.123	0.920	-1.299	-0.397	-1.632
	(1.88)*	(0.98)	(2.43)**	(0.66)	(2.17)**
Terms of trade growth	0.014	0.010	0.032	0.009	-0.038
	(1.55)	(1.39)	(2.57)**	(0.46)	(0.89)
St. deviation of TOT growth	-0.012	0.005	-0.034	-0.013	-0.119
	(1.12)	(0.45)	(2.18)**	(0.58)	(2.09)**
Initial level of policy (Sachs-Warner)	1.966	1.219	2.181	2.117	0.441
	(3.09)***	(1.23)	(4.00)***	(2.81)***	(0.78)
Observations	69	59	77	77	78
R-square	0.72	0.55	0.72	0.60	0.52

All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. As an alternative to instrumentation, lagged or initial values of aid are introduced as regressors. For 1960-2000 and 1960-80, the value of aid is for 1960-70; for 1970-2000, and 1980-00, aid refers to the value for 1970-80; and for 1990-00, it is for 1980-90. Outliers are identified using the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. For descriptions of the variables and their sources, see Appendix 1.

Table 6: Impact of Different Categories of Aid on Growth, IV Estimations
(dependent variable is average annual growth rate of per capita GDP)

Panel A: Social, Economic and Food Aid

	Social			Economic			Food		
	(1) 1970_00	(2) 1980_00	(3) 1990_00	(4) 1970_00	(5) 1980_00	(6) 1990_00	(7) 1970_00	(8) 1980_00	(9) 1990_00
Social sector aid/GDP	-4.717	7.664	-84.776						
	(0.19)	(0.20)	(0.78)						
Economic aid/GDP				-16.511	6.030	895.087			
				(0.31)	(0.08)	(0.72)			
Food aid/GDP							-19.015	-5.433	-206.035
							(0.49)	(0.10)	(1.69)*
Observations	80	81	79	80	81	79	80	81	71
R-square	0.70	0.59	0.52	0.69	0.59	0.52	0.69	0.59	0.51

Panel B: Long-Impact and Short-Impact Aid

	Long-term			Short-term		
	(1) 1970_00	(2) 1980_00	(3) 1990_00	(4) 1970_00	(5) 1980_00	(6) 1990_00
Long-impact aid/GDP	-2.2	2.6	-17.9			
	(0.28)	(0.26)	(0.85)			
Short-impact aid/GDP				-0.030	0.018	-0.423
				(0.38)	(0.13)	(0.85)
Observations	69	77	79	69	77	79
R-square	0.72	0.60	0.52	0.72	0.59	0.52

Panel C: Multilateral and Bilateral Aid

	Multilateral Aid					Bilateral Aid				
	(1) 1960_00	(2) 1960_80	(3) 1970_00	(4) 1980_00	(5) 1990_00	(6) 1960_00	(7) 1960_80	(8) 1970_00	(9) 1980_00	(10) 1990_00
Mult. aid/GDP	-7.385	4.510	-3.073	0.893	-20.658					
	(1.13)	(0.27)	(0.29)	(0.09)	(0.94)					
Bilat. aid/GDP						-4.589	5.693	-1.269	2.739	-29.371
						(1.02)	(0.37)	(0.17)	(0.23)	(0.78)
Observations	74	61	80	81	79	74	61	80	81	79
R-square	0.73	0.55	0.70	0.59	0.52	0.73	0.55	0.70	0.59	0.52

All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid corresponds to equation 2 in Section II of the paper. Outliers are identified using the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates are omitted for presentational simplicity. For descriptions of the variables and their sources, see Appendix 1.

Table 7: Aid and Growth: Diminishing Returns and Impact of Policies and Institutions, IV Estimations
(dependent variable is average annual growth rate of per capita GDP)

	Aid-square term					**Aid interacted with Sachs-Warner Policy measure**				
	(1) 1960_00	**(2) 1960_80**	**(3) 1970_00**	**(4) 1980_00**	**(5) 1990_00**	**(6) 1960_00**	**(7) 1960_80**	**(8) 1970_00**	**(9) 1980_00**	**(10) 1990_00**
Aid/GDP	2.885	2.922	25.649	6.774	140.408	-4.093	1.522	1.365	2.089	-13.395
	(0.30)	(0.28)	(1.18)	(0.24)	(1.03)	(1.16)	(0.38)	(0.21)	(0.32)	(0.80)
Aid/GDP-squared	-17.683	-5.788	-95.526	-22.755	-786.286					
	(0.60)	(0.15)	(1.24)	(0.23)	(1.12)					
Policy (Sachs-Warner)	2.001	1.538	2.634	2.375	0.397	1.731	2.031	2.442	0.523	0.774
	(3.17)***	(1.55)	(3.67)***	(3.00)***	(0.48)	(2.70)***	(1.40)	(4.05)***	(0.46)	(0.95)
Aid/GDP*policy						4.591	-21.645	-5.137	101.180	-0.237
						(0.54)	(0.70)	(0.46)	(1.50)	(0.02)
Observations	74	61	80	81	79	74	61	80	81	79
R-square	0.69	0.54	0.71	0.59	0.52	0.73	0.55	0.70	0.61	0.52

All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid corresponds to equation 2 in Section II of the paper. Outliers are identified using the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates are omitted for presentational simplicity. For descriptions of the variables and their sources, see Appendix 1.

Table 8: Aid and Growth: Impact of Geography, IV Estimations
(dependent variable is average annual growth rate of per capita GDP)

	(1) **1960_00**	**(2)** **1960_80**	**(3)** **1970_00**	**(4)** **1980_00**	**(5)** **1990_00**
Aid/GDP	-1.908	3.129	0.877	2.752	-3.753
	(0.81)	(0.84)	(0.22)	(0.40)	(0.21)
Geography	0.406	-0.309	0.358	0.556	0.092
	(2.90)***	(1.17)	(2.15)**	(2.55)**	(0.22)
Aid/GDP*Geography	0.878	2.961	2.295	2.072	13.175
	(0.68)	(1.60)	(1.10)	(0.66)	(1.75)*
Observations	74	61	80	81	79
R-square	0.73	0.56	0.70	0.59	0.53

All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid corresponds to equation 2 in Section II of the paper. Outliers are identified using the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates are omitted for presentational simplicity. For descriptions of the variables and their sources, see Appendix 1.

