Aid, policies and growth, *redux*

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Abstract

This paper re-assesses research by the World Bank on aid effectiveness. It takes into account existing critiques as well as recent developments in the literature on the determinants of cross-country growth such as systemic differences in the responsiveness of growth to policy reform between sub-Saharan Africa and the rest of the developing world. We find that: (i) when we include country-fixed effects estimates, the central World Bank claim that aid only works in good policy environments collapses; (ii) nevertheless, in sub-Saharan Africa, evidence for this argument remains: aid raises growth only in the presence of a good policy environment; (iii) in countries outside of sub-Saharan Africa, aid raises growth independent of policy; and (iv) changing the manner in which aid is measured, changing the functional form, and using different measures of policy do not affect these results. We then turn attention to whether criteria for allocating aid are sensitive to these econometric concerns. We find that (i) while the magnitudes of marginal aid efficiencies – the number of people lifted out of poverty by an increase in aid – vary widely depending on whose econometric results are used to compute them, (ii) nevertheless, different econometric analyses of the aid-growth relationship tend to generate broadly similar *rankings* of countries by marginal aid efficiency.

1. Introduction

Controversy regarding the efficacy of overseas development assistance has heated up in recent years, even if the volume of flows has not appreciably increased (OECD, 1996; World Bank, 2001). A potential problem facing international organizations' new-found enthusiasm for foreign aid is that much (but certainly not all) past research showed that aid had little effect on economic growth or poverty reduction in the receiving countries. Champions of aid have taken comfort from recent research undertaken largely by World Bank staff and summarized in the Bank's (1998) publication, *Assessing Aid*: these researchers find that aid indeed favors economic growth, but only in the presence of a good policy environment – one characterized by small budget deficits, low inflation, and openness to foreign trade (Burnside and Dollar, 2000; Collier and Dollar, 2001, 2002; World Bank, 1998). Earlier studies had not detected this effect because they had failed to control for the quality of policy-making. Further, because growth reduces poverty – and because the cross-country allocation of existing aid flows does not take such criteria into account – the impact of current aid on poverty reduction is considerably smaller than its potential.

This work is proving to be enormously influential. In recent months the Dutch, German, British and Canadian governments have either announced that they will re-focus their development assistance so that poor countries with good policies will receive larger bilateral aid flows or are considering such measures (see Netherlands, 2000; Federal Ministry for Economic Cooperation and Development (BMZ), 2001; Department for International Development, 2000, p. 86; and Canadian International Development Agency, 2001). Currently, these four countries spend $14 billion on official development

assistance, which is nearly one-quarter of all aid flows from OECD Development Assistance member countries (OECD, 2001).

Given the magnitude of aid spending by these donors – and the possibility that other donors will also draw on these results when determining aid allocations[2] – one would expect that the findings by Burnside, Collier and Dollar have been subject to a number of replication studies and found to be robust. This is not the case. There is a substantial literature criticizing their work, including Beynon (2001), Dalgaard and Hansen (2001), Guillaumont and Chauvet (2001), Hansen and Tarp (2001, 2000), Lensink and White (2001, 2000), and Morrissey (2000). These critiques claim that the Burnside-Collier-Dollar work suffers, *inter alia*, from the following weaknesses. (i) *Incorrect functional-form specification*. Lensink and White (2000, 2001), Dalgaard and Hansen (2001) and Hansen and Tarp (2001) all argue that aid-growth specifications should include a quadratic term whereas Burnside and Dollar (2000) argue that the significance of the quadratic term results from the inclusion of five outlier observations. (ii) *Incorrect econometric specification*. Hansen and Tarp (2001) argue strongly in favor of removing the impact of country-specific fixed effects via differencing. (iii) *Varying growth-poverty elasticities*. Lensink and White (2000) note that growth elasticities of poverty reduction are not constant across countries (as assumed by Collier and Dollar): high-inequality countries will have lower elasticities thus blunting the poverty-reducing impact of higher growth. A common theme across these critiques is that the claim that "aid works, but only in a good policy environment" is incorrect.

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[2] The former US Treasury Secretary, Paul O'Neill, in the past a vocal skeptic of aid flows, was quoted in 2002 saying that "aid works, in the right environment" (*Washington Post*, 2002).

This paper contributes to the re-assessment of the Bank's work on aid effectiveness, but departs from existing critiques in three ways. First, unlike all previous critiques save Dalgaard and Hansen (2001), we use the Burnside and Dollar data set. Thus, any differences between our findings and those found in *Assessing Aid* and Burnside and Dollar (2000) cannot be due to changes in variable definition or the inclusion/exclusion of particular countries.[3] Second, we take into account recent work that suggests that there are systemic differences in the responsiveness of growth to policy reform between sub-Saharan Africa and the rest of the developing world (Collier and Gunning, 1999). Third, we explore whether attention to these critiques substantively changes the lessons donor governments might draw from these studies for allocating aid across countries.

Section 2 of our paper focuses on issues relating to the robustness of the Burnside-Collier-Dollar methodology. We find that: (i) the core Burnside-Dollar finding, that aid only works (in the sense of increasing per capita GDP growth) in a good policy environment, is not robust to the inclusion of country fixed effects; (ii) nevertheless, in sub-Saharan Africa, the Burnside-Dollar thesis remains well-founded even with fixed effects: aid raises growth only in the presence of a good policy environment; (iii) in countries outside of sub-Saharan Africa, aid raises growth independent of policy; and (iv) changing the manner in which aid is measured, changing the functional form, and using different measures of policy do not affect the basic Burnside-Collier-Dollar results.

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[3] An alternative strategy along these lines is adopted by Easterly *et al.* (2003), who use the Burnside-Dollar data set, as we do, but update it by adding another period of observations. Their analysis attracted significant attention: see the story about their work in the *New York Times* (Eviatar, 2003).

