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Abstract

Using a new instrumental variable strategy, we examine whether bilateral development aid increases military expenditure in recipient countries. The instrument is the interaction of donor government fractionalization and the probability of receiving aid. The dataset includes new data on military expenditure for 124 recipient countries over the 1975–2012 period. When accounting for outliers, our results do not suggest that development aid affects military expenditure in the full sample. However, the effect of aid on military expenditure varies across characteristics of recipient and donor countries, even after excluding outliers. First, aid increases military expenditure in countries that depend on aid and are prone to conflicts. Second, aid provided by coordinated market economies increases military expenditure.

JEL code: F35, H56, O11
Keywords: Aid, military expenditure, fungibility, instrumental variables, causality

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Introduction

Donor countries provide Official Development Assistance (ODA) to promote economic growth and development in recipient countries. Scholars disagree, however, as to whether development aid is effective in achieving these goals (e.g., Collier and Hoeffler 2004, Dalgaard et al. 2004, Doucouliagos and Paldam 2008, 2011, Arndt et al. 2015). It is conceivable that aid is ineffective because it is not used for its intended purpose, indicating that aid is fungible.[1] Anecdotal evidence suggests that governments in recipient countries have used development aid to rig the military. In June 2015, for example, *The Telegraph* reported "British aid billions 'subsidising' third world defense budgets."[2] We use a new instrumental variable strategy to examine whether bilateral development aid increases military expenditure in recipient countries.

Finding an effect of ODA on military expenditure would provide evidence for development aid being shifted to purposes other than those intended. Since military aid is not reportable as ODA, ODA should not subsidize the recipient country's military. The study thus elaborates on whether aid is fungible rather than investigating the relationship between military aid and military expenditure. Investigating the fungibility of aid implies a focus on development aid, which is given

[1] By using "intended" we refer to the OECD Development Assistance Committee (DAC) definition of ODA. Aid has often been given and used for reasons other than the promotion of economic development such as for influencing election outcomes in recipient countries (see Faye and Niehaus 2012 and Cruz and Schneider 2017).

[2] http://www.telegraph.co.uk/news/uknews/defence/11654852/British-aid-billions-subsidising-third-world-defence-budgets.html (accessed on December 7, 2015).

with explicitly non-military purposes.[3] A direct effect of diverting aid to unintended purposes such as increasing military activity is that the funds to execute intended development projects are missing. These absent funds are likely to confine the overall growth effects of development aid. More indirectly, even if development aid was used for the intended projects, the recipient government can use the freed-up financial resources for other purposes it would not have subsidized otherwise, such as military expenditure. Rising military expenditure, in turn, is likely to influence the country's level of violence and conflict. While military expenditure can have a stabilizing effect on conflict, Collier and Hoeffler (2007) show that increasing military expenditure has a destabilizing effect through accelerating the risk of conflict. In a similar vein, Pamp et al. (2018) find that extending mid- and long-term arms imports gives rise to a higher likelihood of sparking a new intrastate conflict. An increased risk of violence and conflict may, in turn, decrease economic growth. Consequently, increasing military expenditure might offset, or even exceed the positive effects of aid, resulting in no significant impact of aid on growth at the macro-level.

Our study has three innovations. First and most importantly, we apply a new instrumental variable for aid that has been proposed by Dreher and Langlotz (2017) in their study of aid and growth. More specifically, we use the interaction of donor government fractionalization and the probability of receiving aid as an IV. Whereas donor government fractionalization introduces variation over time, the recipient country's probability of receiving aid provides cross-country

[3] On the relationship between military and non-military aid see, for example, Deger and Sen (1991). Data on military aid are only available for the United States, but not for the other donor countries we examine. We thus control for US military aid to avoid omitted variable bias when estimating the effect of ODA on military expenditure.

variation. The interaction term consequently varies across both time and recipient countries. To the extent that donor government fractionalization affects bilateral aid through its effects on total government spending and thus through the overall aid budget, it has been found to be a powerful IV (Dreher and Langlotz 2017). We use the resulting excludable IV to identify the causal effects of development aid on military expenditure in recipient countries. Second, we use new data on military expenditure for 124 recipient countries over the 1975-2012 period. Third, we examine whether the effect of aid on military expenditure is heterogeneous across characteristics of recipient and donor countries. Investigating heterogeneous effects helps to better tailor policy implications for the different groups of recipient countries. Tailored policy implications for individual groups of recipient countries may prevent donor countries from applying one-size-fits-all policy measures that are likely to harm development aid effectiveness. We consider differences in recipient countries based on political institutions, aid dependence, conflict, corruption, income, and across different types of donor countries.

Previous studies on aid and military expenditure

ODA aims to promote development in recipient countries and officially excludes funds meant to arm recipient countries. One question, however, is to what extent ODA is fungible. Scholars investigate whether governments in recipient countries use ODA on projects other than those the transfers were originally intended for.[4] One example for the fungibility of aid is to divert

[4] For a broader discussion on fungibility see, for instance, Van de Sijpe (2013a, 2013b) and Morrissey (2015). For single-country studies see Pack and Pack (1990, 1993), Van de Walle and

development aid to the military. Some previous studies examine whether increasing development aid gives rise to higher military expenditure. Those studies differ regarding the type of data (cross-sectional or panel data versus data for individual countries) and regarding the identification strategy (correlations versus attempts to identify causal effects).

Cross-sectional and panel data studies elaborating on correlations between aid and military expenditure do not suggest that aid and military expenditure are positively associated. Cashel-Cordo and Craig (1990) disentangle how different types of aid (e.g., loans and grants, bilateral and multilateral aid) are correlated with non-defence and defence public expenditure in a dataset of 46 less developed countries over the 1975-1980 period. Defence expenditure is not correlated with highly conditional IMF disbursements, DAC bilateral ODA loan disbursements, DAC bilateral ODA grants, and local currency disbursements, and negatively correlated with low conditional IMF disbursements and IMF commodity disbursements. Feyzioglu et al. (1998) examine how aid correlates with budget composition in recipient countries by using panel data for 38 developing countries over the 1971-1990 period. They use different types of government expenditure such as spending on defence (as a share of GDP) as the dependent variables. The most important explanatory variables are the share of net disbursements of total foreign aid and net disbursements of foreign aid to individual sectors such as education or health (both measured as a share of GDP). The results do not show that more aid is associated with higher military expenditure.

On the contrary, studies focussing on individual countries show that US aid, in particular, is correlated with military expenditure in recipient countries. For example, US aid to Israel is used

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Mu (2007), and Wagstaff (2011) among others. Empirical evidence on both cross- and single-country studies is mixed.

to increase military expenditure (McGuire 1982 and 1987) and US aid to Pakistan is used for higher military and non-military expenditure (Khilji and Zampelli 1991). However, the previous studies by McGuire (1982 and 1987) and Khilji and Zampelli (1991) are based on very small samples. By using a larger sample of eight major recipient countries over the 1972-1987 period Khilji and Zampelli (1994) arrive at the same conclusion, namely that US aid is highly fungible and positively correlated to a large extent with military expenditure.[5]

There are several reasons why aid should be endogenous with respect to military expenditure. Endogeneity is likely to result from reversed causality and omitted variable bias. Firstly, causality can be reversed as donor countries observe how recipient countries develop. When a recipient country increases military expenditure, donor countries may well believe that the recipient country will threaten other countries or repress its own people. As a result, donor countries will decrease aid to the individual recipient country, either to prevent the recipient from misusing the flows or to avoid encouraging governments that do so. Donor countries are usually democracies, and voters in donor countries are likely to punish (vote out of office) the domestic

[5] Dube and Naidu (2015) focus on the effects of US military assistance in Colombia rather than looking at ODA. The authors therefore examine the direct effects of aid intended to support the military of the recipient country. The results indicate that paramilitary attacks increase in the course of more US military assistance, even after controlling for government attacks. By contrast, US military assistance has not been found to influence guerilla warfare. Dube and Naidu (2015) exploit variance across Colombian municipalities that have military bases and, in turn, receive US military aid. However, the results do not show that US aid increases counter-narcotics activities, despite this being the official intention of US assistance to Colombia.

government for providing foreign aid to governments in recipient countries that do not use aid for its intended purposes. Certainly, some recipient countries that increase military expenditure may well do so to protect themselves during a conflict, and not to threaten other countries or its own people. Secondly, a recipient country is likely to increase military expenditure when being in conflict with other countries. When the recipient country is allied with an individual donor country (for example, as a former colony or participating in a pact such as the US Defense Pact – see Leeds et al. 2002), the allied donor is likely to increase foreign aid during the conflict. Being allied is therefore likely to influence the recipient's military expenditure – apart from its effect through aid – as the recipient expects the allied donor to provide protection. Ordinary Least Squares (OLS) results would thus be biased because of omitted variables that affect both military expenditure and development aid.

Two studies advanced research on the nexus between aid and military expenditure by using instrumental variables for aid to deal with endogeneity (Collier and Hoeffler 2007, Kono and Montinola 2012). We are, however, sceptical as to whether the proposed IVs are excludable, as we will explain below.

