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By Michael A. Clemens, Steven Radelet, Rikhil Bhavnani

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Past research on aid and growth is flawed because it typically examines the impact of aggregate aid on growth over a short period, usually four years, while significant portions of aid are unlikely to affect growth in such a brief time. We divide aid into three categories: (1) emergency and humanitarian aid (likely to be negatively correlated with growth); (2) aid that affects growth only over the long term, if at all, such as aid to support democracy, the environment, health, or education (likely to have no relationship to growth over four years); and (3) aid that plausibly could stimulate growth in four years, including budget and balance of payments support, investments in infrastructure, and aid for productive sectors such as agriculture and industry. Our focus is on the third group, which accounts for about 45% of aid flows. We find a positive, causal relationship between this "short-impact" aid and economic growth (with diminishing returns) over a four-year period. The impact is large: at least two-to-three times larger than in studies using aggregate aid. Even at a conservatively high discount rate, at the mean a $1 increase in short-impact aid raises output (and income) by $8 in present value in the typical country. From a different perspective, we find that higher-than-average short-impact aid to sub-Saharan Africa raised per capita growth rates there by about one percentage point over the growth that would have been achieved by average aid flows. The results are highly statistically significant and stand up to a demanding array of tests, including various specifications, endogeneity structures, and treatment of influential observations. The basic result does not depend crucially on a recipient's level of income or quality of institutions and policies; we find that short-impact aid causes growth, on average, regardless of these characteristics. However, we find some evidence that the impact on growth is somewhat larger in countries with stronger institutions or longer life expectancies (better health). We also find a significant negative relationship between debt repayments and growth. We make no statement on, and do not attempt to measure, any additional long-run effects of aid; four-year panel regressions are not an appropriate tool to examine those relationships.

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Past research on aid and growth is flawed because it typically examines the impact of aggregate aid on growth over a short period, usually four years, while significant portions of aid are unlikely to affect growth in such a brief time. We divide aid into three categories: (1) emergency and humanitarian aid (likely to be negatively correlated with growth); (2) aid that affects growth only over the long term, if at all, such as aid to support democracy, the environment, health, or education (likely to have no relationship to growth over four years); and (3) aid that plausibly could stimulate growth in four years, including budget and balance of payments support, investments in infrastructure, and aid for productive sectors such as agriculture and industry. Our focus is on the third group, which accounts for about 45% of all aid flows. We find a positive, causal relationship between this "short-impact" aid and economic growth (with diminishing returns) over a four-year period. The impact is large: at least two-to-three times larger than in studies using aggregate aid. Even at a conservatively high discount rate, at the mean a $1 increase in short-impact aid raises output (and income) by $8 in present value in the typical country. From a different perspective, we find that higher-than-average short-impact aid to sub-Saharan Africa raised per capita growth rates there by about one percentage point over the growth that would have been achieved by average aid flows. The results are highly statistically significant and stand up to a demanding array of tests, including various specifications, endogeneity structures, and treatment of influential observations. The basic result does not depend crucially on a recipient's level of income or quality of institutions and policies; we find that short-impact aid causes growth, on average, regardless of these characteristics. However, we find some evidence that the impact on growth is somewhat larger in countries with stronger institutions or longer life expectancies (better health). We also find a significant negative relationship between debt repayments and growth. We make no statement on, and do not attempt to measure, any additional long-run effects of aid; four-year panel regressions are not an appropriate tool to examine those relationships.

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1 Introduction

What is the relationship between foreign aid and economic growth in low-income countries? Researchers and policymakers have debated this question for years, with little resolution. Some researchers have concluded that aid does little for growth, with a few suggesting that in the wrong circumstances (such as under a corrupt dictator), aid can undermine growth and development. Others have found that once they carefully control for collateral determinants of growth and allow for diminishing returns, aid supports growth. They conclude that although the growth performance of many countries has been disappointing over the last two decades—especially in sub-Saharan Africa—in the absence of aid the growth performance would have been much worse.

Still other investigators accept the finding of little or no aggregate relationship between aid and growth across all countries, but find a positive relationship in certain subsets of countries, particularly countries with strong policies and institutions. This last line of research has been enormously influential in policy circles, with donor agencies beginning to embrace one implication—that more aid should go to countries with stronger policies and institutions—but more reluctant to embrace another: that they should eliminate aid to countries with poor policies and institutions where this strand of the research finds no aid-growth relationship.

It is not surprising that this research has mixed findings, at least given how it has been conducted to date, for two reasons. First, economic growth is not the sole objective of foreign aid, and in some cases it is not the objective at all. For example, much of the aid that is given following natural disasters is aimed at supporting immediate consumption and humanitarian needs, not building productive capacity. Similarly, aid provided to build political systems or support democracies has growth as only a secondary and distant objective. And of course much aid is given primarily for political purposes (e.g. Alesina and Dollar 2000). Aid from the United States to Israel and Egypt, for example, was designed to support the Camp David peace accords, and current US aid to Pakistan is designed to bolster that country's efforts in fighting the war on terrorism. Similarly, significant amounts of French aid went to its former colonies as a way to ensure continued French influence in those countries. With politically based aid, growth is but a welcome side effect and is not the metric by which policymakers evaluate the effectiveness of aid. Given the variety of ex-ante objectives for aid, it is to be expected that ex-post evaluations of aid efficacy find mixed results when conducted as if growth were the only objective.

Second, the approach used in most studies is not well suited to detect the growth effects of large portions of aid. Almost all the macro-level research on this issue over the past decade has used cross-country growth regressions based on panel data with four-year observations. While growth regressions in general have many weaknesses, one of these is of particular importance in this case. In our view, it is uninformative to test the hypothesis that *all* foreign aid has a positive impact on economic growth over a four year period. Consider, for example, aid for disasters and emergencies—henceforth, "humanitarian" aid. This kind of assistance should have a negative simple correlation with growth, as the disaster simultaneously causes both low growth and large aid inflows. While it is possible that aid might mitigate that fall in growth, any additional pathway of causation from humanitarian aid to growth is extremely difficult to detect. Either one must instrument well for these emergency aid flows—which are inherently unpredictable—or somehow treat observations involving emergencies separately from others. Most researchers attempt neither. Including this kind of aid in standard regressions clearly biases downward (if not reversing the sign of) any aid-growth relationship.

Similarly, it makes little sense to believe that several broad classes of aid can have a positive effect on growth over a four-year period. No one should expect that aid provided to halt environmental degradation or to build democracies would affect growth in four years. Even aid for education and health programs, which may have a strong impact on long-run growth, should not be expected to influence growth in four years. Strengthening primary education systems and reducing infant mortality might support growth in twenty years, but not four. In a cross-country growth regression with observations of just four years, these aid flows aimed at longer-term growth should be expected to have zero (or perhaps a tiny positive) correlation with growth.

Simply extending the observation period does not solve this problem. Longer periods allow more time for the effects of aid to be realized, but they also include much more uncontrolled noise. The variance in growth to be explained rises dramatically with respect to the variance in aid. Regressions of growth on *aggregate* aid face an inescapable tradeoff between comprehensiveness and attribution.

This is not to suggest that researchers should expect to find no relationship at all between aid and growth. By contrast, we *should* expect a certain subset of aid flows to influence growth over four years. Aid to build infrastructure—roads, irrigation systems, electricity generators and ports—should affect growth rates fairly quickly. So should aid to support directly productive sectors, such as agriculture, industry, trade, and services. Aid that comes as cash, such as budget or balance of payments support, also should be expected to positively affect growth fairly immediately if it is to do so at all. For these kinds of aid flows, it is perfectly reasonable for policymakers to expect and for researchers to test for a positive causal relationship with growth over a four-year period.

However, almost all research on aid and growth lump these three kinds of aid flows together—humanitarian aid with a negative association with growth, long-term investments with zero or very little short-term positive relationship, and infrastructure and balance of payments support with a plausibly positive relationship. In addition, most researchers generally subtract debt repayments and examine "net" aid, even though debt repayments are wholly separate financial transactions from aid inflows with possibly a quite different relationship with economic growth. It is little wonder that research that fails to distinguish between these effects gives diverse results that are difficult to interpret. It would be astonishing if these mixed inputs gave any clear results at all.

This paper aims to remedy this weakness in the research on foreign aid and economic growth. Instead of focusing primarily on the heterogeneity of aid recipients, our starting point is to explore the heterogeneity of aid flows and their impact on growth. In short, we disaggregate aid flows into the three types outlined above—aid for humanitarian purposes, aid that would only affect growth in the long run, and aid that might impact growth over a four year period. We also examine aid inflows separately from debt repayments. Our particular focus is the sub-category of aid that might reasonably affect growth over a four-year period, which by our classification constitutes about 45% of all aid flows.

We find a strong, positive, and causal relationship between "short-impact" aid, as we call it, and economic growth (with diminishing returns) over a four-year period. The impact is large, with the estimated relationship conservatively two-to-three times larger than that found in studies using aggregate foreign aid. The results are highly statistically significant and robust over a wide variety of specifications, time periods, and estimation techniques. The basic result does not depend on levels of income, the strength of institutions, or the quality of policies; we find that short-impact aid causes growth, on average, across countries *regardless* of these characteristics. That being said, we find some evidence that the impact on growth is even larger in the presence of strong institutions and in countries with longer life expectancies (better general health conditions). The key point, however, is that we detect a powerful relationship between short-impact aid and growth across all countries on average, not just those with strong institutions or policies, and find a slightly larger relationship in the presence of good institutions. This differs substantially from some previous research, such as Burnside and Dollar (2000), which by using aggregate aid was able to detect a positive aid-growth relationship *only* in the presence of strong policies and institutions.

With respect to the other sub-categories of aid, as expected we find little or no relationship between either humanitarian aid or long-term aid and economic growth over a four year period. But we emphasize that we do not and cannot conclude that these aid flows have no impact on growth.

Rather, we conclude that standard growth regression analysis based on four year panel data is an inappropriate tool to examine the effect of those types of aid. A different approach is needed to explore these relationships, which we leave for future research.

This paper is organized as follows. Section two reviews three decades of literature on aid and growth, distinguishing studies that have found no impact of aid on growth, those that have found a positive relationship, and those that have found a conditional positive relationship in certain sub-groups of recipient countries. Section three outlines our method for disaggregating aid into disbursements for "short-impact" aid, "long-impact" aid, and humanitarian relief. Section four provides our core results. Section five provides a wide range of robustness checks on our core results. Section six carefully interprets the implications of our estimated coefficients and examines the heterogeneity of the relationship across countries. Section seven methodically dissects the differences between our results and two earlier well-known studies on aid and growth. Section eight outlines some broad policy implications. The final section reviews the main quantitative findings and suggests future directions.

2 Unfinished business in 30 years of research

2.1 Early evidence on the growth effect of aid

Studies of aid-growth linkages over the past four decades can be conveniently divided into several phases.[1] The earliest attempts are built on growth models like that of Domar (1947) and Rostow (1956) which assume that inflows of foreign assistance represent an exogenous, one-for-one increment to domestic capital. On this premise, a given desired rate of growth in the absence of other capital flows directly implies an aid requirement. These studies do not attempt to measure the aid-growth relationship directly. Pioneering studies in this vein include the work of two onetime World Bank chief economists, Paul Rosenstein-Rodan (1961) and Hollis Chenery (Chenery and Strout 1966). Their models were the basis for Marris's (1970) calculation of a $23-$28 billion "need for development assistance," roughly 1% of developed countries' GDP at the time.

Phase one begins with Griffin's (1970, p. 108) opening salvo against the earlier research: the assertion that "what evidence I have seen suggests that . . . it is quite possible that the slight positive effect of foreign capital in raising investment will be more than offset by a decline in the capital-output ratio, so that the growth rate actually falls." Griffin and Enos (1970) report zero or negative

[1] Mosley (1980) and Hansen and Tarp (2000) have previously divided the aid-growth literature into phases. Our attempt more closely resembles that of Mosley—extended to the present. There is also a rich empirical literature on linkages from aid to other outcomes such as savings and investment, which we omit for focus.

correlation between aid receipts and growth in 27 countries, sparking spirited response and counter-response articles in the top development economics journal of the day. The OLS regressions of Weisskopf (1972) demonstrate a significant negative correlation between foreign capital inflows and savings in 14 of 17 developing countries studied, in support of Griffin's argument. But the literature up to that point still lacks a broad test of the aid-growth relationship.

Papanek (1972) launches phase two with an attack on what he calls the "revisionist" literature above. He points out that savings were chiefly calculated by subtracting foreign capital inflows from investment—resulting in a vacuous negative correlation between foreign capital and savings. Papanek (1973) separates aid from other types of foreign capital and becomes the first to regress growth on aid, in a model resembling

$$\dot{y}_{i,t}/y_{i,t} = \alpha + \beta d^{net}_{i,t} + X_{i,t}\eta + \varepsilon_{i,t} \tag{1}$$

where $y_{i,t}$ is income per capita in country i at time t, $d^{net}_{i,t}$ is net disbursements of aggregate aid, $X_{i,t}$ is a vector of country characteristics, α and β are constants, η is a vector of constants, $\varepsilon_{i,t}$ is white noise, and a superscript dot represents the derivative with respect to time. Note that he includes no convergence term—a feature this literature only acquires in 1999 and thereafter. Papanek finds a strongly significant positive correlation between aid and growth in 51 countries during three five-year periods over 1950-1965. This global result is not statistically significant when he restricts the sample to the Americas, a finding echoed by Gulati (1978) in a 1966-1969 cross-section of 12 Latin American countries.

Phase three addresses at last the direction of causality, beginning when Mosley (1980) becomes the first to instrument for aid. His model resembles

$$\begin{aligned} \dot{y}_{i,t}/y_{i,t} &= \alpha + \beta d^{net}_{i,t} + X_{i,t}\eta + \varepsilon_{i,t} \\ d^{net}_{i,t} &= Z_{i,t}\zeta + \nu_{i,t} \end{aligned} \tag{2}$$

where $Z_{i,t}$ is a vector of exogenous instruments, ζ is a vector of constants, and $\nu_{i,t}$ is white noise. He finds a negative but insignificant effect of aid on growth over the period 1970-77 in an 83-country cross section. The 2SLS estimate of Dowling and Hiemenz (1982, p. 13) is qualitatively identical, in a three-period panel covering 1968-1979. Dowling and Hiemenz posit that aid may be poorly instrumented in both studies, though neither paper provides any information on what instruments were used or how well they captured variation in aid. There are suggestions in both studies that the data deserve further exploration. For example, when Mosley restricts his sample to the 30 poorest countries and lags aid by five years, or restricts the sample to recipients of bilateral aid from the United Kingdom, the coefficient on aid becomes positive and significant at the 5% level.

The remainder of phase three is a decade of controversy. Gupta and Islam (1983) and Levy (1988) claim a positive and significant effect of aid on growth, and Singh (1985), Mosley et al. (1987) and Boone (1994) find no significant effect. Gupta and Islam study cross sections of ten-year growth in 52 countries, finding the effect of instrumented aid to be significantly positive at the 10% level in the 1960s and at the 1% level in the 1970s. By restricting the sample to African countries and extending the period of observation to 15 years of growth, Levy finds a positive coefficient on aid significant at the 1% level in an OLS cross-section.

In a larger sample of countries, Mosley et al. find an effect of zero in the 1960s, 1970s and 1980-83 without instrumenting for aid, and report instrumented results only for the 1970s—where aid again has no effect on growth. It is noteworthy, however, that the coefficient on aid in the 1970s switches from negative to positive upon instrumentation (though still insignificant) and that the OLS coefficient on aid is positive during 1970-83 when the sample is restricted to Africa. Perhaps most importantly, Mosley et al. use a different definition of 'aid' than do the other studies: gross Official Development Assistance (ODA), rather than net ODA, without controlling for repayments on aid. In a footnote they find that the effect of *net* ODA is positive and significant at the 10% level, but dismiss this as unworthy of further exploration. Singh controls for a subjective indicator of "investment climate" and also finds no effect of aid in the 1960s and 1970s, though he does not instrument for aid. Shan (1994), using observation periods of only one year, finds a significant negative correlation between aid and growth in an OLS framework.

Finally Boone, in a table of six often-cited but unpublished growth regressions,[2] greatly extends the sample to cover 1971 to 1990, in 117 countries. His simple OLS regression with five-year periods and country fixed effects finds a coefficient of zero on aid. In ten-year growth periods without country effects, he finds a positive coefficient significant at the 10% level. The standard error on the latter estimate would reject the null hypothesis that the growth correlation of aid is negative or zero at the 5% level (one-tailed). Boone also ignores the potential endogeneity of aid to growth as well as the fact that his result is sensitive to the period of observation. He nevertheless concludes that "[t]he lack of robustness of the aid variable in the regression ... shows that aid does not create, nor correlate with, those underlying factors which cause growth" (p. 25). In the published version, Boone (1996) retains the claim that "aid programs have not ... engendered or correlated with the basic ingredients

[2] Parts of Boone's (1994) analysis were published in Boone (1996), but without his growth regressions.

that cause ... growth." This causal hypothesis would be easy to test, by instrumenting for aid (in the working paper growth regressions) or by using growth as a dependent variable (in the published version), but Boone does neither.

2.2 The micro-macro paradox: Apologists and gainsayers

Boone's is the paper that launched a thousand regressions. Many researchers have taken Boone's findings as confirmation of a "macro-micro paradox": that so many aid-funded projects report positive micro-level economic returns somehow undetectable at the macro-level. Here begins phase four, in which the literature splits into two strands—one trying to explain the paradox and the other denying its existence.

2.2.1 The 'conditional' strand

The first or "conditional" strand takes Boone to be more or less correct: the average effect of aid across all countries is indeed roughly zero, and only in some countries at some times it is positive. For this group, the challenge is to identify the characteristics that separate those countries (or time periods) in which aid has a positive impact on growth from those where it is negative or zero. This strand has proposed a wide variety of variables as candidates for the country characteristic upon which any positive effect of aid depends, including measures of economic governance, timing, and location. These variables have included inflation, budget balance, and "openness" (Burnside and Dollar 2000, Kudlyak 2002); export price shocks (Collier and Dehn 2001); climatic shocks and trends and volatility in the terms of trade (Guillaumont and Chauvet 2001, Chauvet and Guillaumont 2002); policy and institutional quality (Collier and Dollar 2002); institutional quality alone (Burnside and Dollar 2004); policy and warfare (Collier and Hoeffler 2002); 'totalitarian' government (Islam 2003); an index of economic freedom (Ovaska 2003)[3]; and the degree to which aid is fungible across sectors in the recipient country (Pettersson 2004). All of these studies, unlike Boone and all of his predecessors, also modify the specification to include initial income per capita in some form. The model they estimate resembles

$$\begin{aligned} \dot{y}_{i,t}/y_{i,t} &= \alpha + \beta d^{net}_{i,t} + \gamma q_{i,t} + \delta \left(d^{net}_{i,t} \times q_{i,t} \right) + X_{i,t}\eta + \theta \ln y_{i,t} + \varepsilon_{i,t} \qquad (3) \\ d^{net}_{i,t} &= Z_{i,t}\zeta + \nu_{i,t} \end{aligned}$$

[3] Ovaska (2003) concludes, "Overall, the results of this study do not provide support for the notion that international development aid—at least as in practice between 1975 and 1998—helps developing countries to higher growth trajectories." This conclusion is odd, given that the study's full-sample, unrestricted regression shows aid as having a large, unconditional, significant, positive impact on growth, with diminishing returns (Table 2, column 4). The study is moreover fundamentally flawed in that, unlike all other recent empirical aid-growth work, it strangely controls for investment, education, and government spending on the right-hand side—presumably the very channels through which aid could affect the economy!

where $q_{i,t}$ is some country characteristic on which the effect of aid depends, and γ and δ are constants. Four of the aforementioned studies, when they do not allow for a heterogeneous aid effect (i.e. constraining $\delta = 0$), cannot detect any significant growth effect of aid (Burnside and Dollar 2000, 2004; Chauvet and Guillaumont 2002; Pettersson 2004). The rest focus on measuring δ and do not report results on the significance of the aid term in the absence of an interaction effect.

Nevertheless, the underlying goal of this literature has been to "provide some insight into why aid is not effective in the typical recipient country" (Burnside and Dollar 2000, p. 864). The 'conditional' strand of the literature has been very influential in policy circles, in particular the study by Burnside and Dollar (2000). This research has supported moves by the World Bank, other multilateral development banks, and some donor governments to be more "selective" with their aid and allocate a larger share to countries with stronger policies and institutions where it presumably can have a larger impact on growth.

While many of the interaction effects posited by the 'conditional' strand are intuitively plausible, Roodman (2003) and Easterly, Levine and Roodman (2004) have recently cast serious doubt on the qualitative conclusions of a majority of these studies. They find that the significance of the interaction coefficient δ is frequently highly sensitive to influential observations and extensions of the dataset. Roodman's ongoing work finds that estimates of δ in much of this literature are thus fragile to updated data, influential observations, and corrections for autocorrelation.[4]

2.2.2 The 'unconditional' strand

The second or "unconditional" strand of the phase four literature argues instead that Boone is wrong. Though aid does not have the same effect everywhere, it does have on average a positive growth impact—large and detectable across recipients without conditioning the effect on any other country traits. This strand posits simply that Boone's regression specification is wrong in one of several ways, none of which depends upon heterogeneity of the aid effect.

