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Aid and Income: Another Time-series Perspective

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Summary. — This study provides a replication of the empirical results reported by Nowak-Lehmann, Dreher, Herzer, Klasen, and Martínez-Zarzoso (2012) (henceforth NDHKM). We uncover that NDHKM relied on a regression model which included a log transformation of variables that are not strictly positive. This led to nonrandom omission of a large proportion of observations. Furthermore, we show that NDHKM's use of co-integrated regressions is not a suitable empirical strategy for estimating the causal effect of aid on income. Evidence from a Panel VAR model estimated on the dataset of NDHKM, suggests a positive and statistically significant long-run effect of aid on income.

Key words — foreign aid, growth, time-series, VAR models

1. INTRODUCTION

Researchers interested in foreign aid have, for several decades, done their best to empirically estimate the impact of aid on economic growth. This has not been easy, and both methodologies and results have varied over time. The aid effectiveness literature has passed through at least four different generations with each generation having its own distinguishing analytical features (see Arndt, Jones, & Tarp, 2010; Hansen & Tarp, 2000). A positive aid-growth association has been reported as characteristic across the first three generations of aid-growth empirical work surveyed by Hansen and Tarp (2000); but the fourth generation work discussed in Arndt *et al.* (2010) has suggested that aid may be impotent in spurring growth. [1] The balance of evidence in the last 3–4 years, however, does appear to be shifting again toward noting a positive and significant impact of aid on growth at the macro level. [2]

In terms of methodological focus, the early empirical literature on aid and growth for the most part used simple cross-sectional analysis with limited attention to addressing the problem of endogeneity of aid in the growth regression. [3] However, in the 1990s, with better data available, attention shifted to panel data techniques. This made it possible to account for unobserved country-specific factors and exploit variations both across countries and over time. Subsequently, advances in instrumental variable and more advanced panel data techniques like dynamic panel Generalized Methods of Moments (GMM) shifted the methodological emphasis to yet another level, and the endogeneity problem in aid-growth empirical analysis attracted further attention.

Until very recently, the use of time-series techniques like co-integration analysis and vector autoregressive (VAR) models was quite limited in aid-growth empirical research. Yet, studies are now starting to emerge. One recent contribution is Juselius, Framroze-Møller, and Tarp (2013), who carry out a comprehensive study of the long-run effect of aid on a set of key macroeconomic variables including economic growth for a group of 36 sub-Saharan African (SSA) countries. Their findings provide clear support for a positive long-run impact of aid on the macroeconomy of recipient countries. Another recent time-series contribution is the paper by Nowak-Lehmann, Dreher, Herzer, Klasen, and Martínez-Zarzoso (2012), henceforth NDHKM, who conclude that aid has an "insignificant or minute significant negative impact on per capita income" of recipient countries.

Overall, as noted in Juselius *et al.* (2013), the divergent evidence on aid effectiveness is perplexing in light of the fact that the data on aid and other macro variables used in most papers come from the same publicly available databases. In explaining this, Juselius *et al.* (2013) argue that the choices researchers make regarding data transformations, econometric models, estimation methods, and assumptions related to endogeneity

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or exogeneity are the main underlying reasons behind the observed discrepancies.

The primary objective of the present study is to illustrate the above points with reference to the aid-growth literature. Particularly, we show how misguided data transformations and inappropriate use of time-series techniques can easily lead to misrepresenting key elements about how aid is allocated and to incorrect conclusions about aid effectiveness. We illustrate these points focusing on the recent contribution by NDHKM. Although we welcome their effort as a step toward increasing the application of time-series techniques in empirical aid effectiveness research, the NDHKM paper suffers from serious limitations as demonstrated in detail below. We present alternative empirical evidence on the effects of aid on income, by applying VAR models instead of the single-equation model considered by NDHKM, while using the same dataset. We argue that this methodology accurately addresses the endogeneity problem at hand in the aid-growth relationship, and is a better time-series approach to estimating the dynamic long-term effects of aid on income.

To achieve the objectives of this study, we begin by replicating the regression results reported by NDHKM. For this exercise, we make use of the replication files provided by NDHKM in the data archive of the *Canadian Journal of Economics*. The regressions are for the most part based on a panel of 50 countries, which is claimed to be "virtually balanced" with only 3% of the observations missing (NDHKM, p. 298). Our replication reveals that this is not the case. In most of the regressions only 30–40% of the available observations are actually used for estimation. The main reason for this omission is that NDHKM estimate a regression model that includes logarithmic transformations of variables that are not strictly positive.

Although the unbalancedness of the panel affects the asymptotic and finite-sample properties of the employed estimators,[4] this is not our main point. We acknowledge that imperfect datasets are part of the reality in which empirical economists live. Macroeconomic panels are often unbalanced due to the fact that the starting period from which economic variables are available typically varies across countries. Researchers thus face a choice between optimizing the amount of observations, which then constitute an unbalanced panel, or to balance the panel, by cutting early observations from countries with long time-series data.

The problem we address here goes much deeper and has serious implications for the results and conclusions reached. To begin, the observations in NDHKM are not simply *missing*; they are actually *omitted* by the authors. NDHKM compile an impressive dataset including relatively long time-series on aid, income, and other macroeconomic variables for a large group of countries. However, by trying to take logarithms of variables with negative values, a substantial fraction of this dataset is simply disregarded. While typically an unbalanced panel consists of time-series of different length, in this case the logarithmic transformation creates huge gaps within the time-series. This makes analyzing the dynamic properties of the data very difficult, if not impossible. The regression model, which is a log-linearization of a multiplicative Solow-type growth model, cannot be correctly specified since not all the variables in the model are strictly positive.

Apart from these issues with data and model specification, the estimation results in NDHKM should not be interpreted as a causal effect of aid on income. Although the applied methodology enables the analyst to consistently estimate the co-integrating coefficient, even when the regressor (aid) is endogenous, interpreting this estimate as a causal relationship between aid and income requires strict exogeneity of aid.[5] In view of this, the negative and significant coefficient reported by NDHKM cannot have causal interpretation regarding the impact of aid on growth. Besides, although tempting, interpreting the statistically insignificant co-integrating coefficient as lack of a causal relationship between aid and growth is inappropriate. The insignificant coefficient can at best suggest absence of evidence in the current sample, rather than evidence of absence (see Temple, 2010, chap. 67). In spite of this, NDHKM interpret their statistically insignificant estimate of the co-integrating coefficient as evidence of lack of a causal relationship between aid and income. A serious attempt to isolate potential causal (negative or positive) effects of aid on income is missing. Thus, without a clear identification strategy, finding a negative and significant/insignificant parameter for aid does not necessarily reveal anything about the impact of aid on growth.

Arguably, a system approach such as the VAR model applied in this study provides illuminating insights when estimating the intertemporal effects of aid on income, as will be discussed further in Section 3. Since the seminal work by Sims (1980), VAR models have become the benchmark in empirical macroeconomics. In contrast, in the aid literature VAR models have not yet gained the same popularity, although there have been some recent applications of VAR models, such as Osei, Morrissey, and Lloyd (2005), Hansen and Headey (2010), Gillanders (2011), Juselius *et al.* (2013) and Kang, Prati, and Rebucci (2012). In the present study we apply a Panel VAR model to the dataset of NDHKM to investigate the effect of aid on income. By allowing explicitly for an effect of aid on income as well as an effect of income on aid, we find that the former effect is both positive and significant.