Collier and Hoeffler (2007) examine whether aid increases military expenditure (as a share of GDP) over the 1960-1999 period in 161 recipient countries. When estimating the baseline model by OLS, aid (as a share of GDP) does not turn out to be statistically significant. Collier and Hoeffler (2007) acknowledge that: "aid may be endogenous to the government's chosen level of military spending" (p. 11) and use an IV that considers the extent to which donor and recipient countries are politically, culturally and geographically aligned. To construct the instrumental variables, they focus on bilateral aid outflows of the (then) five largest donor countries (Japan, the United States,

France, Germany, and the United Kingdom).[6] Their IV findings show that when aid (as a share of GDP) increases by one-percentage point, military expenditure (as a share of GDP) increases by around 3.3 percent. We are sceptical as to whether Collier and Hoeffler (2007) identify a causal effect of aid on military expenditure since the exclusion restriction is likely to be violated. For instance, recipient countries that are politically, culturally, and geographically close to important donor countries are likely to be supported by donor countries during a conflict. The recipient may therefore be inclined to keep its military small because it expects that the allied donor country will provide protection.

In a similar vein, Kono and Montinola (2012) examine whether aid increases military expenditure, especially by disentangling aid-induced effects between democratic and autocratic recipient countries. They argue that previous studies have not arrived at a consensus, as the effects of aid on military expenditure are not alike for different types of recipient governments. The authors propose that autocratic leaders maintain power by redistributing resources to a small group of influential supporters. The military is an excellent case in point because it safeguards the autocrats' power and repression. By contrast, in democracies, citizens would be expected to protest against rulers who redistribute resources to a small group of supporters. Kono and Montinola (2012) estimate an error-correction model for 109 countries over the 1960-2004 period. The results

[6] The political, geographic, and cultural distance of each donor from each recipient is measured by four variables. Political distance is proxied for by an index of UN voting affinity (Gartzke and Jo 2002), geographical proximity by the inverse of the distance in kilometres between the capitals of the donor and recipient, and cultural distance by dummy variables capturing common language and common principal religion.

show that aid increases military expenditure in autocratic recipient countries, but does not influence military expenditure in democratic ones. The effects estimated for pure democratic countries do not turn out to be statistically significant. Kono and Montinola (2012) also deal with the endogeneity of aid by using IVs. They use a measure of foreign policy similarity and higher-order moments of each endogenous explanatory variable as IVs, and arrive at similar results compared to their OLS findings.[7] We are, however, again sceptical as to whether this measure of foreign policy similarity is exogenous to the dependent variable, military expenditure, for the very same reason as for Collier and Hoeffler (2007). Consequently, we do not believe that their evidence reflects the causal effect of aid on military expenditure. We therefore examine how foreign aid affects military expenditure by using a valid IV. The next section describes our identification strategy in detail.

Method and data

Our panel data model at the recipient-year-level is:

$$Military\ expenditure_{i,t}=\beta_1 Aid_{i,t} + \boldsymbol{X'_{i,t}}\beta_2 + \beta_3\eta_i + \beta_4\tau_t + \varepsilon_{i,t}, \quad (1)$$

where $Military\ expenditure_{i,t}$ is recipient country i's yearly military expenditure as a share of GDP. We use the new dataset on military expenditure from the Stockholm International Peace Research Institute (SIPRI), which includes data for the OECD's Development Assistance

[7] The measure of foreign policy similarity is that of Signorino and Ritter (1999) – from EUGene (Bennett and Stam 2000). Kono and Montinola (2012) use the Lewbel (1997) solution of including higher-order moments of each endogenous explanatory variable.

Committee (DAC) recipient countries over the 1975-2012 period.[8] $Aid_{i,t}$ ($Aid_{i,t-1}$) is the amount of net ODA as a share of GDP disbursed by the 28 bilateral donors of the DAC in year t (in year $t-1$). ODA includes those transfers i) that are provided by official agencies to developing countries and multilateral institutions; ii) with the main objective of economic development and welfare; and iii) which have a concessional character reflecting that the grant element should be of at least 25 percent.[9]

We regress military expenditure in year t on foreign aid in year t and alternatively in year $t-1$ because the timing of the effect is not clear. Moreover, since military spending often involves

[8] The new dataset is called the SIPRI Extended Military Expenditure Database, Beta Version, 2016. Previous versions of the SIPRI data included military expenditure prior to 1988 only for NATO member countries. SIPRI marks few data entries as more than usually uncertain. In a robustness test we exclude recipient countries that have more than one uncertain observation. Inferences do not depend on the inclusion of these countries. Smith and Cavatorta (2016) also use the extended SIPRI military expenditure data.

[9] Net ODA includes repayments. Consequently, the variable assumes negative values if repayments are larger than the flows received. Predicting aid via the instrumental variable may also produce negative values. To avoid that these negative values are driving our results we set negative (actual) aid to GDP to zero and a) either allow for negative predicted aid values, b) exclude all negative predicted aid values, or c) replace negative predicted aid with zero. In all three cases our main results (presented in Table 1 and 2) remain robust.

long-run contracts with arms manufacturers, we also investigate results using averaged data over four- and five-year periods.[10]

Following related studies examining determinants of military expenditure in developing countries, all regressions contain a vector of contemporaneous control variables $\boldsymbol{X_{i,t}}$, which considers the domestic economic and political environment and security threats: Polity IV democracy index (Marshall et al. 2010), log of constant GDP per capita (WDI), and domestic and interstate conflicts (UCDP/PRIO). Since some of the covariates are potential outcomes of development aid, the inclusion of them could bias the coefficient estimates of our variable of interest. For this reason, we also report IV results excluding covariates apart from year- and country-fixed effects and find our inferences to be unaffected by this choice.[11] Autocratic governments are expected to spend more on the military than democratic governments (e.g., Collier and Hoeffler 2007, Dunne et al. 2008, Nordhaus et al. 2012). For example, autocratic rulers may exploit the military to stay in power. In democracies, citizens are likely to support government spending on collective goods such as education and health care and discourage spending on the military. One possible reason for this is that citizens in democracies may fear that a large military will suppress civil liberties. Empirical evidence on the association between per capita GDP and military expenditure is mixed (e.g., Collier and Hoeffler 2007, Dunne et al. 2008, Nordhaus et al.

[10] In a robustness test, we follow Collier and Hoeffler (2007) in using averaged, rather than yearly data. We compare both four-year and five-year averages. Inferences of the main results do not change when using four- and five-year averages and excluding the most outstanding outlier, Liberia.

[11] Results are shown in Appendix B, Table B2.

2012). Obviously, countries involved in conflicts and fearing threats are expected to have higher military expenditure (e.g., Collier and Hoeffler 2007, Dunne et al. 2008, Nordhaus et al. 2012). In alternative specifications, we include the lagged dependent variable and lagged neighboring countries' military expenditure as a share of total neighboring countries' GDP. In a robustness test we also control for log of population, government ideology, conscription and the presence of an active force. Population is expected to have a negative effect on military expenditure because larger countries seem to inherently feel safer than smaller ones (e.g., Collier and Hoeffler 2007, Dunne et al. 2008). When explaining military expenditure in industrialized countries, scholars also use government ideology as an explanatory variable (see, for example, Kauder and Potrafke 2016, Bove et al. 2017; Blum and Potrafke 2019; for a survey on government ideology and economic policy-making in OECD countries Potrafke 2017). Rightwing governments are often expected to spend more on the military than leftwing governments. It is not quite clear how conscription will affect military expenditure (as a share of GDP). Conscription tends to be negatively correlated with economic growth (Keller et al. 2009); military expenditure as a share of GDP should therefore increase when conscription is present. However, the expected effect of conscription on military budget is negative with given military output because governments take labor without paying for it. The empirical evidence on how conscription correlates with military expenditure is somewhat mixed and suggests, if anything, that conscription increases military expenditure (e.g., Batchelor et al. 2002, Poutvaara and Wagener 2011). The presence of an active force is expected to increase military expenditure.[12]

[12] For the sources and definitions of all variables and descriptive statistics, see Appendix A.

As in Dreher and Langlotz (2017) we follow Frankel and Romer (1999) and Rajan and Subramanian (2008) in using a zero-stage regression to predict bilateral aid from an IV that varies over recipient-donor pairs. The zero-stage regression at the recipient-donor-year-level is:

$$Aid_{i,j,t} = \gamma_1 FRAC_{j,t} * P_{i,j} + \gamma_2 FRAC_{j,t} + \gamma_3 \eta_{i,j} + \gamma_4 \tau_t + \varepsilon_{i,j,t}, \quad (2)$$

where $Aid_{i,j,t}$ describes the bilateral amount of net ODA (as a share of GDP) from donor j disbursed to recipient i in year t. $FRAC_{j,t} * P_{i,j}$ is the proposed IV, which is the interaction of a time-variant variable – donor government fractionalization $FRAC_{j,t}$ – and the recipient country's probability of receiving aid $P_{i,j}$ that varies across donor-recipient pairs. We control for the time-varying level of the IV by including $FRAC_{j,t}$ and we capture the time-invariant level by including donor-recipient-fixed effects $\eta_{i,j}$ in equation (2). As in equation (1) τ_t are year-fixed effects.[13] We use Beck et al.'s (2001) variable of government fractionalization, which measures the probability that two randomly-chosen deputies from among the parties forming the government represent different parties. Because government fractionalization is zero for Canada and the United States throughout the observation period, we use legislature fractionalization for these two countries.