The third section explores whether the cross-country aid allocations that would be generated by these varying studies are sensitive to these econometric concerns. We find that: (i) the magnitudes of marginal aid efficiencies – the number of people lifted out of poverty by an increase in aid – vary wildly depending on whose econometric results are used to compute them; but (ii) different econometric analyses of the aid-growth relationship tend to generate broadly similar *rankings* of countries by marginal aid efficiency.

2. Exploring econometric robustness

2.1. Introduction

The methodology adopted by Burnside, Collier and Dollar has two stages: a) construct an index measure of "good" policy; and b) incorporate this index of policy into a regression where growth in per capita income is the dependent variable and aid, policy and other variables believed to affect growth are the regressors. In Burnside and Dollar (2000), the regression equation takes the following form:

$$G_j = c + b_1 X + b_2 P_j + b_3 A_j + b_4 A_j P_j \qquad (1)$$

for each country j, where G_j is the growth rate of per capita GDP, c is a constant, P_j is the "policy score" rating the quality of the policy environment in j, A_j is the aid-to-GDP ratio in country j, and X is a set of other variables believed to affect growth rates. Good policy P in the Burnside-Dollar work refers to stable macroeconomic policy: lower government deficits, low inflation and openness to trade. Burnside and Dollar find that b_3 is not

significantly different from zero – a finding consistent with the absence of any direct effect of aid on the rate of economic growth – and b_4 is significant and positive; that is, aid increases growth only in the context of a "good" policy environment. Collier and Dollar (2001) use a different index of the "quality" of policy, the Country Policy and Institutional Assessment (CPIA). This is potentially more attractive, as it aggregates more than twenty policy indicators (versus Burnside and Dollar's three), and is available for over 100 countries (versus Burnside and Dollar's 56). A further attraction is that in addition to macro policy variables, it also includes characteristics such as the extent to which government interventions actually reach the poor. The World Bank, unfortunately, does not make the CPIA publicly available.[4] (Dalgaard et al. (2002, p. 20) nevertheless argue that the CPIA is questionable on several counts.) Consequently, we base our results on the work of Burnside and Dollar, who graciously made their data set available to us and answered several queries about their estimates.

2.2. The Burnside and Dollar (2000) methodology

The first step in Burnside and Dollar's methodology is to construct a policy index. To do so, they estimate a variant of equation (1), namely:

$$G_j = c + b_1X_1 + d_1Z_1 + d_2Z_2 + d_3Z_3 \qquad (2)$$

where Z_1 is the budget surplus relative to GDP (following work by Easterly and Rebelo (1993) on the impact of budget surpluses and deficits on growth), Z_2 is the inflation rate (following Fischer (1993)) and Z_3 is a dummy variable for trade openness developed by

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[4] Our initial interest in this topic was sparked by a request by CIDA, the Canadian bilateral aid agency, to explore the robustness of the Bank's work as an input into a review of Canadian aid-allocation policy. Despite this context for our research, the Bank declined an official request made, on our behalf, by the Government of Canada for the CPIA data.

Sachs and Warner (1995). Open economies are defined as those where tariffs on machinery and materials are lower than 40 percent *or* the black-market premium on the foreign-exchange rate is less than 20 percent, and where pervasive government control of major traded goods is absent. They estimate this regression for 56 countries over the period 1970-1993, obtaining estimates of c, b_1, d_1, d_2, and d_3. The policy index for country j is based on the formula:

$$P_j = c + d_1 \text{ x budget surplus} + d_2 \text{ x inflation} + d_3 \text{ x openness} \qquad (3)$$

Burnside and Dollar argue that there are two advantages to this approach: it summarizes policy stance in a single measure, while allowing the data to determine what weights should be attached to the individual components. Second, by adding the constant, "the index can be interpreted as a country's predicted growth rate, given its budget surplus, inflation rate and trade openness, assuming that it had the mean values of all other characteristics." (Burnside and Dollar, 2000, p. 855).

We also note that Burnside and Dollar, as well as Collier and Dollar, depart from work by previous researchers in their measurement of aid. Instead of using official development assistance (ODA), which comprises grants and loans and is reported by donor governments to the OECD's Development Assistance Committee every year, they use "effective development assistance" (EDA) that computes aid as the sum of grants and the grant-equivalent of official loans. (See Chang, Fernández-Arias and Serven (1998) for details). EDA data are highly skewed. The mean level of aid over the 23-year period considered here is 1.48 percent of the receiving country's GDP, but the median level is only 0.78 percent of GDP and indeed, the 75th percentile of the distribution of aid levels is 2.2 percent of GDP.

The second step is to estimate equation (1). Burnside and Dollar include the following country characteristics as components of X: initial level of GDP, ethnic fractionalization, assassinations, ethnic fractionalization multiplied by assassinations, a fixed measure of institutional quality, the ratio of the money supply (M2) to GDP lagged one period, and dummy variables denoting sub-Saharan Africa and East Asia. They estimate this equation using ordinary least squares (OLS), corrected for heteroscedasticity. They also report results using two-stage least squares (2SLS), treating aid and the aid-policy interaction term as endogenous.

2.3. Four questions

We consider whether Burnside and Dollar's results are substantively altered if we (i) use different econometric techniques, (ii) consider whether the impact of aid on growth differs between countries in sub-Saharan Africa and elsewhere, (iii) change the manner in which aid is measured, and (iv) use a different functional form for the specification of aid (for example, including both aid and aid squared as explanatory variables). In this section we consider each of these robustness checks in turn.

2.3.1 Is the Burnside and Dollar model correctly specified and estimated?

There is a vast literature that attempts to explain the determinants of cross-country variations in economic growth (see Temple (1999) for a summary). The Burnside and Dollar approach is an example of a hybrid approach in which variables derived from standard economic growth theory, such as initial GDP per person, are augmented in an *ad*

hoc fashion by country characteristics that are presumed to influence growth via their impact on total factor productivity growth.

