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

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We examine the effects of aid on growth in cross-sectional and panel data—after correcting for the possible bias that poorer (or stronger) growth may draw aid contributions to recipient countries. 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 suggest that for aid to be effective in the future, the aid apparatus will have to be rethought.

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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.[1]

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[1] There is a 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.

What does this paper add to the voluminous literature on aid effectiveness? Essentially two things. First, most papers in the literature examine aid-effectiveness in a typically narrowly defined setting. We attempt to examine the aid-growth relationship under a variety of settings, using one common framework. Second, we examine carefully the issue of endogeneity—the possibility that aid flows could go to countries that are doing particularly badly, or to countries that are doing well, creating a spurious correlation between aid and growth.

More specifically, the cross-country aid-growth literature has typically examined particular aspects of the possible relationship. Burnside and Dollar (2000), for example looked at the impact of aid on growth conditional on the quality of economic policy. Hansen and Tarp (2001) examine the relationship in a panel framework, and more recently, Dalgaard, Hansen, and Tarp (2004) focus on aid's impact conditional on the country's location (geography). Recently, Clemens et. al. (2004) disaggregate aid into what they term short- and long-impact aid. We examine under 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 (contemporaneous and varying lags (from 10-30 years), specifications (cross-section and panel), and samples (including and excluding outliers).

One reason to try and take a more comprehensive perspective is that the aid-growth 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. Thus, our aim is not to target any particular result or paper.[2] 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. We are, no doubt, informed by the literature about where to look.

The existing literature also may have gone down some paths that are worth re-examining. For example, the practice of estimating growth regressions over four year periods is quite common (see, for example, Burnside and Dollar (2000), Collier and Dollar (2003) and Clemens et. al. (2004)). Four-year growth regressions are prone to be affected by cyclical factors, which are hard to control for, even if the attempt is made. Moreover, the issue of key interest is the long-run impact of aid: aid could mechanically increase output and growth in

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[2] This is why we do not attempt an exegesis of individual contributions as in Easterly, Levine and Roodman (2004), Roodman (2004), or Subramanian and Kumar (2005).

the short run but this is not what economists care about.[3] If estimations without country fixed effects are to be done at all, the appropriate horizon is long.

In particular, aid can contribute to development in two ways. It can take a capital starved country to its ultimate steady state potential growth rate faster. Aid can also improve a country's ultimate steady state growth rate (for example, because foreign capital brings know-how or encourages better governance or practices). Clearly, as we examine longer horizons, we will incorporate more spillover effects, and effects that take time to emerge. Since both the positive and adverse effects of aid may stem precisely from these effects (see Rajan and Subramanian (2005)), 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.

But cross-sectional regressions have their well-known problems. Apart from concerns about endogeneity, outliers, model uncertainty, and measurement error, a key drawback is the problem of unobservable heterogeneity or the omitted variable problem. In cross-country regressions, we can never be sure whether we are controlling for all possible ways in which

[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.

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). The inclusion of country fixed effects is, however, not typical in the literature even when the focus is on four year horizons. For the panel estimations, we report results using the Arellano-Bond and Blundell-Blond GMM estimators, which address the potential endogeneity of the regressors, and incorporate (implicitly) fixed effects.

Let us now turn to the second main contribution of the paper. As is well recognized, aid flows are influenced by a country's situation. Aid may go to countries that have just experienced natural disasters—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 will explain later how our methodology adds some value.

In sum, despite lying squarely in the tradition of cross-country and panel growth regressions with all their 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 generalizing, past work on aid and growth. It seeks to answer the question, "even though the cross-country regression framework may be flawed, what it really tells us about the impact of aid on growth?"

Our findings are relatively easy to report. We find little evidence of a robust positive correlation between aid and 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. We find little evidence that aid works better in better policy or institutional environments, or that certain kinds of aid work better than others. We do find weak (and mixed) evidence that aid works better in some geographical settings, but it is hard to see a strong rationale for this finding—and therefore are skeptical whether anything can be generalized from this. Our broad findings hold both in cross-section and panel estimations, across time horizons, and do not depend on whether outliers are included or excluded from the sample.

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, the simple theoretical exercise we present later 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 plagues 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 Rajan and Subramanian (2005), in which we 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. In Section IV, we compare the magnitude of the aid coefficients derived from theory with those obtained in the empirical literature, and conclude in Section V.

I. Tackling Endogeneity: An Instrumentation Strategy

We present in Table 1 the basic descriptive statistics for the data we use in our analysis. Let us start with OLS cross-country regressions in Table 2. The dependent variable in all cases is the average annual growth rate of per capita GDP of a country over the relevant period. The explanatory variable of interest is the average ratio of annual external aid to GDP over that period to that country. Our sample comprises all developing countries which have received aid during the post-war period and for which data are available (see Appendix 3). Because we include all countries that received aid, 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.

The immediate question is what other explanatory variables should we include? The aid and growth literature includes explanatory variables that are somewhat different from the cross-country growth literature (see for the example, the contrast between Burnside and Dollar (2000), Collier and Dollar (2003), Clemens et. al. (2004), Hansen and Tarp (2001), and Dalgaard, Hansen, and Tarp (2004), on the one hand and Bosworth and Collins (2003), Sala-i-Martin et. al., 2004, on the other. In the working paper version (Rajan and Subramanian, 2005), we followed the broader cross-country growth literature. In this paper, however, we follow the aid and growth literature to enhance the comparability of our results to the related literature. The results are qualitatively similar.

The next question is which particular explanatory variables to choose because even within the aid-growth literature there is variation. We chose the four most recently published papers--Burnside and Dollar (2000), Collier and Dollar (2003), Hansen and Tarp (2001), and Dalgaard, Hansen, and Tarp (2004)—and take the intersection set of the variables in these four papers. This comprises: initial level of per capita income; institutional quality; financial depth measured as the ratio of M2 to GDP; assassinations; ethnic fractionalization; trade policy; inflation; and the ratio of budget balance to GDP. We replace assassinations by revolutions only because the latter appears to be more robustly significant. We then add two measures that are likely to be important correlates with growth; a measure for geographical location from Bosworth and Collins (2003) (which is a combination of the average number of frost days per month in winter and the fraction of a country's area in the tropics) and a measure of health (which we proxy for with life expectancy). We stress here that our results on the aid coefficient are virtually identical even if we don't add these two covariates: adding them gives us a more stable and general specification. Note that one difference between our covariates and the papers cited above is that some of them include a composite policy measure that combines trade policy, inflation, and budget balance. It seems more general to include them separately, rather than force a pre-specified relationship.[4]

A final concern before we report results. Easterly (2004) makes the argument that many cross-sectional regression results are driven by outliers. We tested for outliers according to the Hadi (1992) procedure as implemented in Roodman (2003), using the instrumental variable specifications, which constitute the core results of the paper. This procedure yielded two outliers for one time period (1980-00) and none for the other periods. For the sake of comparability across specifications, throughout the paper we report results for the samples excluding these two outliers for the period 1980-00.

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[4] To minimize endogeneity in the cross-section regressions, the values of the potentially endogenous explanatory variables are for the beginning of the relevant time period. Details of how these initial values and other averages over time are constructed are presented in Appendix 1.

We report OLS results in Table 2 for the following four time periods: 1960-2000; 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 estimation periods could also give us a sense of any changes in aid effectiveness over time.

The results are quite clear. In all four cases, the estimated aid coefficient is negative and statistically significant at conventional levels in three (1960-2000, 1970-2000, and 1980-2000). The magnitude in these cases suggests that an increase in aid of 1 percentage point of GDP is correlated with *lower* long-run growth of approximately 0.1 percentage points per year.

One cannot take these estimates seriously as evidence of causality because of the problem of endogeneity. If donors are Good Samaritans and motivated by suffering in the recipient country, the lower the growth (and the more the suffering), the greater will be 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.

This problem is well recognized in the literature as is a possible solution, instrumentation, but the instrumentation strategy used has limitations. Take for example, the instrument sets used in the papers by Burnside and Dollar (2000), Hansen and Tarp (2002) and Clemens et. al. (2004).

Burnside and Dollar (2000)	Hansen and Tarp (2002) (In Table 1)	Clemens et. al. (2004)
1. Dummy for Egypt	1. Dummy for Egypt	1. Egypt dummy
2. Franc zone dummy	2. Arms Imports (t-1)	2. Arms imports
3. Central America dummy	3. Policy (t-1)	3. Policy^2
4. Arms imports (t-1)	4. Policy^2 (t, t-1)	4. Policy^2 (t-1)
5. ln (population)	5. Policy * ln(population)	5. Policy * ln population
6. Policy * ln(population)	6. Policy * ln(initial GDP per capita)	6. Policy * initial GDP per capita
7. Policy * ln(population)^2	7. Policy * ln(initial GDP per capita)^2	7. Policy * initial GDP per capita^2
8. Policy * ln(initial GDP per capita)	8. Policy * aid (t-1)	8. Aid (t-1) * policy (t-1)
9. Policy * ln(initial GDP per capita)^2	9. Policy * aid^2 (t-1)	9. Aid^2 (t-1) * policy (t-1)
10. Arms imports (t-1) * policy	10. aid (t-1)	10. Log repayment (t-1) * policy (t-1)
	11. aid^2 (t-1)	11. Aid (t-1)
		12. Aid^2 (t-1)
		13. Log repayment (t-1)
		14. Policy (t-1)

A variable such as arms imports could be a proxy for strategic reasons for giving aid, and thus is plausibly orthogonal to motives for giving aid that relate to the underlying economic situation of the recipient. Not all variables are so plausibly exogenous. For variables that are some transformation of current or lagged endogenous (aid) and possibly exogenous (policy) variables, the economic motivation is more difficult to understand. Moreover, econometrically, the problem with using lagged values of endogenous variables is that they might be predetermined but still not exogenous, especially if there is serial correlation in the dependent variable. And in all these papers, the possibility of serial correlation is high because growth is measured over a fairly short interval (i.e., growth today is depressed because of the same shock that depressed it four years ago, which prompted aid flows four years ago).[5]

More questionable is the use of the lag of a right hand side variable (policy) as an instrument. This amounts to claiming that contemporaneous policy affects growth directly but lagged policy does not. Put differently, the exclusion restriction underlying the use of lagged policy is that trade reform (and macroeconomic stabilization) in a time period has an important

[5] 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. Roodman (2004) only tests the robustness of recent results reported in a number of papers, but does not discuss endogeneity or instrumentation issues.

effect on growth in that time period (four years in the case of the standard specification in the literature) but absolutely no effect four years later.[6]

It is to address some of these limitations that we attempt a different instrumentation strategy, where we construct instruments for aid. We exploit the fact that aid is often extended for non-economic reasons. Our main identification assumption is that non-economically-motivated aid is unlikely to be driven 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 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.