[13] This approach differs from Dreher and Langlotz (2017) to the extent that we include fixed effects in the zero-stage regression. As Dreher and Langlotz (2017) describe in more detail, the two approaches (excluding or including fixed effects in the zero-stage regression) give rise to identical results under certain conditions such as using a balanced sample. The authors also refer to simulation analyses for the conditions under which these approaches produce the same results. In our sample first-stage F-statistics are, however, borderline if we do not include fixed effects in the zero-stage regression, pointing to the difference of the two approaches in case of an unbalanced sample.

Results remain robust when excluding Canada and the United States.[14] Similar to previous studies (Nunn and Qian 2014, Ahmed 2016), the probability of receiving aid from donor j is defined as $\bar{P}_{i,j} = \frac{1}{38}\sum_{y=1}^{38} P_{i,j,y}$ where $P_{i,j,y}$ is a binary variable taking a value of 1 when recipient i received a positive amount of aid from donor j in year y.

From the zero-stage regression at the dyadic-level we first predict $\widehat{Aid}_{i,j,t}$ by using the exogenous variation of the dyadic instrument. We then aggregate the fitted values $\widehat{Aid}_{i,j,t}$ of the zero-stage (equation (2)) across all 28 donors, j, to compute the aggregated fitted value of aid as a share of GDP ($\widehat{Aid}_{i,t}$) at the recipient-year-level:

$$\widehat{Aid}_{i,t} = \sum_j \left[\widehat{\gamma_1} FRAC_{j,t} * P_{i,j} + \widehat{\gamma_2} FRAC_{j,t} + \widehat{\gamma_3}\eta_{i,j} + \widehat{\gamma_4}\tau_t + \varepsilon_{i,j,t}\right]. \quad (3)$$

The aggregation in equation (3) is essential as we can now switch from the dyadic-data-level to the recipient-level, which is the relevant level for the analysis of whether aid influences military expenditure in recipient countries. We use $\widehat{Aid}_{i,t}$ as an instrument to predict $Aid_{i,t}$ in equation (1) in order to estimate the causal effect of aid on military expenditure.[15] In this step we

[14] Most of the 28 donor countries in our sample have parliamentary systems with proportional representation. Canada has a plurality voting system and the United States have presidential elections, which explains why donor government fractionalization does not vary in these two countries. Moreover, France and the United Kingdom are two other exceptions regarding the electoral rule, but in both countries government fractionalization varies and we do not replace it with legislature fractionalization. Inferences also do not change when we replace government fractionalization with legislature fractionalization for the United Kingdom and France.

[15] In analogy, we use use $\widehat{Aid}_{i,t-1}$ as an instrument to predict $Aid_{i,t-1}$ in equation (1).

revert to the usual 2SLS procedure with the only difference being that we first constructed the instrument from the bilateral regression equation (2). The dataset includes 124 recipient countries over the 1975-2012 period.[16] Standard errors are clustered at the recipient-level.[17]

The interaction of government fractionalization with the probability of receiving aid is likely to be a powerful IV. While government fractionalization has been shown to increase government expenditure, government expenditure is likely to increase the aid budget, therefore increasing bilateral amounts of ODA. Roubini and Sachs (1989), Volkerink and de Haan (2001), Scartascini and Crain (2002), and Martin and Vanberg (2013) show that higher government fractionalization gives rise to higher government expenditure. This effect can be explained by logrolling, which is likely to occur in coalition governments during the budgeting process, as all government parties are interested in getting their favoured projects financed (common pool problem). Higher government expenditure has been shown to increase aid budgets of a donor country (Round and Odedokun 2004, Brech and Potrafke 2014). Dreher and Fuchs's (2011) study

[16] Following Dreher and Langlotz (2017), we include recipient countries that have been on at least one "DAC List of ODA Recipients" between 1997 and 2013. The list of the 28 donor countries and 124 recipient countries is reported in Appendix A, Table A3.

[17] Wooldridge (2010) describes that IV estimates and standard errors are still consistently estimated when using a generated instrument if the condition holds that the second-stage error term is not correlated with the variables that we use to generate the instrument. We do, however, also test our first- and second-stage results with bootstrapped standard errors based on pairwise recipient country clusters. Standard errors at the zero-stage regression are clustered at the recipient-donor-level.

completes the channel from fractionalization to bilateral aid disbursements by showing that higher aid budgets translate into higher bilateral aid disbursements. On the other hand, the probability of receiving aid is likely to influence the extent to which changes in government fractionalization affect bilateral aid disbursements. Nunn and Qian (2014) and Ahmed (2016) show that the probability of receiving aid is significantly correlated with the total amount of aid receipts per recipient country.

Dreher and Langlotz's IV approach is based on Werker et al. (2009), Nunn and Qian (2014), and Ahmed (2016), who use plausibly excludable IVs by interacting a time-variant variable with a country-variant variable.[18] The recipient country's probability of receiving aid is clearly endogenous, but the interaction with an exogenous variable – in our case, government fractionalization – is exogenous when controlling for the levels of the interaction term through year- and country-fixed effects (Nunn and Qian 2014, Bun and Harrison 2014).[19] We deal with the

[18] Werker et al. (2009) examine the effect of aid disbursements from Arab donors – which are induced by changes in oil prices – on growth in Muslim recipient countries. Nunn and Qian (2014) identify the effects of food aid induced by changes in US wheat production on conflict in the recipient country, and Ahmed (2016) investigates the effect of US aid induced by changes in US legislature fractionalization on repression in the recipient country.

[19] In a robustness test we replace the time-invariant probability of receiving aid with a time-varying measure. In order to do so, we compute the probability at four-year periods. In that case we have to control for the level of the time-varying probability, as it is no longer captured by year-fixed effects. Results at the different stages (zero-, first- and second-stage) of the regression model remain robust.

endogenous level of the interaction term (the time-invariant probability of receiving aid) by including donor-recipient-fixed effects in the zero-stage regression. As in Nunn and Qian (2014) the technique resembles a difference-in-difference approach, where we compare the effect of aid on military expenditure in regular and irregular recipients of aid when donor government fractionalization changes. The exclusion restriction would be violated if we omitted a variable that is correlated with donor government fractionalization and that affects military expenditure differently in regular and irregular recipient countries after having controlled for the covariates, the levels of the interaction term, country- and year-fixed effects. Indeed, military aid is likely to be affected by the instrument analogously to development aid. Since data on military aid is only available for the United States, we can only control for a fraction of military aid by all DAC donors. The results remain robust to the inclusion of military aid and the power of the instrument remains high. Apart from military aid, we are not aware of other variables that are likely to violate the exclusion restriction.[20]

[20] See Dreher and Langlotz (2017) for a detailed discussion on the choice of the IV, its excludability, and the channels from government fractionalization to bilateral aid. The authors provide empirical testings of the hypotheses on the channel from government fractionalization to bilateral aid and also discuss potential channels that could violate the exclusion restriction. Despite having another dependent variable, the tested channels of trade and economic freedom are also relevant in our case. If changes in donor fractionalization affected military spending in the recipient country through those channels as well – with a differential effect according to the recipient's probability of receiving aid – our exclusion restriction would be violated. The authors do not find evidence for such an effect through the tested channels.

Following Dreher and Langlotz (2017), we argue that our Local Average Treatment Effect (LATE) is representative for the effects of overall bilateral aid provided by a broad set of donor countries to a broad set of recipient countries. We do not expect that aid induced by changes in donor government fractionalization affects military expenditure in recipient countries differently than aid in general.

Main results

Panel A of Table 1 shows the results with the OLS estimates for comparison using contemporaneous aid in columns 1 to 3 and lagged aid in columns 4 to 6.[21] In three out of six regressions, aid is not statistically significant at conventional levels when estimating the panel data model by OLS (including country- and year-fixed effects). However, in columns 2, 3 and 5 there is a slightly negative effect, statistically significant at the 10- to 5-percent-level. A one-percentage point increase in the aid to GDP ratio is associated with a 0.011- to 0.015-percentage point decrease in the share of military expenditures to GDP. For the average country in our sample this represents a change in military expenditure of less than one percent. Economically speaking, the OLS findings point to a zero and if at all to a negative effect of aid on military spending, which is negligibly small.