The plurality of research in this strand argues that Boone's assumption of a linear impact of aid on growth is less realistic than a nonlinear relationship between the two, in which aid is subject to

[4] He finds that the results of Hansen and Tarp (2001) and Daalgard et al. (2004) are relatively robust to expansion of the dataset, correction for serially correlated errors, and other tests. The results of Collier and Dehn (2001), Collier and Hoeffler (2002), Guillaumont and Chauvet (2001) are weakened by Roodman's tests but do not entirely disappear. The results of Burnside and Dollar (2000) and Collier and Dollar (2002) are very fragile. Of the studies whose results Roodman's tests confirm, Hansen and Tarp (2001) and Dalgaard et al. (2004) report tests of the aid-growth relationship without an interaction term, and find it to be positive and significant; we thus include them in the "unconditional" strand of the recent literature despite the fact that Dalgaard et al. focus on the interaction term between aid and tropical area. In short, none of the recent studies that survive Roodman's interrogation both 1) tests an unconditional relationship between aid and growth and 2) fails to find such a relationship.

diminishing returns and there is some limit to recipients' capacity to absorb more aid productively.[5] The first to allow for a nonlinear aid effect are Hadjimichael et al. (1995), who find a strongly positive impact of aid with diminishing returns in a Generalized Least Squares cross section of 31 African countries, 1986-1992. Durbarry et al. (1998) find the same result in an expanded sample of 58 LDCs across 1970-1993, again using GLS. Dalgaard and Hansen (2000) are the first among these to instrument for aid. Hansen and Tarp (2000, 2001) also find a strong, unconditional, nonlinear impact of instrumented aid on growth that does not depend on influential observations. They are the first to employ a Generalized Method of Moments estimator in an aid-growth regression. They conclude unequivocally, "The micro-macro paradox is *non-existent*" (Hansen and Tarp 2000, emphasis in original). Lensink and White (2001) confirm the nonlinear impact in a greatly expanded sample of 111 countries, 1975-1992, with a 2SLS estimator. Dalgaard et al. (2004), although they emphasize the significant interaction between aid and fraction of land in the tropics, find an unconditional positive and nonlinear effect of aid in the absence of the interaction. In short, each of these estimates a model similar to

$$\begin{aligned} \dot{y}_{i,t}/y_{i,t} &= \alpha + \beta d_{i,t}^{net} + \lambda \left(d_{i,t}^{net}\right)^2 + X_{i,t}\eta + \theta \ln y_{i,t} + \varepsilon_{i,t} \\ d_{i,t}^{net} &= Z_{i,t}\zeta + \nu_{i,t} \end{aligned} \tag{4}$$

A number of other studies do not include a squared aid term, but belong in the "unconditional" strand because they find an impact of aid on growth not conditioned on interaction with any other recipient-country trait. Each modifies the specification or estimator used by Boone in some way other than adding a squared aid term. Lensink and Morrissey (1999) include an indicator of uncertainty in aid flows constructed by comparing aid commitments and disbursements, and extend the observation period to 25 years. They are also the first among aid-growth researchers to include a convergence term in their regressions. Their OLS cross section of 75 countries finds a strongly significant, positive, linear relationship between aid and growth. Cungu and Swinnen (2003) focus on the interaction effect between aid and their indicator of 'economic liberalization', but find that one-year lagged aid has a positive, significant, linear effect on growth unconditional on any interaction. Dayton-Johnson and Hoddinott (2003) uniquely add country fixed effects and find a strongly positive, linear impact of instrumented aid on growth in countries outside Sub-Saharan Africa. Gomanee et al. (2003) find no evidence of diminishing returns but *do* find a nonlinear impact of aid unconditional on any interactions—once countries are past a certain threshold of aid receipts. Moreira's (2003) differenced

[5] Heller and Gupta (2002) review the empirical evidence that very large aid flows can induce Dutch Disease, overwhelm management capacity, undermine alternative revenue collection, and provide greater opportunities for corruption, among other impediments to growth. Hodler (2004) derives an 'inverted U' aid-growth relationship from an endogenous growth model with redistribution to rent-seeking agents.

GMM results show a highly significant, positive, nonlinear impact of aid in 48 countries from 1970 to 1998. Economides et al. (2004) endogenize quality of governance as a determinant of growth, and find a strongly significant, positive, linear impact of aid unconditional on interaction effects.

The ongoing research of Roodman (2003) tests the sensitivity of results from three papers in the 'unconditional' strand to expanded datasets and correction for serially correlated errors. He finds the regressions of Dalgaard et al. (2004) to be quite robust to these changes. The qualitative conclusions of Hansen and Tarp's (2001) GMM results are also robust, though with a larger standard error on the linear aid term.

2.3 A third path: All aid is not alike

As we describe in more detail later in the paper, in this study we propose a third reaction to Boone, a road not traveled by these two strands of recent work, that focuses on matching aid flows to a realistic time period over which they might influence growth.

A review of the earlier research reveals that in almost all studies, the coefficient on instrumented aid is significantly positive when it is allowed ten or more years to have its effect on growth (Mosley; Gupta and Islam; Levy). Boone's results (though he does not instrument for aid) also show that when the periods of observation are extended to ten years across his full sample of countries, the hypothesis that the effect of aid is zero or negative is rejected at the 5% level (one-tailed). Among the pre-phase four studies that do instrument for aid there is only one exception to this pattern: Mosley et al., whose results are—as discussed above—sensitive to their unique definition of aid. Thus, the time period over which the aid-growth relationship is evaluated seems to have a significant bearing on the results.

Any evaluation of a policy intervention without a control group faces an inescapable tradeoff between comprehensiveness and attribution. A long-run analysis captures long-run effects but cannot confidently attribute those effects to the intervention because so many other changes can occur in the interim. A short-run analysis can be more confident of attribution but is less likely to have captured the full effect. We can, however, partly escape this conundrum to the degree that we can identify elements of the intervention whose effects are more likely to be realized in the short run.

Suppose a donor uses a million dollars to build a road in a given country, and another million to build several primary schools. Both the road and the schools are complete within one year. An investigator now wishes to measure the growth impact of the donor's *entire* investment of two million dollars. When should she conduct her assessment? If she does it two years later, she is likely to be able to measure a significant part of the impact of the road, which will already be carrying factors and products to and fro as part of the public capital stock. She will miss almost entirely, however, the impact of the schools on the productive capital stock—aside from the little that some of the construction workers might have invested from their wages. Her estimate of the returns to the entire $2 million portfolio is biased sharply downwards. On the other hand, if she conducts the study 20 years later, when the educated children have brought their human capital to the workforce, much else will have happened in the country. Attribution of changes in growth to the road or to the schools will be extremely difficult, and the effect of the entire aid portfolio cannot be reliably distinguished from zero. *Neither* of these studies can detect the full impact of the two projects jointly.

There are two options for evaluation in facing this tradeoff: varying the period of observation, or disaggregating aid flows (in effect to measure the impact of the road and school separately). First, a longer period may capture more of the long-term effect but can greatly increase noise and impede attribution of growth events to causal aid events in the distant past. Two recent papers have taken this approach. Easterly (2003) addresses the issue by analyzing the Burnside and Dollar dataset with observation periods of 8, 12, and 24 years. The coefficient on aid is insignificantly different from zero throughout, in the presence of an aid-policy interaction term. He does not report 2SLS tests without the (insignificant) aid-policy interaction term, does not allow for a nonlinear effect of aid, and adheres to Burnside and Dollar's definition of the aid variable as "Effective Development Assistance" (EDA) divided by GDP at Purchasing Power Parity (as we discuss later in the paper, this unusual definition of the aid variable is open to question). Brumm (2003) uses a cross section of 23-year periods in his regressions otherwise mimicking those of Burnside and Dollar. Brumm does not test for the growth effect of aid in the absence of the policy interaction term, does not allow for a nonlinear impact of aid, and does not instrument for aid or any other variable. Despite his longer observation period, then, Brumm's brief note does not improve on the aid-growth literature of the 1970s for the purpose of inquiring whether or not aid causes growth; it certainly does not represent grounds for his conclusion that "foreign aid negatively affects economic growth."

With respect to disaggregating aid flows, other very recent studies have in some way considered aid disaggregated by purpose, but none with the aim of analyzing the growth impact of aid within an appropriate time horizon. Owens and Hoddinott (1999) find that household welfare in Zimbabwe is increased by development aid (infrastructure, agricultural extension, etc.) far more than by humanitarian aid (food aid, emergency transfers, etc.), even in humanitarian emergencies. Mavrotas (2002) disaggregates aid to India into "program," "project," and "technical assistance" flows, and finds a negative correlation between growth and all three types of aid in India 1970-92. Mavrotas (2003) uses a time-series error-correction model to test the growth impact of aid in Uganda. He finds a significantly positive effect of "program" aid much larger than than of "project" aid, but significantly negative impacts of technical cooperation and food aid. The study of Gomanee et al. (2002) on aid and growth in Africa excludes food aid and technical cooperation from the ODA measure, especially since the latter would only affect growth "with a long time lag." Cordella and dell'Ariccia (2003) find that the policy sensitivity of aid's growth impact measured by Burnside and Dollar differs for "program" (non-sector allocable) and "project" (sector-specific) aid. Pettersson (2004) disaggregates aid by sector in order to classify aid according to whether or not it is intended for a sector in which aid has been found to be fungible, and finds no difference between the growth effects of "non-fungible" sector aid and "fungible" sectoral aid, at a given policy level.

To continue the road-and-school example from earlier, the investigator escapes the problem almost entirely if she can observe how much of total aid was spent on the road. She simply sets out to measure the impact of the road project—or any other aid flows that might affect growth over a short period—on growth, and conducts her study after two years. Though she remains uninformed about the impact of the school project (or other longer-term aid flows), she can at least confidently claim to capture something resembling the full impact of the road project. This is, in essence, what our study sets out to do: investigate the short-run impacts of that part of aid whose effects on growth can reasonably be expected to affect growth in the short-run. We neither can nor endeavor to make any statement about the long-run effects of aid whose expected effect comes in the long run.

3 Isolating the "short-impact" component of aid

We divide aggregate aid into three mutually exclusive, collectively exhaustive categories:

- *Short-impact aid* is defined as an aid disbursement funding an intervention that can plausibly raise GDP per capita within roughly four years to a permanently higher level. We choose this time window since nearly all recent aid-growth studies have employed cross-country panels of four (or occasionally five) years.
- *Long-impact aid* funds an intervention that might permanently raise GDP per capita but is unlikely to do so within roughly four years of the disbursement.[6]

[6] We assume that all other characteristics of "short-impact" and "long-impact" aid aside from the growth impact of that which they finance are equal. In particular, we assume that the time elapsed between receipt of a disbursement and its expenditure does not vary systematically between the two aid types by more than a fiscal year.

- *Humanitarian aid* is the small portion of total aid that is for very short-term consumption smoothing and is not intended to directly promote long-term increases in income per capita—such as emergency food aid during natural disasters.

While most previous studies implicitly assume that all of these types of aid affect current growth in exactly the same way and estimate the relationship between growth and "net" aid (gross aid inflows minus debt repayments), we instead estimate a model resembling

$$\begin{aligned} \dot{y}_{i,t}/y_{i,t} &= \alpha + \beta^s d^s_{i,t} + \beta^l d^l_{i,t} + \beta^h d^h_{i,t} + \beta^r \ln r_{i,t} + S_{i,t}\lambda + X_{i,t}\eta + \theta \ln y_{i,t} + \varepsilon_{i,t} \\ d^{net}_{i,t} &= Z_{i,t}\zeta + \nu_{i,t}, \end{aligned} \tag{5}$$

where $d^s_{i,t}$ is gross disbursements of "short impact" aid, $d^l_{i,t}$ is gross disbursements of "long impact" aid, $d^h_{i,t}$ is gross disbursements of "humanitarian" aid, and $r_{i,t}$ is gross repayments on aid, giving the identity $d^{net}_{i,t} = d^s_{i,t} + d^l_{i,t} + d^h_{i,t} - r_{i,t}$. Lastly, $S_{i,t}$ is a vector of squared aid disbursements of the various types, and λ is a vector of constants. In terms of (5), most of the recent aid-growth literature has assumed that $\beta^s = \beta^l = \beta^h = -\beta^r$, as well as making a complex set of assumptions about λ such that all types of aid are nonlinear in the same way.[7] We relax, and test, that supposition. Earlier studies have assumed variously that growth did not enter $Z_{i,t}$, that $\theta = 0$, or that $\lambda = 0$. We relax all of these assumptions.

In rough terms, "short-impact" aid is 1) budget support or "program" aid given for any purpose, and 2) project aid given for real sector investments for infrastructure or to directly support production in transportation (including roads), communications, energy, banking, agriculture and industry. These are expenditures whose growth effects might be observed to some degree within a horizon of roughly four years. "Long-impact" aid is 1) technical cooperation given for any purpose, and 2) most social sector investments, including in education, health, population control, and water. "Humanitarian" aid includes emergency assistance and food aid.

Perhaps the most important reason why an analysis of this kind has not previously been attempted is that disaggregated aid disbursements are not readily available. Remarkably, there is no centralized and standardized database that disaggregates foreign aid disbursements according to details of their purpose. Although the Development Assistance Committee (DAC) of the Organization for Economic Cooperation and Development (OECD) has compiled disaggregated aid disbursements covering much of the 1990s for many (but not all) of its donor members, it has not done so for earlier

[7] There are minor variations on this theme, such as Mosley et al. (1987) who assume $\beta^s = \beta^l = \beta^h$ and $\beta^r = 0$, and Gomanee et al. (2002) who roughly assume $\beta^s = \beta^l = -\beta^r$ and $\beta^h = 0$.

years—with the exception of emergency humanitarian aid. Otherwise, the data for the 1970s and 1980s is reported as commitments rather than disbursements. Short-impact aid disbursements and long-impact aid disbursements must therefore be estimated.

The OECD's Creditor Reporting System (CRS) does report aid *commitments* disaggregated by 233 distinct purposes, for all donors and recipients annually since 1973.[8] Examples of the OECD's detailed purpose categories include "plant and post-harvest protection and pest control" (code 31192) and "population policy and administrative management" (code 13010). Each purpose is further split into four categories (which the OECD calls "prefix code"): "investment project," "other resource provision including commodities and supplies," "technical cooperation," and "program aid/cash."

We estimate short-impact, long-impact, and humanitarian aid disbursements in three steps. First, prior to any statistical analysis we assign all 233 OECD purpose codes to one of three categories: short-impact, long-impact, or humanitarian. We begin this process by assigning all aid flows to "long-impact," and then reclassifying selected flows to "short-impact" if we are confident that a large portion of their full impact on growth could be realized within less than eight years. Categories that are moved over to "short-impact" include real sector investments in transportation (including roads), communications, energy, banking agriculture and industry. Commodity and general program assistance is also moved to "short-impact." In each of these groups, monies spent on policy formulation are kept in "long-impact." Items that remain in "long-impact" are most social sector investments, including in education, health, population control, and water. Emergency assistance and food aid are placed in "humanitarian." This results in a three-way division of aggregate aid we term "Definition 1."

We further refine this classification by making use of the four "prefix codes." All aid for technical cooperation, regardless of its purpose, is classified as "long-impact." This reflects our belief that technical cooperation, to the extent that it can affect growth, does so as an input to a complex process of long-term institutional change (e.g. Browne 2002). All "program aid/cash" flows, regardless of purpose, are classified as "short-impact." This springs from the belief that it is at least possible that money provided as budget support could be spent in such a way as to influence growth within a small number of years. The remaining two "prefix" categories, "investment project" and "other resource provision," are divided by purpose as above. This additional classification results in an

[8] The OECD defines a commitment as "a firm obligation, expressed in writing and backed by the necessary funds, undertaken by an official donor to provide specified assistance to a recipient country or a multilateral organisation. Bilateral commitments are recorded in the full amount of expected transfer, irrespective of the time required for the completion of disbursements." A disbursement is defined as "the release of funds to, or the purchase of goods or services for a recipient; by extension, the amount thus spent. Disbursements record the actual international transfer of financial resources, or of goods or services valued at the cost to the donor."

alternative, somewhat more refined three-way division of aggregate aid we call "Definition 2." The "Definition 2" aid measures in each category are very highly correlated with those using "Definition 1." While we use definition 2 in all of our regressions, we will make use of Definition 1 when checking the validity of these disbursement estimates.

Having thus classified the 275,590 donor-recipient aid transactions in the CRS database from 1973-2001 as short-impact, long-impact, or humanitarian,[9] we then aggregate these by recipient country so that each recipient in each year has an observation for each of the three types. Table 1 summarizes this classification for aid in the 1997-2001 period, disaggregating only to OECD's three-digit level. In that period, about 8% of aid was humanitarian, and the remainder split roughly equally between short-impact and long-impact.[10] Appendix Table 1 shows our full classification of all the five-digit CRS categories as short-impact, long-impact, or humanitarian. We omit donor's administrative costs from all of our aid measures.

The second step is to assume that the fraction of *disbursements* in each of our three aid categories in a given period is equal to the fraction of commitments in each category in that period. For example, short-impact aid as a fraction of GDP is estimated as

$$d^s_{i,t} = d^{agg}_{i,t}\left(\frac{d^s_{i,t}}{d^{agg}_{i,t}}\right) \approx d^{agg}_{i,t}\left(\frac{c^s_{i,t}}{c^s_{i,t}+c^l_{i,t}+c^h_{i,t}}\right) \tag{6}$$

where d^s is "short-impact" gross disbursements, d^{agg} is aggregate gross aid disbursements, and c represents commitments of the various types. Naturally, (6) will differ from equality the more commitment fractions of each type vary over time, the longer the delay between commitment and disbursement, and the more variation in this delay there is between different aid types.

Thus the third step is to check how well (6) approximates true disbursements of short-impact aid. As noted above, the CRS database does contain some information on disbursements classified by purpose for the 1990s. We therefore use our method to estimate disaggregated disbursements using disaggregated commitments data for the 1990s, and compare the estimates to the true values. One minor complication in this enterprise is that for the disaggregated disbursements of the 1990s, the CRS disaggregates aid disbursements only by purpose and not according to whether they were

[9] We eliminate from our aid measure the miniscule fraction of aid classified by the CRS as financing donors' administrative expenses. In 1997-2001, this was roughly 0.1% of aid.

[10] Since each three-digit category includes multiple five-digit categories, many contain multiple types of aid. To illustrate this, we include in the table as an example the full five-digit breakdown of code 220, "communications". We take "telecommunications" (code 22020) to be short-impact aid, except for a small portion that is given as technical cooperation. Support for "communications policy" (code 22010) is considered long-impact aid, except for a small portion that is given as program aid or budget support.

technical cooperation, program support, and so on (the "prefix" code). That is, the CRS only allows us to disaggregate disbursements in the 1990s according to Definition 1, not Definition 2. Though we will use the more refined Definition 2 in all regressions, we employ Definition 1 to assess the validity of our approximations.

Table 2 shows summary statistics for actual and estimated disaggregated aid flows. The units of observation are single years. The first two lines show net and gross aggregate ODA for the 1990s. The next three lines summarize estimated disbursements for the 1990s according to (6), where the three categories of aid have been delimited by Definition 1. These are comparable to the summary statistics for actual disbursements immediately below. Following these are summary statistics for estimated disbursements according to Definition 2. The numbers suggest that 1) our estimation procedure is capturing a great deal of information about true disaggregated disbursements, and 2) Definition 1 aid barely differs from Definition 2 aid. The bottom half of the table reports similar summary statistics for the entire 1973-2001 period, omitting of course the unobserved actual disaggregated disbursements during that period.

Figure 1 plots actual disbursements of each type against our estimates, both using Definition 1 and covering the 1990s. The figures suggest that our estimates are in fact highly correlated with disaggregated disbursements of each type. As expected, humanitarian aid is not predicted quite as well, since much emergency aid is not formally committed prior to disbursement. Table 3 shows Pearson's correlation coefficients among the various types of estimates and actual aid in the 1990s. The highlighted cells reveal that our estimation method predicts disaggregated disbursements to a high degree of accuracy, particularly in the case of short-impact disbursements.

We are only able to test the relationship between estimated and actual short-impact aid in the 1990s but not over the entire period 1973-2001. If some part of the 1990s represents an atypical sample of the commitment-disbursement relationship during the entire period, this could systematically bias our results. To partially address this issue, we bootstrap the 1990s data to determine whether or not subsamples of the 1990s data give significantly different results. We draw 694 observations with replacement to calculate the correlation coefficient, and repeat this exercise 1,000 times. This allows us to construct bias-corrected confidence intervals (see Efron and Tibshirani 1994) for the correlations in Table 3. The 95% confidence interval for the correlation between actual and estimated (Definition 1) short-impact aid is (0.82, 0.89), for long-impact aid (0.72, 0.84), and humanitarian aid (0.50, 0.80). The fact that our prediction of actual disaggregated disbursements remains strong in subsamples of the 1990s reinforces our application of the same estimation technique to earlier decades.

Certainly there are many other ways in which the CRS disaggregated commitments data could be exploited to yield disaggregated disbursement estimates besides the simple method of (6). We experimented with several and found (6) to give the best results. For instance, one might exploit the identity

$$\frac{d_{i,t}^{s}}{d_{i,t}^{agg}} = \sum_{\tau=0}^{\infty} \left(\frac{c_{t-\tau}^{s}}{c_{t-\tau}^{agg}}\right) \left(\frac{c_{t-\tau}^{agg}}{d_{t}^{agg}}\right) \left(\frac{c_{t-\tau}^{s,\tau}}{c_{t-\tau}^{s}}\right) \tag{7}$$

where c_t^s is commitments of short-impact aid made a time t, *whether or not they are ever disbursed* and $c_t^{s,\tau}$ is commitments of short-impact aid made at time t and which are *disbursed as planned* at time τ. The subscript i is omitted for clarity. The first two terms of the summation are known from the CRS database for the entire period 1973-2001. The final term could be estimated from the 1990s disbursement data, since each purpose-disaggregated disbursement reports the year in which it was committed, and assumed to hold for all 28 years. This method, and others like it, did not yield estimates that matched actual disbursements data as well as method (6) by criteria of correlation and mean squared error. The complexity of (7) is likely to blame. Estimation of each parenthetical term introduces a measurement error, compounded by multiplication. In practice, furthermore, an additional estimated scaling factor must be used to account for the fact that the dataset contains a finite number of years—introducing yet more error to the product. After testing this and many other similar methods we selected (6) as superior.

4 Core results

We regress growth on aid in a manner typical of the aid-growth literature, but simply use a measure of aid that has some possibility of affecting growth within the period of observation: short-impact aid. Table 4 presents our core result: short-impact aid causes short-run growth, and does so to a much greater degree than previous estimates on aggregate aid.

