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Abstract

Previous aid effectiveness studies have typically attempted to identify recipient-side conditions of aid effectiveness – such as "good policies", political and economic stability, and "tropical effects" – using cross-country growth regressions. An obvious omission from this list of conditions is the extent by which donors are concerned with achieving geopolitical rather than developmental objectives, which may reduce aid effectiveness insofar as strategic donors have less incentive to hold the recipient government accountable for the developmentally effective use of aid receipts. Aid allocation regressions can (and are) used to demonstrate the importance of geopolitical considerations, but the author also shows that such regressions cannot be used to instrument for aid in a second stage growth regression, as is standard practice in this literature, because to do so would invoke the untested assumption that strategically motivated aid is just as effective as developmentally motivated aid. Instead the author tests the effect of lagged aid flows on growth, and subsequently demonstrates that: aggregate aid flows are estimated to have significant but moderately sized effects on growth; multilateral aid flows have roughly twice the effect of bilateral flows; but that the lower average effects of bilateral aid nevertheless obscure a substantial degree of heterogeneity in the bilateral aid coefficient which is again explained by the degree to which these flows are indeed strategically motivated.

Keywords: Foreign aid, Economic Growth, Multilateral Aid, Bilateral aid, Geopolitics.

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Introduction

Most of the 50 year old debate over overseas development assistance has been concerned with whether foreign aid promotes growth "on average". According to a systematic review by Hansen and Tarp (2000), the answer is more often than not a qualified "yes". However, the most recent round of debate, which Hansen and Tarp term the "Third Generation" of aid effectiveness research, has also concerned itself with the conditions of aid effectiveness, or second order rather than first order effectiveness. In this vein, researchers typically run country growth regressions in which they test the coefficients on three variables: AID, X, and AID*X, where X is a determinant of growth, and the last term is an interaction variable such that *X* now doubles as a potential condition of aid effectiveness. These regressions have suggested that there *may* be quite a diverse range of such conditions. Crudely summarised, aid may be effective: *in "good" macroeconomic policy environments* (Burnside and Dollar 2000) *and politically stable countries* (Chauvet and Guillaumont 2002; Headey, Rao et al. 2004), presumably because aid is more likely to be invested rather than consumed in such environments; *outside the tropics*, for reasons not precisely known (Dalgaard, Hansen et al. 2004); *in volatile economies* in which aid appears to buffer economic shocks (Chauvet and Guillaumont 2001; Collier and Dehn 2001); and *in postwar economies* where it facilitates general economic reconstruction (Collier and Hoeffler 2002).

These studies arguably have critical implications for any overall assessment of the general effectiveness of aid, as well as the important policy issue of where donors' funds would best be spent. Collier and Dollar (2002), for example, assume that aid effectiveness is only, and entirely, conditional upon good policies, and that aid cannot improve policies, which implies that foreign aid could be made more effective if only donors would allocate their funds from bad to good policy countries. Their arguments have indeed had an impact on the US, British and Dutch aid agencies in particular, and have at least sparked interest in many other agencies. The results of other authors are somewhat less coherent in practical policymaking terms, as well as disturbingly pessimistic: Dalgaard et al. (2004), strictly speaking, should conclude that since aid is only effective outside the tropics, it should therefore only be allocated to non-tropical developing countries (of which there are few indeed).

As far as Hansen and Tarp's claim is concerned, then, there is very little recent consensus on whether or not aid is effective "on average", whatever that may mean in

the presence of so much heterogeneity in aid efficiency. It may well be that researchers are still left with a new variation of the well known micro-macro paradox (Mosley 1987): why does foreign aid appear to be surprisingly ineffective in macro studies when micro studies typically indicate that aid has high returns?

There are at least three possible solutions to this paradox.

The most obvious is that there are serious methodological problems with micro studies, especially their inability to gauge the external effects of particular projects, as well as the fungibility problem.

A second solution, and one more relevant to the debate at hand, is that there may be serious methodological problems with macro studies. The cross-country growth regression, if not quite as dead as the dodo, seems to be high on the endangered techniques list. Results from cross-country regressions of aid on growth have shown aid-growth regressions to be especially sensitive to different IV techniques, aid measures, data structuring, regression specification (including the timing of aid's effect on growth), treatment of outliers, and sample variations (Roodman 2004). Any one of these factors, or several of them, could be seriously hindering the accuracy and power of these tests, although as we argue below, it is by no means clear that all these variations in technique should be regarded as equivalent: researchers ought to justify and employ the best measure of aid, the best regressor, specification, sample, and so forth.

A third and final explanation of the micro-macro paradox is that not all of the conditions of aid effectiveness have as yet been identified. Headey et al. (2004) attempted to assess all the previously identified conditions, and also show theoretically that any variable which determines the returns to investment, and is therefore a variable which ought to be included in a growth regression, may discernibly influence aid effectiveness. But all the conditions tested in this literature – those of Headey et al. included - thus far have one thing in common: they are primarily recipient-side conditions of aid effectiveness. Their implications are not only grim for those donors interested in promoting economic growth, they are also quite harsh on recipients since they essentially suggest that aid ineffectiveness is the result of the recipient country environment: its macroeconomic policies, its political institutions, the structure of its production and its biogeographical environment. Donors are only at fault insofar as they allocate aid to countries where it is liable to be

ineffective – those with poor conditions - and in disproportionate amounts such that absorptive capacity limits are reached and diminishing returns set in.

This article chiefly explores this third line of reasoning by reassessing aid effectiveness with the other side of the equation in place: the donor side. Our *a priori* hypothesis is quite simple. We suggest that a major explanation of aid's observed ineffectiveness over the period 1970 to 2001 is the large degree to which aid allocations are biased towards geopolitically important countries in which aid is used to achieve non-developmental outcomes, such as political allegiances. In such cases, a selfish donor is unlikely to demand much accountability from the recipient government with respect to the use of their aid flows for fear of weakening political allegiances. This hypothesis is not new. Development experts, policymakers and journalists have been keenly aware of the political element in aid allocation for a long time. As early as 1968, for example, Gunnar Myrdal (1968), concluded that "the present system of bilateral aid has a strong tendency toward what, from an economic point of view, can only be construed as a misallocation." More recent research has also paid lip service to such issues (Harms and Lutz 2004), but in general the most damning evidence of the ineffectiveness of strategic aid is anecdotal (for example, see Russell's (2000) journalistic account of aid to Zaire), as well as very indirect inferences from aid allocation studies, such as in Alesina and Dollar (2000), in which donor motivations are used to explain recipients' aid receipts.

In fact we take our cue from the Alesina and Dollar study (Section II), by testing the influences of an expanded set of geopolitical influences on aid allocation, which encompasses post-colonial, Cold War and Middle East factors. Importantly, these regressions establish that the "strategicness" of aid flows obviously varies considerably across recipients, suggesting that political factors could conceivably explain the considerable variation in aid effectiveness observed in so many other studies.

