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Working Paper

On aid, growth, and good policies

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Abstract

This paper provides a critical analysis of the growth regressions in Burnside and Dollar (2000). First, we analyze the relation between aid and government expenditure in a modified neoclassical growth model. We find that while good policies spur growth they may at the same time lead to decreasing effectiveness of foreign aid. Second, we show that the econometric results in Burnside and Dollar emphasizing the crucial role of interactions between aid and good policies in the growth process are fragile, being extremely data dependent. Finally, we demonstrate that the Burnside and Dollar data lends support to the idea that the association between aid and growth can be approximated by decreasing returns to aid. This finding conforms well to regression results in other recent studies.

Outline

On Aid, Growth, and Good Policies

CARL-JOHAN DALGAARD and HENRIK HANSEN

This paper provides a critical analysis of the growth regressions in Burnside and Dollar [2000]. First, we analyze the relation between aid and government expenditure in a modified neoclassical growth model. We find that while good policies spur growth they may at the same time lead to decreasing effectiveness of foreign aid. Second, we show that the econometric results in Burnside and Dollar emphasizing the crucial role of interactions between aid and good policies in the growth process are fragile, being extremely data dependent. Finally, we demonstrate that the Burnside and Dollar data lends support to the idea that the association between aid and growth can be approximated by decreasing returns to aid. This finding conforms well to regression results in other recent studies.

I. INTRODUCTION

The World Bank policy research report, *Assessing Aid* [World Bank, 1998], provides a careful, and rather self-critical, evaluation of the Bank's recent experience with foreign aid. A large part of the report can be read as advocating policy dialogue, beneficiary involvement, and local ownership instead of policy conditionality and enforced additionality of aid financed projects. There is also a clear recognition of the need for "conditionality" in the design and choice of aid instruments in the sense that the type of aid to a given country must be conditional on the stage of development. In the analysis of the importance of the stage of development the Bank has chosen to concentrate, almost exclusively, on government institutions and macro-economic policy though it is noted that other factors such as civil liberties are also important for the impact of foreign aid.

Nevertheless, in the discussions following the report most of the attention has been focused on the first chapter in which the Bank seems to opt for a new instrument based on policy selectivity in future aid allocations. Specifically, the Overview states: "Financial aid works in a good policy environment [and therefore] financial assistance must be targeted more effectively to low-income countries with sound economic management." [World Bank, 1998, 2 and 4]. The unambiguous policy message has provoked quite a few development economists and resulted in a new wave of studies of the link between aid and growth. Not surprisingly, many of the new

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studies are critical to the policy selectivity results and some even question the robustness of the empirical support underlying the recommendations in the report.[1]

The primary background paper to Chapter 1 in the report is Burnside and Dollar [1997] which is appearing in a slightly modified form in the American Economic Review [Burnside and Dollar, 2000]. By means of cross-country regressions Burnside and Dollar show that foreign aid has no impact on growth in countries with poor macro-economic policies while it leads to faster growth in countries with good policies. In *Assessing Aid* the World Bank take the consequence of this finding in stating that donors should direct foreign aid to countries with good economic policy (since aid is otherwise wasted when the sole purpose of aid is to foster economic growth).

The policy selectivity result in Burnside and Dollar has been questioned in recent studies by Hansen and Tarp [2000a,b] and Lensink and White [1999]. Yet due to differences in data definitions Burnside and Dollar, according to Beynon [1999], rightly claim that their results have never been challenged. The purpose of the present paper is, therefore, to analyze the growth regressions in Burnside and Dollar [2000], using standard regression diagnostics, and their data. By doing this we investigate whether the divergent results are mainly due to differences in data or differences in modeling strategies.

The main finding in the present paper is that the policy selectivity result is very fragile, being extremely data dependent. It appears that 5 influential observations, which are excluded in Burnside and Dollar's preferred regressions, have a critical influence on the parameter of main interest. In a simple counter example it is shown that one may, on an equally valid statistical basis i.e., excluding 5 influential observations, claim that aid spurs growth—unconditionally.

Another finding is that a model with decreasing marginal effect of aid on growth is preferred to the policy selectivity model when the choice is based on statistical significance of parameters. This result was also established in Hansen and Tarp [2000a,b], the novelty lies in the use of the exact Burnside and Dollar data. A new result is that in the model with decreasing returns to aid there is a significant difference between least squares estimates and instrumental variable estimates. This points towards endogeneity of aid in the growth regressions and it highlights the importance of the choice of instruments.

The paper is organized as follows. In section II we question the intuition behind the Burnside and Dollar result by formulating a growth model in which growth stimulating policy reduces the impact of foreign aid. The empirical investigation of the aid-growth results starts in section III with a comparison of different aid measures. In section IV the central growth regression in Burnside and Dollar [2000] is analyzed using standard regression diagnostics. This is followed by a re-specification of the regression model in section V. The reformulation results in a growth regression which is similar to the regressions in Hansen and Tarp [2000a,b] excluding interactions between aid and policy. Finally, section VI concludes the paper.

[1] See Lensink and White [2000] for a critique of the calculation of poverty-efficient aid allocations and McGillivray and Morrissey [2000] for a critique of the fungibility discussion in *Assessing Aid*.

II. THEORETICAL MODELS OF AID AND GROWTH

In view of the importance of foreign aid, both for donors and recipients, and the volumnious aid literature there is a surprising scarcity of theorical models linking aid and growth. Eventhough neocalssical models became (more or less) dominant in development economics from the begining of the 1980s the prototype model used in assessing the macro effectiveness of foreign aid was the Harrod-Domar model and the two-gap model by Chenery and Strout [1966] even until the mid 1990s. Boone [1996] was one of the first to analyze the macro economic impact of aid in a neocalssical growth model. Boone looked at fungibility issues in a standard growth model with productive public expenditure as in Barro [1990]. Boone found no effect of aid in the long run because aid is consumed instead of invested.[2]

Burnside and Dollar [2000] do not consider productive government spending but discuss government consumption and tax distortions. More specifically, they consider an aggregate production function of the form $Y = BK^{\theta}$, where Y is production and K is capital. Assuming that aid can only affect output through capital accumulation, effectiveness of aid can be approximated by

$$\frac{dY}{Y} = \theta \frac{Y}{K} \frac{\partial K}{\partial A} \frac{dA}{Y},$$

where A is real aid and $\theta \frac{Y}{K}$ is the marginal productivity of capital which, in the absence of credit rationing, equals the rate of return on capital.

Burnside and Dollar suggest to interpret the estimated derivative of growth with respect to aid as an estimate of $\theta \frac{Y}{K} \frac{\partial K}{\partial A}$, the product of the marginal productivity of capital and the marginal propensity to invest aid.[3] This interpretation is no different from the many aid-growth regressions in the 1970s and 1980s following the proposal by Papanek [1973]. However, while the marginal productivity of capital was assumed to be (roughly) constant in the early studies, Burnside and Dollar assumes that it varies with economic policy. But, with this interpretation any variable that changes the marginal productivity of capital must be included in an interaction with aid. A cursory reading of the recent growth literature would suggest an overwhelming number of additional variables, such as the Adelman-Morris index of socio-economic development [Adelman and Morris, 1967; Temple and Johnson, 1998], income inequality [Alesina and Rodrik, 1994, among others], and of course human capital, just to mention a few.[4]

It is important to realize that even within a neoclassical framework it is possible to derive predictions directly contrasting Boone and Burnside and Dollar. We will, therefore, in this section show that one can easily formulate and formailze growth models in which (i) fungibility is not the main problem for aid effectiveness, (ii) the marginal effect of foreign aid is not equal to the return to capital, and (iii) good policies (ones that are themselves important for growth) may *reduce* the marginal impact of foreign aid on growth.