Column 1 of Table 4 essentially follows the practice of many previous studies: average growth in real GDP per capita is regressed on aggregate net aid (gross aid minus repayments), aggregate net aid squared, and initial GDP per capita. Aid is instrumented, here and in the rest of the table, by all the independent variables supplemented with instruments drawn from Hansen and Tarp (2001). These are a dummy for Egypt, arms imports, a lagged policy index[11] and its square, population interacted with policy, GDP and its square interacted with policy, and each of the lagged aid variables and the lagged aid variables interacted with policy. The coefficient on "net" aid is not significantly different from zero.

[11] Constructed as a linear combination of the three policy variables included in our preferred specification—budget balance, openness variable and inflation. Weights are determined using the method of Burnside and Dollar (2000).

In column 2 we make one change: we substitute gross short-impact aid for net aggregate aid. The coefficient roughly triples in magnitude and becomes significant at the 5% level. Diminishing returns are evident in the squared term, also significant. This minimal model obviously omits several important growth factors, and accordingly Hansen's (1982) J statistic rejects exogeneity of the instruments at the 5% level. But the large change in the coefficient on aid suggests that there is indeed growth information contained in the short-impact aid flows.

Column 3 adds a control for repayments on aid, per equation (5),[12] as well as a set of covariates central to the most prominent papers in the recent aid-growth literature. These consist of a unique intercept for East Asian countries,[13] an index of institutional quality from the International Country Risk Guide, and indicators of monetary, fiscal, and trade policy.[14] The Hansen test now fails to reject exogeneity of the instruments. Column 4 allows tropical countries an idiosyncratic intercept (similarly to Dalgaard et al. 2004), includes log life expectancy as in Radelet, Sachs and Lee (2001) *inter alia* to capture broader health conditions, and includes controls for civil war as in Collier and Hoeffler (2002). Throughout, the signs of the coefficient estimates are intuitive: being in East Asia, scoring 1 for the Sachs and Warner (1995) "openness" indicator, a higher ICRG institutional quality index, health conditions conducive to longer life expectancy, and recovering from civil warfare are all associated with more rapid growth. Location in the tropics, higher inflation, and current civil war are associated with slower growth. Running a smaller budget deficit is correlated with faster growth, though not to a statistically significant degree. Note that almost all of the right-hand side variables are highly statistically significant, and that they jointly explain about 38% of the variance in growth rates. We are confident that this set of regressors represents a significant improvement over past research on aid in terms of explaining economic growth.

Controlling for these other factors only strengthens the observed conditional relationship between short-impact aid and growth, which grows in magnitude and becomes significant at the 1% level. This result is not driven by influential observations, as Figure 2 reveals. The procedure of Hadi (1992) classifies four observations in column 4 as outliers at the 5% level: Gabon 1974-77, Jordan 1986-89, Nicaragua 1994-97, and Zambia 1994-1997. Removing these observations only strengthens

[12] Essentially the entire aid-growth literature has used some form of net aid flow as its regressor (with the exception of Mosley et al. 1987, who do not control for repayments). Since it is meaningless to disaggregate repayments by purpose, we must set aside repayments as a separate regressor. Repayments on aid are assumed to have a nonlinear effect through the log operator, but are not allowed a parabolic term since this presumes that a sufficient quantity of repayments could actually increase growth. We relax and test this assumption in a later table.

[13] 'East Asia' is Rep. of Korea, China, Indonesia, Malaysia, Philippines, Singapore, and Thailand.

[14] Hansen and Tarp (2000) and Burnside and Dollar (2000), among others, include an East Asia dummy, inflation, budget balance, and the "openness" indicator of Sachs and Warner (1995). Collier and Dollar (2002) use the same ICRGE institutional quality index used herein. Burnside and Dollar (2004) use a similar institutional quality measure that is highly correlated with the ICRGE index.

the fit, as the figure shows and as column 5 of Table 4 demonstrates. The test of Arellano and Bond (1991) for first-order autocorrelated errors rejects the hypothesis of no first-order autocorrelation at the 5% level but fails to reject hypotheses of no second and no higher-order autocorrelation. Clustered standard errors are thus used throughout the table.

The test of Pagan and Hall (1983) rejects the hypothesis of homoskedastic errors in column 5 at the 5% level, suggesting that the generalized method of moments will yield more efficient inference than two-stage least squares (Baum et al. 2003). Column 6 reports the GMM results. The coefficient on short-impact aid is essentially unchanged. Life expectancy enters significantly at the 1% level. The convergence term is negative, though not statistically significant. While we consider the GMM estimates to be of interest as a check on our findings, we retain 2SLS as our core specification for most of the remaining analysis because 1) using GMM does not materially affect the coefficient estimates of interest, 2) 2SLS facilitates comparisons to most of the extant aid-growth literature, 3) 2SLS is a more transparent estimator to most of our audience, and 4) no systematic procedure comparable to Hadi's exists for identifying influential observations in a GMM setting. Thus we consider column five of Table 4—using 2SLS and excluding the four outliers—as our core result.

The magnitude of the coefficient on gross short-impact aid in columns 5 and 6, exceeding 0.9, is much higher than that estimated on net aggregate aid in nearly all preceding studies. It is not an artifact of autocorrelation, weak or endogenous instruments, influential observations, or reverse causation of aid by growth. In the next section we test the robustness of the result to changes in specification, potential endogeneity of other regressors, and further modifications. Moreover, we recognize that by using short-impact aid as the only aid measure on the right-hand side, we are implicitly assuming that the current growth effects of long-impact and humanitarian aid in equation (5) are zero. In the next section we relax and critique these assumptions as well.

5 Robustness of the core result

Cross-country growth regressions are not in fashion. This fact alone is insufficient reason to forsake cross-country analysis entirely in exclusive favor of country case studies incapable of informing donors about the effectiveness of their overall aid portfolios. We choose instead to address as thoroughly as we can the shortcomings of most cross-country analyses that have disaffected many of their former admirers.

Temple (1999) summarizes several widespread concerns about fragility in cross-country growth regressions. These include but are not limited to sensitivity to specification, parameter heterogeneity, endogenous regressors, measurement error, influential observations, and spatial and temporal correlation of errors. Along these lines, Roodman (2003) and Easterly, Levine and Roodman (2004) challenge the entire modern cross-country aid-growth literature, revealing the sensitivity of most (but not all) recent empirical results to influential observations, specification, and estimator. These two papers have permanently raised the credibility bar that all subsequent studies must clear. In this section we explore all of Temple's critiques and find our core result to be robust.

5.1 Sensitivity to changes of specification

In all preceding discussion we have used short-impact aid as our preferred measure of aid flows, implicitly assuming that long-impact aid and humanitarian aid do not have short-run effects on growth. We now relax and test this assumption.

Table 5 begins by estimating the growth impact of aggregate gross ODA, controlling for log repayments on aid. The magnitude of the estimated coefficient is similar to that found in many papers in the "unconditional" strand of the literature, such as Hansen and Tarp (2000, 2001). Column 2 then splits gross ODA into the three types, estimating the unrestricted model in equation (5). The standard error on the short-impact aid coefficient increases, likely due in part to the high correlation between short-impact and long-impact aid seen in Table 3, but short-impact aid remains significant at the 10% level. When the coefficient on long-impact aid is restricted to zero (column 3), the coefficient on short-impact aid increases slightly and the standard error falls notably, suggesting indeed that the standard error in column 2 was inflated by collinearity. The four-year growth effect of humanitarian aid is indistinguishable from zero, and is thus restricted to zero in column 4.

Together, these results confirm the validity of our earlier exclusion of long-impact and humanitarian aid. This restriction increases the efficiency of our inference on the effects of short-term aid without affecting the qualitative conclusion that the effect of short-impact aid is much larger than that of aggregate aid or the other types of aid individually. Even though short-impact aid and long-impact aid levels are typically correlated to a degree of 0.635, the coefficient on short-impact aid is not substantially biased upwards by excluding long-impact aid from the regression.

Table 6 tests the sensitivity of our core result to a set of additional changes in specification. To begin, we explore the nonlinear relationship between repayments on aid and growth. As discussed above, we chose a logarithmic model for repayments out of distaste for the idea of a parabolic turning point past which any level of repayments helps growth. This introduces an asymmetry in the model, however, since we model the nonlinear impact of all gross aid inflows as parabolic. In column 1, repayments are allowed a parabolic effect. The coefficient on short-impact aid only increases; our core finding is unaffected by the change.

The balance of the table introduces several other variables found relevant in different strands of the empirical growth literature. These are land area and coastline (e.g. Radelet, Sachs and Lee 2001), secondary school enrollment (which e.g. Mankiw, Romer and Weil 1992 favor over primary enrollment as an input to their overall human capital measure) the Sachs and Warner (2000) indicator of natural resource abundance, Collier and Dehn's (2001) positive and negative commodity shock indices, and a measure of current and lagged natural disasters.

In four of the five cases the coefficient on short-impact aid is entirely unaffected. Only in the case of the Collier-Dehn commodity shock is the coefficient slightly diminished (though not to a statistically significant degree). This result is broadly consistent with Collier and Dehn's findings of an interaction effect between these shocks and aid, a result robust to the recent analysis of Roodman. Since the commodity shock variables were both statistically significant at the 5% level, we considered using this specification as our core result, but since including these variables led to a loss of 59 observations we use it as a cross-check rather than as the core. Importantly, we note that the coefficient on short-impact aid remains large and significant even when controlling for these shocks in the absence of an interaction term. This is at variance with Collier and Dehn's results, in which the "net" aid term without an interaction with shocks is statistically insignificant. We find that the insignificance of the "net" aid term in Collier and Dehn is because the majority of flows comprised by their aggregate aid measure could not meaningfully affect growth within a four-year period. This is an apt demonstration of our statement on the "conditional" literature: we do not deny that the effect of aid is heterogeneous across countries—on the contrary we do find some evidence for it in the next section—but we find that accounting for this heterogeneity is not at all necessary to detect the impact of foreign aid in macroeconomic data.

5.2 Parameter heterogeneity and endogenous regressors

Pesaran and Smith (1995) point out that if the coefficient on aid is heterogeneous across countries—and doubtlessly it is—dynamic panel regressions will lead to inconsistent results if aid is first-order serially correlated since this will induce first-order serial correlation of the disturbances. The current disturbance will then be correlated with lagged growth, which in turn is correlated with period-initial GDP per capita, biasing all the coefficients including the coefficient on aid. Intuitively, correlation of the errors arises because parameter heterogeneity implies an omitted interaction term between the regressor of interest and some other country characteristic. This becomes part of the error, and since the regressor of interest is serially correlated, the error will be serially correlated as well. Since the lagged dependent variable is correlated with the lagged error, serial correlation of the error means that the contemporaneous error too will be correlated with the lagged dependent variable on the right hand side. In other words, parameter heterogeneity produces endogeneity bias in the presence of 1) a serially correlated regressor and 2) a lagged dependent variable.

This is particularly important since the "conditional" strand of the literature has argued that effect of aid on growth is not homogenous across groups. But it is certainly not the only reason to believe that some of our regressors may be endogenous. Up to this point we have instrumented only for aid, aid squared, and repayments on aid. It is certainly reasonable to think that growth outcomes could reverse-cause inflation, budget balance, and components of the Sachs-Warner indicator within a four-year horizon. It is even possible that institutional quality could be affected within a short time horizon by extraordinary growth performance.

Table 7 begins by instrumenting for initial GDP per capita in our 2SLS core regression.[15] Notably, the magnitude of the convergence effect increases (though it is not statistically significant), suggesting that indeed there is some dynamic panel bias present in the earlier results. Instrumented variables are in bold type. The coefficient on short-impact aid is diminished, but to a small and statistically insignificant degree. Column 2 instruments also for institutional quality, inflation, budget balance, and the Sachs-Warner indicator. Hansen's J statistic and Shea's partial R^2 value suggest that the instrument set is, appropriately, exogenous but correlated with the endogenous variables. All variables retain their signs and magnitudes, and the convergence effect becomes significant at the 10% level. The coefficient on short-impact aid is again slightly diminished, but not to a statistically significant degree.

There is evidence to suggest that growth outcomes can be a determinant of civil wars, even in the short term (Collier and Hoeffler 2000), though causality is not firmly established. Our dataset did not permit proper instrumentation of civil war occurrence, so we cannot directly address the possible endogeneity of civil war. We consider it unlikely, however, that correlation between the war variable and the error term greatly biases our estimates of the coefficient on aid. Columns 3 and 6 in Table 7 reveals that excluding civil war from the regression entirely does not materially affect the aid result.

Finally, the Pagan-Hall test rejects the hypothesis of homoskedasticity in the 2SLS results of

[15] Here and in the rest of the table, the instrument set includes those instruments used previously for aid plus once-lagged values of all variables assumed endogenous. This is appropriate, since an Arellano and Bond (1991) test fails to reject the hypothesis of no second-order autocorrelation of errors in all regressions at the 5% level.

columns 1 to 3, suggesting the corresponding GMM estimates of columns 4 to 6 may be more efficient. The conclusions about the coefficient on aid are not substantially affected. Note also the significance of the convergence term.

5.3 Measurement error and aid fungibility

A frequent critique of growth regressions is the possibility that measurement error in the regressor of interest could lead to biased coefficients and misinterpretation of the results. Figure 1 makes clear that our estimation technique introduces a degree of random error into the short-impact aid regressor, and even the OECD database underlying the technique could contain omissions or incorrect values. Notwithstanding all of these concerns, we instrument (well) for short-impact aid. This reduces the likelihood that the coefficient estimate on short-impact aid is attenuated by measurement error.

One type of measurement error of particular salience in the present case is the possible "fungibility" of aid flows. An assumption implicit to this entire exercise is that aid intended to finance certain sectors (e.g., building roads) does so to some degree—i.e. that aid is not fully fungible across sectors. A significant body of literature argues that aid is, in fact, highly fungible.

Internal World Bank discussion recognized some degree of fungibility as early as 1947 (Devarajan et al. 1999). An influential piece by Singer (1965) agreed that the donors engage in some degree of "make-believe" and "self-deception" if they think that aid actually finances that which it is intended to finance. Soon thereafter, Griffin (1970, p. 103) concludes that "foreign capital was financing not the project to which it was apparently tied but ... the least attractive, or marginal, project."

A large body of empirical research since then has sought to measure the exact degree to which aid influences overall government spending (or is passed along to the private sector in tax breaks), the broad composition of spending (consumption versus investment), and the precise sectoral allocation of investment spending. Perfect fungibility along any of these dimensions would certainly make the present analysis impossible; either short-impact aid would have the same impact as aggregate aid, or neither would have any impact on public investment at all. We believe that this is not a problem for our analysis, for two reasons.

First, a reasonable conclusion from this literature—surveyed in Devarajan and Swaroop (1998) and McGillivray and Morrissey (2000)—is that aid is *partially* fungible. In most developing countries most of the time, aid does not go mostly to tax breaks, and aid finances capital and current expenditures in roughly equal amounts. While the intersectoral fungibility of earmarked sectoral aid varies greatly by sector (e.g. Feyzioglu et al. 1996) and by country, the empirical literature does not find aid to be fully fungible. To the extent that earmarked sectoral aid is partially fungible, short-impact aid is merely a noisy measure of recipient governments' true expenditure on short-impact investment. But as long as this measurement error is uncorrelated with growth shocks, the only effect on the estimated short-impact aid coefficient would be an attenuation bias. This would make our estimated coefficient a lower bound on the true coefficient.

Second, and more simply, we find a clear and statistically significant difference between the effect of aggregate gross aid on growth and that of short-impact aid. If aid were perfectly fungible along any of the aforementioned dimensions, this would not be possible.

5.4 Influential observations

Earlier discussion of Figure 1 and columns 5 and 6 of Table 4 reveal that the coefficient estimates for short-impact aid are not driven by influential observations in aid-growth space. On the contrary, eliminating influential observations identified by the Hadi procedure only strengthens the correlation between instrumented aid and growth.

It is nevertheless possible, however, that the result is driven by a few influential observations in a different dimension. The partial scatter of growth against aid is, after all, conditional on all of the values of all other regressors. In theory, a few highly influential observations in, for example, inflation-growth space could affect the partial distribution of aid and growth even if no observations are visibly influential in aid-growth space.

To eliminate this possibility we bootstrap the sample used in our core 2SLS regression result, column 5 of Table 4. We run 1,000 iterations, and each time draw 368 observations with replacement and run the regression. This generates 1,000 estimated coefficient vectors. Since many of the resulting coefficient distributions fail standard tests of normality, Table 8 reports bias-corrected 95% confidence intervals for all coefficients. The lower end of the interval for the linear short-impact aid coefficient, for example, is higher than the lower bound implied by the standard errors in columns 5 and 6 of Table 4. In other words, the exercise detects no subsample of the 368 observations in which the effect of short-impact aid on growth is lower than that present in the full sample.

5.5 Serially correlated errors

While the Arellano-Bond test detects first-order serial correlation of errors in our core specification and many others, we address this in two ways. First, we use clustered errors throughout the analysis to ensure that this does not artificially inflate t-statistics. Second, since serial correlation in the presence of lagged dependent variables can give rise to dynamic panel bias, we instrument for variables not strictly exogenous in Table 7. Our inference on the aid coefficients in both cases is materially unaffected.

6 Interpreting the coefficient estimates

6.1 Separating short-run and long-run effects

Since the different types of aid are serially correlated, we must be prudent in interpreting the magnitudes of these estimates. First, if short-impact aid is serially correlated, then its coefficient could be measuring a mixture of the short-run and long-run impacts of short-impact aid. Second, to the extent that long-impact aid causes growth several periods later, and that lagged long-impact aid is correlated with current short-impact aid, the coefficient on short-impact aid could also be capturing some of the long-run effects of long-impact aid.

To explore this somewhat complex issue, note first that a growth event must follow an aid event if it is to be caused by that aid event, and if growth and aid events are evenly distributed during a four-year period the average lag between an aid disbursement and a subsequent growth event is only sixteen months.[16] Certainly a project funding women's fertility outreach or broad-based reform of the education ministry could not affect economic growth in such a short time.[17]

But it is not that simple. As Roodman (2003, footnote 16) discusses, measurements of aid disbursed in the current four-year period could contain information about previous periods' disbursements to the extent that aid levels are persistent across periods. These earlier disbursements would have longer than sixteen months to affect growth before the measurement. This issue is not specific to aid—the same is true for any regressor that varies over time, including inflation, trade policy, life expectancy, or education levels. Letting ρ be the interperiod correlation between aid levels, the average lag between a growth event in the current period and a prior aid event measured

[16] Since $\left(\int_0^4\int_t^4 dt_g dt_a\right)^{-1}\left(\int_0^4\int_t^4 (t_g - t_a)\, dt_g dt_a\right) = \frac{4}{3}$, where t_g and t_a are the years of growth and aid events.

[17] Naturally aid will have some effect on GDP at the moment it is spent; specifically, GDP increases by the domestic value added contained in domestically-produced goods and services purchased with aid (GDP does not increase at all when aid is spent on imported goods and services). As long as the time between receipt and expenditure of "short-impact" (e.g. for roads) does not differ systematically from that of "long-impact" aid (e.g. for schools) by more than a fiscal year, however, this effect does not differ between the two types of aid.

by an increase in current aid is

$$\frac{4/3 + 4\rho + 8\rho^2 + 12\rho^3 + \cdots}{1 + \rho + \rho^2 + \rho^3 + \cdots} = \frac{4/3 + 4\rho/\left(1-\rho\right)^2}{1/(1-\rho)} = \frac{4}{3}\left(1-\rho\right) - 4\left(1 - 1/\rho\right)^{-1}. \tag{8}$$

When $\rho = 0.65$, the observed interperiod correlation for short-impact aid in our dataset, this average lag is 7.9 years. In other words, a change in short-impact aid measured for the current four-year period precedes by an average of eight years any measured change in growth during the current period. Since the interperiod correlation of aggregate aid is even higher at $\rho = 0.82$, an observed increase in current aggregate aid conveys information about aggregate aid flows that occurred an average of 19 years prior to the observed growth in the current period.

This does not mean, however, that using panels with very short windows of observation is sufficient to fully capture the effects of a policy intervention if only that intervention is highly correlated across periods. The serial correlation has a cost, which is that it becomes more difficult to separate the short-run and long-run impacts of aid, and short-term fluctuations in current growth can obscure the growth impacts of both. To ensure that the coefficient on short-impact aid is accurately capturing the short-run effects of short-impact aid, we must account for persistence in short-impact aid and cross-correlation between short-impact and long-impact.

To see this, suppose for example that the true regression model resembles (5) with two extra terms (abstracting away from the quadratic terms):

$$\dot{y}_{i,t}/y_{i,t} = \alpha + \beta^s d^s_{i,t} + \beta^s_{-1} d^s_{i,t-1} + \beta^l d^l_{i,t} + \beta^l_{-1} d^l_{i,t-1} + \cdots + \varepsilon_{i,t} \tag{9}$$

Suppose also that $\beta^l = 0$, $\rho^s = \text{corr}\left(d^s_{i,t}, d^s_{i,t-1}\right)$, and $\rho^{sl} = \text{corr}\left(d^s_{i,t}, d^l_{i,t-1}\right)$. If we then use short-impact aid as our only aid variable in a regression, the coefficient on short-impact aid will measure not β^s but instead $\left(\beta^s + \rho^s \beta^s_{-1} + \rho^{sl} \beta^l_{-1}\right)$.

This has the virtue of capturing more of the long-run effects of aid, but has the drawback of poorly reflecting recent changes in aid levels. We thus measure with less accuracy a more comprehensive growth effect. If we run regression (9) instead of (5), we measure with more accuracy a less comprehensive growth effect. In that case we ensure that β^s captures more fully the short-run impacts of more recent innovations in the aid level, but we sacrifice measurement of the long-run impacts of either type of aid.

At any rate, we must run some version of (9) in order to properly interpret the coefficient on short-impact aid form our core regression. Column 5 in Table 5 displays a representative result. We include twice-lagged short-impact and twice-lagged long-impact aid on the right-hand side, allowing innovations in short-impact aid an average of nine years and four months to realize their effect (16 months in the current period plus four years in each lagged period). The coefficient on short-impact aid is smaller, though still much larger than that for aggregate aid and still statistically significant.[18] The result indicates that roughly two thirds of the coefficient we measure on short-impact aid in the core regression reflects the short-run (roughly nine year) impact of short-impact aid. The coefficient is not primarily serving as a proxy for the long-run impacts of short-impact aid nor for the long-run impacts of long-impact aid.