Panel B of Table 1 shows the coefficient estimates of the instrumented aid variable of the second-stage, Panel C presents the first-stage results, and Panel D provides the reduced form estimates. Before turning to the second-stage results at the recipient-level, it is important to note that the bilateral instrument is statistically significant at the 1-percent-level in the bilateral zero-

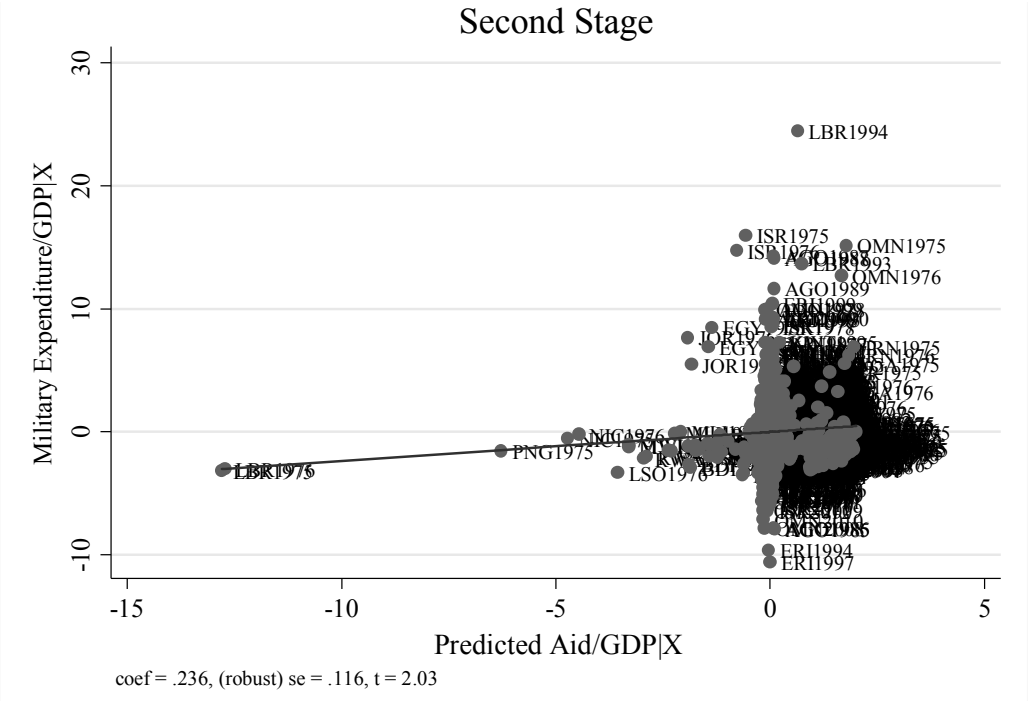
[21] We do not show the covariates in the tables in order to reduce clutter. Full regression results for our main specifications (Table 1) are presented in Tables 14 and 15.

stage regression. A change of donor government fractionalization from 0 to 1 gives rise to an increase of 0.20-percentage points in bilateral aid (as a share of GDP) in regular recipient countries. The effect reduces to 0.1-percentage points for recipient countries that receive positive amounts of aid in only 50 percent of the years. With an average bilateral aid to GDP ratio of 0.14 percent the effect is large. We aggregate the fitted bilateral aid to GDP over all donors and use this exogenous source of variation to predict the causal effect of aid on military expenditure in Table 1, Panel B. The aid variable is statistically significant at the 5-percent-level in all six specifications. The IV results indicate that aid increases military expenditure. The coefficient estimate varies between 0.05 and 0.24, generally with smaller effects when aid is lagged by one year. For the average recipient country, a one-percentage point increase in the aid to GDP ratio amounts to a maximum increase in military expenditure by 8 percent (Table 1, Panel B, column 1). Put differently, a one standard deviation change in aid as a share of GDP gives rise to about 40 percent of a standard deviation change in military expenditure as a share of GDP. However, a one-percentage point increase in aid to GDP occurs only in about 14 percent of the cases in our sample and the average yearly change is 0.1-percentage points. This points to a rather small effect of aid on military expenditure in the overall sample.

The reduced form estimates in Panel D show that our constructed instrument (Fitted Aid/GDP) has a significantly positive effect on military spending in all columns of Table 1. Moreover, the results of the first-stage and the diagnostic tests indicate that this IV is strong. The Kleibergen-Paap first-stage F-statistics (reported in Panel B) are clearly above Staiger and Stock's (1997) rule-of-thumb threshold of ten. The Kleibergen-Paap LM-statistic rejects the Null hypothesis that the equation is under-identified.

Interestingly, the partial leverage plot in Figure 1 shows that the results in Panel B are likely to be driven by one major outlying country, which is Liberia. Over the 1975-2012 period Liberia has experienced many years of violence during its first (1989-1997) and second civil war (1999-2003). Moreover, bilateral aid disbursements have been volatile in Liberia. In 1994, for instance, Liberia spent about 30 percent of its GDP on the military with its aid to GDP ratio amounting to 27 percent in the same year (for a more detailed discussion on this outlying country see Appendix C: Case study on Liberia). We therefore run the main regressions excluding Liberia. The coefficient estimates of development aid remain positive in all columns, but lack statistical significance. Table 2 shows the results of the same specifications as in Table 1 when excluding Liberia. The first-stage coefficients remain similar to those in Table 1 and the Kleibergen-Paap F-statistics remain large. When excluding Liberia we find no evidence that aid affects military expenditure in the overall sample.

Figure 1: ODA/GDP and military expenditure/GDP, Table 1, Panel B, column 1



Robustness

We submit our results to rigorous robustness, Liberia is always excluded.[22] In a first robustness test, we replace ODA with a measure of total flows consisting of ODA and Other Official Flows (OOF).[23] Contrary to ODA, OOF also includes flows by the official sector with a grant element of less than 25 percent, or flows that are not primarily aimed at development. Table B1 shows that the results remain robust to the inclusion of OOF.[24] F-statistics clearly remain above the critical value of ten in all specifications. The effects when using ODA and OOF are similar to those reported in Tables 1 and 2 when using ODA only. Similarly to the results in Table 1, we find that Liberia significantly drives the positive effects. Excluding Liberia from the regression analysis renders the coefficient of aid to lack statistical significance as in Table 2.

In Table B2 we run regressions excluding any covariates apart from recipient- and year-fixed effects. We do so to avoid any concerns about 'bad controls' as our covariates may themselves be potential outcomes of development aid. We report results for contemporaneous (odd

[22] Results including Liberia are available upon request. In Tables B2, B8 and B9 where we report results without covariates, we show estimates for both samples, including and excluding Liberia.

[23] In order to predict the total flows (ODA+OOF) in the first-stage regression, we replace the probability of receiving ODA with the probability of receiving either ODA and/or OOF.

[24] Results for arms imports (Table B7) remain robust to using total flows (ODA+OOF) as an explanatory variable.

column numbers) and lagged aid (even column numbers) and for including (columns 1 and 2) and excluding Liberia (columns 3 and 4). Table B2 shows that the results remain robust.[25]

In Tables B3 and B4 we add further covariates that have been used in the literature: the log of population, government ideology, conscription and the presence of an active force. Inferences don not change when we include these additional covariates.

In Table B5 we control for US military aid, which may be one major omitted variable. What is more, the exclusion restriction might be violated because our instrumental variable might also affect the dependent variable through military aid. Our results at the zero-, first-, and second-stage remain, however, robust to the results presented in Tables 1 and 2.

In another robustness test in Table B6 we replace the Polity IV democracy variable with Cheibub et al.'s (2010) democracy indicator as data availability differs for the two indices. We use the extended version of this dataset by Bjørnskov and Rode (2019). Results remain robust, with the significantly positive effect being driven by Liberia only.

Lastly, in Table B7 we use arms imports as an alternative dependent variable that captures the size of military activity in a recipient country. The OLS results point to an increasing effect of ODA on arms imports, significant at the 5- or 10-percent-level. However, the effects lack statistical significance when using the IV approach.

[25] We also estimate the specifications shown in Tables 1 and 2 by using predetermined covariates, i.e. we lag covariates by one year for contemporaneous aid and by two years for lagged aid. Our results remain robust to this change.

Heterogeneous effects

Aid might affect military expenditure differently across recipient countries with different policies and institutions and across different types of donors. The degree to which aid is fungible and the need to subsidize the military might depend on recipient and donor countries' characteristics. We therefore investigate whether the effect of development aid on military expenditure differs in democracies versus autocracies, conflict-ridden versus peaceful countries, countries' corruption levels, low versus middle and high income countries, the extent to which countries depend on aid, and across different types of donor countries. To investigate heterogeneous effects according to these categories, we employ the control function approach (CFA) (Wooldridge 2015). This approach has a number of advantages. First, to the extent that we control for the endogeneity of aid by including the predicted error from the first-stage regression, there is no need to include a separate instrument for the interaction of Aid/GDP with the additional variable.[26] This makes the approach more efficient than including the interaction of our instrument with the additional variable as a second instrument in the 2SLS setting. Second, contrary to sample split analyses, we do not suffer from a reduction in the sample size and we can compare results across the same sample. Third, there is no need to split the sample at arbitrary levels of the additional variables of

[26] This is based on the assumption that we can consider an interaction between one exogenous and one endogenous variable as exogenous when controlling for the levels of the interaction term (see again, Nunn and Qian 2014, Bun and Harrison 2014). Because of the inclusion of the predicted error term from the first-stage regression, Aid/GDP can now be regarded as the exogenous part of the interaction term in the CFA. This does, however, only allow us to interpret causal effects based on the derivate with respect to Aid/GDP, not with respect to the additional variables like conflict and political institutions because these are likely to be endogenous with respect to the outcome.

interest in cases where there is no clear cut-off value. Given that we identified Liberia as an extreme outlier, we do not include it in the heterogeneity tests.

