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Michael A. Clemens, Steven Radelet, Rikhil R. Bhavnani, and Samuel Bazzi

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JEL Codes: F35, O11, O19

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Working Paper 44
July 2004 (Revised 9-6-11)

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Michael Clemens et al. 2004. "Counting Chickens When They Hatch: Timing and the Effects of Aid on Growth." CGD Working Paper 44. Revised September 6, 2011. Washington, D.C.: Center for Global Development.
http://www.cgdev.org/content/publications/detail/2744

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September 6, 2011

Forthcoming in *Economic Journal*

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*We benefited greatly from extensive discussions with William Easterly, Aart Kraay, Simon Johnson, David Roodman, and Arvind Subramanian. We appreciate numerous substantive suggestions from Martin Alsop, Nancy Birdsall, François Bourguignon, William Cline, Paul Collier, Shanta Devarajan, Alan Gelb, Stephen Knack, Daniel Morrow, Peter Timmer, Nicholas Stern, Mark Sundberg, Jeremy Weinstein, Adrian Wood, four anonymous referees, and seminar participants at the Center for Global Development (CGD), the World Bank, the International Monetary Fund, American University, the U.S. Treasury, the UK Department for International Development, and the U.S. Agency for International Development. We are grateful for the assistance of Valérie Gaveau and Virginia Braunstein of the OECD. This research was generously supported by the William and Flora Hewlett Foundation and CGD. All viewpoints and any errors are the sole responsibility of the authors and do not represent CGD, its Board of Directors, or its funders.

1 Introduction

Economists have spent decades debating, without resolution, the cross-country relationship between foreign aid receipts and economic growth. Some find that aid robustly causes positive economic growth on average. Others cannot distinguish the average effect from zero. Still others find an effect only in certain countries, such as those with good policies or governance. Wearied readers of this literature would be right to wonder what produces diverse findings from apparently the same aid and growth data.

Here we show that two traits of previous research help explain why different studies reach different conclusions. Both traits relate to how these studies treat the *timing* of causal relationships between aid and growth. First, the most cited research has focused on measuring the effect of aggregate aid on contemporaneous growth, while many aid-funded projects can take a long time to influence growth. Funding for a new road might affect economic activity in short order, funding for a vaccination campaign might only affect growth decades later, and humanitarian assistance may never affect growth. Second, because current growth is likely to affect current aid, these studies require a strategy to disentangle correlation from causation. They have tended to rely on instrumental variables, but the instruments that have been used are of questionable validity and strength. When these issues are addressed, the divergence in empirical findings is greatly reduced.

We show this by stepwise altering the research design of the three most influential papers in the aid and growth literature. We hold all else constant: We begin by reconstructing their data and using precisely their regression specifications. This transparency and consistency is essential in a literature that has been alternately described as marred by aid proponents' "confirmation bias" (Easterly 2006, 48) or described as marred by aid opponents' selective reading of the empirical evidence (Hansen and Tarp 2000, 393). We avoid poor-quality instrumental variables and instead address potential biases from reverse and simultaneous causation by the more transparent methods of lagging and differencing. We test only one lag structure (the simplest) and only one disaggregation of aid, both of which were established before running the regressions and were not altered thereafter. All other aspects of the regressions remain as in the authors' original papers.

This exercise reveals that the results of all three studies change markedly 1) when we allow aid to affect growth with a time lag, 2) when first-differencing removes the effects of time-invariant omitted variables, and 3) when we consider only those portions of aid that could be intended or expected to produce growth within a few years. These steps allow us to avoid the use of poor-quality instrumental variables that have pervaded this literature. With these sensible alterations, the data reveal that over the last three decades, substantial increases in aid receipts were followed on average by small increases in investment and growth.

The fact that some amount of growth typically follows aid receipts does not per se establish causation. We discuss and test other possible explanations, but all are less plausible than aid causing some nonzero amount of growth. The results are not an artifact of dynamic panel bias, of our assumed functional form, or of mean reversion in the data. The magnitudes of the coefficients we estimate are reasonable and consistent across different specifications, with a one percentage-point increase in aid/GDP (at mean aid levels) typically being followed within several years by modest increases in investment and growth: a 0.3–0.5 percentage-point increase in investment/GDP and a 0.1–0.2 percentage-point increase in growth of real GDP per capita.

These results do not in any way suggest that aid always "works" or that large amounts of aid can be the central pillar of any given country's growth strategy. The results do suggest that the effect of aid on growth is positive on average across all countries, but is limited and quite modest in comparison with other determinants of growth, and is negative in some countries. We begin by putting these findings in broader context.

2 Four decades of diverse findings

Griffin and Enos (1970) launch this literature by reporting zero or negative bivariate correlation between aid receipts and growth in 27 countries, a finding essentially echoed by Weisskopf (1972). Papanek (1972, 1973) is the first to conduct a multivariate regression of growth on aid, in a model resembling

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

where $y_{i,t}$ is income per capita in country i at time t, $d_{i,t}^{\text{net}}$ is net disbursements of 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. He and Gulati (1978) find a significant positive partial correlation between aid and growth in 51 countries during 1950-1965, but not in the Americas.

Over (1975) and Mosley (1980) first attempt to isolate the causal portion of the aid-growth relationship with instrumental variables, in models resembling

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

where $Z_{i,t}$ is a vector of exogenous instruments, ζ is a vector of constants, and $\nu_{i,t}$ is white noise. Several related studies follow, each using different countries, years, and instruments. All are troubled by relatively short time periods and limited country samples (Gupta and Islam 1983; Mosley et al. 1987; Levy 1988).

Boone (1996) ushers in the current wave of aid-growth studies with more complete data than any predecessor. Across 96 countries, between 1971 and 1990, he finds no relationship between aid receipts and investment. He concludes that "aid programs have not ... engendered or correlated with the basic ingredients that cause ... growth." He bases this conclusion on a restricted sample of countries that eliminates those receiving the most aid; Boone's own regressions with the full sample of countries show a positive and significant relationship between aid and investment, as we discuss in detail below. In any event, an influential article in *The Economist* reporting Boone's early findings was entitled "Down the Rathole". The aid-growth literature since 1996 can be read as a series of responses to Boone's result.

2.1 The 'conditional' strand

The first strand argues that Boone fails to observe a relationship between aid and growth on average because aid only causes growth in some countries and not in others. By far the most influential of these is the work of Burnside and Dollar (2000), who find that aid causes growth only in a subset of countries that maintain low inflation, do not run large budget deficits, and are open to trade.[1] All of these studies use a model resembling

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

where $q_{i,t}$ is some country characteristic on which the effect of aid depends, and γ and δ are constants. Most of these studies fail to detect a significant unconditional effect of aid on growth, that is, when δ is constrained to zero. Easterly (2003), Easterly et al. (2004), and Roodman (2007) cast serious doubt on the conclusions of a majority of these studies. They find that the significance of the interaction coefficient δ is sensitive to influential observations and extensions of the dataset.

[1] Other work in this vein has tested whether or not the aid-growth effect is conditional on 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 2004); policy and institutional quality (Collier and Dollar 2002); institutional quality alone (Burnside and Dollar 2004); policy and warfare (Collier and Hoeffler 2004); and 'totalitarian' government (Islam 2003a).

2.2 The 'unconditional' strand

The second strand argues that Boone fails to observe a positive effect of aid due to the regression specification or time period used.[2] Many papers in this strand argue that aid can have diminishing returns (*contra* Boone) and estimate a model similar to

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

Among these the most influential published work is that of Hansen and Tarp (2001), who find a strong, nonlinear impact of instrumented aid on growth that does not depend on influential observations.[3] They instrument current aid with lagged aid, an identification strategy that rests on the persistence of aid and the absence of direct effects on current growth from lagged aid. This latter assumption in particular is questionable.[4]

[2]Here we discuss only studies that focus on growth as the outcome. Other strands of this literature investigate aid effects beyond growth, such as schooling (Dreher et al. 2008) and governance (Knack and Rahman 2007; Djankov et al. 2008).

[3]The first to allow for a nonlinear 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. Dalgaard and Hansen (2001) are the first among these to instrument for aid, and Hansen and Tarp (2000) are the first to use a Generalized Method of Moments (GMM) estimator in this literature. Lensink and White (2001) confirm the nonlinear impact in a sample of 111 countries, 1975-1992, with a 2SLS estimator. Dalgaard et al. (2004), while emphasizing 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. Other published 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. Lensink and Morrissey (2000) include an indicator of uncertainty in aid flows and extend the observation period to 25 years. They are 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. The differenced GMM results of Moreira (2005) show a highly significant, positive, nonlinear impact of aid in 48 countries from 1970 to 1998.

[4]Clemens et al. (2004) likewise find a positive, unconditional, nonlinear, causal relationship between aid and growth, in some of their regressions that instrument with lagged aid flows. Werker et al. (2009) use oil prices to instrument for aid flows from Gulf donors to predominantly Muslim aid-recipient countries. They find a positive relationship between lagged aid and growth with a coefficient of 0.22, though it is statistically significant only at the 10% level.