However, testing such a hypothesis – indeed, testing the effective of aid in general - is fraught with difficulties, discussed in depth in section III. The standard approach in all aid-growth regressions since Burnside and Dollar (1997) is to employ the aforementioned Alesina and Dollar type instruments to derive an exogenous measure of aid. But these would-be instruments are fundamentally flawed in ways which are largely ignored by the literature to date. The most important problem is that if our *a priori* hypothesis is correct – that is, that strategic and non-strategic aid flows

do have differential effects on growth - then the instrumental variables (IV) estimate of aid is biased because the degree of "strategicness" varies across recipients. These instruments may therefore utterly confound an appropriate test of strategic versus non-strategic aid as well as any test of "aid intended for development", which is what most of aid effectiveness studies aim to achieve in principle. Nevertheless, almost every study to date has employed these instruments as the only alternative to OLS and thereby implicitly invoked the precarious assumption that strategic and non-strategic aid flows are equally effective[1], while one study (Rajan and Subramanian 2005) actually only uses strategic instruments, thereby quite literally deriving a "strategic aid-hat". The only sensible alternative is to lag aid flows, which has other benefits, discussed below.

In section IV we employ what we believe are the best techniques available to test the effectiveness of aid "on average" as well as the differential effectiveness of strategic and developmental aid. We find that improving the techniques with which aid is tested substantially leads to larger and more significant estimates of aid's positive average effect on economic growth. However, there are some important conditional relationships. First, aid looks to have been especially effective in sub-Saharan Africa (admittedly after controlling for low scores in other determinants of growth, such as governance, policies, geographical and political factors), but ineffective in Latin America and perhaps elsewhere. Second, in complete contrast to Ram (2003) – who did not allow for aid's endogeneity at all – we find much larger effects for multilateral rather than bilateral aid, which is shown to be much more strategically motivated than multilateral aid. And third, when we derive admittedly crude indices of the "strategicness" of recipients in the eyes of Western donors, we find considerable evidence that such strategicness reduces the effect of aid flows, especially bilateral aid, as expected.

Section V concludes. This study sought to lend weight to the haphazard evidence that the political dimensions of aid delivery are not independent of aid's outcomes. Nor should we expect them to be: wherever the purpose of aid is to placate or bribe government elites in developing countries, it should hardly be surprising that donors' efforts to make aid as developmentally productive as possible, including using aid to induce policy reform, are substantially reduced. Moreover, although our

[1]With the main exception being Clemens et al. (2004) who also use their short impact measure lagged one period, rather than fully instrumented.

results, like any cross-country growth regression, represent historical findings, there is little doubt that many types of strategic motivations still heavily influence aid allocations, and, by association, the effectiveness of such aid flows. Any attempts to reform the aid industry by depoliticizing aid allocations should be welcome, including increasing the influence of multilateral institutions.

II. Donor motivations and aid allocation

What types of strategic political motivations explain aid allocation? How sensitive are donors to strategic opportunities? The usual means of answering this sort of question is to run a cross-country regression with aid flows as the dependent variable, to be explained by donor motivation variables. This allows researchers to make *ceteris paribus* type inferences on the influe nces of strategic political variables, whilst controlling for needs-based or developmental factors. In Table 1 we carry out this exercise with a wide range of variables explaining aid to GDP ratios across some 56 developing countries at both low and middle income levels (see Table 1A in our appendix). The choice of countries is determined by data availability, but we have also excluded countries which were already substantially developed during the period under consideration (such as Singapore and South Korea), as well as Eastern European countries and countries with small populations (less than a million in 1970). The data are grouped into four year averages over the period 1970 to 2001 (although we sometimes use lags relating to the period 1960 to 1969) to give an unbalanced panel offering around 400 observations. The use of four year averages is useful for several reasons. First, it smoothes out business cycle effects which may be manifest in the GDP series as well as aid flows (e.g. if donor aid flows are procyclical), and reduces measurement error. Second, lagged four-year averages of aid appear to provide a good fit for the short to medium term dynamic effects of aid on growth, as we show in subsequent sections. Shorter periods are unlikely to smooth out business cycle effects substantially, whilst discerning the effects of aid received some 9 to 16 years in the past is probably asking far too much of the data given the high level of statistical noise (and omitted variable bias) present in cross-country growth regressions.

Although the structure of our data conforms to that of most empirical studies in this literature, our dependent variable and some of our explanatory variables are relatively novel (see Table A1 in Appendix A for more detailed definitions). First, our

aid measure is not the standard overseas development assistance measure (ODA). The great weakness of this measure in terms of explaining aid's short to medium run effects on growth is that it includes aid flows which are only likely to influence growth in the long term (such as education expenditure), or unlikely to influence growth at all (Clemens et al., 2004). A prime example of the latter, of course, is humanitarian aid such as emergency assistance and food aid. Therefore alternative measures are desirable. For 1970 to 2001, Clemens et al. derive an estimate of short impact aid from commitments data by multiplying the share of short impact commitments in total commitments (the only measurable short impact aid ratio over this period) by total disbursements, thereby assuming that the measurable short impact commitments ratio is roughly equal to the unmeasurable short impact disbursements ratio. They then compare this to some actual short impact disbursements data for 1990 onwards. Elsewhere (Headey 2005a) we have argued against exclusively employing this measure for several reasons, the most important of which is the very poor coverage of the commitments data and the recent (1990-onwards) data by which it is benchmarked. The percentage of total commitments covered by the OECD's CRS commitments for the earliest year in which it can be estimated is around 70%, while the benchmark disbursements data table has coverage ratios in some years as low as 8% for the world's largest donor, the US. Moreover, the ratio of CRS commitments data to DAC disbursements data was as low as 15% in 1990, and none of these coverage ratios are time invariant. There are probably other errors induced into the estimation procedure (for example, variables which systematically influence the ratio of commitments to present and future disbursements, such as wars, regime changes and natural disasters), but the incompleteness of the primary data source is enough to warrant a rejection of their final aid estimate. We therefore use a more reliable measure, actual aid disbursements less humanitarian aid, derived from the OECD DAC tables, which we term aid for production (PRODA). We use net PRODA but we are also able to separate PRODA inflows from outflows (loan repayments), a novel practice employed by Clemens et al which allows us to more clearly separate the effects of actual projects and program assistance without them being mixed with the additionally heterogenous effects of loan repayments. Nevertheless, ODA and PRODA are indeed highly correlated (ρ=0.95) and yield very similar results for the aid allocation regressions (results available on request).