A limitation of this approach is the *ad hoc* fashion in which certain country characteristics are included and others omitted. There is a wide array of country characteristics that various researchers have argued may affect growth. Among these factors, for example, Sala-i-Martin (1997) and Sachs and Warner (1997) include geography (landlocked or with access to the seas), the general disease environment as captured by latitude (typically, tropical countries are plagued by a greater number of virulent diseases such as malaria), past history (the nature of the colonial government will, for example, have a strong bearing on the legal system that develops), natural resource endowments (which may encourage rent seeking), ethnicity (which may be associated with civil strife) and the identity of one's neighbors (which may stimulate or reduce growth). One approach could be to – perhaps haphazardly, perhaps purposively – include some or all of these variables, producing regression results running into the tens of thousands.

A further limitation is that it is rare for data on all characteristics to be available for all countries; as already noted, Burnside and Dollar limit their sample to only 56 in the face of limited data availability. One could readily construct explanations as to why this process of sample selection is non-random. For example, a covariate used in these regressions is the extent of ethnic fractionalization, a measure based on work by Russian anthropologists published in the 1960s (see Easterly and Levine, 1997). This measure is not available for countries such as Bangladesh, Eritrea or Namibia, all of which formed subsequently as a result of successful secession movements. If we regard selection into

this sample as resulting from country-specific causes – such as independence resulting from successful secession movements – then the results of our regressions will be biased no matter how many times they are estimated. A solution that addresses both concerns is to re-estimate the Burnside and Dollar results using country-level fixed effects (FE) and an instrumental-variables/fixed-effects (IV-FE) model that treats both aid and the aid-policy interaction as endogenous.[5]

Table 1 provides the results of re-estimating Burnside and Dollar's results but adding in country fixed effects. The first two columns replicate the core findings of Burnside and Dollar (2000, Table 4, columns (5) and (6)). The latter two columns report the FE and IV-FE results. Note that we are using exactly the same data as Burnside and Dollar. All that changes is that we account for these country-specific fixed effects. The *F*-statistic is statistically significant at the 5 percent level, indicating that omitting these country-specific dummy variables will produce biased findings. As before, good macroeconomic policy is associated with more rapid rates of growth. If anything, our results in columns (iii) and (iv) of Table 1 indicate that Burnside and Dollar *underestimate* the impact of good macro policy. Aid, by itself, continues to have no statistically significant impact on growth. But the claim that "aid works, but in a good policy environment" no longer holds. Once country-level fixed effects are adequately accounted for, the estimated coefficient on the aid-policy interaction term – b_4 in equation (1) – is no longer statistically significant.[6]

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[5] Lu and Ram (2001) independently suggested the inclusion of country-specific intercepts in an aid-growth equation, although they did not extend their results to the case of regional disaggregation we explore in the following section.

[6] Easterly *et al.* (2003) re-run the basic Burnside-Dollar models with an additional period of data and find that the estimate of b_4 is no longer significant.

It may be argued, of course, that the fixed-effects estimates in Table 1 create more problems than they solve. Given that the fixed-effects regressions can be interpreted as a dynamic panel in which the number of time-series observations is small, they are potentially subject to the biases identified by Nickell (1981). If there is only limited time-series variation in aid flows and quality of macroeconomic policy, then a fixed-effects approach might be predisposed to yield insignificant parameter estimates.[7] The trade-off is one between reducing bias (with fixed effects) and reducing efficiency (with least squares). A compromise is to estimate the regressions separately for different regions, thus still drawing on cross-sectional variation to identify parameters. We explore this approach in the following section.

The preferred approach of Arellano and Bond (1991) to the problem of biased OLS estimates in the context of short panels is to estimate an equation of growth on lagged growth and differences in the variables that affect growth. The twice-lagged level of initial income can be used as an instrument for lagged growth in this specification and twice-lagged levels of any other endogenous variables can be used as instruments as well. Applying this Arellano-Bond approach to the Burnside-Dollar data -- for the whole sample, the sub-Saharan African subsample, or the low-income-countries subsample -- yields no significant results for aid nor for the interaction of aid and policy. This underscores the caution with which aid-growth results should be interpreted.[8]

2.3.2 Does aid have the same effect in sub-Saharan Africa as it does elsewhere?

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[7] On this, see the discussion in Brock and Durlauf (2001).

[8] These results are not included with this paper, but are available from the authors. We are grateful to a referee for suggesting this approach.

Table 2 shows that, in terms of aid flows, there is an almost complete bifurcation between sub-Saharan Africa (SSA) and the rest of the world. For the Burnside-Dollar sample of 56 countries, the EDA data indicate that nearly all the high-aid recipients are in sub-Saharan Africa (SSA). In the bottom quartile, there is only one sub-Saharan African country and thirteen non-SSA countries; in the second quartile there are two SSA and twelve non-SSA countries; in the third quartile there are six SSA and eight non-SSA countries; in the top quartile, finally, we find twelve SSA and two non-SSA countries.

This has two implications. First, the debate over whether aid should be entered linearly or as a quadratic may be missing a more important point. "Diminishing returns to aid" may merely reflect the fact that countries that receive relatively large amounts of aid relative to GDP are more likely to be located in sub-Saharan Africa. Second, it suggests that merely including a dummy variable for sub-Saharan Africa in cross-country regressions may not adequately control for special characteristics of the economy in that region. Put another way, the assumption of parameter homogeneity underlying the regression exercise may fail to hold. This reasoning receives substantial support from the discussion in Collier and Gunning (1999, especially pp. 101-105) to the effect that good policy by itself in sub-Saharan Africa has little growth impact, because such policy reforms are not regarded by agents as credible.[9] These points, taken together, suggest that it might be instructive to separate out sub-Saharan Africa from the rest of the world. In Table 3, therefore, we re-run these models, but separate sub-Saharan Africa from the rest of the sample.

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[9] The problem of aid effectiveness in sub-Saharan Africa is addressed in Hadjimichael et al. (1995) and Gomanee et al. (2002).