Collectively, the results of Table 5 suggest that, out of short-impact aid's marginal growth effect of roughly 0.9, approximately 0.1 actually reflects long-run effects of earlier long-impact aid and approximately 0.2 reflects long-run effects of earlier short-impact aid. Roughly 0.6 represents exclusively the short-run ($\sim$9 year) marginal growth effect of short-impact aid.

6.2 Heterogeneity in the aid effect

Table 9 asks whether the effect of short-impact aid on growth is conditional on an interaction with other included regressors, or is dependent on the time period analyzed. The first 11 columns simply add to the right hand side an interaction term between aid and each of the other regressors, one at a time. The magnitude and significance of the short-impact aid term is unaffected in all but two of these regressions.

Notably, the aid effect does not interact with the level of GDP per capita; we find no evidence that aid works any better or any worse in the poorest countries, all else equal. Nor does the short-impact aid effect interact with the tropics dummy, at variance with the findings of Dalgaard et al. (2004) on aggregate aid.[19] In the final two columns, we also find no statistically significant evidence that the growth effect of aid varies across decades, though the magnitudes of the coefficients and standard errors suggest that there may have been a somewhat smaller effect in the 1980s and a somewhat larger effect in the 1990s.

In columns 4 and 7, however, the coefficient on aid changes greatly in magnitude and significance upon inclusion of interaction term. This suggests that there is in fact a meaningful difference in the way that short-impact aid affects growth between countries with different health conditions

[18] Column 5 loses several observations because the inclusion of twice-lagged variables requires dropping the earliest period for which data are available. Without excluding influential observations identified by the Hadi procedure, the linear coefficient on short-impact aid is 0.636 with a standard error of 0.225.

[19] Dalgaard et al. use fraction of land in the tropics, which correlates closely with a tropics dummy.

(proxied by life expectancy) and different degrees of institutional quality (proxied by the ICRG index). Neither interaction is, however, statistically significant.

A key lesson of Roodman (2003) and Easterly, Levine and Roodman (2004) is that interaction terms of this kind deserve scrutiny. Table 10 takes a more detailed look at five interaction terms of interest, in five pairs of regressions. The first of each pair uses Hadi's procedure to identify influential observations in interaction term-growth space and runs the 2SLS estimator without them. The second of each pair tests this subsample using a GMM estimator.

In columns 1 and 2, we again see no evidence that short-impact aid affects growth differently in the poorest developing countries than it does in richer ones. In columns 3 and 4, in the absence of influential observations on aid × tropics dummy there is a marked change in the aid term upon inclusion of the interaction, though in the more efficient GMM estimate the short-impact aid coefficient is still positive and significant. Its value, 0.605, is not statistically different from the coefficient estimate in column 6 of Table 4. The interaction term, furthermore, negative but is not statistically significant even in the absence of influential observations. We thus see some suggestion that short-impact aid may be less effective in the tropics than elsewhere, in general agreement with the thrust of the findings of Dalgaard et al. (2004) on aggregate aid. We do not find, however, that this heterogeneity in the relationship between short-impact aid and growth is statistically significant nor that it is necessary to detection of the overall aid-growth relationship.

Columns 5 and 6 find that there is a robust interaction between the short-impact aid effect and health conditions in the country, proxied by life expectancy. When influential observations are removed from interaction-growth space in column 4 of Table 9, the result is column 5 of Table 10: the interaction term is large, positive, and significant at the 5% level. A more efficient GMM estimate confirms the relationship in column 6.[20] The magnitude of the interaction coefficient in column 6 implies that coefficient for short-impact aid on growth in the lowest life expectancy country (34.8 years) is $-2.32 + (0.748 \times \ln(34.8)) = 0.334$. Likewise, at the mean life expectancy (59.7 years) the aid coefficient is 0.738 and at the maximum life expectancy (77.5 years) the coefficient is 0.932. These coefficients should not be interpreted literally since the coefficient on the interaction term only captures the slope of the interaction at the mean, not at the extremes of either variable. But they are indicative that 1) short-impact aid is more effective where health conditions are better, and 2) short-impact aid is effective to some degree throughout the entire range of health conditions—which is why the aid effect is easily observed even without an interaction term. It is appropriate to recall

[20] A Pagan-Hall test rejects the hypothesis of homoskedastic errors at the 10% level ($\chi^2_{30} = 41.1$, p-value = 0.0858)

at this point that nearly all aid with a direct impact on health conditions is not included in the "short-impact" aid measure and is instead classified as "long-impact."

Columns 7 through 10 suggest that there may indeed be some heterogeneity in the effect of short-impact aid according to the quality of institutions in the recipient country, though there is no such difference according to the index of inflation, budget, and "openness" policies popularized by Burnside and Dollar. This may help explain why Burnside and Dollar (2004) abandon the earlier policy interaction term from Burnside and Dollar (2000) questioned by Easterly, Levine and Roodman in favor of an interaction with institutional quality, though they continue to refer to the latter as "policies and institutions." In column 7 we see that very little changes when influential observations in interaction-growth space are removed from Table 9 column 7, but the more efficient GMM estimate suggests that the interaction effect may not be entirely spurious.[21] It is not, however, significant at the 5% level, so we cannot be confident in taking the coefficient estimate at face value. Again, our findings on the heterogeneous effect of short-impact aid agree with what Burnside and Dollar (2004) find for aggregate aid: that aid may indeed work better to some degree in countries that effectively battle corruption, guard property rights, and cultivate a respect for law. But—and this is a key difference—we find that when aid is measured in a way commensurate with the very brief period of observation, exploiting this heterogeneity is not at all necessary to detect in macroeconomic data the very large impact of aid on growth. Aid does not work equally well everywhere, but measured correctly it works well even in relatively poor institutional environments.

6.3 How large is the effect?

The *marginal* effect of short-impact aid on growth is much larger than the effect of aggregate aid. In Table 5, for example, we can reject at the 5% level the hypothesis that the coefficient estimate on short-impact aid in column 4 is equal to the coefficient on aggregate gross aid in column 1. Our coefficient estimate on aggregate aid is of an order of magnitude comparable to most estimates of the effect of aggregate aid in the 'unconditional' literature. Hansen and Tarp (2000, 2001), Lensink and White (2001), Gomanee et al. (2002, 2003), Moreira (2003), and Roodman (2003) all find positive, significant coefficients on the linear aid term on the order of 0.1-0.4.[22] Our estimate of the marginal effect of short-impact aid at zero thus exceeds typical estimates of the growth effect of aggregate aid by at least a factor of two or three.

Turning now to the *average* effect of aid, Table 11 examines the contribution of each right-hand

[21] In this case the Pagan-Hall test fails to reject the hypothesis of homoskedastic errors (χ^2_{30} = 36.7, p-value = 0.187).

[22] Exceptionally, Dalgaard et al. (2004) find a coefficient on linear aggregate net aid of 1.35.

side variable (including aid) to the differences in growth rates across regions. The table makes it clear that 1) our model does a good job of explaining interregional growth differences and 2) the contribution of differences in aid levels to differences in growth is substantial. The first column contains the coefficient estimates from our core regression, and the second column contains the implied growth elasticities of each at the regressor mean. The elasticity of growth with respect to aid is high compared to many of the other controls. For example, taking into account the nonlinear impact of aid, variation in aid explains roughly as much variation in growth as does variation in institutional quality or in the three policy variables jointly.

The center section of the table contains the regressor mean values disaggregated by region. The rightmost section of the table then multiplies the regression coefficient by the difference between the regional average regressor value and the world average regressor value. The result is the degree of difference between regional growth and world average growth explained by the fact that the regional average value of that regressor differs from the world average value. The entire column is summed to reach the "average growth difference explained" row, which shows the total difference between regional growth and world average growth explained by regional characteristics included in the model. Immediately below that is the true difference between regional average growth and world average growth. Note that in the case of East Asia, the predicted growth difference is by construction identical to the observed difference since an East Asia dummy variable is included in the model.

The following row reveals that the difference between the two preceding rows is typically small. In all cases, the model explains a large majority of the difference between regional average growth and world average growth. Finally, we note towards the bottom of the table that the contribution of interregional differences in aid levels to interregional growth differences—the result of adding the top two numbers in each row. The estimates indicate that Sub-Saharan Africa and Middle East/North Africa grew substantially more than they otherwise would have because they received more aid than average, and that the remaining regions grew substantially less than they otherwise would have because they received less than average aid.

The coefficient estimates of our core model thus suggest, for example, that while average growth in Sub-Saharan Africa was –0.23%, it would have been 1.08% lower, or –1.31%, had the region received world average short-impact aid flows (2.75% of GDP) instead of the amount it actually received (5.33%). If instead of our core specification coefficient estimates we conservatively use the aid coefficient estimates from Table 5 column 5, which eliminate the longer-term effects of both short-impact and long-impact aid, then growth in Sub-Saharan Africa would have been –0.96% had it received world average short-impact aid levels. Conservatively, then, short-impact aid raised average annual per capita income growth in Sub-Saharan Africa by three quarters of a percentage point or more between 1973 and 2001. That is, while Africa's growth performance has been disappointing, it would have been much worse in the absence of aid. Here, as elsewhere, we take no account whatsoever of any additional impact on long-run growth due to long-impact aid. Three quarters of a percentage point is thus an extremely conservative estimate of the *total* impact of aid on growth in Africa.

These figures make it clear why a well-performing growth model controlling for the confounding growth effects of other forces is essential to detect the effect of aid on growth. As van de Walle (1999) notes, "[S]ince 1980, the donors have collectively spent some US $200 billion in Africa. ... Yet, Africa's economic performance remains mediocre." But this by itself does not mean that aid did not help growth. All else equal, aid did help growth—Africa's growth performance would have been far worse without it.

The coefficient estimate for repayments on aid is also of some interest. In Tables 4 and 7 and elsewhere, repayments have a significant negative effect on growth. This suggests that countries receiving gross aid flows would, all else equal, grow more if they did not have to repay loans. In Sub-Saharan Africa, for example, average repayments are 0.46% of GDP in the sample (whose natural logarithm, shown in the table, is –0.78), above the average of 0.21% for all aid recipients. The right-most column of Table 11 suggests that these repayments cost the average country in Sub-Saharan Africa a third of a percentage point in annual growth during 1973-2001. In other words, income per capita in the region today would be roughly 9% higher if aid flows had been exclusively grants.

6.4 Absorptive Capacity and the Economic returns to aid

The coefficient estimates from our core regression also contain information about average capacity for a recipient country to productively absorb short-impact aid, with implications for the economic returns to aid. The coefficient estimates in column 5 of Table 4 suggest a parabolic or 'inverted-U' effect of short-impact aid, of the type commonly seen for aggregate aid. The global maximum of this average effect occurs when short-impact aid represents 8.1% of GDP, and reaches zero at 16.1% of GDP. This does not mean, of course, that more than 16.1% of GDP in short-impact aid cannot raise growth in a given recipient; some can absorb more, some less. It is worth reinforcing that these figures include only short-impact aid, which averages about 45% of total aid. Thus, the global maximum (short-impact aid of 8.1%) typically occurs when *aggregate* aid is around 18% of GDP and typically reaches zero when aggregate aid reaches around 36% of GDP.

The shape of this parabola has direct implications for the expected economic returns to aid for a given recipient. The average value of short-impact aid in the dataset is 2.75% of GDP, associated with a 2.19% increase in growth relative to zero aid. 3.75% of GDP in short-impact aid causes 2.77% growth, so at the mean an additional 1% of GDP in short-impact aid produces an additional 0.58 percentage points in average annual growth. At face value, this may seem inefficient, as $1 dollar in aid brings an immediate increase in output of $0.58. But of course this is not the whole story, just as it is not the complete story when a private firm invests $1 and gets an increase in output of less than $1 in the first year. We need to explore the increment in output over time associated with an additional $1 in aid. To do so, let ϕ represent the sum of the rate of time preference and the depreciation rate, and let g be the growth effect of a one-period increase in short-impact aid by 1% of GDP. At the mean, the net present value of the growth effect is $\sum_{i=1}^{\infty}\left((1+g)^i-1\right)(1-\phi)^i = \frac{1}{1-(1+g)(1-\phi)} - \frac{1}{\phi}$. When g = 0.58%, the internal rate of return to short-impact aid (the value of ϕ at which the benefit stream equals exactly 1% of GDP in present value) is thus 53%. Thus, short-impact aid causes an expansion of GDP larger than the aid flow itself so long as the sum of the discount rate and the depreciation rate is less than 53%—a very high threshold. For example, even if projects funded by short-impact aid depreciate at 25% a year, it would still require an annual pure time preference in excess of 28% for short-impact aid given to the mean country not to pay off. If we repeat this calculation conservatively using the coefficient estimates from column 5 of Table 7, the internal rate of return is 47%. Looking at it from another perspective, if the sum of the discount rate and the depreciation rate is a conservatively high 30%, at the mean a $1 dollar inflow of short-impact aid leads to a cumulative increase in economic output (and income) of roughly $8 over time.

7 Links to the recent empirical literature

The next two tables explore the relationship between our core findings and two of the most prominent recent aid-growth papers. In brief, we find that there is no "macro-micro paradox." When aid is properly defined as aid that is capable of producing a growth effect within the period of observation, aid's effect on growth is easily detectable in cross-country analysis. This finding questions the premise upon which the "conditional" literature was built—that macroeconomic analysis could only reliably detect a growth impact of aid in countries with certain characteristics, but not in the full sample of LDCs. It thus supports the thrust of the "unconditional" strand of the literature, but extends that literature by finding a much larger effect of aid on growth when aid relevant to the observation period is used.

Table 12 demonstrates this reasoning by dissecting the difference between our results and those of Burnside and Dollar (2000), the flagship of the "conditional" strand of the recent literature. We begin by reproducing their core result (column 1): aid interacted with the policy measure is positive and significant, but aid alone is not (even when the interaction term is not included). We then expand their dataset to that of Roodman (2003) in column 2, which extends to 2001 and includes Eastern Europe, and add a squared aid term (col. 3). As in Roodman (2003) and Easterly, Levine and Roodman (2004), with this larger sample the Burnside an Dollar results disappear, and both aid and the interaction term are insignificant.

Burnside and Dollar use a measure of aid that is relatively rare in the literature: "Effective" Development Assistance (EDA) divided by GDP measured at purchasing power parity (PPP). The numerator is a measure of the net present value of aid flows taken from Chang et al. (1999), and the denominator from the Penn World Table. Both of these choices are subject to question. While official development assistance (ODA, the standard definition of aid as provided by the OECD) captures only current aid flows, EDA captures the net present value of future flows. But is the fact that a road is built with a loan that must be repaid decades into the future relevant to the capacity of that road to produce growth within a four-year period? In a Ricardian world of perfect foresight and perfect credit markets, yes; but such a model is simply irrelevant to how most LDC consumers using the road make decisions. We strongly prefer, along with the large majority of aid-growth studies, to measure aid as ODA. Indeed, in their most recent paper, Burnside and Dollar (2004) themselves switch from EDA to ODA, presumably for these very reasons. Switching from EDA to short-impact gross ODA (col. 4) causes the coefficient on aid to swing from negative to positive and large, though still statistically insignificant.

The Burnside and Dollar denominator of PPP GDP, also rare in the aid-growth literature, is likewise open to question. It can be justified theoretically under the assumption that most aid is spent on goods and services with nontradable local substitutes. Suppose for example that an aid project in Ethiopia purchases a bulldozer that does the work of fifty local laborers. This allows those laborers to do something else, and the gain to the economy is proportional to the value of the bulldozer's services in local terms—that is, at PPP. If on the other hand aid is spent on tradable items with no locally-available substitute—a vaccine, for example—then the gain to the recipient economy is simply that of not having to purchase those items for itself on the international market. This would have to be done by first purchasing dollars, meaning that the value of the aid relative to the whole economy must be computed using GDP at exchange rates. It is not immediately obvious which of these is more fitting for most aid projects, and at the very least we must test the sensitivity of any result to different choices of denominator. Table 12 reveals that Burnside and Dollar's choice was not innocuous. When we use GDP at exchange rates instead of the PPP GDP denominator in column 5, the aid coefficient is statistically significant.

We note two characteristics of the Burnside and Dollar specification: 1) instrumentation for aid and aid squared is frequently weak, with a particularly low Shea (1997) first-stage R^2 in column 5, and 2) growth is not well-modeled, in that most of their non-aid regressors have statistically insignificant growth effects. In column 6 and thereafter we return to our preferred growth model from Table 4. Columns 6 shows that, with growth better modeled by relevant covariates, even the coefficient on EDA is positive (though not significant). When aid is instead measured as net ODA (col. 7) as a fraction of exchange-rate GDP (col. 8) or gross ODA (col. 9), its growth effect is positive and significant. Finally, when we switch to short-impact aid, the effect is much larger (col. 10) and only increases when we use PPP GDP as our denominator (col. 11). The final column removes four outliers to correspond to column 5 of Table 4.

In short, Burnside and Dollar's finding that aid does not affect growth in the absence of a policy interaction term vanishes when aid relevant to a four-year observation horizon, short-impact aid, is considered—even using their weak specification of the growth model (col. 5). With a stronger specification (col. 10 and 11) this result is not sensitive to their questionable choice of denominator.

Table 13 turns to the results of Hansen and Tarp (2000), perhaps the taproot of the "unconditional" strand of the recent aid-growth literature. We begin by reproducing their core result, that aggregate net ODA has a positive, significant, nonlinear impact on growth in the absence of any interaction term (col. 1). We then expand the sample to Roodman's (2003) dataset (col. 2) and use clustered standard errors to correct for first-order autocorrelated errors we detect (col. 3). We then switch to gross short-impact aid while retaining the rest of their specification (col. 4), and the coefficient on aid increases greatly. The same thing happens (col. 5 and 6) when we employ our preferred specification from Table 4.

Hansen and Tarp's result, then, is correct in spirit; it demonstrates that when properly-measured aid is allowed a nonlinear effect on growth, the "micro-macro paradox" disappears. But they understate the growth impact of aid by considering *aggregate* ODA with an observation period of only four years. The growth effect of that portion of aid capable of affecting growth in so short a time is much larger.

8 Broad policy implications

The chronic aid-growth debate has always been much more than a scholarly discussion. Its various results frequently have been interpreted as having direct implications for aid policy, even by those that did not conduct empirical research on the topic. For example, Milton Friedman (1958) draws upon anecdotes of waste and lack of growth impact from aid to argue that American foreign aid should be immediately and permanently canceled. Similarly, based largely on the evidence of Mosley (1980) and Mosley et al. (1987),[23] Keith Griffin (1991) argues that the economic benefits of aid have been "at best negligible" and thus foreign aid for the purpose of promoting economic development "urgently needs to be reconsidered."

Péter Bauer regularly made qualitative, anecdotal arguments in favor of the elimination of foreign aid. Bauer (1966, p. 32) argues that aid "has not served to bring about an appreciable rise in living standards in under-developed countries." Aid requires a "drastic readjustment" and must be allocated "much more selectively both politically and geographically." Even if it were more selective, however, Bauer (1971, p. 135) adds that aid "is unlikely to be a major instrument ... for the material progress of poor countries." Aid often reinforces the centralized power of corrupt or inept regimes, he claims, in addition to financing more consumption than investment, crowding out private investment, and distracting attention from more fundamental determinants of growth. Decades later, Bauer (1991, p. 53) remains convinced that "external donations can do little or nothing for development," though he refers to none of the empirical economics literature on the subject. Bauer (2000, p. 51) at last articulates the logical policy conclusion of his lifelong argument: that "official government-to-government subsidies ought to be terminated or at least drastically curtailed."

By the late 1990s, van de Walle (1999) reports that "policy elites ... are more skeptical about aid than ever before" in part because of "the surprisingly strong consensus that most aid in the past has been ineffective." In fact, "[a]id to Africa is less threatened by the current foreign policy and fiscal pressures than by a complex crisis of legitimacy regarding the enterprise. Despite official rhetoric to the contrary, most aid insiders are disappointed by the current achievements of aid."

The World Bank has counterattacked over the last six years, asserting that aid can work when done right. "The evidence on the effectiveness of aid has at times been ambiguous—but this is because early research failed to distinguish between different types of aid and recipients" (World

[23] Regarding the effect of aid on growth, Griffin also cites the 3SLS regressions of Gupta and Islam (1983), which reject the hypothesis that the growth effect of aid was zero or negative in the 1960s and 1970s. He writes off this evidence as meaning only that "many outcomes are possible" and highlights the authors' conclusion that aid does not offer "the solution" for rapid growth. This is, of course, very different from stating that aid does not affect growth. Griffin also cites work on aid's effect of on non-growth outcomes.

Bank 2002, p. 93). Studies launched primarily by investigators associated with the Bank have had enormous policy influence (as reviewed earlier in this paper and in Roodman 2003). In both academic and policy circles it is today often taken on faith that aid works best in countries with good policies and strong institutions, with the implication that more aid should be allocated to these countries. In the strongest version of this argument, some believe that aid *only* works in countries with strong policies and institutions, and should be significantly curtailed or halted in countries with weak institutions.

The key point is that the World Bank's response to the critics of aid explained the "ambiguous" relationship between aid and growth primarily by heterogeneity among *aid recipients*. Our results suggest that heterogeneity of recipients plays a role, but is *not* the primary reason why growth effects of aid have been difficult to detect. Instead, we find that the heterogeneity in *aid flows* is the key reason for the mixed earlier results. Matching aid flows to the observation period over which they might reasonably affect growth makes the relationship easy to detect. Many of the policy implications taken from 'conditional' literature like the work of Burnside and Dollar (2000, 2004) have thus been based on incomplete information. The *typical* experience of developing countries is that the types of aid that should be expected to cause growth over a four-year period do, in fact, cause growth. This changes the policy conclusions: aid does not need to be restricted to a handful of recipients with the strongest institutions in order for the aid portfolio as a whole to have positive returns. The evidence presented here does suggest that short-impact aid may have a slightly larger impact on growth in countries with strong institutions, but it has a large and positive impact on growth even in countries with weak institutions.