2.3 The 'null' strand

A third strand argues that the evidence gathered since 1990 simply confirms Boone's null result. They expand on Boone by using growth directly as the dependent variable rather than investment, and by using more extensive data. By far the most influential of these is the work of Rajan and Subramanian (2008), whose core results instrument for aid with measures of country size and political ties to donors, much as Boone did. On this null result they base the policy conclusion that "the aid apparatus will have to be rethought." Below we dissect this and the other most influential studies in this literature.

3 A way forward: Timing and identification

In this paper we explore the reasons for the divergence among these strands of literature. We find that when straightforward changes are made to the most influential papers in each strand, much of the divergence disappears. These are 1) allowing aid to affect growth with a time lag, 2) first-differencing to eliminate omitted variable bias from time-invariant unobserved traits, and 3) considering only those portions of aid that might produce growth within a few years. This allows us to avoid the use of potentially invalid and weak instrumental variables and seeking causal identification by more transparent means. Here we discuss the reasons for these choices.

3.1 The timing of aid effects

Clemens et al. (2004) argue that Boone and his successors may have failed to observe a positive effect of aggregate aid because some aid is aimed at activities whose growth impact has slim theoretical basis within the time periods used in the panel. These include flows that are not intended or used to promote expansion in generalized productive capacity (such as humanitarian assistance or disaster relief) as well as flows whose effect on overall national growth, if it ever arrives, might come long after the time period under study (such as a vaccination campaign or school feeding project).[5]

The question of when to test for growth impacts plagues the entire growth literature, not just aid-growth research. Empirical research on the determinants of growth cannot escape the selection of a fixed observation period, but "selecting the time intervals over which to study growth ... is a question that remains largely unsettled" (Temple 1999, 132). Lengthy observation periods make it possible to capture long-term growth consequences of changes in country traits, but require cross-section estimators plagued by limited degrees of freedom, reverse causation, and simultaneity bias from omitted variables.

Short periods decrease the bias from omitted variables that change slowly over time, and permit estimators with country effects (Islam 1995) to entirely remove the bias of omitted time-invariant traits. But the shorter the periods, the more "the model likely misspecifies the timing between growth and its determinants" and comes to be "dominated by measurement error" (Barro 1997, 42 and 15). Nevertheless, "[t]oo often researchers use fixed effects approaches to analyze the effects of variables ... that will affect growth only with a long lag" (Temple 1999, 132). Hypothesis tests regarding these growth determinants will suffer from low power. No consensus solution to this dilemma has emerged. Durlauf and Quah (1999) warn against short periods, while Islam (2003b, 332) agrees with Temple (1999, 113) that "the use of panels is often the best way forward".

Our approach is as follows: 1) We use short-period panel data, which allow country fixed-effects to be differenced away; 2) we allow for the possibility that the growth effect of aid arrives with a time lag; and 3) consider a subset of aid that does not include aid flows whose growth effect is most likely to arrive decades in the future, or never.

[5]Other studies before Clemens et al. (2004) consider aid disaggregated by purpose, but none with the aim of analyzing the growth impact within an appropriate time horizon. Owens and Hoddinott (1999) find that household welfare in Zimbabwe is increased by development aid (such as agricultural extension) more than by humanitarian aid (such as food aid), even in humanitarian emergencies. Mavrotas (2002) disaggregates aid into "program," "project," and "technical assistance" flows, and finds a negative correlation between growth and all three types of aid in India 1970-92.

3.2 A lack of reliable instruments

All of the major studies of aid and growth since Boone's have used instrumental variables for aid. Certainly it is important to employ some strategy for identifying the causal component of the aid-growth relationship, since any observed correlation between aid and growth could plausibly result from reverse causation or simultaneous causation by omitted variables. The use of instrumental variables is only one approach to identification, however, and this literature's search for strong, valid instrumental variables has encountered difficulties.

Here we discuss the instruments used in the most influential published studies in this literature.[6] Three of these, whose regression specifications we recreate below, are Boone, Burnside and Dollar ("BD"), and Rajan and Subramanian ("RS"). We also discuss the instruments used in a fourth published study as influential as these others, Hansen and Tarp ("HT"), and those in Clemens et al. ("CRB"), both of which use regression specifications similar to those in BD. All of these studies use as instruments some measure of political ties to donors (former colonial relationships, arms sales, and so on). The remaining instruments are primarily either related to lagged aid flows (HT, CRB) or to the size of the recipient country (Boone, BD, RS).

All of these instruments raise questions about how strong or valid we can take them to be. Lagged aid flows might well affect current growth, biasing the resulting coefficient on aid. HT and CRB find positive and statistically significant effects of aid on growth. But while both HT and CRB test for correlation between the instruments and the residuals, existing tests of this correlation have notoriously low power to reject the null of no correlation. Instrumenting for current aid with lagged aid raises the possibility that the coefficient on aid is substantially biased by invalid instruments.

[6] We measure influence by citations of all versions of each study in the Google Scholar internet search engine as of July 22, 2010, divided by the number of years since (and including) the year of publication. The four most influential studies are Burnside and Dollar (198 citations/year, 2,176 total), Rajan and Subramanian (125 citations/year, 377 total), Hansen and Tarp (60 citations/year, 597 total), and Boone (49 citations/year, 741 total).

We come now to the core regressions of Boone, BD, and RS, all of which instrument for aid primarily with some combination of political ties to donors and the size of the recipient country's population.[7] In all three studies, population size is responsible for most of the instrumentation power.

In the core results of Boone and RS, in fact, essentially all instrument strength derives from population size alone. There are two ways to see this. First, the single constructed instrument used in the core regressions of RS is almost perfectly correlated with population size. The absolute value of the correlation between the RS instrument and ln(population) is 0.93 in their 1970-2000 period and 0.95 in the 1980-2000 and 1990-2000 periods (Bazzi and Clemens 2010). The three-stage RS procedure constructs an instrument that contains almost no information beyond the size of the recipient's population, and does not materially improve on simply instrumenting with population size.

Second, and more directly, when the instruments containing population are included in the second stage regression, instrumentation power drops dramatically in Boone, BD, and RS. In Boone and RS, it collapses completely. Table 1 shows this result.[8] The first column shows a core result of Boone's with aid instrumented by population size and several variables capturing political ties. The Cragg and Donald (1993) and Kleibergen and Paap

[7]The excluded instruments in the core specification of Boone are ln(population), dummy for 'friend' of US and dummy for 'friend' of OPEC (where 'friend' equals one if the recipient receives more than 1% of the total aid budget of each donor), and dummy for 'friend' of France (defined as membership in the Franc zone). The excluded instruments in BD are ln(population), lagged arms imports/total imports, Egypt dummy, Franc zone dummy, Central America dummy, ln(initial income) $\times$ policy, ln(population) $\times$ policy, (lagged arms imports/total imports) $\times$ policy, $\left(\ln\left(\text{initial income}\right)^2\right) \times$ policy, and $\left(\ln\left(\text{population}\right)^2\right) \times$ policy (where "policy" is an index of inflation, budget balance, and openness to trade). The excluded instrument in RS is a single recipient-level variable constructed from donor-recipient measures of the following instruments: dummies for common language, dummies for current and former colonial relationships, separate dummies for former colonies of four countries (UK, France, Spain, and Portugal), the logarithm of the ratio of donor population to recipient population, and the interaction of the preceding variable with a dummy for current or past colonial relationship.

[8]The regressions of Table 1 use the original datasets of BD and RS. The original dataset used by Boone no longer exists (personal communication from the author). We meticulously reconstructed a dataset to mimic Boone's, using his sources and variable definitions. Regressions identical to Boone's published regressions using this reconstructed dataset give coefficient estimates corresponding very closely to Boone's. Our reconstruction of Boone's data is available upon request and is described in the appendix.

(2006) F-statistics are well over 4, roughly the critical value for strong instrumentation as defined by Stock and Yogo (2005).[9] The second column shows the same regression with ln(population) included in the second stage; instrumentation strength collapses. This means that the remaining instruments do not capture a sufficient degree of variance in aid flows to meaningfully remedy the bias of simply running the regression with OLS.

The next three columns of Table 1 show that the BD instrumentation strategy also relies heavily, but not completely, on population for its strength. When all three BD instruments containing population size are included in the second stage, the Cragg-Donald statistic falls from about 20 to about 7. The next column includes all 'policy' related variables in the second stage, since current growth could easily affect current budget balance or inflation, which would invalidate policy as an instrument. With these in the second stage as well, instrumentation becomes weak.

The remainder of Table 1 shows that when ln(population) is included in the second stage of Rajan and Subramanian's core cross-section regressions, using the authors' original data, instrumentation strength is gone. The Cragg-Donald statistic falls from about 32 to about 0.1. Rajan and Subramanian's other, more plausibly valid "political" instrumental variables therefore do not capture enough variance in aid for their validity to be relevant. The results in Table 1 collectively mean that Boone and RS are effectively instrumenting for aid with population size alone, and BD are resting their instrumentation strength primarily upon population size.[10]

Relying on country population as an instrument throws into serious doubt the validity of the entire instrumentation strategy, and therefore of all regression results. There are several

[9] Stock and Yogo (2005) show that a Cragg-Donald F-statistic over 4 is likely to signify instrumentation of sufficient strength that bias in the second-stage coefficient from weak instrumentation is less than 30% of the OLS bias. Kleibergen and Paap (2006) present a related F-statistic that is robust to heteroskedasticity. The Kleibergen-Paap statistic does not yet have a corresponding set of critical values, but values well below the Cragg-Donald critical values suggest that further investigation is essential.