Table 3 demonstrates that the estimated coefficient on the aid-policy interaction is positive and significant in the sub-Saharan Africa models. If one is suspicious of the OLS result, note that adding in country fixed effects yields an almost identical parameter estimate.[10] If one is worried about the possible endogeneity of aid and aid-policy in the OLS model, note that the fixed-effects/instrumental-variable model for sub-Saharan Africa yields a significant coefficient on the aid-policy interaction, one that is furthermore nearly twice the magnitude of the OLS estimate. The estimated coefficient on the aid variable is not significant for any of the models. Moreover, the policy variable is not significant in any of the models. In sub-Saharan Africa, the Burnside-Dollar (2000) story regarding the aid-policy interaction and growth, is correct (although their story on policy and growth is not).

In the rest of the developing world, however, the estimated coefficient on the aid-policy interaction is never statistically significant. Policy alone is always positive and strongly significant. The effect of aid alone is significant and positive once we control for country fixed-effects; that is, in the developing world outside of sub-Saharan Africa, aid raises growth, independent of policy.

2.3.3 Does functional form matter?

In the Burnside and Dollar work, aid enters the regression equation (1) linearly. In preliminary work, we entered aid in non-linear terms, considering squared, cubic and quartic terms, as well as inverses, inverses squared and splines. Such specifications

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[10] The fixed-effects parameter estimate for the aid-policy interaction is only marginally significant; this may be because adding in the country fixed effects dramatically reduces the degrees of freedom in the

invariably produce statistically insignificant results; and over the range of aid flows we observe in these data, there is no *a priori* reason to believe that such alternative specifications would produce more robust results. Hence, these are not discussed further.

2.3.4. Does it matter how we measure aid?

Most assessments of the impact of aid on growth measure aid flows using official development assistance (ODA) as a share of GDP. As noted in Section 2.2 above, ODA comprises grants and loans, while effective development assistance (EDA) – used by Burnside and Dollar (2000) and Collier and Dollar (1999, 2000) -- computes aid as the sum of grants and the grant-equivalent of official loans. EDA is likewise expressed as a proportion of GDP in the receiving country, and is available for the period 1975-1995.[11] In preliminary work not reported here, we substituted ODA in place of EDA and obtained a similar pattern of results to those described here. (For further discussion, see Dalgaard and Hansen (2001)).

3. Robustness of policy recommendations

3.1. Introduction.

Collier and Dollar (2001) take the Burnside-Dollar analysis assessed above to its logical conclusion: a set of recommendations regarding how current aid flows should be reallocated in order to further reduce poverty. Given estimates of the impact of

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smaller sub-Saharan-African sample. Also, the low *F*-statistic on the sub-Saharan Africa results suggests that these can be discarded in favor of the simple OLS findings.

[11] The methods for calculating EDA are set out in Chang, Fernández-Arias and Serven (1998). We use values of EDA that extend back to 1970, courtesy of Craig Burnside and David Dollar who kindly made these unpublished data available to us.

additional aid flows on economic growth rates in aid-receiving countries, $\partial G_j / \partial A_j$, Collier and Dollar show that the marginal efficiency of aid in country j, λ_j, is given by:

$$\frac{\partial G_j}{\partial A_j} \alpha_j \frac{h_j}{y_j} = \lambda_j \qquad (4)$$

where λ_j is interpreted as the number of people that would be permanently lifted out of poverty by an increase in aid to country j of $1 million (US). h_j is the poverty rate in country j: the fraction of the population in country j with incomes lower than $2 (US) per day. y_j is per capita income in country j. Finally, α_j is the responsiveness of the poverty rate to changes in the growth rate. Collier and Dollar assume, based on the research of Ravallion and Chen (1997), that $\alpha_j = 2$ for all countries. The definition of λ_j (4) indicates that the efficiency of aid to a given country – its impact on poverty reduction – is higher if: (a) growth responds well to aid inflows (that is, if $\partial G_j / \partial A_j$ is high); (b) the poverty rate h_j is comparatively high; and (c) per capita income y_j is comparatively low.

3.2. Estimating marginal aid efficiencies.

Any growth regression like equation (1) that includes aid as an explanatory variable will, together with equation (2), automatically generate estimates of marginal aid efficiencies for any aid-receiving country. If the marginal aid efficiency of, say, Malaysia, is lower than that of, say, Eritrea, then reducing aid flows to the former by $1 million (US) and increasing flows to the latter by the same amount will effect a net reduction in global poverty: the marginal increase in the number of Malaysians with incomes below $2 (US)

per day will be smaller than the reduction in the number of Eritreans with incomes below the same poverty line. Clearly, then, the policy recommendations that result from the econometric work reviewed in Section 2 depend critically on the reliability and credibility of the statistical results underlying estimates of $\partial G_j / \partial A_j$. Our assessment casts some doubt on any reallocation recommendations made on the basis of existing work. How sensitive are such recommendations to the particular estimates of $\partial G_j / \partial A_j$ used?

Table 4 reports several estimates of λ_j for selected countries. For the purposes of this exercise, we keep α constant across countries; the "varying-α" estimates will be described in Section 3.3.1 below. The first of these is based on our FE models, from the second and third panels of Table 3. In our model, aid efficiency is a function of policy in sub-Saharan Africa, but not in other parts of the developing world. The second set of estimates is based on Burnside and Dollar (2000), using the results of their OLS model (refer to our Table 1, column (i)). For them, aid efficiency is a function of policy in all countries. We follow the same procedure to compute the marginal aid efficiencies implied by the econometric work of Hansen and Tarp (2001, Table 3.2), based on their GMM model for the same 56 countries and most of the same explanatory variables.[12] For them, the marginal effect of aid on growth is a function of aid-to-GDP ratios. In addition, we report the estimated aid efficiencies reported by Collier and Dollar, for the purpose of comparison. Collier and Dollar's estimates of marginal aid efficiencies are a function of

[12] We use the marginal effect of aid measured at the median from Hansen and Tarp (2001). The regressions in their paper are dynamic. Thus in the short run (one to four years), there is almost no effect; however, keeping the higher level of aid for eight years will lead to higher numbers. Our estimates use only the short-run effects on the grounds that they are more directly comparable to the other estimates.

policy (like Burnside and Dollar's), but also of current aid flows to each country, so that, all other things remaining the same, the impact of aid on growth will be lower in a country where aid inflows are already high.

