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Does development aid help poor countries converge to our standard of living?

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SUMMARY: *Aid flows are included in the standard convergence equation and estimated using cross-country, panel and GMM regression. Robustness of the result is tested by changing the model and by adding extra variables. The main results are: that absolute convergence and absolute aid effectiveness are both rejected, and conditional convergence is accepted. Aid has an activity effect in the short run, but conditional aid effectiveness is found to be dubious. Finally, we try to divide the countries into an A-group where aid is effective and a B-group where it harms. Several criteria for division are explored, but none are really successful – the most satisfactory is the one that divides countries according to income.*

1. Introduction

Poverty in the less developed countries (LDCs) causes huge losses of welfare in the world. Thus, enormous welfare gains would result if poor countries were to converge to our standard of living. Many believe that development aid is an effective means of generating this convergence, see e.g. Sachs (2005). Since aid programs started in the mid 1960s, the average LDC has received about 7.5% of its aggregate GDP in aid per year.[1] Over 40 years this corresponds to three years of GDP, so it is substantial.

We thank Marias Gestsson for excellent research assistance. We also wish to thank Pia Wichmann Christensen, Chris Doucouliagos and Peter Sandholt Jensen for useful discussions. This paper has had a long gestation period, and we almost gave up due to the results, and when we finally finished the first version, Rajan and Subramanian (2005) came out with similar results. Also, Martin Paldam wants to thank the four students, who over the years have written their MA-thesis in the field. We refer to Kristiansen (2007) for unit-root and causality tests.

1. Aid is heavily biased toward small countries, and about 30% of the data are missing. Countries with no data probably receive below average, so aid to the aggregate LDC world is only about 2.5%.

However, cross-country growth rates are neither correlated to income levels nor to aid shares. The data reject both absolute effects: *There is neither absolute convergence nor absolute aid effectiveness*:[2] Poor countries do not converge, and aid flows seem to have no effect. Many researchers have found the two basic zero-effect results counterintuitive, and this has led to two bodies of empirical cross-country research, the convergence literature, CL, and the aid effectiveness literature, AEL. In both fields, the researchers have strived to demonstrate that by imposing more structure on the seemingly unrelated data, it is possible to make them tell a different – *conditional* – story. The two literatures use models that are curiously parallel, but they rarely refer to each other. The CL in particular rarely seems to refer to the AEL.

The main extra structure imposed on the basic models is to add sets of control variables. Obviously, it is possible to find control sets giving a wide range of results for both the convergence and the effectiveness effect. So, the control sets should be *reasonable*. The most reasonable sets aim at controlling for *country heterogeneity*. Further, the data may be divided into subsets with different explanations.

The two zero-correlation results have led to opposing policy conclusions: The lack of convergence has caused many to propose an increase in development aid, and the aid ineffectiveness result has caused many to doubt that aid works. So it is important to confront the results. This is precisely what we do in this paper, which took off from the idea that there might have been divergence without aid. Consequently, we start our quest from the idea that an answer to the question in the title could be found by merging the CL and the AEL models.

Table 1 introduces the models that define the terms as they are used in this article:[3] Equations (1) defines *absolute* convergence and aid effectiveness, while equations (2) to (4) are *conditional* models.

The many models used are normally presented as being derived from *the* economic theory but they may also be a result of researchers mining the data. This is not problematic in the large CL, where the basic model has a clear link to neoclassical growth theory, and has been analyzed by researchers with many different priors. However, the AEL (of about 100 papers) is smaller, and the theory is less well-established. Furthermore, the AEL is affected by a major prior: The profession is reluctant to publish

2. Both literatures work with unweighted country observations. If countries are weighted by population size, China and India come to dominate. The two giants are still poor, and they have both high growth and small aid shares. A weighting thus causes absolute convergence and negative aid effectiveness.

3. The terminology used is in accordance with the one used in the CL, see e.g. Barro and Sala-i-Martin (2004). Unfortunately, the AEL uses the word *conditional* in a more restricted sense. Consequently, we use the term *conditional aid effectiveness* models for a broader set of models than the ones covered by the AEL termed *conditional models*. They are discussed in Section 2.5

Table 1. The equations analyzed in the paper.

Separate, but parallel equations	CL: Growth equation		AEL: Aid effectiveness equation	
Absolute: Basic equation	$g_{it} = \alpha + \beta y_{it} + u_{it}$	(1a)	$g_{it} = \alpha + \mu h_{it} + u_{it}$	(1b)
Conditional: Controls	$g_{it} = \alpha + \beta y_{it} + \gamma_j x_{jit} + u_{it}$	(2a)*	$g_{it} = \alpha + \mu h_{it} + \boldsymbol{\gamma_j x_{jit}} + u_{it}$	(2b)*
Conditional: Fixed effects	$g_{it} = \boldsymbol{\alpha_{it}} + \beta y_{it} + u_{it}$	(3a)	$g_{it} = \boldsymbol{\alpha_{it}} + \mu h_{it} + u_{it}$	(3b)
(1) or (3) with lagged income	$g_{it} = \alpha + \beta y_{it} + \delta g_{it\text{-}1} + u_{it}$	(4a)	$g_{it} = \alpha + \mu h_{it} + \delta g_{it\text{-}1} + u_{it}$	(4b)

Merged equation		
Absolute: Basic equation	$g_{it} = \alpha + \beta y_{it} + \mu h_{it} + u_{it}$	(1c)
Conditional: Fixed effects	$g_{it} = \boldsymbol{\alpha_{it}} + \beta y_{it} + \mu h_{it} + u_{it}$	(3c)
Conditional: Fixed effects and one control	$g_{it} = \boldsymbol{\alpha_{it}} + \beta y_{it} + \mu h_{it} + \gamma g_{it\text{-}1} + u_{it}$	(3d)
(1) of (3) with lagged income	$g_{it} = \alpha + \beta y_{it} + \mu h_{it} + \delta g_{it\text{-}1} + u_{it}$	(4c)

Variables, coefficients, indices

g_{it}	Growth real per capita growth for $T = 5$ years	α, $\boldsymbol{\alpha_{it}}$	Constant, fixed effects for countries and time
y_{it}	Income, ln gdp, i.e. GDP per capita, PPP prices	β	Estimated coefficient of convergence
h_{it}	Aid share, ODA, in % of GDP	μ	Estimated effect of aid
$\boldsymbol{x_{jit}}$	vector of j controls, one control is not bolded	δ, γ_j	coefficients to be estimated
u_{it}	Residuals	i, t, j	Indices for countries, time and controls

Note: The models with * are not estimated in the present paper. Equation (3d) is used in Section 5. Versions of (3) and (4) are estimated with aid unlagged and with aid lagged, the latter is termed (3bL), (4cL) etc.

negative results on aid effectiveness, as demonstrated by Doucouliagos and Paldam's meta analysis (2007a).[4]

While we use the standard data, ours is the first paper to systematically analyze the CL and AEL models in a parallel way to study what happens when they are merged. It should, however, be noted that we do not examine or test the leading pro-aid models. This has been done in Jensen and Paldam (2006) and Doucouliagos and Paldam (2006b).

Section 2 surveys the theoretical framework and main findings of the two literatures. Section 3 presents the data and descriptive statistics. Section 4 systematically analyzes the cross-country relations, using OLS regression techniques, while Section 5 presents panel data estimates and tests the robustness by inclusion of extra variables. Section 6 tries to divide the countries into an A-group where aid helps and a B-group where aid harms. Section 7 discusses the results, and finally Section 8 summarizes the results.

4. The study considers the distribution of *all* published estimates of aid effectiveness. Meta analysis has developed a test (FAT) to analyze such distributions for asymmetries. These tests detect a significant asymmetry looking as predicted by the reluctancy hypothesis.

2. The CL and the AEL: Two bodies of literature

The models and variables discussed in the paper are listed in Table 1 for easy reference. The two sets of equations have great formal similarity. By far the largest literature is the CL. Here some agreement has been reached about the basic facts, so we are brief in our survey. In the AEL, little agreement has been reached. However, before we turn to the surveys, a general problem should be mentioned.