[10] In the Rajan and Subramanian regressions that allow for a nonlinear effect of aid, instrumentation is weak even when population is not included in the second stage (Bazzi and Clemens 2010).

channels omitted from the second stages of all of these regressions through which population size has been found to directly affect growth in published research. Any one such channel would invalidate population as an instrument in these aid-growth regressions. These channels include the extent of internal and external trade (Frankel and Romer 1999), the mix of goods that a country exports (Hausmann et al. 2007), and the extent of political integration with neighbors (Spolaore and Wacziarg 2005), among others. In all of the above studies and others, population is a strong instrument for a variable that has been found to directly affect growth that is not included in any aid-growth regression. This throws strong doubt on the ability of the Boone and RS studies to test the hypotheses they seek to test.[11]

As a robustness check, RS later employ an alternative identification strategy. They use the Generalized Method of Moments (GMM) with panel data to instrument for current aid levels with lagged differences in aid and other regressors, and to instrument for current differences in aid with lagged levels of aid and other regressors. It is theoretically possible that such instruments could strongly instrument for current levels or differences in aid, but in the RS dataset they do not. Existing panel and system GMM estimators do not provide a clear way to test for weak instruments, but when analogous regressions are run using Two-Stage Least Squares, instrumentation both by lagged levels and lagged differences of the RS regressors is extremely weak, with Cragg-Donald F-statistics far below critical values for strong instruments (Bazzi and Clemens 2010). This is more serious than the aforementioned possible invalidity of lagged aid as an instrument, which is also a concern here; weak instrumentation means that the coefficient estimates in RS can be as plagued with bias as simple OLS estimates (Stock and Yogo 2005).

Many authors have searched energetically for an instrumental variable for aid that is strong and does not raise important questions about its validity. This review nevertheless suggests that readers of the most influential published aid-growth studies should not discard

[11] It also casts doubt on the BD results, though not as clearly, since BD's instruments remain marginally strong by the standard of Stock and Yogo even when population is no longer an excluded instrument.

all doubts that such an instrument has been found.

4 Method: An alternative approach to identification

Lacking an instrumental variable that we can confidently consider both strong and valid, how might causal identification proceed? We apply the following three-step method to the three most influential regression specifications in the aid-growth literature. These steps eliminate most of the major channels through which anything besides causation of growth by aid could generate a positive correlation between growth and aid.

We first re-run the core regressions in each of the studies in order to replicate their results. Holding the regression specification constant, we then lag the aid variable, and difference the results. This step controls for country fixed effects, allows for aid to have an impact on growth in the subsequent time period, and allows us to avoid relying on a potentially weak or invalid instrument to identify the impact of foreign aid on growth. Next, we explore the effects of restricting the aid variable to only those portions of aid that might be expected to affect growth within the relevant time horizon. We call this restricted aid variable "early impact" aid. Finally, we extend the time horizon for the regressions to all years of currently available data, following Easterly et al. (2004).[12]

4.1 Reconstructing data used by previous studies

Our method raises a series of technical challenges. First among these is the challenge of faithfully reconstructing the datasets used in previous studies. For the BD and RS studies this is easy; the authors have graciously made their original datasets available. The Boone analysis required more effort because the original Boone dataset no longer exists. Boone's sources and variable definitions are sufficiently clear, however, to allow us to reconstruct

[12]Because BD and HT use essentially the same data and periods, we treat them as a unit. The results below, then, are divided in three parts, exploring in turn the results of Boone, BD (and HT), and RS.

his dataset from publicly available sources. Regressions on our reconstructed dataset give coefficient estimates closely reflecting those in Boone's paper, so we feel comfortable asserting that in all three cases we are working with data that are either identical to or extremely similar to the data used by the original authors.[13]

4.2 Disaggregating aid

A second challenge is to restrict the aid variable to "early-impact" aid, which excludes those portions of aid that might not be expected to cause growth during the time period under study. If there is any ambiguity about what is being funded—notably in the case of budget-support aid—we leave it in "early impact" aid, since some or all of it *could* be spent on activities that might be expected to affect growth within a few years. The inclusion of aid that would not have such an effect would bias the coefficient on aid towards zero.

"Early impact" aid includes 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. It excludes any aid flow that clearly and primarily funds an activity whose growth effect might arrive far in the future or not at all, such as 1) all technical cooperation, 2) most social sector investments, including in education, health, population control, and water, 3) all humanitarian aid such as emergency assistance during natural disasters and food aid, and 4) donors' administrative/overhead costs and expenditures on "promotion of development awareness".[14]

4.3 Estimating 'early impact' aid

A third challenge is that for most donors in most years covered by the studies we investigate, the OECD reports purpose-disaggregated aid *commitments* but only aggregate *disburse-*

[13]The appendix compares our data with the published results from each paper.
[14]The appendix describes in detail the definition of "early-impact" aid.

ments. Most donors began reporting purpose-disaggregated commitments to the OECD in the early 1970s, but OECD data on purpose-disaggregated aid disbursements only begin for a subset of donors in 1990, and do not embrace all donors until 2002.

One approach to this problem is to use information contained in the historical purpose-disaggregated commitments to estimate purpose-disaggregated disbursements. We illustrate this method by example. To estimate "early impact" disbursements to Ghana in 1983, we begin with early-impact commitments from the United Kingdom to Ghana in 1983 and divide by total UK commitments to Ghana in 1983. The resulting ratio is multiplied by total UK disbursements to Ghana in the same year, resulting in a dollar estimate of "early impact" disbursements from UK to Ghana in 1983. The same procedure is repeated for each of Ghana's donors to achieve a separate estimate for each donor, and finally these amounts are summed across donors to yield an estimate of the total dollar amount of "early impact" aid disbursed to Ghana in 1983 by all donors. This sum is then divided by the size of the Ghanaian economy in that year. This same procedure yields our base estimate of "early impact" aid disbursements for each recipient-year.[15]

This method is theoretically attractive because it is reasonable that the share of aid disbursed for a broad category of purposes generally reflect the share of aid committed for that broad category of purposes. It is not easy to posit a model of donor behavior that would lead donors who commit their aid mostly for roads—consistently over several years—to then consistently disburse aid mostly for schools, or vice versa. It is furthermore empirically attractive

[15] The numerator for early impact aid is the product of gross ODA (Net ODA + Repayments) from OECD DAC Table 2 and the ratio of total early impact ODA commitments as classified in the appendix over total ODA commitments from the OECD CRS. This product is calculated by donor-recipient pair and then summed across all donors for a given recipient-year. The denominator in BD and RS is GDP in current USD and in Boone is GNI in current US dollars. When we include "early impact" aid in any regression, it is accompanied by a term for repayments on aid. This is because by definition, any flow of aid disaggregated by purpose is a gross flow, not a net flow, since repayments on aid are not separated by purpose. Gross repayments on aid must therefore be included as a covariate in any regression that disaggregates aid if the regression results are to be comparable to other regressions whose regressor is net aid (that is, net of repayments). When the aid variable is net aid then repayments can affect growth, so a fundamentally different regression is being run if the aid variable is a gross flow and repayments are excluded. This would not be true if there were a theoretical reason to believe that repayments on aid cannot affect growth; we see no such reason.

because in the few years where true purpose-disaggregated disbursements are available for comparison (2002-2006), the estimated fraction of disbursements in each category is strongly correlated with the true fraction. In this period, the correlation between our estimate of "early impact" disbursements as a fraction of total disbursements and true "early impact" disbursements as a fraction of total disbursements is 0.74.

4.4 True causation versus Granger causation

What remains after these corrections is a measurement that, while it does not meet the strict scientific definition of causation that would arise from a randomized experiment, does answer a question of great policy relevance: Are aid receipts followed within several years by any degree of increase in economic growth? That is, does aid exhibit Granger (1969) causation of growth? Certainly a donor interested in promoting economic growth in the recipient would want to know the answer.

It is naturally possible for non-causal mechanisms to produce Granger causality. It is possible, for example, that aid donors correctly foresee recipients' growth patterns several years into the future. But evidence suggests that growth several years into the future is highly unpredictable (Easterly et al. 1993) and that donors make large errors in forecasting recipients' growth even in the short term (Batista and Zalduendo 2004). This pathway is very doubtful. It is also possible that this pattern could arise as an artifact of mean reversion in growth combined with causation of aid by growth. That is, poor growth performance at time t could be both 1) typically accompanied by more aid at time t and 2) typically followed by better growth performance at time $t+1$, which would produce a correlation between current aid and later growth, as Roodman (2008) conjectures. This is easily tested.

5 Results

The results of the most influential aid-growth regressions in the literature would be different and remarkably uniform if two things had been different about them: first, if they had allowed aid to affect growth with a time lag and removed aid unlikely to affect growth within that window, and second, if they had used an identification strategy that does not rely on poor instrumental variables. Here we demonstrate this using faithful reconstructions of the data and regression specifications used in Boone, BD, and RS. Table 3 presents summary statistics for key variables from the three databases.