We have also attempted to derive new variables which may explain aid allocations. The first is a measure of natural disasters, which is the number of people per million affected by a natural disaster. This proves to successfully explain aid allocations even after removing humanitarian aid form the left hand side. Another important new variable, given the context of this study, is a purely strategic variable. We created a dummy variable equal to one whenever a country received aid of more than 0.2% of GDP from several Warsaw Pact countries (the USSR, Poland, Czechoslovakia), data for which were again available from the OECD DAC statistics. These aid flows are excluded from the ODA and PRODA measures, which are essentially comprised of aid flows from the West during the Cold War (the Warsaw Pact aid dummies only apply to the early 1990s and before). We also employ a lag of this variable. Our rationale here is that foreign aid was one of the tools used by Western donors to compete with the Warsaw Pact countries for the political and ideological allegiance of Third World countries. The significance of the lag may also indicate that Western donors saw the reduction in Warsaw Pact aid as an opportunity to increase their political influence in such countries – to fill the void, as it were - and support economic and political liberalization. This appears to have happened in countries such as Ethiopia, Mozambique, Nicaragua, among others. Finally, we also note that our Warsaw Pact dummy variable is quite highly correlated with arms imports, a variable which has been used in other aid allocation studies to positively explain aid flows, and which is significant in our regressions only when we remove the Warsaw Pact dummy. Thus our measure seems to capture the same sort of political motivation captured in previous studies.

Our second, smaller innovation is to allow for much greater heterogeneity in post-colonial aid. Two of the most significant and robust indicators of aid flows in previous studies are GDP per capita and population size, which of course vary negatively with aid flows. Thus one would expect French and British aid flows (the main post-colonial donors) to their former colonies to vary with income levels and population sizes, just as all other aid flows vary according to these two factors.

A final innovation is not to specify the Burnside and Dollar policy index and the POLITYIV democracy index in levels – which yielded insignificant coefficients on both variables – but in differences. We first found that both changes in policies and changes in democracy scores yielded significant coefficients with expected signs. However we subsequently decided to see whether there were asymmetries in the way

donors responded to decreases and increases in economic and political liberalization. The results are interesting, and suggestive of considerable asymmetry in the way in which donors punish and reward recipients for economic and political changes. As it turns out, donors (on average) only appear to reward policy improvements (and then too, only rather temporarily since they do not reward policy levels, a fact noted by Burnside and Dollar, 2000), but do not punish policy deteriorations (the policy deterioration dummy coefficient has the right sign and is reasonably large, but fails a t-test). This may be because donors frequently fail to punish recipients when conditionalities are not met, but also because some types of aid are intended to alleviate macroeconomic crises, which may be captured in the policy index. Conversely, donors do punish recipients for "de-democratizations", but do not reward democratization (although this coefficient does indeed have the right sign, it easily fails a t-test).

Turning to the results, how important are strategic political factors in determining aid allocations? To answer this let us first define variables as either developmental (non-strategic), semi-strategic and strategic. Developmental variables are either indicators of need (e.g. GDP per capita, life expectancy) or efficiency, reward or punishment (policy or democracy scores). Several other variables in this category were tested here, including governance scores and measures of political instability, but these were not robust. Semi-strategic variables include the log of population, since the higher aid levels which flows to smaller countries, some of which are less deserving of aid flows relative to populous poor countries like India (which may contain as much as a third of the world's poor), are partly motivated by the ease with which political influence may be purchased in small countries (a vote in the United Nations General Assembly from tiny Togo is worth just as much as a one from massive India). Other semi-strategic variables include colonial factors. On the one hand, former colonies and expatriates from the donor country often still have close trade links which may be bolstered by aid-funded imports, while aid dependency also offers ex-colonial donors a means of extending their political influence into the post-colonial era. But on the other hand, post-colonial donors may have certain advantages (as well as perceived moral responsibilities) in making their aid flows more productive, including a common language and the possession of local knowledge and expertise (Cassen 1994). Finally, we argue that the Warsaw Pact

dummies, the Middle East dummy (covering Egypt, Jordan and Syria) and the arms imports interaction can safely be considered strategic political variables.

Separating the variables thus, it is possible to gauge their relative significance by looking at the coefficients on the normalized explanatory variables. The coefficients therefore represent the change in aid receipts resulting from a change in the explanatory variable of one standard deviation, except with the dummy variables in which case they represent the effects of a dichotomous change. How influential are strategic variables relative to developmental variables? Compare, for example, the effect of a recipient's income and life expectancy being one standard deviation lower than the mean (a substantial US$ 1,575 and 11.6 years), which results in an increase in aid over GDP of 3.6 percentage points (1 percentage point due to GDP per capita and 2.6 percentage points due to lower life expectancy). This increase is considerably less than the extra aid a recipient would receive if it accepted Warsaw Pact aid during the Cold War (about 5.8 extra percentage points), while the effect of being a Middle Eastern country of strategic importance (Egypt, Jordan, Syria) is also substantial. In fact, these strategic effects are much larger than any rewards to policy improvements or punishments to anti-democratic regime shifts. The colonial effects depend on income or population levels, but when these are held to their means (equal to zero since GDP and LPOP have been normalized) the colonial effect is also substantial – some two and half percentage points of extra GDP. This is not to say that all donors give aid strategically – donor-specific aid allocation regressions unsurprisingly give highly varied results – and some donors, such as Scandinavian donors, appear to be almost solely motivated by developmental considerations (Alesina and Dollar 2000). However, it is also worth noting that even though these countries are generous in relative terms, the U.S., Japan, France, the U.K. and Germany account for around 70% of total bilateral aid flows, and all employ aid strategically, albeit it to varying degrees.

So given that donors, especially the largest donors, are frequently so sensitive to political motivations, should we be surprised to find that they are commensurately insensitive to the developmental impact of their strategic aid flows? At the very least, the substantial influence of political motivations in aid allocation suggests that such a hypothesis should be investigated in det.

[insert Table 1]

Section III. Strategic motivations and biases in IV estimation

By and large, strategic motivations have only warranted peripheral consideration in most recent aid effectiveness studies where the primary task has been to ask whether aid is effective on average, or whether recipient-side conditions explain the substantial heterogeneity in aid's effect on growth. Insofar as they are considered, strategic motivations are only incorporated as a means of rendering aid flows exogenous with respect to growth, in that the aid allocation regression in Section II, for example, will serve as a first stage regression providing fitted aid values for a second stage growth regression. But if the political factors considered in such regressions are not independent of aid effectiveness, then this approach is highly inappropriate even if one is only interested in average aid effectiveness. In other words, by employing both developmental and strategic variables in a first stage aid regression, one is invoking the untested hypothesis that strategic aid is just as effective as developmental aid, even though common sense dictates that this unlikely to be the case. A recent paper by Rajan and Subramanian (2005) in fact to goes to one extreme by only employing strategic variables to explain donor-specific aid allocations to all recipients, after multilateral flows are re-assigned to each bilateral donor by reassigning its funding of multilateral institutions back to its bilateral aid flows). The fitted values from these regressions for each donor are then summed to give aggregate exogenous aid levels for each recipient. Unlike many other recent studies, they find almost no evidence that aid increases growth (they also employ some unusual techniques, typically using cross-sectional regressions on a sample which includes relatively small countries and very large aid recipients, decisions which are much more likely to bias down the coefficient on aid). But of course, the hypothesis they are really testing is that strategic aid increases growth. The authors let us know that they are aware of this (in their appendix II), but argue that there are no valid reasons to believe that the effect of strategic aid differs from that of non-strategic aid. As evidence of this they point out that aid has been given to "high-growth dictators" as well as bad (although there is no evidence that aid caused that high growth, or that such countries have high levels of "residual growth" which is the effective regressor in well controlled growth regressions), that strategic aid goes to the "good" social sector as much as total aid (which hardly seems relevant at all – where is strategic aid supposed to turn up in national accounts?), and that their results show