Our key idea for instrumentation is to model the *supply* of aid based on donor-related rather than recipient-specific characteristics. In other words, we base our instrument on considerations that drive individual donors to give aid to a country other than those related to a country's level of income or growth. So, our construction of instruments starts from the

[6] Put differently, a rationale for not using lagged policy as an instrument is that the process which generates lagged policy may involve error terms which are correlated with the error terms on growth in the future. Intuitively it means that countries which experience positive policy shocks may also be experiencing positive growth shocks 4 years from now (because of good leadership, for instance).

bilateral (donor-recipient) relationship and aggregates up (as Frankel and Romer (1999) have done in the trade literature). This is in contrast to the literature that picks instruments directly at the level of the recipient country.

Start first with how a donor would choose to allocate aid across recipients. If we make the plausible assumption that in a fair world a donor wants to induce similar growth rates in per capita incomes of all poor recipients then, provided the technologies of production of recipients and capital shares are similar, the donor (as we show later in section IV) should allocate similar amounts of aid (relative to recipient GDP) for each recipient. Of course, most donors are likely to depart from this allocation for at least two reasons: history and influence. The greater the extent of historic relationships between a donor and a recipient the more likely that a donor will want to give aid. We capture historic relationships through colonial links and commonality of language.

To capture to the effects of influence, we make the plausible assumption that donors are more likely to want to give aid the more they expect to have influence over the recipient. One way to proxy for influence is through the relative size of donor and recipient. The bigger the donor is relative to the recipient, the more influence the donor is likely to have, so the ratio of the donor population to that of the recipient could be a good proxy for influence. In addition, a donor's influence is likely to be particularly pronounced if it is relatively large <u>when</u> it has close links with the recipient (so that it not only understands the pathways of influence but also potentially scares away other donors from seeking influence). We capture this channel by including the interaction between relative size and colonial links.

An example should help fix ideas; the United Kingdom should be willing to give more aid per capita to Uganda than to India; but it will be more willing to give aid to Uganda than to a similar-sized country in Africa, say Senegal, because France is likely to have a significant aid presence in the latter, thus diluting any possibility of British influence.

The aid supply decision from a donor (d) to a recipient (r) can be expressed by the following model:

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\begin{aligned}
& A_{drt} / GDP_{rt} = \beta_1 ' Y_{drt} + \upsilon_{drt} \\
& = \beta_0 + \beta_1 COMLANG_{dr} + \beta_2 CURCOL_{dr} + \beta_3 COMCOL_{dr} + \beta_4 COMCOLUK_{dr} + \beta_5 COMCOLFRA_{dr} \\
& + \beta_6 COMCOLSPA_{dr} + \beta_7 COMCOLPOR_{dr} + \beta_8 \log(POP_d / POP_r) \\
& + \beta_9 \log(POP_d / POP_r) * COMCOL_{dr} + \beta_{10} \log(POP_d / POP_r) * COMCOLUK_{dr} \\
& + \beta_{11} \log(POP_d / POP_r) * COMCOLFRA_{dr} + \beta_{12} \log(POP_d / POP_r) * COMCOLSPA_{dr} \\
& + \beta_{13} \log(POP_d / POP_r) * COMCOLPOR_{dr} + \upsilon_{drt}
\end{aligned} \tag{1}
$$

where A_{drt} is the aid given by donor d to recipient r in time period t. GDP_{rt} is the recipient country's GDP.[7] The first three RHS variables capture historic factors: COMLANG is a dummy that takes a value of one if the donor and recipient share a common language;

[7] In order to estimate equation 1, we need to compute the 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.

CURCOL is a dummy that takes a value of one if the recipient is *currently* a colony of the donor. COMCOL is a dummy that takes a value of one if the recipient was ever a colony of the donor. The next four variables simply disaggregate the colonial variable to capture difference across donors in the susceptibility of their giving to colonial ties (COMCOLUK, COMCOLFRA, COMCOLSPA, and COMCOLPOR refer in turn to colonial relationships involving respectively France, Portugal, Spain, and the United Kingdom).

The next set of variables relate to relative size. Donor size relative to the recipient is measured by the ratio of the logarithm of the populations of donor and recipient ($\log(POP_d / POP_r)$)[8]. Donor influence relative to other donors is additionally measured by the interaction of the relative population size and the colonial dummy ($\log(POP_d / POP_r) * COMCOL_{dr}$).

The estimated equation is then aggregated across donors to yield a level of the fitted value of aid-to-GDP for the recipient for that period.

$$\left(\bar{A}_{rt} / GDP_{rt} \right) = \sum_{d} \hat{\beta}_1 Y_{drt} \tag{2}$$

[8] To minimize problems, we use the initial rather than the contemporaneous value of population.

In Table 3, we present estimates for the model in (1).[9] Virtually all the instrumenting variables are significant for all the time horizons, and between them the variables account for a reasonable share (between 33 and 42 percent) of the variation in the donor allocation decision. All the colonial relationships are significant as are the two measures of influence. The larger the donor relative to the recipient, the greater the aid given; and this effect is magnified, as conjectured, in cases where the donor had a colonial relationship with the recipient. For example, for the period 1960-00, a 1 percent increase in the ratio of donor size relative to recipient is associated with a 0.1 percent increase in the ratio of aid to GDP given by the average donor, an amount which is increased by 0.2 percent for a U.K. colony (which is the sum of 0.6 percent for the average colony minus 0.4 percent for the U.K. colony dummy), 1 percent for a Spanish colony; 1.9 percent for a French colony and 3.3 percent for a Portuguese colony. Thus, influence seems to matter a lot for donors, especially for Portugal and France.

How much information about aid is contained in our instrument? In Chart 1, we depict the first-stage relationship between actual and fitted aid calculated from the estimates of model 1 for the period 1960-00 after partialling out the effects of other covariates included in the first stage. The relationship between actual and fitted aid is strong, with a t-statistic of about 5.

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

Our instrument appears to contain a lot of, hopefully exogenous, information about actual aid.

II. Aid and Growth: Revisiting the Cross-Section Evidence

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

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. [10] In Tables 4A, we present the second-stage of the core instrumental variable (IV) specifications, which is representative of the results we obtain more broadly. The equations are reasonably well specified as many of the standard covariates are significant and have the expected sign. The coefficient on the aid term is not significantly different from zero in three of four periods, while it is negative and significant in the period 1990-00. We should not put too much weight on this last result because, as we will see from later tests, the instrumentation strategy is least reliable for this period.

[10] The standard errors are affected by the fact that the instruments are estimated. The standard error correction we used to estimate the correct standard errors is based on Frankel and Romer (1999). The results were virtually unaffected by this correction.

The magnitudes of the estimated coefficient of aid are generally quite small, suggesting that aid has a very small effect—positive or negative—on growth. Also, for the first three columns, instrumenting seems to make the impact of aid less negative or more positive (compare the aid coefficients columns in Table 2 columns 1-3 with those in Table 4A columns 1-3). In other words, the IV strategy tempers the tendency of the OLS to magnify the negative impact of aid.[11]

Robustness

Let us now turn to possible concerns about our instruments. First, do they satisfy the exclusion restriction—that is, are they plausibly exogenous? 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. In our framework, the colonial and influence variables may be correlated with growth other than through the endogenous variable. For example, proximity to donors (say through a common language) might be bad because donors require bad policies or support bad leaders or require greater defense-related spending. Alternatively, proximity to

[11] As indicated in Table 4B, the coefficient estimate for the instrument is significant at the 1 percent significance level for all estimation periods. In the three long horizons, the F-test for excluded instruments comfortably exceeds the weak instrumentation threshold of ten suggested by Staiger and Stock (1997) for the case of one endogenous regressor. For the 1990_2000 horizon, the F-statistic is about 8.5.

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. One response to this concern is that in general we control for the different ways (policies and institutions) in which historical variables can affect growth.

A crude way of testing whether our colonial variables pass the exclusion restriction is to simply include the colonial variables for France, Spain and the United Kingdom directly in the second stage. When we do this (Table 5A), the magnitudes of the coefficient estimates on aid are not significantly altered across periods (compare Table 5A and Table 4A) and we do not find a consistent pattern of the colonial variables being significant. This suggests that a direct relationship between colonial variables and growth is not responsible for our results.

A second check is to see if our instrument passes over-identification tests. For this, of course, we need a second instrument. As is well-known, it is very difficult to find such instruments, especially one that is very different from the instrument that we have proposed in this paper. But to the extent that over-identification tests have merit, a possible candidate is country size; for example, even if donors did not care about influence, economies of scale (in dealing with recipients, monitoring programs, etc.) might, a priori, suggest recipient size as a factor determining aid decisions. Given our use of population as a scaling variable for measuring influence in our preferred instrument for aid, we use the logarithm of recipient area as the second instrument. So, we re-run the growth equation, this time using as instruments in the first stage both our instrument derived from model (1) as well as the log of area. The results are presented in Tables 5B (second-stage) and Table 5C (first-stage).[12]

The first thing to note is that the coefficient on the aid variable is insignificant (Table 5B), establishing the robustness of the core result. Second, the p-values for the Sargan test for over-identifying restrictions are greater than 0.1 in three out of four cases, suggesting that our preferred instrument at least passes these minimum requirements. The over-identification test is, however, not met for the 1990-2000 horizon, which is why the IV strategy (and the negative and significant coefficient) for this period should be viewed with caution.

