NBER WORKING PAPER SERIES

AID AND GROWTH: WHAT DOES THE CROSS-COUNTRY EVIDENCE REALLY SHOW?

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Working Paper 11513
http://www.nber.org/papers/w11513

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
June 2005

We are grateful to Simon Johnson for many useful conversations, to Andy berg, Nancy Birdsall, Oya Celasun, Ajay Chhibber, Michael Clemens, Tito Cordelia, Bill Easterly, Graham Hacche, Nurul Islam, Aart Kraay, Sendhil Mullainathan, Jonathan Ostry, Alessandro Prati, Lant Pritchett, Steve Radelet, Rodney Ramcharan, David Roodman, Marta Ruiz-Arranz, and Thierry Tressel, for helpful comments. Manzoor Gill, Naomi Griffin, and Iannis Tokatlidis provided superb research assistance. This paper reflects the authors' views and not necessarily those of the International Monetary Fund, its management, or its Board. The views expressed herein are those of the author(s) and do not necessarily reflect the views of the National Bureau of Economic Research.

Aid and Growth: What Does the Cross-Country Evidence Really Show?
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NBER Working Paper No. 11513
July 2005, Revised July 2006
JEL No. 1, 4

ABSTRACT

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

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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] 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 (short-term versus long-term), specifications (cross-section and panel), and samples (including and excluding outliers).

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

One reason to try and take a more holistic 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 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. From a development perspective, we are interested in whether aid takes a country to its ultimate steady state potential (or to a higher steady state if it improves the country's potential growth) faster. Clearly, as we examine longer horizons, we will incorporate spillover effects and effects that take time to emerge. Since the 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 variables problem. In cross-country regressions, we can never be sure whether we are controlling for all possible ways in which countries might differ. Panel estimations have the virtue of addressing, albeit partially, the problem of unobservable heterogeneity by incorporating country fixed effects, which means that we essentially ask whether changes in aid over time for a country contemporaneously

[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).

[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.

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.

So, one important contribution of this paper is to test the impact of aid on growth in *both* cross-section and panel contexts.

Let us be more specific about the second contribution. 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 believe, for reasons explained below, that 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 does it *really* tell us about the impact of aid on growth?"

Our findings are relatively easy to report. We find little evidence of a robust positive impact of aid on growth, and this despite the fact that our instrumentation strategy corrects the bias of conventional (ordinary least squares) estimation procedures against finding a positive impact of aid. 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, a simple theoretical model suggests that the predicted positive effects of aid inflows on growth are likely to be smaller than suggested by advocates, even if inflows are utilized well. If noise in the data plague all findings, then strong claims about aid effectiveness based on cross-country evidence are unwarranted, and aid policies that rely on such claims should be re-examined.

However, the effects of other interventions (such as good policies) on growth are indeed discernible in the data and are robust. If noise in the data is not the entire explanation for the lack of a robust finding, the interesting question then is not "whether" but "why?" That is, what is it that offsets the transfers and subsidized credit inherent in aid and prevents it from having a robust positive effect on growth? Further research of this kind is essential to improve aid effectiveness. This is the focus of 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. Section IV compares the magnitude of the aid coefficients derived from theory with those obtained in the empirical literature. Section V offers some brief concluding remarks.

Then fools'

I. TACKLING ENDOGENEITY: AN INSTRUMENTATION STRATEGY

We present in Table 1 the basic descriptive statistics for the data we use in our analysis. For our cross-sectional specifications, we report results for the following 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 time periods will also give a sense of changes in aid effectiveness over time.

We begin by reporting the simple OLS regressions in Table 2; that is, in these regressions we do not take into account the endogeneity of aid. In Table 2, columns (1-4), we report the results of running the standard cross-sectional growth regressions over the different time horizons described above. The dependent variable in all cases is the average annual growth rate of per capita GDP of a country over the period. The explanatory variable of interest is the average ratio of annual external aid to GDP over that period to that country.

One basic choice that we have to make relates to the list of covariates to include. The aid and growth literature have covariates 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. The results are qualitatively similar.

The next question is which particular covariates to chose 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 covariates 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 it seems more robustly significant. We then add two further measures that seem to be important correlates with growth, geography and a measure of health (which we proxy 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. Again, it seems more general to include them separately, rather than force a pre-specified relationship. [4]

Easterly (2004) makes the argument that many cross-sectional regression results are driven by outliers, which suggests their deletion as a possible response. In all our cross-sectional specifications, we drop outliers according to the Hadi (1992) procedure as implemented in Roodman (2003). However, we also run all the regressions reported in the paper for the full sample, without a qualitative change in the results.

The results are quite clear. In all four cases, the estimated aid coefficient is negative, with it being significant in three (1960-2000, 1970-2000, and 1980-2000). The magnitude in this case suggests that an increase in aid of 1 percentage point of GDP would *lower* long-run growth by about 0.1 percentage points per year.

One cannot take these estimates too seriously 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).

[4] In order to minimize endogeneity associated with the regressors in the cross-section regressions, the values of the potentially endogenous covariates are for the beginning of the relevant time period.

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

Most 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 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]

[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. In Roodman's (2004) excellent testing of the robustness of the recent results, endogeneity and instrumentation issues are not addressed. Another issue in all these papers is that there are multiple endogenous regressors, but there is no testing for whether the requirements for strong instruments are met (Stock and Yogo, 2004).

