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Abstract:
This paper examines the long-run effect of foreign aid on income inequality for 21 recipient countries using panel cointegration techniques to control for omitted variable and endogeneity bias. We find that aid exerts an inequality increasing effect on income distribution.

Keywords: Inequality, foreign aid, panel data, cointegration.

JEL classification: D31, F35, C23

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1. Introduction

Taking donor strategies at face value, the urgent need for foreign aid of poor and disadvantaged people living in developing countries represents the overarching motive of official development assistance (ODA). The donor community committed itself in 2000 to achieving the Millennium Development Goals which would mainly benefit the poorest population segments and, thereby, reduce inequality in the recipient countries. Apart from halving absolute poverty by 2015, particularly relevant targets include the provision of universal primary education and universal access to reproductive health. Donors also agreed at the G8 summit in Gleneagles in 2005 that ODA has to be scaled up substantially to finance these important dimensions of pro-poor growth. According to Sachs and McArthur (2005: 347), ODA is a critical constraint to finance "what is crushingly expensive for the poorest of the poor." Increases in ODA, if properly directed, could thus improve the poverty reducing impact of a given rate of economic growth. Pro-poor growth, in turn, could enhance political stability and social cohesion which tend to be undermined by increasing inequality (OECD, 2006).[1]

It is open to debate, however, whether ODA effectively supports pro-poor growth. Recent surveys on the growth impact of aid come to sharply opposing verdicts. Doucouliagos and Paldam (2009: 433) conclude that "after 40 years of development aid, the preponderance of the evidence indicates that aid has not been effective." By contrast, McGillivray et al. (2006: 1031) summarize that more recent research "agrees with its general finding that aid works to the extent that in its absence, growth would be lower." Yet, even this positive assessment does not resolve the question of whether it is primarily the poor whose income prospects improve through ODA.[2] Anecdotal evidence rather suggests that mainly the political elite benefits from aid. It is estimated that the former president of Zaire, Mobutu Sese Seko, looted the treasury of at least $5 billion (an amount

[1] Bourguignon et al. (2009: 9) argue that if the high level of global income inequality "were to exist within a single country, that country would probably experience substantial social strive."

[2] On the other hand, the negative verdict of Doucouliagos and Paldam (2009) does not necessarily preclude favorable effects on the distribution of income as aid may alleviate poverty without having discernible average growth effects (Arvin and Barillas, 2002; Chong et al., 2009).

equal to the country's entire external debt at the time he was ousted in 1997). The funds allegedly embezzled by the former presidents of Indonesia and the Philippines, Mohamed Suharto and Ferdinand Marcos, are still higher (Svensson, 2005).

In contrast to the fairly large literature on growth effects, the distributional effects of ODA have received scant attention in previous empirical research. It is important to fill this gap in order to improve the allocation of aid and its effectiveness. Theoretical considerations show that aid may induce self-interested recipients to engage in rent-seeking activities aimed at appropriating resource windfalls (Svensson, 2000; Hodler, 2007; Economides et al., 2008). Moreover, donors may allocate aid in a way that deviates from the pro-poor growth rhetoric. As discussed in more detail in Section 2, several channels exist through which aid may result in more pronounced income inequality. The few existing empirical findings are mixed. Chong et al. (2009), using cross-section and system GMM panel techniques, find that aid has no robust effect on inequality. The random effects panel analysis by Bjørnskov (2010) reveals that the interaction of foreign aid and democracy in the recipient country is robustly and positively associated with income inequality. Shafiullah (2011) estimates fixed and random effects models and concludes that aid reduces income inequality.[3]

Against the backdrop of scarce and ambiguous findings, we re-assess the question of the distributional effects of foreign aid. Specifically, we contribute to the empirical literature by employing panel cointegration techniques to examine the long-run effect of aid on income inequality. Panel cointegration estimators are most appropriate in the present context. We argue in Section 3 that they are robust under cointegration to a variety of estimation problems that often plague empirical work, including omitted variables, slope heterogeneity, and endogenous regressors. To anticipate the results, we find that aid exerts an inequality increasing effect on the distribution of income.

[3] Bourguignon et al. (2009: 1) find that aid has a weak equality enhancing effect on the international distribution of income. But these authors abstract from "the admittedly critical element of within country inequality."

The remainder of this paper is composed of four sections. In Section 2, we discuss the related literature. Section 3 sets out the basic empirical model and describes the empirical methodology. The data and the estimation results are presented in Section 4, and Section 5 concludes.

2. Related literature

Two critical conditions would have to be met for ODA to be effective in reducing income inequality in recipient countries.[4] Donors would have to allocate aid in line with their rhetoric on pro-poor growth, by targeting the most needy and deserving. At the same time, the authorities in the recipient countries would have to ensure that aid actually reaches the poor. The following arguments suggest that both conditions tend to be violated once it is taken into account that foreign donors are not purely altruistic and local authorities have incentives to divert aid funds for personal benefit.

From the literature on aid allocation across recipient countries it is well known that donors have both altruistic and egoistic motives (e.g., Berthélemy, 2006). Specifically, this literature reveals that ODA is partly motivated by commercial and political self-interest. According to Alesina and Dollar (2000: 33), there is "considerable evidence that the direction of foreign aid is dictated as much by political and strategic considerations, as by the economic needs and policy performance of the recipients."[5] Alesina and Weder (2002) find that ODA is distributed indiscriminately between countries with honest and corrupt governments. Admittedly, the cross-country evidence does not allow any direct inference as to the distributional effects of ODA within recipient countries. Little is known about the distribution of aid within countries, either geographically or between segments of the population. Yet selfish donor motives are likely to

[4] For a similar line of reasoning, see Drazen (2007: 669), according to whom the ineffectiveness of aid may be "due to the actions of aid agencies (e.g., giving aid repeatedly, no matter what performance has been) and/ or recipients (e.g., misuse of aid)."
[5] More recently, Fleck and Kilby (2010) find that the emphasis placed on need has weakened for core recipients of US aid during the War on Terror.