5.1 The Boone specification

Table 4 explores the effect of piecewise changes to the regressions of Boone (1996). Columns 1 and 2 show no relationship between aid and investment in Boone's OLS and 2SLS regressions, holding the sample constant (following Boone's Table 5, column 2 and Boone's Table 4, column 4, row 3). However, these regressions use Boone's "base" sample of countries, which deletes from the sample all countries that received more than 15% of GNI in aid. He does this "because it appears that beyond these levels aid is no longer fungible" (Boone 1996, 305). The elimination of these observations dramatically changed the results.

The fungibility of aid is irrelevant to testing the overall impact of aggregate aid on growth, and it is precisely less-fungible aid flows that might have the greatest impact. Dropping these observations is therefore difficult to defend. We tested to see if these observations should be eliminated as statistical outliers, and they failed that test. These observations should have been retained throughout the original analysis, thus we retain them in our remaining regressions. Column 3 shows that with the full sample, even contemporaneous aid is positively and statistically significantly correlated with investment in the OLS specification, with a similar but not statistically significant coefficient in the 2SLS specification on the same sample (column 4).[16] As Hansen and Tarp (2001) note, Boone's original paper shows that using the full sample makes the difference between significance and insignificance for aid—once the full sample is used, the aid coefficient becomes positive and statistically significant in the 2SLS regression (Boone's Table 4, column 5)—but the paper does not discuss this. In other words, Boone's research, which is often cited as showing no relationship between aid and investment, actually shows a positive and significant relationship.

If reverse causation of higher aid by lower investment were an important determinant of the aid coefficient, we would expect lagging aid to raise the coefficient on aid. Columns 5 and 6 show the effect of lagging aid by one (10-year) period; the coefficient on aid rises substantially. If simultaneous causation by omitted time-invariant country traits that depress investment and raise aid were an important determinant of the aid coefficient, we would expect first-differencing to raise the coefficient on aid. Columns 7 and 8 show the effect of first-differencing, again holding the sample constant; the coefficient on aid rises somewhat.

Columns 9 and 10 allow the aid-growth relationship to be nonlinear by including a quadratic term, as suggested by Hansen and Tarp (2001). When aid is lagged and differenced the coefficient on aid remains similar to those in prior columns but is no longer statistically significant. Finally, Column 11 replaces net ODA in column 9 with "early-impact" aid.[17] The resulting coefficients suggest a positive and statistically signficant aid-investment relationship, though with increasing rather than diminishing returns. The final row tests the hypothesis that the "turning point"—the level of aid at which marginal increases in aid are associated with no additional investment (the extremum of the parabola)—equals zero.

Table 5 repeats the analysis of Table 4 but extends Boone's data by one additional ten-year period (1991-2000). The broad pattern of coefficients on aid in the previous table

[16] Several pairs of regressions in the tables hold the sample constant, indicated by matching letters in the "Constant Sample" row of each table.

[17] The same is not possible for column 10 because lagged and differenced "early-impact" aid with Boone's 10-year periods would require purpose-disaggregated aid flows from the 1960s, which were not collected by the OECD. The coefficients on aid and aid squared in column 11 are not jointly statistically significant (F test p-value 0.44).

remains the same, ranging between roughly 0.2 and 0.5, but these become more precisely estimated in most cases. The coefficients on "early-impact" aid in columns 11 and 12 are now at the top of the range of coefficients on aggregate aid, though not statistically significant. In the last row of the table, a Wald test in column 10 shows that the "turning point" implied jointly by the linear and quadratic aid terms is positive and statistically significant at the 1% level. In column 12, the other first-differences quadratic specification, the turning point is positive and significant at the 10% level.[18] Note that these extrema, following Boone, are expressed in aid as a fraction of GNI (not as a percentage).

These results are not incompatible with Boone's important finding that large portions of aid are consumed rather than invested. Such a conclusion is sensible, partly for fungibility reasons, as Boone argues, but also partly because some aid, such as emergency food aid and disaster relief, is designed to increase consumption rather than investment and growth. Our findings are, however, incompatible with Boone's conclusion that aid receipts and investment have no relationship whatsoever. Indeed, Boone's expressed conclusion is at odds with his own finding of a positive and significant relationship when using his full sample. Boone's null conclusion appears to arise from 1) the questionable omission of all countries with the largest aid flows, and 2) a failure to identify the causal portion of the aid-investment relationship, which Boone either analyzes in simple contemporaneous correlation (in OLS regressions) or with a single strong but plausibly invalid instrumental variable (in IV regressions)—population size.

5.2 The Burnside and Dollar specification

Table 6 explores the effects of piecewise changes to the regressions of Burnside and Dollar (2000). It begins with an OLS analog of their core regression, using standard net ODA

[18] In interpreting the standard errors on these delta-method estimates of the extremum in a U-shaped relationship, Lind and Mehlum (2010) suggest using a one-sided test, thus the 10% level of significance. Note that the exact finite sample estimates based on the Fieller procedure produce qualitatively similar results to the Delta method in this case.

instead of the nonstandard measure of "Effective Development Assistance" used by Burnside and Dollar—as Burnside and Dollar (2004) themselves do in later work.[19]

Once again, if reverse causation of aid by poor contemporaneous growth outcomes were an important bias on the aid coefficient, we would expect lagging aid to substantially raise the aid coefficient. Column 2 does this, using the same sample as column 1. The coefficient does substantially rise. Likewise, if omitted country traits that attract aid and depress growth were important determinants of the aid-growth correlation, first-differencing should raise the aid coefficient. Column 4 does this, using the same sample as column 3. The aid coefficient rises markedly, though it remains statistically insignificant.

Because all of these regressions include initial GDP per capita, they are vulnerable to the well-known problem of dynamic panel bias (Nickell 1981). Column 5 instruments for the contemporaneous difference in initial GDP per capita with the once-lagged difference in initial GDP/capita (Anderson and Hsiao 1982).[20] This Anderson-Hsiao instrumentation of initial GDP/capita is uniformly strong by the criteria of Stock and Yogo. Aid remains uninstrumented throughout.

[19] Burnside and Dollar (2000) are unique in the literature in using "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 debatable. With respect to EDA: In a Ricardian world of perfect foresight and perfect credit markets, the fact that a road is built with a loan that must be repaid decades into the future is relevant to the capacity of that road to produce growth within a four-year period. But such a model is irrelevant to how most developing-country consumers using the road make decisions. We prefer, along with almost all aid-growth studies, to measure aid as ODA. Their denominator of PPP GDP, also rare in the aid-growth literature, can be justified theoretically under the assumption that most aid is spent on goods and services with nontradable local substitutes. Suppose 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.

[20] We avoid the more efficient but much more complex difference-GMM and system-GMM estimators here because 1) those estimators do not allow us to assess the strength of instrumentation (Bazzi and Clemens 2010) while the Anderson-Hsiao estimator does, 2) the system-GMM estimator is now known to generate biased coefficients in many applied settings (Roodman 2009; Mehrhoff 2009), and 3) the Anderson-Hsiao estimator does substantially eliminate dynamic panel bias in expectation even if it is less efficient for statistical inference than more complex estimators.

Columns 6-8 show that when a quadratic aid term is added to columns 2, 4, and 5, allowing aid to have a nonlinear relationship with growth, the coefficient on aid rises sharply and approaches but does not quite attain statistical significance.[21] Finally, columns 9-11 repeat the preceding columns with early-impact aid. The coefficients rise further and are significant at the 10% level in two of these columns. The "turning point" at which the positive aid-growth association becomes zero—now expressed, as in Burnside and Dollar, as a percentage of GDP—is positive and statistically significant at the 5% level in columns 6–10 and at the 10% level in column 11.[22]

Table 7 extends the Burnside and Dollar database by three four-year periods (1994-7, 1998-2001, and 2002-5), following Easterly et al. (2004) who extend it by two periods. The results are broadly similar to those in the preceding table: The coefficient on the linear portion of aid is in the range 0.15-0.40 when aid is allowed to have a nonlinear relationship with growth. In half of these regressions the coefficient is significant at the 5% or 10% level. Lagging and differencing raises the aid coefficient substantially. The "turning point" is positive in columns 6–11, but only statistically significant in columns 6–8 (using net ODA).

In the Burnside and Dollar data and design, then, aid receipts are followed on average by increases in growth for the average recipient when a nonlinear relationship is allowed for. This pattern is not statistically precise in many of the specifications, but it is either precise or close to precise in most of them, and the estimated coefficients hover within an unchanging range close to 0.2–0.3. This result is not sensitive to updating the database to the most recent available data. As a whole, Tables 6 and 7 suggest that the null result of Burnside and Dollar in column 1 can be attributed to 1) the imposition of a strictly linear effect of aid (Hansen and Tarp 2001) and 2) a failure to identify the causal portion of the aid-growth relationship, either by testing contemporaneous correlations (OLS) or using

[21] Hansen and Tarp (2001) also find that allowing for a nonlinear relationship in BD greatly alters the results.

[22] In one of the six regressions it is only significant at the 10% level, which passes the bar for statistical significance in the one-sided test recommended by Lind and Mehlum (2010) in this setting.

potentially invalid instruments (IV). It does not arise from the limited range of years they use.