This result does not mean that aid is always beneficial everywhere. Bauer was right that aid in the wrong circumstances can help centralize the power of autocratic despots, as it did for Joseph Mobutu and Ferdinand Marcos. In addition, aid is subject to diminishing returns, so ever-larger increments of aid can lead to weaker (and possibly negative) returns because of absorptive capacity issues in some countries. Moreover, our results show that aid does not work equally in all countries to produce growth: some countries have a stronger aid-growth relationship, while others have a weaker one, as Tables 9 and 10 confirm and the scatterplots of Figure 2 illustrate. The big story, however, is that donors' collective, overall aid portfolio has had positive returns. Although some aid certainly has been wasted and aid quality could be much improved (including through a reallocation of aid to certain countries where it is likely to be more productive), overall positive returns to the aid portfolio do not depend upon dropping large numbers of recipients from the rolls.

9 Conclusions

Most research on foreign aid and economic growth has mistakenly tried to find the relationship between total aid flows and growth over a relatively short period—typically four years—when significant portions of aid are not designed to have this kind of impact. All foreign aid is not the same. By focusing on aid flows that might reasonably be expected to stimulate growth in four years – such as balance of payments support, investments in infrastructure, and support for productive sectors such as agriculture and industry—we find a very strong, positive, and robust relationship in which "short-impact" aid causes growth. The result is robust over a wide variety of specifications. It holds (with only modest changes in the estimated coefficients) when we control for no other right-hand side variables or when we control for any or all of ten or more commonly-used right hand side variables that might affect growth. It holds over various time periods, stands up whether we include or exclude influential observations, and remains robust when controlling for the possible endogeneity of several independent variables.

In our core result (shown in Table 4, column 5), the impact of short-impact aid on growth is substantial. At the mean (where short-impact aid is about 2.75% of GDP), we find that an additional one percentage point of GDP in short-impact aid produces an additional 0.58 percentage points of annual growth over the four year period. Since the increment in GDP will be maintained to some degree over time, with plausible assumptions we find a high payoff for short-impact aid, with an internal rate of return at the mean of around 53%. As one example, even if the sum of the discount rate and the depreciation rate is as high as 30%, then at the mean a $1 increase in short-impact aid raises output (and income) by a cumulative $8 over time. From a different perspective, we find that higher-than-average short-impact aid to sub-Saharan Africa raised per capita growth rates there by about one percentage point over the growth that would have been achieved by average aid flows.

We find a non-linear relationship between short-impact aid and growth in which the impact of aid on growth is subject to diminishing returns. This suggests some limits on the ability of typical recipient countries to absorb very large amounts of short-impact aid. However, the turning point is well above the amount of aid that most countries receive. Whereas the average country receives short-impact aid flows of about 2.75% of GDP, our estimates indicate that the maximum growth effect occurs when short-impact aid represents 8.1% of GDP, and declines thereafter. Note that since short-impact aid represents about 45% of total aid on average, the 8.1% turning point for short-impact aid occurs when total aid reaches around 18% of GDP in the typical country. This does not mean that in any particular country, short-impact aid flows greater than 8.1% of GDP are a bad idea. Instead, this represents the typical pattern over the last 30 years. Absorptive capacity can change over time, and some countries undoubtedly can absorb more aid flows than others.

We find strong evidence of a fairly robust relationship between other variables and economic growth in low-income countries. All else being equal, stronger institutions, more open trade, lower inflation, and lower budget deficits are associated with faster economic growth. In addition, countries that are located outside of the tropics, that have higher life expectancies, and are not experiencing civil conflict also have achieved faster growth. We also find a negative and statistically significant relationship between debt repayments and growth—controlling for other variables, the larger a country's debt service payments, the slower its economic growth.

Our finding that "short-impact" aid has a strong and statistically significant relationship with growth does not mean that this category of aid "works" everywhere and all the time. As with any statistical relationship, there are points above the line (where a given amount of aid is associated with even higher than average growth) and points below the line (where abundant aid has not been associated with growth). Rather, our results indicate that for the average (and above average) country short-impact aid clearly has stimulated growth, but the relationship does not hold in all countries. We further explore this heterogeneity in the relationship by examining whether the aid-growth relationship is stronger or weaker in countries with particular characteristics—such as those with better policies, and stronger institutions. We find modest evidence that the aid-growth relationship has greater force in countries with stronger institutions as well as for those with higher life expectancy (i.e., better health). Thus, short-impact aid does seem to be somewhat more powerful in countries with healthier populations and strong institutions. However, unlike some previous research we do not find that a positive aid-growth relationship depends on strong institutions. Rather, we find a substantial positive relationship even in countries with weak institutions, and a slightly more powerful one in countries with more capable institutions.

The short-impact aid-growth relationship does not vary across countries to a statistically significant degree according to other characteristics like their different policies, levels of income, or other attributes. As mentioned we find strong evidence that policies (trade, inflation, budget) matter for growth, and play an important role in distinguishing rapidly growing from slowly growing economies. But we find that the distinction between good and poor policy is not necessary to find a positive relationship between "short-impact" aid and growth.

Emphatically our results also do not mean that social-sector investments (or "long-impact" aid) or humanitarian aid have no long-run growth effect. We consciously, deliberately ignore those effects—whatever they may be—in order to more accurately measure the short-run growth effects of short-impact aid alone. We note that the results in Table 5 do suggest an important long-run positive impact on growth from long-impact aid, but this study does not intend or attempt to quantify it. We leave that to future research. Here, we quantify the growth impact of only one portion of aggregate aid flows. Any growth effects accruing in the long run from social sector investments must be considered over and above the growth effects we quantify in this study.

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Table 1. Overview of aid classification
(See Appendix Table 1 for a more detailed disaggregation)

CRS three digit and sample 5 digit codes	Commitments, 1997-01 in $	In percent of total	Short-impact (In percent of category total)	Long-impact	Humanitarian
110 Education	14,939	6.6	3.9	96.1	0.0
120 Health	10,427	4.6	3.9	96.1	0.0
130 Population Policies/Programmes and Reproductive Health	4,927	2.2	3.0	97.0	0.0
140 Water Supply and Sanitation	15,364	6.8	2.3	97.7	0.0
150 Government and Civil Society	15,887	7.0	13.0	87.0	0.0
160 Other Social Infrastructure and Services	11,530	5.1	16.0	84.0	0.0
210 Transport and Storage	27,423	12.1	91.0	9.0	0.0
220 Communications	1,780	0.8	65.8	34.2	0.0
22000 Unclassified	0	0.0	100.0	0.0	0.0
22010 Communications policy and administrative management	165	0.1	0.2	99.8	0.0
22020 Telecommunications	1,196	0.5	97.6	2.4	0.0
22030 Radio/television/print media	418	0.2	0.5	99.5	0.0
230 Energy Generation and Supply	15,230	6.7	78.1	21.9	0.0
240 Banking and Financial Services	3,534	1.6	59.0	41.0	0.0
250 Business and Other Services	5,339	2.4	62.9	37.1	0.0
311 Agriculture, Forestry, Fishing	16,952	7.5	66.1	33.9	0.0
321 Industry, Mineral Resources and Mining and Construction	4,186	1.8	78.0	22.0	0.0
331 Trade and tourism	888	0.4	33.0	67.0	0.0
400 Multisector/Cross-Cutting	39	0.0	0.0	100.0	0.0
410 General Environmental Protection	5,308	2.3	10.6	89.4	0.0
420 Women In Development	258	0.1	14.2	85.8	0.0
430 Other Multisector	12,447	5.5	7.2	92.8	0.0
510 Structural Adjustment Assistance With World Bank/IMF	6,145	2.7	100.0	0.0	0.0
520 Developmental Food Aid/Food Security Assistance	7,422	3.3	0.0	0.0	100.0
530 Other General Programme and Commodity Assistance	12,717	5.6	100.0	0.0	0.0
600 Action Relating to Debt	17,369	7.7	100.0	0.0	0.0
700 Emergency Assistance	392	0.2	0.0	0.0	100.0
710 Emergency Food Aid	1,819	0.8	0.0	0.0	100.0
720 Other Emergency and Distress Relief	9,590	4.2	0.0	0.0	100.0
910 Administrative Costs of Donors	256	0.1	0.0	0.0	0.0
920 Support to Non-Governmental Organisations	85	0.0	0.0	100.0	0.0
998 Unallocated/ Unspecified	2,520	1.1	1.2	98.5	0.0
Total in USD	226,550		102,523	104,546	19,217
(percent of total)	100.0		45.3	46.1	8.5

Note: Aid is classified into short-impact, long-impact and humanitarian categories using CRS purpose codes and "purpose prefixes". The former are summarized at the three-digit level, above and are detailed at the full, five-digit level in the appendix.

Table 2. Summary statistics for aid variables
(In percent of GDP)

Variable	Obs	Mean	Std. Dev.	Min	Max
Data points for which we have actual disaggregated disbursements (1990 onwards)					
Net ODA	694	6.18	8.27	-0.45	58.60
Gross ODA	694	7.19	9.18	0.02	74.97
Estimated short-impact, definition 1	694	3.39	4.78	0.00	35.49
Estimated long-impact, definition 1	694	3.39	4.91	0.00	52.13
Estimated humanitarian, definition 1	694	0.41	1.09	0.00	17.61
Actual short-impact, definition 1	694	3.91	6.11	0.00	58.11
Actual long-impact, definition 1	694	2.72	4.27	0.00	34.12
Actual humanitarian, definition 1	694	0.53	1.64	0.00	15.89
Estimated short-impact, definition 2	694	3.37	4.76	0.00	35.18
Estimated long-impact, definition 2	694	3.41	4.89	0.00	51.90
Estimated humanitarian, definition 2	694	0.41	1.08	0.00	17.26
All data					
Net ODA	3,526	8.79	13.50	-0.55	241.78
Gross ODA	3,528	9.40	13.91	0.00	241.78
Estimated short-impact, definition 1	3,528	4.75	9.06	0.00	232.51
Estimated long-impact, definition 1	3,528	3.79	6.31	0.00	70.51
Estimated humanitarian, definition 1	3,528	0.86	3.40	0.00	92.20
Estimated short-impact, definition 2	3,528	4.69	8.98	0.00	232.51
Estimated long-impact, definition 2	3,528	3.86	6.39	0.00	70.72
Estimated humanitarian, definition 2	3,528	0.86	3.39	0.00	92.20

Note: Definition 1 estimates of aid are constructed by classifying real sector flows as short-impact aid and social sector flows as long-impact aid. These estimates are compared to actual 1990s data compiled on a definition 1 basis. Definition 2 estimates of aid modify definition 1 so that budget support is classified as short-impact aid and technical cooperation is classified as long-impact aid, regardless of sector. Definition 2 is our preferred estimate and is used in the regression analysis. For details see text.

Table 3. Simple correlations between estimates and actual values of of aid using data from the 1990s

	Net ODA	Gross ODA	Estimated short-impact, definition 1	Estimated long-impact, definition 1	Estimated humanitarian, definition 1	Actual short-impact, definition 1	Actual long-impact, definition 1	Actual humanitarian, definition 1	Estimated short-impact, definition 2	Estimated long-impact, definition 2	Estimated humanitarian, definition 2
Net ODA	1.000										
Gross ODA	0.987	1.000									
Estimated short-impact, definition 1	0.883	0.894	1.000								
Estimated long-impact, definition 1	0.880	0.896	0.622	1.000							
Estimated humanitarian, definition 1	0.468	0.464	0.337	0.314	1.000						
Actual short-impact, definition 1	0.868	0.888	0.852	0.755	0.333	1.000					
Actual long-impact, definition 1	0.753	0.750	0.581	0.778	0.261	0.408	1.000				
Actual humanitarian, definition 1	0.310	0.322	0.306	0.159	0.647	0.170	0.059	1.000			
Estimated short-impact, definition 2	0.885	0.897	0.996	0.632	0.333	0.851	0.593	0.298	1.000		
Estimated long-impact, definition 2	0.886	0.900	0.632	0.996	0.322	0.763	0.772	0.170	0.635	1.000	
Estimated humanitarian, definition 2	0.469	0.465	0.338	0.316	1.000	0.334	0.262	0.648	0.335	0.323	1.000

Note: All correlations, except actual humanitarian versus actual long, are significant at the 1% level.

Figure 1. Comparing actual to estimated disaggregated aid in the 1990s

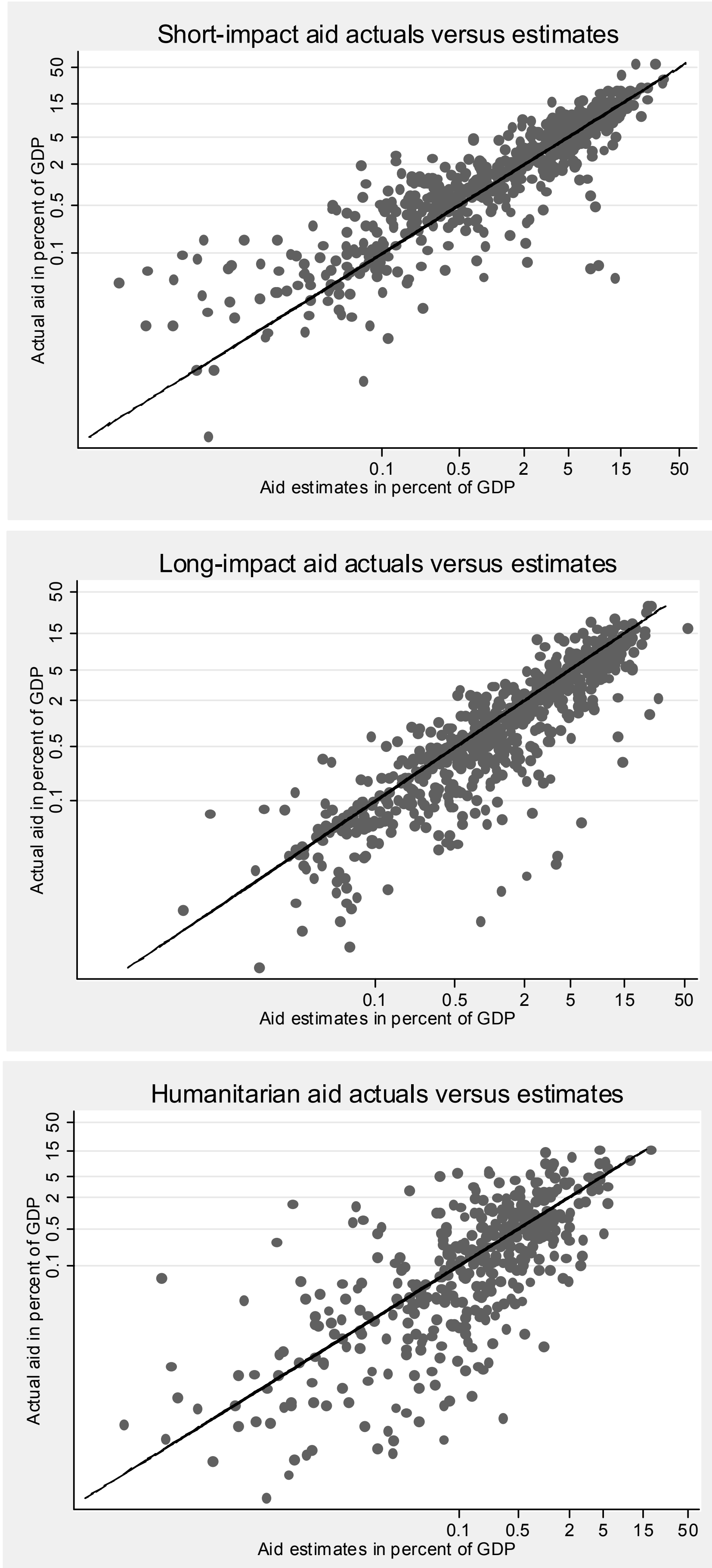
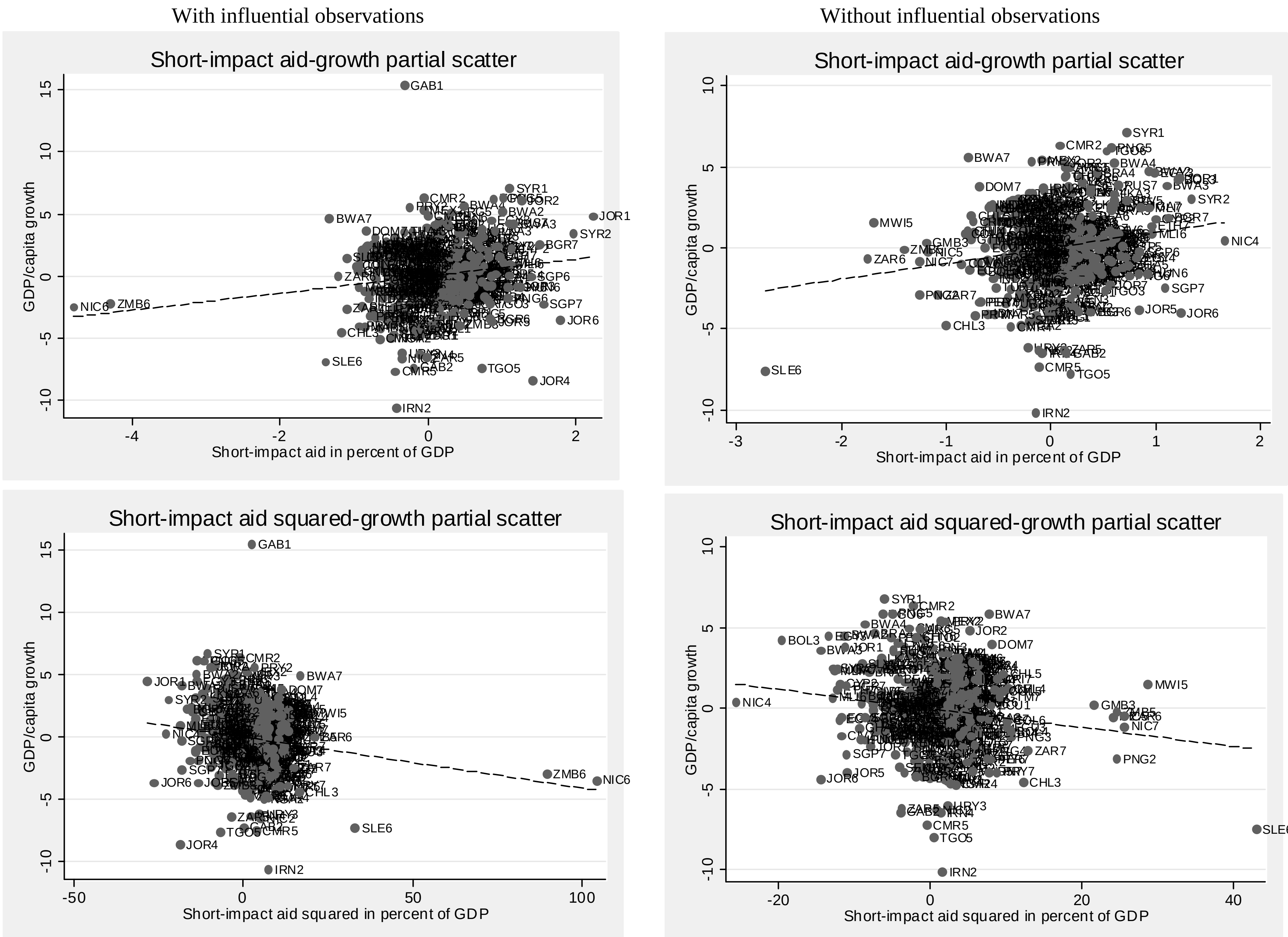


Table 4. Core results

	1 2SLS	2 2SLS	3 2SLS	4 2SLS	5 2SLS Core	6 GMM
Net ODA	0.212					
	(0.138)					
Net ODA squared	-0.00763					
	(0.00457)*					
Short-impact aid		0.620	0.522	0.684	0.960	0.930
		(0.241)**	(0.231)**	(0.195)***	(0.328)***	(0.251)***
Short-impact aid squared		-0.0408	-0.0330	-0.0408	-0.0588	-0.0507
		(0.0159)**	(0.0141)**	(0.0120)***	(0.0264)**	(0.0190)***
Log repayments			-0.161	-0.204	-0.384	-0.508
			(0.192)	(0.180)	(0.188)**	(0.159)***
Log initial GDP per capita	1.55	1.78	0.753	0.217	-0.0593	-0.253
	(0.522)***	(0.501)***	(0.378)**	(0.446)	(0.493)	(0.439)
East Asia			1.95	2.35	2.39	2.62
			(0.795)**	(0.639)***	(0.648)***	(0.601)***
Institutional quality			0.368	0.315	0.333	0.323
			(0.135)***	(0.105)***	(0.114)***	(0.106)***
Inflation			-2.01	-1.71	-1.60	-1.30
			(0.452)***	(0.513)***	(0.558)***	(0.403)***
Budget balance			3.28	6.42	8.28	6.22
			(6.29)	(6.18)	(5.47)	(4.24)
Openness Sachs-Warner			1.18	1.17	1.41	1.47
			(0.518)**	(0.438)***	(0.456)***	(0.388)***
Tropics				-1.86	-2.13	-2.28
				(0.393)***	(0.398)***	(0.290)***
Log initial life expectancy				2.24	3.49	4.06
				(1.75)	(1.85)*	(1.54)***
Civil war				-2.39	-2.19	-1.82
				(0.888)***	(0.891)**	(0.813)**
Lagged civil war				2.03	1.86	1.56
				(0.703)***	(0.730)**	(0.644)**
Observations	372	372	373	372	368	368
R-squared	0.117	0.105	0.296	0.375	0.388	0.383
Hansen *J* stat of over-identifying restrictions (*p*-value)	0.008	0.011	0.332	0.450	0.773	0.773
Shea partial *R*-squared for aid	0.607	0.549	0.493	0.491	0.314	0.314
Shea partial *R*-squared for aid squared	0.593	0.509	0.468	0.471	0.257	0.257
Shea partial *R*-squared for log repayments			0.587	0.576	0.537	0.537
Influential observations removed using Hadi procedure	N	N	N	N	Y	Y

Note: Robust and clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include period dummies and a constant term. Aid, aid squared and repayments are instrumented. For all regressions, *F*-tests for the hypothesis that the instruments are not relevant are rejected at the 1% level. For details see text.