2.1. The mining observation

The AEL is the smaller literature of the two. The aid data starts in the mid 1960s, when aid started, and now amount to about 5000 annual observations. The models are estimated on data averaged over periods of four to ten years. This reduces the data to between 500 and 1200 observations. On subsets of these data 1025 regressions have been published, but many more have surely been run. Consequently, these data have been thoroughly mined.

The CL can use data as far back as they are available, but much more research has been done, so the mining done is probably even larger.

Test limits in econometrics are, by convention, the ones of *one* analysis run on virgin data.[5] Mining reduces the risk of Type I errors (rejection of true model) and increases the risk of Type II errors (acceptance of false models). Two conclusions follow: Results that are not clearly visible in the raw data – or follow from basic models – should only be believed after independent replication; that is, by other authors on new data. Results hinging upon controls that are not strongly justified should be treated with suspicion.

2.2. CL: Absolute convergence rejected. Conditional models find convergence

Recent surveys of the CL can be found in Barro and Sala-i-Martin (2004, pp 1-84 and 511-66) and Aghion and Durlauf (2005, especially chapters 1, 7, 8 and 9).

One of the key theories of economics is the neo-classical growth model. Under rather general assumptions, it shows that economies accumulate capital so as to reach the same steady state income per capita. A robust (theoretical) prediction from the model is that eventually all countries will converge to the same level of income.[6] The starting point for the convergence literature is the question: Does convergence actually happen?

Data for y and g cover many countries and five to twenty decades, and as mentioned they are basically uncorrelated. However, perhaps the lack of convergence is due to

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5. Formulas have been proposed in the econometric literature adjusting the significance limits of the tests to the degrees of freedom left. They have not been applied in the EAL, and to our knowledge, neither in the CL.
6. Two mechanisms secure the catch-up: (1) the logic of the model itself, i.e., diminishing returns to the two inputs (labor and capital), (2) technological catch-up.

country differences, so that convergence occurs if country heterogeneity is controlled for. This point was made in the classical study of cross-country growth patterns by Barro (1991) as extended in Barro and Sala-i-Martin (1995, 2004).[7] It uses a two step model:

Step 1 analyzes absolute convergence by equation (1a) $g_{it} = \alpha + \beta y_{it} + u_{it}$, which corresponds to the correlation between g and y. When it is estimated for rich countries only, see Baumol (1986), convergence does occur, but when the LDCs are included absolute convergence is rejected as $\beta \approx 0$.

Step 2 uses the two extended models (2a) or (3a) to control for country heterogeneity. Conditional convergence means that $\beta < 0$ in the extended model.

Version (2a) $g_{it} = \alpha + \beta y_{it} + \gamma'_j x_{jit} + u_{it}$. The j controls are country levels of, e.g., education, health, investment, governance, resources. A fairly broad range of the most credible x-sets turn β negative. It appears optimistic that 5-10 controls can account for all country differences, but it is appealing that the controls are concrete. However, each variable of the x-set normally has missing observations, so with a large x-set only a subset of the (g, y)-data can be used. Also, the choice of x-set gives the researcher considerable control over the estimate of β, causing a problem of moral hazard, as it allows him to tailor results to his priors.[8] However, comprehensive robustness tests have been made (by Doppelhofer, Miller and Sala-i-Martin, 2004, and Sturm and Haan, 2005) showing that a little more than 10 variables do have a robust effect on growth, while another 5 to 10 are borderline robust.[9]

Version (3a) $g_{it} = \alpha_i + \beta y_{it} + u_{it}$. The constant is decomposed into a set of *fixed effects* for countries, α_i.[10] This cannot be done in cross-country regressions, so panel techniques are used. Fixed effects for countries assume that country heterogeneity is constant for the period analyzed, so the models can be estimated for all data where observations for g and y are available. Fixed effects turn β negative.

Consequently, in the CL (2a) and (3a) tell almost the same story, though β is normally a little larger numerically in (3a) than in (2a).

2.3. AEL: Absolute aid effectiveness rejected. Conditional models disagree[11]

The AEL is covered by a number of surveys too; the two most recent are McGillivray

7. Several alternative methods are used to study cross-country growth patterns, see Aghion and Durlauf (2005).

8. These models are estimated by two- or three-stage estimators to control for reverse causality. Hereby a set of first stage instruments is also included, so the choices of the researcher become even larger.

9. Aid has never made it to either list, nor is aid mentioned in Aghion and Durlauf's two volume survey (2005) covering the CL and everything else known about economic growth.

10. Also, a set of fixed effects for time periods α_t is often included to delete international economic fluctuations.

11. The aid effectiveness discussion is less known. It started with theoretical papers by Friedman (1958) and Bauer (1971), who argued from an (explicit) libertarian position that aid goes to governments, and thus

continues ...

et al. (2006) and Doucouliagos and Paldam (2006b, 2007a).[12] Their conclusions are almost opposite:

McGillivray et al. is a qualitative study with assessed results. The key conclusion is that an upward kink appears in the results in the late 1990s. Before that it was unclear whether aid works, but now we know that it does. Doucouliagos and Paldam's surveys are quantitative (meta) studies which systematically compare all results. They show a steadily falling trend in the estimate for aid effectiveness, with no signs of a structural break. The AEL is a field where even the surveys disagree.

Step 1 considers absolute aid effectiveness, using the basic model (1b) $g_{it} = \alpha + \mu h_{it} + u_{it}$, where aid effectiveness means that $\mu > 0$. In most large data sets – see Tables 4 and 5 – absolute aid effectiveness is rejected as $\mu \approx 0$. This is uncontroversial. Step 2 is the two extended models (2b) and (3b) controlling for country heterogeneity:

Version (2b) $g_{it} = \alpha + \mu h_{it} + \gamma_j x_{jit} + u_{it}$. The j controls of the x-set are, once again, meant to control for country heterogeneity. In the results published $\mu > 0$, $\mu \approx 0$ and $\mu < 0$ in 38%, 56%, and 6% respectively.[13] However, as mentioned the distribution is clearly downward censored by the reluctancy bias, and no agreement has been reached as regards the right control set. Here it is worrying that the standard control sets that generate conditional convergence fail to generate conditional aid effectiveness.

Version (3b) $g_{it} = \alpha_i + \mu h_{it} + u_{it}$ uses fixed effects for countries. It is rarely used in the papers published, which is strange since fixed effects convert the cross-country data (in a basic way) into time series, precisely as demanded by the key policy question: What happens to development in the typical country if aid is either increased or decreased?

In choosing between version (2a) and (3a), we prefer the latter as it has six advantages: (1) Higher policy relevance; (2) simplicity; (3) it is void of moral hazard; (4) dummies are always available so the full (g_{it}, y_{it})-set can be analyzed; (5) the dummies are truly exogenous; and finally (6) it leaves the variables from the standard x-set for robustness tests, which we carry out in Section 5.2.

Many aggregate time series contain cyclical components. When the series are aggregated to 5 years, the cyclical component may appear as residual autocorrelation, as

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continued ...

encourages countries to pursue unsound socialist policies. The AEL, see Doucouliagos and Paldam (2006a), confirms that about 75% of the marginal activity financed by aid is public consumption. However, data for the degree of public ownership to trade and industry are not strongly correlated to the share of public consumption. The empirical AEL started with a critique of aid by Griffin (1970) and Weisskopf (1972), who argued from an (explicit) left wing position, and demonstrated that aid did not cause increased capital accumulation. Since then aid data have multiplied and almost 100 papers have been published.

12. Christensen, Doucouliagos and Paldam (2007a) is a master list of the AEL, while Christensen, Doucouliagos and Paldam (2007b) lists the even larger literature dealing with aid allocation, i.e. with the reverse causality.

13. The percentage numbers in the brackets are from Doucouliagos and Paldam (2007a).

found below. Therefore we include lagged growth, $\delta g_{it\text{-}1}$, in models (4a) and (4a). Sections 4 and 5 show that the term is often significant, but does not have much effect on the other coefficients estimated.