5.3 The Rajan and Subramanian specification

Table 8 is identical to Table 6 but uses the regression specification, data, and periods used in the panel regressions of Rajan and Subramanian (2008). The results are quite different. The coefficient on aid still rises when aid is lagged, and rises when the regressions are run in first-difference, as expected if a downward endogeneity bias on the aid coefficient is present. But the coefficient on aid is roughly zero in most columns, and is never statistically significant and positive, though the aid "turning point" is positive and statistically significant in column 11—in first differences, using lagged "early-impact" aid.

Why this difference? The reason becomes clear in Table 9, which extends the Rajan and Subramanian database by three additional periods–two backward in time (1970-4 and 1975-9) and one forward (2001-2005).[23] Now, the pattern of the coefficients on aid broadly resembles that in the Burnside and Dollar specification of Tables 6 and 7. The aid "turning point" jointly implied by the linear and quadratic aid terms is positive and statistically significant at the 5% level in four of the six nonlinear specifications, and significant at the 10% level in the other two.[24] The key difference is data coverage. The original Rajan and Subramanian panel regressions omit the 1970s, that is, one third of the data then available.

It makes sense that consideration of a broader range of years would affect the result.

[23]Extending the sample in this fashion is impossible using precisely RS's variables, for the sole reason that the time-variant measure of institutional quality they use begins in 1981. Including this variable thus forces them to throw away one third of the available data on all other regressors and the dependent variable. Rather than do this, BD use a time-invariant version of the same measure of institutional quality: the rating for the early 1980s, held constant. Thus Table 9 uses the time-invariant institutional variable, allowing analysis of over 50% more datapoints than if the time-variant version is used. This is innocuous for two reasons: 1) the large majority of variance in the institutional quality variable is across countries, not within countries, and 2) RS conclude that the relationship between aid and growth does not depend on whether or not countries have "good policies and institutions". If this is correct, then even dropping the institutional quality variable from the regression entirely would not affect the partial aid-growth relationship.

[24]Lind and Mehlum (2010) recommend a one-sided test—thus the 10% level of significance—in this setting.

The debt crisis of the 1980s, the crisis of the Heavily Indebted Poor Countries (HIPC) in the 1990s, and destabilization following the end of the Cold War in the 1990s were times of generally poor growth in developing countries, and the original Rajan and Subramanian panel regressions treat only those years. Conditions were more favorable in the 1970s and the early 2000s, years included in the regressions of Table 9. When these years are included in the sample, the pattern of coefficients on aid in the Rajan and Subramanian specifications (Table 9) does not materially differ from the pattern in the Burnside and Dollar specifications (Table 7). These show an aid coefficient roughly in the range 0.15-0.40.

The results in Table 9 are not necessarily incompatible with Rajan and Subramanian's conclusion that "the predicted positive effects of aid inflows on growth are likely to be smaller than suggested by advocates". While some advocates claim a large relationship, the results in this and other tables herein show a relationship that is modest rather than large. These results do not, however, offer grounds for Rajan and Subramanian's strong conclusion that aid and growth have no detectable relationship whatsoever or that the cross-country data may be purely "noise". They suggest that Rajan and Subramanian's null result might be attributable to 1) the limited sample of years in their panel regressions, 2) their restriction in most regressions that the aid effect be linear,[25] 3) a failure to identify the causal portion of the aid-growth relationship in their panel regressions due to weak instruments, and 4) a failure to identify the causal portion of the aid-growth relationship in their cross-section regressions, due to reliance on a single instrument (population size) that is weak in nonlinear specifications and plausibly invalid in all specifications.[26]

[25]As mentioned above, instrumentation is very weak in all of Rajan and Subramanian's published regressions that allow for a nonlinear effect (Bazzi and Clemens 2010). In two of their panel regressions where aid is instrumented RS do include a squared term, but this only aggravates the problem of instrument weakness.

[26]Arndt et al. (2010) corroborate some of these issues with the Rajan and Subramanian specification, and discuss others.

5.4 Magnitude and diminishing returns

The coefficient estimates in the differenced regressions with lagged aid in Tables 4 to 9 fall in a broad but uniform positive range. The typical coefficients collectively imply that—at mean aid levels—a one percentage-point increase in Aid/GDP is typically followed several years later by a modest increase in annual average real GDP per capita growth of 0.1–0.2 percentage points, and by a modest increase in average Investment/GDP of 0.3–0.5 percentage points.[27]

There is substantial evidence of decreasing returns in this relationship, so that returns may be much lower at high levels of aid. Typical coefficients in the tables suggest an inflection point in the average aid-growth and aid-investment relationships when aid exceeds roughly 15-25% of GDP.[28]

6 Robustness

Here we briefly discuss a series of checks to ensure that the results in Tables 4 to 9 are not spuriously generated by mean reversion, by the method of estimating "early-impact" aid disbursements, or by assumptions on the functional form of the aid-growth relationship.[29]

6.1 Mean reversion and reverse causation

Roodman (2008) conjectures that a positive relationship between lagged aid and current growth could spuriously arise if 1) countries with poor growth outcomes in the recent past

[27] For example, the coefficients in Table 7 column 6 and the mean Aid/GDP of 5.5% in Table 3 suggest that an additional percentage point in aid is associated with a change in growth the following period of $\left((0.165 \times 6.5 + (-0.004) \times 6.5^2\right) - \left((0.165 \times 5.5 + (-0.004) \times 5.5^2\right) = +0.117$. The same calculation with other representative coefficients yields +0.265 (Table 7 column 7), +0.206 (Table 7 column 8), and +0.187 (Table 9 column 7). A corresponding calculation for investment (noting that the Boone data are in fractions rather than percent) yields +0.310 (Table 5 column 8) and +0.522 (Table 5 column 7).

[28] For example, the coefficients in Table 7 column 6 suggest an inflection point of $(-1 \times -0.165)/(-0.004 \times 2) = 20.6$. The same calculation with other representative coefficients yields 22.6 (Table 7 column 7), 17.4 (Table 7 column 8), 24.7 (Table 9 column 7), and 27.3 (Table 5 column 10).

[29] In results not reported here, we tested the robustness of the findings to the use of the "supply-side" instrumental variables proposed by Tavares (2003). Although those instruments are often weak in the quadratic specifications used here according to the criterion of Stock and Yogo (2005), in regressions where the instruments are strong the results remain essentially similar.

have better current growth outcomes, and vice versa, and 2) poor growth is followed by greater aid. In such a situation, if the timing of these two phenomena were suitably arranged, aid would be followed by growth simply because growth later rebounds from the poor growth that caused the aid, not because aid causes growth.

Such a mechanism is plausible, but it cannot generate the results presented here. Table 10 takes representative regressions from Table 9 and includes twice-lagged growth as a control variable. If the correlation between current growth and lagged aid arose primarily because aid flows in the past were being caused by poor growth outcomes before them, then controlling for twice-lagged growth would substantially change the results. Including this variable does not substantially change the magnitude of the coefficient estimates.[30]

6.2 'Early impact' aid estimation

As discussed above, the subset of the regressions presented here that use "early impact" aid cannot directly use "early impact" disbursements because the OECD data do not contain purpose-disaggregated disbursements for most of the years in question. For this reason we estimate purpose-disaggregated disbursements using the method outlined in subsection 4.3. Because this method involves using a variable not directly obtained from the OECD data, it is useful to check the robustness of the results to use of an alternative measure.

Table 11 shows the effect of replacing estimated *disbursements* of "early-impact" aid with raw data on *commitments* of "early-impact" aid from the OECD data. This makes the variable more transparent, at the cost of greater measurement error, since it does not make use of historical information on *total* aid disbursements for each donor-recipient pair, as our estimated "early impact" disbursements variable does. At any rate, this replacement causes no substantial difference in the representative regressions shown in Table 11. We performed

[30]We carried out the same exercise with all regressions in the paper and the same is true throughout. Karras (2006) likewise finds that the aid-growth relationship in time series is not sensitive to controlling for lagged growth.

the same check on all other regressions using "early-impact" aid with generally the same result.[31]

6.3 Influential observations and functional form

Dalgaard and Hansen (2001), Easterly et al. (2004), and Roodman (2007) raise the concern that the results of some aid-growth regressions are driven by a few influential observations that swing the regression line. Roodman (2008) furthermore speculates that many aid-growth regressions including a squared term to capture nonlinear effects of aid may generate spurious coefficient estimates due to collinearity between the squared and linear terms.

Both of these concerns can be directly addressed with semiparametric methods. First, different intervals of the support of conditional aid can be analyzed separately, so that influential observations in one interval need not affect patterns detected in other intervals. This is preferable to the controversial practice of selectively deleting observations from the data to test for sensitivity to influential observations (Chatterjee et al. 2000, 108). Second, semiparametric analysis makes no assumptions about the functional form of the partial aid-growth relationship (conditional on the retained assumption of a linear relationship between growth and the other covariates). If semiparametric analysis of the partial aid-growth relationship reveals an upward slope across substantial portions of the data, then neither influential observations nor the quadratic functional form assumption could be generating the statistically significant coefficient estimates in the preceding tables.