Two factors should be taken into account when considering the estimates of λ_j presented in Table 4. First, where aid inflows or macroeconomic policy stance are used to compute these estimates, the data used are averages for the years 1990-1993 (with the exception of the Collier-Dollar estimates, explained below.) Thus, in particular, some countries with favourable policy stances in the early 1990s might have witnessed a deterioration of policy discipline subsequently. The figures reported in Table 4 should *not* be used for allocating 2002 aid dollars, and merely provide an illustration of the technique.[13] Second, Collier and Dollar's estimates are based on data from 1995-1997, and are therefore not strictly comparable with the other estimates in the table.

Despite these caveats, all of the numbers in Table 4 are measured in the same units: people with incomes below $2 (US) a day who would be permanently lifted out of poverty. The table immediately reveals that the *magnitude* of the relationship between aid and poverty reduction is quite different depending on whose econometric estimates are used. In general, Hansen and Tarp's analysis suggests the largest poverty-reduction impact per $1 million (US) of aid, while Burnside and Dollar's produce the smallest impact. For all estimates, incremental increases in aid flows to those countries with higher GDP per capita will have a relatively smaller impact on poverty reduction: thus, estimates of the marginal aid efficiency of an additional $1 million (US) in aid to Korea,

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[13] The principal constraint for providing more up-to-date estimates of these marginal aid efficiencies is that Sachs and Warner's (1995) series on openness to trade, a component of the Burnside-Dollar policy index, ends in 1993.

for example, ranges from 34 to 93 people lifted out of poverty, versus 550 to 5,703 people in Ethiopia.

Some divergences in the estimates result from the different variables that influence aid's impact on growth. Thus, while Burnside and Dollar's (and Collier and Dollar's) estimates are based on the notion that aid efficiency is higher where the macro policy stance is better, Hansen and Tarp's do not. In Zambia, therefore, where early-1990s policy was poor, Burnside and Dollar's results would indicate that further aid would actually *increase* poverty; Hansen and Tarp's results, in contrast, would indicate a sizeable reduction in poverty, as the aid efficiency calculated on the basis of their results is solely a function of the poverty rate and average income. Our results mirror Burnside and Dollar's for sub-Saharan Africa – our econometric evidence suggests that aid works only in good policy environments on that continent, as does Burnside and Dollar's – but our results suggest that the marginal impact of aid in that region will be higher than in Burnside and Dollar's analysis. Thus, consider Ethiopia, a very poor country with reasonably good macro policy in the early 1990s. Burnside and Dollar's estimates suggest that 550 Ethiopians would be lifted out of poverty with an increase in $1 million (US) in aid, while our results suggest the number would be 2,343 Ethiopians. (In fact, such statements understate the overall impact of aid implied by these parameter estimates since some of those who remain in poverty -- or who started out better off -- will also see their circumstances improve.) Finally, our results diverge from Burnside and Dollar's outside of sub-Saharan Africa. In Nicaragua, where macro policy in the early 1990s was very poor, Burnside and Dollar would predict an increase in poverty with an increase in aid; our results suggest instead a big decrease in poverty in Nicaragua, given that,

according to our results, aid reduces poverty independent of policy outside of sub-Saharan Africa.

3.3. Responsiveness of poverty reduction to growth ("α ")

Collier and Dollar (2001) have used a value of 2 for α for all countries: a one-percent increase in the level of GDP per capita reduces poverty by two percent. This is based on the work of Ravallion and Chen (1997).[14] But α is likely to vary from country to country. For example, where inequality is greater, growth of average income will have a smaller impact on poverty reduction. Consider a simple example with two countries, *A* and *B*. Suppose that GDP per capita is the same in both countries, and that inequality is greater in *A*. Then *A*'s poverty rate will also be higher. If *everybody's* income rose by precisely one percent in both countries – so that per capita GDP grew by one percent – the impact on poverty reduction would be greater in *B*. Intuitively, more of the poor were initially "close" to the poverty line in *B*, and thus lifted out of poverty, while poverty remains deep in *A*. A second reason that α would be lower in *A* is that the underlying mechanisms that produced higher inequality in *A* in the first place – a weak social safety net, imperfect capital markets, an unequal distribution of wealth – mean that any income growth in *A* is unlikely to be equally distributed, further dampening the effect on poverty reduction.

Ravallion (2001) suggests that α_j can be rewritten as

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[14] See also the lower panel of Figure 3.3 from the 2000/2001 *World Development Report*, which tells essentially the same story. A best-fit line through the observations has a slope of about -2: the poverty rate declined about twice as fast as per capita consumption levels rose. But there is considerable variation around the best-fit line (World Bank, 2001a, p.47).

$$\alpha_j = \bar{\alpha}(1 - GINI_j) \qquad (5)$$

for country j, where $GINI_j$ is the Gini coefficient of inequality in j, and $\bar{\alpha}$ is a constant. We gathered data on income inequality for 47 of the 56 countries in the Burnside-Dollar data set[15] and calibrated $\bar{\alpha}$ so that the average value of α equals 2. Then, using equation (4), we recalculated the marginal aid efficiencies in Table 6. For the columns labeled "variable-α", one can see that for countries with below-average inequality (like Ghana or India), this raises aid efficiency; for countries with above-average inequality (like Brazil or Guinea-Bissau), this reduces aid efficiency.