that multilateral aid (relatively non-strategic aid) is just as ineffective as bilateral aid (relatively strategic aid). This last argument is the most perplexing, however, and illustrates the fundamental problem of using strategic instruments in 2SLS, since the authors cannot convincingly use strategic instruments – such as colonial dummies, common language dummies and strategic alliances – to explain multilateral aid flows. The test, therefore, is utterly confounded.

But this problem is by no means confined to the Rajan and Subramanian study: all IV results in this literature suffer from the strategic bias problem, as well as several others. With standard IV approaches the "strategicness" of aid still varies across countries, so the results are still biased, and there is no way of knowing to what extent we are testing overall aid effectiveness or strategic aid effectiveness. In addition, the developmental instruments are probably not valid instruments at all in that life expectancy and indicators of political changes and policy reforms are unlikely to be robustly independent of growth. Finally, using an IV approach can also induce the undiscussed problem of multicollinearity between aid and other explanatory variables. After all, a fitted aid value derived from the regressions in Table 1, for example, will effectively be an index of GDP per capita, life expectancy, and policy and institutional factors, all of which arguably belong in a growth regression. When the aid-hat variable is re-introduced into the growth regression, it is likely to be more highly correlated with the other regressands than was the original un-instrumented aid variable. Multicollinearity between "aid-hat" and these variables could therefore induce an additional bias in IV estimates.

Let us now back up these criticisms with some empirical support. Table 2 shows the correlations with three variables: non-instrumented aid flows, development aid (the fitted values of regressing aid against non-strategic variables only) and strategic aid (the fitted values of regressing aid against strategic variables only). The variables in column 1 are all variables which regularly appear in cross-country growth regressions. To show that multicolloniearity is exacerbated by using developmental instruments, compare the correlations between columns 2 and 3: fitting aid values to developmental variables substantially increases the correlations between most of the growth regressands and aid (especially those used in the first stage regression) and decreases the uncontrolled correlation with growth. An opposite effect generally takes place for strategic aid precisely because strategic aid tends to flow to countries which are typically not in the greatest need of aid. So multicollinearity could well confound

general aid tests, as well as any attempts to use IV approaches to separately test for differential effects between developmental and strategic aid.

[insert Table 2]

Now let us look at multilateral versus bilateral aid. Table 3 uses the instruments from table 1 to explain these two aid flows. We report both the normalized regression coefficients and the raw correlations. Notice that only one of the strategic variables significantly explains multilateral aid, the lag of Warsaw Pact aid, which may simply mean that multilateral donors were simply attempting to fill some of the financing void left over by the end of the Cold War. In any event, even the coefficient for this variable is much smaller than for bilateral aid, for which all the coefficients on the strategic variables are highly significant and large in magnitude. Looking at the semi-strategic variables we find that the colony dummy-GDP interaction is in fact now insignificant for bilateral aid: bilateral post-colonial aid allocations do not appear to significantly affected by per capita income considerations, only by population size. Instead, the reverse is true for multilateral aid, as one would expect. Turning to the developmental variables, most of the relationships are roughly the same, although bilateral aid appears to be slightly more sensitive to democracy decreases (although actually, there are relatively few such episodes), and about half as sensitive to natural disaster incidences, in which some multilateral agencies specialize.

We extract two messages from this table. First, as expected, multilateral aid is much less strategic than bilateral aid, so these aid flows probably do serve as reasonably (perhaps the best available) proxies for non-strategic and strategic aid flows respectively. Second, it is not possible to derive a valid IV estimate of multilateral aid flows because they are not significantly explained by the only plausible instruments available. The only option in cross-country growth regressions, then, is to employ lagged flows, which we do in the next section.

[insert table 3]

Section IV. Average, Strategic and Non-Strategic Aid Effectiveness

In table 4 we show the effects of improving the methods used to test aid's effect on growth, including the lagging of aid.[2] But before discussing the results, it is worth considering what sort of coefficient we should expect on the foreign aid variable. Rajan and Subramanian (2005) use a neoclassical production function with some plausible parameters and an important assumption that aid has no spillovers to predict that the aid coefficient should be somewhere in the order of 0.16: a 1 percentage point increase in aid over GDP from its mean should raise growth by 0.16 of a percentage point. Of course, one would hope that aid does have positive spillovers, but 0.16 is in any event a helpful benchmark. Those authors of course do not find much evidence that aid has an effect of anything like 0.16. Using what we have argued is a much better methodology, do we find any evidence that aid has these sort of returns?

Regression 1 runs the sort of specification used in much of the literature, in that it uses an aid measure which does not exclude humanitarian aid, and does not lag aid. Switching to PRODA – that is, excluding humanitarian aid – without lagging immediately makes the coefficient on aid significant at the 10%, with a coefficient of 0.05. In regressions 3 and 4 we lagged ODA and PRODA respectively, and the significance levels jump to 2.98 and 3.69 respectively, while the coefficients roughly double. In regression 5 we investigate the dynamic relationship some more by using contemporaneous aid (PRODA), and aid lagged one and two periods. This specification allows us to control somewhat for serial correlation in the aid variable. The results clearly suggest that the only significant relationship between aid and growth is a dynamic one: aid lagged one period is highly significant, while contemporaneous aid and aid lagged twice are most definitely not. Moreover, the coefficient on aid lagged by just one period is a relatively high 0.15, just under the 0.16 benchmark. In regression 6 we use the Arellano and Bond (1991) dynamic GMM estimator with only one lag. The choice of just one lag may seem strange, but as Roodman (2004) discusses, previous results using GMM in this literature have tended to over-instrument for aid, which can lead GMM results to converge to OLS. The GMM estimate of the coefficient on aid is larger than OLS estimates (which seems to

[2] The lagging of aid has several advantages. First, as Roodman (2004) and Clemens et al. (2004) discuss, if growth and aid events are evenly distributed during a four-year period, then the average lag between an aid disbursement and a subsequent growth event is only sixteen months, far too short a time for most investments to have much impact, especially since we are employing an aid measure which contains long impact as well as short impact aid. Lagging aid means that we are more likely to test the effects of aid over the medium term, which is probably all the data are capable of doing.

be a consistent result in this literature) at around 0.22, but the GMM results are sensitive in other ways: most of the coefficients on other variables in the model are insignificant, and I also find that using longer lags (2 and 3) renders the aid coefficient insignificant and reduces it to around 0.15. Given the sensitivity of GMM results, it is not our preferred estimator and we do not employ it again.