The study is structured as follows. In Section 2, after presenting the replication results, we discuss the data-handling concerns uncovered by the replication exercise. In Section 3, we review the problems with the empirical strategy of NDHKM, and introduce our own strategy. Section 4 presents the results from estimating VAR models on the NDHKM dataset. Section 5 concludes that when a Panel VAR model is applied to the same dataset as in NDHKM, a positive and statistically significant long-run effect of aid on growth emerges.

2. REPLICATION RESULTS

We begin the replication exercise by noting that we are able to exactly replicate virtually all the empirical results reported

Table 1. *Impact of Aid on Income*

Dependent variable	*LY*	*LY*	*LY*	*LY*
LPOPGPLUS	–	–	–	0.00
LSDOMY	–	0.08	0.07	0.07
LSEXTNY	–	–	0.04	0.05
LSNATY	−0.02	−0.01	−0.01	−0.02
ρ	0.97	0.97	0.98	0.99
N	57	56	50	50
T	41	41	41	41
K	2120	1693	794	755
$K/(N*T)$	0.91	0.74	0.39	0.37

Notes: Estimates of Eqn. (1). *t*-Values are identical to NDHKM and therefore not reported. *N* refers to the cross-sectional dimension (amount of countries), *T* to periods, and *K* to amount of observations used for estimation. Variable descriptions are as follows: *LY* (log of real per capita income growth), *LSDOMY* (log of domestic savings to Gross Domestic Product (GDP) ratio), *LSEXTNY* (log of net external savings to GDP ratio), and *LSNATY* (log of net aid transfer to GDP ratio).
Source: See text.

Table 2. *Differing Impact Depending on Aid-to-GDP Ratio*

Aid-to-GDP ratio Dependent variable	Above average *LY*	Below average *LY*
LPOPGPLUS	0.04	0.37
LSDOMY	0.05	0.16
LSEXTNY	0.04	0.06
LSNATY	−0.03	−0.01
ρ	0.98	0.99
N	23	27
T	41	41
K	343	412
$K/(N*T)$	0.36	0.37

Notes: See Table 1.

Table 3. *Differing Impact Depending on HDI*

HDI Dependent variable	<0.5 *LY*	0.5–0.799 *LY*	>0.8 *LY*
LPOPGPLUS	−0.53	0.43	0.68
LSDOMY	0.06	0.09	1.91
LSEXTNY	0.02	0.05	−1.01
LSNATY	−0.03	−0.01	−0.17
ρ	0.97	1.00	0.35
N	20	25	4
T	41	41	41
K	303	413	30
$K/(N*T)$	0.37	0.40	0.18

Notes: See Table 1.

Table 4. *Differing Impact Depending on Income Level*

Income level Dependent variable	LDC *LY*	GNI < 735 *LY*	736 < GNI < 9075 *LY*
LPOPGPLUS	−0.23	−0.30	0.23
LSDOMY	0.05	0.06	0.18
LSEXTNY	0.08	0.05	0.06
LSNATY	−0.01	−0.02	−0.01
ρ	0.97	0.98	0.99
N	18	24	24
T	41	41	41
K	295	397	321
$K/(N*T)$	0.40	0.40	0.33

Notes: See Table 1.

Table 5. *Differing Impact Depending on Region*

Region Dependent variable	Caribbean *LY*	Latin America *LY*	Africa *LY*	Asia *LY*
LPOPGPLUS	2.87	0.58	−0.10	−0.51
LSDOMY	0.16	0.12	0.06	0.02
LSEXTNY	0.06	0.06	0.04	0.02
LSNATY	−0.04	−0.02	−0.01	−0.03
ρ	0.98	0.95	0.96	1.01
N	5	11	25	6
T	41	41	41	41
K	69	117	356	136
$K/(N*T)$	0.34	0.26	0.35	0.55

Notes: See Table 1.

by NDHKM. Tables 1–7 show the replications of the corresponding Tables 1–7 in NDHKM. Except for the sixth column of Table 6,[6] these tables match the results reported by NDHKM. After outlining the empirical model, we discuss our concerns raised by this replication exercise. NDHKM estimate the following model, relating income per capita (*LY*) to population growth (*LPOPGPLUS*), domestic savings (*LSDOMY*), net external savings (*LSEXTNY*), and net aid transfers (*LSNATY*), with all variables measured in logs:

$$LY_{i,t} = b_{0,i} + b_1 LSDOMY_{i,t} + b_2 LSEXTY_{i,t} + b_3 LSNATY_{i,t} + b_4 LPOPGPLUS_{i,t} + u_{i,t} \quad (1)$$

Domestic savings, external savings, and net aid transfers are expressed as (log) ratios to GDP. Tables 1–5 provide estimates of this model, using different subsamples of the smaller "balanced" panel of 50 countries, while Table 6 presents estimates based on a larger panel of 131 countries. Table 7 presents the estimated effects of aid on investment, domestic savings, and the real exchange rate. For the estimates reported in Tables 1–5 and 7, NDHKM apply the Dynamic Generalized Least Squares (DGLS) estimator, by Stock and Watson (1993).[7] This method involves adding l lagging and leading differences of all regressors to Eqn. (1). Throughout their paper, NDKMH set $l = 2$, without further elaboration on this choice.

Since all variables are subjected to a logarithmic transformation, it is necessary that the original series (in levels) are strictly positive. As it turns out, this is not the case and results in the omission of a large fraction of the available data. To illustrate this problem, we focus on the fourth column of Table 1 where the estimates of Eqn. (1) are reported, using all the covariates, for a panel of 50 countries over the period 1960–2006. After adjusting the endpoints to the dynamic specification of the model, there are 41 observations available per country. A balanced panel should therefore include $41 \times 50 = 2{,}050$ observations. Our replication shows that the full model in the fourth column of Table 1 in NDKMH is based on only 755 observations, implying a loss of 63% of the available observations.

Consider, for example, the top-left plot in Figure 1, which depicts domestic savings, net external savings, and net aid transfers, in levels, for Algeria. For all the three variables, full time-series data over the period 1960–2006 are available. However, since net external savings are negative during several periods, these observations are lost after the logarithmic transformation (bottom-left plot).

Because Eqn. (1) is supplemented with two leading and lagging differences of all regressors, at least six subsequent observations are required within a country, to include one observation for estimating the model. Making matters even more challenging, the DGLS estimator requires one additional observation for estimating ρ, the autocorrelation parameter of the residual term u. Therefore, in order to include observation t for country c in the estimation, all variables need to be observed for seven periods, from period $t-4$ to $t+2$. As Figure 1 shows, this happens only once for Algeria, during 1984–90. As a result, $t = 1988$ is the only observation from Algeria used for estimating Eqn. (1). The right-hand side plots in Figure 1 tell a similar story for Swaziland. In levels, there are two short gaps in the observed data. After the logarithmic transformation, there is only one interval left during which all variables are observed for at least seven subsequent periods: 1986–92. The only observation from Swaziland used for estimating Eqn. (1) is $t = 1990$.

In addition to resulting in the omission of observations, the logarithmic transformation of domestic savings and net external savings is questionable for other reasons. In levels, Figure 1 shows a very clear co-movement between these variables. After taking logs, this information is lost. Given that the observations are not randomly omitted, but are systematically dropped for country-year pairs with nonpositive savings values, the coefficient estimate of aid may therefore be potentially

Table 6. *Impact of Aid on Income (Sample of 131 Countries)*

Method	FE	FE	FE + GLS	GMM	GMM	SUR
Data	Annual	5y-averages	5y-averages	5y-averages	5y-averages	5y-averages
Dependent variable:	*LY*	*LY*	*LY*	*LY*	*LY*	*LY*
LPOPGPLUS	−0.12	−0.08	0.17	0.37	0.28	–
LSDOMY	0.09	0.10	0.02	0.04	0.01	–
LSEXTNY	–	0.01	0.01	0.01	0.01	–
LSNATY	−0.06	−0.05	−0.02	−0.02	−0.02	–
N	131	131	131	131	131	–
T	41	8	8	8	8	–
K	1728	346	198	198	115	–
$K/(N*T)$	0.32	0.33	0.19	0.19	0.11	–

Notes: See Table 1. FE = Fixed effects, GLS = Generalized Least Squares, GMM = Generalized Methods of Moments and SUR = Seemingly Unrelated Regressions.