compromise the needs- and merit-based allocation of aid within countries, too. For instance, commercial donor interests may have as a consequence that ODA, e.g., in the area of physical infrastructure, is concentrated in industrial clusters rather than remote areas where the poorest people are living. Likewise, using aid as a means to buy political support by the local elite implies that it favors the rich rather than the poor within a particular country.[6]

Even if donor countries did not use aid to foster commercial or political self-interest, the incentives of aid agencies might work against inequality reducing effects of ODA. As noted by Drazen (2007), agencies have strong incentives to continue disbursing aid independent of whether it is effective. The incentive to "push money out the door" (Drazen, 2007: 672) also implies that agencies favor large-scale operations rather than small projects reflecting indigenous creativity (Easterly, 2006), even though the latter may be better suited to reduce inequality. Information asymmetries create additional incentive problems working against inequality reducing effects of aid. On the one hand, taxpayers in the donor countries usually do not have the information required to assess the success or failure of specific aid interventions. On the other hand, political authorities are interested in proving the case for ODA. As a result, aid agencies might be inclined to 'plant their flag' and engage in highly visible projects in order to secure future funding. This may explain why Thiele et al. (2007) find that various donors prefer granting aid for higher levels of education, but spend very little on primary education even though the MDGs require donors to concentrate on universal access to basic education as a pre-condition for pro-poor growth. Moreover, agencies that have to demonstrate success to political authorities can be expected to minimize the risk of project failure which weakens their incentive to operate in remote regions where aid may be needed most.[7] In other words, the poverty orientation of aid agencies could be affected by pressure from their

[6] In a similar vein, Shafiullah (2011: 91) argues that aid tends to strengthen the political, social and economic influence of local elites that serve the donors' political and commercial interests.
[7] This line of reasoning is closely related to the principal-agent model of Fruttero and Gauri (2005). These authors argue that financial dependence of NGOs (the agents) on external funding (notably from official principals) drives a wedge between organizational imperatives related to future funding and charitable objectives in locations where NGOs are active.

principals to demonstrate project-related impact in the short run. Immediate and visible results are easier to achieve when addressing less entrenched forms of poverty. However, inequality reducing effects of ODA become less likely if agencies are reluctant to work in 'difficult environments.'

Furthermore, aid recipients are typically subjected to conditionality through which donors may affect the effectiveness of aid (Dalgaard, 2008). Specifically, aid in the past often took the form of structural adjustment lending which required the recipients to undertake policy reforms that donors deemed necessary for aid to be effective. However, critics of conditional aid blamed structural adjustment programs for having negative distributional effects, e.g., by involving cuts in local government spending on poverty relevant items such as basic education and health.[8] Negative distributional effects may also result from aid-related Dutch disease. Aid inflows may impair a country's competitiveness through real exchange rate appreciation (Doucouliagos and Paldam, 2008; Rajan and Subramanian, 2011). Poorer population segments may be affected most seriously, either because real incomes earned in the informal sector are not protected against higher inflation or because of lay-offs in low-skilled labor intensive production for exports (Bjørnskov, 2010).

Turning to the authorities in the recipient countries, it is widely acknowledged in the literature that local incentive problems stand in the way of ODA reaching the poor.[9] Svensson's (2000) seminal contribution departs from the puzzle that the macroeconomic effects of ODA are ambiguous even though it represents an important source of revenue in various recipient countries. Svensson explains this puzzle by developing a game theoretic rent-seeking model in which social groups within the recipient country compete over common-pool resources. Foreign aid may be used to provide public goods, or it may be appropriated for private benefit. The latter may happen "either by means of direct appropriation (e.g., seizure of power) or manipulation of bureaucrats and politicians to implement favorable transfers, regulations or other redistributive policies" (Svensson,

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[8] See Easterly (2003) for a critical discussion.
[9] See Drazen (2000, section 12.9) for an overview.

2000: 438). From the model, it can be derived theoretically that an increase in aid may even reduce the supply of public goods. It follows that aid does not necessarily lead to increased welfare.

The effects of aid on rent seeking and corruption in the recipient countries figure prominently in more recent papers, too. However the focus is typically on adverse growth implications (e.g., Hodler, 2007; Economides et al., 2008; Angeles and Neanidis, 2009).[10] In the present context, it is important to note that local elites and rich population segments tend to get the upper hand in aid-induced rent-seeking contests. Almost by definition, local elites are endowed with a disproportionate share of the country's economic and political power (Angeles and Neanidis, 2009). At the same time, the transfer that a group receives tends to be proportional to its expenditures on rent seeking as a fraction of total expenditures on rent seeking by all groups (Drazen, 2000: 606). Taken together, aid-induced rent seeking tends to favor the rich.[11] For instance, aid may exacerbate inequality by aggravating unequal access to education (Shafiullah, 2011). Indeed, Reinikka and Svensson (2004) find that the extent to which school grants in Uganda actually reached the ultimate beneficiaries depends on the bargaining power of local communities. Schools in better-off communities received a higher share of their entitlements.

In sum, there are various channels – involving agents on the side of both donors and recipients – through which aid may affect the distribution of income within aid-dependent countries. Importantly, the subsequent panel cointegration analysis does not attempt to isolate the effects of ODA on income distribution working through specific transmission channels. To the contrary, our objective is to capture overall effects; this provides a major argument in favor of the bivariate approach and against controlling for factors such as exchange rates and local institutions through

[10] As mentioned in the Introduction, few empirical studies focus on the distribution of income within recipient countries, notably Chong et al. (2009), Bjørnskov (2010), and Shafiullah (2011). Boone (1996: 293) uses infant mortality as the dependent variable which "can be considered a flash indicator of improvements in economic conditions of the poor."