Finally, regressions 7 and 8 test aid in the two continents with the most observations, sub-Saharan Africa (SSA) and Latin America. In previous research (Headey et al. 2004) we found that aid appears to have been ineffective in Latin America (and in non-SSA samples, the results of which are available upon request). In fact, this appears to very nearly constitute a stylized fact in this literature given that it was uncovered even in the very early work on aid effectiveness (Papanek 1973; Gulati 1978). We reconfirm this result here, and also show that aid has been highly effective in low growth sub-Saharan Africa. We do not make any extensive attempt to explain this apparent "stylized fact" here,[3] but we do believe it is an interesting and easy way to show that the result that aid is effective on average certainly does not mean that it is effective everywhere.

We now turn to the important question of whether the strategicness of donors' motivations renders their aid less effective. Perhaps the most obvious and least controversial means of testing this hypothesis is to reconsider the multilateral-bilateral dichotomy. This question has previously been considered by Ram (2003) – and more recently by Rajan and Subramanian (2004). However, Ram uses the Burnside and Dollar aid data, which we have already argued is inappropriate, not least because it includes humanitarian assistance. But more importantly, Ram makes no allowances for the endogeneity of aid. It is little surprise, then, that he gets some unusual results: he implausibly finds that bilateral aid has very strong effects on growth – from 0.42 to 0.77 when outliers are removed – while multilateral aid either has no significant effect, or a massive negative effect (-1.02). But because there is no allowance made for reverse causation, all this may really say is that multilateral aid goes to countries experiencing low growth episodes. The IMF, for example, frequently sends aid to countries undergoing macroeconomic crises, while many UN agencies likewise

[3] One possible explanation is that aid outside Africa has been used quite strategically, often in relatively well off developing countries where its marginal returns are low. Though Africa is often said to lack absorptive capacity, especially in terms of local bureaucracy, the low capital stock ought to mean that aid has relatively high returns, according, at least, to the standard neoclassical production function. We cannot prove this postulate, but we do provide some evidence below which is at least consistent with it.

specialize in humanitarian disasters. The Rajan and Subramanian paper runs several regressions for different periods and finds no significant effects of either aid flow. However, as we alluded to earlier this paper employs an unusual methodology which involves an especially unconvincing instrumentation for multilateral aid. Their regressions therefore suffer from the same biases as Ram's.

Table 4 re-tests multilateral and bilateral aid non-humanitarian aid flows. In the first two regressions we employ the two incorrect methodologies, a non-dynamic OLS specification (Regression 1) and 2SLS with the instruments from Table 3, which are of course inappropriate insofar as multilateral is not explained by strategic factors. As might be expected, neither multilateral nor bilateral aid is significant, although the coefficient on multilateral aid is roughly double that of the bilateral aid coefficient. In regressions 3 and 4 we lag aid flows and inflows respectively. The coefficients are all approximately significant at the 5% level except for bilateral aid inflows which are highly insignificant. The coefficient on multilateral aid is estimated to be around 0.17 for inflows or 0.19 for net aid, which accords almost exactly with Rajan and Subramanian's neoclassical benchmark of 0.16. However, the best bilateral estimate suggests an effect of some 0.09, about half the effect of multilateral aid. We were also curious to see whether longer lags yielded similar results, and indeed they do: aid flows lagged two periods yield a slightly higher coefficient on multilateral aid (0.21) but an insignificant coefficient for bilateral aid inflows. As an additional robustness check we can remove one of the aid flows at a time, in case multicollinearity is confounding our test. This actually has the effect of increasing the coefficients on both terms: for multilateral flows the coefficient increases to about 0.25, for bilateral flows to 0.13. Finally, we note that there are no obvious outliers driving these results, although as usual there are countries which receive relatively large amounts of aid. However, quadratic specifications do not suggest that a non-linear specification is appropriate, and using the least absolute error regressor, for example, only strengthens our results (results available upon request).

So this is some fairly strong evidence that aid may be less effective when it is strategically motivated. But what other evidence can we discern? One relatively novel approach is to use the coefficients from our first stage aid regressions to define indices which gauge the strategic attractiveness of a recipient to a donor. Thus for our index of strategic attractiveness, we use the coefficients on the Warsaw Pact aid dummy and its lag, the Middle East dummy, as well as the coefficients on population size, GDP

per capita and life expectancy, although we render the latter two coefficients positive. We justify incorporating the last three variables because significant amounts of aid to relatively wealthy countries (with high GDP per capita and high life expectancy) are likely to be strategically motivated, while smaller countries are also attractive to donors because of the greater ease by which political influence may be obtained, for example in UN voting. We also form a semi-strategic (or colonial-strategic) index along similar lines, which basically just adds a post-colonial effect to the first index. We do this for all aid flows as well as bilateral aid, the weights for which are obtained as before. The weighting of the four indices are given in Table 5, but these indices where then normalized before being used in the regressions in Table 6, which reports aid interactions with these indices for both total aid inflows and bilateral aid inflows.

[insert Table 5]

[insert Table 6]

In Table 6 we again lag aid flows, as well as the motivation indices. For each index we run two regressions, one in which we only introduce the motivational index into an interaction term, and one in which we also add it separately. Whether one should include an interaction term separately or not is somewhat unclear. On the one hand, we could let the data decide, but on the other there are some theoretical grounds for leaving the motivational indices in the specification. Strategically important countries might possess unobserved characteristics, for example, which are not independent of growth (e.g. post-conflict recovery). Most of the results, however, are relatively insensitive to whether or not a motivation index is included separately. The main exception is the strategic aid interaction for all aid, which is negative but insignificant when the motivation index is excluded. However, the motivation index by itself is only marginally insignificant at the ten percent level. All the other interactions are always significant at the 10% level at least. Moreover, the sizes of the effects are considerable. Recalling that the motivation indices are normalized, we observe that aid effectiveness depends considerably on these political factors. Note that the effect on growth on aid is given by:

(1) $\delta g/\delta a = \beta_a + \beta_{am}.M$

where β_a is the coefficient on the relevant aid flow (all aid or bilateral aid), β_{am} is the coefficient on the aid*motivation index interaction, and *M* is the normalized level of the motivation index. Since the variable is normalized, the effect of aid on growth at the mean level of the motivation index is just given by β_a. For total aid flows these values (0.11-0.13) are generally equivalent to those which we derived in Table 3, although for bilateral aid flows these coefficients (0.12-0.14) are considerably higher than they were in Table 4 (0.09). Moreover, these interactions explain substantial variation around these levels of "average aid effectiveness". The bottom three rows of Table 6 give all the maximum, minimum and mean effects of aid. When foreign aid is allocated to non-strategic countries, its maximum effect is estimated to vary from 0.15-0.25 for total aid flows, while bilateral aid's maximum effect is estimated to be as high as 0.23- 0.30, well and truly on par with the high average levels of multilateral aid effectiveness (which are in fact somewhat sensitive to the specification of these interactions). Conversely, aid can have no effect, or even small negative effects, when it is strategically motivated.