Table 7. *Indirect Impact of Aid*

Dependent variable	Investment	Domestic savings	Real exchange rate
LSDOMY	0.42	–	–
LSEXTNY	0.29	−0.12	−0.14
LSNATY	0.04	–	−0.51
ρ	0.54	0.58	0.72
N	50	56	20
T	41	41	41
K	795	1915	327
$K/(N*T)$	0.39	0.83	0.40

Notes: See Table 1.

underestimated. For a given level of aid, country-year pairs with nonpositive saving values are cases where the returns to aid may be higher.

Although Algeria and Swaziland are the worst cases, the problem is widespread. Figure 2 shows the distribution of observations per country included in the estimation of Eqn. (1). The full potential of 41 observations is realized in only one country (Egypt). In 37 out of the 50 countries, less than half the observations are actually used. All our tables show the number of observations actually used for estimation relative to the potential number of observations.

A similar critique also applies to the estimates based on the larger panel of 131 countries, reported in Table 6. We have been able to replicate all the results in this table except the sixth column. The issue of missing observations as the result of a failed logarithmic transformation applies here as well, as is evident from Figure 3. This figure shows the distribution of observations per country included in the estimation reported in the third column of Table 6. This estimation is based, on average, on only 1.5 observations per country. This is clearly insufficient to offer a time-series perspective.

The regression model (1) is derived in NDHKM (p. 293) by log-linearizing a Cobb-Douglas production function in which income is the product of the inputs of domestic savings, net external savings, and net aid transfers. Given that income is strictly positive, the inputs are required to be strictly positive as well, for this multiplicative relation to hold.[8] Our finding of negative inputs therefore clearly reveals, in addition to the empirical problem of missing data, mis-specification of the theoretical model.

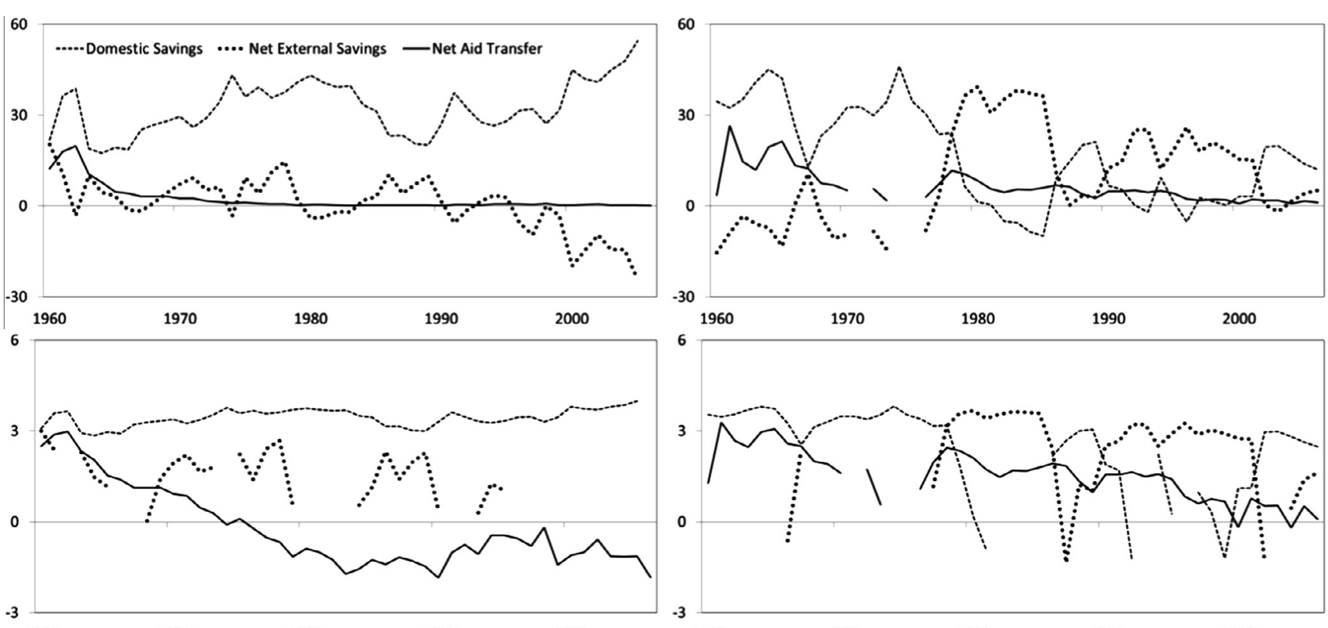

Figure 1. *Domestic savings, net external savings, and net aid transfer for Algeria (left) and Swaziland (right), in levels (top) and logs (bottom), for the period 1960–2006. Source: Authors' illustration.*

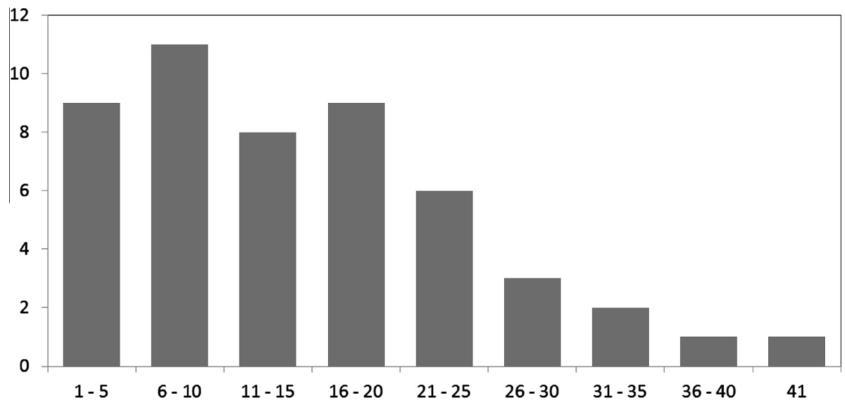

Figure 2. *Distribution of included observations for the estimation of Eqn. (1): Table 1, 4th column. Histogram depicts the number of countries (y-axis) with the number of included observations (x-axis). Source: Authors' illustration.*

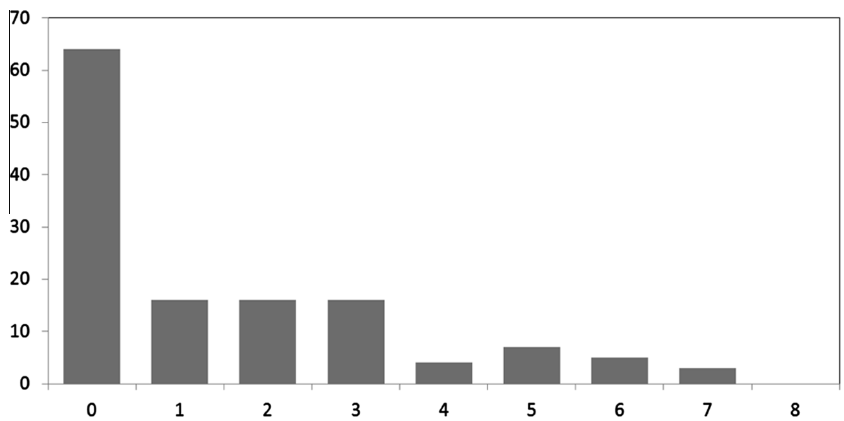

Figure 3. *Distribution of included observations for the estimation of Eqn. (1): Table 6, 3rd column. Histogram depicts the number of countries (y-axis) with the number of included observations (x-axis). Source: Authors' illustration.*

Overall, in our assessment, the reported results in NDHKM do not provide any evidence in favor or against the effectiveness of aid. Apart from the panels being highly unbalanced, the remaining observations are often not clustered, but scattered over the full potential sample. This makes time-series analysis nearly impossible.