Third, adding area as an additional instrument serves to shed light on our preferred instrument. It is interesting to note in the first-stage results (Table 5C) that area is not significant in the three long horizons while our preferred instrument is. This suggests that our instrument contains a lot of information over and above that relating simply to recipient size.[13] Finally, we present in Table 5D, an intuitive way of illustrating the exclusion

[12] When the log of area is used as the sole instrument, it yields good first-stage results (results available from the authors).

[13] While a measure of country size could in itself be a plausible instrument, the reason not to make it the preferred one is that there is uncertainty whether it can satisfy the exclusion restriction; that is, a number of reasons can be advanced as to why a recipient's size would have an independent effect on growth. Our instrument, on the other hand, contains information that is not based on recipient size, and the finding in Table 5C is consistent with that.

restriction (as suggested in Acemoglu et. al., 2001). We estimate the first stage, instrumenting aid with log area and in the second stage also introduce our preferred instrument directly. The instrument is insignificant in three out of four cases (and only marginally significant in the 1990-00 horizon), again suggesting that it has no independent effect on growth.

Of course, none of our attempts at addressing endogeneity through instrumentation is fool-proof. Another, albeit rough way of doing this is simply to use initial or lagged values of aid instead of instrumenting for contemporaneous values. We do this in Table 6, attempting to be comprehensive by encompassing all possible timings of impact. Moving across the table are columns representing the different growth horizons: 1960-00, 1960-90 etc, etc. In all, there are nine such horizons. Moving down the table, we use different lags for the aid variable. So the first row represents the impact of aid given in 1960-70 on growth in 1960-70 (column 1), 1970-80 (column 2), and 1960-80 (column 6) and so on. The table thus presents the OLS estimate for lagged aid in the standard growth regression for different horizons, time periods, and lags for aid. In no case is the estimated coefficient on lagged aid positive and significant. Fully 32 of 37 coefficients are negative, of which 11 are statistically significant. Typically, the estimated coefficient is small (below 0.1 in magnitude).

Non-linear and conditional effects

Let us turn to concerns about specification. In Panel A of Table 7, we ask whether there are diminishing returns to aid. To test this, we include a squared aid term in the regression. All the aid terms are insignificant as are the aid-squared terms. There is no consistent pattern in their signs (a negative squared term would imply diminishing returns).

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 Panel B, we examine whether aid is more effective in better policy environments. We interact aid with two measures, first, the Sachs-Warner measure updated by Warcziarg and Welch (2003). We find that the coefficient on the aid-policy interaction terms is never significant. This is consistent with the results in Easterly, Levine, and Roodman (2004). We find similar results when we interact aid with the World Bank's CPIA ratings.

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. But in the absence of a strong theoretical rationale, any such result might simply be an (ex post) way of characterizing countries where aid has "worked" from countries where it has not, without offering an explanation.[14] 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 Panel C of Table 6, in addition to the standard covariates, we introduced a term, interacting aid with the measure of geography due to Bosworth and Collins (2003).[15] While geography itself usually has a positive correlation with growth, the aid-geography coefficient is never significant.

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[14] Put differently, there will always be a sample of countries where the aid growth correlation is positive, even if the true average effect is zero or negative. Given that there are many characteristics by which we can sort countries, it is not hard to find a characteristic that lines up with the countries for which the correlation is positive. However, it is not clear what conclusion one can draw from such a finding.

[15] The Bosworth and Collins (2003) geography measure is a combination of the average number of frost days per month in winter and the fraction of a country's area in the tropics, whereas the Dalgaard et. al. (2004) measure is just the latter but the two are highly correlated (correlation of 0.92 in our sample).

Categories of aid: "Good" and "bad" aid

One of the increasingly popular strands in the aid-growth literature is to examine the impact of different categories of aid. That is, researchers are moving from analyzing the impact of aggregate aid to analyzing different components. Four different basis of distinguishing aid are:

- Motives for granting aid (why?)
- Donor type (who is granting aid?)
- The use to which aid is put (for what: health, social sector, technical assistance?)
- The timing of impact (when?)

A general problem with making distinctions between aid is fungibility. Earmarking of aid is not easy, especially for small donors, and even if it is, governments can divert traditional sources of financing to other uses, undermining the earmarking objectives of aid (that is for fungibility to be violated, two conditions need to be fulfilled: it should be possible to earmark aid for particular uses and it should not be possible for the government to then divert other expenditure that would otherwise have gone to those uses). At the very least, claims about earmarking (which are implicit in slicing up aid into different sub-categories) need to be substantiated.

However, let us set aside the possibility that aid is fungible (though see Devarajan and Swarup, (1998) for evidence that aid is fungible) and examine whether sub-categories of aid have differential effects. Consider first, the distinction based on motives.

One possible concern with our instrumentation strategy is that it highlights an “influence” channel for giving aid that may not be in the best interests of the recipient. More generally, some recent papers (see Bourgingnon and Sundberg (2007)) tend to dismiss strategic aid as “bad” aid, and thus emphasize there should be no prior expectation of beneficial effects from that kind of aid.

An important distinction needs to be made here between the *motives* for giving aid and how it is used. Why aid is given need have very little relationship to how it is used. To characterize strategic aid as “bad” aid is mixing motives and consequences. Indeed, for strategic aid to be “bad” aid (as in badly used aid leading necessarily to bad growth outcomes) two things would have to be shown; First, that strategic aid is systematically given to countries with bad policies or institutions; Second, it would also have to be shown that in some way researchers are not taking into account the effect of these bad policies and institutions that mediate aid into outcomes.

The second factor is relatively easy to dispose because typically, most growth regression control for a host of policies and institutions—macroeconomic, structural, openness and property rights etc.—that potentially affect growth. In other words, our regressions suggest that aid has no systematic effect even *after* controlling for any effect of strategic aid on policies and institutions.

On the first point, it should be noted just as an empirical matter, that while aid was given for strategic reasons to Mobuto in Zaire and Marcos in the Philippines and to Egypt, it was also given to Suharto in Indonesia, Pinochet in Chile, military regimes in Korea and Israel, all of which seem to have flourished. Equally, a lot of well-intentioned aid was given by the Nordic donors to Tanzania, Ghana, Kenya and a host of other African countries that do not seem to have grown strongly. Thus strategic aid is not synonymous with badly used aid.

To bolster this point consider the comparison between (1) the correlation between the Sachs-Warner variable (a good proxy for policies and institutions) and our predicted aid and (2) the correlation between the Sachs Warner variable and aid from the top 5 good donors to recipients (from Reddy and Miniou (2006)): For the four periods, 1960-00, 1970-00, 1980-00, and 1990-00, covered in our analysis, the correlations are -.03 vs -.016 (1960-00); -.04 vs -.14 (1970-00); -.02 versus -.07 (1980-00); and -.14 versus -0.17 (1990-00).[16] In other words, in three out of four cases, our predicted aid was less negatively correlated with good policies and institutions than so-called "good" aid. This suggests the whole enterprise of classifying aid as good or bad is on relatively weak empirical ground.

Another way of checking whether our instrument for aid proxies for "bad" aid is to see if the variables that we used to explain total aid also help explain "good" aid. Appendix Table 1 reproduces the estimation in Table 3 but this time with bilateral *social* aid as the dependent

[16] The pattern is broadly similar when we look at the World Bank's CPIA ratings instead of the Sachs-Warner variable and also when we consider the top 10 good donors instead of the top 5.

variable (and social aid is typically thought of as "good" aid). 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 similar share of variation. We now proceed to the other distinctions.

A second basis for distinguishing "good" or development aid is by type of donor. Reddy and Minoiu (2006) argue that aid given by a set of countries—mostly Scandinavian—is good aid, or better than that given by others and they similarly argue that multilateral aid is better than bilateral aid.[17] We test these propositions. One can indeed make the argument that multilateral aid is less explicitly "political" than bilateral aid and should therefore have a different, and presumably more favorable, 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. But we find no difference between the effects of multilateral and bilateral aid (Table 8A); both sets of coefficients are insignificant. We also ran the specifications in Table 6, using another Reddy and Miniou definition of good donors (both the top 5 and the top 10). As can be seen from Table 8B (aid is that from the top 5 donors), in only 13 of 37 cases is the aid coefficient positive, and in only one is it

[17] One additional problem of distinguishing aid simply by donor type is that it is then difficult to understand why one should expect similar consequences if it is given to two very different types of policy regimes. Why should aid by Norway to Tanzania produce the same result as aid by Norway to Zaire under Mobutu?

significant at conventional levels. 24 of 37 coefficients are negative, and significant in 3 cases. If anything, even after a careful selection of donors, the weight of evidence is towards a slight negative effect of aid.[18]

The third basis for distinguishing aid is by purpose. This basis is particularly prone to the fungibility/earmarking problem. If donors cannot individually or collectively alter the plans and priorities of recipient governments, then it is hard to justify slicing aid.[19] In Table 8C, we try to see if two different categories—aid for social uses and aid for economic uses have significant positive effects.[20] This does not turn out to be the case.

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[18] We estimate these using OLS because there is no variation for the top 5 donors in historic and language links. The results are similar when we use the top 10 good donors.

[19] One category of aid that might avoid the fungibility problem is technical assistance because of the manner in which it is provided: essentially, donors pay foreign consultants to provide some service to recipient governments. But technical assistance too does not seem to have any impact on aid either.

[20] The categories come from the OECD's DEC database that provides data on ODA commitments by purpose (CRS). Social sector aid includes education, health and population, and water supply and sanitation; and economic aid includes energy and transport and communications.

Finally, Clemens et. al. (2004) claim that aid needs to be distinguished by the timing of its impact—early versus late impact. Clemens et. al (2004) argues that the effect of early-impact aid will be easier to detect in the data than late-impact aid. Subramanian and Kumar (2005) contain a critique of their framework and findings. We re-run our specifications using their aid categories and find that there is no significant effect (Table 8D**)**. [21] In fact, our result in Tables 6 and 8B where we estimate the impact of aid over different lags (and hence is a more general test) also bears on the point made by Clemens et. al. (2004) about the timing of aid's impact. We see in these tables that regardless of when aid's impact can be thought to be felt, there seems little evidence that aid affects growth.