[2]Other authors, e.g., Lensink and White [1999] have modified the neocalssical model with productive public expenditure in other ways than Boone, and they have reached different conclusions.

[3]The last factor is reflecting the fungibility problem.

[4]See Durlauf and Quah [1998] for an extensive list of other variables which have appeared in recent growth regressions.

Below we develop a growth model, based on the Ramsey-Cass-Koopmans framework, which is capable of illustrating these points. However, it must be stressed from the outset that this model only serves as an example. We do not consider this to be the only way to model the relationship between aid and growth. But we do belive the model captues some important aspects of aid and growth in developing countries.

Starting with the standard assumptions, we consider a closed economy with competitive factor markets and perfect credit markets. For simplicity, there is no exogenous technical progress and the population is constant.

The model deviates from the standard neoclassical growth model in two respects. First, we include foreign aid by considering pure income transfers which enter the budget of the representative consumer. Second, following Barro and Sala-i-Martin [1995, 159-60] we let producers face a risk of expropriation, or some similar loss of the return to capital. Specifically, assume the owners of the factors of production are able to collect their returns only with some probability, p, which is external to the individuals but internal to the economy as a whole. While Barro and Sala-i-Martin model the return probability as a function of government expenditure relative to the total economic activity, G/Y, Alesina and Perotti [1996] make p a function of the distribution of income and the standard of living. The presumption is that a lower standard of living for the masses is likely to lead to social unrest, riots, and thievery, all of which reduce the expected return on investments.[5]

Combining the two types of models we may formalize the return probablilty as

$$p = p(G(t)/Y(t), c(t), \Delta^2),$$

where $G(t)/Y(t)$ is the relative size of government expenditure (or more loosley policy), $c(t)$ is per capita consumption (the standard of living), and Δ^2 is a measure of income inequality. Based on the above, p is expected to be increasing in policy and consumption but decreasing in inequality. Throughout, income inequality is assumed to be exogenous and constant over time. Consequetly, it is dropped from the p-function. For government expenditure we make the standard assumption of a balanced, tax financed budget at all times:

$$G(t) = \tau Y(t),$$

where τ is the constant, proportional tax rate.

As p is external to the individuals it follows that producers will employ capital and labour until the expected, after tax marginal productivity equals the price on each factor. Allowing for capital depreciation at the rate δ, it holds at all points in time that

$$\begin{aligned} R(t) &= p(\tau, c(t))(1-\tau)\frac{\partial F(K(t), L)}{\partial K(t)} - \delta, \\ w(t) &= p(\tau, c(t))(1-\tau)\frac{\partial F(K(t), L)}{\partial L}, \end{aligned} \tag{1}$$

[5]In Barro and Sala-I-Martin [1995] G represents government activities aimed at maintaining property rights such as police services.

where $R(t)$ is the expected after tax return to capital, $w(t)$ is the real wage, $K(t)$ is capital input and L the (constant) labor force. The production technology, $F(K, L)$, is a standard neoclassical production function with constant returns to scale.

The consumers maximize the discounted utility from consumption $c(t)$. In the present model this is formalized as[6]

$$\max_{(c(t))_{t=0}^{\infty}} U(0) = \int_0^{\infty} \frac{c(t)^{1-\sigma}}{1-\sigma} e^{-\rho t} dt,$$

subject to

$$\dot{k}(t) = R(t)k(t) + w(t) + a - c(t), \; k(0) \text{ given}$$
$$k(t) \geq 0 \text{ for all } t.$$

In the budget constraint current total income is augmented by a transfer of foreign aid, a. For simplicity the aid inflow is assumed to be constant.

The usual computations lead to the Keynes-Ramsey rule and, by using (1) and the production function in intensive form ($f(k(t)) = \frac{1}{L}F(K(t), L)$), it follows that

$$\frac{\dot{c}(t)}{c(t)} = \frac{1}{\sigma}[p(\tau, c(t))(1-\tau) f'(k(t)) - \delta - \rho]. \tag{2}$$

Hence, consumption will be growing if the expected return on capital investments exceeds the rate of time preference, ρ. Additionally, the equation which governs the accumulation of capital is

$$\dot{k}(t) = p(\tau, c(t))(1-\tau) f(k(t)) + a - c(t) - \delta k(t), \tag{3}$$

and the solution of the model is given by the two differential equations (2) and (3). The corresponding phase-diagram is illustrated in Figure 1.[7]

At any given point in time the economy will be traveling along the saddle-path, the SS-curve in Figure 1. The main difference to the standard neoclassical growth model is that the $\dot{c} = 0$ curve is upward sloping, being only asymptotically vertical. This is because a higher level of consumption increases the expected return on capital.[8] This change of the standard growth model results in quite interesting predictions about aid, policies, and growth. Some of these are discussed below.

First of all, foreign aid has an impact on the long run level of income as long as the return probaility, p, is less than unity. We expect p to be low in poor countries as they have low levels of per capita consumption. In these countries foreign aid leads to an increase in consumption which in turn increases the expected return on investment for a given level of government

[6] As the economy is closed we assume that total wealth equals the capital stock, $K(t)$. For this reason we have replaced the standard no-ponzi-game condition with a non-negativity constraint.

[7] In order to ensure existence of a (saddle point stable) steady state the following condition is assumed to hold for all k, c: $(d\dot{k}/dc)_{|k=\bar{k}} < 0$. In drawing the figure we, furthermore, assume $\partial^2 p/\partial c^2 < 0$ and $f'''(k(t)) < 0$.

[8] Note that $\tilde{k}$ and $\hat{k}$ are, implicitly, given by $\bar{p} f'(\tilde{k}) = \rho + \delta$ and $f'(\hat{k}) = \rho + \delta$.