2.4. Third generation AEL-studies: The division hypothesis of an A- and a B-group

Another way to consider the absolute aid ineffectiveness result is to adopt the *division hypothesis*: where the countries are divided into an A-group of countries where aid works and a B-group where it harms.

This also happens if you toss a coin: In half the cases you win, and in the other half you loose. You can always find a division criterion ex post that divides a series of tosses into an A-group of heads and a B-group of tails. However, if the criterion is subjected to independent replication, it may fail. Thus, a credible division condition, z, is needed, and (as usual) it is only believable after independent replication.

A neat formalization results if z is scaled to have approximately as many positive as negative values. Then the A-group occurs for $z > 0$, and the B-group occurs for $z < 0$ and aid effectiveness can be estimated by one regression (5a).

$$g_{it} = \alpha + \sigma z_{it} + \mu h_{it} + \omega z_{it} h_{it} + u_{it} \tag{5a}$$

$$g_{it} = \alpha + \mu h_{it} + \omega h_{it}^2 + u_{it} \tag{5b}$$

The model (5b) is slightly different as $z = h$ is used as the interaction term.

By far the most influential conditional model is Burnside and Dollar's *Good Policy Model* (2000),[14] where z is an index of good policy, which is scaled to divide the countries in two groups as mentioned. The original estimate was $\sigma \approx 1$, $\mu \approx 0$ and $\omega > 0$. The good policy index is almost an index of outcomes, so it is trivial that $\sigma > 0$, and $\mu \approx 0$ is not surprising either. However, it is important that $\omega > 1$, as it means that aid increases growth in the A-group of countries with good policies, while it decreases growth in the B-group of countries with bad policies. This result has appeared credible to many development practitioners, and it was popularized in World Bank (1998), *The Economist*, etc.[15]

The second most studied conditionality model is the *Medicine Model* (5b), where aid is interacted with itself. This idea has been pursued by several authors – most vi-

14. Burnside and Dollar use the EDA aid data from Chang, Fernandez-Arias and Serven (1998), where each aid loan/gift is cleaned for non-grant elements. The results are seemingly invariant to this refinement as the standard ODA and the new EDA data have a coefficient of correlation of 0.83.

15. It was also advocated in Paldam (1997a) and in several popular articles by the author till he attempted to replicate the model on new data; see Jensen and Paldam (2006), which also analyzes the Medicine Model.

gorously by Hansen and Tarp (2000). Their findings are that $\mu > 0$ and $\omega < 0$,[16] so that while some aid increases growth, too much aid is harmful, just like medicine. Thus, the model predicts that an optimal dose of aid, h^*, exists. Tables 6a and 6b below include aid squared, with no effect.[17]

Both models can be extended with controls or fixed effects. As at the start of 2005 no less than 31 papers have proposed 10 conditioning variables.[18] The pace of publications during the last five years predicts that many more studies in this family may appear during the next five years. For reasons given above, the key criterion for the credibility of an effect is that it has been independently verified by other researchers using new data. This has only been done for the two models mentioned. Here the replication failed, so none of the proposed conditioning factors have, as of now, been established.

2.5. Three types of possible biases in the AEL

In estimates of cross-country equations between macro variables, it is difficult to single out an equation where the explanatory variables are fully independent and exogenous. It is thus always possible to point to possible biases – this is also the case at present. We shall discuss the possible biases under three headings:

(B1) ***Reverse causality***: Many studies adjust the estimates for reverse causality (by TSLS or GMM estimates), but this appears to have little effect on the results (see Doucouliagos and Paldam 2007a).[19] The reason becomes apparent when we turn to the aid allocation literature. A priori the effect of growth on aid is unclear: (*i*) It is negative if aid is given to alleviate crisis. (*ii*) It is positive if aid is used to finance projects with high benefit/cost ratios, as countries with high growth generate more such projects. A total of 30 studies analyze the effect of recipient growth on aid allocation.[20] The 211 estimates published are generally small, but on average positive so (ii) dominates – see

16. In a model where aid has no effect, the addition of aid squared will – due to the correlation of aid and aid squared – cause the coefficients to aid and aid squared to move in the opposite direction, so that one becomes positive and the other negative. With a bit of luck and the right choice of control variables, both may turn significant. However, it appears to be equally easy to get either of the two sign combinations.

17. The two models are discussed and tested in considerable detail in Jensen and Paldam (2006) and in Doucouliagos and Paldam (2006c). Therefore, we treat the two models briefly at present.

18. Doucouliagos and Paldam (2006b) is a meta-study of the 31 papers.

19. In discussions of aid effectiveness, it is surprisingly common to meet the classical mistake of confusing flows (growth rates) and stocks (income levels). It is wrong to argue that a reverse causality bias exists in the growth-aid relation because we know that aid is negatively correlated to the income level. However, it is a good reason to control the growth-aid relation for the income level, as done below. Figure 1 shows how the relation between growth and aid goes to zero as the period, T, becomes small and increasingly negative as T rises.

20. See Doucouliagos and Paldam (2007c) on the effect of growth aid and Doucouliagos and Paldam (2007d) on the effect of country size and income on aid.

however Figure 2. To deal with the potential bias, we lag the aid variable relative to the *g* and *y* variables in Section 5, and we also control some of the estimates with GMM.

(B2) Not distinguishing between ***activity and capacity effects***. We want aid to cause development, that is, growth. We know that aid leads to public spending (see note 11), so consequently aid must lead to economic activity in the short run. Activity has multiplier effects, so we can count on aid to cause an increase in *y* in the same year, and for perhaps two more years – this will appear as a short-run positive correlation between aid and growth as shown in Section 3. This is not a growth effect. It would appear even in the proverbial case where the aid is used to finance the digging of holes and filling them up again. It appears that some of the positive aid-to-growth effects reported in the AEL are activity effects.

(B3) ***Omitted variable biases***: Many have been proposed, but only a couple have been confirmed. Two of these are: (B3.1) The big country bias: Large LDCs tend to grow faster than small ones and to receive less aid. (B3.2) The scaring-away bias: Bad rulers in particular tend to scare away both aid and investment, see Levy (1988). We have decided to disregard these biases, assuming that they cancel each other out. However, we control for the Gastil index for democracy in Section 5.

Our conclusions from the AEL review are: (R1) Absolute aid effectiveness is rejected. (R2) Weak and non-conclusive conditional relations from aid to growth have been found. (R3) Aid may work differently in different countries. That is, we may be able to identify an A-group of countries, where aid works, and a B-group, where it rather harms.

2.6. Merging the models – what may happen?

When the two equations (1a) and (1b) are merged, they become: (1c) $g_{it} = \alpha + \beta y_{it} + \mu h_{it} + u_{it}$, as shown in Table 1 The same happens to equations (2a) and (2b), etc. In each set of equations, (*#a*) and (*#b*) are *unmerged*, while equation (*#c*) is the *merged* version. We thus get an unmerged estimate of β and μ and a merged set of estimates of the same two variables.

The main purpose of our study is to systematically examine what happens to the two coefficients, β and μ, when the merged equation is estimated. Table 2 lists the five possibilities. One possible result is that aid and convergence is basically independent, but as aid shares are correlated with poverty, it is only possible if aid effectiveness is zero, $\mu \approx 0$. The other possibilities show that aid and convergence are interdependent. When we started, we hoped for possibility 1, where aid is effective and prevents divergence. As the reader will see, the results are closer to possibility 4.

3. Data and some descriptive statistics

This section describes the data and takes a first look at some descriptive statistics,

Table 2. The five possible effects of merging the two basic equations.