Of course, there are probably several legitimate obstacles to accepting these results with perfect confidence. The method of using aid allocation coefficients is necessarily inexact since our ability to explain aid flows, and strategic aid flows in particular, is obviously somewhat crude. But any tool used to gauge these rather nuanced effects is not infallible. Furthermore, these results can be defended on several quite compelling grounds. Perhaps most importantly, the results in Table 6 are quite consistent with those of Table 4: multilateral aid, which we have shown to be considerably less strategic than bilateral aid, seems to have a larger average effect than bilateral aid, unless one controls for the political influences on bilateral aid effectiveness. When we do invoke these controls, we find that multilateral and bilateral aid flows do in fact have very similar mean effectiveness levels. Presenting two quite distinct forms of evidence in support of the same underlying hypothesis should make us reasonably confident that we have uncovered a real causal process, rather than a statistical artifact.

Section V. Conclusions

This paper has presented evidence which supports the hypothesis that donors' political motivations - and not just recipient-side conditions such as good

macroeconomic policies - also have a significant and plausible impact on the effectiveness of foreign aid. We first showed that previous studies have generally not used appropriate econometric methods, the two key elements of which are an improved measure of foreign aid which roughly gauges "aid intended to promote growth", and a dynamic growth specification which simultaneously renders aid exogenous (without relying on untested assumptions regarding the equal effectiveness of strategic and non-strategic aid) and identifies a plausible dynamic relationship between the timing of aid inflows and the eventual returns to those flows. We then considered one of the easiest and, in policy terms, the most important means by which one can decompose aggregate aid flows into strategic and non-strategic (or less strategic) influences, the multilateral-bilateral division. As expected, multilateral aid flows are not well explained by strategic aid variables, whereas bilateral flows are, such that the latter constitutes a reasonable proxy for aid flows that are relatively strategic in orientation. Testing multilateral and bilateral flows produced the predicted result that the multilateral aid produces growth effects roughly twice the size of bilateral flows. Moreover, the construction of motivational indices and their interaction with both aggregate and bilateral aid flows also suggests that a great deal of the variation in aid outcomes is explained by political strategic factors. In fact, these results show that bilateral aid can be quite effective when it is not politically motivated, while multilateral aid tends to be more effective on average.

In this final section we address some possible objections to these findings. The most important of these is that the cross-country growth regression is a highly flawed means of testing the effects of a wide range of potential determinants of growth. Testing the effects of foreign aid on growth is arguably an especially hazardous exercise: aid is likely to be endogenous, it is difficult to measure accurately, and, most importantly of all, it presumably affects growth through several quite distinctive transmission mechanisms (Hudson and Mosley 2001; Gomanee, Girma et al. 2002): investment and consumption levels, macroeconomic outcomes and policies, and institutions, to name the most important. We are entirely in agreement with assessment. In our view, growth regressions cannot be used to make statements such as "aid has been a success". Their strength, of course, is their ability to control for so many other determinants of growth so that researchers can make statements such as "controlling for initial incomes, policies, geography, and institutions, aid has had a positive effect on growth when it has not been used as a mere instrument of

geopolitics". But this strength is also a weakness: another way to interpret these results is to conclude that "aid has increased growth net of its effects on other determinants of growth", especially policies and institutions. It is very difficult to look around the world and find countries which have experienced aid-induced "miracles", and likewise easy to find aid dependent "disasters".

But whether this is because aid does indeed worsen policies and/or institutions, however, is by no means clear. Whether or not aid improves or deteriorates governance, for example, has been shown to depend on which instruments one uses (Harms and Lutz 2004). Moreover, when we update Knack's (2001) regressions of governance change against aid, we find that lagging aid reverses Knack's result (see Appendix B). With regard to aid's effects on policies, there is further ambiguity. While there is little doubt that conditionalities have often failed and, even when they have worked, have worked extremely sluggishly, the last twenty years also appears to have been a period of unprecedented reform. It therefore seems difficult to reconcile statements by Dollar and colleagues (Burnside and Dollar 2000; Collier and Dollar 2002) that "Aid has not systematically affected . . . policies during the 1970-93 period" with statements by Rodrik (2003) that "countries such as Mexico, Argentina, Brazil, Colombia, Bolivia, and Peru did more liberalization, deregulation and privatization in the course of a few years than East Asian countries have done in four decades". It seems hard to believe that donors – the Washington institutions in particular - did not play any role in influencing these regimes. Moreover, several of the most stubborn African reformers – Ethiopia, Madagascar, Malawi, Kenya, Tanzania - were finally relenting to World Bank pressure at almost precisely at the time of Burnside and Dollar's initial publications.

Our view is that whether one regards aid as a success or a failure very much depends on what one expects aid to achieve. If institutions are primal, is it reasonable to expect aid to be an engine of growth? Probably not. If institutions are rigid and bad institutions are especially difficult to break down under the best of circumstances, is it reasonably to expect aid and aid donors to substantially improve them? At best, only very slowly. Aid operates in a Second (or Third or Fourth) Best universe. And while the effectiveness of aid flows can doubtlessly be improved along a number of dimensions, it is unrealistic to expect aid to easily produce the deep institutional changes necessary for growth. Foreign aid is essentially a proximate determinant of growth, but the evidence here suggests that, measured against a more realistic

institutionalist yardstick, it is a reasonably effective one, especially when its motivations are, in a developmental sense, relatively pure. Moreover, donors can make aid substantially more effective simply by reducing the strategic biases of their aid allocations.

Appendix A

Table A1. Definitions of variables used in this study.

Code	Definition
Growth Variables	
GDP (LGDP)	Initial GDP per capita (the log of)
TROPLAND	Proportion of land area in tropics
OPEN	Sachs and Warner Openness index
INFL	Log of the CPI+1
GOV	ICRG Governance Index
PINSTAB	Political instability (assassinations and revolutions)
DTKPOP	Change in the number of war deaths
LPOP	Log of population size
POPG	Population growth
SURP*	Budget surplus (%GDP)
SCHOOLYR*	School years
Aid Variables (all %GDP)	
ODA	Overseas development assistance
PRODA	ODA less humanitarian aid
PRODA_IN	PRODA inflows only
REPAY	Repayment on ODA loans (aid outflows)
_MULTILATERAL	
_BILATERAL	
Instruments for Aid	
DISPOP	Deaths per 1,000,000 people from natural disasters
DDEMOC	Change in democracy score (0-10)
DPOLICY	A change in the Burnside and Dollar (2000) good policy index
DEMOC_DOWN	A dummy variable equal to 1 when there is any reduction in the democracy score
POLICY_UP	A dummy variable defined as 1 when there is a standard deviation increase in the Burnside and Dollar policy index The standard deviation is used to overcome statistical volatility in the policy score which is due to its inflation and budget deficit components
COLONY	Dummy variables equal to 1 for recent (20 century) former colonies
EGYPT	Dummy variable equal to 1 for Egypt
ARMS1	Lag of ratio of arms imports to total imports
WPACTAID	A dummy variable equal to 1 if a country received aid flows of at least 02% of GDP from the USSR, Poland, Czechoslovakia or Lithuania

Notes:

*Excluded from base regression.