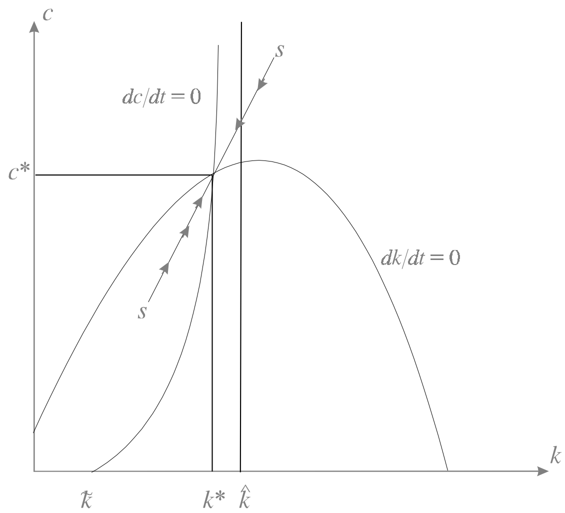


FIGURE 1
PHASE-DIAGRAM

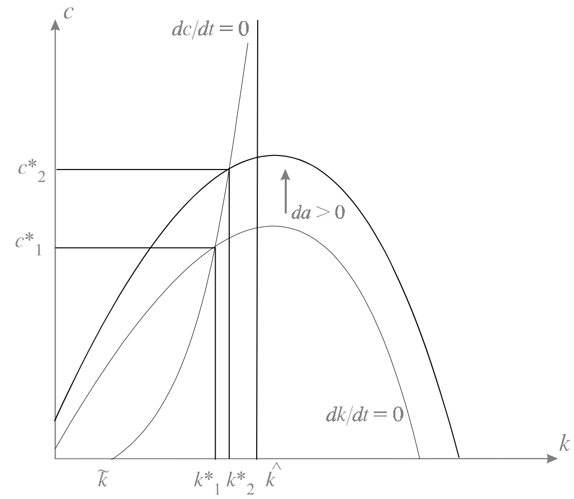


FIGURE 2
THE EFFECT OF AN UNANTICIPATED
PERMANENT INCREASE IN FOREIGN AID

expenditure. This result is illustrated in Figure 2. It is also clear from the model that there may be diminishing returns to aid, as found in Lensink and White [1999] and Hansen and Tarp [2000a,b].

Another result, closely related to the first, is that the marginal return to foreign aid is quite different form the expected return on capital. Specifically we find the marginal effect of aid to be

$$\frac{dy}{y} = \left(1 + \frac{f'(k)}{f(k)}\frac{\partial k}{\partial p}\right)\frac{\partial p}{\partial c}\frac{\partial c}{\partial a}\, da,$$

which may be larger or smaller than $R(t)$ in equation (1).

Turning to the interplay between aid and good policy we first need to be specific about the notion of a good policy. In most models with tax financed, productive public expenditure there is an inverse u-shaped relation between the relative size of the public sector ($G/Y = \tau$) and growth. The present model also has this property. Therefore we will start by assumming that government policies designed to ensure private property rights are initially below the growth maximizing level.[9] This makes an increase in τ a good policy.

Good policy has an impact on the transmission of aid to long run growth if it changes the slope of the $\dot{c} = 0$ curve. The slope is given by

$$\frac{dc}{dk} = -\frac{p(\tau, c(t))}{\partial p/\partial c}\frac{f''(k(t))}{f'(k(t))} > 0.$$

From this it is clear that interaction effects between aid and policy depends on the form of p. Suppose $\partial^2 p/\partial c\partial\tau < 0$. In this case the $\dot{c} = 0$ curve becomes steeper following a once and for

[9]The growth maximizing level, τ^* say, is given as the solution to $p(\tau, c(t)) = (1-\tau)\frac{\partial p}{\partial \tau}$. In our model τ^* is a function of the standard of living $c(t)$.

all increase in government expenditure. This makes the effect of increasing consumption per capita smaller. Hence, good policies can be beneficial for growth and at the same time reduce the effectiveness of foreign aid. This is so because good policies and aid are “substitutes” in the prensent model as they both decrease the probablity of social unrest.

If government expenditure is initially above the growth maximizing level, then reducing τ is a good policy. (The security provided is too costly). In this case good policy will increse the effectiveness of aid. Overall, the result is that aid can replace government expenditure when $\partial^2 p/\partial c\partial\tau < 0$, while government expenditure can only replace aid up to a certain point.

Of course one may also assume the opposite relation between consumption and government investment, $\partial^2 p/\partial c\partial\tau < 0$, eventhough we do not think of this as a natural assumption in the present context. This reverses the above results: Growth stimulating policies enhance the effect of foreign aid when expenditures are initially too low, while they lower the effect when expenditures are initially too high.

In sum, the links between aid, growth and good policies are ambigous. This makes empirical work all the more important and it stresses the need for careful testing of new empirical regularities before wide ranging policy changes are initiated. In the sections to follow we will therefore test Burnside and Dollar’s empirical result of increased effectiveness of aid in a good policy environment.

III. Real effective aid versus nominal official aid

Burnside and Dollar are the first to use a new database on foreign aid compiled by Chang et al. [1998] for the World Bank. The main difference between the new aid measure (effective development assistance, EDA) and the measure used by other authors (official development assistance, ODA) is that EDA is the sum of grants and the grant equivalents of official loans whereas ODA includes both the direct grants and concessional loans for which the grant component is above 25 percent.

Furthermore, Burnside and Dollar refrain from the standard practise of relating the aid flows in current dollars to GDP in current dollars. Instead they construct real aid, measured in constant 1985 dollars, using the unit-value of imports price index from the IFS. Real effective development assistance is subsequently divided by real GDP from the Penn World Tables, Mark 5.6 [Summers and Heston, 1991].

While the EDA measure in all likelihood provides a better picture of resource flows compared to ODA it seems odd to name the flow *effective* development assistance when the effect of procurement tying of aid by bilateral donors is not even touched upon. But, in relation to the growth regressions in Burnside and Dollar it is more interesting to discuss the distinction between real (PPP-adjusted) and nominal flows. According to Beynon [1999], Burnside and Dollar argue that nominal aid to nominal GDP is vulnerable to suggesting spurious changes in aid levels in response to rapid changes in the exchange rate. The example used in Beynon is that “a 50% devaluation that effectively halves the $ denominated level of GDP would imply an instant but erroneous (assuming the bulk of aid dollars to be spent on foreign currency items) doubling in aid.” [Beynon, 1999, Annex 2, 20]

The assumption that “the bulk of aid dollars is to be spent on foreign currency items”

FIGURE 3

CROSS PLOTS OF AID MEASURES

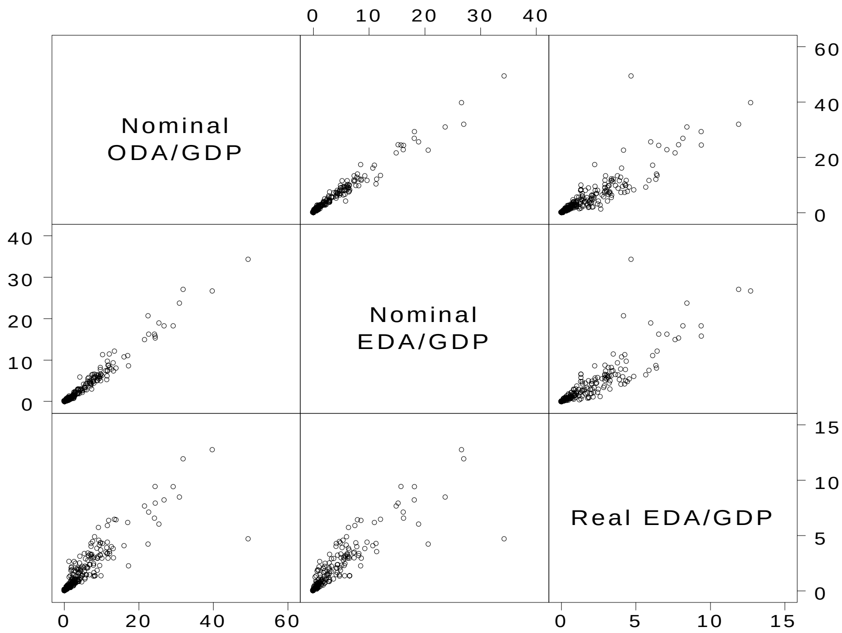


TABLE 1

CORRELATIONS BETWEEN AID MEASURES

	Nominal ODA/GDP	Nominal EDA/GDP	Real EDA/GDP
Nominal ODA/GDP		0.98 (.98)	0.89 (0.94)
Nominal EDA/GDP	0.98		0.88 (0.93)
Real EDA/GDP	0.95	0.95	

Note: Pearson (Spearman) correlations above (below) diagonal. Pearson correlations when Somalia 78-81 is excluded are in parentheses.

has to be seen in conjunction with the results of Chapter 3 in *Assessing Aid*, pointing to the conclusions that “aid is financing the entire public sector” [World Bank, 1998, 3]. If aid is financing the entire public sector it seems odd to use one denominator for the budget surplus and another for aid in the growth regressions. It may be a good idea to use the IFS index for all government activities as this will measure the resource flows in terms of a world price index. Although this does seem to bring back memories of Cost-Benefit discussions from the seventies the savings/consumption discussion related to the choice of price index may be important in growth regressions.