3. EMPIRICAL STRATEGY

Regardless of the issues related to data, estimating Eqn. (1) by DGLS is not a suitable empirical strategy when the aim is to estimate the causal effect of aid on income. The estimation results cannot be given a causal interpretation in the presence of an endogenous regressor (aid). NDHKM acknowledge that aid is likely to be endogenous with respect to GDP. Identification of causal effects in the presence of endogenous regressors is usually achieved through a proper application of instrumental variables. Finding valid instruments, which are uncorrelated with the dependent variable but sufficiently correlated with the endogenous regressors to ensure strong identification, is often a daunting task (see Clemens & Bazzi, 2009). Clemens *et al.* (2012) also discuss the challenge of finding a reliable instrument for aid as a major problem in aid-growth empirical research. While lags of the endogenous regressors are often legitimate instruments, their use in this application would be invalid because the residuals from the co-integrating relationship are strongly autocorrelated.

Partly because of the difficulties associated with finding valid instruments, NDHKM choose a completely different empirical strategy. They estimate a co-integrating relation between aid and income using the DGLS estimator. The authors cite the result of Stock and Watson (1993), who note that the DGLS estimator is unbiased even when the regressors are endogenous. Given that the variables are co-integrated, the DGLS estimator does indeed give unbiased estimates of the co-integrating vector. However, it is a misunderstanding that the parameters of a co-integrating vector can be interpreted as a causal effect. As indicated in Stock and Watson (2011, p. 697), strict exogeneity of the regressors is required for such a causal interpretation. Even in the case of a bivariate model for aid and income, where there can be at most one stationary long-run relation, this co-integrating vector by itself does not reveal any direction of cause and effect. Hence no conclusions on the "impact of aid on income" can be drawn based on the DGLS estimates reported by NDHKM.

As an arguably meaningful alternative, we present in the next section a Vector Autoregressive (VAR) model, which treats all variables as endogenous, for aid and income, estimated on the same dataset as NDHKM. The VAR framework is well suited to address the issue of a bi-directional relationship between aid and growth. It allows for joint modeling of the dynamics of income and aid, by explicitly formulating separate equations for both endogenous variables.

Moreover, rather than identifying an instantaneous (static) causal effect, VAR models are able to show the dynamic intertemporal impact of a shock to one variable on the future path of another variable. Since aid is not necessarily supposed to improve income per capita immediately, but rather to improve conditions for growth in the longer run, a dynamic model such as a VAR provides one way of assessing the long-term impact of aid on income. By computing impulse response functions based on the estimated VAR, we analyze the dynamics of income over a period of 10 years following a shock to aid. With both aid and income treated as endogenous a priori, the VAR allows us in addition to explicitly consider a shock to income and its effects on aid.

In order to deal with the nonstationarity and co-integration of the variables, we estimate the VAR as a Vector-Error-Correction Model (VECM). Even if this bivariate representation allows for only one co-integrating vector, our impulse responses show that there are two separate dynamic effects, of opposite sign, at work between aid and income. This finding highlights that the co-integrating relation itself should not be confused for an economic causal relation.

VAR models have become the benchmark tool in macroeconometrics, for example for estimating the effects of monetary and fiscal policies.[9] For such applications a multi-equation model is attractive. Fiscal and monetary policies affect the performance of the economy but the state of the economy is likely as well to have an impact on the policies.[10] The same argument applies to the relationship between aid and income. Surely, when donor countries make decisions regarding development aid, they take into consideration the economic conditions in the recipient countries. This is, for example, built into the aid allocation formula of the International Development Association (IDA). When estimating the effect of aid on income, it is therefore essential to disentangle it from the (allocation) effect of income on aid.

In a recent study, Juselius *et al.* (2013) estimate separate co-integrated VAR models for 36 African countries, which are supplemented with country-specific dummies to indicate periods of economic and political turmoil. In our approach the observations of all countries are instead pooled to estimate a Panel VAR (PVAR) with fixed parameters. Our PVAR is therefore a dynamic multiple-equation extension of the fixed-effects model considered by NDHKM. The advantage of pooling the data is that it dramatically increases the size of the dataset. Rather than estimating the country-specific VAR using T observations, we estimate a PVAR with $T \times N$ observations. In our case, $T = 37$ and $N = 59$. By assuming fixed parameters, there are many more observations available to estimate the parameters but this, of course, comes at a cost. Assuming constant parameters across countries can be highly restrictive, while country-specific dummies to account for extreme events are not included. We acknowledge these restrictions and emphasize that we estimate the average effects of aid for the reasons outlined in the introduction. In specific countries these effects may well differ from the ones presented in Section 4.

On this background, we aim to keep our model parsimonious. In order to provide an answer to the question raised by NDHKM ("Does foreign aid raise per capita income?"), we fit a bivariate VAR model to aid and GDP—both expressed in log per capita terms. This provides a telling alternative to the NDHKM single-equation regression model with aid as the only regressor (Table 1, 1st column), for which NDHKM report a negative correlation between aid and income. The logarithmic transformation of the variables follows the convention in the aid literature. Juselius *et al.* (2013) report evidence in favor of a multiplicative rather than additive relationship between aid and income, which makes the logarithmic transformation required. Nevertheless, we avoid the problems with taking logs of negative numbers (discussed in Section 2), by excluding domestic and external savings from the model and by considering countries that are net aid receivers only. As a robustness check, however, we also estimate a VAR supplemented with domestic and external savings, while keeping both these variables in levels rather than logs and expressing them in per capita terms instead of as a share of GDP.

Unlike NDHKM, we do not use the aid-to-GDP ratio. Although considering aid as a ratio of GDP is not uncommon in the literature on aid effectiveness, it implies a certain restriction on the long-term relation between GDP, aid and population, which Juselius *et al.* (2013) test and reject for all the 36 African countries in their dataset. Moreover, transforming aid into a ratio of GDP makes it harder to identify the effect of aid on GDP. For example, consider a negative exogenous shock to GDP, which by construction reduces GDP per capita and raises the aid-to-GDP ratio. In the model by NDHKM, this negative co-movement between the regressor and explanatory variable would be interpreted as a negative effect of aid on GDP, even if the original shock to GDP could be entirely unrelated to aid. Our VAR model therefore includes aid and GDP both expressed in logs per capita, while we also examine the robustness of our results by considering the aggregate levels of both aid and GDP with and without taking logs of these variables.