[11] Boone (1996) raises the hypothesis that democratic and liberal political regimes use aid differently from 'elitist' regimes. However, Boone's empirical findings reveal that all political regimes direct aid to wealthy elites.

which aid may affect the distribution of income. Thereby, we also avoid serious measurement problems, e.g., with respect to rent-seeking activity (Economides et al., 2008).

3. Model and empirical methodology

The analysis will examine the long-run relationship between foreign aid and income inequality. In this section, we present the basic empirical model, discuss some econometric issues, and describe the empirical methodology.

3.1. Model

Following common practice in (panel) cointegration studies (see, e.g., Herzer, 2008; Pedroni, 2007; Chintrakarn, et al., 2012), we estimate a bivariate model of the form

$$Inequality_{it} = a_i + \beta Aid_{it} + \varepsilon_{it} \,, \qquad (1)$$

where $Inequality_{it}$ stands for the estimated household income inequality (EHII) in Gini format (measured on a 0 to 100 scale) of country i in year t and Aid_{it} represents net aid transfers (NAT) to i in t (as a percentage of GDP). The coefficient β in Eq. (1) can be interpreted as the long-run elasticity of inequality with respect to aid, measuring the change in the EHII Gini index in response to a one percentage point change in the share of aid in GDP. Moreover, we include country-specific fixed effects, a_i, to control for any country-specific omitted factors that are relatively stable over time.

Eq. (1) assumes that there is a long-run bivariate relationship between aid and inequality and that therefore no other variables are required to produce unbiased estimates of the long-run effect of aid on inequality. An attractive feature of Eq. (1), and the reason why we use it, is thus its parsimonious structure. However, a necessary condition for Eq. (1) to be a correct description of the data is that both the individual time series for the EHII Gini coefficient and the individual time series for aid (relative to GDP) are nonstationary or, more specifically, integrated of the same order and that $Inequality_{it}$ and Aid_{it} form a cointegrated pair.

A regression consisting of two cointegrated variables has a stationary error term, ε_{it}, in turn implying that no relevant integrated variables are omitted; any omitted nonstationary variable that is part of the cointegrating relationship would enter the error term, thereby producing nonstationary residuals and thus leading to a failure to detect cointegration. Eq. (1) would in this case represent a spurious regression in the sense of Granger and Newbold (1974) (if the variables are nonstationary and not cointegrated). If, on the other hand, a cointegrating relationship exists among a set of nonstationary variables, the same cointegrating relationship also exists in extended variable space (Johansen, 2000). Thus, an important implication of finding cointegration is that no relevant integrated variables are omitted in the cointegrating regression. Cointegration estimators are therefore robust to the omission of non-stationary variables that do not form part of the cointegrating relationship (Pedroni, 2007).

Of course, there are several factors (such as economic growth, trade, foreign investment, and redistribution policies) that affect inequality. Adding further variables may therefore result in further cointegrating relationships. Since the cointegration property is invariant to extensions of the information set, the estimates will not be significantly affected by the presence (or absence) of additional variables (Juselius, 2006). This justifies considering small subsystems, such as Eq. (1) (if the variables are cointegrated), while the inclusion of additional variables would unnecessarily increase the number of cointegrating equations that would have to be identified and estimated.

Another assumption underlying Eq. (1) is that income inequality is endogenous in the sense that, in the long run, changes in aid flows over time cause changes in income distribution over time. However, although cointegration implies the existence of long-run causality in at least one direction (Granger, 1988), income distribution may also act as a determinant of aid flows. For example, it may be that donors reward more equal countries with increased aid for successful poverty reduction or that aid goes to countries with higher inequality because these countries have higher poverty rates. The empirical implication is that it is important not only to examine the data for unit roots and cointegration, but also to account for this potential endogeneity.

Another important issue is the potential cross-country heterogeneity in the relationship between aid and inequality. Countries differ in terms of per capita income, economic, political, and social structures (and other characteristics). The implicit assumption of traditional, homogeneous panel estimators that the coefficients on the variables of interest are the same across all countries can therefore be unduly restrictive. The problem is that homogeneous (within-dimension) estimators may produce inconsistent and potentially misleading estimates in the presence of slope heterogeneity (Pesaran and Smith, 1995). For this reason, we use heterogeneous (between-dimension) panel estimators based on the mean group approach and thus allow the coefficients to vary across countries.[12]

A final econometric issue is the potential cross-sectional dependence among the variables through common time effects. Cross-sectional dependence may be the result of global business cycles and other common factors. Examples of such common factors that affect aid flows in several countries at the same time might include global natural disasters, regional wars and famines, and international donor coordination. Inequality is likely to be affected by common factors such as technological progress and the globalization of trade and investment. Given that standard panel unit root and cointegration tests may be biased in the presence of such cross-sectional dependence, we also use recent advances in panel data econometrics to account for this issue.

3.2. Empirical methodology

As the above discussion implies, the first step in the analysis is to examine the time series properties of the data. To this end, we use the panel unit root test of Im, Pesaran, and Shin (2003) (IPS), which is based on the augmented Dickey-Fuller (ADF) regression for the *i*th cross-section unit:

[12] Group-mean estimators are based on the between dimension of the panel, while the pooled estimators are based on the within dimension of the panel (Pedroni, 2000).

$$\Delta x_{it} = z_{it}'\gamma + \rho_i x_{it-1} + \sum_{j=1}^{k_i} \varphi_{ij} \Delta x_{it-j} + \varepsilon_{it}, \tag{2}$$

where k_i is the lag order and z_{it} represents deterministic terms (such as a constant and a trend). The unit root null hypothesis, $H_0: \rho_i = 0$, $\forall i = 1, 2, \ldots, N$, is tested against the alternative of (trend) stationary, $H_1: \rho_i < 0$, $i = 1, 2, \ldots, N_1$; $\rho_i = 0$, $i = N_1 + 1$, $N_1 + 2$, …, N, using the standardized t-bar statistic

$$\Gamma_{\bar{t}} = \frac{\sqrt{N}[\bar{t}_{NT} - \mu]}{\sqrt{v}}, \tag{3}$$

where $\bar{t}_{NT}$ is the average of the N cross-sectional ADF t-statistics, and μ and v are, respectively, the mean and variance of the average of the individual t-statistics, tabulated by Im et al. (2003).