However, long discussions of theoretical consistency and spurious changes in aid flows seem immaterial once we look at the data. Figure 3 shows cross-plots of three aid measures that may all enter the growth regression. The top row (left column) in the matrix plot has the standard, nominal ODA to nominal GDP on the vertical (horizontal) axis. The center row (column) has nominal EDA to nominal GDP on the vertical (horizontal) axes. Finally, the bottom row (right column) has real EDA to real GDP, as defined by Burnside and Dollar, on the vertical (horizontal) axes.

As seen, ODA is somewhat higher than EDA, but it is quite easy to draw a straight line through most of the points. Hence, despite the valuable effort by Chang et al. [1998] in their construction of a better measure of aid flows, the difference between EDA and ODA seems to be a simple transformation. This is confirmed by the correlations between nominal ODA and EDA given in Table 1. In the Table, standard Pearson correlations are given above the diagonal while Spearman's rank correlations are reported below the diagonal. The correlation between the two nominal measures is 0.98 using either formula.

Turning to the relation between the nominal and real measures we find a higher dispersion in the cross-plots. However, much is caused by a single outlier. The outlier in the plots is Somalia 78-91. The nominal EDA/GDP ratio for Somalia is 32 percent while the real ratio is 6 percent. The reason for this discrepancy can be found in the ratio of the PPP-adjusted GDP from PWT to the constant dollar GDP from the World Bank. The PPP-adjusted GDP is more than six times the GDP from the World Bank for Somalia in all periods for which we have data. Hence, this is not a rapid change in the exchange rate. Calculating correlations between the real and nominal aid measures with and without the observation for Somalia results in some differences in the Pearson correlations but no change in the Spearman correlations. Yet, even including Somalia 78-91 we find a Pearson correlation of 0.89 between the new measure used by Burnside and Dollar and the standard aid measure. This correlation increases to 0.94 when the single Somalia observation is left out.

In conclusion, with respect to discussions of the proper aid measure Burnside and Dollar may, or may not, be right in the way they have chosen to measure aid flows in terms of a world price metric. But this certainly depends on the underlying theoretical model. Based purely on simple statistical properties of the different aid measures it seems as if the aid effectiveness results obtained by Burnside and Dollar, using real effective development assistance, are comparable to studies using nominal official development assistance. Thus, the cause of the divergence must be sought elsewhere.

IV. Influential observations and outliers

In this section we will take a close look at Burnside and Dollar's preferred growth regressions. The focus will be on detecting influential observations and outliers in the data set. The reason for this special interest is that while the important interaction between aid and policy is insignificant in the full sample of 56 developing countries and in a subsample of 40 low income countries, Burnside and Dollar show that once 5 "big outliers" are excluded from the regressions the interaction is significant. As shown below, there are many other influential observations in the data set which may deserve special attention.

In the analysis we make use of standard regression diagnostics for influential observations and outliers [see Belsley, Kuh, and Welsch, 1980, among many others]. This means that all diagnostics are based on ordinary least squares regressions with no account for possible heteroskedasticity. Some results will change if heteroskedasticity consistent standard errors are used. But this is at the expense of the simple relations between diagnostics, residuals, and regressor influence. As the regression diagnostics have been developed as tools in informal analyses, i.e., we do not use formal hypothesis testing, and for the sake of easy replicability, we have chosen to apply the standard tools.[10]

Table 2 report results of re-estimations of Burnside and Dollar's preferred growth equations. Regressions (1)-(3) include all 56 countries while regressions (4)-(6) only include the 40 lower income countries as defined in Burnside and Dollar [2000]. For the sake of clarity, we will briefly browse through the list of regressors, even though the specification and the data is identical to the Burnside and Dollar study.

The dependent variable is the average growth rate in real GDP per capita over six four year periods, starting with 1970-73 and ending with 1990-93. The GDP variable is from Penn World Tables (Mark 5.6).

The first regressor, Initial GDP, is the logarithm of GDP per capita in the last year preceding the period for which the growth rate is calculated. The variable is expected to have a significant negative influence on the growth rate, capturing the conditional convergence effect.

The following three regressors, Ethnic fractionalization, Assassinations, and the product of the two, are included in growth regressions to capture political instability. The number of assassinations varies over time while ethnic fractionalization is time constant (based on data from 1960). The two variables are expected to have a negative influence on growth.

The two regressors, Institutional quality and M2/GDP, are included as proxies for the quality of institutions and the financial markets. The first variable is an index based on evaluations of five different institutional indicators made by the private international investment risk service, International Country Risk Guide. The five indicators are; Quality of the bureaucracy, Corruption in Government, Rule of Law, Expropriation Risk, and Repudiation of Contracts by Government [Knack and Keefer, 1995]. It is worth noticing that the, time constant, institutional quality variable is based on evaluations in 1982 or later, which is roughly in the middle of the sample. Hence, there is a strong assumption of constancy and exogeneity of institutions as measured by the five indicators. The proxy for the development of financial markets is broad money (M2) relative to income (GDP). In the growth regression predetermined observations are used in order to avoid simultaneity problems.

Turning to policy, Burnside and Dollar create an index covering aspects of fiscal, monetary, and trade policies. Fiscal policy is measured by the budget surplus. The success or failure of monetary policy is measured by the level of inflation, while trade policy is proxied by a binary (0/1) openness indicator, constructed by Sachs and Warner [1995]. To avoid collinearity problems Burnside and Dollar create an index using a weighted average of the three measures:

$$Policy = 1.28 + 6.85\ Budget\ surplus - 1.40\ Inflation + 2.16\ Trade\ openess.$$

As seen, the construction of the index is such that good policy, in terms of a budget surplus, low inflation and an open economy, leads to a high value of the index. Hence, the effect on growth is expected to be positive.

Finally aid is included in the growth regression. Aid is real EDA to real GDP as discussed in section III. In the preferred regressions aid enters as a regressor on its own and multiplied by the policy index. This latter regressor is denoted the interaction effect.

[10] All regression diagnostics presented in this section are standard output in statistical programs such as Stata [StataCorp, 1999].