Political institutions. Kono and Montinola (2012) argue that foreign aid increases military expenditure in autocratic recipient countries. Descriptive statistics indicate that the share of military expenditure to GDP is almost twice as large in autocratic compared to democratic countries over the 1975-2012 period in our sample of 124 recipient countries.[27] To investigate heterogeneous effects of aid on military expenditure in democracies and non-democracies, we measure democracy by the Polity IV index, the democracy-dictatorship data by Cheibub et al. (2010) and extended by Bjørnskov and Rode (2019) and the constraints on the chief executive measure of Polity IV (Tables 3, 4 and 5). We include the interaction term between aid and a variable measuring political institutions. When we measure political institutions by the Polity IV index, the aid variable and the interaction term between aid and the democracy variable do not turn out to be statistically significant.[28] We have computed marginal effects that do not suggest that aid influences military expenditure, the level of political institutions notwithstanding. Inferences are very similar when we measure political institutions as proposed by Cheibub et al. (2010) and extended by Bjørnskov and Rode (2019) and the constraints on the chief executive measure. The

[27] We use the proposed thresholds of the Polity IV index of larger than/equal to 6 for democratic, smaller than/equal to -6 for autocratic countries, and values in-between for anocracies.

[28] This remains robust to defining democracy as a dummy variable with countries classified to be autocratic with a Polity IV index of smaller than or equal to -6 rather than including both, autocracies and so-called anocracies.

interaction term between the aid and democracy variable is positive and statistically significant at the 10% level in column 1 of Tables 4 and 5, but the marginal effects of aid on military expenditure lack statistical significance, in democracies and dictatorships. Thus, our results do not confirm Kono and Montinola's (2012) findings.[29]

Aid dependence. The effect of aid on military expenditure may well depend on the extent to which a recipient country depends on aid. Aid dependence is measured by ODA Net Total disbursements of all DAC donors as a percentage of the recipient's general government final consumption expenditure. We expect the effect of aid on military expenditure to be larger in aid-dependent recipient countries than in countries less dependent on aid. Recipient countries that depend on aid have been described to have low quality of governance, as measured by indices of bureaucratic quality, corruption, and the rule of law (e.g., Knack 2001). Diverting aid to the military is expected to be easier in countries with low quality of governance and rulers may be inclined to divert aid to the military to gratify the preferences of political elites or to prevent riots. We include the aid dependence variable and its interaction term with aid in the regression. The results in Table 6 suggest that aid increases military expenditure in aid-dependent recipient countries. While the marginal effect is significant for any level of aid dependence in columns 1 and 4, it is statistically

[29] In the working paper version, we did not use the CFA, but sample splits. Splitting the sample in democracies and autocracies suggested that aid increased military expenditure in autocracies, depending on how we measured political institutions. At first sight, this result seemed to be in line with Kono and Montinola (2012). However, inferences in the sample of democratic countries were also driven by an outlier, which was Israel.

significant in columns 2 and 5 only for highly aid-dependent countries, where aid exceeds the recipient's general government final consumption expenditures.[30]

Conflicts, corruption and low-income countries. We examine whether the effect of aid on military expenditure depends on the recipient country being involved in a domestic or interstate conflict as these countries generally have higher military expenditure than countries not exposed to armed conflict (Collier and Hoeffler 2007, Dunne et al. 2008). In particular, the threat of international wars increases military expenditure according to Nordhaus et al. (2012). We would expect that countries involved in conflicts are more prone to divert aid to the military than countries at peace. The results suggest that aid is somewhat more likely to be diverted to the military in conflict-ridden countries than in countries at peace. The point estimates of the aid variable and the interaction term between aid and conflict individually lack statistical significance in Table 7, but they are jointly significant in columns 1, 4 and 5. The marginal effects of aid in column 4 are, for example, statistically significant when the conflict variable assumes a value of one.

Secondly, we examine whether the effect of aid on military expenditure depends on corruption as measured by the ICRG corruption index. Following Gupta et al. (2001) corrupt recipients are likely to spend a higher share of their GDP on military expenditure than less corrupt countries. Military affairs are usually dealt with in secrecy, therefore making corruption difficult

[30] In column 3 the marginal effect lacks statistical significance and in column 6 it is statistically significant at very extreme values of aid dependence, which are only represented by three observations.

to reveal.[31] That is why both foreign and domestic firms may try to bribe governments in recipient countries. We can thus expect development aid to subsidize the military to a higher extent in corrupt countries than in less corrupt countries. However, we do not find evidence for a differential effect according to the level of corruption (Table 8).

Thirdly, we investigate whether low-income compared to middle- or high-income recipient countries have a different likelihood in diverting aid to the military. One may well expect that high-income countries (which are often democracies) are less likely to divert aid to the military. However, previous studies have arrived at different conclusions regarding the association between income and military expenditure. GDP per capita is positively correlated with military expenditure in large sample studies (Collier and Hoeffler 2007, Nordhaus et al. 2012), but negatively correlated in a smaller sample of developing countries (Dunne et al. 2008). Again, after controlling for the major outliers, the effects of aid on military expenditure do not differ in low- compared to middle- and high-income countries (Table).[32]

Donor types. We follow Dietrich (2016) in examining heterogeneous effects across different types of donor countries. Donor countries have been found to differ substantially in the types of aid they give and the channels they use for aid delivery. Some donor countries prefer to bypass aid through non-state actors as non-governmental organizations (NGOs) or international organizations, while other donor countries prefer government-to-government aid. Dietrich (2016) argues that the extent

[31] See Gupta et al. (2001) for a more detailed discussion.

[32] We have used the country classification of the World Bank to split the recipients into low- and middle/high-income countries.

to which donors bypass aid depends on "different national orientations about the appropriate role of the state in public service delivery" (p. 65). She classifies the United States, the United Kingdom, Ireland, Australia, New Zealand, Canada, and the Scandinavian countries as liberal market economies (LME).[33] On the contrary, France, Germany, Japan, South Korea, Austria, Netherlands, Switzerland, and Belgium are classified as coordinated market economies (CME). According to Dietrich (2016) LMEs provide larger shares of bypass aid than CMEs.

We use this distinction and split the sample at the bilateral level according to the two donor types. We estimate the effect of aid on military expenditure for LME and CME donors (Tables 10 and 11). Our instrument remains relevant for both donor groups showing that the first stage results are not driven by an individual donor or by specific groups of donors. Interestingly, we find that aid from CMEs has a significantly positive effect on military expenditure, even after excluding Liberia. The results on contemporaneous effects do not depend on individual observations (columns 1 to 3 in Table 11).[34] The numerical meaning of the estimated effects is that when the aid to GDP ratio increases by one-percentage point, military expenditure as a share of GDP increases by about 0.26 to 0.98-percentage points (in Table 11). In columns 5 and 6, however, where we measure aid in year $t-1$, results lack statistical significance at the 5-percent-level. For the LMEs we do not find a positive effect of aid on military expenditure, the inclusion and exclusion of Liberia notwithstanding. We examine whether the Scandinavian donors, who are

[33] Dietrich (2016) does not include Iceland in her analysis. We include Iceland in the group of Scandinavian donors and thus in the liberal market economy category.

[34] We also excluded Angola, Eritrea, and Oman, which are likely to be the most influential countries according to the partial leverage plot, and the effects still remain significantly positive.

often described as being good donors especially in comparison to the Anglo-American donor countries, drive the results for the LMEs. Results for the LME group remain robust to the exclusion of the Scandinavian donors. It is conceivable that aid from CME donors increases military expenditure but aid from LME donors does not influence military expenditure because LMEs and CMEs differ in their relative provision of bypass aid as put forward by Dietrich (2016). As the LME donor countries are supposed to deliver higher shares of aid through non-state actors, aid is less likely to be captured by the government for the purpose of financing the military.[35]

An alternative to distinguish between CME and LME donor countries is using actual shares of bypass aid to total aid. By using OECD data on bypass aid we measure the average share of bypass aid to total aid at the donor-level and classify donors according to the sample median.[36]

[35] In Tables B8 and B9 we report results on heterogeneous effects across the two groups of donor countries (LME versus CME) when we exclude covariates. Table B8 reports results for LME and Table B9 for CME donor countries. We report results for contemporaneous (odd columns numbers) and lagged aid (even column numbers) and for including (columns 1 and 2) and excluding Liberia (columns 3 and 4). Our main findings remain robust to this change.