Table 9: Impact of Different Aid Measures on Growth, GMM Estimations
(dependent variable is average annual growth rate of per capita GDP)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Aid/GDP	-0.231	-3.781							
	(0.12)	(3.97)***							
Short-term aid/GDP			-7.794	-23.937					-7.888
			(1.30)	(2.42)**					(1.06)
Economic Aid/GDP					43.347	44.773	9.746	59.185	
					(2.26)**	(3.21)***	(0.24)	(1.26)	
Log Inflation	-1.742	-1.947	-1.677	-1.810	-1.623	-1.544	-1.744	-1.020	-1.566
	(5.48)***	(3.59)***	(5.04)***	(3.84)***	(5.00)***	(3.02)***	(2.92)***	(1.16)	(4.20)***
Institutional quality	7.152		7.259		4.013		0.017		6.025
	(1.98)*		(1.87)*		(1.06)		(0.00)		(1.31)
Geography	0.409		0.306		0.575		0.878		0.405
	(1.62)		(1.08)		(1.90)*		(2.49)**		(1.34)
Initial per cap. GDP	-0.578	-3.452	-1.631	-6.050	-0.131	-4.319	0.111	-5.552	-1.077
	(0.97)	(4.98)***	(2.14)**	(6.64)***	(0.19)	(4.91)***	(0.15)	(4.37)***	(1.35)
Revolutions	-1.178	-2.282	-1.198	-2.780	-1.513	-2.492	-2.710	-5.269	-0.965
	(1.46)	(2.57)**	(1.49)	(3.20)***	(1.87)*	(2.73)***	(2.41)**	(4.42)***	(0.89)
Policy (Sachs-Warner)	0.564	-1.537	0.771	-1.489	0.658	-1.529	1.409	-0.251	0.850
	(1.11)	(1.61)	(1.29)	(1.33)	(1.18)	(1.37)	(2.40)**	(0.25)	(1.42)
Government consumption/GDP	-0.038	-0.011	-0.026	0.060	-0.058	-0.020	-0.055	-0.045	
	(1.51)	(0.21)	(0.92)	(1.02)	(1.79)*	(0.38)	(1.96)*	(0.61)	
Observations	539	451	417	329	423	332	423	332	417
Number of Groups	88	86	87	83	88	85	88	85	87
Chi-Square (Hansen over-id test)	1.000	1.000	1.000	0.959	1.000	0.936	0.952	0.419	0.866
AR(2) (test for serial correlation)	0.481	0.352	0.254	0.067	0.179	0.121	0.246	0.484	0.288
GMM estimation method	System	Difference	System	Difference	System	Difference	System	Difference	System
Endogenous variables	Initial income,	Initial income,	Initial income,	Initial income,	Initial income,	Initial income,	Initial income,	Initial income,	Initial income,
used as instruments	aid, govt.	aid, govt.	aid, govt.	aid, govt.	aid, govt.	aid, govt.	aid, govt.	aid,	aid,
	cons. , policy,	cons. , policy,	cons. , policy,	cons. , policy,	cons. , policy,	cons. , policy,	cons. , policy,	cons. , policy,	policy,
	inflation	inflation	inflation	inflation	inflation	inflation	inflation	inflation	inflation
No. of lags of endogenous	unrestricted	unrestricted	unrestricted	unrestricted	unrestricted	unrestricted	three	three	unrestricted
varuiables used in instrumentation									
Exogenous variable used	Geography	Geography	Geography	Geography	Geography	Geography	Geography	Geography	Geography
as instrument									

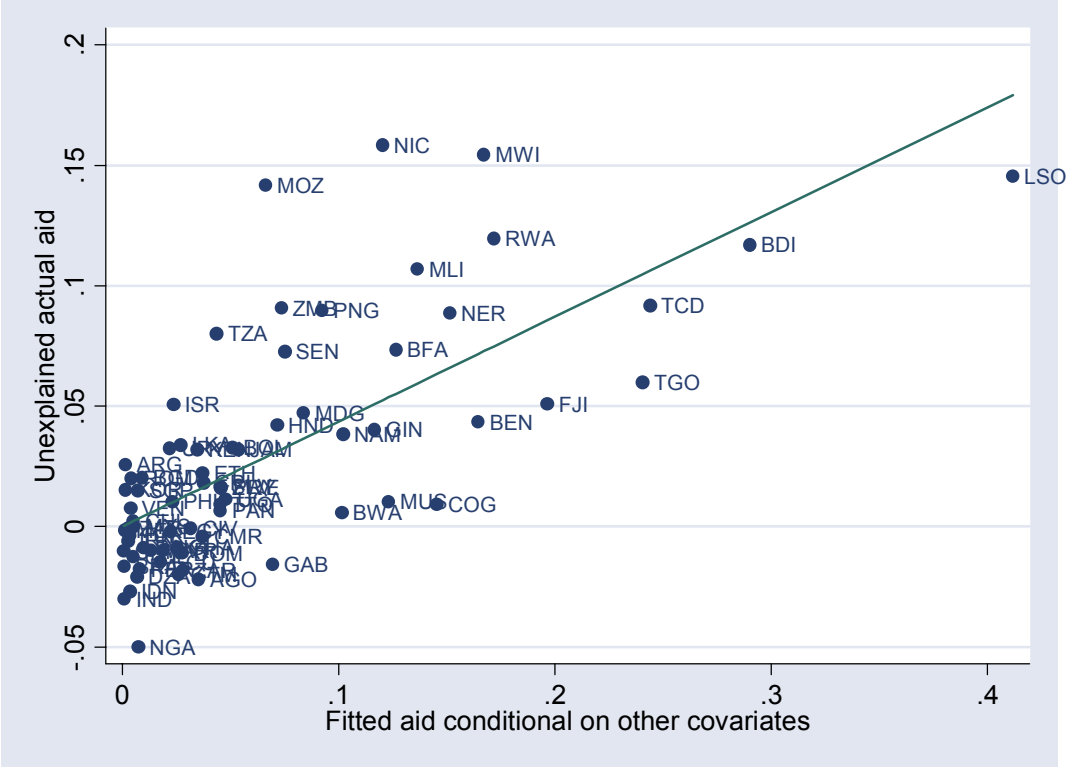
All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. Details of the GMM estimation procedure, with all the choices, are reported in the last rows of the table. For descriptions of the variables and their sources, see Appendix 1.