[6] A strictly technical 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

(continued)

It is to address some of these limitations that we attempt a different instrumentation strategy, where we construct instruments for aid that are more likely to be exogenous and satisfy the exclusion restrictions. We exploit the fact that aid is often extended for non-economic reasons. Our main identification assumption is that non-economically-motivated aid is 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.

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

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

where θ_{drt} is the share of donor country d's aid allocated to recipient r in year t, and Y is the vector of explanatory variables that capture different (non-economic) aspects of donor-recipient relationships.[7] The variables include: STRAT takes on a value of 1 if the donor and recipient are common members of, or signatories to, an Entente or Alliance in any given time period;[8] USISEG takes on a value of 1 for US-Egypt and US-Israel observations after the Camp David agreement; COMCOL a value of one if the recipient was ever a colony of the donor, COMCOLUK, COMCOLFRA, COMCOLSPA, and COMCOLPOR refer in turn to colonial relationships involving respectively the U.K. France, Spain and Portugal); CURCOL

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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).

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

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

a value of one if there is a contemporaneous colonial relationship between donor and recipient; and COMLANG is a dummy that takes a value of one if the donor and recipient share a common language. A key identifying assumption is that the right hand side variables proxy for reasons for giving aid that are uncorrelated with the recipient country's economic performance. The data to estimate these equations are discussed in Appendix 1.[9]

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

$$\hat{A}_{rt} = \frac{\sum_{d} GDP_{dt} A_{dt} . \hat{\theta}_{drt}}{GDP_{rt}} \qquad \text{--- (2)}$$

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

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

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

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

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

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

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.[11]

The basic IV results

We now present estimates for the cross-sections presented earlier in Table 2, with the difference that we instrument for aid using $\hat{A}_{rt}$.[12] In Table 4A, we present the core instrumental variable (IV) specification, which is representative of the results we obtain more broadly. A substantial fraction of the variation in growth is explained by our core specifications, with R-squares typically being greater in the longer horizons (76 and 69 percent, respectively in the 1960-2000 and 1970-2000 horizons). Also, the equation is reasonably well specified as many of the standard covariates are significant and have the expected sign. The coefficient on the aid term is negative in all four cases, and significant in one (1990-2000). We obtain qualitatively similar results if we retain outliers in the sample.

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

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

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

If donors give aid to countries that are doing well (i.e. growing faster), the OLS estimate would be biased upwards (that is, it would be reflected in a more positive coefficient on aid). The "true" (IV) estimate would correct for this bias and hence result in a coefficient that is lower than the OLS coefficient. However, our IV estimates are consistently greater than the OLS estimates, suggesting that our instrumentation is correcting for a negative endogeneity bias, resulting from the tendency of donors, on average, to give aid to countries that are faring poorly.[13]

Could our results be a result of invalid or "weak" instruments? Table 4B, which presents the first-stage equations corresponding to each of the four cross-section specifications, sheds evidence on this. In all but one equation, the coefficient on the instrument is significant at the 1 percent significance level with high F-values. In the three long horizons, the F-test for excluded instruments varies from 17.3 to 71.1 which comfortably exceeds the weak instrumentation threshold of ten suggested by Staiger and Stock (1997) for the case of one endogenous regressor.[14]

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

Robustness to instrumentation

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

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

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

[14] The results for the 1990-2000 are weaker and so need to be interpreted cautiously. We do not report the first-stage regressions in the subsequent cross-section results, but they are consistently similar to those reported in Table 4B, with fitted aid always being positive and statistically significant, typically at the 1 percent confidence level.

Non-linear and conditional effects

In Panel A of Table 6, we ask whether there are diminishing returns to aid. To test this, we include a squared aid term in the regression. The aid terms remain insignificant, and all the aid-square terms have positive returns, suggesting increasing rather than diminishing returns. The aid-squared term is itself never significant.

Burnside and Dollar (2000) and Collier and Dollar (2003) suggest that aid, even if it does not unconditionally help growth, is helpful in those countries that have good policies and institutions. In Panel B, we ask whether aid is more effective in better policy environments. To answer this, we interact aid with two measures of policy: the Sachs-Warner measure updated by Warcziarg and Welch (2003) which is reported in the table, and the World Bank's CPIA ratings which is reported in Appendix 2. In both cases, we find that the coefficient on the aid-policy interaction terms is never significant.[15] This is consistent with the results in Easterly, Levine, and Roodman (2004).

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 absent 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.[16] 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 a measure of geography (due to Bosworth and Collins, 2003)). While geography itself usually has a positive correlation with growth, the aid-geography coefficient is positive and significant in only one of the four time horizons, and negative in two.

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[15] We obtained similar results when the aid-policy and aid-square terms were included simultaneously.

[16] 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.

Categories of aid

In Table 7, we try to distinguish the impact of different types of aid in the spirit of Clemens et. al. (2004). We disaggregate aid in three different ways: by sector (social, economic, and food);[17] by timing of impact (short and long impact);[18] and by type of donor (multilateral and bilateral). The results are shown in three panels in Table 7. Various arguments can be made as to why some categories but not others should affect long-run growth. For example, food aid should typically not be expected to affect long-run growth while economic and social sector aid should because they lead to an increase in physical and human capital. Clemens et. al (2004) argue that the effect of short-impact aid will be easier to detect in the data than long-impact aid. Similarly, one can make the argument that multilateral aid is less explicitly "political" than bilateral aid and should therefore have a different impact. The argument for a possible differential impact between multilateral and bilateral aid could also relate to the type of aid given or to the nature and effectiveness of conditionality. What we find, however, is that no sub-categories have any significant impact—positive or negative--on growth Nor is there any evidence of diminishing returns to aid.[19]

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

[18] We need to instrument for short impact and long impact aid. In the Clemens et. al. (2004) categorization, the former contains many of the economic aid categories and the latter the social aid categories. So in our specifications that use the Clemens et. al. (2004) variables, we instrument for short impact aid with our instrument for economic aid and for long impact aid with the instrument for social aid. 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.