Figure 1 displays a scatterplot of the partial aid-growth relationship in one representative lagged differenced regression from Table 7, column 7. In the original parametric regression, aid has a statistically significant quadratic relationship with growth. The plot in Figure 1

[31] In the process of testing separately for effects of aggregate aid and of purpose-delimited aid, we implicitly assume that aid is not perfectly fungible. A reasonable conclusion from the aid fungibility literature—surveyed in Devarajan and Swaroop (2000) 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 aid appears to vary by sector (e.g Feyzioğlu et al. 1998) and by country, the literature does not find aid to be fully fungible.

partials out the same non-aid covariates from growth (vertical axis) and aid (horizontal axis). The figure shows that the positive partial correlation in the parametric regression result is not produced by one or two influential observations, and is not generated spuriously by the assumption of a quadratic partial relationship between aid and growth.

7 Conclusions

These results imply that straightforward changes to the research designs of the most cited papers in the aid-growth literature move us closer to resolving the divergence between their findings. There is one broad finding from the regression specifications used in all of these studies: Aid inflows are systematically associated with modest, positive subsequent growth in cross-country panel data. The principal reasons that other studies have not observed this relationship are that they tested for aid effects within an inappropriate time horizon, relied too much on weak or invalid instrumental variables, and looked at historical time series that were too short.

Most of the substantial disagreements in the literature's most influential studies disappear when aid is allowed to affect growth with a lag, when only portions of aid relevant to short-term growth are tested for short-term growth effects, and when the historical time series under observation is extended to include all available data. This finding does not depend on assumptions about the functional form of the aid-growth relationship, does not arise from a handful of influential observations, and is not an artifact of mean reversion.

Clearly, the fact that increases in aid are typically followed by increases in growth is a necessary but not sufficient condition to demonstrate scientifically that aid *causes* growth. There are related debates about the direction of causality between investment and growth, savings and growth, health outcomes and growth, and institutions and growth, to name a few. In other words, Granger causality does not strictly imply true causality. As we point out, however, the aid-growth literature does not currently possess a strong and patently valid instrumental variable with which to reliably test the hypothesis that aid strictly causes growth, raising significant doubts about the conclusions of the studies that have relied on instrumentation. The most plausible explanation for the fact that aid increases are systematically followed by growth increases on average is that aid does cause modest positive increases in growth on average. There is little empirical support for the notion that aid systematically reduces growth (Temple 2010).

The results do not by any means imply that aid "works" everywhere, or even in the median country. First, even if "working" is taken to mean contributing to economic growth, a universal trait of aid-growth analyses (and growth studies more broadly) is that a very large number of countries lie well above and well below the regression line; it is clear that in many countries, even large aid inflows have been insufficient to spark growth over any time horizon. Second, there are many other metrics against which aid could be judged to "work" even if there were no growth impact; the aid money that supported the smallpox eradication campaign accomplished its goal, whether or not that campaign's success will ever be felt in the national accounts. Finally, aid appears to have a nonlinear effect on growth, and there may be limits on the degree to which even large aid receipts can further increase growth in the typical recipient (Gupta and Heller 2002).

These results do not suggest that aid can or should be the main driver of growth. As Kraay (2006) points out, far more of the variance in growth across countries is accounted for by the non-aid covariates in these regressions than by the aid variable. Many important growth successes across the developing world have been accomplished with relatively little foreign aid, such as in post-Mao China and post-renovation (*Đổi Mới*) Vietnam. But the findings do suggest that on average—over all countries, over many decades, and regardless of the regression specification—aid has had a modest positive effect on growth.

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Table 1: **Previous studies rely on population size for instrument strength**

	Boone		Burnside and Dollar			Rajan and Subramanian	
Population IVs in 2nd stage[a]	—	Yes	—	Yes	Yes	—	Yes
Policy IVs in 2nd stage[b]	—	—	—	—	Yes	—	—
Cragg-Donald F-stat	15.70	1.77	19.74	7.04	4.65	31.63	0.13
Kleibergen-Paap F-stat	7.57	1.69	15.76	5.90	2.77	36.12	0.07
Kleibergen-Paap LM stat p-val.	< 0.01	0.20	< 0.01	0.06	0.12	< 0.01	0.77
Observations	132	132	275	275	275	78	78
Years	1971-1990		1970-1993			1970-2000	

[a] "Population IVs in 2nd stage" means that the ln(population) instrument is included in the second stage in the Boone regression and in the Rajan and Subramanian regression. In the Burnside and Dollar regression it means that all three instruments containing population are included in the 2nd stage; these are ln(population), ln(population) $\times$ 'policy', and $(\ln(\text{population}))^2$ $\times$ 'policy'.

[b] "Policy IVs in 2nd stage" means that all instruments containing 'policy' are included in the second stage: These are ln(initial income) $\times$ policy, $(\ln(\text{initial income})^2)$ $\times$ policy, and (lagged arms imports/total imports) $\times$ policy. The regressions replicated and modified are Boone (1996) Table 4, column V, row 3; Burnside and Dollar (2000) Table 4, column 3 2SLS; and Rajan and Subramanian (2008) Table 4A, column 2.

Table 2: **Covariates used in the most influential aid studies**

Boone	Burnside and Dollar	Rajan and Subramanian
Log GNI relative to OECD	Initial GDP/capita	Initial GDP/capita
(Log GNI relative to OECD)^2	Ethnic Fractionalization	Initial Policy
GNI/capita Growth	Assassinations	Log Initial Life Expectancy
Population Growth	Ethnic Frac. $\times$ Assassinations	Geography
Terms of Trade	Sub-Saharan Africa	Institutional Quality
Debt Rescheduling	East Asia	Log Inflation
Sub-Saharan Africa	Institutional Quality	Initial M2/GDP
Asia	M2/GDP, Lagged	Budget Balance/GDP
Latin America/Caribbean	Policy	Revolutions
Period dummies	Period dummies	Ethnic Fractionalization
Constant	Constant	Sub-Saharan Africa
		East Asia
		Constant

Definitions of variables can be found in the appendix.

Table 3: **Summary statistics for aid, growth, and investment**

Dataset	Years	Variable	N	mean	std. dev.	min	max
Boone	1971-1991	Investment/GNI	154	0.23	0.07	0.09	0.43
		Net ODA/GNI	155	0.06	0.08	0.00	0.50
	1971-2001	Investment/GNI	252	0.23	0.08	0.08	0.50
		Net ODA/GNI	254	0.07	0.09	−0.001	0.52
Burnside and Dollar	1970-1993	Growth	329	1.17	3.76	−12.96	18.79
		Net ODA/GDP	330	5.45	7.95	−0.03	53.82
	1970-2005	Growth	503	1.34	3.40	−12.96	18.79
		Net ODA/GDP	495	5.52	7.88	−0.13	53.82
Rajan and Subramanian	1980-2000	Growth	278	0.98	3.44	−11.52	19.92
		Net ODA/GDP	281	5.51	8.12	0.005	50.56
	1970-2005	Growth	491	1.62	3.28	−12.30	13.12
		Net ODA/GDP	405	5.12	7.45	0.005	50.56

Further detail on the databases are in the appendix.

Table 4: Boone **specification, original years (1971-1990)**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Sample	Base	Base	Full	Full	Full	Full
Estimator	OLS	2SLS	OLS	2SLS	OLS	OLS
Aid Lagged?	—	—	—	—	—	Yes
First Difference?	—	—	—	—	—	—
Early-impact ODA?	—	—	—	—	—	—
Constant Sample	a	a	b	b	c	c
Aid/GDP	−0.126	−0.093	0.287***	0.235	0.365***	0.418***
	(0.225)	(0.355)	(0.100)	(0.198)	(0.119)	(0.156)
Observations	116	116	132	132	126	126

	(7)	**(8)**	**(9)**	**(10)**	**(11)**
Sample	Full	Full	Full	Full	Full
Estimator	OLS	OLS	OLS	OLS	OLS
Aid Lagged?	Yes	Yes	Yes	Yes	Yes
First Difference?	—	Yes	—	Yes	—
Early-impact ODA?	—	—	—	—	Yes
Constant Sample	d	d	e	f	g
Aid/GDP	0.336***	0.357***	0.079	0.335	−1.226
	(0.123)	(0.105)	(0.452)	(0.394)	(1.091)
$(\text{Aid/GDP})^2$			1.621	0.100	24.170***
			(1.829)	(1.631)	(8.743)
Observations	56	56	126	56	68
Turning point, in Aid/GNI†			−0.024	−1.671	0.025*
			(0.166)	(29.095)	(0.015)

Dependent variable is Investment/GNI, standard errors in parentheses. All regressions use reconstruction of Boone database for Boone's original years and include non-aid covariates identical to Boone's. "Base" sample drops countries that received more than 15% of GNI in aid, "full" sample includes them. All regressions that use "early-impact" ODA also include Repayments/GDP and $(\text{Repayments/GDP})^2$, since "early-impact" aid is a gross flow and aggregate ODA is a net flow. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. †Shows a Wald test (delta method) of the hypothesis that $\overline{A} = 0$, where $\overline{A} = \text{Aid}|d(\text{Investment})/d(\text{Aid}) = 0$, i.e. $H_0 : -\beta_{\text{Aid}}/2\beta_{\text{Aid}^2} = 0$.