Table 10: Impact of Aid Conditional on Policies and geography, GMM Estimations
(dependent variable is average annual growth rate of per capita GDP)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid/GDP					11.406	20.179	9.377
					(1.36)	(1.25)	(1.15)
Area in tropics					-0.227		-0.319
					(0.39)		(0.55)
Aid/GDP * area in tropics					-14.137	-24.601	-11.810
					(1.71)*	(1.51)	(1.48)
Economic aid/GDP	70.304			83.924			
	(1.90)*			(1.85)*			
Economic aid/GDP Square	-144.278						
	(0.59)						
Initial level of policy	0.564	0.149	-2.079	0.098	0.634	-0.642	0.612
	(1.08)	(0.22)	(1.57)	(0.09)	(1.32)	(0.69)	(1.25)
Economic aid/Gdp*Policy				-112.193			
				(1.65)			
Short-term aid/GDP		-0.096	-0.226				
		(1.43)	(2.42)**				
Short-term aid/Gdp*Policy		0.146	0.083				
		(1.76)*	(0.93)				
Log Inflation	-1.595	-1.587	-1.878	-1.080			-0.957
	(4.85)***	(4.91)***	(3.76)***	(1.24)			(3.50)***
Inflation (percent)					-0.001	-0.001	
					(2.97)***	(1.76)*	
Institutional quality	3.967	6.533			11.185		11.036
	(0.97)	(1.69)*			(3.14)***		(3.16)***
Geography	0.681	0.313					
	(2.18)**	(1.12)					
Initial per cap. GDP	-0.554	-1.488	-6.062	-6.429	-1.529	-3.138	-1.488
	(0.79)	(2.27)**	(6.59)***	(4.77)***	(2.89)***	(4.83)***	(2.77)***
Revolutions	-1.165	-1.814	-2.759	-4.844	-1.560	-2.243	-1.370
	(1.56)	(2.29)**	(3.15)***	(4.09)***	(2.34)**	(2.65)***	(1.96)*
Initial government consumption	-0.080	-0.026	0.062	-0.054	-0.078	-0.040	-0.071
	(2.31)**	(0.92)	(1.14)	(0.74)	(2.54)**	(1.19)	(2.31)**
Observations	423	417	329	332	512	431	512
Number of Groups	88	87	83	85	81	80	81
Chi-Square (Hansen over-id test)	1.000	1.000	0.999	0.752	1.000	1.000	1.000
AR(2) (test for serial correlation)	0.166	0.243	0.066	0.497	0.621	0.495	0.685
GMM estimation method	System	System	Difference	Difference	System	Difference	System
Endogenous variables used as instruments	Initial income, aid, aid-square, gov. cons., policy, inflation	Initial income, aid, aid*policy gov. cons., policy, inflation	Initial income, aid, aid*policy gov. cons., policy, inflation	Initial income, aid, aid*policy gov. cons., policy, inflation	Initial income, aid, aid*policy gov. cons., policy, inflation	Initial income, aid, aid*tropics gov. cons., policy, inflation	Initial income, aid, aid*tropics gov. cons., policy, inflation
No. of lags of endogenous variables used in instrumentation	unrestricted	unrestricted	unrestricted	three	unrestricted	unrestricted	unrestricted
Exogenous instrumenting variable	Geography	Geography	Geography	Geography	Tropics	Tropics	Tropics

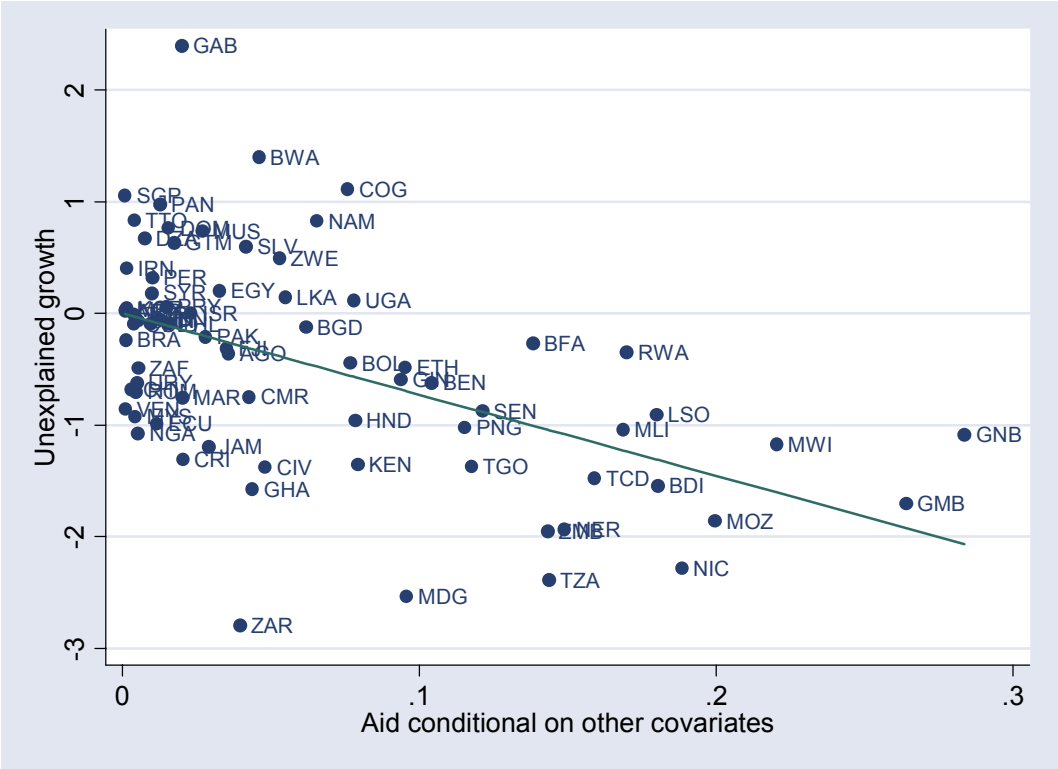
All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. Details of the GMM estimation procedure, with all the choices, are reported in the last rows of the table. For descriptions of the variables and their sources, see Appendix 1.