[21] We need to instrument for early impact and late 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 early impact aid with our instrument for economic aid and for late impact aid with the instrument for social aid. Recall that our instrument for economic aid is derived by running an equation explaining bilateral economic aid flows between donors and recipients (as we did for bilateral total aid in Table 3). We then constructed a series for fitted economic aid by aggregating across donors for each recipient. Similarly, we develop the instrument for social aid. The first-stage results (available from the authors upon request) show that these instruments work well.

In general, what we find, however, is that no sub-categories have any significant impact—positive or negative—on growth. There is, however, a more general concern with slicing and dicing up aid; it could become a fishing expedition where the researcher hunts for any sub category that has a positive (or negative) correlation with growth, and then finds an (ex-post) rationale to focus on that sub-category, depending on the researcher's biases. This is why the a priori theoretical rationale for focusing on a particular sub-category is important. Unfortunately, fungibility renders most such rationales suspect.

III. 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 approach that addresses the potential endogeneity of all the regressors and also incorporates fixed effects is to use panel GMM regressions.[22] These come in two flavors.

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[22] It is important to stress that fixed effects are not a panacea and come with their own problems: first, they do not help inference if there are time-varying omitted factors affecting the dependent variable and correlated with the right hand side variables. They may also exacerbate measurement error by removing a significant portion of the variation in the right hand side variables.

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.

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 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). The BB estimator has another deficiency: the instruments for the level equation, namely the lagged differences of the RHS variables, are valid only if they are orthogonal to the fixed effect. Given neither estimator is perfect, we report the results using both procedures.

In Tables 9 and 10, we report the results of the GMM estimations corresponding to the core specifications in Tables 4 and Tables 7A-7C (the specifications in Table 9 use the Arellano-Bond procedure while those in Table 10 use the Blundell-Bond procedure). In all these specifications, we report the results of using the unrestricted lags of all the endogenous variables as instruments. The results do not change when we use fewer lags (available from the authors).

The results are easy to summarize. In all the GMM specifications, which we would emphasize are really fixed effects panel versions of the cross-section regressions reported in tables 4 and 7, the results on aid remain broadly unchanged. In column 1 of Tables 9 and 10, we report the core specification corresponding to Table 4. In column 2, we test whether there are diminishing returns by adding the squared aid term (compare with Table 7, Panel A). Column 3 is the counterpart of Table 7, Panel B, which tests whether aid works better in better policy environments. Finally, in column 4 the aid-geography interaction is examined (compare with Panel C of Table 7). In no case, does aid have a positive and significant effect (the coefficient is negative and significant in Table 9, column 1 and in Table 10, column 4). There is no evidence of aid working in better policy environments (see the coefficients of the interaction between aid and policy in column 3 of Tables 9 and 10). There is some evidence for aid working better in better geographic environments using the AB estimate (Table 9, column 4) but the sign of the coefficient estimate for the aid-geography interaction is reversed (and significant) in the system GMM BB estimate in Table 10.

In Appendix Tables 2 and 3, corresponding respectively to the AB and BB procedures, we examine the impact of different categories of aid as in Table 8. Only early impact aid is significant at the 10 percent level (and it has diminishing effectiveness as suggested by the negative coefficient for the squared term). However, in the system GMM BB estimates in Appendix Table 3, the signs of the coefficient estimates are reversed, and they are no longer significant. It is hard to argue that even in the panel estimates, there is a robust effect of aid on growth.

IV. QUANTITATIVE IMPACT OF AID: THEORY AND EMPIRICS

What should one expect? Suppose the primary channel through which aid worked was by increasing public investment.[23] 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 2 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

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

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. Of course, if the public investment financed by aid has some spillover effects and hence an effect on productivity growth, the impact of aid could be slightly higher.

How does this theoretical estimate compare with the magnitudes in the empirical 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. The Clemens et. al. (2004) estimations yield values for the aid coefficient of about 0.64 and 0.33 in their IV and OLS estimations, respectively. The GMM estimations of Hansen and Tarp (2000, Table 1) yield values close to 0.3, and in Dalgaard et. al. (2004) the implied value varies from -0.1 to 1.2. Many of these are quite high relative to what theory might expect. In the estimation reported in this paper, many of the coefficients on aid are negatively signed, but when they are positively signed, the magnitudes are in the range of about 0.01 and 0.2, which are much closer to what theory would predict.[24]

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

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 (crude) theoretical estimate of 0.1 that we derive.

V. 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 and by addressing the problem of the endogeneity of aid.

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 instrumentation 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;
- cross-section and panel contexts
- types of aid distinguished by:
    - what it is used for (economic, social, food, etc.);
    - who gives it (multilateral donors, bilateral donors, good donors and others);

- 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 have effect (contemporaneous versus lagged).

In sum, we find that it is difficult to discern any systematic effect of aid on growth. 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).

Table 1: Summary Statistics

Variable	*Mean*	*Std. Dev.*	*Min*	*Max*
1960-00 *(74 Obs.)*				
Real annual average per capita GDP growth (PPP)	1.559	1.739	-3.373	6.794
Aid to GDP	6.105	6.949	0.087	28.378
Fitted aid to GDP	5.201	4.829	-7.046	19.707
Initial level of per capita (PPP) GDP	7.399	0.687	6.037	8.967
Initial level of trade policy	0.257	0.258	0.000	1.000
Initial level of life expectancy at birth	48.391	9.796	32.380	71.680
Geography	-0.572	0.742	-1.040	1.528
Institutional quality	0.528	0.123	0.225	0.859
Initial inflation	14.755	32.605	-0.835	173.199
Initial M2/GDP	19.838	12.609	2.628	72.980
Initial budget balanc/GDP	-3.874	4.936	-23.145	5.837
Average no. of revolutions	0.222	0.188	0.000	0.829
Ethnic fractionalization	0.479	0.288	0.004	0.902
Mult. aid/GDP	1.916	2.550	0.006	9.797
Bilat. aid/GDP	3.917	4.199	0.072	19.388
1980-00 *(77 Obs.)*				
Real annual average per capita GDP growth (PPP)	0.931	2.177	-5.557	6.273
Aid to GDP	5.430	7.098	0.049	43.853
Fitted aid to GDP	4.406	4.177	-6.450	19.087
Initial level of per capita (PPP) GDP	7.886	0.833	6.094	9.347
Initial level of trade policy	0.434	0.349	0.000	1.000
Initial level of life expectancy at birth	57.426	9.817	35.400	74.600
Geography	-0.490	0.807	-1.040	1.528
Institutional quality	0.534	0.128	0.225	0.859
Initial inflation	32.531	57.411	-0.835	351.970
Initial M2/GDP	30.675	15.647	8.133	78.361
Initial budget balanc/GDP	-4.956	6.363	-39.088	3.384
Average no. of revolutions	0.248	0.264	0.000	1.286
Ethnic fractionalization	0.460	0.300	0.004	0.902
Mult. aid/GDP	2.170	3.226	0.001	17.184
Bilat. aid/GDP	3.768	4.673	0.047	29.702
Panel *(239 Obs.)*				
Real annual average per capita GDP growth (PPP)	1.032	3.254	-11.518	15.762
Aid to GDP	4.689	6.756	0.011	50.069
Fitted aid to GDP	7.407	15.744	0.099	150.308
Initial level of per capita (PPP) GDP	8.062	0.810	6.094	9.794
Initial level of trade policy	0.364	0.482	0.000	1.000
Initial level of life expectancy at birth	61.425	9.564	35.200	77.350
Geography	-0.409	0.837	-1.040	1.784
Institutional quality	0.485	0.172	0.056	0.912
Initial inflation	0.277	0.544	-0.005	4.192
Initial M2/GDP	35.097	20.753	3.932	124.251
Initial budget balanc/GDP	-2.841	4.191	-21.556	14.526
Average no. of revolutions	0.244	0.414	0.000	2.600
Ethnic fractionalization	0.422	0.300	0.004	0.902
Mult. aid/GDP	1.510	2.803	0.000	21.770
Bilat. aid/GDP	3.015	4.070	0.019	26.169

Table 2: Impact of Total Aid on Growth, OLS Estimations

(Dependent variable is average annual growth of per capita GDP)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	-0.063	-0.076	-0.120	-0.008
	(0.028)**	(0.043)*	(0.069)*	(0.048)
Initial per cap. GDP	-1.332	-1.668	-1.632	-1.147
	(0.284)***	(0.317)***	(0.376)***	(0.559)**
Initial level of policy (Sachs-Warner)	1.788	2.278	2.303	-0.159
	(0.431)***	(0.472)***	(0.773)***	(0.551)
Initial level of life expectancy	0.024	0.016	0.063	0.151
	(0.020)	(0.030)	(0.041)	(0.063)**
Geography	0.346	0.386	0.505	0.693
	(0.132)**	(0.179)**	(0.224)**	(0.408)*
Institutional quality	3.944	4.023	1.361	2.951
	(1.490)**	(2.223)*	(2.281)	(3.146)
Initial Inflation	-0.003	-0.004	-0.001	-0.001
	(0.002)	(0.003)	(0.002)	(0.000)***
Initial M2/GDP	0.017	0.016	-0.010	-0.003
	(0.010)	(0.015)	(0.023)	(0.014)
Initial Budget Balance/GDP	-0.007	-0.014	-0.023	0.205
	(0.024)	(0.033)	(0.035)	(0.059)***
Revolutions	-1.261	-1.310	-0.669	-0.491
	(0.506)**	(0.488)***	(0.627)	(0.652)
Ethnic Fractionalization	-0.102	-0.391	0.045	1.742
	(0.448)	(0.704)	(0.903)	(1.084)
Observations	74	78	75	70
R-squared	0.77	0.70	0.64	0.63

All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. All specifications include dummies for sub-Saharan African and East Asian countries. For descriptions of the variables and their sources, see Appendix 1. The sample decreases for the 1980-00 and 1990-00 time periods because data on budget balance becomes sparser for the 1980s and 1990s, so the initial value of budget balance cannot be computed for these periods. Outliers are excluded based on the Hadi (1992) procedure on the core IV specification in Table 4, which results in two countries being dropped from the 1980-00 horizon in all the results that are presented.