Table 5: Boone **specification, extended years (1971-2000)**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Sample	Base	Base	Full	Full	Full	Full
Estimator	OLS	2SLS	OLS	2SLS	OLS	OLS
Aid Lagged?	—	—	—	—	—	Yes
First Difference?	—	—	—	—	—	—
Early-impact ODA?	—	—	—	—	—	—
Constant Sample	a	a	b	b	c	c
Aid/GDP	0.230*	0.209	0.283***	0.279*	0.308***	0.310***
	(0.131)	(0.248)	(0.064)	(0.161)	(0.065)	(0.078)
Observations	198	198	214	214	207	207
	(7)	**(8)**	**(9)**	**(10)**	**(11)**	**(12)**
Sample	Full	Full	Full	Full	Full	Full
Estimator	OLS	OLS	OLS	OLS	OLS	OLS
Aid Lagged?	Yes	Yes	Yes	Yes	Yes	Yes
First Difference?	—	Yes	—	Yes	—	Yes
Early-impact ODA?	—	—	—	—	Yes	Yes
Constant Sample	d	d	e	f	g	h
Aid/GDP	0.340***	0.183**	0.435**	0.553***	0.577	0.615
	(0.070)	(0.081)	(0.201)	(0.144)	(0.537)	(0.516)
$(\text{Aid/GDP})^2$			−0.340	−1.011***	0.559	−3.659**
			(0.419)	(0.251)	(2.089)	(1.822)
Observations	130	130	208	131	147	71
Turning point, in Aid/GNI†			0.088	0.274***	−0.517	0.084*
			(0.149)	(0.027)	(2.401)	(0.048)

Dependent variable is Investment/GNI, standard errors in parentheses. All regressions use reconstruction of Boone database extending 10 additional years to 2000, and include non-aid covariates identical to Boone's. "Base" sample drops countries that received more than 15% of GNI in aid, "full" sample includes them. All regressions that use "early-impact" ODA also include Repayments/GDP and $(\text{Repayments/GDP})^2$, since "early-impact" aid is a gross flow and aggregate ODA is a net flow. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. †Shows a Wald test (delta method) of the hypothesis that $\overline{A} = 0$, where $\overline{A} = \text{Aid}|d(\text{Investment})/d(\text{Aid}) = 0$, i.e. $H_0 : -\beta_{\text{Aid}}/2\beta_{\text{Aid}^2} = 0$.

Table 6: Burnside and Dollar **specification, original years (1970-1993)**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Estimator	OLS	OLS	OLS	OLS	A-H
Aid Lagged?	—	Yes	—	Yes	Yes
First Difference?	—	—	—	Yes	Yes
Early-impact ODA?	—	—	—	—	—
Constant Sample	a	a	b	b	b
Aid/GDP	0.004	0.062	−0.024	0.104	0.097
	(0.035)	(0.069)	(0.029)	(0.111)	(0.102)
Observations	273	273	216	216	216

	(6)	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
Estimator	OLS	OLS	A-H	OLS	OLS	A-H
Aid Lagged?	Yes	Yes	Yes	Yes	Yes	Yes
First Difference?	—	Yes	Yes	—	Yes	Yes
Early-impact ODA?	—	—	—	Yes	Yes	Yes
Constant Sample	c	b	b	d	e	e
Aid/GDP	0.321***	0.387*	0.407*	0.546*	0.790*	0.742
	(0.103)	(0.234)	(0.226)	(0.281)	(0.465)	(0.575)
$(\text{Aid/GDP})^2$	−0.009***	−0.009*	−0.009*	−0.056***	−0.067**	−0.066**
	(0.002)	(0.005)	(0.005)	(0.017)	(0.028)	(0.028)
Observations	273	216	216	232	175	175
Turning point, Aid as % of GDP†	17.21***	21.21***	21.68***	4.86***	5.94***	5.64*
	(2.06)	(5.01)	(4.97)	(1.47)	(2.10)	(3.06)

Dependent variable is growth of real GDP per capita, standard errors in parentheses. "A-H" means Anderson-Hsiao estimator with the contemporaneous difference in initial GDP/capita instrumented by the once-lagged difference in initial GDP/capita. Aid is *not* instrumented in any regression. All regressions use original Burnside and Dollar database and include non-aid covariates identical to Burnside and Dollar's. All regressions that use "early-impact" ODA also include Repayments/GDP and $(\text{Repayments/GDP})^2$, since "early-impact" aid is a gross flow and aggregate ODA is a net flow. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. †Shows a Wald test (delta method) of the hypothesis that $\overline{A} = 0$, where $\overline{A} = \text{Aid}|d(\text{Growth})/d(\text{Aid}) = 0$, i.e. $H_0 : -\beta_{\text{Aid}}/2\beta_{\text{Aid}^2} = 0$.

Table 7: **Burnside and Dollar specification, extended years (1970-2005)**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Estimator	OLS	OLS	OLS	OLS	A-H
Aid Lagged?	—	Yes	—	Yes	Yes
First Difference?	—	—	—	Yes	Yes
Early-impact ODA?	—	—	—	—	—
Constant Sample	a	a	b	b	c
Aid/GDP	0.005	0.045	−0.012	0.096	0.004
	(0.032)	(0.036)	(0.034)	(0.080)	(0.087)
Observations	418	418	361	361	358

	(6)	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
Estimator	OLS	OLS	A-H	OLS	OLS	A-H
Aid Lagged?	Yes	Yes	Yes	Yes	Yes	Yes
First Difference?	—	Yes	Yes	—	Yes	Yes
Early-impact ODA?	—	—	—	Yes	Yes	Yes
Constant Sample	d	b	c	e	f	f
Aid/GDP	0.165**	0.361**	0.314	0.147	0.460*	0.324
	(0.078)	(0.181)	(0.203)	(0.167)	(0.234)	(0.316)
$(\text{Aid/GDP})^2$	−0.004**	−0.008**	−0.009*	−0.012	−0.030**	−0.036*
	(0.002)	(0.004)	(0.005)	(0.010)	(0.012)	(0.020)
Observations	418	361	358	380	323	323
Turning point, Aid as % of GDP†	18.59***	23.21***	18.01***	47.84	26.39	4.55
	(2.66)	(3.88)	(4.72)	(510.8)	(150.2)	(3.15)

Dependent variable is growth of real GDP per capita, standard errors in parentheses. "A-H" means Anderson-Hsiao estimator with the contemporaneous difference in initial GDP/capita instrumented by the once-lagged difference in initial GDP/capita. Aid is *not* instrumented in any regression. All regressions use original Burnside and Dollar database extended by three additional four-year periods to 2005, and include non-aid covariates identical to Burnside and Dollar's. All regressions that use "early-impact" ODA also include Repayments/GDP and $(\text{Repayments/GDP})^2$, since "early-impact" aid is a gross flow and aggregate ODA is a net flow. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. †Shows a Wald test (delta method) of the hypothesis that $\overline{A} = 0$, where $\overline{A} = \text{Aid}|d(\text{Growth})/d(\text{Aid}) = 0$, i.e. $H_0 : -\beta_{\text{Aid}}/2\beta_{\text{Aid}^2} = 0$.

Table 8: Rajan and Subramanian **specification, original years (1981-2000)**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Estimator	OLS	OLS	OLS	OLS	A-H
Aid Lagged?	—	Yes	—	Yes	Yes
First Difference?	—	—	—	Yes	Yes
Early-impact ODA?	—	—	—	—	—
Constant Sample	a	a	b	b	c
Aid/GDP	−0.038	−0.019	−0.128***	−0.019	0.158
	(0.048)	(0.052)	(0.035)	(0.093)	(0.193)
Observations	236	236	164	164	161

	(6)	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
Estimator	OLS	OLS	A-H	OLS	OLS	A-H
Aid Lagged?	Yes	Yes	Yes	Yes	Yes	Yes
First Difference?	—	Yes	Yes	—	Yes	Yes
Early-impact ODA?	—	—	—	Yes	Yes	Yes
Constant Sample	d	b	c	e	f	f
Aid/GDP	−0.071	−0.098	0.139	−0.011	−0.186	1.907
	(0.113)	(0.159)	(0.280)	(0.319)	(0.428)	(2.503)
$(\text{Aid/GDP})^2$	0.002	0.003	0.001	−0.004	−0.005	−0.167
	(0.004)	(0.006)	(0.006)	(0.030)	(0.035)	(0.192)
Observations	236	164	161	226	159	158
Turning point, Aid as % of GDP†	16.59	52.69	−99.96	−1.27	−20.38	5.72***
	(15.64)	(95.82)	(1078.6)	(44.69)	(197.7)	(1.25)

Dependent variable is growth of real GDP per capita, standard errors in parentheses. "A-H" means Anderson-Hsiao estimator with the contemporaneous difference in initial GDP/capita instrumented by the once-lagged difference in initial GDP/capita. Aid is *not* instrumented in any regression. All regressions use original Rajan and Subramanian database, and include non-aid covariates identical to Rajan and Subramanian's. All regressions that use "early-impact" ODA also include Repayments/GDP and $(\text{Repayments/GDP})^2$, since "early-impact" aid is a gross flow and aggregate ODA is a net flow. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. †Shows a Wald test (delta method) of the hypothesis that $\overline{A} = 0$, where $\overline{A} = \text{Aid}|d(\text{Growth})/d(\text{Aid}) = 0$, i.e. $H_0 : -\beta_{\text{Aid}}/2\beta_{\text{Aid}^2} = 0$.