[19] When the specifications in Table 7 are estimated without the squared term, the results on the aid coefficient remain unchanged (available from the authors upon request).

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.[20] These come in two flavors. There is the difference-GMM estimator due to Arellano and Bond (AB)(1991) and the system-GMM estimator due to Blundell and Bond (BB) (1998). In both, identification relies on first-differencing and using lagged values of the endogenous variables as instruments.

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. For these reasons, the Arellano-Bond procedure might be superior in this context.

In Tables 8 and 9, we report the results of the GMM estimations corresponding strictly to the specifications in Tables 4-8; that is, the covariates are the same as in the latter, the only difference being that there is only one time horizon rather than several. In these tables we report the results of the AB procedure, while in Appendix Tables 5 and 6, we report the results of the BB 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 4-8, the results on aid remain broadly unchanged. In column 1 of Table 8, we report the core specification corresponding to Tables 4 and 5. In column 2, we test whether there

[20] 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.

are diminishing returns by adding the squared aid term (compare with Table 6, Panel A). Column 3 is the counterpart of 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 6). There is some evidence for aid working better in better geographic environments but the sign of the coefficient estimate for the aid-geography interaction is reversed (and significant) in the system GMM BB estimate in Appendix Table 5. Aid is not significant in any of the other specifications. The GMM equations also seem well specified because many of the standard covariates help explain the time series variation in growth.

In Table 9, we examine the impact of different categories of aid as in Table 7. 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 6, 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.[21] What then would be the quantitative impact on growth? A theoretical estimate of this impact can be obtained from a simple growth model. This model yields the conclusion that, even under the most optimistic assumptions about the use of aid (optimistic in the sense that all aid is invested and none of it is wasted or consumed), the impact of aid should be positive but relatively small in magnitude.

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

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

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

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

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? This comparison also serves as a robustness check on the results of this paper as well as others in the literature. Sampling a few of the more influential papers that find a positive impact of aid on growth, the pattern that one discerns is that OLS and two-stage least squares estimations yield lower magnitudes than GMM estimations. 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.[22] 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.

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

- time horizons;
- time periods;
- cross-section and panel contexts
- types of aid distinguished by:
    - what they are used for (economic, social, food, etc.);
    - who gives it (multilateral donors, bilateral donors etc.);

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

- who it is given to (those with good policies and institutions and others);
- who it is given to (those in the tropics and outside); and
- how long it takes to impact (short and long impact).

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).

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

Table 1: Summary Statistics

Variable	*Mean*	*Std. Dev.*	*Min*	*Max*
1960-00 ***(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	8.902	14.834	0.052	92.357
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
Fitted mult. aid/GDP	2.524	4.310	0.016	27.254
Bilat. aid/GDP	3.917	4.199	0.072	19.388
Fitted bilat. aid/GDP	6.963	11.531	0.037	72.637
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	7.968	15.920	0.039	104.651
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
Social sector aid/GDP	0.955	1.251	0.016	8.155
Fitted social sector aid/GDP	4.874	9.676	0.024	61.892
Economic aid/GDP	5.928	5.848	0.048	23.210
Fitted economic aid/GDP	71.687	150.084	0.418	1018.064
Early-impact aid/GDP	3.735	4.160	0.018	23.336
Late-impact aid/GDP	3.171	4.015	0.037	21.637
Mult. aid/GDP	2.170	3.226	0.001	17.184
Fitted mult. aid/GDP	2.005	4.127	0.011	26.954
Bilat. aid/GDP	3.768	4.673	0.047	29.702
Fitted bilat. aid/GDP	6.870	13.653	0.033	88.761
Policy (World Bank's CPIA ratings)	3.004	0.930	0.000	5.000
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
Social sector aid/GDP	0.692	1.129	0.002	9.707
Fitted social sector aid/GDP	4.069	9.094	0.028	85.776
Economic aid/GDP	5.003	6.131	0.001	33.548
Fitted economic aid/GDP	54.055	131.567	0.360	1366.901
Early-impact aid/GDP	3.004	4.092	0.000	24.847
Late-impact aid/GDP	2.140	3.570	0.000	31.065
Mult. aid/GDP	1.510	2.803	0.000	21.770
Fitted mult. aid/GDP	1.309	2.653	0.034	25.622
Bilat. aid/GDP	3.015	4.070	0.019	26.169
Fitted bilat. aid/GDP	6.094	13.099	0.037	124.686