Table 3: Estimation of Exogenous Variation in the Allocation of Aid by Donors Across Recipients

(Dependent variable is aid / recipient GDP)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Dummy for pairs that ever had a colonial relationship	0.017	0.017	0.014	0.014
	(0.003)***	(0.002)***	(0.002)***	(0.003)***
Dummy for pairs currently in a colonial relationship	-0.017	-0.010	-0.015	-0.011
	(0.005)***	(0.006)*	(0.006)**	(0.008)
Dummy for pairs that have common language (Language Dummy)	0.001	0.001	0.001	0.000
	(0.000)*	(0.000)*	(0.000)	(0.000)
Dummy for country that ever had a colonial relationship with UK	-0.019	-0.020	-0.015	-0.010
	(0.003)***	(0.003)***	(0.003)***	(0.003)***
Dummy for country that ever had a colonial relationship with France	-0.045	-0.035	-0.026	-0.016
	(0.005)***	(0.005)***	(0.004)***	(0.005)***
Dummy for country that ever had a colonial relationship with Spain	-0.034	-0.029	-0.023	-0.019
	(0.005)***	(0.004)***	(0.004)***	(0.004)***
Dummy for country that ever had a colonial relationship with Portugal	0.002	0.008	0.016	0.025
	(0.005)	(0.004)*	(0.004)***	(0.005)***
Ratio of logarithm of population of donor relative to recipient	0.001	0.001	0.001	0.001
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
Ratio of logarithm of population of donor relative to recipient*colony dummy	0.006	0.006	0.005	0.006
	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Ratio of logarithm of population of donor relative to recipient*UK colony dummy	-0.004	-0.004	-0.004	-0.006
	(0.001)***	(0.001)***	(0.001)***	(0.001)***
Ratio of logarithm of population of donor relative to recipient*Spanish colony dummy	0.004	0.002	0.001	-0.002
	(0.002)*	(0.002)	(0.002)	(0.002)
Ratio of logarithm of population of donor relative to recipient*French colony dummy	0.013	0.010	0.010	0.007
	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Ratio of logarithm of population of donor relative to recipient*Portugese colony dummy	0.027	0.029	0.025	0.026
	(0.002)***	(0.002)***	(0.002)***	(0.002)***
Observations	3288	3288	3288	3249
R-squared	0.41	0.42	0.38	0.33

Estimation is by ordinary least squares. The estimated equation corresponds to equation 1 in Section II of the paper. All standard errors are robust. Absolute value of 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 of per capita GDP)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	0.063	0.096	-0.004	-0.389
	(0.061)	(0.070)	(0.095)	(0.194)**
Initial per cap. GDP	-1.175	-1.409	-1.454	-2.193
	(0.387)***	(0.435)***	(0.446)***	(0.692)***
Initial level of policy (Sachs-Warner)	1.620	2.139	2.332	-0.065
	(0.666)**	(0.619)***	(0.835)***	(0.726)
Initial level of life expectancy	0.059	0.076	0.102	0.047
	(0.028)**	(0.039)*	(0.050)**	(0.089)
Geography	0.526	0.606	0.605	0.211
	(0.187)***	(0.259)**	(0.255)**	(0.421)
Institutional quality	4.558	4.077	0.843	6.437
	(1.698)***	(2.328)*	(2.484)	(3.588)*
Initial Inflation	-0.003	-0.005	-0.002	-0.001
	(0.004)	(0.005)	(0.003)	(0.001)*
Initial M2/GDP	0.017	0.010	-0.011	-0.003
	(0.012)	(0.020)	(0.025)	(0.014)
Initial Budget Balance/GDP	0.016	0.016	0.011	0.195
	(0.029)	(0.036)	(0.042)	(0.093)**
Revolutions	-1.144	-1.406	-0.719	-0.350
	(0.618)*	(0.656)**	(0.670)	(0.778)
Ethnic Fractionalization	0.712	0.788	0.818	-0.092
	(0.609)	(0.851)	(1.055)	(1.414)
Observations	74	78	75	70
R-squared	0.66	0.59	0.61	0.37

Table 4B: Impact of Total Aid on Growth, First Stage Regressions

(Dependent variable is average of the ratio of total aid to GDP)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Fitted Aid/GDP	0.695	0.639	0.603	0.494
	(0.139)***	(0.114)***	(0.111)***	(0.169)***
Observations	74	78	75	70
R-squared	0.69	0.75	0.81	0.68
F-value	25.20	31.58	29.38	8.53

All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. All specifications include dummies for sub-Saharan African and East Asian countries. The instrument for aid corresponds to that in (1) and (2) in Section II of the paper. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates in the first-stage regression are omitted for presentational convenience. Throughout the paper, the values of the potentially endogenous covariates are for the beginning of the relevant time period (or strictly speaking the first available value closest to the initial year of the relevant time period). Outliers are excluded based on the Hadi (1992) procedure. For descriptions of the variables and their sources, see Appendix 1.

Table 5A: Impact of Total Aid on Growth, IV Estimations

(Dependent variable is average annual growth of per capita GDP)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	0.051	0.080	-0.052	-0.308
	(0.052)	(0.065)	(0.090)	(0.158)*
Colony UK	0.019	0.557	0.905	-0.604
	(0.445)	(0.538)	(0.548)	(0.785)
Colony France	0.418	0.931	0.585	-1.461
	(0.500)	(0.656)	(0.559)	(0.741)*
Colony Spain	0.407	0.225	-0.941	-1.096
	(0.544)	(0.674)	(0.517)*	(1.142)
Observations	74	78	75	70
R-squared	0.69	0.63	0.67	0.50

This specification is exactly as in Table 4A except for the addition of three colony dummies.

Table 5B: Impact of Total Aid on Growth, IV Estimations (Second-Stage)

(Dependent variable is average annual growth of per capita GDP)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	0.052	0.096	0.008	-0.251
	(0.052)	(0.068)	(0.094)	(0.150)
Observations	74	78	75	70
R-squared	0.68	0.59	0.60	0.52
P-value for test of over-identifying restrictions	0.52	0.90	0.14	0.05

Table 5C: Impact of Total Aid on Growth, IV Estimations (First Stage)

(Dependent variable is average of the ratio of total aid to GDP)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Fitted Aid/Initial GDP	0.560	0.596	0.569	0.279
	(0.171)***	(0.145)***	(0.143)***	(0.207)
Log Area	-0.582	-0.160	-0.097	-0.613
	(0.436)	(0.332)	(0.259)	(0.354)*
Observations	74	78	75	70
R-squared	0.70	0.75	0.81	0.70

In Tables 5B and 5C, there are two instruments for aid. The first is based on equation 1 in the text and the second is the logarithm of a country's area (in sq. kms.). Table 5B is the second stage while Table 5C is the first stage.

Table 5D: Impact of Total Aid on Growth, IV Estimations (Second Stage)
(Dependent variable is average annual growth of per capita GDP)

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	-0.091	-0.006	2.367	0.157
	(0.152)	(0.761)	(7.554)	(0.286)
Fitted Aid/GDP	0.107	0.065	-1.428	-0.270
	(0.116)	(0.494)	(4.547)	(0.158)*
Observations	74	78	75	70
R-squared	0.81	0.69	n.a.	0.65

In Table 5D, aid is instrumented by the logarithm of a country's area (in sq. kms.) while fitted aid is the instrument based on equation 1 in the paper.

Table 6: The Effect of Aid Under Alternative Assumptions About Timing of Impact: OLS Estimations

(Dependent variable is average annual growth of per capita GDP)

	Growth Horizon=10 years				Growth Horizon=20 years		Growth Horizon=30 years		Growth Horizon=40 years
	(1) 1960-70	(2) 1970-80	(3) 1980-90	(4) 1990-00	(5) 1960-80	(6) 1980-00	(7) 1960-90	(8) 1970-00	(9) 1960-00
Aid Horizon=10 years									
1960-70	-0.163 (0.127)	0.061 (0.154)	-0.281 (0.116)**	-0.082 (0.087)	0.027 (0.073)	-0.171 (0.104)	-0.078 (0.058)	-0.072 (0.072)	-0.088 (0.056)
1970-80		0.049 (0.080)	-0.153 (0.065)**	-0.035 (0.060)	0.012 (0.042)	-0.047 (0.051)	-0.046 (0.034)	-0.013 (0.043)	-0.046 (0.032)
1980-90	n.a.	n.a.	-0.182 (0.057)***	-0.028 (0.067)	n.a.	-0.062 (0.046)	n.a.	n.a.	n.a.
1990-00	n.a.	n.a.	n.a.	-0.003 (0.094)	n.a.	n.a.	n.a.	n.a.	n.a.
Aid Horizon=20 years									
1960-80	n.a.	n.a.	-0.117 (0.051)**	-0.025 (0.047)	0.019 (0.034)	-0.033 (0.040)	-0.049 (0.024)**	-0.007 (0.036)	-0.032 (0.027)
1980-00	n.a.	n.a.	n.a.	-0.017 (0.099)		-0.120 (0.069)*	n.a.	n.a.	n.a.
Aid Horizon=30 years									
1960-90	n.a.	n.a.	n.a.	-0.014 (0.058)	n.a.	n.a.	-0.059 (0.024)**	n.a.	-0.043 (0.019)**
1970-00	n.a.	n.a.	n.a.	-0.022 (0.093)	n.a.	n.a.	n.a.	-0.094 (0.040)**	-0.085 (0.029)***
Aid Horizon=40 years									
1960-00	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	-0.076 (0.027)***

All standard errors are robust, and are reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates are omitted for presentational simplicity. Each entry corresponds to the coefficient of aid in a regression whose time period is denoted by the column heading and where the aid variable is averaged over the period denoted by the row heading. For descriptions of the variables and their sources, see Appendix 1.