3.4. Ranking aid recipients.

It might be argued that, whatever their precision as point estimates, all of these statistically-based estimates of aid efficiencies would lead to fundamentally similar country allocations of aid. One way to verify that is to consider the correlation among the country-specific aid efficiencies generated by each study. That is, if marginal aid efficiencies are lower in Zambia, say, and higher in Ethiopia, than elsewhere, regardless of which set of estimates one considers, then perhaps aid flows should be increased toward Ethiopia, relative to Zambia. If this is the case, then the correlation among estimates will be high. Table 5 reports these correlation coefficients for those countries from the Burnside-Dollar dataset for which data are available to compute these aid efficiencies. The Pearson correlation coefficients between our estimates and those based on Burnside-Dollar or Hansen-Tarp range from around 0.6 to 0.8; other correlations are

[15] The data used are from the most updated version of the data set described in Deininger and Squire (1996); updates are available from the World Bank's Web site.

as low as 0.4. This means that highly aid-efficient country according to one study might not necessarily be terribly aid-efficient according to another. This poses a problem for policy-makers.

The lower panel of Table 5 reports Spearman rank-correlation coefficients among the various estimates of aid efficiency. In every case, the Spearman coefficient is higher than the corresponding Pearson coefficient, suggesting that all four estimates of aid efficiency would generate broadly similar ranking of aid recipients. Accordingly, in Table 6, we have ranked the "Top 10" countries for each of the four series of estimates of marginal aid efficiencies (for both $\alpha = 2$, and for variable α's). Two things should be borne in mind when looking at these rankings. First, data were available to compute the aid efficiencies of only 40 of the 56 countries in the Burnside-Dollar data set. Thus, that Ghana tops Burnside and Dollar's list (and ours) does not mean that it was the most efficient place to channel aid in the early 1990s; the most efficient country might have been one left off the list because of the absence of available data. Second, these rankings are based on data from the early 1990s (or, in the case of Collier and Dollar, the mid-1990s); a more up-to-date ranking (impossible given data constraints) would certainly differ.

If we consider the aid-efficiency "Top 10" proposed by the constant-α and Collier-Dollar estimates, we note that five countries (Ghana, Malawi, Ethiopia, Madagascar and Kenya) appear on all four lists and two (Honduras and India) appear on three lists. In very gross terms, then, there is a group of countries to which all of the econometric estimates point as productive recipients of aid spending. In regional terms, there is moreover a predominance of sub-Saharan African countries, as well as especially

poor Latin American (e.g., Bolivia, Guatemala, Honduras) and Asian countries (e.g., India, Pakistan) on all of the Top-10 lists. Allowing aid efficiency to vary with income inequality changes the ranking only slightly. Similarly, looking at the ten *least*-efficient aid recipients in the lower panel of Table 6, there is a similarity of ranking: five countries (Korea, Malaysia, Venezuela, Colombia and México) appear in all four lists.[16]

4. Conclusions

In this paper, we have revisited the debate over the inter-relationships between aid, policy, growth and poverty reduction. We find that: (i) when we introduce country fixed effects, the core Burnside-Dollar finding, that aid only works (in the sense of increasing per capita GDP growth) in a good policy environment, collapses; (ii) nevertheless, in sub-Saharan Africa, the Burnside-Dollar thesis remains well-founded: aid raises growth only in the presence of a good policy environment; (iii) in countries outside of sub-Saharan Africa, aid raises growth independent of policy; and (iv) changing the manner in which aid is measured, changing the functional form, and using different measures of policy do not affect the basic Burnside-Collier-Dollar results.

To assess the policy recommendations that flow from the statistical analysis, we computed the "marginal aid efficiency" for a large number of countries, based on three statistical models: ours (as reported in the second and third panels of Table 3 of this

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[16] More problematic are very poor countries that appear on a Top-10 and Bottom-10 list, like Nicaragua and Zambia. These exceptional cases highlight differences in the estimation of the four different series of marginal aid efficiencies. Zambia has poor policy (which makes it an unlikely recipient in the Burnside-Dollar model) and is in sub-Saharan Africa (which tends to amplify the negative effect of bad policy in our model); at the same time, it is a high-poverty, low-income country, which makes it a promising candidate for aid in the Hansen-Tarp model. Nicaragua, on the other hand, has poor policy (which reduces its aid efficiency in the Burnside-Dollar model, but not in ours), and it is a high-poverty, low-income country, again favoring aid efficiency in the Hansen-Tarp model.

report), Burnside and Dollar (2000), and Hansen and Tarp (2001). (In addition, we compare these to marginal aid efficiencies computed independently by Collier and Dollar (2001).) The marginal aid efficiency measures the number of people that would be lifted out of poverty by an incremental $1 million (US) of aid in a given country. Three broad trends emerge. First, the magnitude of poverty reduction implied by the various econometric results varies considerably – while one set of results might suggest that an additional $1 million (US) in aid to Ethiopia would lift 550 people out of poverty, another set of results puts that number at 5,703. Second, the correlation among the different estimates is on the order of 0.7. This indicates that there are many countries that one statistical model suggests are especially efficient recipients of aid, but another model suggests are poor targets for aid. But, third, a correlation coefficient of 0.7 also suggests that, more than half the time, a country highlighted as a promising recipient of aid by one model will also be highlighted by another model.

To conclude, we suggest that the focus of much of the existing debate is misplaced. In our view, the most important question in the context of this debate is whether, given limited resources, there are gains – in the sense of increased poverty reduction – associated with allocating aid flows according to some criteria. In this context, discussions about econometric technique, choice of regressors and sample selection matter only insofar as they lead to different allocation criteria. Our findings suggest that aid will have the greatest effect on poverty reduction if it is directed: (a) toward sub-Saharan Africa, and to a lesser degree, to South Asia; (b) within sub-Saharan Africa, toward countries with sound macro (fiscal, monetary and trade) policy; and to (c)

countries with high rates of absolute poverty and lower levels of income inequality in all regions.