Possibility 1	Merger causes $\beta\downarrow$ and $\mu\uparrow$
Interpretation	Aid works. Less convergence with no aid
Possibility 2	Merger causes $\beta\uparrow$ and $\mu\uparrow$
Interpretation	Aid works. More convergence with no aid. Inconsistent
Possibility 3	Merger causes $\beta\uparrow$ and $\mu\downarrow$
Interpretation	Aid harms. More convergence with no aid
Possibility 4	Merger causes $\beta\downarrow$ and $\mu\downarrow$
Interpretation	Aid harms. More divergence with no aid. Inconsistent
Possibility 5	Merger has no effect on β and μ
Interpretation	Convergence is independent of aid. Consistent if $\mu \approx 0$

Note: Two inconsistent possibilities are shaded in grey.

showing the structure of correlations. PWT is Penn World Tables and WDI is World Development Indicators. See the reference list for the web addresses of all sources.

3.1. Data: Three main time series and six control variables

Growth rate for GDP per capita, $g = (\ln(GDP_t) - \ln(GDP_{t-T}))/T$, where T = averages over 5 years. In fixed domestic prices that reflect the trade-offs agents actually face. Source WDI.

Initial gdp, y = logarithm of RGDPCH (real GDP per capita in 1985 international prices, chain index). Source PWT.[21]

Aid share, h = ODA/GDP, both in current US \$. ODA (Official development assistance) is net disbursements of loans and grants made by official agencies of the members of DAC and some Arab countries to promote economic development and welfare in recipient economies listed as developing by DAC. Only loans with a grant element of more than 25% are included. ODA also includes technical cooperation and assistance. Source WDI.[22]

Investment share: This is an average of real gross domestic investment, private and public, in proportion to GDP. Source PWT.

Inflation: Defined as infl/(1+infl) where infl is the average of the log difference of the GDP deflator. This transformation reflects the magnitude of the inflation distortion

21. See Nuxoll (1994) on the combination of WDI-data in local prices and Penn World Tables in PPP prices.
22. ODA refers to aid flows from official donors to LDCs and the transition economies of Eastern Europe and the former Soviet Union as well as to certain advanced developing countries and territories as determined by DAC. Official aid is provided under terms and conditions similar to those for ODA.

Table 3. Averages in 1971-2000 based on 72 aid recipients where full data set exists.

	Sorted according to growth			Sorted according to aid	
Three fractiles	Growth	Aid share	Three fractiles	Aid share	Growth
Slowest growth	-1.7%	9.9%	Highest aid	14.3%	-0.4%
Middle group	0.8%	5.9%	Middle group	4.7%	1.1%
Fastest growth	3.5%	3.9%	Lowest aid	0.7%	1.8%

Note: The averages are unweighted. Guinea-Bissau and Sao Tome and Principe are excluded. They have had moderate growth, but an average aid inflow of no less than 48% and 62% respectively.

in production, see Herbertsson (1999) and, equivalently, the implicit inflation tax rate. It attempts to capture the nonlinear relationship between growth and inflation: Growth is thus less sensitive to an increase in inflation from 500 to 600% per year than, say, an increase from 2 to 100% per year. The deflator is derived by dividing current price estimates of GDP at purchaser values (market prices) by constant price estimates. Source WDI.

External debt: The average of foreign debt divided by GDP at market prices. Foreign debt consists of the outstanding stock or recognized direct liabilities of the government to the rest of the world, generated in the past and scheduled to be extinguished by government operations in the future or to continue as perpetual debt. Source WDI.

Openness: The average of the sum of exports and imports of goods and services divided by GDP. Exports (imports) of goods and services represent the value of merchandise exports (imports) plus amounts receivable from (payable to) nonresidents for the provision of nonfactor services to residents. Nonfactor services include transportation travel, insurance, and other nonfactor services such as government transactions and various fees. Source PWT.

Economic freedom: The logarithm of economic freedom. Source Fraser Institute.

Political freedom: The Gastil index of political freedom. Source Freedom House.

3.2. Some correlations

Table 3 shows a negative connection between aid and growth. This can be interpreted in the two ways the data are sorted: (*i*) The countries with the slowest growth receive most aid; or (*ii*) the more aid countries receive the slower they grow. If argument (*ii*) is true, then aid is harmful. The top line in the table considers (72/3 =) 24 countries with average aid shares of 14.3% and an average growth of -0.4%, while the bottom line shows that the 24 countries that receive 0.7% in aid grow by 1.8%, no less than 2.2% faster per year.

Figure 1 gives a systematic presentation of the coefficiciens of correlations calculated for the periods $T = 5, 10, \dots, 45$ years. For $T = 5$ each of the two points are the

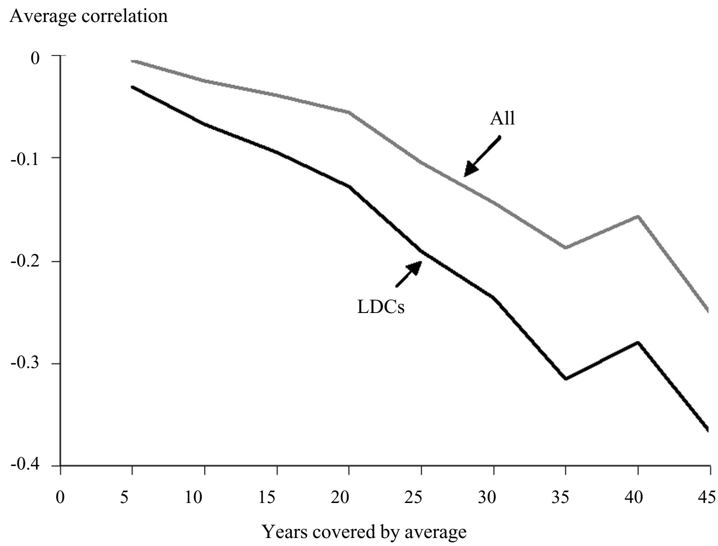

Figure 1. Correlations between average growth and the average aid share.

average of 9 correlations, where the first is for all observations with averages from 1960-65, the second is for all observations from 196570, etc. For $T = 10$, the points are the averages of the 8 correlations, where the first is from 1960-70, the next is from 1965-75, etc. This continues until $T = 45$ which contains only one correlation from 1960-2005.

For small T's, the correlation is zero, but as T goes up, the correlation turns more and more negative as it increasingly come to reflect the relation between the aid share and the level of incomes.

3.3. Correlograms: A first look at the dynamics

Figure 2 shows average correlograms for the annual data for the same 72 countries analyzed in Table 3. For the data of country i, we have calculated the correlogram $c_{ij} = \text{corr}(h_{it}, g_{it} + j)$, where $j = -14, -9, \ldots, +14$. This generated 72 correlograms, but we only present (2 × 3) averages, corresponding to the (2 *x* 3) data sets of Table 3. Each point is calculated from between 1'152 and 2'160 observations, with less at the two ends and most at the center. The correlations move by 0.2 to 0.3 around the zero axis, so even when most of the averages are in the range from -0.1 to +0.1, and of dubious significance, there are nevertheless some movements in the curves.[23]

23. If data were independent between countries, we could use the significance limits of about ±0.12. However, some cross-country correlation exists, and the true significance levels are higher. They are hard to calculate, but they are likely to be between ±0.15 (df = 400) and ±0.20 (df =100).

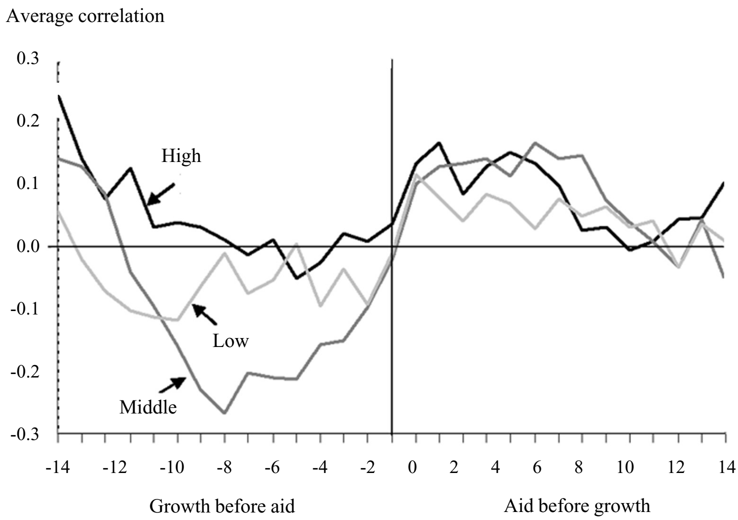

Figure 2a. Aid-growth correlograms for the 72 countries of Table 3, sorted by aid share.