Notes: For sources of the variables as well as fuller descriptions, see Headey (2005)

Table A2. List of Countries (N=56)

Algeria	Guatemala	Papua New Guinea
Argentina	Haiti	Paraguay
Bangladesh	Honduras	Peru
Bolivia	India	Philippines
Brazil	Indonesia	Senegal
Burkina Faso	Jamaica	Sierra Leone
Cameroon	Jordan	Sri Lanka
Chile	Kenya	Syrian Arab Republic
Colombia	Madagascar	Tanzania
Congo, Dem. Rep.	Malawi	Thailand
Congo, Rep.	Malaysia	Togo
Costa Rica	Mali	Tunisia
Cote d'Ivoire	Mexico	Turkey
Dominican Republic	Morocco	Uganda
Ecuador	Mozambique	Uruguay
Egypt, Arab Rep.	Nicaragua	Venezuela
El Salvador	Niger	Zambia
Ethiopia	Nigeria	Zimbabwe
Ghana	Pakistan	

Appendix B – Aid and Governance Changes

In his 2001 paper, Knack found that aid levels appeared to cause lower changes in the composite ICRG governance scores over the period 1985-1995. Svensson (2000) engages in a very similar exercise and finds that aid raises graft in ethnically fractionalized countries. Tavares (2003) finds that aid reduces corruption. According to Lutz and Lux , however, whether this results holds or not depends upon the instrumentation strategy adopted. While Svensson uses income, the terms of trade and population size as instruments for aid, Tavares focuses on variables that capture geographic and cultural proximity. In this appendix we also show that lagging aid flows also reverse the result.

Note that our dependent variable is updated so that we are measured changes in governance scores from 1985 to 2001. Our control variables are the initial governance score, the proportion of land within 100 km from the coast and GDP per capita in 1985. We also tried using other variables such as ethnic fractionalization, age dependency, illiteracy rates, population and population densities, measures of mineral resources, colonial dummy variables, continental dummies, and so forth, but none of these proved to be significant. Changes in these governance scores are fairly hard to explain, which also makes us somewhat suspicious that aid really has such a strong and systematic effect on governance in either direction. Table B1 reports our findings. In regression 1 we basically re-test Knack's regressions using OLS and we do indeed find that aid dependent countries experienced small improvements in governance (an equally weighted index of government corruption, the rule of law and quality of the bureaucracy) over the period. Rather than instrument for aid flows however, we simply lag them. We do so for two reasons. First, IV estimates are clearly problematic in general, and given that it seems very difficult to effectively explain governance changes it is not easy to be sure that instruments used by Knack or Tavares are valid. Second, it seems such as plausible to define aid dependency as a very long term "disease", so that aid flows in the 1960s, 1970s and all of the 1980s should also explain governance changes in the 1980s and 1990s. When we use two lagged measures of aid flows, we in fact nullify or reverse the Knack result. So as per Tavares, it even seems possible that aid flows may in fact have improved the their recipient's governance. The Knack result therefore seems very sensitive to alternative specifications.

Table B1. Explaining changes in the ICRG governance index from 1985 to 2001.

Regression	1	2	3
Observations	49	49	49
R-squared	0.63	0.61	0.62
Period of aid measure	1985-2001	1960-1989	1970-1989
Variable			
Initial governance score	-1.02***	-1.03***	-1.07***
Proportion of land near coast	5.49*	6.07*	6.06*
Initial GDP per capita	0.15*	0.28***	0.30***
Aid flows	-0.33*	0.39	0.56*

Tables and Figures

Table 1. Explaining aid allocation with donor motivations.

Var. Type	Variable	Normalized Coefficient[#]	t-statistic
Time dummies	1974-77	1.10	1.28
	1978-81	1.64	1.39
	1982-85	1.93	1.57
	1986-89	3.91	3.99
	1990-93	6.51	7.00
	1994-97	6.78	6.86
	1998-01	6.08	5.66
	Constant	5.09	3.94
Developmental	GDP per capita (GDP)	-0.95	-2.10
	Life expectancy	-2.58	-6.42
	BD Policy improvement dummy	-1.13	-3.15
	Democracy decrease dummy	1.88	2.47
	Natural disaster incidence	-1.55	-2.43
Semi-Strategic	Log of Population (LPOP)	7.58	2.94
	Colony dummy	2.51	1.71
	Colony dummy *LPOP	-1.65	-2.26
	Colony dummy*GDP	-0.75	-1.74
Strategic	Warsaw Pact aid dummy	2.77	2.72
	Warsaw Pact aid dummy (lagged)	3.91	4.04
	Middle East dummy	3.58	3.05
	Observations	402	
	R-squared	0.59	
	Adjusted R-squared	0.57	

Notes:

All aid variables exclude humanitarian assistance and food aid. Standard errors are Newey-West robust.

Table 2. Correlations with Developmental and Strategic Aid

	Non-instrumented aid	Developmental Aid Proxy	Strategic Aid Proxy
Growth in GDP per capita	-0.13	-0.25	-0.09
GDP per capita	-0.45	-0.63	-0.32
Tropical Land	0.20	0.36	-0.03
Openness	-0.02	-0.02	0.07
Inflation	0.01	-0.04	0.02
Governance	-0.03	-0.12	0.01
Political Stability	-0.06	-0.08	-0.04
Change in war deaths	-0.14	-0.06	-0.11
Log of Population	-0.25	-0.35	0.12
Population growth	0.26	0.29	0.16
Life expectancy	-0.49	-0.68	-0.27
Non-instrumented aid	1.00	--	--
Developmental Aid	0.52	--	--
Strategic Aid	0.72	--	--

Notes:

Development aid is the fitted values from including developmental variables only (the log of population (which is really semi-strategic), GDP per capita, life expectancy, the natural disasters index, the policy improvement dummy, and the democracy decrease dummy).Strategic aid flows are the fitted values from including semi-strategic and strategic variables only (three colony dummies, Warsaw Pact aid and its lag, and the Middle East dummy).