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Table 1: Testing the Burnside-Dollar (2000) results for robustness to country fixed effects

	Burnside and Dollar (2000, Table 4, columns (5) and (6))			
	(i) Ordinary least squares	(ii) Two stage least squares	(iii) Country fixed effects	(iv) Instrumental variables: Country fixed effects
Policy	0.71	0.73	1.08	1.17
	(3.63)**	(3.60)**	(3.36)**	(2.95)**
Aid	-0.02	-0.32	0.36	-1.06
	(0.13)	(0.87)	(1.24)	(1.07)
Aid x policy	0.19	0.18	-0.14	-0.28
	(2.61)**	(1.63)	(0.80)	(1.19)
F statistic on country-level fixed effects	-	-	1.69**	1.45**

Notes:
1. Regressions control for initial level of GDP, ethnic fractionalization, assassinations, ethnic fractionalization x assassinations, a fixed measure of institutional quality, the ratio of M2 to GDP lagged one period and dummy variables denoting sub-Saharan Africa and East Asia, and dummy variables for period of observation being 1974-77, 1978-81, 1982-85, 1986-89 and 1990-93.
2. Instruments for aid and aid-policy interacted in the 2SLS and IV-FE results are: log of population, log (arms imports/all imports) lagged one period, dummy variables for membership in Franc zone, Egypt or Central America, log of initial income interacted with policy, log of population interacted with policy, log of lagged arms imports interacted with policy, log of initial GDP squared interacted with policy and log population squared interacted with policy.
3. Sample size is 270.
4. t-ratios in parentheses. OLS and 2SLS results use Huber/White corrected standard errors.
5. * significant at the 10% level; ** significant at the 5% level.

Table 2: Mean aid flows, 1970-1993, 56 countries

Country	Mean Aid	SSA	Not SSA	Country	Mean Aid	SSA	Not SSA
Bottom Quartile				*Third Quartile*			
Venezuela	0.01		X	Tunisia	1.38		X
México	0.01		X	Jamaica	1.48		X
Argentina	0.01		X	Sierra Leone	1.69	X	
Brazil	0.02		X	Bolivia	1.79		X
Trinidad & Tobago	0.06		X	Syrian Arab Republic	1.85		X
Colombia	0.12		X	Haiti	1.85		X
Uruguay	0.12		X	El Salvador	1.86		X
Nigeria	0.13	X		Gabon	1.90	X	
Chile	0.15		X	Ghana	1.92	X	
Malaysia	0.20		X	Cameroon	2.00	X	
Rep. Korea	0.20		X	Honduras	2.18		X
Turkey	0.21		X	Zaïre (D.R.Congo)	2.23	X	
Thailand	0.24		X	Egypt	2.27		X
India	0.25		X	Kenya	2.33	X	
Second Quartile				*Top Quartile*			
Ecuador	0.32		X	Madagascar	2.40	X	
Indonesia	0.39		X	Ethiopia	2.88	X	
Algeria	0.39		X	Nicaragua	3.14		X
Perú	0.41		X	Guyana	3.73		X
Philippines	0.43		X	Somalia	3.96	X	
Guatemala	0.49		X	Sénégal	4.36	X	
Dominican Republic	0.60		X	Malawi	4.69	X	
Paraguay	0.68		X	Zambia	4.80	X	
Pakistan	0.76		X	Botswana	5.85	X	
Morocco	0.94		X	Togo	5.47	X	
Costa Rica	1.01		X	Tanzania	5.38	X	
Sri Lanka	1.16		X	Niger	5.89	X	
Côte d'Ivoire	1.26	X		Mali	6.19	X	
Zimbabwe	1.36	X		The Gambia	7.08	X	

Aid is expressed as a percentage of GDP, as measured by "effective development assistance" (Chang *et al.*, 1998). "SSA" refers to sub-Saharan Africa.

Table 3: Aid, policy and growth in sub-Saharan Africa and elsewhere in the developing world

Countries in sub-Saharan Africa

	Ordinary least squares	Two stage least squares	Country fixed effects	Instrumental variables: Country fixed effects
Policy	-1.25	-1.88	-1.74	-3.10
	(0.93)	(1.24)	(1.01)	(1.46)
Aid	-0.19	-0.48	0.06	0.84
	(0.80)	(1.40)	(0.12)	(1.11)
Aid x policy	0.65	0.81	0.73	1.21
	(2.36)**	(2.48)**	(1.52)	(1.95)*
F statistic on country-level fixed effects	-	-	1.24	1.45**

Countries outside sub-Saharan Africa

	Ordinary least squares	Two stage least squares	Country fixed effects	Instrumental variables: Country fixed effects
Policy	0.79	0.84	1.07	1.06
	(4.14)**	(4.09)**	(4.05)**	(3.91)**
Aid	0.03	-0.22	0.54	0.85
	(0.14)	(0.71)	(2.11)**	(2.29)**
Aid x policy	0.05	0.01	-0.07	-0.06
	(1.53)	(0.22)	(0.90)	(0.74)
F statistic on country-level fixed effects	-	-	2.29**	2.23**

Notes:
1. Regressions control for initial level of GDP, ethnic fractionalization, assassinations, ethnic fractionalization x assassinations, a fixed measure of institutional quality, the ratio of M2 to GDP lagged one period and dummy variables for period of observation being 1974-77, 1978-81, 1982-85, 1986-89 and 1990-93.
2. Instruments for aid and aid-policy interacted in the 2SLS and IV-FE results are: log of population, log (arms imports/all imports) lagged one period, dummy variables for membership in Franc zone, Egypt or Central America, log of initial income interacted with policy, log of population interacted with policy, log of lagged arms imports interacted with policy, log of initial GDP squared interacted with policy and log population squared interacted with policy.
3. Sample size is 82 for sub-Saharan Africa and 168 for all other developing countries.
4. t-ratios in parentheses. OLS and 2SLS results use Huber/White corrected standard errors.
5. * significant at the 10% level; ** significant at the 5% level.