However, the standard IPS test can lead to spurious inferences if the errors, ε_{it}, are not independent across i (for example, due to common shocks). Therefore, we also employ the cross-sectionally augmented IPS test proposed by Pesaran (2007), which is designed to filter out the cross-section dependence by augmenting the ADF regression with the cross-section averages of lagged levels and first-differences of the individual series. Accordingly, the cross-sectionally augmented ADF (CADF) regression is given by

$$\Delta x_{it} = z_{it}'\gamma + \rho_i x_{it-1} + \sum_{j=1}^{k_i} \varphi_{ij} \Delta x_{it-j} + \alpha_i \bar{x}_{t-1} + \sum_{j=0}^{k_i} \eta_{ij} \Delta \bar{x}_{t-j} + v_{it}, \tag{4}$$

where $\bar{x}_t$ is the cross-section mean of x_{it}, $\bar{x}_t = N^{-1}\sum_{i=1}^{N} x_{it}$. The cross-sectionally augmented IPS statistic is the simple average of the individual CADF statistics and is defined as

$$CIPS = t\text{-bar} = N^{-1} \sum_{i=1}^{N_i} t_i, \tag{5}$$

where t_i is the OLS t-ratio of ρ_i in Eq. (4) Critical values are tabulated by Pesaran (2007).

Another potential problem is that standard unit-root tests are biased towards a non-rejection of the null hypothesis of a unit root in the presence of structural breaks. Therefore, as an additional test, we apply the structural break unit-root test of Perron (1997) to each individual series of the panel. This test has that advantage that the potential break points are determined endogenously from the data. We estimate two models:

$$x_{1t} = \mu_1 + \theta_1 DU_t + b_1 t + \delta_1 D(TB)_t + a_1 x_{t-1} + \sum_{i=1}^{k} c_{1i} \Delta x_{t-1} + e_{1t}, \tag{6}$$

$$\begin{aligned} x_{2t} &= \mu_2 + b_2 t + \delta_2 DT_t + \hat{x}_t \\ \hat{x}_t &= a_2 \hat{x}_{t-1} + \sum_{i=1}^{k} c_{2i} \Delta \hat{x}_{t-1} + e_{2t} \end{aligned}, \tag{7}$$

where μ_1 and μ_2, respectively, are constants, t is a deterministic trend, $TB \in T$ ($1 \le t \le T$) denotes the time at which the break in the trend function occurs and $DU_t = 1(t > TB)$, $D(TB)_t = 1(t = TB + 1)$, $DT_t = 1(t > TB)(t - TB)$ are indicator dummy variables for the break at time TB.

Eq. (6) allows for a change in the intercept of the trend function, while Eq. (7) allows for a change in the slope of the trend function. TB is selected as the value which minimizes the t-statistic for testing $a_1 = 1$ and $a_2 = 1$, respectively:

$$t_a^*(i) = Min_{TB} t_{\hat{a}}(i, TB, k), \tag{8}$$

where $t_{\hat{a}}(i, TB, k)$ is the t-statistic for testing $a = 1$ under model $i = 1, 2$ [Eq. (6) and (7)]. If $Min_{TB} t_{\hat{a}}(i, TB, k)$ exceeds (in absolute value) the critical value reported by Perron (1997), the null hypothesis of a unit root is rejected in favor of (broken) trend stationarity.

If, as expected, the variables are nonstationary (because of the presence of a unit root), the next step is to test for cointegration. We use the standard tests of Pedroni (1999) and Kao (1999) for this purpose. It is well known, however, that these tests may suffer from severe size distortions in the presence of cross-sectional dependence.

To also test for cointegration in the presence of possible cross-sectional dependence, we use a two-step residual-based procedure in the style of Holly et al. (2010). Specifically, we employ the common correlated effects (CCE) estimation procedure of Pesaran (2006) in the first step. Like the cross-sectionally augmented IPS test, the CCE estimator allows for cross-sectional dependencies that potentially arise from multiple unobserved common factors and permits the individual responses to these factors to differ across countries. It augments the cointegrating regression (Eq. (1)) with the cross-sectional averages of the dependent variable and the observed regressors as proxies for the unobserved factors. Accordingly, the cross-sectionally augmented cointegrating regression for the *i*th cross-section is given by

$$Inequality_{it} = a_i + \beta_i Aid_{it} + g_{1i}\overline{Inequality_t} + g_{2i}\overline{Aid_t} + \xi_{it}, \qquad (9)$$

where $\overline{Inequality_t}$ and $\overline{Aid_t}$ are the cross-section averages of $Inequality_{it}$ and Aid_{it} in year t. In the second step, we compute the residuals, $\hat{\mu}_{it}$, of the individual CCE long-run relations (estimated using Eq. (9)), $Inequality_{it} = \hat{\beta}_i Aid_{it} + \hat{\mu}_{it}$, and apply the CADF test to the computed residuals, including an intercept. This allows us to account for unobserved common factors that could be correlated with the observed regressors in both steps.