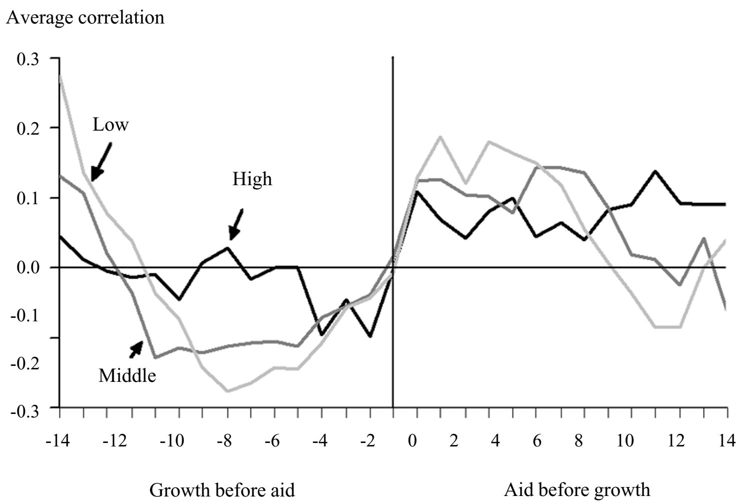

Figure 2b. Aid-growth correlograms for the 72 countries of Table 3, sorted by growth.

It is important to note that the figures provide no information about the catch-up of poor countries. They only illustrate the dynamics, and hence the causality, between aid and growth within the average country of each group. It is hence a starting point when we adresss two biases: (B1) the reverse causality bias and (B2) the activity effect that should be separated from the growth effect.

For $j = 0$, we look at the simultaneous relationship between the two variables – it is almost zero. However, to both sides there are clear signs of some causal connection.

To the left is the ***growth-before-aid*** part of the graphs. This part analyzes the causal link from growth to aid. Here we see – on most of the 6 curves – a *negative hump* for $j = -10$ to -2, so an economic crisis may give a little more aid.

To the right is the ***aid-before-growth*** part of the graphs. This part analyzes the causal link from aid to growth. For $j = 10$ to 14, we see how aid in year t is correlated with growth 10 to 14 years later. With such a long lag, there should be very little to see, as is indeed the case. However, for $j = 1$ to 8, we see the effect of aid in the form of a *positive hump* on curves. The positive humps are in the order of 0.05 to 0.15, so the growth hump due to aid is small. Also, Figure 2a shows that both humps are largest for countries receiving the least aid, and smallest for the countries receiving most aid. In Figure 2b, the hump is lowest for the countries with the lowest growth – countries that already receive high aid and have low growth benefit little from additional aid.

This interpretation of the two figures is problematic for two reasons. First, the largest positive growth effect (hump) is found in the countries receiving less than 1% in aid. This appears to be unreasonable. Second, it is well known that correlograms are sensitive to cyclical movements in the series. That is, some of the systematic movements may be cyclical movements in the growth rate and the level of aid. In other words, the positive humps on the right hand side of the two figures may be an upturn automatically following the downturn shown on left hand side.

An example demonstrates this problem: Imagine that a poor country has a civil war lasting some years, causing negative growth, and aid is stopped. Then peace returns, a lot of aid comes in, and there is also a resumption of normal activity, which results in high growth.

This will generate a cyclical pattern in both variables that makes it look as if aid is a much more powerful variable than it actually is. Much the same story can be told of droughts.

The possibility of cyclicality explains why we include the term $\delta g_{it\text{-}1}$ in equation (4) of Table 1. With lags of five years between growth and past growth, cyclicality should cause a negative coefficient to past growth. We expect some multicollinearity between the effect of aid and the effect of past growth if past growth produces negative coefficients, but here the results are somewhat mixed.

3.4. On the property of the data and causality tests

Before we start on the regressions we should mention that many authors have discussed the statistical properties of the data and models we use.

Table 4a. Explaining growth by aid and income level, all countries Stacked OLS regressions with fixed effects for periods, N = 970.

Model	Income, β	Aid, μ	Aid lagged, μ	Growth lagged, δ	AR^2	MAR^2(aid)
1a	**0.398** (4.1)				0.332	[0.011] (*a*)
1b		**-0.047** (-4.2)			0.332	0.011
1b lag			-0.019 (-1.6)		0.322	0.001
1b both		**-0.103** (-5.1)	**0.070** (3.3)		0.339	0.018
1c	**0.258** (2.3)	**-0.031** (-2.4)			0.335	0.003
1c lag	**0.424** (3.8)		0.006 (0.5)		0.332	-0.000
1c both	**0.312** (2.8)	**-0.090** (-4.4)	**0.077** (3.7)		0.344	0.012
4a	**0.056** (2.6)			**0.184** (6.3)	0.358	[0.004] (*a*)
4b		**-0.038** (-3.4)		**0.189** (6.6)	0.361	0.007
4b lag			-0.015 (-1.3)	**0.200** (7.0)	0.354	-0.000
4b both		**-0.083** (-4.2)	**0.056** (2.7)	**0.181** (6.3)	0.365	0.011
4c	0.118 (1.0)	**-0.031** (-2.4)		**0.183** (6.3)	0.361	0.003
4c lag	**0.256** (2.3)		-0.000 (-0.0)	**0.184** (6.3)	0.357	-0.001
4c both	0.169 (1.5)	**-0.077** (-3.8)	**0.061** (2.9)	**0.172** (5.9)	0.366	0.008

Note: AR^2 is the R^2 adjusted for degrees of freedom. $AR^2 = 0.321$ for the model with fixed effects for the 8 periods only. The MAR^2(aid) is the marginal AR^2 due to the one or two aid variables as appropriate. The average MAR^2(aid) is 0.006. (*a*) The MAR^2 for income.

Table 4b. Same as Table 4b but for LDCs only, N = 749.

Model	Income, β	Aid, μ	Aid lagged, μ	Growth lagged, δ	AR^2	MAR^2(aid)
1a	**0.222** (4.9)				0.306	[0.022]
1b		**-0.029** (-2.4)			0.289	0.005
1b lag			-0.004 (-0.3)		0.284	-0.001
1b both		**-0.088** (-4.0)	**0.075** (3.2)		0.298	0.013
1c	**0.266** (5.7)	**-0.046** (-3.8)			0.318	0.012
1c lag	**0.235** (5.1)		-0.017 (-1.3)		0.307	0.001
1c both	**0.266** (5.8)	**-0.106** (-4.8)	**0.075** (3.3)		0.327	0.021
4a	**0.177** (3.9)			**0.178** (5.5)	0.332	[0.013]
4b		**-0.026** (-2.2)		**0.199** (6.1)	0.323	0.004
4b lag			-0.006 (-0.5)	**0.202** (6.2)	0.319	-0.001
4b both		**-0.073** (-3.3)	**0.059** (2.5)	**0.189** (5.8)	0.328	0.008
4c	**0.218** (4.7)	**-0.041** (-3.4)		**0.169** (5.2)	0.341	0.009
4c lag	**0.190** (4.1)		-0.017 (-1.3)	**0.178** (5.5)	0.333	0.001
4c both	**0.222** (4.8)	**-0.090** (-4.1)	**0.062** (2.7)	**0.158** (4.8)	0.347	0.015

Note: See Table 4a. $AR^2 = 0.285$ for the model with fixed effects for the 8 periods only. MAR^2(lagged income) is 0.034. The average MAR^2(aid) is 0.007.

It is all quite bulky to present, but fortunately Kristiansen (2007) contains careful tests on virtually the same data used in this paper.

We have found no indication that it is problematic to estimate the models we present from the diagnostic tests – as the matrices can be inverted, there is no singularity, and hence no unit roots.[24] Also, Kristiansen (2007; 50-54) contains a set of unit root tests that indicate that the are no problems. This is probably because we use (non-overlapping) 5-year time periods, which reduce the autocorrelation in the series.