TABLE 2

GROWTH REGRESSIONS WITH INTERACTION EFFECTS BETWEEN AID AND POLICY

	All 56 countries			40 lower income countries		
Regression	(1)	(2)	(3)	(4)	(5)	(6)
Initial GDP	−0.618	−0.595	−0.451	−0.730	−0.697	−0.358
	(0.581)	(0.586)	(0.575)	(0.833)	(0.847)	(0.794)
Ethnic fractionalization	−0.564	−0.431	−0.498	−0.784	−0.587	−0.766
	(0.744)	(0.747)	(0.732)	(0.849)	(0.840)	(0.835)
Assassinations	−0.441	−0.449*	−0.425	−0.748	−0.787*	−0.670
	(0.271)	(0.268)	(0.265)	(0.478)	(0.458)	(0.482)
Ethnic frac. x Assassin.	0.807*	0.794*	0.824*	0.926	0.678	1.108
	(0.457)	(0.455)	(0.449)	(0.943)	(0.962)	(0.927)
Institutional quality	0.645**	0.693**	0.704**	0.784**	0.856**	0.887**
	(0.177)	(0.177)	(0.169)	(0.205)	(0.206)	(0.192)
M2/GDP (lagged)	0.013	0.012	0.004	0.027	0.023	0.015
	(0.014)	(0.015)	(0.013)	(0.017)	(0.018)	(0.015)
Policy Index	0.956**	0.705**	1.041**	1.107**	0.541*	1.168**
	(0.190)	(0.195)	(0.142)	(0.323)	(0.320)	(0.189)
Aid/GDP	0.017	−0.016	0.249**	−0.036	−0.173	0.214*
	(0.125)	(0.165)	(0.124)	(0.135)	(0.175)	(0.126)
(Aid/GDP) x Policy	0.013	0.184**		0.005	0.265**	
	(0.050)	(0.071)		(0.062)	(0.089)	
Observations	275	270	270	189	184	184
R^2	0.39	0.39	0.42	0.46	0.47	0.51
Partial $R^{2(a)}$	0.22	0.26	0.25	0.29	0.41	0.35

Note: The dependent variable is real per capita GDP growth. All regressions include time dummies for each period in the sample and dummies for Sub-Saharan Africa and East Asia. White's heteroskedasticity consistent standard errors in are in parentheses. $^{(a)}$R^2 when the effect of time and regional dummies is partialled out. *Significant at the 10 percent level. **Significant at the 5 percent level.

In addition to the the above mentioned regressors, the model also include time dummies and dummies for Sub-Saharan Africa and East Asia. The dummies are found significant in almost all empirical growth studies. However, in terms of 'explaining' differences in growth rates they have little to offer and they are, therefore, not reported in the following. Furthermore, all tables report two goodness-of-fit measures: The first is the standard R^2 measure while the second is the partial R^2 for the model conditional on time dummies and regional dummies.

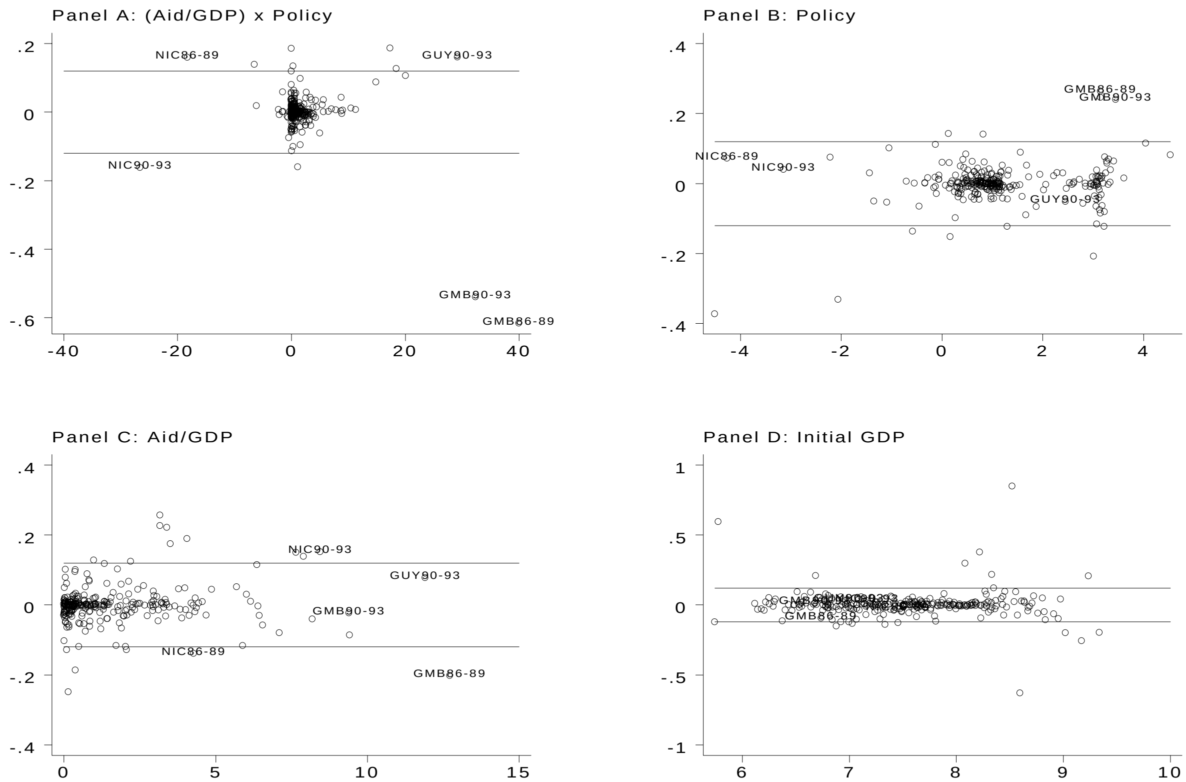
Regressions (1) and (4) in Table 2 give results for the preferred specifications estimated on the two full samples. The regressions show that only two variables are significant at a 5 percent level: Institutional quality and the policy index. Most importantly, aid has no significant impact on growth.

Moving from regressions (1) and (4) to (2) and (5) shows the changes in the parameters when five observations are excluded from the samples. The five observations are Gambia 1986-89, 1990-93, Guyana 1990-93, and Nicaragua 1986-89, 1990-93. While the coefficient to the interaction effect is small and highly insignificant in (1) and (4) it increases more than ten fold in the large sample and even fifty fold in the lower income country sample. In addition, in (2) and (5) the interaction parameters are significant at the 5 percent level.

The reason for excluding the five observations is apparently that these observations have

FIGURE 4

INFLUENTIAL DATA POINTS FOR SOME PARAMETERS OF INTEREST



a very big influence on the coefficient to the aid-policy interaction term. Each observation's influence on the estimated parameters can be investigated simply by estimating the model on a sample in which the single observation is excluded. Figure 4 shows cross-plots of the scaled changes in the estimated coefficients for four of the regressors in the model when observations are excluded one-by-one. The changes in the estimated coefficients are plotted against the excluded observation.[11] Burnside and Dollar use a slightly different measure as they do not scale the change in the estimated coefficient by the estimated standard error. While there are different views on whether or not the changes should be scaled, in the present context the scaled measures have the advantage that as the unit of measurement is in terms of (approximate) standard errors it is possible to compare the magnitude of changes across parameters. Moreover, for the scaled measure there are simple rules of thumb for changes worth investigating. Belsley et al. suggest to use $\pm 2/\sqrt{n}$ as a cut-off point, where n is the number of observations in the regression. Others have suggested less stringent cut-off values such as ± 1. In Figure 4 the cut-off points $\pm 2/\sqrt{n}$ are indicated by horizontal lines.