Figure 2. Partial scatters for core regressions



Note: Each country is denoted by its 3-letter ISO country code and a period code, where period 1 is 1974-77, and so on in 4-year periods.

Table 5. A more detailed disaggregation

2SLS	1	2	3	4	5
Gross aid	0.232				
	(0.091)**				
Gross aid squared	-0.00522				
	(0.00275)*				
Short-impact aid		0.793	0.801	0.960	0.583
		(0.413)*	(0.291)***	(0.328)***	(0.241)**
Short-impact aid squared		-0.0556	-0.0503	-0.0588	-0.0326
		(0.0285)*	(0.0219)**	(0.0264)**	(0.0144)**
Long-impact aid		0.146			
		(0.266)			
Long-impact aid squared		0.0000762			
		(0.012)			
Humanitarian aid		-0.407	-0.0412		
		(1.29)	(1.16)		
Humanitarian aid squared		0.146	0.0973		
		(0.317)	(0.282)		
Twice lagged short-impact aid					0.172
					(0.206)
Twice lagged short-impact aid squared					-0.0184
					(0.0135)
Twice lagged long-impact aid					0.0429
					(0.171)
Twice lagged long-impact aid squared					0.00253
					(0.00645)
Log repayments	-0.256	-0.404	-0.309	-0.384	-0.579
	(0.166)	(0.189)**	(0.178)*	(0.188)**	(0.214)***
Log initial GDP per capita	-0.308	0.0223	-0.146	-0.0593	-0.719
	(0.419)	(0.496)	(0.479)	(0.493)	(0.504)
East Asia	2.23	2.33	2.31	2.39	2.38
	(0.616)***	(0.635)***	(0.637)***	(0.648)***	(0.643)***
Tropics	-2.05	-2.23	-2.00	-2.13	-2.18
	(0.359)***	(0.423)***	(0.419)***	(0.398)***	(0.418)***
Log initial life expectancy	3.76	3.12	4.06	3.49	3.41
	(1.67)**	(2.12)	(1.96)**	(1.85)*	(1.82)*
Civil war	-1.81	-1.75	-2.29	-2.19	-1.23
	(0.727)**	(0.767)**	(0.882)***	(0.891)**	(0.757)
Lagged civil war	1.66	1.27	1.65	1.86	1.36
	(0.495)***	(0.617)**	(0.714)**	(0.730)**	(0.560)**
Institutional quality	0.331	0.299	0.331	0.333	0.525
	(0.104)***	(0.121)**	(0.121)***	(0.114)***	(0.118)***
Inflation	-1.92	-1.83	-1.67	-1.60	-1.70
	(0.539)***	(0.530)***	(0.538)***	(0.558)***	(0.555)***
Budget balance	7.97	6.90	8.74	8.28	6.54
	(5.36)	(5.18)	(5.34)	(5.47)	(5.41)
Openness Sachs-Warner	1.32	1.32	1.26	1.41	1.51
	(0.423)***	(0.460)***	(0.462)***	(0.456)***	(0.440)***
Observations	367	366	368	368	320
R-squared	0.441	0.398	0.399	0.388	0.402
Hansen J stat of over-identifying restrictions (p-value)	0.307	0.067	0.486	0.773	0.753
Shea partial R-squared for gross aid	0.598				
Shea partial R-squared for short aid		0.252	0.323	0.314	0.431
Shea partial R-squared for long aid		0.370			
Shea partial R-squared for humanitarian aid		0.220	0.204		
Shea partial R-squared for log repayments	0.585	0.532	0.536	0.537	0.539

Note: Robust and clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include period dummies and a constant term. Aid, aid squared and repayments are instrumented. For all regressions, F-tests for the hypothesis that the instruments are not relevant are rejected at the 1% level. Influential observations in aid-growth space removed using Hadi (1992) procedure. For details see text.

Table 6. Robustness tests, changing specification

2SLS	1	2	3	4	5	6
Short-impact aid	1.05	0.958	0.952	0.964	0.841	1.01
	(0.329)***	(0.328)***	(0.328)***	(0.331)***	(0.354)**	(0.320)***
Short-impact aid squared	-0.0664	-0.0583	-0.0583	-0.0587	-0.0489	-0.0602
	(0.0276)**	(0.0268)**	(0.0262)**	(0.0267)**	(0.0274)*	(0.0258)**
Repayments	-2.21					
	(1.15)*					
Repayments squared	0.407					
	(0.247)*					
Log repayments		-0.171	-0.340	-0.373	-0.393	-0.283
		(0.177)	(0.193)*	(0.191)*	(0.235)*	(0.181)
Log initial GDP per capita	0.0143	0.293	-0.133	0.00720	-0.0284	0.802
	(0.476)	(0.448)	(0.492)	(0.492)	(0.496)	(0.554)
East Asia	2.28	2.23	2.29	2.41	2.72	1.79
	(0.603)***	(0.452)***	(0.679)***	(0.648)***	(0.673)***	(0.563)***
Tropics	-2.08	-2.10	-2.00	-2.04	-2.18	-1.96
	(0.384)***	(0.318)***	(0.420)***	(0.460)***	(0.399)***	(0.353)***
Log initial life expectancy	2.60	2.88	2.90	3.33	4.19	2.58
	(2.02)	(1.75)*	(1.74)*	(1.72)*	(1.65)**	(1.79)
Civil war	-1.84	-1.69	-2.25	-2.18	-1.91	-1.73
	(0.840)**	(0.687)**	(0.876)**	(0.887)**	(0.724)***	(0.696)**
Lagged civil war	1.36	1.73	1.88	1.84	1.67	1.37
	(0.707)*	(0.655)***	(0.730)**	(0.727)**	(0.604)***	(0.626)**
Institutional quality	0.351	0.270	0.331	0.339	0.296	0.261
	(0.111)***	(0.104)***	(0.114)***	(0.113)***	(0.114)***	(0.098)***
Inflation	-1.61	-1.99	-1.63	-1.60	-1.66	-1.46
	(0.580)***	(0.459)***	(0.567)***	(0.551)***	(0.513)***	(0.493)***
Budget balance	8.31	8.45	8.90	8.53	5.40	8.64
	(5.22)	(5.29)	(5.52)	(5.60)	(5.57)	(5.34)
Openness Sachs-Warner	1.43	1.65	1.32	1.39	1.46	1.58
	(0.471)***	(0.399)***	(0.449)***	(0.465)***	(0.498)***	(0.396)***
Land area		0.400				
		(0.073)***				
Coastline		-4.20				
		(19.5)				
Secondary school enrollment ratio			0.0129			
			(0.0123)			
Natural resource abundance				-0.0082		
				(0.0173)		
Positive commodity shock					1.75	
					(0.793)**	
Negative commodity shock					-2.78	
					(1.25)**	
Natural disaster						0.479
						(0.483)
Lagged natural disaster						1.29
						(0.606)**
Observations	367	367	368	368	309	367
R-squared	0.393	0.425	0.392	0.389	0.456	0.414
Hansen *J* stat of over-identifying restrictions (*p*-value)	0.745	0.955	0.744	0.780	0.958	0.688

Note: Robust and clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include period dummies and a constant term. Aid, aid squared and repayments are instrumented. For all regressions, *F*-tests for the hypothesis that the instruments are not relevant are rejected at the 1% level. Influential observations in aid-growth space removed using Hadi (1992) procedure. For details see text.

Table 7. Robustness tests, testing for endogeneity of variables

	1	2	3	4	5	6
	2SLS	2SLS	2SLS	GMM	GMM	GMM
		(Boldface coefficients correspond to instrumented variables)				
Short-impact aid	**0.880**	**0.702**	**0.798**	**0.804**	**0.648**	**0.720**
	(0.313)***	**(0.324)****	**(0.317)****	**(0.246)*****	**(0.256)****	**(0.241)*****
Short-impact aid squared	**-0.0532**	**-0.0401**	**-0.0415**	**-0.0344**	**-0.0479**	**-0.0358**
	(0.0253)**	**(0.0192)****	**(0.0258)**	**(0.0199)***	**(0.0261)***	**(0.0190)***
Log repayments	**-0.518**	**-0.490**	**-0.506**	**-0.630**	**-0.551**	**-0.590**
	(0.199)***	**(0.192)****	**(0.193)*****	**(0.165)*****	**(0.170)*****	**(0.168)*****
Log initial GDP per capita	**-0.826**	**-0.985**	**-0.810**	**-1.04**	**-1.09**	**-1.00**
	(0.526)	**(0.524)***	**(0.485)***	**(0.444)****	**(0.447)****	**(0.408)****
East Asia	2.26	2.11	2.15	2.42	2.34	2.37
	(0.621)***	(0.620)***	(0.629)***	(0.588)***	(0.597)***	(0.595)***
Tropics	-2.14	-2.07	-2.32	-2.38	-2.35	-2.54
	(0.390)***	(0.407)***	(0.372)***	(0.301)***	(0.338)***	(0.297)***
Log initial life expectancy	5.26	5.07	4.62	6.16	5.36	5.18
	(1.88)***	(1.80)***	(1.71)***	(1.52)***	(1.53)***	(1.47)***
Institutional quality	0.392	**0.393**	**0.361**	0.375	**0.376**	**0.332**
	(0.118)***	**(0.141)*****	**(0.134)*****	(0.108)***	**(0.124)*****	**(0.121)*****
Inflation	-1.73	**-1.59**	**-1.54**	-1.64	**-1.59**	**-1.61**
	(0.588)***	**(0.685)****	**(0.668)****	(0.415)***	**(0.416)*****	**(0.404)*****
Budget balance	8.46	**9.33**	**11.1**	7.49	**10.5**	**12.5**
	(5.47)	**(7.18)**	**(6.79)**	(4.40)*	**(5.93)***	**(5.28)****
Openness Sachs-Warner	1.56	**1.77**	**1.80**	1.58	**1.59**	**1.72**
	(0.456)***	**(0.506)*****	**(0.514)*****	(0.384)***	**(0.457)*****	**(0.469)*****
Civil war	-2.02	-1.88		-1.47	-1.32	
	(0.870)**	(0.853)**		(0.792)*	(0.774)*	
Lagged civil war	1.72	1.45		1.36	1.12	
	(0.705)**	(0.665)**		(0.641)**	(0.565)**	
Observations	368	367	365	368	367	365
Hansen *J* stat of over-identifying restrictions (*p*-value)	0.689	0.562	0.592	0.689	0.562	0.592
First order autocorrelation (*p*-value)	0.057	0.100	0.140	0.054	0.102	0.094
Second order autocorrelation (*p*-value)	0.085	0.066	0.057	0.077	0.061	0.049
Shea partial *R*-squared for aid	0.318	0.351	0.344	0.318	0.351	0.344
Shea partial *R*-squared for aid squared	0.260	0.296	0.290	0.260	0.296	0.290
Shea partial *R*-squared for log repayments	0.530	0.515	0.520	0.530	0.515	0.520
Shea partial *R*-squared for log inital GDP per capita	0.788	0.775	0.768	0.788	0.775	0.768
Shea partial *R*-squared for institutional quality		0.800	0.793		0.800	0.793
Shea partial *R*-squared for inflation		0.751	0.746		0.751	0.746
Shea partial *R*-squared for budget balance		0.400	0.410		0.400	0.410
Shea partial *R*-squared for openness		0.692	0.698		0.692	0.698

Note: Robust and clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include period dummies and a constant term. For all regressions, *F*-tests for the hypothesis that the instruments are not relevant are rejected at the 1% level. Influential observations in aid-growth space removed using Hadi (1992) procedure. For details see text.

Table 8. Robustness test, bootstrapped core regression

Variable	Observed	Bias	Std. Err.	[95% Conf.	Interval]
Short-impact aid	0.960	-0.156	0.317	0.525	1.90
Short-impact aid squared	-0.0588	0.0116	0.0238	-0.132	-0.025
Log repayments	-0.384	0.0419	0.206	-0.899	-0.035
Log initial GDP per capita	-0.0593	-0.112	0.439	-0.799	0.944
East Asia	2.39	-0.0425	0.504	1.47	3.43
Tropics	-2.13	0.0582	0.363	-2.93	-1.48
Log initial life expectancy	3.49	0.133	1.73	0.0394	7.00
Civil war	-2.19	-0.0592	0.825	-3.91	-0.673
Lagged civil war	1.86	0.0544	0.736	0.395	3.25
Institutional quality	0.333	-0.000630	0.107	0.138	0.563
Inflation	-1.60	-0.0604	0.511	-2.56	-0.494
Budget balance	8.28	0.181	4.41	-0.405	16.6
Openness Sachs-Warner	1.41	-0.00863	0.426	0.610	2.28

Note: For bootstrapping, 368 observations are drawn, with replacement, from the full sample. The core regression is run each time on the resulting dataset, and this procedure is repeated 1,000 times. The resulting coefficients form the sampling distribution from which the 95 percent (bias-corrected) confidence intervals are calculated.

Table 9. Initial exploration of aid interaction effects

2SLS	1	2	3	4	5	6	7	8	9	10	11	12	13
Short-impact aid	0.961	1.01	0.897	-2.72	1.04	0.973	0.436	1.02	0.840	0.915	1.06	0.956	0.594
	(1.08)	(0.333)***	(0.328)***	(2.07)	(0.411)**	(0.381)**	(0.545)	(0.346)***	(0.303)***	(0.381)**	(0.403)***	(1.117)	(0.423)
Short-impact aid squared	-0.0460	-0.0603	-0.0580	-0.0370	-0.0686	-0.0648	-0.0483	-0.0667	-0.0429	-0.0570	-0.0637	-0.0769	-0.0466
	(0.0135)***	(0.0268)**	(0.0263)**	(0.0120)***	(0.0360)*	(0.0341)*	(0.0316)	(0.0293)**	(0.0113)***	(0.0277)**	(0.0294)**	(0.0630)	(0.0236)**
Log repayments	-0.395	-0.409	-0.350	-0.426	-0.434	-0.384	-0.475	-0.440	-0.384	-0.381	-0.407	-0.451	-0.331
	(0.186)**	(0.195)**	(0.188)*	(0.165)**	(0.204)**	(0.190)**	(0.179)***	(0.197)**	(0.185)**	(0.186)**	(0.193)**	(0.251)*	(0.180)*
Log initial GDP per capita	-0.170	0.116	-0.056	-0.187	-0.109	-0.0175	-0.102	-0.0285	-0.190	-0.0845	-0.0334	-0.144	-0.235
	(0.644)	(0.528)	(0.535)	(0.535)	(0.476)	(0.535)	(0.603)	(0.499)	(0.591)	(0.500)	(0.500)	(0.531)	(0.534)
East Asia	2.44	0.648	2.39	2.39	2.33	2.33	2.62	2.33	2.43	2.42	2.34	2.33	2.34
	(0.621)***	(1.470)	(0.646)***	(0.604)***	(0.657)***	(0.652)***	(0.683)***	(0.653)***	(0.672)***	(0.658)***	(0.660)***	(0.653)***	(0.636)***
Tropics	-1.95	-2.24	-2.16	-2.13	-2.12	-2.06	-2.08	-2.18	-1.97	-2.10	-2.17	-2.11	-2.04
	(0.401)***	(0.443)***	(0.607)***	(0.389)***	(0.396)***	(0.395)***	(0.500)***	(0.417)***	(0.395)***	(0.418)***	(0.420)***	(0.517)***	(0.517)***
Log initial life expectancy	3.87	3.12	3.52	-0.95	4.09	3.38	3.66	3.23	3.72	3.54	3.35	3.87	3.39
	(1.88)**	(1.93)	(1.88)*	(3.39)	(2.02)**	(1.89)*	(1.97)*	(1.88)*	(1.88)**	(1.84)*	(1.85)*	(1.69)**	(1.79)*
Civil war	-2.22	-2.16	-2.01	-1.37	-3.44	-2.17	-2.23	-2.27	-2.24	-2.21	-2.20	-2.52	-2.23
	(0.891)**	(0.903)**	(0.873)**	(0.759)*	(1.713)**	(0.895)**	(0.906)**	(0.923)**	(0.888)**	(0.879)**	(0.910)**	(1.00)**	(0.881)**
Lagged civil war	1.98	1.80	1.56	1.48	1.83	0.698	2.00	1.81	2.00	1.86	1.84	2.21	2.06
	(0.705)***	(0.727)**	(0.703)**	(0.649)**	(0.797)**	(1.800)	(0.713)***	(0.737)**	(0.711)***	(0.722)***	(0.740)**	(0.959)**	(0.810)**
Institutional quality	0.342	0.406	0.345	0.424	0.323	0.331	0.115	0.331	0.335	0.344	0.326	0.332	0.359
	(0.121)***	(0.139)***	(0.114)***	(0.117)***	(0.117)***	(0.115)***	(0.244)	(0.117)***	(0.106)***	(0.120)***	(0.117)***	(0.130)**	(0.116)***
Inflation	-1.68	-1.54	-1.62	-1.87	-1.72	-1.70	-1.63	-1.99	-1.67	-1.62	-1.74	-1.67	-1.64
	(0.634)***	(0.542)***	(0.553)***	(0.667)***	(0.607)***	(0.611)***	(0.583)***	(0.775)**	(0.581)***	(0.567)***	(0.639)***	(0.602)***	(0.580)***
Budget balance	9.64	9.57	8.47	7.53	8.13	7.90	8.76	8.38	3.37	8.20	8.76	8.36	8.04
	(5.79)*	(5.71)*	(5.44)	(4.87)	(5.44)	(5.44)	(5.63)	(5.47)	(11.77)	(5.53)	(5.74)	(7.69)	(6.06)
Openness Sachs-Warner	1.27	1.30	1.33	1.39	1.40	1.39	1.45	1.50	1.31	1.25	1.56	1.46	1.38
	(0.431)***	(0.506)**	(0.451)***	(0.424)***	(0.488)***	(0.465)***	(0.450)***	(0.465)***	(0.429)***	(0.738)*	(0.530)***	(0.518)***	(0.489)***
Short-impact aid x log inital GDP per capita	-0.0234												
	(0.134)												
Short-impact aid x East Asia		2.94											
		(2.49)											
Short-impact aid x tropics			0.0464										
			(0.252)										
Short-impact aid x log inital life expectancy				0.833									
				(0.514)									
Short-impact aid x civil war					0.362								
					(0.393)								
Short-impact aid x lagged civil war						0.312							
						(0.420)							
Short-impact aid x institutional quality							0.0975						
							(0.113)						
Short-impact aid x inflation								0.167					
								(0.153)					
Short-impact aid x budget balance									1.94				
									(3.43)				
Short-impact aid x openness										0.0545			
										(0.173)			
Short-impact aid x policy											-0.0305		
											(0.0493)		
Short-impact aid in 1982-89												-0.148	0.316
												(1.72)	(0.898)
Short-impact aid squared in 1982-89												0.0296	
												(0.114)	
Short-impact aid in 1990-01												0.0565	0.150
												(1.30)	(0.511)
Short-impact aid squared in 1990-01												0.0231	
												(0.0648)	
Observations	368	368	366	364	368	368	366	368	370	368	368	370	368
R-squared	0.386	0.358	0.391	0.375	0.355	0.374	0.354	0.376	0.363	0.391	0.382	0.322	0.356
Hansen *J* stat of over-identifying restrictions (*p*-value)	0.488	0.681	0.591	0.400	0.813	0.726	0.743	0.864	0.404	0.727	0.802	0.452	0.505

Note: Robust and clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include period dummies and a constant term. Aid, aid squared and repayments are instrumented. For all regressions, *F*-tests for the hypothesis that the instruments are not relevant are rejected at the 1% level. Influential observations in aid-growth space removed using Hadi (1992) procedure. For details see text.