The data permits us to run Granger causality tests, Kristiansen (2007; 54-58). They show precisely what could be expected from Figure 2: With a short time period ($T = 1$ to 4), there is causality both ways, but with increasing lags both causalities quickly vanish.

4. Cross-country estimates: The pattern of coefficient changes

In this section, we look for the contribution of aid to catch-up in a standard cross-country convergence equation. As in section 5, we use a basic time period of 5 years. This gives us 9 periods between 1960 and 2005. However, the first period is used for lags. All regressions are run for 1965 to 2005.

4.1. The basic cross-country regressions

Table 4 shows the basic cross-country regressions for the largest sample the data permits, 1965 to 2005, (Table 4a) and for LDCs alone (Table 4b). No control variables are included, and no countries have been deleted for any reason other than missing data. Each section contains stacked OLS-regressions with fixed effects for periods.

Thanks to the many observations included, most coefficients in both tables are significant; even when the power – measured by their marginal AR^2-scores – of the income term and the aid term(s) are modest. In particular, the MAR^2(aid) in the 24 estimates presented are less than 0.007 on average.

However, the results do give a significant divergence in all but two of the regressions. The aid variables always give negative coefficients when only one is included. When both aid variables are included, their sum is negative in all four cases. So the results point to absolute aid ineffectiveness. Also, in most cases the merger of the two equations causes possibility 3 from Table 2 – the worst outcome. However, the movements in the coefficients are small indeed.

5. Panel estimates: Robustness to extra variables

Table 5 presents the same results as in Table 4, but for the panel version of the model.

24. Doucouliagos and Paldam's meta analysis (2006b, 2007a and b) include variables indicating the statistical techniques used. It is found that new data are far more important for the results than new estimators.

We have here used all available data between 1960 and 2005, divided in 9 periods of 5 years. We include fixed effects for countries and this changes the results, notably for the income and the lagged growth variables. For some of the regressions we also use Arellano and Bond GMM-estimates to control for simultaneity. In this section, the program is allowed to pick the highest number of observations possible for each regression. Section 5.3 contains a set of robustness tests.

5.1. Conditional convergence confirmed

When we control for fixed effects for countries, we always get conditional convergence, $\beta < 0$. All simple panel estimates of β are in the small range from -0.038 to -0.051, with *t*-ratios between 9 and 13. The β's barely react to neither the merger between the two models, nor to the inclusion of lagged growth. In fact, when we try the six controls in 5.3, the estimate of β is still unchanged. Conditional convergence is thus confirmed as expected.

Tables 5a and 5b present the GMM-estimates using the Arellana-Bond estimator. These estimates actually decrease the estimated β's. In fact, the estimates here appear unreasonably large (numerically). For reasons explained in Section 2.5, we prefer the OLS-estimates.

In Barro and Sala-i-Martin (2004; Chapters 11 and 12), it is argued that the estimates for β are in the range of -0.015 to -0.040. However, the meta-analysis by Abreu et al (2002) covering 48 studies shows a rather large range of estimates. It is difficult to imagine how fast countries would converge if they had precisely the same starting point and just differed by the income level.

5.2. Aid ineffectiveness remains

While the convergence coefficient, β, becomes significant, the aid effectiveness coefficient, μ, remains dubious. Of the 24 estimates, there is an almost equal number of positive and negative coefficients. And the GMM estimates are smaller than the ones of the panel estimates. There is no sign of a negative bias in the OLS-estimates. If there is a bias, it is upwards, as expected from the studies of the effect on growth of aid allocation.

We observe that μ has a systematic change of sign. When aid is unlagged, it is always insignificant and mostly negative, but when lagged, it is positive and significant in all four lagged regressions (3aL) and (4aL). At first glance, we thus find that aid works. However, as soon as we merge the equations – that is, control for development level – all four estimates of μ fall and become significantly negative or insignificant if lagged. This also happens in the GMM-estimates.

The coefficient of past growth plays a small role in the models, but it does suggest that there is a cyclical component in growth. This did not appear in the cross-

Table 5a. Panel estimates for all countries with fixed effects for countries and time periods.

Model	Start	Income, β	Aid, μ	Aid lagged, μ	Growth lagged, δ	N	NC
3a	60	**-0.039** (-11.8)				1214	178
3b	60		0.017 (0.8)			1200	182
3bL	65			**0.065** (3.4)		1038	179
3c	60	**-0.043** (-12.6)	-0.029 (-1.4)			1144	174
3cL	65	**-0.038** (-11.2)		0.021 (1.1)		1003	174
4a	65	**-0.041** (-11.2)			0.003 (0.1)	1057	177
4b	65		0.014 (0.6)		***-0.054*** (-1.7)	1053	180
4bL	65			**0.070** (3.4)	***0.045*** (1.6)	1018	179
4c	65	**-0.046** (-12.0)	***-0.044*** (-1.9)		-0.037 (-1.2)	1013	174
4cL	65	**-0.039** (-11.3)		0.021 (1.0)	**0.079** (2.9)	985	174
			GMM-estimates using the Arellano-Bond estimator:				
4a	70	**-0.123** (-22.5)			0.006 (0.2)	878	173
4b	70		***-0.056*** (-1.8)		**0.108** (3.0)	868	174
4bL	70			**0.076** (2.7)	**0.162** (4.1)	834	167
4c	70	**-0.123** (-24.0)	**-0.116** (-4.6)		**-0.068** (-2.5)	835	170
4cL	70	**-0.118** (-22.4)		-0.025 (-1.1)	0.006 (0.2)	807	164

Note: Calculated by STATA 9 that does not report R^2's.

Table 5b. Same estimates as in Table 5a, but for LDCs only.

Model	Start	Income, β	Aid, μ	Aid lagged, μ	Growth lagged, δ	N	NC
3a	60	**-0.042** (-9.3)				912	137
3b	60		0.016 (0.6)			891	142
3bL	65			**0.060** (3.0)		763	140
3c	60	**-0.048** (-10.1)	-0.036 (-1.5)			853	135
3cL	65	**-0.040** (-8.5)		0.018 (0.9)		738	135
4a	65	**-0.045** (-8.9)			**0.024** (9.3)	789	137
4b	65		0.009 (0.4)		***-0.066*** (-1.7)	782	141
4bL	65			**0.065** (2.9)	***0.054*** (1.6)	749	140
4c	65	**-0.051** (-9.6)	**-0.050** (-1.9)		-0.024 (-0.7)	753	135
4cL	65	**-0.042** (-8.9)		0.015 (0.7)	0.111 (3.4)	726	135
			GMM-estimates using the Arellano-Bond estimator:				
4a	70	**-0.124** (-17.7)			0.044 (1.2)	651	133
4b	70		***-0.062*** (-1.8)		**0.081** (2.0)	638	135
4bL	70			**0.064** (2.1)	**0.156** (3.5)	606	129
4c	70	**-0.126** (-19.0)	**-0.119** (-4.2)		-0.042 (-1.4)	615	131
4cL	70	**-0.121** (-17.7)		-0.028 (-1.1)	***0.055*** (1.6)	588	126

Note: See note to Table 5a.

country regressions (Table 4) because past growth worked as a proxy for country differences. When it is included, it nearly always reduces the coefficient to aid. If we accept this interpretation, we may go back to Figure 2 and interpret the growth effect we thought we found at lags $j = 1$ to 8 as a cyclical effect.

Our basic result is thus is that aid has a negative rather than a positive effect on growth, but all in all this is dubious. As the numbers of observations are from 588 to 1214, even small effects should show up as significant. The results are much like the cross-country results as regards aid. Nothing is credible and significant even though the other two variables produce statistically significant results, and a very stable conditional convergence coefficient is found.