[36] OECD Creditor Reporting System (CRS) data only reports the delivery channels since 1995, which would reduce our sample by about 50 percent. We therefore use the average over the years 1995-2012 and extrapolate this average to the previous years. This also allows us to compare these findings to the main results and the results based on the CME and LME donor classification over the same period. The sample of donor countries based on the bypass aid data covers more donors than the sample by Dietrich (2016) classifying CME and LME countries. We also do not arrive at a perfect match of LME countries and countries with high shares of bypass aid and CME countries

One group of donors channels more than the median share through non-governmental organizations and the other channels less than the median through non-governmental organizations. The results in Tables 12 and 13 do not suggest that the effect of aid on military expenditure depends on the share of bypass aid. However, the direction of the effects is in line with our results using Dietrich's (2016) classification. The coefficient estimate of aid from donors that are providing on average high shares of bypass aid (which according to Dietrich is more frequently the case in LME countries) is negative.[37] For the group providing low shares of bypass aid, which is more often the case in CME countries, the coefficient estimate is positive.

Conclusion

Many empirical studies suggest that aid is not effective, for example, in increasing economic growth or supporting governance (e.g., Collier and Hoeffler 2004, Dalgaard et al. 2004, Doucouliagos and Paldam 2008, 2011, Arndt et al. 2015). A prime example is that governments in recipient countries use aid to finance military expenditure, rather than education, infrastructure, public health etc. Previous studies examined the extent to which development aid is fungible. By examining the effect of aid on military expenditure, we elaborate on whether aid is fungible, because subsidizing the military is not the intended purpose of ODA. We have used a new IV strategy and new data on military expenditure for 124 recipient countries over the 1975-2012

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and countries with a low share of bypass aid when we split the two types of donor countries according to the sample median.

[37] The F-statistic on the excluded instrument is, however, quite small when we focus on aid from donors with a high share of bypass aid.

period. The instrument is the interaction of donor government fractionalization and the probability of receiving aid, an IV that was proposed by Dreher and Langlotz (2017).

The results indicate that development aid increases military expenditure in the full sample, at the 5-percent-level of significance. Inferences for the overall sample depend, however, on one outlying country: Liberia. When excluding Liberia, the coefficient estimates of the instrumented development aid variable remain positive, but do not reach statistical significance. We have also used arms imports as a dependent variable. When estimating the model by OLS, the results indicate that development aid and arms imports are positively correlated. By contrast, when estimating the model by 2SLS, development aid does not turn out to be statistically significant. These results suggest that using our new IV strategy to identify the causal effects of development aid is useful in avoiding inferences that are likely to be based on biased and inconsistent estimates such as OLS estimates.

The effect of aid on military expenditure varies across characteristics of recipient and donor countries. For example, aid increases military expenditure in countries that depend on aid and are prone to conflicts. Investigating heterogeneous effects based on political institutions, corruption, and income in the recipient country does not suggest that aid is prone to increase the military's budget in either of these groups. We have examined the effect of aid on military expenditure conditional on donor types as proposed by Dietrich (2016). According to Dietrich (2016) liberal market economies (LMEs: e.g., the United States, the United Kingdom, Australia) provide much larger shares of bypass aid than coordinated market economies (CMEs: e.g., France, Germany, Japan). We therefore believe that aid from LME donor countries is less likely to be captured by the government for the purpose of financing the military as they are supposed to deliver higher

shares of aid through non-state actors. Our results show that aid provided by CMEs increases military expenditure, an effect being robust to excluding outlying countries.

Since individual types of aid – including aid from specific donors, aid through specific delivery channels, and aid to specific recipients – are likely to affect military expenditure in different ways, future research should investigate which individual types, groups of countries, and delivery channels of aid are more likely to be linked to a diversion to the military; the implication being that donors should grant any of the types identified with greater caution. Scholars who examine whether development aid is fungible in ways other than by diversion to the recipient's military and scholars examining aid effectiveness would be advised to consider specific types and delivery channels of aid as well as specific groups of donor countries.

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Table 1 Main results

	(1)	(2)	(3)	(4)	(5)	(6)
			A. OLS			
Aid/GDP	-0.015	-0.011**	-0.011**	-0.024	-0.015*	-0.014
	(0.014)	(0.005)	(0.005)	(0.018)	(0.008)	(0.009)
Adj. R-squared	0.167	0.688	0.686	0.180	0.683	0.673
			B. Second-Stage			
Aid/GDP	0.236**	0.120**	0.082***	0.200**	0.055**	0.053**
	(0.116)	(0.047)	(0.028)	(0.093)	(0.024)	(0.022)
Kleibergen-Paap F stat.	181.713	147.137	151.940	209.237	199.621	282.852
K-P LM stat. p-val.	0.039	0.056	0.070	0.041	0.044	0.054
			C. First-Stage			
Fitted Aid/GDP	1.132***	1.121***	1.191***	1.207***	1.224***	1.305***
	(0.084)	(0.092)	(0.097)	(0.083)	(0.087)	(0.078)
			D. Reduced Form			
Fitted Aid/GDP	0.267**	0.134**	0.097***	0.241**	0.068**	0.069**
	(0.129)	(0.052)	(0.034)	(0.110)	(0.029)	(0.029)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3466	3360	2957	3282	3282	2894
No. of Countries	124	124	109	123	123	109

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. All first- and second-stage regressions include as control variables: log of GDP/capita, Polity IV, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table 2 Main results, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	0.266	0.142	0.060	0.223	0.049	0.045
	(0.229)	(0.098)	(0.070)	(0.182)	(0.041)	(0.040)
	B. First-Stage					
Fitted Aid/GDP	1.061***	1.040***	1.137***	1.126***	1.131***	1.233***
	(0.115)	(0.134)	(0.137)	(0.117)	(0.117)	(0.109)
Kleibergen-Paap F stat.	85.052	60.718	69.272	92.129	93.595	128.770
K-P LM stat. p-val.	0.000	0.000	0.000	0.000	0.000	0.000
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. All first- and second-stage regressions include as control variables: log of GDP/capita, Polity IV, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Political institutions

Table 3 Heterogeneous effects: Polity IV, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.241	0.108	0.045	0.207	0.038	0.034
	(0.183)	(0.075)	(0.060)	(0.150)	(0.032)	(0.034)
Aid/GDP*Polity IV	0.003	0.001	0.001	0.003	0.000	0.000
	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged Dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108
P-val. joint sig.	0.490	0.414	0.519	0.609	0.913	0.919

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications (over both stages of the CFA) are in parentheses (clustered at the recipient country level). Significance levels: * 0.10, ** 0.05, *** 0.01. P-val. joint sig. describes the p-value of an F-Test testing whether Aid/GDP and Aid/GDP*Polity IV are jointly statistically significant.

Table 4 Heterogeneous effects: Democratic versus autocratic countries, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.202	0.099	0.036	0.172	0.032	0.031
	(0.189)	(0.076)	(0.061)	(0.155)	(0.032)	(0.034)
Aid/GDP*Democracy	0.054*	0.010	0.013	0.048	0.007	0.007
	(0.031)	(0.010)	(0.012)	(0.031)	(0.011)	(0.012)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged Dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3434	3331	2932	3254	3254	2870
No. of Countries	122	122	108	121	121	108
P-val. joint sig.	0.100	0.504	0.528	0.140	0.582	0.738

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications (over both stages of the CFA) are in parentheses (clustered at the recipient country level). Democracy is defined according to Cheibub et al. (2010), updated by Bjørnskov and Rode and (2018). Significance levels: * 0.10, ** 0.05, *** 0.01. P-val. joint sig. describes the p-value of an F-Test testing whether Aid/GDP and Aid/GDP*Democracy are jointly statistically significant.

Table 5 Heterogeneous effects: Constraints on the executive, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.124	0.071	0.001	0.100	0.012	0.010
	(0.192)	(0.074)	(0.056)	(0.161)	(0.034)	(0.036)
Aid/GDP* Constraints	0.016*	0.003	0.003	0.015	0.003	0.003
on the Executive	(0.009)	(0.003)	(0.003)	(0.010)	(0.003)	(0.003)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged Dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3250	3156	2762	3083	3083	2703
No. of Countries	119	119	105	118	118	105
P-val. joint sig.	0.225	0.558	0.590	0.326	0.634	0.610

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications (over both stages of the CFA) are in parentheses (clustered at the recipient country level). Significance levels: * 0.10, ** 0.05, *** 0.01. P-val. joint sig. describes the p-value of an F-Test testing whether Aid/GDP and Aid/GDP*Constraints on the Executive are jointly statistically significant.

Aid dependence

Table 6 Heterogeneous effects: Aid dependence, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.342*	0.121	0.052	0.298**	0.051	0.046
	(0.177)	(0.079)	(0.062)	(0.144)	(0.035)	(0.039)
Aid/GDP* Aid	0.022**	0.006	0.006	0.021**	0.005*	0.005
Dependence	(0.011)	(0.007)	(0.007)	(0.009)	(0.003)	(0.003)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged Dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3298	3206	2836	3145	3145	2787
No. of Countries	123	123	108	122	122	108
P-val. joint sig.	0.037	0.281	0.307	0.053	0.164	0.304

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications (over both stages of the CFA) are in parentheses (clustered at the recipient country level). Aid dependence is defined as ODA Net Total disbursements of all DAC donors as a percentage of recipient general government final consumption expenditure. Significance levels: * 0.10, ** 0.05, *** 0.01. P-val. joint sig. describes the p-value of an F-Test testing whether Aid/GDP and Aid/GDP*Aid Dependence are jointly statistically significant.