Chart 1: Conditional Relationship between Aid and its Instrument, 1960-00



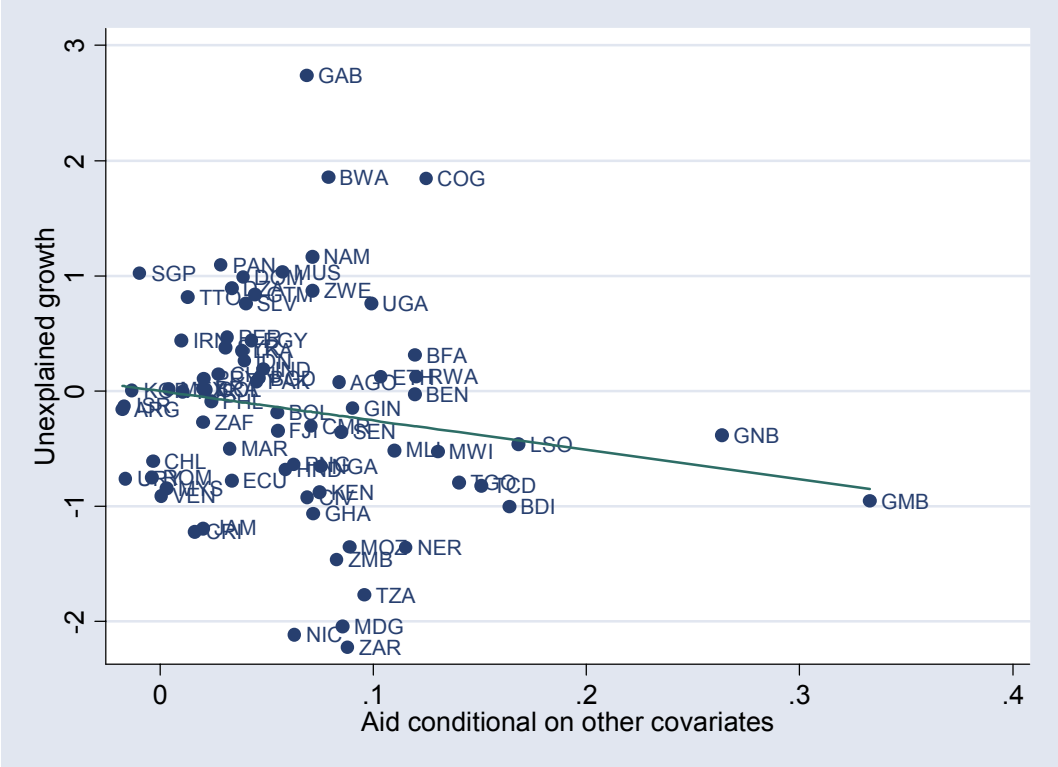
The chart plots the first-stage relationship between actual and the instrument (fitted aid), conditional on all the covariates that enter the second-stage growth regression. The slope of the line is the coefficient on fitted aid in this first-stage regression (also shown in Table 4B). For presentational purposes, it excludes two countries that are included in the sample in Table 4.

Chart 2: Conditional Correlation between Growth and Total Aid, 1960-00
(OLS estimation)



Coefficient=-7.28; t-statistic=2.79
The chart plots the relationship between growth and actual aid, conditional on all the covariates. The slope of the line is the coefficient on aid in the OLS regression in column 1 of Table 2.

Chart 3: Conditional Correlation Between Growth and Total Aid, 1960-00 (IV Estimations)



Coefficient value=-2.55; t-statistic=1.08

The chart plots the relationship between growth and aid, conditional on all the covariates. The slope of the line is the coefficient on aid in the instrumental variable regression in column 1 of Table 4A.

Appendix I. Data Description and Sources

Heston, Alan, Robert Summers and Bettina Aten, *Penn World Table* Version 6.1, Center for International Comparisons at the University of Pennsylvania (CICUP), October 2002.

OECD, DAC (Development Assistance Committee) database on Aid, 2002.

World Bank, 2004. *World Development Indicators*, Washington, D.C.: World Bank

IMF (International Monetary Fund), 2004,. *World Economic Outlook,* Washington, D.C.: IMF.

Bosworth, B., and S. Collins, 2003, "The Empirics of Growth: An Update," mimeo, Brookings Institution, Washington D.C.

Barro and Lee, 1994: Data Set for a Panel of 138 Countries. The data set contains variables for the panel estimation. Data are presented either quinquennially for the years 1960-1985, i.e., 1960, 1965,1970, 1975, 1980, and 1985, or for averages of five years' sub-periods over 1960-1985. Barro, R., Lee, J-W., 1994, *Data Set for a Panel of 138 Countries*. Revised January 1994.

Arthur S. Banks Banks, Arthur S. *CROSS-NATIONAL TIME SERIES, 1815-2002* [Computer file]. Databanks International ed. Binghamton, NY: Databanks International [Producer and Distributor], 2002.