The two changes when the model is merged can be calculated for 12 (dependent) pairs. The values of $\Delta\beta$ change signs, and are small, with the average $\Delta\beta \approx$ -0.001 (0.2). However, $\Delta\mu$ is negative in all cases, with an average value of $\Delta\mu \approx$ -0.059 (3.3), where the parentheses contains *t*-ratios for cross-estimate stability. The size of $\Delta\mu$ is substantial relative to the size of the coefficients, and shows that the positive effect of aid lagged in the non-merged equation is an artifact. Thus, as in Section 4, we find no clear evidence of aid effectiveness. We cannot conclude that aid has prevented convergence, but only that if the level of GDP is not controlled for, the estimate is misspecified.

5.3. Extra variables – some robustness tests

Table 6a tests the basic equation (3c) of Table 5 for robustness by including a set of extra variables. Table 6b does the same for (3cL). We have included only variables with some basis in economic theory, so that each of the new variables included might tell a story.

The first line repeats the regression tested from Table 5a (but for a shorter period), and then six extra variables are included one at the time. The convergence coefficient stays as constant as one could wish. However, the coefficient to aid also remains largely stable. Only one of the extra variables changes the coefficient in size, but two of them destroy the significance, so the robustness of the aid effect is also reasonably good. The economic interpretation is straightforward:

Investment is known to be the most robust variable in growth regressions since Levine and Renelt (1992), and investment does get a large and significant coefficient in the estimate. However, it leaves the two other coefficients unchanged. The aid flows and domestic investments give rise to no multicollinearity. The next four variables are meant to catch aspects of domestic policies.

High inflation and high debt are strong signs of unsuccessful policies. However, while the inflation tax is an alternative to borrowing money, aid is not all gifts, so aid

Table 6a. Equation (3c) with one control variable, γx.

Aid unlagged	β	μ	γ	R^2	N	NC
Extra variable *x* =	Catch-up	Aid effect	Effect of *x*			
Equation (3c)	**-0.047** (6.89)	-0.050 (1.56)		0.43	610	141
Investment	**-0.044** (6.52)	-0.040 (1.27)	**0.135** (3.48)	0.44	595	129
Inflation	**-0.046** (7.15)	**-0.053** (1.74)	-0.000 (0.04)	0.44	587	128
Debt burden	**-0.060** (7.62)	**-0.082** (2.41)	**-0.001** (6.63)	0.46	547	126
Openness	**-0.055** (7.99)	**-0.065** (2.10)	**0.050** (4.86)	0.46	595	129
Economic freedom	**-0.054** (7.86)	**-0.075** (1.71)	**0.067** (7.66)	0.53	448	98
Gastil index	**-0.049** (6.48)	-0.046 (1.38)	0.001 (0.08)	0.42	562	130
Aid squared	**-0.048** (6.96)	**-0.085** (1.76)	0.057 (0.97)	0.43	610	141

Note: This is done for the data from 1970-2000. Fixed effects for countries are included. Once more we have done everything for the LDCs alone and found the same pattern at a marginally lower level of significance.

Table 6b. Equation (3cL) - for aid lagged - with one control variable, γx.

Aid unlagged	β	μ	γ	R^2	N	NC
Extra variable *x* =	Catch-up	Aid effect	Effect of *x*			
Equation (3cL)	**-0.046** (6.07)	0.037 (1.43)		0.54	499	141
Investment	**-0.042** (5.60)	0.036 (1.41)	**0.121** (2.77)	0.55	484	128
Inflation	**-0.046** (6.03)	0.043 (1.69)	**0.020** (2.01)	0.55	481	128
Debt burden	**-0.047** (5.29)	0.043 (1.63)	0.001 (1.28)	0.53	450	126
Openness	**-0.050** (6.75)	0.013 (0.51)	**0.050** (4.84)	0.57	484	128
Economic freedom	**-0.058** (7.10)	-0.066 (1.54)	**0.059** (6.06)	0.56	391	98
Gastil index	**-0.048** (5.55)	0.041 (1.51)	0.001 (0.10)	0.54	456	130
Aid squared	**-0.046** (6.04)	0.038 (1.44)	-0.008 (0.54)	0.54	499	141

Note: See note to Table 6a.

adds to debt. These connections are quite visible in our regressions – there is an uncannily strong (and unreasonable) negative effect of aid when the debt burden is added. However, when the aid variable is lagged the effect disappears.

The effect of adding the economic freedom index decreases the effect of aid. This is likely to be an effect of multicollinearity, but we note that the economic freedom index has a rather strong effect on growth. Our experience is that it works better in log form, indicating that while it helps to go from a very highly restricted economy to a more liberal one, the additional effect of going all the way to laissez faire is not so large. Also, we have tried the Gastil index of democracy with little success. Finally we controlled for aid squared to test the Medicine Model from Section 2.4. It fails in both regressions.

6. The division hypothesis: Can an A- and a B-group be identified?

The literature survey above, and some of our findings, suggest a *division hypothesis*. Aid recipients may fall in two groups: The A-group where aid increases growth and the B-group where it harms growth. When the aid effectiveness relation is estimated on the data for both groups together, it becomes zero; but if we could delete the countries in the B-group, aid effectiveness would show up. This is what we try to do in the present section.

6.1. A line that should rise, as the countries are concentrated in the A-group

Below, we propose three criteria for sorting the countries into two such groups, and we use each criterion to make an experiment aimed at gradually deleting the potential B-group countries. Each experiment is represented by a line in Figure 3. To compare the three lines, we need a consistent sample, so this section looks at the 72 LDCs where the data set is complete.

We start each experiment by sorting the countries by the grouping criterion, and estimate the aid equation on all data. The starting estimate is therefore the same in all three division experiments. It is the regression:

$$g_{it} = \alpha \underset{(7.9)}{-0.083}\, y_{it} \underset{(0.4)}{-0.023}\, h_{it} + u_{it}, \text{ where } R^2 = 0.61,\ N = 198,\ NC = 72. \tag{3c}$$

The estimate of the coefficient of *h* is -0.023, which is the point »·« at 0 on Figure 3 (for no countries excluded). As in Table 5 the starting point is close to zero.

The idea is to delete one country at a time, from the expected »*bad*« end, which should be the one of the B-group, and then to re-estimate the equation to see whether the coefficient of *h* rises. This gives observation 1, 2,…, 40, as drawn. If the grouping criterion is correct, the sample should concentrate more and more on the A-group. This should make the curve rise. By using three division criteria, we should be able to tell which line rises the most, and this should indicate the best division criterion.

6.2. Three division criteria: by growth, by aid share and by income level

We have encountered two theories about the nature of the groups in the literature, and in addition we have noted that development theory contains a number of low level equilibrium trap models, see Azariadis and Stachurski (2005). We thus operate with three basic divisions:

By growth rate: We here delete the country with the lowest growth first, then the one with the second lowest growth, etc., and consequently concentrate more and more on the most successful countries. By the Good Policy Model, this should concentrate the

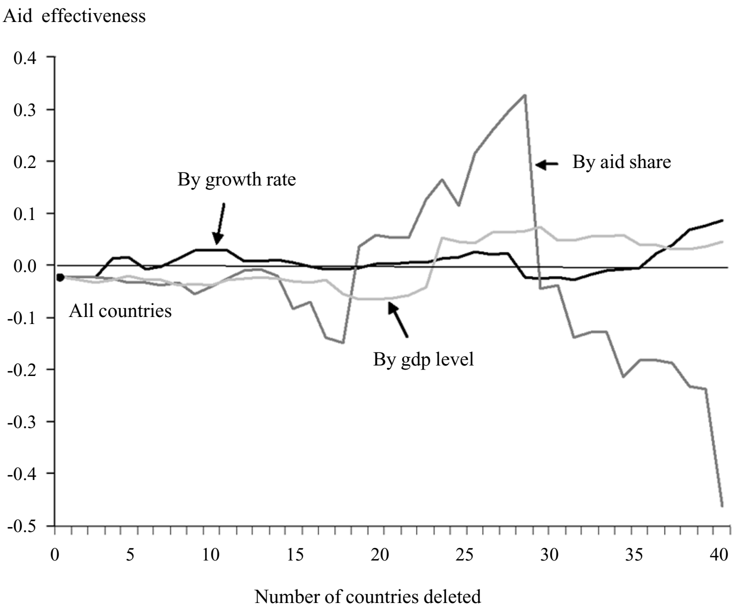

Figure 3. The three experiments: Looking for a rising curve.

countries more and more in the A-group, making the estimated aid effect rise. It is obvious that this experiment fails. There is no systematic rise in the relevant curve.