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	-0.076	-0.094	-0.120	-0.003
	(0.027)***	(0.040)**	(0.069)*	(0.094)
Initial per cap. GDP	-1.327	-1.661	-1.632	-1.138
	(0.268)***	(0.296)***	(0.376)***	(0.591)*
Initial level of policy (Sachs-Warner)	1.746	2.212	2.303	-0.164
	(0.411)***	(0.463)***	(0.773)***	(0.566)
Initial level of life expectancy	0.031	0.032	0.063	0.153
	(0.018)*	(0.025)	(0.041)	(0.065)**
Geography	0.370	0.449	0.505	0.693
	(0.134)***	(0.177)**	(0.224)**	(0.409)*
Institutional quality	2.765	2.264	1.361	2.963
	(1.119)**	(1.595)	(2.281)	(3.159)
Initial Inflation	-0.002	-0.004	-0.001	-0.001
	(0.003)	(0.003)	(0.002)	(0.000)***
Initial M2/GDP	0.017	0.013	-0.010	-0.004
	(0.010)	(0.015)	(0.023)	(0.015)
Initial Budget Balance/GDP	-0.010	-0.017	-0.023	0.204
	(0.024)	(0.034)	(0.035)	(0.059)***
Revolutions	-1.044	-1.342	-0.669	-0.499
	(0.461)**	(0.447)***	(0.627)	(0.656)
Ethnic Fractionalization	-0.058	-0.190	0.045	1.756
	(0.458)	(0.676)	(0.903)	(1.104)
Observations	73	77	75	69
R-squared	0.78	0.72	0.64	0.62
Outliers	Congo, Dem. Rep.	Congo, Dem. Rep.	Congo, Dem. Rep. Guinea Bissau	Nicaragua

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

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

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

Estimation is by ordinary least squares. The estimated equation corresponds to equation 1 in Section II of the paper. All standard errors are robust. 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	-0.020	-0.022	-0.100	-0.257
	(0.043)	(0.072)	(0.107)	(0.144)*
Initial per cap. GDP	-1.291	-1.560	-1.599	-1.813
	(0.314)***	(0.334)***	(0.409)***	(0.658)***
Initial level of policy (Sachs-Warner)	1.723	2.255	2.323	-0.129
	(0.507)***	(0.531)***	(0.783)***	(0.675)
Initial level of life expectancy	0.035	0.033	0.079	0.084
	(0.024)	(0.035)	(0.050)	(0.077)
Geography	0.420	0.444	0.538	0.382
	(0.153)***	(0.214)**	(0.232)**	(0.407)
Institutional quality	4.437	4.249	0.842	5.120
	(1.679)**	(2.460)*	(2.335)	(3.350)
Initial Inflation	-0.003	-0.005	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.001)**
Initial M2/GDP	0.016	0.014	-0.010	-0.003
	(0.011)	(0.016)	(0.023)	(0.013)
Initial Budget Balance/GDP	0.008	0.010	-0.016	0.197
	(0.028)	(0.035)	(0.045)	(0.077)**
Revolutions	-1.220	-1.385	-0.605	-0.388
	(0.548)**	(0.514)***	(0.647)	(0.703)
Ethnic Fractionalization	0.160	-0.024	0.172	0.523
	(0.483)	(0.704)	(1.135)	(1.207)
Observations	70	74	74	69
R-squared	0.76	0.69	0.64	0.52
Outliers	Gambia Guinea Bissau Guyana Lesotho	Gambia Guinea Bissau Guyana Lesotho	Congo, Dem. Rep. Gambia Guinea Bissau	Gambia

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	0.488	0.421	0.230	0.340
	(0.075)***	(0.071)***	(0.055)***	(0.130)**
Observations	70	74	74	69
R-squared	0.77	0.78	0.79	0.71
F-value	18.03	18.25	27.43	12.40

All standard errors, including in the first stage, are robust, and are reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. In all the IV estimations in the paper, R-squares in the second stage are not based on the Stata IVREG command; instead they are based on running the second stage with fitted values for the endogenous variable obtained from the first stage. The instrument for aid in Table 4A corresponds to equation 2 in Section II of the paper. All regressions exclude outliers, which are identified according to the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates in the first-stage regression are omitted for presentational convenience. For descriptions of the variables and their sources, see Appendix 1.

Table 5: Impact of Total Aid on Growth, OLS Estimations Using Lagged Aid

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

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Lagged Aid/GDP	-0.088	-0.013	-0.047	-0.028
	(0.056)	(0.043)	(0.051)	(0.067)
Initial per cap. GDP	-1.300	-1.584	-1.520	-1.180
	(0.392)***	(0.354)***	(0.425)***	(0.598)*
Initial level of policy (Sachs-Warner	1.214	2.141	2.395	-0.193
	(0.616)*	(0.510)***	(0.837)***	(0.597)
Initial level of life expectancy	0.033	0.044	0.081	0.154
	(0.029)	(0.038)	(0.047)*	(0.063)**
Geography	0.439	0.579	0.649	0.728
	(0.164)***	(0.202)***	(0.251)**	(0.388)*
Institutional quality	4.436	4.180	1.440	3.380
	(1.491)***	(2.294)*	(2.638)	(3.221)
Initial Inflation	-0.001	-0.004	-0.002	-0.001
	(0.003)	(0.004)	(0.003)	(0.000)***
Initial M2/GDP	0.024	0.020	-0.013	-0.003
	(0.011)**	(0.018)	(0.025)	(0.014)
Initial Budget Balance/GDP	0.011	0.009	-0.007	0.197
	(0.025)	(0.034)	(0.039)	(0.061)***
Revolutions	-1.194	-1.336	-0.790	-0.486
	(0.578)**	(0.527)**	(0.656)	(0.661)
Ethnic Fractionalization	0.229	0.077	0.504	1.816
	(0.501)	(0.768)	(0.961)	(1.037)*
Observations	66	75	72	68
R-squared	0.76	0.69	0.59	0.63
Outliers	Botswana Ecuador Papua New Guinea		Congo, Dem. Rep	Gambia

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

Table 6: Aid and Growth: Diminishing Returns and Conditional Impacts, IV Estimations
(Dependent variable is average annual growth of per capita GDP)