Starting with Panel A in Figure 4 it is obvious that each of the five excluded observations has a critical influence on the estimated coefficient to the interaction term. Especially the

[11] The scaled change in the coefficient reported in Figure 4 is often denoted DFBETAS, see Belsley et al. [1980] and appendix A for the precise definitions of the various influence measures used in this section.

two observations for Gambia move the estimate towards zero. However, notice that the five observations are not the only ones having a critical influence on the estimate, there are six other observations outside the more strict cut-off value while none of the scaled changes exceed one in absolute value. Hence, without more information some investigators would not find any cause for action, such as deleting observations from the sample.

Moving to Panel B, we find nine possibly critical observations for the policy coefficient, of which only Gambia is in the Burnside and Dollar exclusion set. In Panel C it is possible to pinpoint no less than 12 influential observations for the aid coefficient when the strict cut-off value is applied. Among these we find the two observations for Nicaragua and one of the observations for Gambia. The most interesting plot, however, is for the initial GDP. In this plot (Panel D) there are no less than 19 influential observations, none of which are in the excluded set. Notice that some of the scaled changes in the coefficient to initial GDP are larger in absolute value than the scaled changes in the coefficient to the aid policy interaction. Thus, using this metric there are other observations more liable as candidates for deletion.

Figure 4 reveals that the five excluded observations are the extreme values of the aid policy interaction regressor. Such points are not considered as outliers in classical regression analysis; they are (possible) leverage points. Of course they may still be deleted if the information they convey is considered to be different from the rest of the observations. But this deletion rule is clearly *ad hoc* and it is rather odd to limit the variation in the central regressor in this way.

In order to provide a more comprehensive picture of the influential observations in the sample we have listed influence measures for 23 observations in Table 3. The criteria for inclusion in the Table is that at least two influence measures must exceed a pre-selected cut-off value. Of the many possible influence diagnostics we have, arbitrarily, chosen the scaled change in the coefficients for the four variables shown in Figure 4 and the scaled change in the overall fit. In addition to the scaled changes in the parameters and the overall fit we also report the studentized residuals and a leverage measure. (The diagonal of the hat matrix, see Appendix A).

Table 3 makes it clear that the five deleted observations are not outliers in the sense of having extreme studentized residuals. None of the studentized residuals for these observations exceed 2 in absolute value. However, four of the five observations are leverage points, meaning that they have an above-average influence on the fitted values. But, there are other observations in the sample with even higher leverage values.

The danger of deleting observations from the sample based on high influence on one or a few special parameters is revealed in regressions (3) and (6) in Table 2. In these regressions we have ommitted the interaction term and searched for a sample, of the same size as the Burnside and Dollar sample, in which the coefficient to aid by it self is positive and significant. By excluding five observations; Gambia 1986-89, 1990-93, Nigeria 1970-73, 1990-93, and Nicaragua 1978-81, we obtain the result we are looking for: Aid has a significant impact on growth. In the 56 country sample the parameter is highly significant, while it is only significant at the 9 percent level in the sample excluding middle income countries.[12] Notice that the two most influential observations in the Burnside and Dollar regressions are also excluded in (3)

[12]The same result can be obtained in a slightly more sophisticated way, simply by excluding the two countries, Gambia and Nigeria from the sample.

TABLE 3

POTENTIALLY INFLUENTIAL OBSERVATIONS

Country	Period	Outliers	Leverage	Influence on (Aid/GDP)xPolicy	Influence on Policy	Influence on Aid/GDP	Influence on Initial GDP	Fit
Argentna	74-77		.315		−.137			.673
Bolivia	82-85			.139	−.374			
Botswana	78-81			.187				.557
Brazil	86-89			.186	−.331			
Chile	82-85			.134	−.209			
Cameroon	78-81	3.67					−.151	.717
	90-93	−3.05				.124		−.661
Ecuador	70-73			−.160	.246			
Egypt, Arab Rep.	82-85	2.17					−.129	.558
Ethiopia	82-85	−3.92				.226	.593	−.913
Gabon	70-73					.221	.376	.548
	74-77	4.46					.846	1.214
	78-81	−3.41			−.123		−.631	−.912
Gambia, The	86-89†		.295	−.617	.263	−.202		−.989
	90-93†		.186	−.539	.240			−.778
Guyana	90-93†		.299	.160				
Nicaragua	78-81	−3.52				−.129		−.702
	86-89†			.159		−.139		
	90-93†		.406	−.162		.152		
Nigeria	70-73	2.11				−.187		.507
Philippines	82-85				.141			−.512
Syrian, Arab Rep.	74-77	2.59				.256	.297	.601
	78-81					.174	.215	

Note: Observations are included in the table if they exceed at least two cutoff values. The cutoff values are: |DFITS| > .5, |DFBETAS| > .12. The studentized residuals (Outliers) are only reported if they exceed 2 in absolute value. The leverage points are only reported if they exceed .18. See Appendix A for definitions of the applied influence measures. †Outlier in the Burnside and Dollar study.

and (6). As such it seems extremely difficult to reject regressions (3) and (6) and at the same time accept (2) and (5). Yet, the former model *does not* have a policy selectivity rule.

Finally, it should be noted that if outliers are detected and down-weighted in a mechanical way, using a robust regression method, the main change in the result compared to (1) and (4) is a significant negative coefficient to initial GDP, indicating conditional convergence.[13] Aid and the interaction term are still insignificant in the robust regressions. It must be stressed, though, that the robust regression does not restrict influence from outlying points in the regressor space unless they lead to large residuals. Therefore, the robust regression results can only be used to show that the lack of significance of the interaction term cannot be attributed to big residual outliers.

[13]The robust regression method is iterative re-weighted least squares using Huber- and bi-weights. The procedure is standard in Stata.

V. FUNCTIONAL FORM AND ENDOGENEITY OF AID

The lack of robustness of the Burnside and Dollar specification may be due to model misspecification. In particular, the studies by Hadjimichael et al. [1995]; Durbarry et al. [1998]; Lensink and White [1999], and Hansen and Tarp [2000a,b] show that modeling decreasing returns to aid may be important, as they all find a significant effect of including aid squared as an additional regressor.

Whether one should prefer a policy selectivity model with interaction between aid and policy or a diminishing returns model with a polynomial effect of aid—or even a combination—is a simple testable hypothesis. In Hansen and Tarp [2000a,b] it is argued that a full model must include five aid-policy terms: aid, policy, aid squared, policy squared, and aid interacted with policy. The argument is quite simple; these five terms define a complete, second order, polynomial response surface in the growth-aid-policy space.

To be precise, consider the stylized growth regression equation

$$g_t = \gamma' Z_t + \beta_1 P_t + \beta_2 A_t + \alpha_{11} P_t^2 + \alpha_{22} A_t^2 + \alpha_{12} P_t A_t + \varepsilon_t,$$

where g_t is the growth rate, Z_t is a set of controls, P_t is a policy index, A_t is aid and γ, β, α denotes parameters.