After estimating the model, we compute impulse response functions. The impulse response functions show the dynamic interaction between the variables of interest. In particular, from the impulse responses one can see the response of one variable (e.g., income) following a shock to another variable (aid) and the duration it takes for the observed effect of aid to peter out. In order to identify the shocks, we impose a recursive structure, which makes the order of the variables relevant. Since the seminal paper by Sims (1980), in this literature it is generally considered sufficient (and even preferable) to provide a loose/intuitive justification for the ordering of variables, rather than to formulate an exact structural economic theory. For example, Caldara and Kamps (2008) argue that there is in general a considerable delay between political decisions on government spending and the actual spending. Macroeconomic conditions therefore have an impact on government spending only after a lag, while the reverse effect may occur immediately. The same argument can be applied to spending on aid.[11] In our VAR, aid is therefore placed before GDP. We acknowledge that relying on this recursive identification approach has limitations. Nevertheless, our methodology is arguably superior to the approach applied by NDHKM in terms of addressing the endogeneity issue inherent in aid-growth empirical work. Moreover, given that we consider a VAR of only two variables, it is relatively straightforward to check the sensitivity of the results with respect to our assumptions by simply reversing the order of the variables. This we do in the next section as one of our robustness checks. Our results indicate that, in this application, the recursive order matters only for the estimated short-term impact of aid, while the effects of the ordering on the estimated long-term impact (which is our primary interest) are rather small.

A potential shortcoming of VAR models is that, unlike a correctly specified structural model, they do not always reveal the mechanism through which the effects occur even if it provides an empirical description of the dynamic interaction between the variables. To uncover these structural mechanisms, the VAR should include all relevant variables. Recent studies therefore consider a VAR for aid and income supplemented with additional information. For example, Osei *et al.* (2005) investigate the effects of aid on fiscal variables, Hansen and Headey (2010) consider trade balances, while Kang *et al.* (2012) add exchange rates to their VAR. We consider a bivariate VAR including only aid and GDP, because our main purpose is to illustrate how VAR–based results differ from the single-equation framework applied by NDHKM.[12]

4. VAR RESULTS

Our analysis is based on the following Panel VAR:

$$y_{i,t} = \mu_i + \sum_{j=1}^{p} A_j y_{i,t-j} + \varepsilon_{i,t}, \qquad i = 1 \ldots N, \quad t = 1 \ldots T, \tag{2}$$

in which $y_{i,t}$ is a $k \times 1$ vector defining the state of the k endogenous variables in country i during period t, μ_i is a $k \times 1$ country-specific intercept term, A_j are $k \times k$ matrices of coefficients, $\varepsilon_{i,t}$ is a $k \times 1$ residual term and p denotes the number of lags.

We apply this VAR to net aid receipts per capita and real income per capita, both measured in logs; $y_{i,t} = (aid_{i,t}, gdp_{i,t})$, using data from 1970 to 2006 ($T = 37$) for 59 countries ($N = 59$). The Appendix provides more details on the dataset.

Because the two variables are co-integrated, we estimate the VAR in its VECM representation:

$$\Delta y_{i,t} = \mu_i + \alpha\beta' y_{i,t-j} + \sum_{j=1}^{p-1} B_j \Delta y_{i,t-j} + \varepsilon_{i,t},$$
$$i = 1 \ldots N, \ t = 1 \ldots T, \tag{3}$$

in which Δ is a difference operator $\Delta y_{i,t} = y_{i,t} - y_{i,t-1}$, α (the loading matrix) and β (the co-integrating matrix) are $k \times r$ vectors, and B_j are $k \times k$ matrices of coefficients. The co-integrating rank (r), is in our case $r = 1$ (see Table 9), while the number of lags, selected based on Bayesian Information Criteria, is $p = 2$ (see Table 10).

In estimating the Panel VECM (Eqn. (3)), we follow the two-stage estimator proposed by Breitung (2005). It involves estimating separate models for each country in the first stage to obtain estimates of the country-specific intercepts, after which the remaining parameters are estimated in a pooled regression.

Moving to the results, Figure 4 shows the orthogonalized impulse response functions, with the dynamic effects over 10 years on aid (left) and income (right) following a positive shock to either aid (top row) or GDP (bottom row), with 95% bootstrap confidence bounds based on 100,000 replications. The bootstrap implementation is explained in the Appendix.

Figure 4b illustrates the effect of a one unit positive shock to aid per capita on income per capita. The impulse response function clearly demonstrates a positive and significant response. Since both variables are measured in logs, the shock can be interpreted as a 1% increase in net aid receipts per capita. This 1% increase in aid receipts is estimated to raise income per capita by 0.17% over a period of 10 years, compared to the situation in which aid receipts would have remained constant. Although a 0.17% increase in income per capita may seem rather modest, one should take into account that, on average over our sample, aid as a percentage of GDP is no more than 7.6%. Averaged over countries and time, average aid receipts per capita are roughly US\$39, while income per capita is US\$1,085. Hence, an increase of 1% in aid receipts per capita would be on average US\$0.39 per capita in the first year, which would reduce to extra aid receipts per capita of 0.3% $\times$ 39 = US\$0.12 after 10 years (see Figure 4a). The effect on income, after 10 years, would be an increase of 0.17% $\times$ 1,085 = US\$1.84 per capita. In the countries we

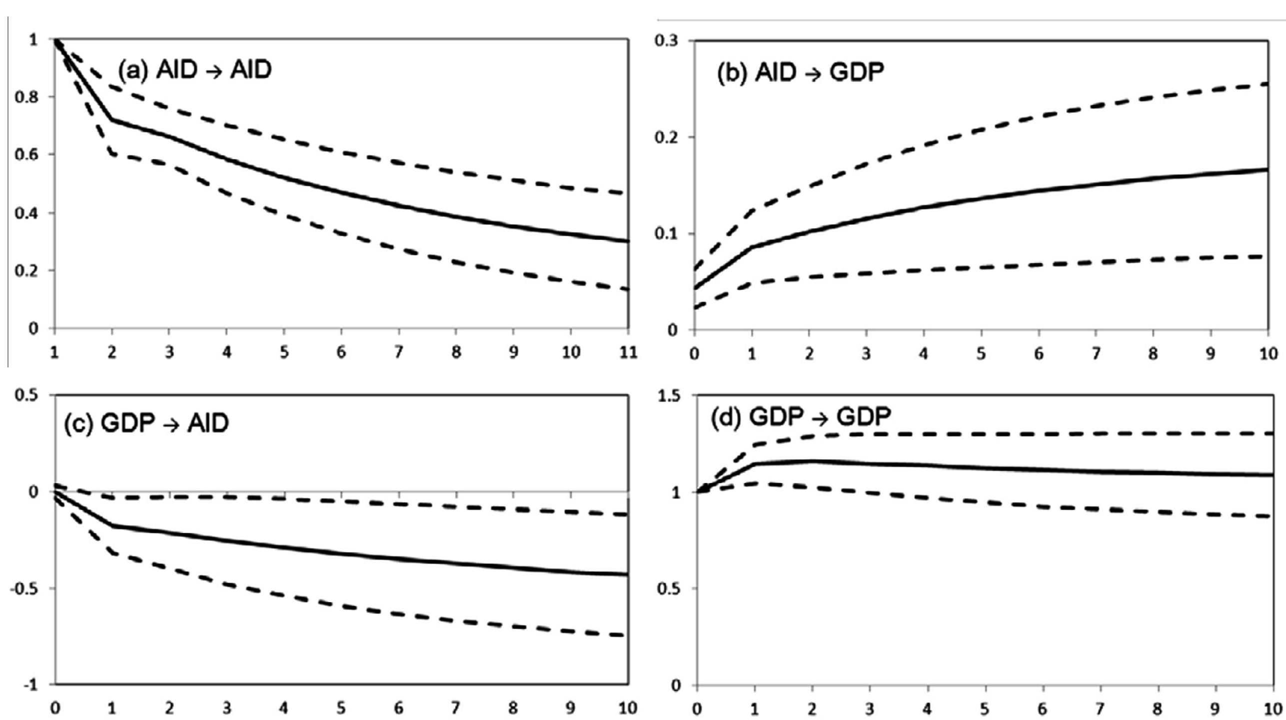

Figure 4. *Impulse response functions based on PVAR (2) with p = 2 for log-Aid per capita and log-GDP per capita. T = 37 (1970–2006) and N = 59. Bootstrap 95% confidence bounds are based on 100,000 replications. Source: Authors' illustration.*

focused on in Section 2, in Algeria and Swaziland, average income per capita in our sample is respectively US$1,835 and US$1,052, while average net aid receipts per capita in these countries are respectively US$8 and US$43. Therefore, in Algeria, an increase in aid receipts per capita of US$0.08 is estimated to lead over 10 years to an increase in income per capita of US$3.12. In Swaziland, the effect on income of an initial US$0.43 increase in aid per capita after 10 years is US$1.79.