Table 10. More detailed exploration of aid interaction effects
(Influential observations identified on the interaction term and excluded)

	1 2SLS	2 GMM	3 2SLS	4 GMM	5 2SLS	6 GMM	7 2SLS	8 GMM	9 2SLS	10 GMM
Short-impact aid	0.931	0.798	0.616	0.605	-3.47	-2.32	0.424	0.356	0.751	0.884
	(1.09)	(0.856)	(0.384)	(0.291)**	(1.98)*	(1.45)	(0.341)	(0.273)	(0.248)***	(0.217)***
Short-impact aid squared	-0.0437	-0.0448	-0.0282	-0.0316	-0.0416	-0.0379	-0.0519	-0.0651	-0.0458	-0.0400
	(0.0133)***	(0.0108)***	(0.0124)**	(0.0093)***	(0.0118)***	(0.0081)***	(0.0211)**	(0.0131)***	(0.0149)***	(0.0121)***
Log repayments	-0.304	-0.413	-0.200	-0.299	-0.450	-0.535	-0.460	-0.568	-0.407	-0.516
	(0.173)*	(0.151)***	(0.162)	(0.147)**	(0.173)***	(0.160)***	(0.178)***	(0.161)***	(0.186)**	(0.156)***
Log initial GDP per capita	-0.227	-0.363	-0.573	-0.375	0.010	-0.404	-0.138	-0.153	-0.349	-0.700
	(0.640)	(0.480)	(0.485)	(0.428)	(0.517)	(0.414)	(0.591)	(0.479)	(0.488)	(0.386)*
East Asia	2.33	2.46	2.31	2.38	2.44	2.70	2.51	2.96	2.31	2.24
	(0.634)***	(0.601)***	(0.611)***	(0.574)***	(0.607)***	(0.564)***	(0.688)***	(0.624)***	(0.660)***	(0.632)***
Tropics	-1.93	-2.05	-1.68	-2.13	-2.27	-2.35	-2.13	-2.36	-2.01	-2.23
	(0.389)***	(0.349)***	(0.584)***	(0.535)***	(0.372)***	(0.290)***	(0.507)***	(0.353)***	(0.390)***	(0.331)***
Log initial life expectancy	3.89	3.94	3.16	3.62	-2.05	0.15	3.52	3.80	3.78	4.72
	(1.87)**	(1.54)**	(1.75)*	(1.47)**	(3.31)	(2.46)	(1.90)*	(1.61)**	(1.82)**	(1.56)***
Civil war	-2.22	-1.61	-1.60	-1.72	-1.32	-1.70	-2.23	-1.57	-2.21	-2.03
	(0.880)**	(0.795)**	(0.706)**	(0.615)***	(0.771)*	(0.721)**	(0.901)**	(0.827)*	(0.904)**	(0.848)**
Lagged civil war	2.01	1.32	1.38	1.39	1.39	1.38	2.01	1.56	1.95	1.63
	(0.705)***	(0.623)**	(0.517)***	(0.481)***	(0.625)**	(0.582)**	(0.698)***	(0.621)**	(0.710)***	(0.614)***
Institutional quality	0.358	0.380	0.389	0.337	0.419	0.398	0.103	0.008	0.333	0.290
	(0.118)***	(0.108)***	(0.118)***	(0.099)***	(0.118)***	(0.108)***	(0.272)	(0.196)	(0.112)***	(0.102)***
Inflation	-1.66	-1.05	-1.29	-1.14	-1.86	-1.68	-1.63	-1.06	-1.86	-1.69
	(0.630)***	(0.423)**	(0.461)***	(0.392)***	(0.658)***	(0.534)***	(0.558)***	(0.354)***	(0.519)***	(0.450)***
Budget balance	9.44	6.49	9.06	6.82	6.89	5.90	7.78	4.64	9.18	10.40
	(5.70)*	(4.81)	(5.21)*	(4.29)	(4.86)	(4.22)	(5.34)	(4.58)	(6.50)	(5.81)*
Openness Sachs-Warner	1.28	1.26	1.10	1.32	1.52	1.43	1.52	1.50	1.33	1.80
	(0.430)***	(0.388)***	(0.440)**	(0.384)***	(0.431)***	(0.371)***	(0.468)***	(0.383)***	(0.585)**	(0.513)***
Short aid-impact x log inital GDP per capita	-0.0279	-0.00569								
	(0.133)	(0.102)								
Short aid-impact x tropics			-0.198	-0.019						
			(0.431)	(0.355)						
Short aid-impact x log inital life expectancy					1.04	0.748				
					(0.485)**	(0.352)**				
Short aid-impact x institutional quality							0.104	0.162		
							(0.122)	(0.086)*		
Short aid-impact x policy									0.0123	-0.102
									(0.0952)	(0.0733)
Observations	368	368	356	356	363	363	370	370	367	367
R-squared	0.385	0.368	0.399	0.379	0.374	0.386	0.350	0.278	0.387	0.375
Hansen *J* stat of over-identifying restrictions (*p*-value)	0.315	0.315	0.286	0.286	0.563	0.563	0.613	0.613	0.523	0.523

Note: Robust and clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include period dummies and a constant term. Aid, aid squared and repayments are instrumented. For all regressions, *F*-tests for the hypothesis that the instruments are not relevant are rejected at the 1% level. Influential observations in interaction term-growth space removed using Hadi (1992) procedure. For details see text.

Table 11. Growth accounting

	Regression coefficient	Elasticities	**Average values of regressors**							**Contibution of each variable to the difference in per capita growth relative to average for all countries**					
			All	LAC	Asia		E. Europe	MENA	SSA	LAC	Asia		E. Europe	MENA	SSA
					All	*E. Asia*					All	*E. Asia*			
Short-impact aid	0.960	2.084	2.75	1.33	1.73	*0.59*	0.32	3.62	5.33	-1.36	-0.98	*-2.07*	-2.33	0.83	2.48
Short-impact aid squared	-0.0588	-0.925	19.90	6.22	9.68	*0.61*	0.19	30.08	43.69	0.81	0.60	*1.14*	1.16	-0.60	-1.40
Log repayments	-0.384	-0.472	-1.56	-2.02	-1.61	*-2.12*	-2.96	-1.31	-0.78	0.18	0.02	*0.22*	0.54	-0.10	-0.30
Log initial GDP per capita	-0.0593	-0.356	7.60	7.96	7.76	*8.07*	8.39	7.91	6.77	-0.02	-0.01	*-0.03*	-0.05	-0.02	0.05
East Asia	2.39	-	0.12	0.00	0.62	*1.00*	0.00	0.00	0.00	-0.29	1.20	*2.11*	-0.29	-0.29	-0.29
Tropics	-2.13	-	0.70	0.79	0.68	*0.80*	0.00	0.00	1.00	-0.18	0.05	*-0.20*	1.49	1.49	-0.64
Log initial life expectancy	3.49	11.3	4.09	4.18	4.16	*4.20*	4.25	4.16	3.87	0.33	0.24	*0.39*	0.55	0.25	-0.76
Civil war	-2.19	-	0.11	0.13	0.08	*0.05*	0.00	0.11	0.11	-0.05	0.05	*0.13*	0.23	0.00	-0.01
Lagged civil war	1.86	-	0.10	0.12	0.07	*0.05*	0.00	0.11	0.11	0.04	-0.06	*-0.10*	-0.19	0.01	0.02
Institutional quality	0.333	1.18	4.50	4.33	5.14	*5.51*	5.73	4.28	4.11	-0.06	0.22	*0.34*	0.41	-0.07	-0.13
Inflation	-1.60	-0.297	0.23	0.36	0.08	*0.07*	0.30	0.09	0.22	-0.20	0.25	*0.26*	-0.10	0.23	0.02
Budget balance	8.28	0.215	-0.03	-0.03	-0.03	*-0.01*	-0.04	-0.05	-0.03	0.03	0.04	*0.21*	-0.07	-0.10	-0.01
Openness Sachs-Warner	1.41	0.470	0.42	0.43	0.58	*0.85*	0.76	0.41	0.24	0.01	0.23	*0.60*	0.48	-0.02	-0.25
1978-81 dummy	-1.25	-	0.14	0.15	0.14	*0.14*	0.10	0.16	0.13	-0.01	0.00	*0.01*	0.06	-0.02	0.01
1982-85 dummy	-2.76	-	0.15	0.15	0.14	*0.14*	0.10	0.13	0.17	0.00	0.02	*0.03*	0.14	0.04	-0.06
1986-89 dummy	-1.64	-	0.14	0.14	0.14	*0.14*	0.10	0.11	0.15	0.00	-0.01	*0.00*	0.07	0.05	-0.02
1990-93 dummy	-2.18	-	0.15	0.14	0.15	*0.16*	0.10	0.16	0.17	0.03	-0.01	*-0.02*	0.12	-0.02	-0.04
1994-97 dummy	-2.27	-	0.15	0.15	0.15	*0.16*	0.24	0.16	0.15	0.02	0.00	*-0.01*	-0.19	-0.01	0.01
1998-2001 dummy	-2.93	-	0.16	0.15	0.14	*0.14*	0.33	0.18	0.14	0.04	0.05	*0.06*	-0.52	-0.08	0.05
Constant	-12.86		1.00	1.00	1.00	*1.00*	1.00	1.00	1.00	0.00	0.00	*0.00*	0.00	0.00	0.00
Average growth rate, observed			1.27	0.66	3.55	*4.31*	2.87	2.40	-0.23						
Average growth difference explained										-0.70	1.89	*3.05*	1.53	1.59	-1.26
Average growth difference observed, check										-0.60	2.29	*3.05*	1.60	1.13	-1.50
Difference										-0.10	-0.40	*0.00*	-0.07	0.46	0.23
Average effect of aid on difference in growth										**-0.56**	**-0.38**	***-0.94***	**-1.17**	**0.23**	**1.08**
Observations			368	131	71	*44*	21	38	107	131	71	*44*	21	38	107
Countries			67	20	11	*7*	7	7	22	20	11	*7*	7	7	22

Note: Countries are classified into regions according to standard World Bank definitions. East Asia includes China, Indonesia, South Korea, Malaysia, Philippines, Singapore, and Thailand. For consistency, elasticities for dummy variables are excluded.

Table 12. Links to the recent literature: The evolution from Burnside and Dollar to our results

2SLS	1 B&D replication	2 B&D replaication, larger dataset	3 With aid squared	4 Switch to Short/PPP GDP	5 Switch to Short/XR GDP	6 With aid squared	7 Replace EDA with ODA	8 Replace PPP with XRGDP	9 Switch to gross and repayments	10 Switch to Short/XR GDP	11 Switch to Short/PPP GDP	12 Remove influential obs. Core
Net EDA/PPP GDP	-0.242	-0.218	-1.17			0.428						
	(0.259)	(0.266)	(0.825)			(0.446)						
Net EDA/PPP GDP * policy	0.253	0.0719										
	(0.121)**	(0.0678)										
Net EDA/PPP GDP squared			0.177			-0.0384						
			(0.149)			(0.0533)						
Short-impact ODA/PPP GDP				0.470							0.822	
				(0.403)							(0.379)**	
Short-impact ODA/PPP GDP squared				-0.0602							-0.100	
				(0.0366)							(0.0370)***	
Short-impact ODA/XR GDP					0.506					0.684		0.960
					(0.227)**					(0.228)***		(0.328)***
Short-impact ODA/XR GDP squared					-0.0309					-0.0408		-0.0588
					(0.0131)**					(0.0130)***		(0.0264)**
Net ODA/PPP GDP							0.358					
							(0.205)*					
Net ODA/PPP GDP squared							-0.0268					
							(0.0126)**					
Net ODA/XR GDP								0.285				
								(0.111)**				
Net ODA/XR GDP squared								-0.00845				
								(0.00346)**				
Gross ODA/XR GDP									0.278			
									(0.104)***			
Gross ODA/XR GDP squared									-0.00702			
									(0.00295)**			
Log repayments/PPP GDP				-0.231							-0.366	
				(0.291)							(0.243)	
Log repayments/XR GDP					-0.090				-0.219	-0.204		-0.384
					(0.290)				(0.216)	(0.224)		(0.188)**
Log initial GDP per capita	-0.832	-0.365	-0.604	-0.207	0.248	0.046	0.0507	0.277	0.0828	0.217	-0.220	-0.0593
	(0.774)	(0.638)	(0.669)	(0.628)	(0.680)	(0.622)	(0.576)	(0.557)	(0.655)	(0.625)	(0.620)	(0.493)
Ethnic fractionalization	-0.672	-0.121	0.036	0.071	0.286							
	(0.844)	(0.816)	(0.834)	(0.750)	(0.741)							
Assasinations	-0.760	-0.293	-0.249	-0.337	-0.315							
	(0.445)*	(0.223)	(0.232)	(0.227)	(0.224)							
Ethnic * assasinations	0.634	0.047	-0.017	0.135	0.162							
	(0.903)	(0.605)	(0.607)	(0.611)	(0.611)							
Sub-Saharan Africa	-2.11	-1.39	-1.20	-1.65	-1.84							
	(0.727)***	(0.648)**	(0.643)*	(0.705)**	(0.768)**							
East Asia	1.46	0.80	0.47	0.78	1.01	2.18	2.27	2.43	2.38	2.35	2.07	2.39
	(0.712)**	(0.519)	(0.502)	(0.507)	(0.528)*	(0.475)***	(0.471)***	(0.471)***	(0.471)***	(0.465)***	(0.455)***	(0.648)***
Institutional quality	0.845	0.395	0.426	0.398	0.409	0.302	0.316	0.316	0.321	0.315	0.326	0.333
	(0.193)***	(0.134)***	(0.141)***	(0.136)***	(0.139)***	(0.111)***	(0.107)***	(0.104)***	(0.104)***	(0.105)***	(0.107)***	(0.114)***
M2/GDP lagged	0.025	0.006	0.001	0.001	-0.007							
	(0.017)	(0.012)	(0.013)	(0.010)	(0.011)							
Policy index	0.591	0.869	1.04	1.00	0.942							
	(0.378)	(0.171)***	(0.140)***	(0.148)***	(0.146)***							
Tropics						-1.81	-1.80	-1.85	-1.84	-1.86	-1.78	-2.13
						(0.374)***	(0.377)***	(0.369)***	(0.371)***	(0.378)***	(0.382)***	(0.398)***
Log initial life expectancy						1.81	1.92	2.50	2.76	2.24	2.14	3.49
						(1.91)	(1.90)	(1.95)	(2.12)	(2.14)	(2.13)	(1.85)*
Civil war						-2.37	-2.33	-2.39	-2.32	-2.39	-2.30	-2.19
						(0.810)***	(0.796)***	(0.798)***	(0.804)***	(0.814)***	(0.810)***	(0.891)**
Lagged civil war						1.90	1.86	2.06	1.93	2.03	1.71	1.86
						(0.693)***	(0.666)***	(0.671)***	(0.664)***	(0.679)***	(0.667)**	(0.730)**
Inflation						-1.96	-1.93	-1.73	-1.82	-1.71	-2.05	-1.60
						(0.411)***	(0.422)***	(0.445)***	(0.457)***	(0.453)***	(0.436)***	(0.558)***
Budget balance						5.13	4.60	6.94	6.43	6.42	4.07	8.28
						(4.58)	(4.63)	(4.48)	(4.48)	(4.66)	(4.80)	(5.47)
Openness Sachs-Warner						1.18	1.07	1.00	1.10	1.17	1.37	1.41
						(0.401)***	(0.404)***	(0.400)**	(0.417)***	(0.430)***	(0.439)***	(0.456)***
Observations	184	361	361	361	361	372	372	372	372	372	372	368
R-squared	0.472	0.346	0.313	0.338	0.332	0.392	0.391	0.393	0.389	0.375	0.379	0.388
Hansen *J* stat of over-identifying restrictions (*p*-value)	0.233	0.005	0.019	0.002	0.001	0.155	0.168	0.256	0.256	0.646	0.436	0.773
Shea partial *R*-squared for aid	0.393	0.272	0.108	0.532	0.460	0.316	0.690	0.641	0.627	0.491	0.544	0.314

Note: Robust and clustered standard errors in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include period dummies and a constant term. Aid, aid squared and repayments are instrumented. For all regressions, *F*-tests for the hypothesis that the instruments are not relevant are rejected at the 1% level. For details see text.

Table 13. Links to the recent literature: The evolution from Hansen and Tarp to our results

	1	2	3	4	5	6
2SLS	H&T replication	New dataset	New dataset, autocorr correction	Reg 3, with dissag aid	Reg 3, with new controls	Core
Net ODA	0.241	0.237	0.237		0.248	
	(0.103)**	(0.117)**	(0.133)*		(0.105)**	
Net ODA squared	-0.00764	-0.00654	-0.00654		-0.00660	
	(0.00320)**	(0.00337)*	(0.00370)*		(0.00358)*	
Short-impact aid				0.846		0.960
				(0.405)**		(0.328)***
Short-impact aid squared				-0.0491		-0.0588
				(0.0207)**		(0.0264)**
Log repayments				-0.539		-0.384
				(0.607)		(0.188)**
Log initial GDP per capita	0.087	0.437	0.437	0.152	0.0178	-0.059
	(0.605)	(0.627)	(0.531)	(0.616)	(0.402)	(0.493)
Ethnic fractionalization	0.111	0.455	0.455	0.251		
	(0.940)	(0.733)	(0.988)	(1.015)		
Assasinations	-0.460	-0.313	-0.313	-0.340		
	(0.228)**	(0.228)	(0.211)	(0.215)		
Ethnic * assasinations	0.919	0.171	0.171	0.222		
	(0.423)**	(0.612)	(0.595)	(0.601)		
Sub-Saharan Africa	-2.25	-1.71	-1.71	-1.96		
	(0.755)***	(0.673)**	(0.788)**	(1.01)*		
East Asia	1.52	1.25	1.25	1.28	2.33	2.39
	(0.627)**	(0.562)**	(0.734)*	(0.789)	(0.630)***	(0.648)***
Institutional quality	0.811	0.423	0.423	0.465	0.301	0.333
	(0.177)***	(0.132)***	(0.138)***	(0.148)***	(0.107)***	(0.114)***
M2/GDP lagged	0.010	-0.004	-0.004	-0.005		
	(0.018)	(0.012)	(0.015)	(0.016)		
Inflation	-1.13	-1.91	-1.91	-1.87	-1.80	-1.60
	(0.493)**	(0.405)***	(0.436)***	(0.429)***	(0.541)***	(0.558)***
Budget balance	9.12	7.42	7.42	6.92	8.29	8.28
	(3.66)**	(4.84)	(6.68)	(7.04)	(5.30)	(5.47)
Openness Sachs-Warner	1.69	1.38	1.38	1.68	1.16	1.41
	(0.505)***	(0.421)***	(0.446)***	(0.519)***	(0.435)***	(0.456)***
Tropics					-2.11	-2.13
					(0.374)***	(0.398)***
Log initial life expectancy					3.37	3.49
					(1.71)**	(1.85)*
Civil war					-1.90	-2.19
					(0.726)***	(0.891)**
Lagged civil war					1.78	1.86
					(0.494)***	(0.730)**
Constant	-2.98	-4.44	-4.44	-3.17	-13.69	-12.86
	(3.96)	(4.76)	(4.10)	(4.15)	(5.98)**	(6.24)**
Observations	231	357	357	356	368	368
R-squared	0.379	0.323	0.323	0.279	0.444	0.388
Hansen *J* stat of over-identifying restrictions (*p*-value)	0.795	0.158	0.263	0.514	0.300	0.773
Influential observations removed using Hadi procedure	N	Y	Y	Y	Y	Y

Note: Robust standard errors in parentheses for regressions 1 and 2. Robust and clustered standard errors for regression 3 onwards, to correct for autocorrelation. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include period dummies and a constant term. Aid, aid squared and repayments are instrumented. For all regressions, *F*-tests for the hypothesis that the instruments are not relevant are rejected at the 1% level. For details see text.

A Data Appendix

Time periods. Observations for all variables are four-year arithmetic averages unless data are missing (in which case averages for non-missing data are used) or otherwise specified. Time period 1 represents the years 1974-77. Likewise, 2: 1978-81, 3: 1982-85, 4: 1986-89, 5: 1990-93, 6: 1994-97, 7: 1998-2001.

Short-impact, long-impact and humanitarian aid. In percent of GDP. Data for the numerator come from the Creditor Reporting System (CRS) 2003 and Donor Assistance Committee (DAC) 2003 databases maintained by the OECD. The numerator for "short-impact" aid is the product across all donors, for each recipient, of "Total ODA (OA) Gross" from the online DAC 2003 database, Table 2a, with the elements of CRS field "usd_amount" classified as "S" in Appendix Table 1, divided by the sum of all aid in DAC field "Total ODA (OA) Commitments," multiplied by 100. "Long-impact" and "humanitarian" aid are similarly calculated, according to the "L" or "H" classification in Appendix Table 1. The denominator is GDP in current USD from the World Development Indicators 2003. Tables 3 and 4 and figure 1 compare these aid estimates to actual data for the 1990s. The latter are calculated as follows: from the CRS database, the field "usd_disbursement," is totaled by the recipient-year and type of aid, "S," "L" or "H." This is divided by total disbursements in the CRS database, which is then multiplied by "Total ODA (OA) Gross" from the DAC 2003 CD-ROM.

Net ODA. In percent of GDP. Numerator is the field "Total ODA (OA) Net" from the DAC 2003 online database, table 2a. The denominator is GDP in current USD from World Development Indicators 2003.

Gross ODA. In percent of GDP. Numerator from the DAC 2003 CD-ROM. Denominator is GDP in current USD from the World Bank's *World Development Indicators 2003*.

Per capita real GDP growth. Annual percentage growth rate of GDP per capita based on constant local currency. From the *World Development Indicators 2003* field "GDP per capita growth (annual %)."

Repayments on aid. In percent of GDP or natural logarithm of repayments in percent of GDP. "Total ODA (OA) Gross" minus "Total ODA (OA) Net" from the DAC database as a percent of GDP in current USD from *World Development Indicators 2003*.

Log initial GDP per capita. GDP per capita in 1996 USD for the first year of the period. Data from Penn World Tables version 6. Data first introduced in robert Summers and Alan Heston (1991), "The Penn World Table (Mark 5): An Expanded Set of International Comparisons, 1950-88," *Quarterly Journal of Economics*, 106 (2): 327-368.

East Asia. Dummy for China, Indonesia, South Korea, Malaysia, Philippines, Singapore and Thailand.

Tropics. 0/1 dummy, indicative of location in tropics. William Easterly (2001) "The Middle Class Consensus and Economic Development" *Journal of Economic Growth*, 6 (4): 317-336.

Log initial life expectancy. Missing data linearly interpolated. From *World Development Indicators 2003*.

Institutional Quality. Continuous variable between 0 and 10, commonly known as the International Country Risk Guide (ICRGE) indicator. Average of corruption, bureaucratic quality and rule of law indicators published in Political Risk Group's IRIS III dataset. Composite indicator extended by David Roodman, "Anarchy of Numbers: Aid, Development and Cross-Country Empirics." Center for Global Development, Working Paper 32, September 2003.

Inflation. Natural logarithm of 1+consumer price inflation rate. *World Development Indicators 2003* and International Financial Statistics 2003.

Budget balance. Ratio to GDP. Numerator is "overall budget balance, including grants" field from the World Development Indicators 2003; denominator is GDP in current USD from the World Development Indicators 2003. Where data are missing series 80 and 99b (local-currency budget surplus and GDP) of the IMF's *International Financial Statistics 2003* are used.

Openness. Dummy variable averaged over four years. From Jeffrey D. Sachs and Andrew Warner

(1995), "Economic reform and the process of global integration." *Brookings Papers on Economic Activity*, 1995: 1-118; Romain Wacziarg and Karen Welch Horn (2002),. "Trade Liberalization and Growth: New Evidence," processed, Stanford University, November; and Roodman (2003).

Policy index. Linear combination of inflation, budget balance and openness variables as per Craig Burnside and David Dollar (2000), "Aid, Policies, and Growth." *American Economic Review*, 90 (4): 847-868.

Arms imports. Lagged, and in percent of GDP. Data compiled by Burnside and Dollar (2000) from US Department of State, *World Military Expenditures and Arms Transfers,* Washington, DC, various years.

Land area. Kilometers. Data from *CIA World Factbook* 2003.