Table 9: Rajan and Subramanian **specification, extended years (1971-2005)**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	
Estimator	OLS	OLS	OLS	OLS	A-H	
Aid Lagged?	—	Yes	—	Yes	Yes	
First Difference?	—	—	—	Yes	Yes	
Early-impact ODA?	—	—	—	—	—	
Constant Sample	a	a	b	b	c	
Aid/GDP	−0.009	0.023	−0.031	0.109	0.058	
	(0.035)	(0.044)	(0.034)	(0.083)	(0.091)	
Observations	404	404	323	323	322	
	(6)	**(7)**	**(8)**	**(9)**	**(10)**	**(11)**
Estimator	OLS	OLS	A-H	OLS	OLS	A-H
Aid Lagged?	Yes	Yes	Yes	Yes	Yes	Yes
First Difference?	—	Yes	Yes	—	Yes	Yes
Early-impact ODA?	—	—	—	Yes	Yes	Yes
Constant Sample	d	b	c	e	f	f
Aid/GDP	0.106	0.247	0.191	0.380**	0.675**	0.513
	(0.079)	(0.152)	(0.166)	(0.169)	(0.265)	(0.384)
$(\text{Aid/GDP})^2$	−0.003	−0.005	−0.004	−0.022	−0.052**	−0.042
	(0.003)	(0.004)	(0.004)	(0.015)	(0.023)	(0.029)
Observations	404	323	322	343	268	268
Turning point, Aid as % of GDP†	16.13**	27.77*	22.58*	8.46***	6.95***	6.05***
	(6.85)	(16.16)	(11.63)	(3.15)	(1.95)	(1.79)

Dependent variable is growth of real GDP per capita, standard errors in parentheses. "A-H" means Anderson-Hsiao estimator with the contemporaneous difference in initial GDP/capita instrumented by the once-lagged difference in initial GDP/capita. Aid is *not* instrumented in any regression. All regressions use original Rajan and Subramanian database extended by three additional five-year periods—back to 1971 and forward to 2005—and include non-aid covariates identical to Rajan and Subramanian's. All regressions that use "early-impact" ODA also include Repayments/GDP and $(\text{Repayments/GDP})^2$, since "early-impact" aid is a gross flow and aggregate ODA is a net flow. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. †Shows a Wald test (delta method) of the hypothesis that $\overline{A} = 0$, where $\overline{A} = \text{Aid}|d(\text{Growth})/d(\text{Aid}) = 0$, i.e. $H_0 : -\beta_{\text{Aid}}/2\beta_{\text{Aid}^2} = 0$.

Table 10: **Robustness check: Include twice-lagged growth**

	(1)	**(2)**	**(3)**	**(4)**
Estimator	OLS	OLS	OLS	OLS
Aid Lagged?	Yes	Yes	Yes	Yes
First Difference?	—	—	—	—
Early-impact ODA?	—	—	Yes	Yes
Aid/GDP	0.106	0.040	0.380**	0.379**
	(0.079)	(0.074)	(0.169)	(0.185)
$(\text{Aid/GDP})^2$	−0.003	−0.003	−0.022	−0.032**
	(0.003)	(0.002)	(0.015)	(0.016)
Twice-lagged growth		0.016		0.001
		(0.046)		(0.045)
Observations	404	391	343	336
R^2	0.371	0.383	0.420	0.427

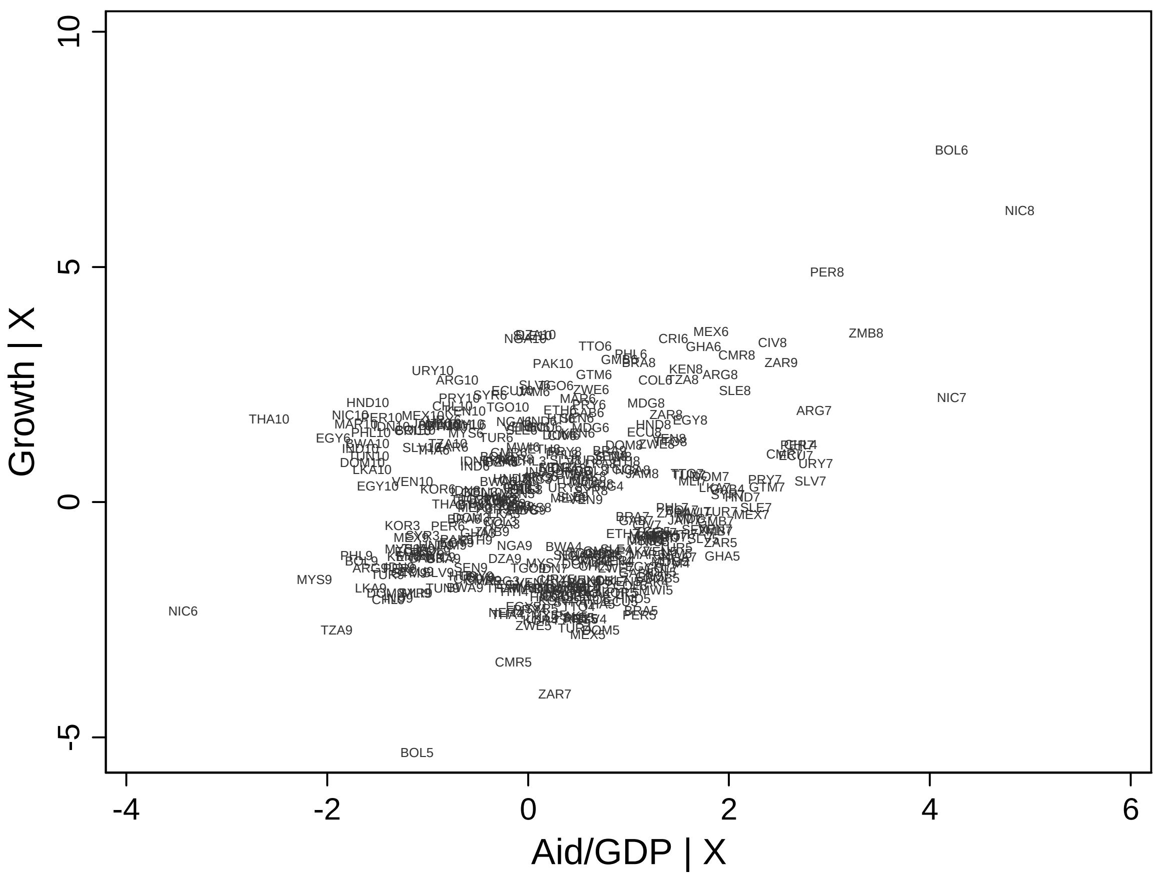
Dependent variable is growth of real GDP per capita, standard errors in parentheses. Column 1 above is identical to Table 9 column 6, and column 3 above is identical to Table 9 column 9. All regressions that use "early-impact" ODA also include Repayments/GDP and $(\text{Repayments/GDP})^2$, since "early-impact" aid is a gross flow and aggregate ODA is a net flow. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 11: **Robustness check: Replace estimated disbursements of early-impact aid with commitments of early-impact aid**

	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Estimator	OLS	OLS	A-H	OLS	OLS	A-H
Aid Lagged?	Yes	Yes	Yes	Yes	Yes	Yes
First Difference?	—	Yes	Yes	—	Yes	Yes
Early-impact ODA commitments?	Yes	Yes	Yes	Yes	Yes	Yes
Aid/GDP	0.101	0.304	0.293	0.577***	0.730***	0.631**
	(0.147)	(0.226)	(0.279)	(0.156)	(0.264)	(0.315)
$(\text{Aid/GDP})^2$	−0.010	−0.015	−0.018	−0.043***	−0.054***	−0.050***
	(0.007)	(0.012)	(0.017)	(0.010)	(0.017)	(0.018)
Observations	380	323	323	343	268	268
R^2	0.353	0.230	−0.030	0.441	0.390	0.367
KP LM stat (p-val.)			< 0.01			0.02

Dependent variable is growth of real GDP per capita, standard errors in parentheses. Columns 1-3 above recapitulate Table 7 columns 9-11, replacing estimated early-impact disbursements there with early-impact commitments here. Columns 4-6 above recapitulate Table 9 columns 9-11, replacing estimated early-impact disbursements there with early-impact commitments here. All regressions that use "early-impact" ODA also include Repayments/GDP and $(\text{Repayments/GDP})^2$, since "early-impact" aid is a gross flow and aggregate ODA is a net flow. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 1: **Semiparametric view of the partial aid-growth relationship in Table 7, column 7**



The vertical axis shows the residual when growth is regressed on all covariates except Aid/GDP and $(\text{Aid/GDP})^2$. The horizontal axis shows the residual when Aid/GDP is regressed on all other covariates except $(\text{Aid/GDP})^2$.