If there is evidence of cointegration between $Inequality_{it}$ and Aid_{it}, the long-run effect of aid on income inequality is estimated using the group-mean fully modified ordinary least squares (FMOLS) and dynamic ordinary least squares (DOLS) estimators suggested by Pedroni (2001). Pedroni (2000, 2001) emphasizes several advantages of between-dimension group-mean-based estimators over the within-dimension approach. For example, the between-dimension approach allows for greater flexibility in the presence of heterogeneous cointegrating vectors, whereas under the within-dimension approach, the cointegrating vectors are constrained to be the same for each country (as noted above). Another advantage of the between-dimension estimators is that the point estimates provide a more useful interpretation in the case of heterogeneous cointegrating vectors, since they can be interpreted as the mean value of the cointegrating vectors, which does not apply to the within estimators. And finally, the group-mean estimators suffer from a much lower level of small-sample-size distortions than is the case with the within-dimension estimators.

In general, group-mean estimators involve estimating separate regressions for each country and averaging the slope coefficients:

$$\hat{\beta} = N^{-1}\sum_{i=1}^{N}\hat{\beta}_i \;. \tag{10}$$

The *t*-statistic is the sum of the individual *t*-statistics divided by the root of the number of cross-sectional units:

$$t_{\hat{\beta}} = \sum_{i=1}^{N} t_{\hat{\beta}_i} / \sqrt{N} \;. \tag{11}$$

The basic idea behind both the FMOLS estimator and the DOLS estimator is to account for possible serial correlation and endogeneity of the regressor(s). Thus, an important feature of these estimators is that they generate unbiased estimates for variables that cointegrate, even with endogenous regressors. Clearly, this is an important advantage for applications such as the present one since one cannot exclude the possibility that inequality is also a determinant of aid flows (as discussed above). In addition, the estimators are superconsistent under cointegration, and they are also robust to the omission of variables that do not form part of the cointegrating relationship.

The FMOLS estimator employs a non-parametric correction to eliminate the endogeneity bias using ε_{it} and ΔAid_{it}, whereas the DOLS estimator employs a parametric correction for the potential endogeneity by augmenting Eq. (1) with leads, lags, and contemporaneous values of the differenced aid variable:

$$Inequality_{it} = a_i + \beta_i Aid_{it} + \sum_{j=-k_i}^{k_i} \Phi_j \Delta Aid_{it-j} + \nu_{it} \;. \tag{12}$$

A potential disadvantage of the DOLS procedure in comparison to the FMOLS method is that the estimates may be sensitive to the choice of the lead and lag structure.

However, both the FMOLS estimator and the DOLS estimator may be biased in the presence of cross-sectional dependence. Therefore, we check the robustness of our results by using the CCE mean group (CCEMG) estimator of Pesaran (2006). This estimator is the simple average of the individual CCE estimators given by Eq. (9).

4. Data and empirical results

4.1. Data

Several studies have used the Gini data set constructed by Deininger and Squire (1996). However, it is well known at least since the work of Atkinson and Brandolini (2001) that the Deininger-Squire data suffer from deficiencies such as sparse coverage, problematic measurements, and the combination of diverse data types into a single dataset, thus limiting the comparability, not only across countries but also over time. Therefore, many studies rely on Gini data from the Luxembourg Income Study (LIS) database or the UNU-WIDER World Income Inequality Database (WIID). The major deficiency of all these sources is the lack of continuous and consistent inequality data over time (Galbraith, 2009).

In this paper, we use the estimated household income inequality (EHII) data set developed by the University of Texas Inequality Project (UTIP, 2008). These data are fully comparable across space and time (Galbraith and Kum, 2005). Another advantage is that the EHII data are available for a reasonably large number of countries over a sufficiently long and continuous time period.

The EHII index is in Gini format (measured on a 0 to 100 scale) and is constructed by combining information from the Deininger-Squire data set with information from the UTIP-UNIDO dataset. The latter is a set of measures of manufacturing wage inequality, using the between-groups component of a Theil index, measured across industrial categories in the manufacturing sector based on the Industrial Statistics database of the United Nations Industrial Development Organization (UNIDO). Specifically, the EHII index is computed by regressing the Deininger-Squire Gini indices on the UTIP-UNIDO Theil inequality measures (and on several control variables), and then using the predicted values as (estimated) Gini coefficients. The intention of this procedure is to separate the useful from the doubtful information in the Deininger-Squire data set (Galbraith and Kum, 2005).

Many of the more recent income inequality studies use the EHII Gini coefficient (Meschi and Vivarelli, 2009; Gimet and Lagoarde-Segot, 2011; Herzer and Vollmer, 2012). The inherent limitation of this index is that it is estimated, and estimates may be biased (for several reasons). Therefore, we will check the robustness of our results by using the Gini coefficient (based on net income) from the Standardized World Income Inequality Database (SWIID, 2011) developed by Solt (2009). The SWIID combines information from LIS and UNU-WIDER data to create an improved dataset with greater coverage than the LIS data and greater comparability than the UNU-WIDER data. A problem with the SWIID data is that they are estimated (like the EHII data) and that, furthermore, missing values are imputed. Therefore, and also because the SWIID data are not widely used in economic studies, the EHII Gini is our preferred measure of income inequality.

For our aid variable, we use Net Aid Transfers (NAT), as suggested by Nowak et al. (2012). Following common practice (see, e.g., Burnside and Dollar, 2000; Dalgaard et al. 2004; Herzer and Grimm, 2012), aid is expressed as a share of GDP (in percent). Data on GDP (in current prices) are from the World Development Indicators of the World Bank (2011). NAT data (in current prices) are from the Center for Global Development and have been calculated by Roodman (2011). The NAT variable alters two aspects of the standard net official development assistance (ODA) measure that can be problematic for aid research. First, net ODA is net only of principal payments received on ODA loans, not of interest received on such loans, while NAT is net of both. Second, NAT omits debt relief. Particularly the writing off of old non-aid loans (in the form of export credits or loans with excessively high interest rates) artificially boosts net ODA in the year of debt relief; therefore it is removed in NAT (Roodman, 2006).