In this model Burnside and Dollar set $\alpha_{11} = \alpha_{22} = 0$ and Hadjimichael et al., Durbarry et al., and Lensink and White set $\alpha_{11} = \alpha_{12} = 0$, all without testing the hypothesis. Lensink and White [1999, Table 7] test $\alpha_{12} = 0$ conditional on $\alpha_{11} = 0$. They find the interaction term to be highly insignificant.

When the specifications are tested within the full model Hansen and Tarp [2000a,b] find statistical support for diminishing returns ($\alpha_{11} = \alpha_{12} = 0$). According to Beynon [1999], Burnside and Dollar reconcile this finding by stressing that they are using different data compared to the other studies. Therefore we end the analysis of the Burnside and Dollar data by showing that when the set of instruments is chosen to achieve a good (time series) fit of the endogenous aid variables the diminishing returns model is preferred to a policy selectivity model.

Table 4 presents results of instrumental variable regressions of real growth in GDP per capita in which all regressors that are functions of aid are modeled as endogenous variables. The Table is organized as Table 2 in that the first three columns (regressions 7-9) give results for the full sample of 56 countries while the last three columns (regressions 10-12) give results for the sample of 40 low income countries. In Table 4 there is no exclusion of observations due to outliers but the first estimation period (1970-73) is excluded because lagged observations of all aid regressors are used as instruments. This is why there are only 223 and 153 observations in the two samples instead of 275 and 189.

Regressions (7) and (10) reveals that aid and aid squared are both significant while the aid-policy interaction and policy squared are both insignificant, at any conventional level of significance, when all four terms are included in the growth regression. This result is in contrast to Burnside and Dollar [2000] but in agreement with Hansen and Tarp [2000a].

Moving to regressions (8) and (11) it is clear that omitting the two statistically insignificant variables, aid-policy and policy squared, leaves the significant parameters virtually unchanged. This result is substantiated by the Wald type test of the joint exclusion of the two variables. As

TABLE 4

INSTRUMENTAL VARIABLE GROWTH REGRESSIONS WITH ENDOGENOUS AID

	All 56 countries			40 lower income countries		
Regression	(7)	(8)	(9)	(10)	(11)	(12)
Initial GDP	−0.007	0.012	−0.372	−0.011	−0.021	−0.346
	(0.811)	(0.771)	(0.747)	(1.181)	(1.037)	(1.031)
Ethnic fractionalization	0.549	0.575	−0.176	0.309	0.302	−0.350
	(0.992)	(0.969)	(0.862)	(1.120)	(1.067)	(0.986)
Assassinations	−0.455*	−0.453*	−0.414	−1.019*	−1.018**	−0.823
	(0.268)	(0.267)	(0.275)	(0.438)	(0.431)	(0.495)
Ethnic frac. x Assassin.	0.887*	0.882*	0.779	1.583	1.589	1.334
	(0.466)	(0.460)	(0.474)	(0.991)	(0.989)	(1.039)
Institutional quality	0.862**	0.865**	0.698**	0.933**	0.933**	0.878**
	(0.223)	(0.224)	(0.200)	(0.250)	(0.246)	(0.228)
M2/GDP (lagged)	0.009	0.009	0.012	0.026	0.026	0.024
	(0.020)	(0.019)	(0.015)	(0.020)	(0.020)	(0.017)
Policy Index	0.927**	0.958**	1.056**	1.127**	1.133**	1.273**
	(0.267)	(0.153)	(0.227)	(0.415)	(0.206)	(0.416)
Aid/GDP	1.327**	1.352**	0.229	1.031*	1.027**	0.166
	(0.549)	(0.530)	(0.211)	(0.546)	(0.514)	(0.212)
(Aid/GDP) squared	−0.126**	−0.127**		−0.095**	−0.095**	
	(0.046)	(0.049)		(0.044)	(0.045)	
Policy squared	0.012			−0.002		
	(0.064)			(0.076)		
(Aid/GDP) x Policy	0.006		−0.052	0.002		−0.062
	(0.065)		(0.071)	(0.081)		(0.083)
Effect of aid at mean	0.931**	0.946**	0.167	0.633*	0.629*	0.092
	(0.390)	(0.385)	(0.180)	(0.362)	(0.339)	(0.175)
Observations	223	223	223	153	153	153
R^2	0.36	0.36	0.39	0.45	0.45	0.48
Partial $R^{2(a)}$	0.30	0.27	0.22	0.40	0.35	0.32
Wald test[(b)]		0.981	0.019		0.999	0.093
Sargan test[(c)]	0.942	0.942	0.494	0.998	0.998	0.959
DWH test[(d)]	0.016	0.004	0.121	0.046	0.014	0.221
Partial R^2 in reduced form regressions[(e)]						
Aid/GDP	0.71	0.68	0.67	0.75	0.78	0.74
Aid/GDP squared	0.34	0.32		0.43	0.46	
Aid/GDP x Policy	0.48		0.45	0.51		0.46

Note: The dependent variable is real per capita GDP growth. All regressions include time dummies for each period in the sample and dummies for Sub-Saharan Africa and East Asia. White's heteroskedasticity consistent standard errors in are in parentheses. [(a)]R^2 when the effect of time and regional dummies is partialled out. [(b)]The *p*-value of a Wald type test of the imposed restrictions. [(c)]The *p*-value of a Sargan type test of over-identifying restrictions. [(d)]The *p*-value of a Durbin-Wu-Hausman type test of equality of OLS and IV estimates. [(e)]R^2 in the reduced form regressions when the effect of the exogenous regressors in the growth equation are partialled out. * Significant at the 10 percent level. ** Significant at the 5 percent level.

Instruments: See Table 5. The instrument, $(\text{Aid/GDP})^2$ lagged, is not used in (9) and (12) as it leads to rejection of a test of over-identifying restrictions.

TABLE 5

INSTRUMENTS IN BURNSIDE AND DOLLAR'S REGRESSIONS AND IN TABLE 4

Specific to Burnside and Dollar	Common instruments	Specific to Table 4
Egypt dummy	Franc Zone dummy	Aid/GDP, lagged
Central America dummy	Policy x (logarithm of Initial GDP)	$(\text{Aid/GDP})^2$, lagged
Arms imports, lagged	Policy x (logarithm of Initial GDP)^2	(Policy x Aid/GDP), lagged
Policy x (Arms imports, lagged)	Policy x (logarithm of population)	
Policy x (logarithm of population)^2		
Logarithm of population		

seen from the Table the *p*-values of the restriction are out of the ordinary, making it difficult to maintain the assumption of important aid-policy interaction effects in the growth equation. In contrast, regressions (9) and (12) reveal that exclusion of aid squared and policy squared leading to Burnside and Dollar's preferred specification, is rejected quite strongly in the 56 country sample and marginally in the 40 country sample. Moreover, when the two variables are excluded, the effect of aid on growth becomes insignificant. Overall, these results underline that the insignificance of the aid-policy interaction is not caused by collinearity problems between the three aid regressors.

A new result in Table 4 is the significance of the Durbin-Wu-Hausman (DWH) test for equality of the IV and OLS results. As this test is often interpreted as a test of endogeneity we give quite strong evidence in favor of endogeneity of aid in all four regressions involving aid and aid squared.