Table 4: Marginal aid efficiencies based in 4 models: People lifted out of poverty by a $1-million (US) increase in aid

Country	Dayton-Johnson & Hoddinott		Burnside & Dollar (2000)		Hansen & Tarp (2001)		Collier & Dollar (2000)
	constant alpha	*Varying alpha*	*constant alpha*	*varying alpha*	*constant alpha*	*varying alpha*	
Bolivia	605	644	425	453	739	787	169
Brazil	185	144	-43	-34	226	176	95
Cameroon	1,194	1,119	280	262	982	920	293
Ethiopia	2,343	2,403	550	564	5,703	5,848	1,664
Ghana	3,742	4,460	878	1,046	1,565	1,866	391
Guinea-Bissau	4,304	3,470	1,010	814	2,923	2,357	-1,051
India	1,197	1,509	182	230	1,462	1,843	741
Republic of Korea	76	90	57	66	93	109	34
Nicaragua	985	900	-693	-632	1,203	1,098	-69
Zambia	-368	-339	-86	-79	2,953	2,781	246

Notes:
1. Estimates computed on the basis of econometric results provided in the referenced papers; "Dayton-Johnson & Hoddinott" refers to this paper.
2. "Constant alpha" denotes estimates computed with a constant income-elasticity of poverty reduction of 2 for all countries (see the discussion of alpha in Section 3.3). "Varying alpha" denotes estimates in which the income-elasticity of poverty reduction varies inversely with the level of income inequality.

Table 5: Correlation coefficients among four sets of estimates of marginal aid efficiencies

Pearson product-moment correlations (with number of observations used in parentheses)

	Dayton-Johnson & Hoddinott	Burnside & Dollar (2000)	Hansen & Tarp (2001)	Collier & Dollar (2000)
Dayton-Johnson & Hoddinott	1.000 (45)	0.777 (45)	0.573 (45)	0.525 (44)
Burnside & Dollar (2000)		1.000 (45)	0.400 (45)	0.422 (44)
Hansen & Tarp (2001)			1.000 (53)	0.718 (52)
Collier & Dollar (2000)				1.000 (53)

Spearman rank correlation coefficients (with number of observations used in parentheses)

	Dayton-Johnson & Hoddinott	Burnside & Dollar (2000)	Hansen & Tarp (2001)	Collier & Dollar (2000)
Dayton-Johnson & Hoddinott	1.000 (45)	0.783 (45)	0.796 (45)	0.654 (44)
Burnside & Dollar (2000)		1.000 (45)	0.557 (45)	0.597 (44)
Hansen & Tarp (2001)			1.000 (53)	0.764 (52)
Collier & Dollar (2000)				1.000 (53)

Table 6: Ranking of marginal aid efficiency: Various estimates

rank	Dayton-Johnson & Hoddinott		Burnside & Dollar (2000)		Hansen & Tarp (2001)		Collier & Dollar (2000)
Most efficient aid recipients							
	constant alpha	*varying alpha*	*constant alpha*	*varying alpha*	*constant alpha*	*varying alpha*	
1	GHANA	GHANA	GHANA	GHANA	ETHIOPIA	ETHIOPIA	ETHIOPIA
2	MALAWI	ETHIOPIA	MALAWI	ETHIOPIA	MALAWI	MADAGASCAR	INDIA
3	ETHIOPIA	MALAWI	ETHIOPIA	GUYANA	ZAMBIA	ZAMBIA	MADAGASCAR
4	MADAGASCAR	MADAGASCAR	HONDURAS	MALAWI	MADAGASCAR	MALAWI	NIGERIA
5	KENYA	KENYA	GUYANA	HONDURAS	KENYA	TANZANIA	KENYA
6	BOTSWANA	INDIA	MADAGASCAR	INDONESIA	TANZANIA	GHANA	TANZANIA
7	INDIA	BOTSWANA	PHILIPPINES	MADAGASCAR	GHANA	INDIA	PAKISTAN
8	CAMEROON	CAMEROON	GUATEMALA	BOLIVIA	INDIA	KENYA	GHANA
9	NICARAGUA	GUYANA	BOLIVIA	PHILIPPINES	NICARAGUA	GUYANA	MALAWI
10	HONDURAS	NICARAGUA	KENYA	GUATEMALA	ZIMBABWE	NICARAGUA	HONDURAS
Least efficient aid recipients							
1	ZAMBIA	ZAMBIA	NICARAGUA	NICARAGUA	KOREA	MALAYSIA	NICARAGUA
2	NIGERIA	NIGERIA	ZAMBIA	ZAMBIA	MALAYSIA	VENEZUELA	KOREA
3	KOREA	MALAYSIA	NIGERIA	NIGERIA	VENEZUELA	KOREA	MALAYSIA
4	MALAYSIA	VENEZUELA	PERU	PERU	COLOMBIA	COLOMBIA	VENEZUELA
5	VENEZUELA	KOREA	KOREA	GABON	THAILAND	MEXICO	THAILAND
6	COLOMBIA	COLOMBIA	VENEZUELA	MALAYSIA	MEXICO	THAILAND	COLOMBIA
7	THAILAND	MEXICO	MALAYSIA	VENEZUELA	TUNISIA	CHILE	CHILE
8	MEXICO	THAILAND	GABON	KOREA	CHILE	TUNISIA	MEXICO
9	TUNISIA	CHILE	COLOMBIA	COLOMBIA	MOROCCO	BRAZIL	MOROCCO
10	CHILE	TUNISIA	MEXICO	MEXICO	BRAZIL	GABON	GABON

Notes.
1. Ranking based on number of people that would be lifted out of poverty in each country by increasing its aid receipts by $1 million (US).
2. "Constant alpha" denotes estimates computed with a constant income-elasticity of poverty reduction of 2 for all countries (see the discussion of alpha in Section 3.3). "Varying alpha" denotes estimates in which the income-elasticity of poverty reduction varies inversely with the level of income inequality.
3. The first six columns are based on computations for 1990-1993; the seventh, for 1995-1997.

Comment:
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