Panel 6A: Aid-square term

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	-0.025	-0.027	-0.082	-13.356
	(0.077)	(0.108)	(0.113)	(75.882)
Aid/GDP-squared	0.000	0.001	0.003	0.597
	(0.003)	(0.004)	(0.002)	(3.443)
Observations	73	77	77	69
R-squared	0.76	0.68	0.57	
Outliers	Gambia	Gambia		Gambia

Panel 6B: Aid interacted with Sachs-Warner Policy measure

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	-0.033	-0.038	-0.050	-0.172
	(0.045)	(0.076)	(0.123)	(0.131)
Policy (Sachs-Warner)	1.644	2.184	0.510	-0.298
	(0.549)***	(0.590)***	(1.363)	(0.793)
Aid/GDP*policy	0.023	0.017	1.087	0.029
	(0.101)	(0.127)	(0.777)	(0.094)
Observations	73	77	75	68
R-squared	0.76	0.69	0.46	0.56
Outliers	Lesotho	Lesotho	Gambia Guinea Bissau	Lesotho Sierra Leone

Panel 6C: Aid interacted with Geography

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	0.211	0.333	-0.145	-0.313
	(0.143)	(0.254)	(0.359)	(0.285)
Geography	0.204	0.146	0.593	0.614
	(0.188)	(0.243)	(0.304)*	(0.485)
Aid/GDP*Geography	0.242	0.362	-0.093	-0.112
	(0.143)*	(0.243)	(0.321)	(0.266)
Observations	70	74	74	68
R-squared	0.77	0.69	0.59	0.57
Outliers	Gambia Guinea Bissau Guyana Lesotho	Gambia Guinea Bissau Guyana Lesotho	Gambia Guinea Bissau Lesotho	Gambia Lesotho

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 2 in Section II of the paper. Outliers are identified using the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates are omitted for presentational simplicity. For descriptions of the variables and their sources, see Appendix 1.

Table 7: Impact of Different Categories of Aid on Growth, IV Estimations
(Dependent variable is average annual growth of per capita GDP)

Panel 7A: Social, Economic and Food Aid

	Social			Economic		
	(1) 1970_00	**(2) 1980_00**	**(3) 1990_00**	**(4) 1970_00**	**(5) 1980_00**	**(6) 1990_00**
Social sector aid/GDP	-0.088	-0.642	-17.049			
	(0.617)	(0.676)	(27.594)			
Social sector aid/GDP-squared	0.023	0.102	3.073			
	(0.107)	(0.065)	(4.803)			
Economic aid/GDP				-0.576	-0.554	6.948
				(2.109)	(0.754)	(14.358)
Economic aid/GDP-squared				0.027	0.022	-0.393
				(0.099)	(0.032)	(0.818)
Observations	77	76	69	77	75	69
R-squared	0.68	0.61		0.26	0.45	
Outliers	Gambia	Congo, Dem. Rep.	Gambia	Gambia	Guinea Bissau Guyana	Gambia

Panel 7B: 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	-0.066	-0.183	-3.912			
	(0.172)	(0.193)	(2.970)			
Late-impact aid/GDP-squared	0.006	0.012	0.229			
	(0.010)	(0.007)*	(0.177)			
Early-impact aid/GDP				-0.112	-0.262	2.132
				(0.159)	(0.255)	(2.675)
Early-impact aid/GDP-squared				0.006	0.012	-0.157
				(0.006)	(0.007)*	(0.170)
Observations	70	73	69	70	73	69
R-squared	0.70	0.56		0.71	0.58	0.15
Outliers	Gambia		Gambia	Gambia		Gambia

Panel 7C: Multilateral and Bilateral Aid

	Multilateral Aid				Bilateral Aid			
	(1) 1960_00	**(2) 1970_00**	**(3) 1980_00**	**(4) 1990_00**	**(5) 1960_00**	**(6) 1970_00**	**(7) 1980_00**	**(8) 1990_00**
Mult. aid/GDP	-0.087	-0.183	-0.336	3.964				
	(0.319)	(0.473)	(0.220)	(8.843)				
Mult. aid/GDP-squared	0.005	0.021	0.023	-0.394				
	(0.034)	(0.052)	(0.010)**	(0.793)				
Bilat. aid/GDP					-0.026	-0.025	-0.113	-8.568
					(0.117)	(0.161)	(0.199)	(17.003)
Bilat. aid/GDP-squared					0.000	0.002	0.006	0.503
					(0.006)	(0.008)	(0.005)	(1.011)
Observations	73	77	76	69	73	77	77	69
R-squared	0.76	0.69	0.63		0.75	0.68	0.57	
Outliers	Gambia	Gambia	Congo, Dem. Rep	Gambia	Gambia	Gambia		Gambia

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 2 in Section II of the paper. Outliers are identified using the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates are omitted for presentational simplicity. For descriptions of the variables and their sources, see Appendix 1.

Table 8: System GMM Panel Regressions

(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

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.