Possible endogeneity of aid disbursements has been recognized since the early 1970s. Papanek [1972] was the first to argue that a negative correlation between aid and savings may be caused, in part, by a needs based allocation of aid. Yet, until the 1990s Mosley [1980] was the only study in which endogeneity of aid flows was taken into account in the econometric analysis. In the 1990s Boone [1994, 1996] has had a significant impact on later studies by his emphasis on endogeneity and the choice of instruments. Both Hadjimichael et al., Burnside and Dollar, and Lensink and White discuss endogeneity and Burnside and Dollar are inspired by Boone in the choice of instruments in their regressions. Interestingly, none of the studies find significant bias in the OLS regressions when they apply DWH type tests.

While Boone has only one endogenous regressor (aid) in his studies, Burnside and Dollar have two (aid and aid times policy), and there are three in Table 4 (aid, aid times policy, and aid squared). The increase in the number of endogenous regressors gives rise to increasing demands for good instruments in terms of variation and correlation with the endogenous regressors. The increase in demands is reflected in the choice of instruments in the studies. Boone [1996] use three different sets of instruments; (i) the log of population, (ii) Friends of US, Friends of OPEC, and Friends of France, and (iii) twice lagged aid. Burnside and Dollar [2000] combine the two first sets of instruments and add interactions with policy as seen from Table 5, while we in Table 4 use one of the political variables (Friends of France, denoted Franc Zone following Burnside and Dollar), lagged aid, and some of the interactions with policy.

The most important difference in the choice of instruments between Burnside and Dollar and Table 4 is that and Burnside and Dollar rely much on time constant dummy variables as instruments for aid; a dummy for Egypt (Friend of US), Friends of France, and a dummy for

Central America (see Table 5). In addition, the logarithm of population is only changing slowly over time. This means that the relation between aid and the instruments is mainly a cross country correlation leaving the time series variation in aid unexplained. Following Hansen and Tarp [2000a,b] we try to increase the time series variation and the identification of the individual regressors by including lags of the three endogenous regressors. As seen from the bottom part of Table 4 there is a substantial correlation between the endogenous regressors and the instruments even after the variation which is correlated with the exogenous regressors in the growth regressions has been removed. Furthermore, the Sargan type test reported in Table 4 does not lead us to reject the validity of the instruments.

In sum, we have shown that combining the specification from Hansen and Tarp [2000a] with the data from Burnside and Dollar [2000] lead us to the same conclusion as reached in Hansen and Tarp.

VI. CONCLUSION

In this paper we have reassessed the aid effectiveness results in “Aid, policies, and growth” by Burnside and Dollar [2000], using the same data set as the original study. We develop a neo-classical growth model in which aid spurs growth even in economies in which aid is consumed and we show that in this model the interplay between good policy and aid is ambigous. If anything, good policy is likely to reduce the growth effect of aid because they act as substitutes in the growth process. This shows that the Burnside and Dollar result is far from obvious.

The main outcome of the empirical re-examination is that the finding of a more positive impact of aid on growth in good policy environments is not a robust result. It depends crucially on deletion of a few influential observations. We show that once we apply sample selection procedures in which a single parameter of interest determines the estimation sample it is possible to obtain different results. In particular we obtain a positive effect of aid on growth in any policy environment.

A related result is that the Burnside and Dollar data is consistent with a non-linear relation between aid and growth in which there is diminishing returns to aid. This result conforms well to other recent empirical aid effectiveness studies.

Based on the above results we find it premature to apply policy selectivity rules in future aid allocations as advocated in chapter one of *Assessing Aid*. This is so even though applying the policy selectivity rule will, almost surely, increase returns to aid when these returns are measured as the correlation between aid and growth in income per capita. But this is because good policy leads to higher growth. None of the recent aid effectiveness studies question the importance of good policy. Yet, what is stressed in many of the papers challenging the Burnside and Dollar result is that aid effectiveness must be evaluated *after* we have conditioned on good policy. Once we condition on policy in the regressions we find that aid spurs growth regardless of the policy environment.

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A. THE INFLUENCE STATISTICS

In this appendix we list the formulas for the influence statistics used in the main text. The classical reference in econometrics is Belsley et al. [1980], but see also Cook and Weisberg [1982] for a likelihood oriented approach, and Chatterjee and Hadi [1986] (with comments) for comparisons of different measures and an enlightening discussion.

We consider the linear regression model

$$y = Xb + e \tag{4}$$

where y is a n-dimensional column vector, X is the $n \times k$ matrix of explanatory variables, b is the estimated k-dimensional coefficient vector, and e is the vector of residuals. The objective is to look at the effect on various quantities of omitting a single row of observations from the regression.

The leverage measure h_i is given as the diagonal elements of the least-squares projection matrix, it can be given as

$$h_i = x_i(X'X)^{-1}x_i' = \tilde{x}_i(\tilde{X}'\tilde{X})^{-1}\tilde{x}_i' + \frac{1}{n}, \tag{5}$$

where x_i is the i'th row of the matrix of regressors and a tilde denotes centered variables. Following Belsley et al. [1980] an observation is termed a leverage point if h_i exceeds $2k/n$. However as k is small compared to n in this study we will use the less stringent value $3k/n$ as suggested by Velleman and Welsch [1981].

The residuals can be scaled in several ways. In this paper we make use of the studentized residuals defined as

$$r_i = \frac{e_i}{s(i)\sqrt{1-h_i}} \tag{6}$$

where $s(i)$ is the root mean square error based on a regression in which the ith row is omitted and h_i is defined in (5). The studentized residuals may (loosley) be compared to a $t(n-k-1)$ distributed random variable.

The influence on the individual estimated coefficients of omitting the ith observation is calculated as the scaled change in the parameter estimate

$$\text{DFBETAS}_{ij} = \frac{b_j - b_j(i)}{s(i)\sqrt{(X'X)^{-1}_{jj}}}, \tag{7}$$

where $b_j(i)$ is the estimated coefficient based on a regression in which the ith row is omitted and $s(i)$ is defined above.

One advantage of using the scaled measure of change is that these are comparable across coefficients. The unit of measurement is (approximate) standard errors of the estimated parameters. Belsley et al. [1980] suggest to use $\pm 2/\sqrt{n}$ as cut-off values for influential observations but less stringent values (unity) are often used.

The overall influence on all parameters can be measured by the scaled change in the fitted value

$$\text{DFITS}_i = \frac{\hat{y}_i - \hat{y}_i(i)}{s(i)\sqrt{h_i}}. \tag{8}$$

As $s(i)\sqrt{h_i}$ is the root mean square error of the prediction, this measure is also in terms of standard errors and $\pm 2\sqrt{k/n}$ are typically considered as reasonable cut-off values for influential observations. Again there is an alternative suggestion to look at "one standard error" changes.

It is important to be aware of the limitations of these influence measures and of the dangers of a mechanical usage. Specifically, as noted by Belsley et al. [1980, 15]:

> "A word of warning is in order here, for it is obvious that there is room for misuse of the above procedures. High-influence data points could conceivably be removed solely to effect a desired change in a particular estimated coefficient, its t-value, or some other regression output. While this danger surely exists, it is an unavoidable consequence of a procedure that successfully highlights such points. It should be obvious that an influential point is legitimately deleted altogether only if, once identified, it can be shown to be uncorrectably in error. Often no action is warranted, and when it is, the appropriate action is usually more subtle than simple deletion."