Coastline. Squared Kilometers. Data from *CIA World Factbook* 2003.

Secondary school enrolment ratio. Gross (ratio of the total enrollment, regardless of age, to the population of the secondary school age group), percent. Missing data are linearly interpolated. Data from World Development Indicators 2003.

Natural resource abundance. Primary products exports as a percent of GDP. Calculated as (ores and metal exports + agricultural exports + fuel exports + food exports, all as a percent of merchandise exports) times merchandise exports (where unavailable, exports of goods), as a percent of GDP. All series from *World Development Indicators 2003.*

Positive and negative commodity shocks. Shocks in percent of GDP, and are based on commodity price index changes. From Jan Dehn (2000), "Commodity Price Uncertainty in Developing Countries." Working Paper 122. Centre for the Study of African Economies Working Paper Series. May. Supplemented by dataset extension in Roodman 2003.

Natural disaster. Number of droughts, earthquakes, famines and floods divided by GDP. Numerator from the Emergency Disasters Database (EM-DAT) 2003, compiled by the Center for Research on the Epidemiology of Disasters. Denominator is GDP in current USD from *World Development Indicators 2003.*

Civil War. The dichotomous civil war variable was constructed by combining information from the Correlates of War 2 (COW2) database (Meredith Reid Sarkees [2000], "The Correlates of War Data on War: An Update to 1997," *Conflict Management and Peace Science* 18 [1]: 123-144) and the Peace Research Institute, Oslo (PRIO) Armed Conflict Database (Nils Petter Gleditsch, Peter Wallensteen, Mikael Eriksson, Margareta Sollenberg and Håvard Strand [2002], "Armed conflict 1946-2001: A new dataset," *Journal of Peace Research* 39 [5]: 615-637). First, a country-period was assigned the value of 1 if any part of an "intrastate war" in COW2 occurred during that period. Country-periods of war were then modified if the dates of the conflict in PRIO were materially different (giving PRIO precedence), ir the number of deaths was very small relative to the population, or if the area affected by the war was isolated from economic centers and therefore unlikely to substantially affect national economic growth. Accordingly the following changes were made: Colombia starting period changed from 3 to 4 and ending period from 6 to 5 based on PRIO; Iran ending period changed from 3 to 4 based on PRIO; Sri Lanka ending period changed from 3 to 4 based on PRIO; Sierra Leone ending period changed from 6 to 7 based on PRIO; and the following country-periods were omitted based on the small number of casualties relative to population or isolation of war-affected area from economic centers: AZE5-6, BDI4-5, ETH1-3, GEO5-6, IND3-7, IRN2, KHM5-6, NGA2-3, PAK1, PAK6, PHL1-2, ROM4, RUS6, RWA5, TUR5-6, UGA6-7, YEM4, YEM6, YUG5, ZAR5, ZAR6-7. PHL3 and PHL5 were set to "1" based on PRIO. Thus the following were considered catastrophic civil wars likely to influence national economic growth: AFG2-5, AGO1-5, AGO5-6, BDI5-7, BIH5-6, COG6-7, COL4-7, DZA5-7, ETH1-5, GNB7-7, GTM2-3, IRN2-4, IRQ1, IRQ3-6, KHM1-5, LBN1-5, LBR4-6, LKA4-7, MMR3-6, MOZ2-5, NIC2-5, PER3-6, RWA6-6, SDN3-7, SLE5-7, SLV2-5, SOM3-6, TCD2-4, TJK5-6, UGA2-4, ZWE1-2.

Appendix Table 1. Classifying aid by lag of expected impact
S = short-impact aid, L = long-impact aid, and H = humanitarian aid

DAC5 (3 digit) code and CRS (5 digit) purpose codes	1997-01 commitments (USD m)	percent	Category
110 EDUCATION	324	0.1	L
111 Education, level unspecified	643	0.3	L
11110 Education policy and administrative management	2,383	1.1	L
11120 Education facilities and training	761	0.3	L
11130 Teacher training	234	0.1	L
11181 Educational research	13	0.0	L
112 Basic education	211	0.1	L
11220 Primary education	3,965	1.8	L
11230 Basic life skills for youth and adults	486	0.2	L
11240 Early childhood education	51	0.0	L
113 Secondary education	0	0.0	L
11320 Secondary education	843	0.4	L
11330 Vocational training	1,002	0.4	L
114 Post-secondary education	8	0.0	L
11420 Higher education	3,586	1.6	L
11430 Advanced technical and managerial training	428	0.2	L
120 HEALTH	52	0.0	L
121 Health, general	290	0.1	L
12110 Health policy and administrative management	3,850	1.7	L
12181 Medical education/training	166	0.1	L
12182 Medical research	45	0.0	L
12191 Medical services	1,128	0.5	L
122 Basic health	10	0.0	L
12220 Basic health care	1,720	0.8	L
12230 Basic health infrastructure	1,026	0.5	L
12240 Basic nutrition	378	0.2	L
12250 Infectious disease control	1,546	0.7	L
12281 Health education	128	0.1	L
12282 Health personnel development	89	0.0	L
130 POPULATION POLICIES/PROGRAMMES & REPROD. HEALTH	0	0.0	L
13010 Population policy and administrative management	460	0.2	L
13020 Reproductive health care	1,248	0.6	L
13030 Family planning	1,302	0.6	L
13040 STD control including HIV/AIDS	1,916	0.9	L
13081 Personnel development for population and reproductive health	1	0.0	L
140 WATER SUPPLY AND SANITATION	0	0.0	L
14010 Water resources policy and administrative management	1,218	0.5	L
14015 Water resources protection	596	0.3	L
14020 Water supply and sanitation - large systems	10,193	4.5	L
14030 Water supply and sanitation - small systems	1,663	0.7	L
14040 River development	1,035	0.5	L
14050 Waste management/disposal	624	0.3	L
14081 Education and training in water supply and sanitation	35	0.0	L
150 GOVERNMENT AND CIVIL SOCIETY	0	0.0	L
15010 Economic and development policy/Planning	6,165	2.7	L

Appendix Table 1, continued.

DAC5 (3 digit) code and CRS (5 digit) purpose codes	1997-01 commitments (USD m)	percent	Category
15020 Public sector financial management	1,589	0.7	L
15030 Legal and judicial development	1,060	0.5	L
15040 Government administration	1,457	0.6	L
15050 Strengthening civil society	2,261	1.0	L
15061 Post conflict peace-building (UN)	898	0.4	S
15062 Elections	363	0.2	L
15063 Human rights	590	0.3	L
15064 Demobilisation	1,163	0.5	S
15065 Free flow of information	82	0.0	L
15066 Land mine clearance	258	0.1	S
160 OTHER SOCIAL INFRASTRUCTURE AND SERVICES	118	0.1	L
161 Employment	0	0.0	L
16110 Employment policy and administrative management	666	0.3	L
162 Housing	1	0.0	S
16210 Housing policy and administrative management	306	0.1	L
16220 Low-cost housing	313	0.1	S
163 Other social services	1	0.0	L
16310 Social/ welfare services	3,617	1.6	L
16320 General government services	1,294	0.6	L
16330 Settlement	88	0.0	L
16340 Reconstruction relief	1,314	0.6	S
16350 Culture and recreation	1,063	0.5	L
16361 Narcotics control	2,313	1.0	L
16362 Statistical capacity building	45	0.0	L
16381 Research/scientific institutions	389	0.2	L
210 TRANSPORT AND STORAGE	0	0.0	S
21010 Transport policy and administrative management	2,283	1.0	L
21020 Road transport	15,236	6.8	S
21030 Rail transport	5,737	2.6	S
21040 Water transport	2,273	1.0	S
21050 Air transport	1,880	0.8	S
21061 Storage	13	0.0	S
21081 Education and training in transport and storage	2	0.0	L
220 COMMUNICATIONS	0	0.0	S
22010 Communications policy and administrative management	165	0.1	L
22020 Telecommunications	1,196	0.5	S
22030 Radio/television/print media	418	0.2	L
230 ENERGY GENERATION AND SUPPLY	4	0.0	S
23010 Energy policy and administrative management	2,711	1.2	L
23020 Power generation/non-renewable sources	1,697	0.8	S
23030 Power generation/renewable sources	186	0.1	S
23040 Electrical transmission/ distribution	3,678	1.6	S
23050 Gas distribution	204	0.1	S
23061 Oil-fired power plants	161	0.1	S

Appendix Table 1, continued.

DAC5 (3 digit) code and CRS (5 digit) purpose codes	1997-01 commitments (USD m)	percent	Category
23062 Gas-fired power plants	369	0.2	S
23063 Coal-fired power plants	2,224	1.0	S
23064 Nuclear power plants	962	0.4	S
23065 Hydro-electric power plants	2,393	1.1	S
23066 Geothermal energy	157	0.1	S
23067 Solar energy	232	0.1	S
23068 Wind power	185	0.1	S
23069 Ocean power	0	0.0	S
23070 Biomass	20	0.0	S
23081 Energy education/training	35	0.0	L
23082 Energy research	12	0.0	L
240 BANKING AND FINANCIAL SERVICES	2	0.0	S
24010 Financial policy and administrative management	1,534	0.7	L
24020 Monetary institutions	93	0.0	S
24030 Formal sector financial intermediaries	1,237	0.6	S
24040 Informal/semi-formal financial intermediaries	665	0.3	S
24081 Education/training in banking and financial services	3	0.0	S
250 BUSINESS AND OTHER SERVICES	0	0.0	S
25010 Business services	3,307	1.5	S
25020 Privatisation	2,032	0.9	S
311 AGRICULTURE	0	0.0	S
31110 Agricultural policy and administrative management	3,514	1.6	L
31120 Agricultural development	1,835	0.8	S
31130 Agricultural land resources	1,161	0.5	S
31140 Agricultural water resources	3,029	1.3	S
31150 Agricultural inputs	1,076	0.5	S
31161 Food crop production	490	0.2	S
31162 Industrial crops/export crops	280	0.1	S
31163 Livestock	333	0.1	S
31164 Agrarian reform	234	0.1	S
31165 Agricultural alternative development	5	0.0	L
31181 Agricultural education/training	160	0.1	L
31182 Agricultural extension	102	0.0	S
31183 Agricultural research	433	0.2	L
31184 Livestock research	26	0.0	L
31191 Agricultural services	415	0.2	S
31192 Plant and post-harvest protection and pest control	54	0.0	S
31193 Agricultural financial services	688	0.3	S
31194 Agricultural co-operatives	70	0.0	S
31195 Livestock/veterinary services	69	0.0	S
312 FORESTRY	1	0.0	S
31210 Forestry policy and administrative management	580	0.3	L
31220 Forestry development	1,140	0.5	S
31261 Fuelwood/charcoal	27	0.0	L
31281 Forestry education/training	22	0.0	L

Appendix Table 1, continued.

DAC5 (3 digit) code and CRS (5 digit) purpose codes	1997-01 commitments (USD m)	percent	Category
31282 Forestry research	34	0.0	L
31291 Forestry services	68	0.0	S
313 FISHING	0	0.0	S
31310 Fishing policy and administrative management	264	0.1	L
31320 Fishery development	353	0.2	S
31381 Fishery education/training	75	0.0	L
31382 Fishery research	56	0.0	L
31391 Fishery services	304	0.1	S
321 INDUSTRY	2	0.0	S
32110 Industrial policy and administrative management	218	0.1	L
32120 Industrial development	860	0.4	S
32130 SME development	1,289	0.6	S
32140 Cottage industries and handicraft	32	0.0	S
32161 Agro-industries	170	0.1	S
32162 Forest industries	27	0.0	S
32163 Textiles, leather and substitutes	73	0.0	S
32164 Chemicals	92	0.0	S
32165 Fertilizer plants	103	0.0	S
32166 Cement/lime/plaster	57	0.0	S
32167 Energy manufacturing	72	0.0	S
32168 Pharmaceutical production	6	0.0	S
32169 Basic metal industries	32	0.0	S
32170 Non-ferrous metal industries	4	0.0	S
32171 Engineering	12	0.0	S
32172 Transport equipment industry	82	0.0	S
32181 Technological research and development	80	0.0	L
322 MINERAL RESOURCES AND MINING	0	0.0	S
32210 Mineral/mining policy and administrative management	311	0.1	L
32220 Mineral prospection and exploration	81	0.0	S
32261 Coal	326	0.1	S
32262 Oil and gas	187	0.1	S
32263 Ferrous metals	0	0.0	S
32264 Nonferrous metals	8	0.0	S
32265 Precious metals/materials	12	0.0	S
32266 Industrial minerals	1	0.0	S
32267 Fertilizer minerals	1	0.0	S
32268 Offshore minerals	0	0.0	S
323 CONSTRUCTION	1	0.0	L
32310 Construction policy and administrative management	41	0.0	L
331 TRADE	0	0.0	S
33110 Trade policy and administrative management	346	0.2	L
33120 Wholesale/retail trade	140	0.1	S
33130 Export promotion	104	0.0	S
33140 Multilateral trade negotiations	82	0.0	L
33181 Trade education/training	5	0.0	L

Appendix Table 1, continued.

DAC5 (3 digit) code and CRS (5 digit) purpose codes	1997-01 commitments (USD m)	percent	Category
332 TOURISM	0	0.0	L
33210 Tourism policy and administrative management	208	0.1	L
400 MULTISECTOR/CROSS-CUTTING	39	0.0	L
410 General environmental protection	2	0.0	L
41010 Environmental policy and administrative management	2,399	1.1	L
41020 Biosphere protection	990	0.4	L
41030 Bio-diversity	754	0.3	L
41040 Site preservation	143	0.1	L
41050 Flood prevention/control	879	0.4	L
41081 Environmental education/ training	77	0.0	L
41082 Environmental research	64	0.0	L
420 Women in development	0	0.0	L
42010 Women in development	258	0.1	L
430 Other multisector	0	0.0	L
43010 Multisector aid	4,544	2.0	L
43020 Multisector aid for basic social services	567	0.3	L
43030 Urban development and management	2,456	1.1	L
43040 Rural development	4,753	2.1	L
43050 Non-agricultural alternative development	48	0.0	L
43081 Multisector education/training	79	0.0	L
500 COMMODITY AID AND GENERAL PROGRAMME ASSISTANCE	0	0.0	S
510 Structural Adjustment Assistance with World Bank/IMF	0	0.0	S
51010 Structural adjustment	6,145	2.7	S
520 Developmental food aid/Food security assistance	0	0.0	H
52010 Food aid/Food security programmes	7,422	3.3	H
530 Other general programme and commodity assistance	5	0.0	S
53010 Balance-of-payments support	10,144	4.5	S
53020 Budget support	1,034	0.5	S
53030 Import support (capital goods)	452	0.2	S
53040 Import support (commodities)	1,082	0.5	S
600 ACTION RELATING TO DEBT	0	0.0	S
60010 Action relating to debt	141	0.1	S
60020 Debt forgiveness	11,639	5.2	S
60030 Relief of multilateral debt	496	0.2	S
60040 Rescheduling and refinancing	4,829	2.1	S
60061 Debt for development swap	86	0.0	S
60062 Other debt swap	117	0.1	S
60063 Debt buy-back	61	0.0	S
700 EMERGENCY ASSISTANCE	392	0.2	H
710 Emergency food aid	0	0.0	H
71010 Emergency food aid	1,819	0.8	H
720 Other emergency and distress relief	0	0.0	H
72010 Emergency/distress relief	7,964	3.5	H
72020 Aid to refugees (in donor country)	823	0.4	H
72030 Aid to refugees (in recipient countries)	803	0.4	H

Appendix Table 1, continued.

DAC5 (3 digit) code and CRS (5 digit) purpose codes	1997-01 commitments (USD m)	percent	Category
910 ADMINISTRATIVE COSTS OF DONORS	0	0.0	--
91010 Administrative costs	256	0.1	--
920 SUPPORT TO NON-GOVERNMENTAL ORGANISATIONS	0	0.0	L
92010 Support to national NGOs	39	0.0	L
92020 Support to international NGOs	17	0.0	L
92030 Support to local and regional NGOs	29	0.0	L
998 UNALLOCATED/ UNSPECIFIED	5	0.0	L
99810 Sectors not specified	2,474	1.1	L
99820 Promotion of development awareness	41	0.0	--

Summary statistics, 1997-01 commitments	Humanitarian	Short impact	Long impact	Total
Total (USD m)	19,222	101,801	103,302	224,708
Percent of total	8.6	45.3	46.0	100.0

Appendix Table 2. Summary statistics for variables used in growth regressions

Variable	Obs	Mean	Std. Dev.	Min	Max
Per capita GDP growth	372	1.28	3.53	–12.74	16.49
Net ODA	372	4.77	6.28	–0.12	36.03
Gross aid	372	5.34	6.86	0.01	41.61
Short-impact aid	372	2.84	3.69	0.00	20.02
Long-impact aid	372	2.04	3.10	0.00	24.78
Humanitarian aid	372	0.46	0.84	0.00	7.67
Repayments	372	0.57	0.86	0.00	5.59
Net ODA squared	372	62.10	155.12	0.00	1297.93
Gross aid squared	372	75.34	187.34	0.00	1731.67
Short-impact aid squared	372	21.61	50.54	0.00	400.80
Long-impact aid squared	372	13.74	47.52	0.00	614.25
Humanitarian aid squared	372	0.91	4.02	0.00	58.79
Repayments squared	372	1.06	3.66	0.00	31.21
Log repayments	372	–1.54	1.44	–8.24	1.68
Log initial GDP per capita	372	7.60	0.80	5.22	9.71
East Asia	372	0.12	0.32	0.00	1.00
Tropics	372	0.70	0.46	0.00	1.00
Log initial life expectancy	372	4.09	0.18	3.55	4.35
Civil war	372	0.10	0.31	0.00	1.00
Lagged civil war	372	0.10	0.30	0.00	1.00
Institutional quality	372	4.50	1.71	0.00	10.00
Inflation	372	0.23	0.40	–0.04	3.22
Budget balance	372	–0.03	0.04	–0.31	0.15
Openness, Sachs Warner	372	0.42	0.48	0.00	1.00
Policy index	372	1.28	1.17	–5.89	3.19
Egypt	372	0.02	0.13	0.00	1.00
Arms imports	372	0.05	0.11	0.00	1.16
Land area	372	0.89	1.73	0.00	17.08
Coastline	372	0.00	0.01	0.00	0.05
Secondary school gross enrollment ratio	371	41.65	23.18	1.93	103.25
Natural resource abundance	372	15.47	11.88	0.69	74.09
Positive commodity shock	314	0.08	0.20	0.00	1.08
Negative commodity shock	314	0.05	0.14	0.00	0.78
Natural disaster	372	0.27	0.47	0.00	4.86
Short aid × log initial GDP per capita	372	19.97	25.54	0.00	141.72
Short aid × East Asia	372	0.07	0.26	0.00	2.11
Short aid × tropics	372	2.35	3.57	0.00	20.02
Short aid × log initial life expectancy	372	11.27	14.49	0.00	84.27
Short aid × civil war	372	0.39	1.78	0.00	16.15
Short aid × lagged civil war	372	0.44	2.02	0.00	20.02
Short aid × institutional quality	372	11.81	17.01	0.00	116.51
Short aid × inflation	372	0.61	2.09	–0.10	31.10
Short aid × budget balance	372	–0.13	0.28	–2.84	0.49
Short aid × openness, Sachs Warner	372	0.98	2.52	0.00	20.02
Short aid × policy index	372	3.31	6.68	–48.34	46.83
Short aid × 1982-89 dummy	372	0.83	2.21	0.00	15.53
Short aid × 1990-2001 dummy	372	1.36	3.11	0.00	20.02
Short aid squared × 1982-89 dummy	372	5.54	23.79	0.00	241.33
Short aid squared × 1990-2001 dummy	372	11.52	41.09	0.00	400.80

Appendix Table 3. Country Sample

Latin America & Carribean	*Asia*	*Eastern Europe*	*Middle East and North Africa*	*Sub-Saharan Africa*
Argentina, 1974-2001	China*, 1990-2001	Bulgaria, 1994-2001	Algeria, 1998-2001	Botswana, 1978-2001
Bolivia, 1974-2001	India, 1974-2001	Cyprus, 1978-2001	Egypt, Arab Rep., 1978-2001	Burkina Faso, 1974-2001
Brazil, 1974-2001	Indonesia*, 1974-2001	Hungary, 1994-2001	Iran, Islamic Rep., 1978-2001	Cameroon, 1978-2001
Chile, 1974-2001	South Korea*, 1974-1997	Poland, 1998-2001	Jordan, 1974-2001	Congo, Dem. Rep., 1974-2001
Colombia, 1974-2001	Malaysia*, 1974-2001	Romania, 1994-2001	Morocco, 1974-2001	Congo, Rep., 1994-2001
Costa Rica, 1974-2001	Pakistan, 1974-2001	Russian Federation, 1998-2001	Syrian Arab Republic, 1974-2001	Cote d'Ivoire, 1982-2001
Dominican Republic, 1974-2001	Papua New Guinea, 1978-2001	Turkey, 1974-2001	Tunisia, 1974-2001	Ethiopia, 1982-2001
Ecuador, 1974-2001	Philippines*, 1974-2001			Gabon, 1974-1993
El Salvador, 1974-2001	Singapore*, 1974-2001			Gambia, The, 1974-1985
Guatemala, 1974-2001	Sri Lanka, 1974-2001			Ghana, 1974-1997
Haiti, 1974-2001	Thailand*, 1974-2001			Kenya, 1974-2001
Honduras, 1974-2001				Madagascar, 1974-2001
Jamaica, 1978-2001				Malawi, 1982-1993
Mexico, 1974-2001				Mali, 1990-2001
Nicaragua, 1974-2001				Niger, 1978-1981
Paraguay, 1974-2001				Nigeria, 1974-2001
Peru, 1974-2001				Senegal, 1974-2001
Trinidad and Tobago, 1978-1997				Sierra Leone, 1974-2001
Uruguay, 1974-2001				Togo, 1978-2001
Venezuela, RB, 1974-2001				Uganda, 1982-2001
				Zambia, 1974-1997
				Zimbabwe, 1982-1997

* East Asia.