Conflicts, corruption and low-income countries

Table 7 Heterogeneous effects: Conflict, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.225	0.100	0.037	0.187	0.031	0.028
	(0.184)	(0.076)	(0.059)	(0.149)	(0.032)	(0.033)
Aid/GDP* Conflict	0.063	0.015	0.016	0.076	0.028	0.027
	(0.046)	(0.023)	(0.024)	(0.052)	(0.020)	(0.020)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged Dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108
P-val. joint sig.	0.008	0.231	0.264	0.008	0.066	0.101

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications (over both stages of the CFA) are in parentheses (clustered at the recipient country level). Significance levels: * 0.10, ** 0.05, *** 0.01. P-val. joint sig. describes the p-value of an F-Test testing whether Aid/GDP and Aid/GDP*Conflict are jointly statistically significant.

Table 8 Heterogeneous effects: Corruption, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.166	0.100	0.008	0.168	0.043	0.028
	(0.274)	(0.090)	(0.082)	(0.233)	(0.045)	(0.048)
Aid/GDP* Corruption	0.004	-0.008	-0.003	-0.008	-0.008	-0.007
	(0.036)	(0.008)	(0.009)	(0.036)	(0.010)	(0.012)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged Dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	2881	2809	2516	2740	2740	2459
No. of Countries	97	97	87	96	96	87
P-val. joint sig.	0.907	0.356	0.710	0.814	0.407	0.558

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications (over both stages of the CFA) are in parentheses (clustered at the recipient country level). Due to limited data availability we take the mean corruption value of each country rather than the time-varying values. Significance levels: * 0.10, ** 0.05, *** 0.01. P-val. joint sig. describes the p-value of an F-Test testing whether Aid/GDP and Aid/GDP*Corruption are jointly statistically significant.

Table 9 Heterogeneous effects: Low-income countries, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.300	0.113	0.051	0.254	0.041	0.037
	(0.221)	(0.074)	(0.063)	(0.206)	(0.058)	(0.062)
Aid/GDP* Low Income	-0.053	-0.006	-0.008	-0.042	-0.003	-0.001
	(0.176)	(0.027)	(0.030)	(0.180)	(0.052)	(0.055)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged Dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108
P-val. joint sig.	0.763	0.824	0.797	0.815	0.961	0.991

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Pairs cluster bootstrap standard errors with 500 replications (over both stages of the CFA) are in parentheses (clustered at the recipient country level). Significance levels: * 0.10, ** 0.05, *** 0.01. P-val. joint sig. describes the p-value of an F-Test testing whether Aid/GDP and Aid/GDP*Low Income are jointly statistically significant.

Donor types

Table 10 Heterogeneous effects: Liberal market economies (LME), excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	-0.344	-0.047	-0.155	-0.264	-0.020	-0.012
	(0.624)	(0.267)	(0.217)	(0.478)	(0.067)	(0.061)
	B. First-Stage					
Fitted Aid/GDP	0.986***	1.078***	1.241***	1.027***	1.037***	1.132***
	(0.111)	(0.179)	(0.209)	(0.116)	(0.123)	(0.131)
Kleibergen-Paap F stat.	79.159	36.285	35.362	78.917	70.740	74.661
K-P LM stat. p-val.	0.016	0.000	0.000	0.016	0.015	0.015
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108

Table 11 Heterogeneous effects: Coordinated market economies (CME), excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	0.982**	0.357**	0.260*	0.856**	0.147*	0.146*
	(0.500)	(0.165)	(0.139)	(0.407)	(0.079)	(0.085)
	B. First-Stage					
Fitted Aid/GDP	1.283***	1.337***	1.443***	1.301***	1.376***	1.498***
	(0.157)	(0.164)	(0.213)	(0.150)	(0.155)	(0.189)
Kleibergen-Paap F stat.	66.639	66.117	45.827	75.615	79.211	62.775
K-P LM stat. p-val.	0.000	0.000	0.000	0.000	0.000	0.000
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108

Notes for Tables 10 and 11: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. All first- and second-stage regressions include as control variables: log of GDP/capita, Polity IV, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level). Donor countries are classified as LME or CME countries according to Dietrich (2016). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table 12 Heterogeneous effects: High bypass aid share, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	-3.152	-0.513	-0.584	-2.996	-0.531	-0.669
	(2.217)	(0.397)	(0.437)	(2.006)	(0.404)	(0.469)
	B. First-Stage					
Fitted Aid/GDP	2.852**	3.019**	3.555**	2.843**	2.856**	3.182**
	(1.144)	(1.191)	(1.451)	(1.124)	(1.127)	(1.321)
Kleibergen-Paap F stat.	6.211	6.420	6.005	6.397	6.424	5.807
K-P LM stat. p-val.	0.013	0.012	0.021	0.011	0.011	0.022
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108

Table 13 Heterogeneous effects: Low bypass aid share, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	0.235	0.139	0.052	0.199	0.043	0.040
	(0.244)	(0.105)	(0.078)	(0.195)	(0.043)	(0.042)
	B. First-Stage					
Fitted Aid/GDP	0.987***	0.950***	1.020***	1.037***	1.040***	1.121***
	(0.095)	(0.122)	(0.136)	(0.095)	(0.095)	(0.090)
Kleibergen-Paap F stat.	108.063	60.556	56.056	119.317	121.037	156.610
K-P LM stat. p-val.	0.000	0.000	0.000	0.000	0.000	0.000
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108

Notes for Tables 12 and 13: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. All first- and second-stage regressions include as control variables: log of GDP/capita, polity, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level). Donor countries are classified according to the median of bypass aid share. Significance levels: * 0.10, ** 0.05, *** 0.01.

Table 14 Second-stage full regressions, IV

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.236**	0.120**	0.082***	0.200**	0.055**	0.053**
	(0.116)	(0.047)	(0.028)	(0.093)	(0.024)	(0.022)
Polity IV	-0.021	-0.014	-0.011	-0.023	-0.013**	-0.011*
	(0.026)	(0.008)	(0.008)	(0.025)	(0.006)	(0.007)
Log GDP per capita	-0.022	0.183	-0.024	-0.273	-0.121	-0.204
	(0.734)	(0.271)	(0.243)	(0.679)	(0.250)	(0.278)
Interstate conflict	1.315*	0.750	0.733	1.311*	0.735	0.739
	(0.713)	(0.553)	(0.549)	(0.733)	(0.552)	(0.554)
Domestic conflict	0.528**	0.103	0.083	0.560**	0.138	0.120
	(0.227)	(0.096)	(0.097)	(0.242)	(0.094)	(0.097)
Military exp./GDP (t-1)		0.792***	0.779***		0.771***	0.762***
		(0.063)	(0.074)		(0.064)	(0.076)
Neighbor ME (t-1)			0.022			0.023
			(0.029)			(0.029)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3466	3360	2957	3282	3282	2894
No. of Countries	124	124	109	123	123	109

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Standard errors are in parentheses (clustered at the recipient-country-level). These are the full 2SLS regression results of Table 1. Significance levels: * 0.10, ** 0.05, *** 0.01.

Table 15 Second-stage full regressions, OLS

	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	-0.015	-0.011**	-0.011**	-0.024	-0.015*	-0.014
	(0.014)	(0.005)	(0.005)	(0.018)	(0.008)	(0.009)
Polity IV	-0.007	-0.006	-0.006	-0.006	-0.007	-0.006
	(0.022)	(0.005)	(0.005)	(0.021)	(0.005)	(0.005)
Log GDP per capita	-1.107*	-0.381	-0.414	-1.185*	-0.409	-0.459
	(0.603)	(0.282)	(0.338)	(0.661)	(0.305)	(0.358)
Interstate conflict	1.174*	0.672	0.681	1.196*	0.701	0.712
	(0.695)	(0.535)	(0.540)	(0.708)	(0.545)	(0.550)
Domestic conflict	0.513**	0.105	0.088	0.507**	0.124	0.108
	(0.235)	(0.095)	(0.099)	(0.240)	(0.097)	(0.100)
Military exp./GDP (t-1)		0.789***	0.775***		0.767***	0.756***
		(0.063)	(0.074)		(0.064)	(0.075)
Neighbor ME (t-1)			0.028			0.030
			(0.028)			(0.028)
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3466	3360	2957	3282	3282	2894
No. of Countries	124	124	109	123	123	109
Adjusted R-squared	0.167	0.688	0.686	0.180	0.683	0.673

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Standard errors are in parentheses (clustered at the recipient-country-level). These are the full OLS regression results of Table 1. Significance levels: * 0.10, ** 0.05, *** 0.01.