In our main analysis, we will focus on the cointegrating relationship between the EHII Gini coefficient and NAT as a share of GDP. The identification and estimation of cointegrating relationships requires the use of continuous data over a sufficiently long period of time. Since panel cointegration methods can be implemented with shorter data spans than their time-series counterparts (due to exploitation of both the time-series and cross-sectional dimensions of the data), a period of about 25 observations should be more than sufficient for our purpose. We include all countries for which continuous data are available over a sufficiently long period of time, resulting in a balanced panel of 546 observations on 21 countries over the period 1970-1995 (26 years).

[*Table 1 about here*]

The countries along with the average values for $Inequality_{it}$ and Aid_{it} over the period of observation are listed in Table 1. There are considerable differences in the values of these variables across countries. Egypt had the highest NAT-to-GDP ratio, while the share of aid in GDP was lowest in Kuwait. Kuwait was the country with the highest inequality, while Malta ranked at the bottom of the inequality scale.

4.2. Empirical results

We begin this section by first examining the basic time-series properties of the data. Then, we test for the existence of a long-run or cointegrating relationship between $Inequality_{it}$ and Aid_{it}. Finally, we provide estimates of this relationship and test the robustness of our results.

Accordingly, the first step in the analysis is to investigate the time-series properties of the variables. We use the IPS test of Im et al. (2003) and the cross-sectionally augmented IPS test of Pesaran (2007) for this purpose. The results of these tests are presented in Table 2. Both the IPS and the cross-sectionally augmented IPS tests are unable to reject the null hypothesis that $Inequality_{it}$ and Aid_{it} have a unit root in levels. Since the null hypothesis of a unit root in first differences is rejected, it can be concluded that all series are integrated of the same order (one), I(1),—the necessary condition for cointegration in a bivariate context.

[*Table 2 about here*]

In the presence of a structural break, standard unit root tests are however biased towards the non-rejection of a unit root. We therefore need to make sure that the non-rejection of the unit root hypothesis for the variables in levels is not due to structural breaks. To this end, we apply the endogenous break unit root tests proposed by Perron (1997) to each individual series. The results of these tests are reported in Table 3. They show that even when potential structural breaks are taken into account, the unit root null hypothesis cannot be rejected for all level series.

[*Table 3 about here*]

Having established that the variables display all the characteristics of I(1) variables, we next turn to testing Eq. (1) for cointegration. To this end, we use the standard panel cointegration tests of Pedroni (1999) and Kao (1999). A potential problem with these tests is that they do not allow for cross-sectional dependence. Therefore, we also test for cointegration in the presence of possible cross-sectional dependence by using the Holly et al. (2010) approach. The results of the tests, which are presented in Table 4, show that there exists a cointegrating relationship between Aid_{it} and $Inequality_{it}$, as assumed as assumed in Eq. (1).

[*Table 4 about here*]

To estimate the relationship, we use the group-mean FMOLS and DOLS estimators suggested by Pedroni (2001). These estimators have the advantage that they produce unbiased estimates even with endogenous regressors and that they allow the coefficients to differ across countries. The estimation results are presented in Table 5. They show that the effect of aid on income inequality is statistically significant and *positive*. The DOLS estimate is somewhat higher than its FMOLS counterpart, which could be due to the loss of degrees of freedom as a result of adding a lag and lead of each explanatory variable. We therefore prefer the FMOLS result, according to which an increase in the share of aid in GDP by one percentage point leads to an increase in the EHII Gini coefficient by 4.21 units. This effect is not only statistically significant but also quantitatively important. The average NAT/GDP ratio for our 21 sample countries is slightly above two percent. Taken at face value, scaling up aid by about half that ratio would widen income inequality to the degree of almost one quarter of the difference in the EHII Gini coefficients between the two extreme values reported for Malta and Kuwait in Table 1. Recalling the discussion of possible transmission channels in Section 2, it has to be stressed that this is an overall effect which cannot be attributed to any particular channel. Specifically, in contrast to public opinion in donor countries, it is not only corruption and rent seeking in the recipient countries to blame. The behavior of donors, too, may bear major responsibility for the inequality increasing effects of aid.

[*Table 5 about here*]

We now perform several robustness tests. First, because the estimated effect of aid may be biased by the presence of potential cross-sectional dependencies, we report in the first column of Table 6 the result of the CCEMG estimator of Pesaran (2006). As can be seen, the CCEMG estimator and the group-mean FMOLS estimator produce similar results, suggesting that cross-sectional dependence is not a serious problem. However, the CCEMG estimator is intended for the case in which the regressors are exogenous, so that we lose the ability to account for the likely endogeneity of aid. We therefore continue our robustness analysis with the FMOLS estimator.

[*Table 6 about here*]

Next, we test whether the estimated β-coefficient is biased due to potentially unmodeled structural breaks in the individual FMOLS regressions. To this end, each individual FMOLS regression is re-estimated using step dummy variables for each possible break date in the period of observation. Following Ahmed and Rogers (1995), the significance of these dummies is assessed by a sequential Wald test, which is $\chi^2_{(1)}$ distributed. If any of the sequentially computed Wald statistics are larger than the conventional five-percent critical value of $\chi^2_{(1)}$, the null hypothesis of no structural break is rejected. Accordingly, the dates of the potential structural breaks are identified endogenously (as with the structural break unit root test of Perron (1997)). We include dummy variables for each break point detected by this procedure, although the sequential Wald test might tend to reject the null of no structural break too often at the (nominal) five-percent significance level, in particular when the sample period is short. Consequently, the individual coefficient estimates may be biased by falsely included dummy variables. Given, however, that the biases might be randomly and equally distributed across the countries, we can construct the group-mean panel FMOLS estimator for β from the sample average of the individual FMOLS estimators. The result of this estimation procedure is reported in the second column of Table 6. Again, the coefficient is positive.