Wacziarg, Romain and Karen Horn Welch (2003) "Trade Liberalization and Growth: New Evidence," Mimeo, Stanford University.

Correlates of War 2: This data set records all formal alliances among states between 1816 and 2000, including mutual defense pacts, non-aggression treaties, and ententes.
Correlates of War Project, 2002, *Correlates of War 2*, University of Michigan

Rose, A.K., "Do we really know that the WTO increases trade?" 2004, *American Economic Review*.

Variable Name	Variable Description	Source
Rgdpchg	Annual growth rate of real GDP per capita	PWT
aid_gdp	The ratio of aid to GDP	OECD, DAC
FaN_gdp	The ratio of fitted aid to GDP based on exploiting exogenous variation in the allocation of aid by donors across recipients	Authors' calculations
bilat_gdp	The ratio of bilateral aid to GDP	OECD, DAC
multi_gdp	The ratio of multilateral aid to GDP	OECD, DAC

social_gdp	The ratio of social sector aid to GDP	OECD, DAC
Fsocial_gdp	The ratio of fitted social sector aid to GDP	Authors' calculations
economic_gdp	The ratio of economic aid to GDP	OECD, DAC
Feconomic_gdp	The ratio of fitted economic aid to GDP	Authors' calculations
food_gdp	The ratio of food aid to GDP	OECD, DAC
Ffood_gdp	The ratio of fitted food aid to GDP	Authors' calculations
yc_penn	Initial period per capita (PPP) GDP	PWT, 6.1
le_wdi	Initial period life expectancy at birth	WDI
gadp6099	Institutional Quality	Bosworth & Collins, 2003
geog6099	Geography	Bosworth & Collins, 2003
cg	Government consumption as share of GDP	PWT, 6.1
cg_i	Initial period government consumption as share of GDP	PWT, 6.1
revol	No. of revolutions	Arthur S. Banks
tot_av	Terms of trade	WDI
tot_stdev	Standard deviation of terms of trade	WDI
sw1	Trade policy	Wacziarg & Welch, 2003
aid_gdpsq	The ratio of aid to GDP squared	Authors' calculations

Tropicar	The fraction of a country's land area in the tropics	Doppelhoffer et. al. (2004)
aid_sw1	The ratio of aid to GDP * Trade policy	Authors' calculations
sw1_i	Initial level of trade policy	Wacziarg & Welch, 2003
aid_sw1_i	The ratio of aid to GDP * Initial trade policy	Authors' calculations
cpia	Policy and institutional rating	World Bank
aid_cpia	The ratio of aid to GDP * cpia	Authors' calculations
cpia_i	Initial level of policy and institutional rating	World Bank
aid_cpia_i	The ratio of aid to GDP * initial policy	Authors' calculations
ggb_gdp	The ratio of donor country's general government balance to GDP	WEO
lurn	Donor country's natural rate of unemployment	WEO
aid_shr	Donor country's aid share to recipient country	Authors' calculations
allidum_3	Dummy that takes a value of 1 if donor and recipient country are part of the same entente alliance	Correlates of War, 2
egy_isr_dum	Dummy that takes a value of 1 if donor is US and recipient is Egypt or Israel	Authors' calculations
Colony	Dummy that takes a value of 1 if donor and recipient country were ever in colonial relationship	Rose, 2004
Curcol	Dummy that takes a value of 1 if donor and recipient country enjoy a current colonial relationship	Rose, 2004

Appendix II. Issues Relating to the Instrumentation Strategy

In this appendix, we discuss the possible concerns with our instrumentation strategy and how we address them.

Exclusion restriction

The first concern with our instruments is that, while they may be correlated with the endogenous aid variable, they may not satisfy the exclusion restriction. That is, strategic variables, colonial relationships, proximity to donors etc., may have an impact on growth over and above their impact through the endogenous variable. For example, proximity (geographical and strategic) to donors might be bad because donors require bad policies or support bad leaders or require greater defence-related spending. Alternatively, proximity to donors might be good because they impose good conditionality. Also, certain colonial relationships may imply a certain quality of current institutions with impacts on growth. In each of these cases, the exclusion restriction might not be satisfied.

In general, it is not easy to ensure that exclusion restrictions are satisfied. As Durlauf et. al. (2004) point out growth theory is so broad and all-encompassing that it is always possible to find a story about why an instrument merits inclusion in the second-stage regression, invalidating instrumentation. Moreover, since we have one instrument for one endogenous regressor, tests of overidentification restrictions cannot be applied. Moreover, notice that we constructed our instruments based on bilateral relationships (i.e. every observation in our basic instrumenting equation is a dyad, a donor-recipient pair), whereas our first and second-stage regressions have recipient countries as the observation. So it is not easy to see how the exclusion restriction can be formally tested.

One heuristic, possibly crude, way of checking our instrumentation strategy, is to see whether

donor-type has an independent effect on growth. For example, we calculated for each recipient the share of aid from the Scandinavian countries (the so-called good donors). The results are presented in Appendix Table 3. In general, introducing this variable did not affect significantly the coefficient on aid. And there was no sign of an independent effect of this variable. It was either insignificant, or "wrongly" signed: the prior that Scandinavian aid is good aid is not confirmed by the data.

On variables such as colonial links, while it is true that one cannot rule out independent links from these variables to growth, which might invalidate the exclusion restriction, it is not often that we see such variables in cross-country growth regressions. For example, in Sala-i-Martin et. al. (2004) none of the colonial links variable, except possibly for links with Spain, is a significant independent determinant of growth.

On the exclusion restriction, our instrumentation strategy needs to be compared with the alternatives in the literature. As described earlier, the latter typically use lagged values of aid and policy variables as instruments. While lagged values can be pre-determined, it is highly

unlikely that they will satisfy the exclusion restriction under the plausible assumption that shocks to growth (the LHS variable) persist over time.