The figures reported above are obviously highly simplified "back-of-the-envelope" calculations, which do no justice to the potential nonlinearities and heterogeneities across countries.[13] Nevertheless, the result that a shock to aid is in itself transitory (Figure 4a), while its effect on income seems persistent (Figure 4b), suggests that a temporary increase in aid spending may push income to a permanently higher level, which is certainly a more positive assessment of aid than reported by NDHKM. Our results are in line with other recent VAR-based analyses of aid effectiveness. Gillanders (2011) fits a fixed-effects PVAR to aid per capita and GDP growth and finds that aid has a significant positive, although small, effect on growth. Juselius *et al.* (2013) estimate country-specific co-integrated VARs for aid, income, and other macroeconomic variables and find a positive effect of aid for most of the countries.

Figure 4c illustrates how the use of a single-equation framework may lead to confusion about the impact of aid. A shock to income has an estimated negative and persistent effect on aid, which is of larger magnitude than the positive effect of aid on income. Both the intertemporal effects in Figure 4b and c play a role in the long run. Given the opposite signs of these effects, and the larger size of the negative effect, it should come as no surprise that a negative and/or insignificant long-run relation between aid and income is found using a single-equation framework, even if the impact of aid on income is positive and significant. Moreover, the impulse response functions show a clear difference between short- and long-term impacts, demonstrating why a dynamic model structure, like in a VAR, is crucial for evaluating these effects. A static model, like Eqn. (1) only considers the instantaneous impact, but is unable to capture the long-term effect, i.e., the impact on income, multiple years after receiving aid.

Figure 4b, showing the intertemporal effects of aid on income, is reproduced in Figure 5 based on several alternative VAR specifications, listed in Table 9 of the Appendix. In Figure 5a and b, the starting date is set to 1960 and 1980, respectively. This leads not only to a different length of the time-series, but also to the inclusion or exclusion of certain countries. In Figure 5c, we consider aggregate aid levels instead of aid per capita, while in Figure 5d both aid and income are measured in aggregate levels. In Figure 5e, we reverse the order of the variables, placing GDP before aid. The main result, a positive effect of aid on income in the long run, is robust to all these alternatives. Only in Figure 5a is the long-term impact of aid not significant at the 5% level, although the confidence interval still lies almost entirely in the positive domain. Comparing Figures 4b and 5a and b further suggests that, over time, the evidence has become more decisive toward a positive impact of aid.

The remaining models (f)–(i) are estimated as an unrestricted VAR (Eqn. (2)), rather than a VECM. For estimating

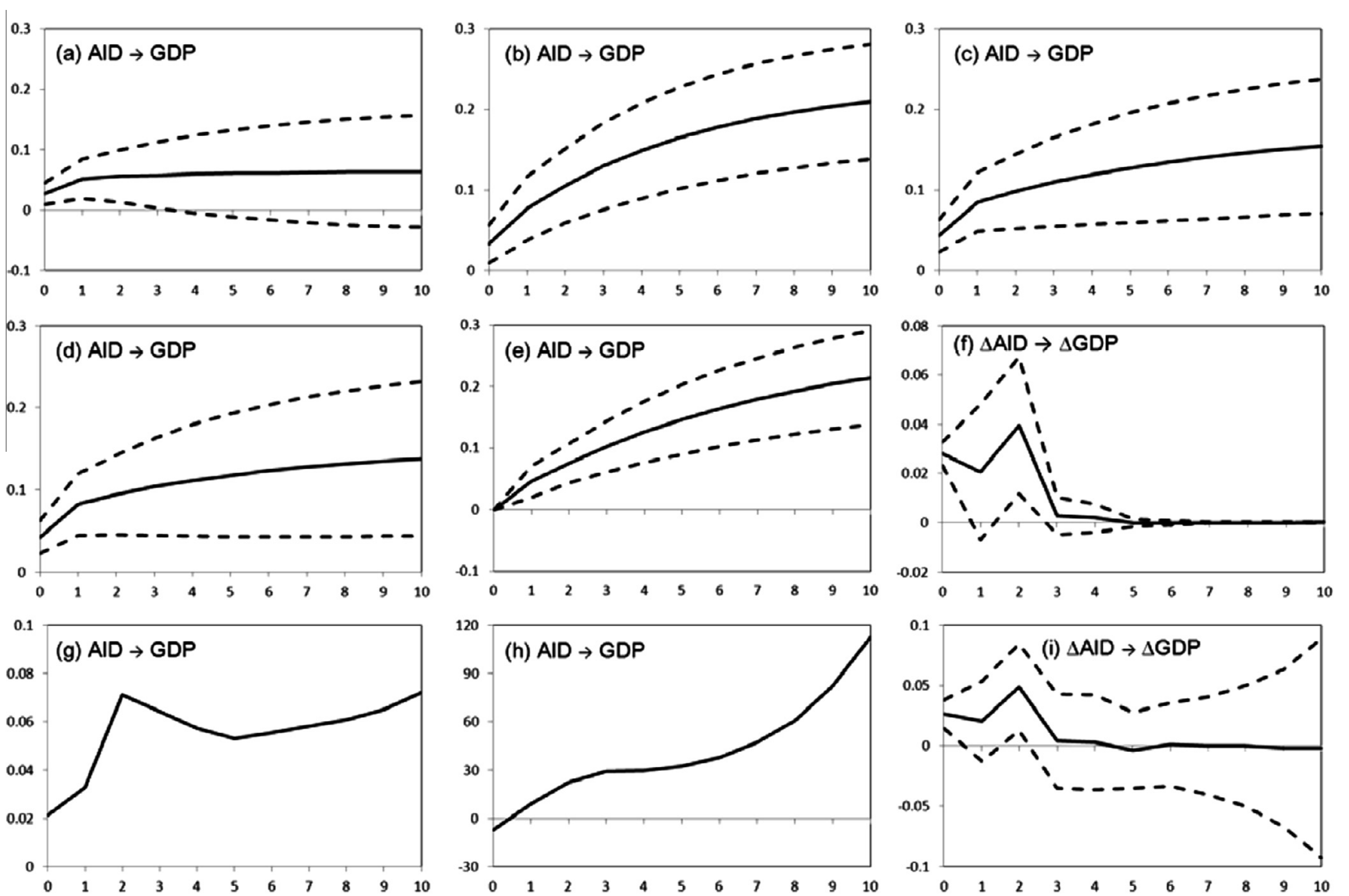

Figure 5. *Robustness checks for Figure 4b. See Table 9 for details on the alternative VAR specifications. Bootstrap 95% confidence bounds based on 100,000 replications. Source: Authors' illustration.*

the unrestricted PVAR we follow common practice by combining first-differencing and GMM estimation, applying lagged values as instruments (Arellano & Bond, 1991). First-differencing Eqn. (2) eliminates the country-specific intercept, thereby avoiding the problem of inconsistency of the fixed-effects estimator for dynamic panel data regressions (Nickell, 1981).