We also examine whether our results are robust to the use of alternative inequality data. Specifically, we employ the Gini coefficients from the SWIID and include all countries for which continuous data are available over a 26-year period from 1980 to 2005, yielding a sample of 30 countries.[13] The results are presented in the third column of Table 6. The coefficient on the aid variable is positive and highly significant. Because we use a different sample and time period in this estimation, it can be concluded that our results are not only robust to different data sets, but also to different samples and time periods.

Finally, given the small number of countries in our sample, we need to ensure that the positive average effect of aid on inequality is not due to outliers. To this end, we re-estimate the FMOLS regression, excluding one country at a time from the sample. The sequentially estimated coefficients and their *t*-statistics are presented in Fig. 1. They fluctuate between 2.32 (due to the exclusion of Kuwait) and 5.00 (due to the exclusion of Turkey) and are always significant at least at the 5% level, suggesting that the positive effect of aid on income inequality is not the result of outliers.

[*Figure 1 about here*]

[13] These countries are: Argentina, Bangladesh, Brazil, Chile, China, Colombia, Costa Rica, Egypt, El Salvador, Guatemala, India, Indonesia, Jordan, Kenya, Madagascar, Malawi, Malaysia, Mauritius, Mexico, Morocco, Pakistan, Panama, Philippines, Sierra Leone, Trinidad and Tobago, Tunisia, Turkey, Uruguay, Venezuela, and Zambia.

5. Conclusion

In this paper, we examined the nature of the effect of foreign aid on income inequality using panel cointegration techniques. Employing data for 21 countries over the period 1970-1995, we found that aid exerts an inequality increasing effect on income distribution—an effect that is robust to different estimation methods, potential structural breaks, different inequality data sets, and possible outliers. This finding adds an important dimension to the aid effectiveness literature by complementing the long-lasting and still controversial debate on the growth impact of aid. In particular, our results contradict the optimistic view that aid might be effective in alleviating poverty in recipient countries even if it had no discernible average growth effects.

It has to be stressed that our analysis captured overall effects that cannot be attributed to any particular transmission mechanism. This has important implications. Previous literature focuses almost exclusively on inequality increasing effects through rent seeking by local elites in the recipient countries and the diversion of foreign aid for personal benefit. This invites calls for donors to strengthen the conditionality of aid (e.g., Boone 1996), to focus on countries with institutions that restrict rent seeking (e.g., Hodler, 2007), to prevent leakage by better monitoring the flow of aid resources within recipient countries (e.g., Bjørnskov, 2010), and to initiate and support "country-level processes that are formal, transparent and take account of the interests of the poor" (OECD, 2006: 12).

While these recommendations may help prevent inequality increasing effects of aid, they are not sufficient once other transmission channels are taken into account. Better accountability is required on both sides, recipients as well as donors. Donors do not necessarily allocate aid in line with their rhetoric on pro-poor growth, by targeting the most needy and deserving. The temptation of aid agencies to put all the blame on rent seeking in recipient countries tends to ignore that their own incentive problems may prevent aid from reducing inequality. Public outrage in the North about corruption in the South abstracts from the selfish aid motives that may lead donors to favor rich local elites. Overcoming selfish behavior and agency problems on the part of donors is unlikely to be easier than overcoming rent seeking and leakage on the part of recipients.

Isolating the impact of specific transmission mechanisms clearly warrants further research. This could help clarify the responsibility of donors and recipient countries for the distributional effects of foreign aid. Apart from more direct tests of specific transmission mechanisms, another approach may lend itself more easily to the panel cointegration methods employed in this paper. For instance, deeper insights may be gained by differentiating aid from different types of donors, using classifications of particularly selfish and more altruistic donors available from the aid allocation literature (e.g., Berthélemy, 2006). In a similar vein, it could be analyzed whether the distributional effects vary between major forms of aid such as project-specific and general budget support.

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Table 1

Countries and country summary statistics

Country	$Inequality_{it}$ (EHII Gini)	Aid_{it} (NAT/GDP)	Country	$Inequality_{it}$ (EHII Gini)	Aid_{it} (NAT/GDP)
Barbados	43.61	0.84	Kuwait	51.93	0.02
Bolivia	45.32	5.03	Malaysia	40.59	0.47
Chile	44.01	0.16	Malta	33.98	3.24
Colombia	42.68	0.40	Mexico	40.55	0.07
Ecuador	42.71	1.14	Philippines	45.43	0.69
Egypt	40.16	7.06	Singapore	38.80	0.26
India	43.23	0.79	Syria	42.93	5.89
Indonesia	44.51	4.09	Turkey	43.00	0.31
Israel	40.52	3.40	Venezuela	42.33	0.04
Kenya	47.25	6.44	Zimbabwe	43.64	2.83
Korea (Republic)	38.80	0.54			

Source: Own calculations.

Table 2

Panel unit root tests

Variables	Deterministic terms	IPS statistics	CIPS statistics
Levels			
$Inequality_{it}$	constant, trend	0.53	-2.00
Aid_{it}	constant, trend	-1.15	-2.23
First differences			
$\Delta\log(Inequality_{it})$	constant	-3.43**	-2.66**
$\Delta\log(Aid_{it})$	constant	-5.60**	-2.79**
$\Delta\log(Inequality_{it})$	constant, trend	-1.90*	-2.53**
$\Delta\log(Aid_{it})$	constant, trend	-3.72**	-2.72**

Source: Author's estimates. *Note:* Four lags were selected to adjust for autocorrelation. The relevant 1% (5%) critical value for the CIPS statistics is -2.92 (-2.73) with an intercept and a linear trend, and -2.40 (-2.21) with an intercept. ** (*) denote significance at the 1% (5%) level.