Bias

Another related concern is that our strategic variables are not really exogenous because for example, having a strategic alliance may mean lending to "bad leaders" or dictators, which is often associated with aid during the cold war. The effect of this might be that instrumenting imparts a bias toward finding a negative impact on aid.

First, the distinction needs to be made between the motives for giving aid and the how it is used. It is well-known that the cross-country evidence shows no robust relationship between dictatorship or democracy and long-run growth. Thus, the fact that aid may have been given to dictators should not per se induce any bias. Moreover, it should be noted as an empirical matter, that while aid for strategic reasons was given to Mobuto in Zaire and Marcos in the Philippines, and successive Egyptian regimes, who had a detrimental effect on growth, it was also given to Suharto in Indonesia, Pinochet in Italy, successive military regimes in Korea and successive democratic regimes in Israel, who successfully promoted growth. Furthermore, the fact that multilateral and bilateral aid have such similar effects (Table 6, Panel C) is also suggestive that strategic motives for giving aid do not necessarily map into how it is used.

Second, we would note that our results clearly show that instrumenting has the effect of consistently *increasing* the size of the aid coefficient relative to the OLS estimate (compare the results in Table 2 with those in Table 4 and the discussion in Section III). Thus, instrumentation gives aid a better chance to show up as having a positive impact. The greater IV estimate suggests that it is indeed correcting the negative endogeneity bias, namely the tendency of donors to give aid to countries that are doing poorly in growth terms.

Another way of checking whether strategic aid is bad aid is to see if the strategic variables that we used to explain total aid also help explain "good" aid. Appendix Table 4 reproduces the estimation in Table 3 but this time with bilateral *social* aid as the dependent variable. It is remarkable how similar the two equations: all the variables have the same sign and significance (although the magnitudes of the coefficients vary) and both equations explain a broadly similar share of variation.

In order to diminish any bias from politically-motivated aid, we could try and extract the exogenous variation stemming from more "neutral" factors such as macroeconomic and budgetary conditions in the donor countries. That is, we can estimate the regression

$$A_{dt} = \alpha_0 + \alpha_1 FB_{dt} + \alpha_2 UN_{dt} + \in_{dt} = \alpha' X_{dt} + \in_{dt} \qquad \text{-------(3)}$$

where FB_{dt} is the overall fiscal balance as a share of GDP in donor country d in time period t, and UN_{dt} is the unemployment rate. The idea is that countries are more likely to be forthcoming with aid when their budgetary positions are more favorable, a factor that is

likely to be exogenous to a recipient country's long run growth. The explanatory variables are obtained from the IMF's World Economic Outlook (WEO) database.

We then use the predicted value, $\hat{A}_{dt} = \alpha' X_{dt}$, to estimate the instrument

$$\bar{A}_{rt} = \frac{\sum_{d} GDP_{dt} \hat{A}_{dt} . \hat{\theta}_{drt}}{GDP_{rt}} \qquad \text{-----(4)}$$

We present results using $\bar{A}_{rt}$ as the instrument for aid in Appendix Table 1, which as noted earlier is similar to that in the core cross-section specification (Table 4A). Note that equation 4, exploits both the exogenous variation in the donor decision to allocate across recipients (as in equation 2) as well as the exogenous variation in the donor decision on the aggregate amount of aid to give (represented by equation 3).

It is, of course, true that macroeconomic and budgetary conditions in donor countries could have an independent effect on growth in recipient countries, thereby violating the exclusion restriction. But these are cyclical factors, which would clearly have an impact on the cyclical variation in recipient country growth, but whose impact on *trend* growth in recipient countries, especially when measured over 20, 30, and 40 years (as we do in our cross-section), would be lower.

Other concerns

Another concern with our instrumentation strategy is that it is essentially being driven by (the inverse of) economic size. It is true (as in Frankel and Romer, 1999) that our fitted aid is correlated with country size. But our instrument actually conveys a lot of information additional to that in country size. This is illustrated most clearly in Chart 1 (and Table 4B), which shows the first-stage relationship between actual and fitted aid, after controlling for all the second-stage regressors, including the level of GDP. The coefficient on fitted aid is positive and highly significant.