Table 3. Instrumentation attempts for multilateral and bilateral aid

	Dependent Variable: Variable	Bilateral Aid Inflows		Multilateral Aid Inflows	
		Norm. Coef.	Raw correlation	Norm. Coef.	Raw correlation
Time dummies	1974-77	0.63		0.53	
	1978-81	0.68		0.63	
	1982-85	0.84		0.66	
	1986-89	2.29***		1.70***	
	1990-93	4.87***		2.33***	
	1994-97	3.90***		2.98***	
	1998-01	2.71***		2.86***	
	Constant	3.57***		1.57***	
Developmental	GDP per capita (GDP)	-0.56*	-0.41	-0.39**	-0.48
	Life expectancy	-1.76***	-0.47	-0.81***	-0.46
	BD Policy improvement	0.91*	0.13	0.89***	0.20
	Democracy decrease	-1.10**	-0.15	-0.40	-0.07
	Natural disaster incidence	2.64	0.14	4.85***	0.28
	Log of Population (LPOP)	-0.69***	-0.28	-0.44***	-0.20
Semi-Strategic	Colony dummy	2.06**	0.32	0.46	0.29
	Colony dummy *LPOP	-0.55*	0.13	-0.19	0.15
	Colony dummy*GDP	-0.24	-0.31	-1.28***	-0.48
Strategic	Warsaw Pact aid	2.27***	0.26	0.27	0.11
	Warsaw Pact aid_1	2.95***	0.32	1.18***	0.23
	Middle East dummy	4.77***	0.25	-0.29	-0.03
	Observations	395		395	
	R-squared	0.55		0.56	
	Adjusted R-squared	0.53		0.53	

Notes:

All aid variables exclude humanitarian assistance and food aid. Standard errors are Newey-West robust.

Table 4. Is aid effective "on average"?

Reg. No.	1	2	3	4	5	6	7	8
No. Obs.	405	405	413	394	386	343	150	
R^2	0.40	0.40	0.26	0.43	0.31	0.18	0.43	0.42
Ra^2	0.37	0.37	0.23	0.39	0.27	0.14	0.34	0.36
Sample	All	All	All	All	All	All	Sub-Saharan Africa	Latin America
Regressor	OLS	OLS	OLS	OLS	OLS	GMM (1 lag)	OLS	OLS
Type of aid	ODA	PRODA	ODA Lagged	PRODA Inflows	PRODA (lags)	PRODA	PRODA	PRODA
AID	0.03	0.05			-0.04	0.22		
	(1.40)	(1.75)			(0.78)	(2.14)		
AID_1			0.09	0.11	0.15		0.15	-0.06
			(2.98)	(3.69)	(2.88)		(2.96)	(0.56)
AID_2					-0.72			
					(0.89)			
REPAY_1				-0.61			-0.63	-0.07
				(2.71)			(1.63)	(0.13)

Notes:

All aid variables exclude humanitarian assistance and food aid. Standard errors are Newey-West robust. Control variables are excluded for presentational simplicity only.

Table 4. Testing multilateral vs. bilateral aid inflows, all countries.

Reg. No.	1	2	3	4	5	6	7
No. Obs.	394	341	348	343	301	348	348
R^2	0.43	0.45	0.45	0.45	0.46	0.45	0.45
R_a^2	0.38	0.41	0.42	0.41	0.41	0.42	0.42
Method	OLS, aid inflows	2SLS, aid inflows	OLS, lagged aid	OLS, lagged aid inflows	OLS, twice lagged aid inflows	OLS, lagged aid inflows	OLS, lagged aid inflows
Aid Type:							
Multilateral	0.08	0.07	0.19**	0.17*	0.21		0.25
	(0.92)	(0.49)	(2.06)	(1.91)	(1.98)		(3.16)
Bilateral	0.04	0.03	0.04	0.09*	(0.04)	0.13	
				(1.91)	(1.98)	(3.17)	
Repayments	0.27	-0.13		-2.61***	-0.22	-0.55	-0.38
	(1.21)	(0.64)		(2.62)	(0.88)	(2.92)	(1.96)

Notes:

All aid variables exclude humanitarian assistance and food aid. Standard errors are Newey-West robust. Control variables are excluded for presentational simplicity only.

Table 5. Weights in the donor motivation indices

Aid Type	All aid		Bilateral Aid	
Index	Semi-Strategic	Strategic	Semi-Strategic	Strategic
GDP per capita (GDP)	+0.0005	+0.0005	+0.0003	+0.0003
Life expectancy	+0.2255	+0.2255	+0.1540	+0.1540
Log of Population (LPOP)	-0.1731	-0.1731	-0.0603	-0.0603
Colony dummy	+14.477		+9.648	
Colony dummy *LPOP	-0.0009		-0.0001	
Colony dummy*GDP	+0.7543		+0.5531	
Warsaw Pact aid	+2.7654	+2.7654	+2.2728	+2.2728
Warsaw Pact aid_1	+3.9071	+3.9071	+2.9549	+2.9549
Middle East dummy	+3.5772	+3.5772	+4.7718	+4.7718

Notes:
Weights are derived from the specifications in Table 3, but with non-normalized dependent variables.

Table 6. Tests of donor motivation indices.

Motivation Index	Strategic		Strategic		Semi-Strategic		Semi-Strategic	
Aid type	All aid		Bilateral aid		All aid		Bilateral aid	
Observations	394	394	348	348	394	394	348	348
Aid_1	0.11	0.11	0.14	0.14	0.12	0.13	0.12	0.13
	(2.52)	(3.61)	(2.55)	(2.53)	(3.91)	(4.01)	(3.59)	(2.71)
Aid_1*Motivation Index_1	-0.02	-0.04	-0.05	-0.06	-0.03	-0.05	-0.05	-0.07
	(1.40)	(2.10)	(1.72)	(2.74)	(1.73)	(2.02)	(2.23)	(2.31)
Motivation Index_1		0.48		-0.03		0.19		0.32
		(1.58)		(0.16)		(0.75)		(1.09)
Multilateral Aid			0.16	0.16			0.13	0.11
			(1.77)	(1.76)			(1.39)	(1.24)
Repayments_1	-0.51	-0.46	-0.59	-0.61	-0.58	-0.49	-0.45	-0.43
	(2.15)	(1.91)	(2.67)	(2.74)	(2.59)	(2.14)	(2.01)	(1.94)
R-squared	0.43	0.43	0.46	0.46	0.43	0.43	0.47	0.47
Adjusted R-squared	0.40	0.40	0.43	0.43	0.40	0.40	0.43	0.43
Minimum effect of aid	0.04	-0.03	-0.02	-0.06	0.00	-0.08	-0.10	-0.17
Maximum effect of aid	0.15	0.20	0.23	0.25	0.19	0.25	0.24	0.30
Mean Effect of aid	0.11	0.11	0.14	0.14	0.12	0.13	0.12	0.13

Notes:

The motivational indices are normalized, so that the coefficients represent the change in aid effectiveness conditional upon a change of one standard deviation in the motivational index in question. Indices weightings are given in Table 5. All aid variables exclude humanitarian assistance and food aid. Standard errors are Newey-West robust. Control variables are excluded for presentational simplicity only.

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