By aid share: Here we delete the country with the highest aid share first, and then the one with the second highest aid share, etc. By the Medicine Model, this should make the estimated aid effect rise.[25] When we look at the relevant curve it does move a great deal, and until 28 countries are deleted, it rises as it should, but then it turns around and falls. Thus, the experiment does not support the model.

By income level: Here the idea is that the poorest countries are in a low level trap, and if aid is not very large, it has no effect. However, once a country is out of the trap, aid works, so here we first delete the poorest country, then the second poorest, etc. Since the trap works until a certain threshold is reached, we expect a step up in aid effectiveness at a certain level. And this is very much what we see after the deletion of 23 countries. However, the step is not very high, so it is of dubious significance.

When everything is put together, we have to conclude that the evidence for the division hypothesis remains weak.

7. How »bad« are our bad results?

The above analysis tried to answer the following question: Does development aid

25. As the model has the terms $g = \mu h + \omega h^2$, the marginal effect of decreasing the aid share is $g' = \mu + 2\omega h$, which is linear. So by the model, the aid effectiveness should rise linearly.

help poor countries converge to our income level? The question is analyzed by standard cross-country and panel regression techniques. The answer is: The present aid flows have no clear effect on convergence.

The results of our study confirm the results of the recent meta-studies cited above so readers who are familiar with the literature should not be surprised. However we know that our results are controversial, see e.g., the more optimistic conclusions in the set of papers introduced by Hudson (2004) and the survey by McGillivray et al. (2006).

Our results are in line with Rajan and Subramanian (2005) and Easterly (2006). Many authors prefer more structured models, where the equation is controlled for half a dozen possible biases. However, it also appears that only few of the leading researchers are prepared to say that there exists clear macro evidence demonstrating that the present aid flows are an effective tool to close the gap between poor and rich countries.

This raises two questions: How can it be explained? Can the aid be reformed so as to generate catch-up? The next sections address these questions.

7.1. The micro-macro paradox and the negative growth externality of aid

If we accept the argument that the aid flows have little impact on the growth rate, this brings us to the micro-macro paradox. That is, the micro literature evaluating projects shows that about half of all aid projects succeed, the rest fail, but very few harm.[26] Thus, this literature suggests that the average project leads to some development. We can even assess how much.

Aid projects are often decided based on feasibility studies using social cost-benefit analysis which assess the contribution of the project to economic growth. The rule of thumb is that the cost-benefit ratio should be above 10% (0.1). When half the projects work, it corresponds to a realized social rate of return of at last 0.05. The average aid share is about 7.5%. Thus, aid should give 7.5·0.05 percentage points = 0.375 percentage points extra growth.[27] The average growth rate of the LDCs is about 1.5%. Consequently, no less than 100·0.375/1.5% = 25% of the observed growth should be attributable to aid. This is substantial, and it should be easy to find by the methods used, but it does not show up. This is known as the *micro-macro paradox*.

It implies that even successful projects must have a negative externality on the real growth rate. Hence, there must be an invisible villain in the aid game. Three actors appear to be able to play that role.

(*i*) The Dutch Disease effect of aid.[28] In a quasistatic analysis, it is obvious that any transfer from abroad must appreciate the real exchange rate. The dynamic mechanism

26. See e.g., Cassen (1994), the annual IBRD-OED reports and Paldam (1997a).

27. These calculations can be endlessly refined, but the refinements we have considered go both ways.

28. The *disease* is the effect from a *booming aid sector* on the rest of the economy. The problem used to be termed the *transfer problem*. See Gylfason, Herbertsson and Zoega (1999).

Table 7. The status quo bias of aid.

1	A country is in disequilibrium, needing an adjustment
2	The adjustment has short-run political costs
3	The country receives an external donation, and does not need to adjust
4	The disequilibrium grows, and so does the adjustment costs
5	Go to 1

The country is hooked once the disequilibrium is so large that the cost of adjustment exceeds the limit the government can bear and stay in office.

whereby this is brought about depends upon the exchange rate regime, domestic policies, etc. The appreciation will inevitably harm the domestic tradables sector. If the projects generate little growth, and the tradables sector is the one that should generate the growth, a lopsided development might result. This mechanism can be dramatically observed in a few small countries receiving very much (resource rent or) aid, see Paldam (1997b) on Greenland and Hall and Herbertsson (2003) on Uganda.

(*ii*) The hidden cost syndrome. Studies of development often conclude that many LDCs suffer from a lack of executive capacity; i.e., both public and private sectors have too few competent personnel. Also, it is well known that aid projects use relatively much of this scarce resource. Hence, such projects may deprive other LDC activities of competent personnel, and thus make them fail. Consequently, aid projects may thus cause unrelated projects to fail.

(*iii*) The status quo bias of aid. Table 7 describes a likely mechanism. Many detailed studies of policymaking in countries receiving aid support the existence of a mechanism such as that described (see e.g., White, 1998).[29] However, aid is also sometimes used as a device for supporting reform, so perhaps the status quo bias has not been as strong in the recent past as it used to be.

7.3. Can aid become more effective and can the vicious circle be reversed?

In our opinion aid is caught in *a vicious circle*. It is caused by *the growing gulf between* promises and accomplishment in development aid. The gulf causes aid fatigue that comes in waves. When aid decreases, the need for bigger promises to everybody is necessary.

Aid is now rising, and promises have never been bigger. This has increased the number of goals of each development project, making projects less easy to monitor and evaluate, and consequently less efficient. This widens the gulf both ways.

29. The core of the book is four studies of countries that followed dirigist policies which failed and were reversed: Guinea-Bissau, Nicaragua, Tanzania and Zambia. They were for long supported by donors for political/ideological reasons. It is demonstrated that the support delayed adjustment. However, Burnside and Dollar (2000) claim that they could not find a connection between aid flows and economic policy changes.

Development aid is now supposed to abolish world poverty, bring about several types of sustainability, reduce discrimination against women and minorities of all kinds, build social capital, stop corruption and increase good governance, curb terrorism, prevent out-migration, increase the export of donor countries, make peace between warring states and tribes, reward friends, secure employment for aid workers, reconstruct countries after wars and natural disasters, etc. In addition, numerous NGOs are involved with still more diverse goals. There is hardly a problem in the world that aid is not supposed to cure or at least reduce.

All this is to be accomplished for about 0.3% of the aggregate GDP of the rich countries or about 2.5% of the aggregate GDP of the LDCs.

We suggest that aid could be made more effective, simply by reducing the number of goals and by making them (much) more coordinated, concrete and *realistic*. We thus propose aiming for simple, easily monitored, quantitative goals. The simple devise of first doing what can be done – i.e., start by harvesting the low-hanging fruit – may increase the efficiency of aid. If this could be shown to work, surely the willingness to give would increase. Maybe one could even hope that the vicious circle could be turned into a good one.

8. Conclusions

The purpose of this study was to explore convergence and aid effectiveness together, and to do so by fully transparent models. Everything is done as simply as possible; all observations are included, etc. The results are clear:

(1) We found absolute divergence, and robust and significant conditional convergence. Both findings are very much as expected from the literature.
(2) We found absolute aid ineffectiveness, and it remained ineffective in the conditional models. This is also as expected from the literature, though it has remained controversial despite 40 years of research.
(3) Finally, as regards the interaction between the two models: Aid hardly has any effect on catch-up, but the level of income does reduce the effect of the aid effectiveness term.

The results (2) for aid effectiveness are controversial; not for reasons of economic research – they do confirm the results of almost 40 years of inconclusive research, but they have remained controversial simply because all of us want them to be different.

Perhaps we should simply note that the results are in accordance with the teaching of economics: Trade is better than aid.

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