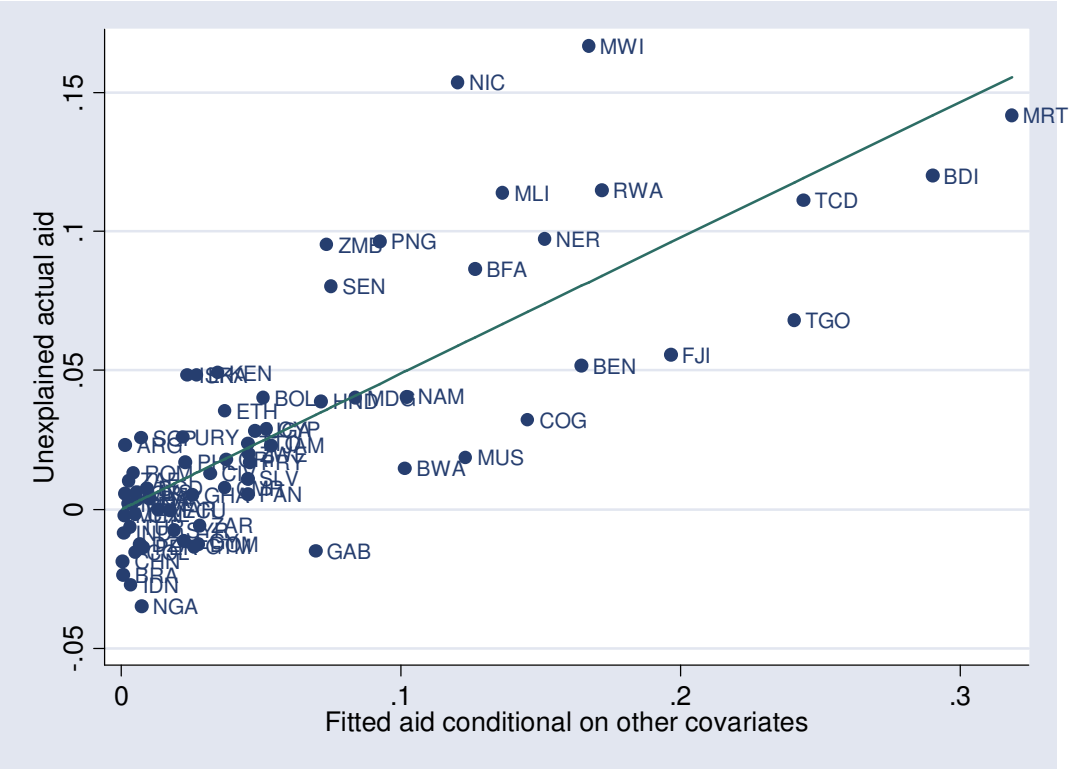
Table 9: System GMM Panel Regressions

(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 tes	0.379	0.337	0.643	0.299	0.296	0.688
AR(2) (test for serial correlatio	0.056	0.067	0.220	0.167	0.186	0.221

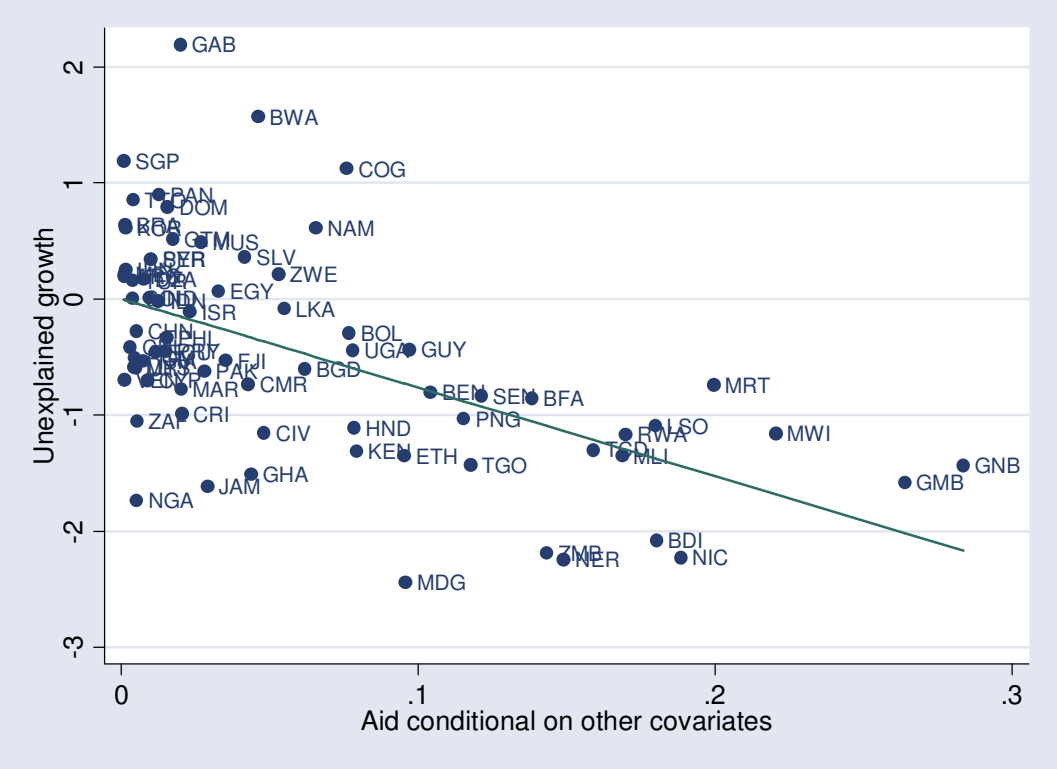
All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. Regressions use the Arellano and Bond (1991) difference GMM estimator. For descriptions of the variables and their sources, see Appendix 1.