Table 7: Aid and Growth: Diminishing Returns and Conditional Impacts, IV Estimations
(Dependent variable is average annual growth of per capita GDP)

Panel 7A: Aid-square term

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	-0.258	-0.169	8.573	1.044
	(0.191)	(0.166)	(47.949)	(2.797)
Aid/GDP-squared	0.013	0.011	-0.576	-0.081
	(0.010)	(0.008)	(3.165)	(0.145)
Observations	74	78	75	70
R-squared	0.60	0.58		

Robust standard errors in parentheses; *,**, and *** denote significance at 10 %, 5%, and 1%, respectively

Panel 7B: Aid interacted with Sachs-Warner Policy measure

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	0.148	0.141	-0.037	-0.460
	(0.161)	(0.153)	(0.107)	(0.241)*
Policy (Sachs-Warner)	2.943	2.687	-0.492	-2.340
	(1.539)*	(1.079)**	(2.205)	(2.019)
Aid/GDP*policy	-0.356	-0.144	1.747	0.610
	(0.489)	(0.361)	(1.462)	(0.564)
Observations	74	78	75	70
R-squared	0.58	0.59	0.32	0.24

Robust standard errors in parentheses; *,**, and *** denote significance at 10 %, 5%, and 1%, respectively

Panel 7C: Aid interacted with Geography

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	0.034	0.093	-0.120	-0.490
	(0.058)	(0.081)	(0.159)	(0.180)***
Geography	0.644	0.617	0.972	0.582
	(0.223)***	(0.249)**	(0.310)***	(0.487)
Aid/GDP*Geography	-0.043	-0.004	-0.187	-0.180
	(0.054)	(0.068)	(0.156)	(0.147)
Observations	74	78	75	70
R-squared	0.63	0.59	0.53	0.47

All standard errors are robust, and are reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid corresponds to equation 3 in Section II of the paper. Other covariates are omitted for presentational simplicity. For descriptions of the variables and their sources, see Appendix 1.

Table 8: Impact of Different Categories of Aid

Panel 8A: Multilateral and Bilateral Aid

	Multilateral Aid				Bilateral Aid			
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
	1960_00	**1970_00**	**1980_00**	**1990_00**	**1960_00**	**1970_00**	**1980_00**	**1990_00**
Mult. aid/GDP	0.184	0.241	-0.062	-0.702				
	(0.189)	(0.206)	(0.207)	(0.374)*				
Bilat. aid/GDP					0.091	0.139	0.003	-0.504
					(0.087)	(0.098)	(0.139)	(0.249)**
Observations	74	78	75	70	74	78	75	70
R-squared	0.65	0.57	0.62	0.42	0.67	0.60	0.60	0.39

Panel 8B: Impact of Aid Given by Top 5 "Good Donors," OLS Regression Results

(Dependent variable is average annual growth of per capita GDP)

	Growth Horizon=10 years				Growth Horizon=20 years		Growth Horizon=30 years		Growth Horizon=40 years
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	1960-70	1970-80	1980-90	1990-00	1960-80	1980-00	1960-90	1970-00	1960-00
Aid Horizon=10 years									
1960-70	-1.122	7.312	-2.304	1.695	2.587	-0.381	0.054	0.982	0.755
	(4.041)	(4.227)*	(4.011)	(3.525)	(2.802)	(2.343)	(2.417)	(1.939)	(1.676)
1970-80	n.a.	0.623	-1.375	-0.593	-0.071	-1.088	-0.378	-0.203	-0.428
		(0.658)	(0.590)**	(0.559)	(0.342)	(0.701)	(0.264)	(0.367)	(0.284)
1980-90	n.a.	n.a.	-0.566	0.735	n.a.	-0.291	n.a.	n.a.	n.a.
			(0.326)*	(0.613)		(0.313)			
1990-00	n.a.	n.a.	n.a.	0.721	n.a.	n.a.	n.a.	n.a.	n.a.
				(0.527)					
Aid Horizon=20 years									
1960-80	n.a.	n.a.	-1.066	-0.421	-0.012	-0.824	-0.261	-0.168	0.055
			(0.433)**	(0.401)	(0.234)	(0.514)	(0.191)	(0.277)	(0.239)
1980-00	n.a.	n.a.	n.a.	0.411		-0.231	n.a.	n.a.	n.a.
				(0.528)		(0.372)			
Aid Horizon=30 years									
1960-90	n.a.	n.a.	n.a.	0.473	n.a.	n.a.	-0.458	n.a.	-0.165
				(0.779)			(0.360)		(0.165)
1970-00	n.a.	n.a.	n.a.	0.461	n.a.	n.a.		-0.325	-0.206
				(0.705)				(0.323)	(0.302)
Aid Horizon=40 years									
1960-00	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	n.a.	-0.169
									(0.270)

Panel 8C: Social and Economic Aid

	Social			Economic		
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	1970_00	**1980_00**	**1990_00**	**1970_00**	**1980_00**	**1990_00**
Social sector aid/GDP	0.359	-0.048	-1.148			
	(0.280)	(0.420)	(0.516)**			
Economic aid/GDP				0.088	-0.020	-0.476
				(0.067)	(0.090)	(0.354)
Observations	78	75	70	78	75	70
R-squared	0.64	0.61	0.45	0.60	0.61	0.30

Panel 8D: Late-Impact and Early-Impact Aid

	Late-impact			**Early-impact**		
	(1) **1970_00**	**(2)** **1980_00**	**(3)** **1990_00**	**(4)** **1970_00**	**(5)** **1980_00**	**(6)** **1990_00**
Late-impact aid/GDP	0.170	-0.007	-0.478			
	(0.111)	(0.146)	(0.228)**			
Early-impact aid/GDP				0.197	-0.026	-0.653
				(0.141)	(0.169)	(0.397)
Observations	71	71	70	71	71	70
R-squared	0.64	0.60	0.40	0.61	0.61	0.36

All standard errors are robust, and are reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. Other covariates are omitted for presentational simplicity. In Panel 8B, top 5 donors comprise Denmark, Finland, Norway, Sweden, and Iceland. In Table 8B, estimation uses OLS. For descriptions of the variables and their sources, see Appendix 1.

Table 9: GMM Panel Regressions (Arellano-Bond Procedure)

(Dependent variable is average annual growth of per capita GDP)

	(1)	(2)	(3)	(4)
Aid/GDP	-0.151	-0.015	-0.168	0.163
	(0.077)**	(0.207)	(0.140)	(0.140)
Policy (Sachs-Warner)	-1.774	-1.326	-1.309	-0.990
	(0.933)*	(0.843)	(0.993)	(1.129)
Aid/GDP-squared		-0.005		
		(0.005)		
Aid/GDP*policy			-0.022	
			(0.050)	
Aid/GDP*Geography				0.376
				(0.113)***
Initial per cap. GDP	-8.347	-7.998	-7.772	-8.165
	(1.543)***	(1.414)***	(1.552)***	(1.260)***
Initial level of life expectancy	-0.393	-0.209	-0.229	-0.213
	(0.183)**	(0.157)	(0.156)	(0.153)
Institutional quality	6.953	5.665	6.093	6.575
	(2.767)**	(2.225)**	(2.350)***	(2.392)***
Log Inflation	-1.985	-1.838	-1.978	-1.687
	(0.671)***	(0.596)***	(0.882)**	(0.829)**
M2/GDP	-0.002	-0.015	-0.015	-0.001
	(0.032)	(0.037)	(0.036)	(0.031)
Budget Balance/GDP	0.164	0.117	0.141	0.139
	(0.082)**	(0.076)	(0.070)**	(0.082)*
Revolutions	-0.972	-1.174	-1.321	-1.427
	(0.625)	(0.624)*	(0.831)	(0.675)**
Observations	167	167	167	167
Number of Groups	68	68	68	68
Chi-Square (Hansen over-id test)	0.485	0.423	0.544	0.536
AR(2) (test for serial correlation)	0.198	0.269	0.255	0.199
GMM estimation method	Difference	Difference	Difference	Difference
Endogenous variables used as instruments	Initial income, aid, policy, inst. quality, inflation, M2, budget balance, revolutions, life expectancy	Initial income, aid, policy, inst. quality, inflation, M2, budget balance, revolutions, life expectancy	Initial income, aid, policy, inst. quality, inflation, M2, budget balance, revolutions, life expectancy	Initial income, aid, policy, inst. quality, inflation, M2, budget balance, revolutions, life expectancy
No. of lags of endogenous varuiables used in instrumentation	Eight	Eight	Eight	Eight
Exogenous variable used as instrument	Ethnic Geography	Ethnic Geography	Ethnic Geography	Ethnic Geography

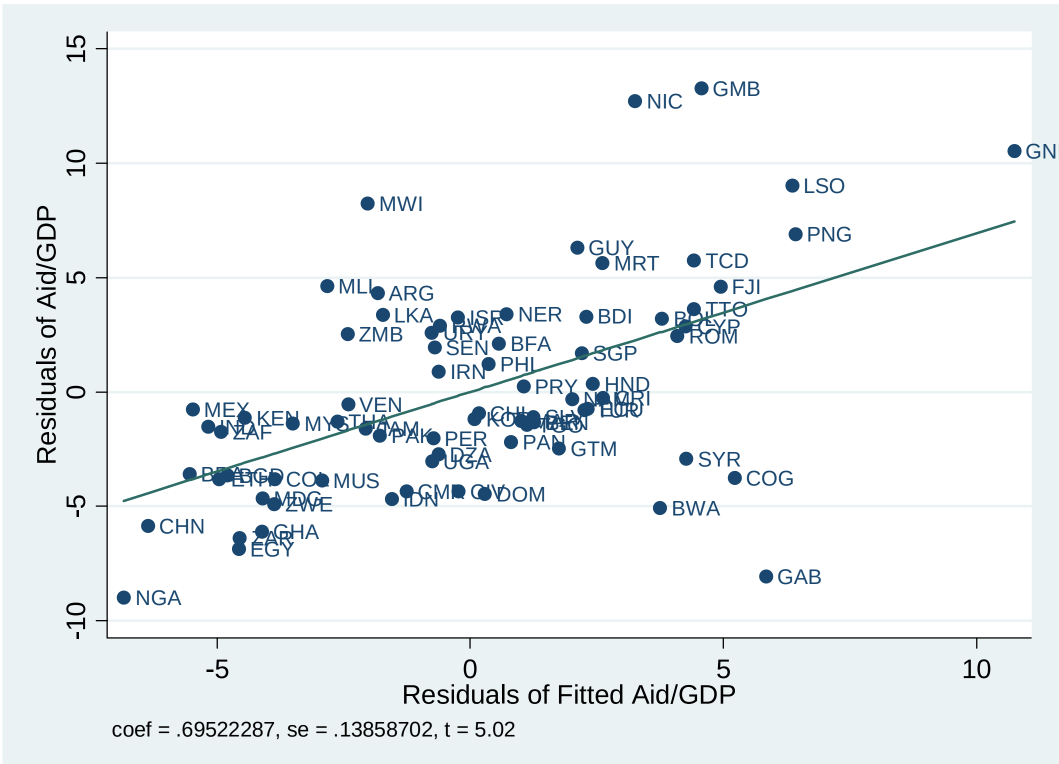
All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. Regressions use the Arellano and Bond (1991) difference GMM estimator. For descriptions of the variables and their sources, see Appendix 1.