In Figure 5f, we take first-differences of aid and GDP (both in levels per capita). A shock to aid seems to have a positive short-run effect on differenced income, which converges to zero after some periods. The gradual decline of the impact is consistent with the decreasing slope of the impulse response function for the VARs in levels. In the final row of Figure 5, domestic and external savings per capita ($sav_{i,t}$ and $ext_{i,t}$) are added to the VAR. Since we cannot take logs of domestic and external savings (which would lead to the problems raised in Section 2), we consider the model first with aid and GDP measured in logs and domestic and external saving in levels (Figure 5g), and second with all four variables measured in levels (Figure 5h). In both these instances, the estimated model turns out to be unstable, presumably due to the inclusion of variables in levels rather than logs, while the underlying economic relation between aid and income is multiplicative rather than additive. Due to this instability, it is impossible to execute the bootstrap simulation in order to obtain confidence bounds for the impulse response functions. We therefore report these impulse response functions without confidence bounds, and interpret this issue as a strengthening of our argument in Section 2 that the Solow-type model including domestic and external savings (Eqn. (1)) is not correctly specified. Finally, Figure 5i is based on the VAR with all four variables measured in differences. Apart from the widening confidence bounds, the resulting impulse response function looks similar to Figure 5f.

Although providing a definitive answer to the aid-effectiveness question is not the primary objective of this study, we believe that the results from this exercise can, with some caution, be considered as indicative of time-series evidence on aid effectiveness. Overall, the results presented in Figures 4 and 5 consistently suggest a positive long-term impact of aid on income. This is in stark contrast to the results reported by NDHKM, even though the results are based on the same dataset.

5. CONCLUSION

The main purpose of this study was to illustrate how data mishandling, model mis-specification, and inappropriate application of time-series techniques can lead to misguided conclusions and inferences regarding the effectiveness of foreign aid. More specifically, we have demonstrated how a system of equations approach based on VAR models addresses the well-recognized issue of endogeneity in aid-growth analysis. In the process, we have also shown how the single-equation approach applied in NDHKM is not well suited to handle the aid-growth relationship.

In light of the serious problems related to data handling (taking logs of nonpositive numbers) and usage of time-series techniques (interpreting a co-integrating vector as a causal model), we argue that the evidence in NDHKM does not offer a sound time-series perspective on aid and growth. Even when appropriate methodology and data are applied, insignificant results, in the terminology of Temple (2010, chap. 67), only amount to "absence of evidence" and should not be confused for "evidence of absence" of the effect of aid on income.

When the same dataset as in NDHKM is evaluated using a Panel VAR model, which better addresses the fact that causality in the aid-growth relationship runs in both directions with potentially opposing effects, a positive and statistically significant long-run effect of aid on growth emerges. This result is consistent with other emerging time-series evidence, and indeed with results from the aid-growth literature more generally.

NOTES

1. See Rajan and Subramanian (2008) and Moyo (2009).

2. See, for example, Clemens, Radelet, Bhavnani, and Bazzi (2012) and Arndt *et al.* (2010).

3. This problem mainly arises due to the bi-directional relationship between aid and growth: donors give more aid to poor countries and lower their assistance as recipient countries get richer. This bi-directional relation creates a problem of endogeneity, which is a widely accepted challenge in the aid-growth empirical research.

4. For example, Wooldridge (2001, chap. 17) shows that both fixed-effects and random-effects estimators can be inconsistent for unbalanced panels when the sample selection process is not strictly exogenous.

5. The DGLS results from NDHKM show a negative and significant (in the bivariate model) and negative and insignificant (in the full model) impact of aid on per capita income. (see Table 1 in NDHKM, p. 299)

6. Column six of Table 6 is estimated using Seemingly Unrelated Regressions (SUR) which is a multi-equation model. But since there is no information in the replication files provided by NDHKM regarding the equations involved in this estimation, this column cannot be replicated.

7. The authors indicate that in estimating the impact of aid on growth their preferred approach is DGLS (the results reported from Tables 1–5).

8. Here it should be noted that we are not expecting savings to be always positive. Savings can legitmately be negative in the data. Our concern is that NDHKM end up dropping the nonpositive saving values from the data in an effort to make a logarithmic transformation of a variable which is not always positive, leading to a nonrandom omission of observations.

9. See Caldara and Kamps (2008), Chung and Leeper (2007), Mountford and Uhlig (2009), Stock and Watson (2001) and the papers cited therein.

10. For applications to fiscal policy, see Blanchard and Perotti (2002).

11. That is, since donors have to observe the GDP shock in the recipient country before making a political decision and allocate aid, it is reasonable to assume that aid allocation occurs with some lags after the GDP shock takes place. On the other hand, the potential effect of aid on GDP can be expected to start improving the conditions for growth right away. See also Dalgaard, Hansen, and Tarp (2004).

12. We are limiting attention to a bivariate VAR, apart from one of our robustness checks, in which we supplement the VAR with domestic and external savings. Even if we are aware that the bivariate approach

potentially leads to omitted-variable biases, we abstain from using variables not used by NDHKM in our main analysis to ensure comparability of the results of the two studies.

13. Without the log transformation of the variables, the impulse response would display the actual effects in real amounts rather than percentages, allowing for a more intuitive interpretation. After log-linearizing, a percentage change in aid is assumed to have a fixed-percentage effect on income, without taking the real level of aid and income into account. This is a consequence of the choice to model aid and income as a multiplicative system, following the convention in the literature (e.g., Juselius *et al.* 2013).

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APPENDIX A

A.1 *Data and model specification*

We apply the exact same dataset as NDHKM. It is included in the replication files provided in the data archive of the *Canadian Journal of Economics*. From this dataset, we obtain the variables "real income" (*gdp*), "net aid receipts" (*aid*), "domestic savings" (*sav*) and "external savings" (*ext*). The dataset also includes time-series on the population of each country, which we use to transform all variables to per capita terms. Section 3 in NDHKM provides details on the original sources of the data. The dataset features observations on 131 countries, for a maximum of 47 periods (1960–2006). We choose to work with a balanced dataset only, because the estimator for the co-integrated PVAR is derived in a balanced panel context (Breitung, 2005). We therefore include only countries for which the entire time-series of observations are available. With a starting date of 1960, we can include 47 countries in a balanced panel. When we postpone the starting date to 1970, the number of available countries increases to 59. With 1980 as the starting date, there are sufficient data for 70 countries. We choose the middle ground here, with $T = 37$ (1970–2006) and $N = 59$. Table 8 provides a list of countries. In addition to increasing the number of available countries, excluding the early years of aid data can be justified for data quality reasons (e.g., Juselius *et al.*, 2013). We also verify the robustness of our results by varying the starting date to 1960 and 1980.

Our benchmark model is a bivariate VAR for the variables aid per capita and real income per capita, both measured in logs. In addition, we estimate VARs with several alternative specifications. We consider the robustness of the results by changing the starting date, by measuring aid and income in aggregates instead of per capita terms (but still in logs), by reversing the recursive order of the variables, and by first-differencing aid and income (per capita, in logs). In addition, we supplement the VAR with domestic savings per capita and external savings per capita (which are not in logs, since these variables are not strictly positive). Details for all specifications are listed in Table 9, in which ($*$) refers to the benchmark model, and (a)–(i) to nine alternative specifications.