Appendix III. Sample of Countries							
Country	**1960_00**	**1980_00**	**Panel**	**Country**	**1960_00**	**1980_00**	**Panel**
Albania	no	no	yes	Korea, Rep.	yes	yes	yes
Algeria	yes	yes	yes	Kyrgyz Republic	no	no	yes
Angola	yes	yes	yes	Latvia	no	no	yes
Argentina	yes	yes	yes	Lebanon	no	no	yes
Armenia	no	no	yes	Lesotho	yes	yes	yes
Azerbaijan	no	no	yes	Macedonia, FYR	no	no	yes
Bangladesh	yes	yes	yes	Madagascar	yes	yes	yes
Belize	no	no	yes	Malawi	yes	yes	yes
Benin	yes	yes	yes	Malaysia	yes	yes	yes
Bolivia	yes	yes	yes	Mali	yes	yes	yes
Botswana	yes	yes	yes	Mauritania	yes	yes	yes
Brazil	yes	yes	yes	Mauritius	yes	yes	yes
Bulgaria	no	no	yes	Mexico	yes	yes	yes
Burkina Faso	yes	yes	yes	Moldova	no	no	yes
Burundi	yes	yes	yes	Morocco	yes	yes	yes
Cameroon	yes	yes	yes	Mozambique	yes	yes	yes
Cape Verde	yes	yes	yes	Namibia	yes	yes	yes
Central African Rep.	no	no	yes	Nepal	no	no	yes
Chad	yes	yes	yes	Nicaragua	yes	yes	yes
Chile	yes	yes	yes	Niger	yes	yes	yes
China	yes	yes	yes	Nigeria	yes	yes	yes
Colombia	yes	yes	yes	Pakistan	yes	yes	yes
Comoros	no	no	yes	Panama	yes	yes	yes
Congo, Dem. Rep.	yes	yes	yes	Papua New Guinea	yes	yes	yes
Congo, Rep.	yes	yes	yes	Paraguay	yes	yes	yes
Costa Rica	yes	yes	yes	Peru	yes	yes	yes
Cote d'Ivoire	yes	yes	yes	Philippines	yes	yes	yes
Croatia	no	no	yes	Poland	no	yes	yes
Czech Republic	no	no	yes	Romania	yes	yes	yes
Dominican Republic	yes	yes	yes	Russian Federation	no	no	yes
Ecuador	yes	yes	yes	Rwanda	yes	yes	yes
Egypt, Arab Rep.	yes	yes	yes	Senegal	yes	yes	yes
El Salvador	yes	yes	yes	Sierra Leone	no	yes	yes
Equatorial Guinea	no	no	yes	Singapore	yes	yes	yes
Ethiopia	yes	yes	yes	Slovak Republic	no	no	yes
Fiji	yes	yes	yes	Slovenia	no	no	yes
Gabon	yes	yes	yes	South Africa	yes	yes	yes
Gambia, The	yes	yes	yes	Sri Lanka	yes	yes	yes
Georgia	no	no	yes	Swaziland	no	no	yes
Ghana	yes	yes	yes	Syrian Arab Republic	yes	yes	yes
Guatemala	yes	yes	yes	Tanzania	yes	yes	yes
Guinea	yes	yes	yes	Thailand	yes	yes	yes
Guinea Bissau	yes	yes	yes	Togo	yes	yes	yes
Haiti	no	yes	yes	Trinidad & Tobago	yes	yes	yes
Honduras	yes	yes	yes	Tunisia	no	yes	yes
Hungary	no	yes	yes	Turkey	yes	yes	yes
India	yes	yes	yes	Uganda	yes	yes	yes
Indonesia	yes	yes	yes	Ukraine	no	no	yes
Iran, Islamic Rep.	yes	yes	yes	Uruguay	yes	yes	yes
Israel	yes	yes	yes	Venezuela, RB	yes	yes	yes
Jamaica	yes	yes	yes	Yemen, Rep.	no	no	yes
Jordan	no	yes	yes	Zambia	yes	yes	yes
Kazakhstan	no	no	yes	Zimbabwe	yes	yes	yes
Kenya	yes	yes	yes				

Appendix IV. Prediction of the Standard Growth Model of the Quantitative Impact of Aid [28]

In this appendix we derive a theoretical estimate of the impact of aid on growth based on the standard Solow-Swan Growth model. The model assumes that a fraction of aid goes toward financing public investment, which has an impact on long-run growth via capital accumulation.

$$Y = AK^{\alpha}L^{1-\alpha} \qquad \text{---(1)}$$

Equation 1 is a simple Cobb-Douglas production function, with α representing the share of capital in income, and A the technology parameter. In per worker terms, equation 1 can be re-written as:

$$y = Ak^{\alpha}$$

Where $y = Y/K$ and $k = K/L$

The equation for capital accumulation is:

$$\dot{K} = I - \delta K = I_G + I_P - \delta(K_P + K_G) \qquad \text{---(2)}$$

where the subscripts refer to the private and government sectors, and δ the depreciation rate. Assuming that a fraction β of aid is invested by the government, with the rest representing consumption or waste, equation (2) can be re-written as:

$$\dot{K} = \beta Aid + I_P - \delta(K) \text{ and}$$

$$\dot{k} = \frac{\beta Aid}{L} + \frac{I_P}{L} - (n+\delta)(k) \qquad \text{---(3)}$$

where n represents the population growth rate.

The rate of growth of output per worker γ_y can be expressed in terms of the rate of growth of capital stock per worker:

$$\gamma_y = \frac{\dot{y}}{y} = \alpha\left(\frac{\dot{k}}{k} + \frac{\dot{A}}{A}\right) \qquad \text{---(4)}$$

[28] We are grateful to Marta Ruiz-Arranz for this proof.

Substituting equation (3) in (4) yields

$$\gamma_y = \alpha(\frac{\beta Aid}{kL} + \frac{I_P}{kL}) - \alpha(n+\delta) + \alpha\frac{\dot{A}}{A} \quad \text{---(5)}$$

The coefficient in cross-country growth regressions measures the change in growth with respect to the change in the ratio of aid to GDP. We need to convert equation (5) into one that expresses aid in terms of GDP on the right hand-side. Thus (5) can be re-written as:

$$\gamma_y = \frac{\alpha\beta Aid}{Y}\frac{Y}{K} + \frac{\alpha I_P}{kL} - \alpha(n+\delta) + \alpha\frac{\dot{A}}{A} \quad \text{---(6)}$$

Differentiating equation (6) with respect to aid-to-GDP yields:

$$\frac{\delta\gamma_y}{\delta(\frac{Aid}{Y})} = \alpha\beta\frac{Y}{K} \quad \text{---(7)}$$

Equation 7 implies that the coefficient of aid in a cross-country growth regression should be related to the capital share in income (α), the fraction of aid that is invested (β), and the output capital ratio (Y/K).

Assuming that all aid is invested (β =1), and using a value of capital share =0.35 computed by Bosworth and Collins (2003), and the average value of the output-capital ratio for the developing countries in our regressions sample which is about 0.45, the magnitude of the regression coefficient amounts to 0.16; that is, a 1 percentage point increase in the ratio of aid to GDP should raise the growth rate by 0.16 percent, even on the most optimistic assumption that all aid is usefully invested. More realistically, if half of all aid is wasted or consumed, the coefficient value should be 0.08 or close to 0.1.