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



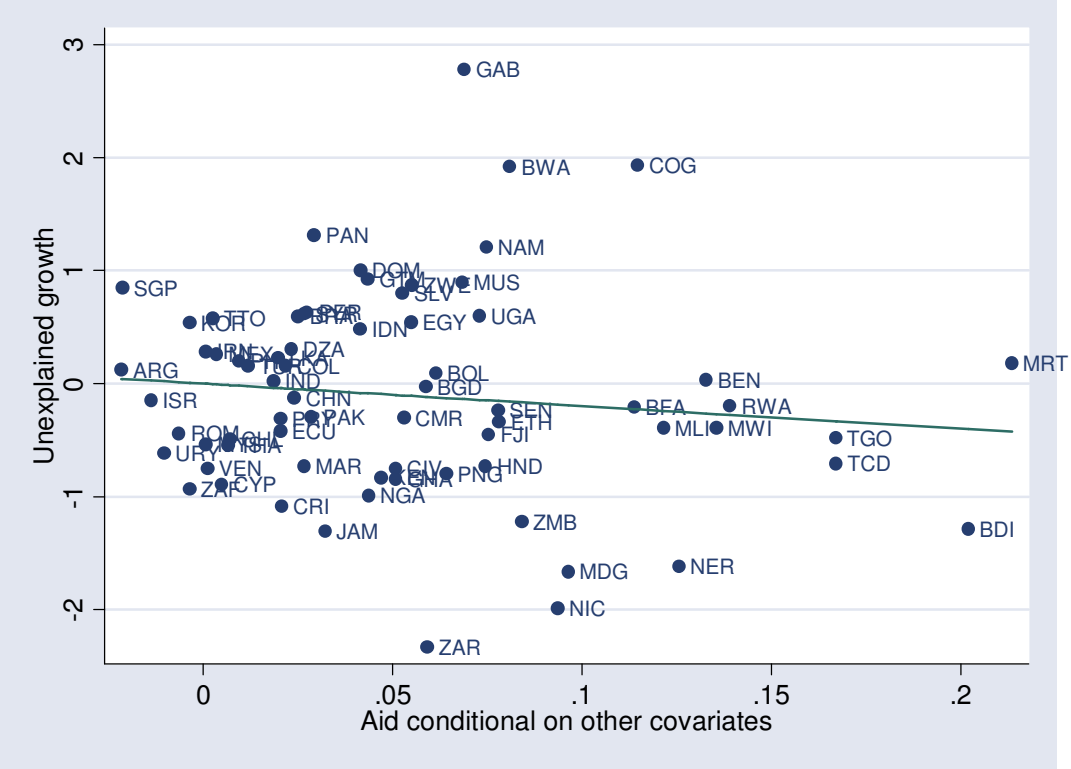
The chart plots the first-stage relationship between actual and the instrument (fitted aid), conditional on all the covariates that enter the second-stage growth regression. The slope of the line is the coefficient on fitted aid in this first-stage regression (also shown in Table 4B). For presentational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

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



Coefficient=-7.64; t-statistic=2.88
The chart plots the relationship between growth and actual aid, conditional on all the covariates. The slope of the line is the coefficient on aid in the OLS regression in column 1 of Table 2.

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



Coefficient value=-1.98; t-statistic=0.46
The chart plots the relationship between growth and aid, conditional on all the covariates. The slope of the line is the coefficient on aid in the instrumental variable regression in column 1 of Table 4A.

Appendix 1. Data Description and Sources

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

social_gdp	The ratio of social sector aid to GDP	OECD, DAC
F_socN_gdp	The ratio of fitted social sector aid to GDP	Authors' calculations
economic_gdp	The ratio of economic aid to GDP	OECD, DAC
F_ecoN_gdp	The ratio of fitted economic aid to GDP	Authors' calculations
yc_penn	Initial period per capita (PPP) GDP	PWT, 6.1
le_wdi	Initial period life expectancy at birth	WDI
gadp6099	Institutional Quality	Bosworth & Collins, 2003
geog6099	Geography	Bosworth & Collins, 2003
Inf5_ES	Initial inflation	Easterly
M2_GDP5_ES	Initial M2/GDP	Easterly
BB_GDP5_WDI	Initial budget balanc/GDP	WDI
revol	No. of revolutions	Arthur S. Banks
sw1	Trade policy	Wacziarg & Welch, 2003
aid_gdpsq	The ratio of aid to GDP squared	Authors' calculations
aid_sw1	The ratio of aid to GDP * Trade policy	Authors' calculations
sw1_i	Initial level of trade policy	Wacziarg & Welch, 2003
aid_sw1_i	The ratio of aid to GDP * Initial trade policy	Authors' calculations
cpia	Policy and institutional rating	World Bank
aid_cpia	The ratio of aid to GDP * cpia	Authors' calculations

cpia_i	Initial level of policy and institutional rating	World Bank
aid_cpia_i	The ratio of aid to GDP * initial policy	Authors' calculations
ggb_gdp	The ratio of donor country's general government balance to GDP	WEO
lurn	Donor country's natural rate of unemployment	WEO
aid_shr	Donor country's aid share to recipient country	Authors' calculations
allidum_3	Dummy that takes a value of 1 if donor and recipient country are part of the same entente alliance	Correlates of War, 2
egy_isr_dum	Dummy that takes a value of 1 if donor is US and recipient is Egypt or Israel	Authors' calculations
colony	Dummy that takes a value of 1 if donor and recipient country were ever in colonial relationship	Rose, 2004
curcol	Dummy that takes a value of 1 if donor and recipient country enjoy a current colonial relationship	Rose, 2004

Appendix 2: Issues Relating to the Instrumentation Strategy

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

Exclusion restriction

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

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

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

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

On the exclusion restriction, our instrumentation strategy needs to be compared with the alternatives in the literature. As described earlier, the latter typically use lagged values of aid and policy variables as instruments. While lagged values can be pre-determined, it is highly unlikely that they will satisfy the exclusion restriction under the plausible assumption that shocks to growth (the LHS variable) persist over time.