Table 3
Structural break unit root tests

Country	Series	Test statistic ($t_{\hat{a}}$) Model 6	Test statistic($t_{\hat{a}}$) Model 7	Country	Series	Test statistic ($t_{\hat{a}}$) Model 6	Test statistic ($t_{\hat{a}}$) Model 7
Barbados	$Inequality_t$	-1.88	-3.99	Kuwait	$Inequality_t$	-2.23	-1.89
	Aid_t	-2.29	-4.12		Aid_t	-4.11	-4.32
Bolivia	$Inequality_t$	-3.33	-1.21	Malaysia	$Inequality_t$	-1.11	-0.91
	Aid_t	-4.33	-2.23		Aid_t	-2.24	-2.17
Chile	$Inequality_t$	-1.23	-2.89	Malta	$Inequality_t$	-2.45	-3.22
	Aid_t	-4.01	-2.45		Aid_t	-4.23	-4.11
Colombia	$Inequality_t$	-2.23	-3.15	Mexico	$Inequality_t$	-3.99	-3.26
	Aid_t	-3.44	-2.01		Aid_t	-4.82	-4.00
Ecuador	$Inequality_t$	-2.78	-3.81	Philippines	$Inequality_t$	-2.67	-1.69
	Aid_t	-1.55	-2.23		Aid_t	-3.54	-2.12
Egypt	$Inequality_t$	-1.76	-1.73	Singapore	$Inequality_t$	-3.67	-2.37
	Aid_t	-4.12	-3.45		Aid_t	-4.15	-2.10
India	$Inequality_t$	-3.98	-3.78	Syria	$Inequality_t$	-2.34	-1.85
	Aid_t	-4.82	-3.16		Aid_t	-3.23	-3.21
Indonesia	$Inequality_t$	-1.99	-2.45	Turkey	$Inequality_t$	-3.55	-4.20
	Aid_t	-4.99	-3.56		Aid_t	-2.19	-3.01
Israel	$Inequality_t$	-3.34	-3.44	Venezuela	$Inequality_t$	-3.41	-2.57
	Aid_t	-2.65	-3.14		Aid_t	-3.33	-3.69
Kenya	$Inequality_t$	-4.32	-4.21	Zimbabwe	$Inequality_t$	-1.76	-2.09
	Aid_t	-3.01	-2.06		Aid_t	-2.89	-2.16
Korea (Rep.)	$Inequality_t$	-2.77	-2.45				
	Aid_t	-4.21	-3.98				

Source: Author's estimates. *Note:* The number of lags was determined by the Schwarz criterion with a maximum number of four lags. The relevant 5% critical value for Model 6 (Model 7) is -5.23 (-4.83) (see Perron, 1997, Table 1, p. 362).

Table 4
Panel cointegration tests

Holly et al. (2010)	
CIPS statistic	-2.49**
Pedroni (1999)	
Panel *v*-statistic	2.01*
Panel PP *rho*-statistic	-8.81**
Panel PP *t*-statistic	-9.25**
Panel ADF *t*-statistic	-5.39**
Group PP *rho*-statistic	-4.26**
Group PP *t*-statistic	-6.19**
Group ADF *t*-statistic	-5.08**
Kao (1999)	
DF *rho*-statistic	-16.10**
DF *t*-statistic	-9.14**
ADF *t*-statistic	-2.06*
DF^* *rho*-statistic	-2.21*
DF^* *t*-statistic	-3.37**

Note: ** (*) indicate a rejection of the null of no cointegration at the one percent level. The relevant 1% critical value for the CIPS statistic is -2.40. All other test statistics are asymptotically normally distributed. The number of lags was determined by the Schwarz criterion with a maximum number of four lags. *Source:* Author's estimates.

Table 5
Estimates of the long-run effect of aid on income inequality

FMOLS mean group estimator	DOLS mean group estimator
4.210**	5.629*
(3.51)	(2.57)

Source: Author's estimates. *Note:* *t*-statistics in parenthesis. ** (*) indicate significance at the 1% (5%) level. The DOLS regressions were estimated with one lead and one lag.

Table 6
Robustness estimates of the long-run effect of aid on income inequality

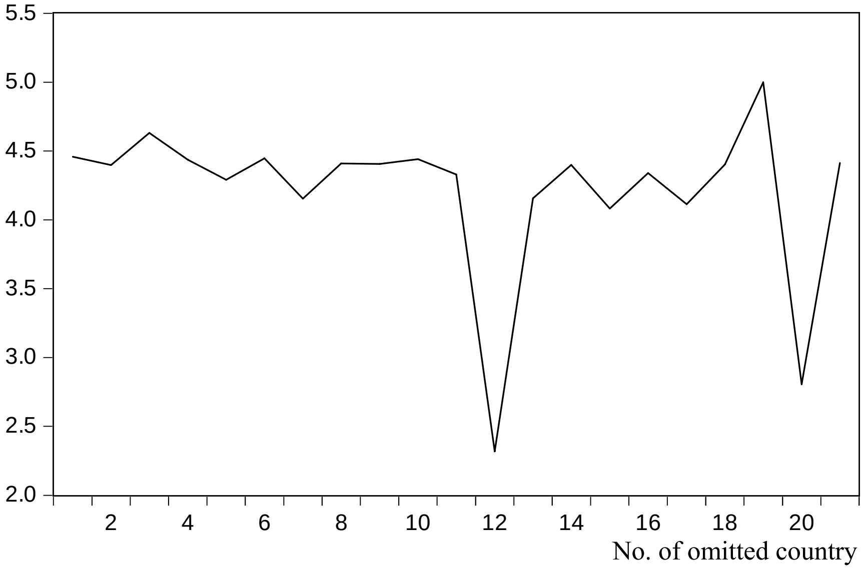
CCE mean group estimator	FMOLS mean group estimation with dummy variables for structural breaks	FMOLS mean group estimation with income inequality data from the SWIID
4.442**	3.177**	3.390**
(5.87)	(3.07)	(5.66)

Source: Author's estimates. *Note:* *t*-statistics in parenthesis. ** indicate significance at the 1% level.

Figure 1

FMOLS estimation with single country excluded from the sample

Coefficients on Aid_{it}



t-statistics of the coefficients