Appendix Table 1: Impact of Aid on Growth Using Alternative Instrument
(dependent variable is average annual growth rate of per capita GDP)

	Total Aid				
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
	1960_00	**1960_80**	**1970_00**	**1980_00**	**1990_00**
Aid/GDP	-2.513	1.725	-0.951	1.528	-14.284
	(1.08)	(0.42)	(0.22)	(0.27)	(0.89)
Initial per cap. GDP	-1.124	-1.070	-1.475	-1.284	-1.157
	(4.40)***	(1.75)*	(4.62)***	(2.90)***	(1.54)
Initial level of life expectancy	0.038	0.065	0.044	0.062	-0.009
	(2.13)**	(1.42)	(1.86)*	(1.44)	(0.08)
Institutional quality	4.035	7.934	3.359	2.384	5.331
	(2.32)**	(3.15)***	(1.48)	(0.77)	(1.25)
Geography	0.430	-0.181	0.406	0.603	0.310
	(3.25)***	(0.77)	(2.55)**	(3.15)***	(0.86)
Initial level of government consumption	-0.008	-0.016	-0.020	-0.047	-0.030
	(0.39)	(0.45)	(1.00)	(2.35)**	(0.96)
Revolutions	-1.257	1.109	-1.358	-0.343	-1.772
	(2.52)**	(1.05)	(2.75)***	(0.55)	(2.20)**
Terms of trade growth	0.015	0.011	0.030	0.011	-0.042
	(1.79)*	(1.82)*	(2.46)**	(0.59)	(1.00)
St. deviation of TOT growth	-0.014	0.002	-0.033	-0.013	-0.120
	(1.36)	(0.21)	(2.22)**	(0.53)	(1.96)*
Initial level of policy (Sachs-Warner)	1.933	1.551	2.217	2.343	0.774
	(3.48)***	(1.58)	(4.02)***	(3.11)***	(1.31)
Observations	74	61	80	81	79
R-square	0.73	0.55	0.70	0.59	0.52

All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid is discussed in Appendix 2. Outliers are identified using the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. For descriptions of the variables and their sources, see Appendix 1.

Appendix Table 2: Impact of Aid on Growth Conditional on Policy and Institutions, IV Estimations
(dependent variable is average annual growth rate of per capita GDP)

	(1) 1980_00	(2) 1990_00	(3 1980_00	(4) 1990_00
Aid/GDP	-4.591	-9.529	3.280	-13.415
	(0.30)	(0.32)	(0.48)	(0.78)
Policy (Sachs-Warner)			0.556	0.779
			(0.48)	(0.94)
Aid/GDP*policy (Sachs-Warner)			107.860	-0.202
			(1.58)	(0.02)
Policy (World Bank's CPIA ratings)	0.239	1.071		
	(0.86)	(1.72)*		
Aid/GDP*policy (World Bank's CPIA ratings)	0.741	-1.008		
	(0.21)	(0.12)		
Observations	80	76	80	76
R-square	0.54	0.53	0.62	0.50

This table reproduces the results in Table 7, except that the World Bank's CPIA rating substitutes for the Sachs-Warner-Warcziarg-Welch measure of policy. The estimation is for a sample that is common across the two measures of policy. All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid corresponds to equation 2 in Section II of the paper. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates are omitted for presentational simplicity. The first-stage is also omitted as it is virtually unchanged from that in Table 9. For descriptions of the variables and their sources, see Appendix 1.

Appendix Table 3: Impact of Aid on Growth: Does Donor Type Matter? IV Estimations
(dependent variable is average annual growth rate of per capita GDP)

	(1) 1960_00	**(2) 1960_80**	**(3) 1970_00**	**(4) 1980_00**	**(5) 1990_00**
Aid/GDP	-2.489	3.241	-0.823	1.197	-12.799
	(1.04)	(0.81)	(0.19)	(0.20)	(0.72)
Aid share of Scandinavian countries	-0.670	-4.108	-1.101	-0.233	0.630
	(0.30)	(1.59)	(0.38)	(0.08)	(0.16)
Observations	74	61	80	81	79
R-square	0.73	0.57	0.7	0.59	0.52

All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid is discussed in Appendix 2. Outliers are identified using the Hadi (1992) procedure. Scandinavian countries include Denmark, Finland, Netherlands, Norway, and Sweden. All specifications include dummies for sub-Saharan African and East Asian countries. For descriptions of the variables and their sources, see Appendix 1.

Appendix Table 4: Estimating the Exogenous Variation in Donors' Allocation of Social Aid Across Recipients

(dependent variable is share of donor i's social aid to recipient j)

	(2) 1970_00	(3) 1980_00	(4) 1990_00
Dummy for common membership in Entente Alliance (Alliance Dummy)	0.126	0.475	0.283
	(3.14)***	(5.87)***	(6.39)***
Dummy for Egypt and Israel after Camp David (Egypt Israel Dummy)	0.233	0.226	0.189
	(10.30)***	(10.58)***	(10.02)***
Dummy for pairs that ever had a colonial relationship	0.101	0.086	0.064
	(10.86)***	(9.91)***	(7.99)***
Dummy for pairs currently in a colonial relationship	0.006	-0.038	-0.025
	(0.17)	(1.02)	(0.56)
Dummy for pairs that have common language	0.006	0.005	0.003
	(2.90)***	(2.74)***	(1.85)*
Dummy for country that ever had a colonial relationship with UK	-0.089	-0.076	-0.061
	(8.60)***	(7.75)***	(6.88)***
Dummy for country that ever had a colonial relationship with France	-0.091	-0.078	-0.054
	(8.11)***	(7.42)***	(5.69)***
Dummy for country that ever had a colonial relationship with Spain	-0.074	-0.058	-0.034
	(5.92)***	(4.92)***	(3.20)***
Dummy for country that ever had a colonial relationship with Portugal	0.040	0.056	0.083
	(2.49)**	(3.73)***	(6.12)***
Observations	2315	2303	2213
R-squared	0.15	0.16	0.16

Estimation is by ordinary least squares. The estimated equation corresponds to equation 1 in Section II of the paper. The dependent variable is the share of *social sector aid* given by donor i to recipient j. All standard errors are robust. T-statistics reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. For descriptions of the variables and their sources, see Appendix 1.

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