Table 10: GMM Panel Regressions (Blundell-Bond procedure)
(Dependent variable is average annual growth of per capita GDP)

	(1)	(2)	(3)	(4)
Aid/GDP	-0.054	-0.187	-0.046	-0.165
	(0.114)	(0.135)	(0.119)	(0.087)*
Policy (Sachs-Warner)	1.370	0.722	1.026	0.855
	(1.015)	(0.933)	(0.845)	(0.900)
Geography	0.496	0.490	0.596	0.717
	(0.353)	(0.335)	(0.340)*	(0.353)**
Aid/GDP-squared		0.005		
		(0.004)		
Aid/GDP*policy			-0.046	
			(0.092)	
Aid/GDP*Geography				-0.122
				(0.070)*
Initial per cap. GDP	-2.456	-2.536	-2.497	-1.994
	(1.057)**	(0.603)***	(0.819)***	(0.694)***
Initial level of life expectancy	0.086	0.076	0.105	0.093
	(0.098)	(0.080)	(0.098)	(0.087)
Institutional quality	2.748	3.173	3.644	3.705
	(2.579)	(1.932)	(2.327)	(2.211)*
Log Inflation	-1.498	-1.812	-1.685	-1.693
	(0.663)**	(0.627)***	(0.692)**	(0.580)***
M2/GDP	0.010	0.008	-0.003	0.010
	(0.021)	(0.017)	(0.017)	(0.016)
Budget Balance/GDP	0.101	0.111	0.168	0.138
	(0.075)	(0.070)	(0.068)**	(0.082)*
Revolutions	-0.073	-0.184	-0.301	-0.508
	(0.992)	(0.437)	(0.582)	(0.763)
Ethnic Fractionalization	0.129	-0.178	0.331	1.246
	(1.809)	(2.139)	(1.870)	(2.552)
Observations	239	239	239	239
Number of Groups	72	72	72	72
Chi-Square (Hansen over-id test)	0.391	0.598	0.287	0.371
AR(2) (test for serial correlation)	0.298	0.224	0.287	0.285
GMM estimation method	System	System	System	System
Endogenous variables used as instruments	Initial income, aid, policy, inst. quality, inflation, M2, budget balance, revolutions, life expectancy	Initial income, aid, policy, inst. quality, inflation, M2, budget balance, revolutions, life expectancy	Initial income, aid, policy, inst. quality, inflation, M2, budget balance, revolutions, life expectancy	Initial income, aid, policy, inst. quality, inflation, M2, budget balance, revolutions, life expectancy
No. of lags of endogenous varuiables used in instrumentation	Eight	Eight	Eight	Eight
Exogenous variable used as instrument	Ethnic Geography	Ethnic Geography	Ethnic Geography	Ethnic Geography

All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. Regressions use the Blundell-Bond (1998) system GMM estimator. For descriptions of the variables and their sources, see Appendix 1.

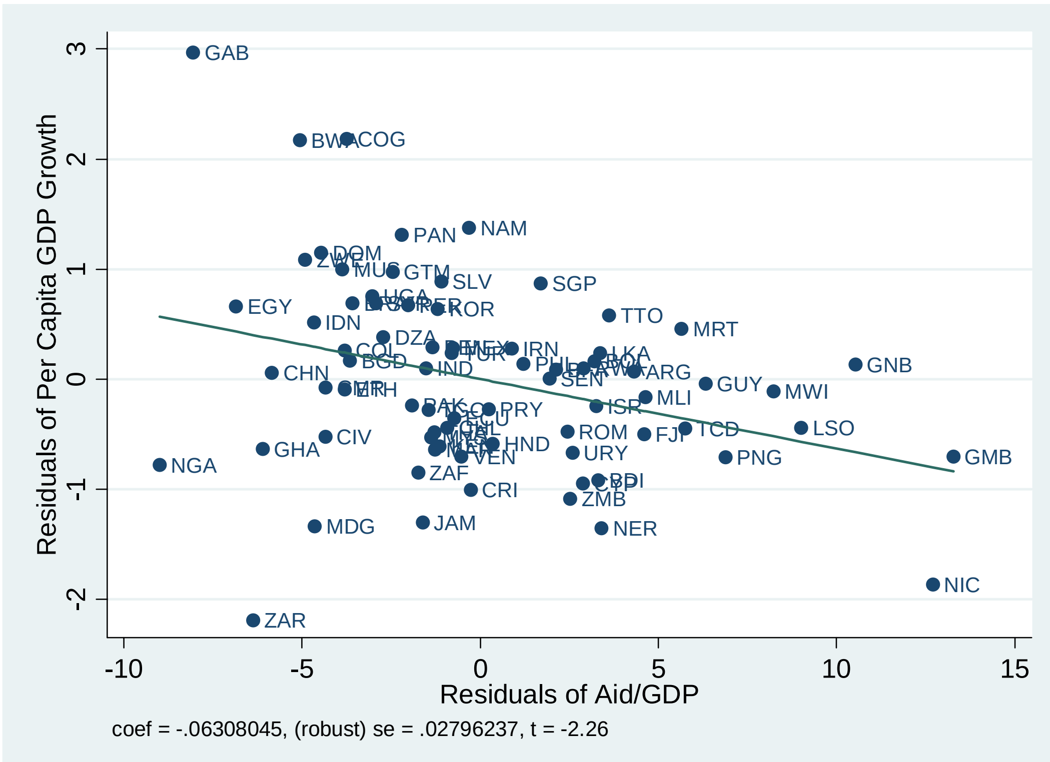
Chart 1: Conditional Relationship between Aid and its Instrument, 1960-00



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).

Chart 2: Conditional Correlation between Growth and Total Aid, 1960-00

(OLS Estimation)



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.

Appendix 1. Data Description and Sources

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

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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.*

Easterly website *(Macro Time Series 2005)*

Variable Name	Variable Description	Source
Real economic growth	Annual average growth rate of real GDP (PPP) per capita where the averages are taken over the relevant time period. Countries are included in the sample for the 1960-00 horizon if there are data for at least 35 years; for the 1970-00 horizon for at least 25 years of data; for the 1980-00 horizon for at least 15 years of data; for the 1990-00 horizon for at least 5 years; and for the panel for at least 3 years.	PWT, version 6.1

Aid to GDP	The ratio of aggregate net development assistance that is disbursed in current US dollars to GDP in current US dollars. It includes all loan and grants undertaken by the official sector with the promotion of economic development as the main objective and where loans have a grant element of at least 25 percent. In all the regressions, the aid-to-GDP is averaged over the relevant time period in the regression. If the aid horizon is 1960-2000, then the aid-to-GDP variable in the regression is average over this period of the annual aid-to-GDP ratios. The variables are averaged similarly (i.e over 5 years) in the panel regressions. These averaging procedures are used for all the aid variables.	OECD, DAC
Bilateral aid to GDP	Bilateral aid includes aid from 22 donor countries defined in the OECD's DAC.	OECD, DAC
Multilateral aid to GDP	Multilateral aid is identified in the OECD's DAC database and includes assistance from the World Bank, and the regional development banks.	
Social sector aid to GDP	Social sector aid includes assistance for education, health and population and water supply and sanitation. Data on this category of aid is in terms of commitments. This was converted to disbursements by taking the ratio of commitments in this sector to overall commitments and then multiplying this by aggregate aid disbursements.	OECD, DAC
Economic aid to GDP	Economic aid includes assistance for energy and transport and communications. Data on this category of aid is in terms of commitments. This was converted to disbursements by taking the ratio of commitments in this sector to overall commitments and then	OECD, DAC

	multiplying this by aggregate aid disbursements.	
Initial GDP	Log of per capita (PPP) GDP at the beginning of the relevant time period. For example, for the horizon, initial GDP is for the year 1960.	PWT, 6.1
Life expectancy	Life expectancy at birth in years at the beginning of the relevant time period. For example, for the horizon, initial life expectancy is for the year 1960 or for the closest year for which data are available. The same procedure is applied in the panel estimations.	WDI
Geography	Average of number of frost days and tropical land area	Bosworth & Collins, 2003
Institutional quality	For the cross-section, the data are from Bosworth and Collins who use the ICRGE index averaged over the period 1986-1995. For the GMM regressions, data are averages for the relevant 5-year period.	Bosworth & Collins, 2003
Inflation	The average annual rate of growth of CPI-based inflation for the first five years of the relevant time horizon. For example, for the period 1960-00, this variable is measured for the period 1960-64. In the panel, the inflation measures are averages of the relevant 5-year period.	Easterly's website (www.nyu.edu/fas/institute /dri/global
Financial depth	The ratio of M2/GDP for the first five years of the relevant time horizon. In the panel, averages of the relevant 5-year period are used.	Easterly's website
Budget balance	The ratio of general government budget balance to GDP for the first five years of the relevant time horizon. In the panel, averages of the relevant 5-year period are used.	WDI
Revolutions	The average number of revolutions per year in the relevant time horizon. Revolutions are defined as any illegal or forced change in	Arthur S. Banks

	the top governmental elite, any attempt at such a change, or any successful or unsuccessful armed rebellion whose aim is independence from the central government.	
Initial policy	The Sachs-Warner trade policy index as updated by Wacziarg and Welch and prevailing at the beginning of the relevant time horizon or the year closest to it.	Wacziarg & Welch, 2003
Colony	Dummy that takes a value of 1 if donor and recipient country were ever in colonial relationship	Rose, 2004
Current colony	Dummy that takes a value of 1 if donor and recipient country enjoy a current colonial relationship	Rose, 2004