Table 8. *Countries*

Algeria[a,d]	Dominica[c]	Lesotho[a]	Senegal[a]
Argentina[b]	Ecuador[a]	Liberia[a,d]	Seychelles[a,d]
Bangladesh[c]	Egypt[a]	Madagascar[a]	Sierra Leone[a,d]
Belize[a,d]	El Salvador[a]	Malawi[a,d]	Sri Lanka[a]
Benin[a,d]	Fiji[a,d]	Mali[b]	Sudan[a]
Bhutan[c]	Gambia[b,d]	Mauritania[a]	Suriname[a,d]
Bolivia[a]	Ghana[a]	Morocco[a]	Swaziland[c]
Botswana[a]	Grenada[c]	Mozambique[c]	Syria[b]
Burundi[a]	Guatemala[a]	Nepal[a]	Togo[a,d]
Cameroon[a]	Guinea[c]	Nicaragua[a]	Tonga[c]
Central African Rep.[a]	Guinea-Bissau[c]	Niger[a,d]	Tunisia[b]
Chad[a,d]	Guyana[a,d]	Nigeria[a]	Turkey[b]
China[c]	Haiti[a]	Pakistan[a]	Uganda[a,d]
Colombia[b]	Honduras[a]	Panama[a,d]	Uruguay[b]
Comoros[c]	India[a]	Peru[b]	Venezuela[c]
Congo, D.R.[a]	Indonesia[b]	Philippines[a]	Zambia[a,d]
Congo, R.[a]	Jordan[b,d]	Rwanda[a]	
Cote d'Ivoire[a]	Kenya[a]	Saudi Arabia[b]	

Notes: Countries included in VAR analysis.
Source: See text.
[a] Countries included in datasets 1960–2006, 1970–2006 and 1980–2006.
[b] Countries included in datasets 1970–2006 and 1980–2006.
[c] Countries included in dataset 1980–2006.
[d] Countries not included in dataset including domestics and external savings.

Table 9. *VAR Specifications*

	y	T	N	Per capita	Logs	VECM
(∗)	(aid, gdp)	37 (1970–2006)	59	Yes	Yes	Yes, $r = 1$
(a)	(aid, gdp)	47 (1960–2006)	47	Yes	Yes	Yes, $r = 1$
(b)	(aid, gdp)	27 (1980–2006)	70	Yes	Yes	Yes, $r = 1$
(c)	(aid, gdp)	37 (1970–2006)	59	GDP only	Yes	Yes, $r = 1$
(d)	(aid, gdp)	37 (1970–2006)	59	No	Yes	Yes, $r = 1$
(e)	(gdp, aid)	37 (1970–2006)	59	Yes	Yes	Yes, $r = 1$
(f)	(Δaid, Δgdp)	37 (1970–2006)	59	Yes	Yes	No
(g)	(aid, gdp, ext, sav)	37 (1970–2006)	41	Yes	Aid and GDP only	No
(h)	(aid, gdp, ext, sav)	37 (1970–2006)	41	Yes	No	No
(i)	(Δaid, Δgdp, Δext, Δsav)	37 (1970–2006)	41	Yes	Aid and GDP only	No

Notes: VAR specifications. (∗): Benchmark model with impulse response functions presented in Figure 4. (a–i): Alternative VAR specifications presented in Figure 5(a–i). T: Time-series dimension. N: Cross-sectional dimension. "Per capita" indicates whether variables are measured in per capita terms or in aggregates. "Logs" indicates whether variables are transformed to logs. "VECM" indicates whether model is estimated in VECM (Eqn. (3)) or unrestricted VAR (Eqn. (2)) representation. In the case of a VECM representation, r denotes the co-integrating rank (see Breitung, 2005, for details).
Source: See text.

Table 10. *Lag Selection*

p	1	2	3	4	5	6
(∗)	9.807	*9.778*	9.782	9.798	9.808	9.819
(a)	9.511	*9.481*	9.490	9.502	9.516	9.524
(b)	9.710	*9.705*	9.718	9.737	9.744	9.758
(c)	9.816	*9.780*	9.785	9.798	9.811	9.824
(d)	9.837	*9.809*	9.815	9.828	9.838	9.848
(e)	9.807	*9.778*	9.782	9.798	9.808	9.819
(f)	9.885	*9.881*	9.890	9.904	9.916	9.926
(g)	43.336	42.881	42.642	*42.565*	42.578	42.566
(h)	65.299	64.675	64.514	*64.375*	64.377	64.381
(i)	43.409	42.950	42.758	*42.745*	42.748	42.793

Notes: Bayesian information criteria (BIC) for Panel VAR (p) model (Eqn. (2)), with p = 1–6. See Table 9 for details on the 10 different VAR specifications. Minimum BIC in each row is depicted in italics.
Source: See text.

The final column of Table 9 depicts whether each VAR is estimated as an unrestricted VAR (Eqn. (2)), or as a co-integrated VAR in VECM representation (Eqn. (3)). For the benchmark model (*), and for alternatives (a)–(e), we find that aid and income are co-integrated with rank = 1, based on the test procedure of Breitung (2005). For the differenced variables (f) and (i), we estimate a stationary VAR (Eqn. (2)).

For the VARs with four variables (g)–(h), we are not successful, despite the nonstationarity of the variables, in fitting a VECM (Eqn. (3)). We estimate specifications (g)–(h) therefore as an unrestricted VAR (Eqn. (2)), although even in this case the estimated VAR turns out unstable such that we are not able to compute confidence bounds for the impulse response functions. These problems presumably arise due to the inclusion of variables in levels rather than logs.

Before estimating the VAR, the lag-order p needs to be selected. We base this selection on the Bayesian Information Criterion (BIC). Table 10 denotes the BIC for all sets of variables, based on OLS estimates of a standard VAR (Eqn. (2)), with one up to six lags. For all bivariate VARs, the BIC is minimized for $p = 2$, while for the VARs including domestic and external savings a lag order of $p = 4$ is selected.

APPENDIX B. BOOTSTRAP

The impulse response functions are derived from the estimated matrices $\hat{A}_j$ in Eqn. (2). Models that are estimated as a VECM (Eqn. (3)), are after estimation transformed into a standard VAR (Eqn. (2)), i.e.: $\hat{A}_j = \hat{\alpha}\hat{\beta}' + \hat{B}_1$ and $\hat{A} + 2 = -\hat{B}_2$. Given the estimates μ_i, $\hat{A}_j$, and the fitted residuals $\hat{\varepsilon}_{i,t}$, the bootstrap simulation is executed as follows:

1. Draw (with resampling) T+100 $k\times 1$ vectors of errors from the set of residuals $\hat{\varepsilon}_i$.
2. Using μ_i, $\hat{A}_j$, and the sample of random errors from step 1, generate a $k \times (100 + T)$ matrix of simulated observations y_i.
3. Disregard the first 100 observations of y_i. Repeat steps 1–2 for all countries, to obtain a simulated $N \times T$ panel of observations.
4. Use the simulated panel to estimate $\hat{A}_j$ (either directly, or first as a VECM if this is the original specification), and to compute the corresponding impulse-responses.
5. Repeat steps 1–4 R times. In this paper, $R = 100.000$. Compute the sample standard deviation from the R impulse response functions. The 95% confidence bounds are computed by adding or subtracting 1.96 times the bootstrapped standard deviation from the originally estimated impulse response.