Bias

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

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

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

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

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

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

where FB_{dt} is the overall fiscal balance as a share of GDP in donor country d in time period t, and UN_{dt} is the unemployment rate. The idea is that countries are more likely to be forthcoming with aid when their budgetary positions are more favorable, a factor that is likely to be exogenous to a recipient country's long run growth. The explanatory variables are obtained from the IMF's World Economic Outlook (WEO) database.

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

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

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

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

Other concerns

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

Appendix 3. 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
GuineaBissau	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 4. Prediction of the Standard Growth Model of the Quantitative Impact of Aid [23]

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

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

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

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

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

The equation for capital accumulation is:

$$overval_i = \log p_i - (\alpha + \beta \log y_i) \qquad \text{---(2)}$$

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

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

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

where n represents the population growth rate.

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

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

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

Substituting equation (3) in (4) yields

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

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

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

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

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

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

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

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:

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

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. Impact of Aid on Growth Using Alternative Instrument For Aid

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

	(1) 1960_00	(2) 1970_00	(3) 1980_00	(4) 1990_00
Aid/GDP	-0.018	-0.018	-0.086	-0.084
	(0.044)	(0.073)	(0.106)	(0.115)
Initial per cap. GDP	-1.290	-1.556	-1.578	-1.319
	(0.314)***	(0.334)***	(0.411)***	(0.599)**
Initial level of policy (Sachs-Warner)	1.724	2.258	2.326	-0.226
	(0.511)***	(0.536)***	(0.790)***	(0.626)
Initial level of life expectancy	0.035	0.034	0.083	0.115
	(0.024)	(0.036)	(0.051)	(0.073)
Geography	0.423	0.451	0.549	0.678
	(0.155)***	(0.219)**	(0.234)**	(0.447)
Institutional quality	4.444	4.249	0.816	3.905
	(1.683)**	(2.460)*	(2.348)	(3.142)
Initial Inflation	-0.003	-0.005	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.000)***
Initial M2/GDP	0.016	0.014	-0.010	-0.003
	(0.011)	(0.017)	(0.024)	(0.014)
Initial Budget Balance/GDP	0.008	0.010	-0.012	0.215
	(0.028)	(0.036)	(0.045)	(0.066)***
Revolutions	-1.221	-1.393	-0.618	-0.265
	(0.550)**	(0.513)***	(0.653)	(0.692)
Ethnic Fractionalization	0.171	0.006	0.268	1.380
	(0.484)	(0.716)	(1.167)	(1.031)
Observations	70	74	74	66
R-squared	0.76	0.69	0.64	0.60

All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid is based on equations 3 and 4 in Appendix 2. Outliers are identified using the Hadi (1992) procedure. All specifications include dummies for sub-Saharan African and East Asian countries. For descriptions of the variables and their sources, see Appendix 1.

Appendix Table 2: Impact of Aid on Growth Conditional on Policy and Institutions, IV Estimations

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

	(1) 1980_00	(2) 1990_00	(4) 1980_00	(5) 1990_00
Aid/GDP	0.333	-0.078	0.071	-0.235
	(0.454)	(0.321)	(0.169)	(0.144)
Policy (Sachs-Warner)			0.497	-0.375
			(1.561)	(0.854)
Aid/GDP*Policy (Sachs-Warner)			1.149	0.065
			(0.895)	(0.103)
Policy (World Bank's CPIA ratings)	0.580	0.538		
	(0.333)*	(0.752)		
Aid/GDP*policy (World Bank's CPIA ratings	-0.099	-0.032		
	(0.127)	(0.100)		
Observations	71	66	71	66
R-squared	0.48	0.54	0.38	0.49
Outliers	Chad	Sierra Leone	Chad	Sierra Leone
	Gambia		Gambia	
	Guinea Bissau		Guinea Bissau	
	Malawi		Malawi	
	Togo		Togo	

This table reproduces the results in Table 6B, except that the World Bank's CPIA rating substitutes for the Sachs-Warner-Warcziarg-Welch measure of policy. The estimation is for a sample that is common across the two measures of policy. All standard errors are robust and reported below coefficient estimates. ***, **, and * denote significance at 1, 5, and 10 percent, respectively. The instrument for aid corresponds to equation 2 in Section II of the paper. All specifications include dummies for sub-Saharan African and East Asian countries. Other covariates are omitted for presentational simplicity. The first-stage is also omitted as it is virtually unchanged from that in Table 9. For descriptions of the variables and their sources, see Appendix 1.

Appendix Table 3. Impact of Aid on Growth: Does Donor Type Matter? IV Estimations

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

	(1) **1960_00**	**(2)** **1970_00**	**(3)** **1980_00**	**(4)** **1990_00**
Aid/GDP	-0.020	-0.021	-0.051	-0.252
	(0.044)	(0.074)	(0.109)	(0.141)*
Aid share of Scandinavian countries	-0.230	0.331	1.143	3.479
	(2.560)	(3.376)	(3.365)	(3.637)
Observations	70	74	75	69
R-squared	0.76	0.69	0.58	0.53
Outliers	Gambia Guinea Bissau Guyana Lesotho	Gambia Guinea Bissau Guyana Lesotho	Gambia Guinea Bissau	Gambia

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

Appendix Table 4. 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)

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

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

Appendix Table 5: System GMM Panel Regressions

(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

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.

Appendix Table 6: GMM Regressions

(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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