Appendix 2. Sample of Countries

Country	1960-00	1980-00	Panel	Country	1960-00	1980-00	Panel
Algeria	yes	yes	yes	Korea, Rep.	yes	yes	yes
Argentina	yes	yes	yes	Lesotho	yes	yes	no
Bangladesh	yes	yes	yes	Madagascar	yes	yes	yes
Benin	yes	no	no	Malawi	yes	yes	yes
Bolivia	yes	yes	yes	Malaysia	yes	yes	yes
Botswana	yes	yes	yes	Mali	yes	yes	yes
Brazil	yes	yes	yes	Mauritania	yes	no	no
Bulgaria	no	no	yes	Mauritius	yes	yes	no
Burkina Faso	yes	yes	yes	Mexico	yes	yes	yes
Burundi	yes	yes	no	Morocco	yes	yes	yes
Cameroon	yes	yes	yes	Namibia	yes	yes	yes
Chad	yes	yes	no	Nicaragua	yes	yes	yes
Chile	yes	yes	yes	Niger	yes	yes	no
China	yes	yes	yes	Nigeria	yes	yes	yes
Colombia	yes	yes	yes	Pakistan	yes	yes	yes
Congo, Dem. Rep.	yes	yes	yes	Panama	yes	yes	yes
Congo, Rep.	yes	yes	yes	Papua New Guinea	yes	yes	yes
Costa Rica	yes	yes	yes	Paraguay	yes	yes	yes
Cote d'Ivoire	yes	yes	yes	Peru	yes	yes	yes
Cyprus	yes	yes	yes	Philippines	yes	yes	yes
Dominican Republic	yes	yes	yes	Poland	no	yes	yes
Ecuador	yes	yes	yes	Romania	yes	yes	yes
Egypt, Arab Rep.	yes	yes	yes	Russian Federation	no	no	yes

El Salvador	yes	yes	yes	Rwanda	yes	yes	no
Ethiopia	yes	yes	yes	Senegal	yes	yes	yes
Fiji	yes	yes	no	Sierra Leone	no	yes	yes
Gabon	yes	yes	yes	Singapore	yes	yes	yes
Gambia, The	yes	yes	yes	South Africa	yes	yes	yes
Ghana	yes	yes	yes	Sri Lanka	yes	yes	yes
Guatemala	yes	yes	yes	Syrian Arab Republic	yes	yes	yes
Guinea-Bissau	yes	yes	yes	Thailand	yes	yes	yes
Guyana	yes	yes	no	Togo	yes	yes	yes
Haiti	no	yes	yes	Trinidad & Tobago	yes	yes	yes
Honduras	yes	no	no	Tunisia	no	yes	yes
Hungary	no	yes	yes	Turkey	yes	yes	yes
India	yes	yes	yes	Uganda	yes	yes	yes
Indonesia	yes	yes	yes	Uruguay	yes	yes	yes
Iran, Islamic Rep.	yes	yes	yes	Venezuela, RB	yes	yes	yes
Israel	yes	yes	yes	Yemen, Rep.	no	no	yes
Jamaica	yes	yes	yes	Zambia	yes	yes	yes
Jordan	no	yes	yes	Zimbabwe	yes	yes	yes
Kenya	yes	yes	yes				

Appendix 3. Prediction of the Standard Growth Model of the Quantitative Impact of Aid [25]

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.

$$Y = AK^{\alpha} L^{1-\alpha} \tag{1}$$

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

The equation for capital accumulation is:

$$\dot{K} = I - \delta(K) = Ig + Ip - \delta(Kp + Kg) \tag{2}$$

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:

$$\dot{K} = \beta Aid + Ip - \delta K \text{ and}$$

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

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

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:

$$\gamma_y = \frac{\dot{y}}{y} = \alpha(\frac{\dot{k}}{k} + \frac{\dot{A}}{A}) \tag{4}$$

Substituting equation (3) in (4) yields

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

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:

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

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

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

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.

It is possible that equation 7 underestimates the value of aid because it ignores the fact that the public investment financed by aid has spillovers and hence economy-wide productivity impacts. Incorporating this would yield the following variant of equation 7:

$$\frac{\delta \gamma_y}{\delta(\frac{Aid}{Y})} = \alpha\beta\frac{Y}{K} + \frac{\delta(\dot{A}/A)}{\delta(\frac{Aid}{Y})} \qquad (7)'$$

The last term on the right hand side captures the effect of aid on productivity growth. It is difficult to know whether and to what extent public investment has had such productivity impacts in aid-receiving countries. The IT revolution in the US since the mid-1990s added about 0.5 percent per year to productivity growth. Extrapolating from this, it seems that an upper limit for the impact of aid on growth would be about 0.2-0.25 percent per year for every 1 percentage point increase in the received aid to GDP ratio.

Appendix Table 1. Estimation of Exogenous Variation in the Allocation of Social Aid by Donors Across Recipients

(Dependent Variable is share of donor's social aid to recipient)

	(1) 1970_00	(2) 1980_00	(3) 1990_00
Dummy for pairs that ever had a colonial relationship	0.009 (0.001)***	0.007 (0.001)***	0.007 (0.001)***
Dummy for pairs currently in a colonial relationship	0.001 (0.004)	-0.006 (0.005)	-0.005 (0.007)
Dummy for pairs that have common language (Language Dummy)	0.001 (0.000)***	0.001 (0.000)***	0.001 (0.000)***
Dummy for country that ever had a colonial relationship with UK	-0.010 (0.002)***	-0.008 (0.002)***	-0.007 (0.002)***
Dummy for country that ever had a colonial relationship with France	-0.018 (0.002)***	-0.015 (0.002)***	-0.012 (0.002)***
Dummy for country that ever had a colonial relationship with Spain	-0.019 (0.002)***	-0.016 (0.002)***	-0.012 (0.002)***
Dummy for country that ever had a colonial relationship with Portugal	-0.001 (0.002)	0.002 (0.002)	0.006 (0.002)***
Ratio of logarithm of population of donor relative to recipient	0.000 (0.000)***	0.000 (0.000)***	0.000 (0.000)***
Ratio of logarithm of population of donor relative to recipient*colony dummy	0.003 (0.001)***	0.003 (0.001)***	0.003 (0.001)***
Ratio of logarithm of population of donor relative to recipient*UK colony dummy	-0.002 (0.001)***	-0.002 (0.001)***	-0.003 (0.001)***
Ratio of logarithm of population of donor relative to recipient*Spanish colony dummy	0.004 (0.001)***	0.003 (0.001)***	0.000 (0.001)
Ratio of logarithm of population of donor relative to recipient*French colony dummy	0.002 (0.001)***	0.002 (0.001)**	0.002 (0.001)***
Ratio of logarithm of population of donor relative to recipient*Portugese colony dummy	0.013 (0.001)***	0.012 (0.001)***	0.012 (0.001)***
Observations	2275	2263	2205
R-squared	0.43	0.37	0.37

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.

Appendix Table 2: GMM Panel Regressions (Arellano-Bond Procedure)

(Dependent variable is average annual growth of per capita GDP)

	(1)	(2)	(3)	(4)	(5)	(6)
Social sector aid/GDP	-0.892					
	(0.813)					
Social Sector aid/GDP Square	0.014					
	(0.051)					
Economic aid/GDP		-0.088				
		(0.183)				
Economic aid/GDP Square		-0.001				
		(0.007)				
Early-impact aid/GDP			0.687			
			(0.376)*			
Early-impact aid/GDP Square			-0.058			
			(0.019)***			
Late-impact aid/GDP				-0.160		
				(0.423)		
Late-impact aid/GDP Square				-0.005		
				(0.014)		
Mult. aid/GDP					-0.620	
					(0.331)*	
Mult. aid/GDP Square					0.007	
					(0.016)	
Bilat. aid/GDP						-0.116
						(0.215)
Bilat. aid/GDP Square						-0.004
						(0.005)
Observations	165	162	163	163	167	167
Number of Groups	68	67	66	66	68	68
Chi-Square (Hansen over-id test)	0.379	0.337	0.643	0.299	0.296	0.688
AR(2) (test for serial correlation)	0.056	0.067	0.220	0.167	0.186	0.221

All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. Regressions use the Arellano and Bond (1991) difference GMM estimator. For descriptions of the variables and their sources, see Appendix 1.

Appendix Table 3: GMM Panel Regressions (Blundell-Bond Procedure)

(Dependent variable is average annual growth of per capita GDP)

	(1)	(2)	(3)	(4)	(5)	(6)
Social sector aid/GDP	0.107					
	(0.728)					
Social Sector aid/GDP Square	-0.038					
	(0.063)					
Economic aid/GDP		-0.303				
		(0.169)*				
Economic aid/GDP Square		0.011				
		(0.008)				
Early-impact aid/GDP			-0.263			
			(0.303)			
Early-impact aid/GDP Square			0.005			
			(0.016)			
Late-impact aid/GDP				-0.254		
				(0.243)		
Late-impact aid/GDP Square				0.011		
				(0.009)		
Mult. aid/GDP					-0.441	
					(0.346)	
Mult. aid/GDP Square					0.026	
					(0.020)	
Bilat. aid/GDP						-0.212
						(0.191)
Bilat. aid/GDP Square						0.007
						(0.010)
Observations	237	235	235	235	239	239
Number of Groups	72	72	72	72	72	72
Chi-Square (Hansen over-id test)	0.579	0.427	0.545	0.428	0.546	0.464
AR(2) (test for serial correlation)	0.287	0.123	0.320	0.157	0.248	0.234

All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. Regressions use the Blundell and Bond (1998) system GMM estimator. For descriptions of the variables and their sources, see Appendix 1.

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