Appendix A: Data source and descriptive statistics

Table A1 Data source

Variable	Description	Data Source
Active Force	Equals 1 if there were standing armed forces for the country.	Toronto (2014)
Aid Dependence	ODA Net Total disbursements of all DAC donors as a percentage of recipient general government final consumption expenditure.	OECD (2014/2015), Table DAC2a, WDI (2015)
Aid/GDP	ODA Net Total disbursements of all DAC donors, current prices (USD) divided by recipient GDP.	OECD (2014/2015), Table DAC2a, WDI (2015)
Arms imports	Arms imports (trend indicator values).	WDI (SIPRI) (2015)
Bypass Aid Share	Bypass Aid defined according to Dietrich (2016) as a share of ODA.	OECD (2019), CRS
Conflict	Dummy 1 if domestic or interstate conflict equals 1.	UCDP/PRIO Armed Conflict Dataset, version 2014 (Gleditsch et al. 2002)
Conscription	Dummy 1 if there is conscription in the country.	Toronto (2014)
Constraints on Executive	Executive Constraints: scale from 1 (unlimited authority) to 7 (executive parity).	Marshall et al. (2015)
Corruption	Corruption, annual averages (ICRG), ranges from 0 to 6: most corrupt (0), least corrupt (6).	ICRG (2012)

Democracy	Democracy index: dummy 1 for democracy.	Cheibub et al. (2010), updated by Bjørnskov and Rode and (2018)
Domestic conflict	Dummy 1 for minor domestic conflict, between 25 and 999 battle-related deaths in a given year.	UCDP/PRIO Armed Conflict Dataset, version 2014 (Gleditsch et al. 2002)
Fitted Aid/GDP	Instrumental variable, constructed from the bilateral zero-stage regression.	Own construction
Fractionalization (Frac)	Probability that two deputies picked at random from among the government parties will be from different parties.	Database of Political Institutions (Beck et al. 2001)
Government ideology (right-wing)	Government ideology with respect to economic policy: Right (1); Left (0); Center (0); No information (missing).	Database of Political Institutions (Beck et al. 2001)
Interstate conflict	Dummy 1 if the recipient country is involved in an interstate conflict.	UCDP/PRIO Armed Conflict Dataset, version 2014 (Gleditsch et al. 2002)
Log GDP per capita	Log of GDP per capita (constant 2005 US$).	WDI (2015)
Log military expenditure	Log of military expenditure constant (2012) USD millions.	SIPRI (2016)
Log population	Log of population total.	WDI (2014)

Low income	Dummy 1 in case of low-income country, zero in case of middle/high income country. Defined according to the World Bank classification.	WDI (2014)
Military expenditure/GDP	Military expenditure (percent of GDP).	SIPRI (2016)
Neighbor ME	Neighbor military expenditure as a share of total neighbor GDP.	WDI (SIPRI) (2016)
(ODA+OOF)/GDP	Total official flows by country (ODA+OOF), current prices (USD) divided by recipient GDP.	OECD (2015), WDI (2015)
Polity IV	Polity IV democracy index, ranges from -10 to 10: Autocracies (-10 to -6); Democracies (6 to 10); Anocracies (-5 to 5).	Polity IV (2015), Marshall et al. (2010)
Probability over all periods	Probability of receiving aid from donor *j* within the whole observation period from 1975-2012.	Own construction based on ODA Data from OECD
US military aid/GDP	US military aid divided by recipient GDP.	US Overseas Loans and Grants Greenbook (2016)

Table A2 Descriptive statistics

Base Regression Sample Table 1	count	mean	sd	min	max
Military expenditure/GDP	3466	3.01	3.36	0.00	34.38
Arms imports	2234	270.80	539.56	0.00	5559.00
Aid/GDP	3466	3.62	5.70	-0.68	99.43
(ODA+OOF)/GDP	3466	3.76	6.08	-3.84	113.64
Neighbor's military exp./neighbor's GDP	3059	3.05	2.91	0.00	42.91
US military aid/GDP	3439	0.20	1.02	0.00	23.93
Polity IV	3466	0.56	6.80	-10.00	10.00
Democracy	3461	0.39	0.49	0.00	1.00
Log GDP per capita	3466	7.27	1.28	3.99	10.97
Interstate conflict	3466	0.02	0.13	0.00	1.00
Domestic conflict	3466	0.19	0.40	0.00	1.00
Conflict	3466	0.20	0.40	0.00	1.00
Corruption	2029	2.62	1.01	0.00	6.00
Constraints on executive	3271	4.06	2.15	1.00	7.00
Aid dependence	3319	0.27	0.49	-0.07	11.33
Bypass aid share	3227	47.89	12.28	24.45	100.00
Log population	3466	16.09	1.53	12.69	21.02
Government ideology (right-wing)	1795	0.34	0.47	0.00	1.00
Active force	2969	0.97	0.17	0.00	1.00
Conscription	3033	0.48	0.50	0.00	1.00

Note: Descriptive statistics are based on the sample of column 1, Table 1.

Table A3 Sample

Recipient Countries			
Albania	Dominican Republic	Laos	Rwanda
Algeria	Ecuador	Lebanon	Saudi Arabia
Angola	Egypt	Lesotho	Senegal
Argentina	El Salvador	Liberia	Serbia
Armenia	Equatorial Guinea	Libya	Sierra Leone
Azerbaijan	Eritrea	Macedonia, FYR	Singapore
Bahrain	Ethiopia	Madagascar	Slovenia
Bangladesh	Fiji	Malawi	South Africa
Belarus	Gabon	Malaysia	Sri Lanka
Benin	Gambia	Mali	Sudan
Bolivia	Georgia	Mauritania	Swaziland
Botswana	Ghana	Mauritius	Syria
Brazil	Guatemala	Mexico	Tajikistan
Burkina Faso	Guinea	Moldova	Tanzania
Burundi	Guinea-Bissau	Mongolia	Thailand
Cambodia	Guyana	Montenegro	Timor-Leste
Cameroon	Haiti	Morocco	Togo
Cape Verde	Honduras	Mozambique	Trinidad and Tobago
Central African Rep.	India	Namibia	Tunisia
Chad	Indonesia	Nepal	Turkey
Chile	Iran	Nicaragua	Turkmenistan
China	Iraq	Niger	Uganda
Colombia	Israel	Nigeria	Ukraine
Congo, Dem. Rep.	Jamaica	Oman	United Arab Emirates
Congo, Rep.	Jordan	Pakistan	Uruguay
Costa Rica	Kazakhstan	Panama	Uzbekistan
Cote d'Ivoire	Kenya	Papua New Guinea	Venezuela
Croatia	Korea	Paraguay	Vietnam
Cuba	Kosovo	Peru	Yemen
Cyprus	Kuwait	Philippines	Zambia
Djibouti	Kyrgyz Republic	Qatar	Zimbabwe

Donor Countries			
Australia	France	Korea	Slovak Republic
Austria	Germany	Luxembourg	Slovenia
Belgium	Greece	Netherlands	Spain
Canada	Iceland	New Zealand	Sweden
Czech Republic	Ireland	Norway	Switzerland
Denmark	Italy	Poland	United Kingdom
Finland	Japan	Portugal	United States

Appendix B: Robustness

Table B1 ODA and OOF, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	0.203	0.111	0.031	0.170	0.033	0.030
	(0.219)	(0.093)	(0.066)	(0.178)	(0.038)	(0.036)
	B. First-Stage					
Fitted Aid/GDP	1.094***	1.064***	1.130***	1.139***	1.148***	1.219***
	(0.106)	(0.116)	(0.110)	(0.110)	(0.108)	(0.103)
Kleibergen-Paap F stat.	106.305	84.164	105.584	107.112	112.571	139.317
K-P LM stat. p-val.	0.000	0.000	0.001	0.000	0.000	0.001
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3439	3335	2932	3258	3258	2870
No. of Countries	123	123	108	122	122	108

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. All first- and second-stage regressions include as control variables: log of GDP/capita, Polity IV, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table B2 Main results without covariates, including and excluding Liberia

	(1)	(2)	(3)	(4)
	A. Second-Stage			
Aid/GDP	0.234***	0.211***	0.247	0.212
	(0.083)	(0.068)	(0.177)	(0.144)
	B. First-Stage			
Fitted Aid/GDP	1.505***	1.547***	1.346***	1.392***
	(0.102)	(0.102)	(0.125)	(0.142)
Kleibergen-Paap F stat.	218.389	217.280	116.366	96.019
K-P LM stat. p-val.	0.066	0.065	0.000	0.000
Aid lagged?	No	Yes	No	Yes
Liberia excluded?	No	No	Yes	Yes
No. of Observations	3466	3282	3439	3258
No. of Countries	124	123	123	122

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. No covariates included. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table B3 Additional covariates I, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	-0.010	-0.044	-0.103	-0.007	0.014	-0.004
	(0.207)	(0.107)	(0.125)	(0.176)	(0.051)	(0.043)
Polity IV	-0.047	-0.018	-0.016	-0.051	-0.023*	-0.020
	(0.040)	(0.014)	(0.014)	(0.043)	(0.014)	(0.013)
Log GDP per capita	-0.922	-0.296	-0.325	-0.931	-0.144	-0.137
	(1.144)	(0.410)	(0.356)	(1.059)	(0.269)	(0.208)
Log population	-1.148	-0.611	-0.633	-1.588	-0.451	-0.280
	(2.864)	(1.062)	(1.122)	(2.773)	(0.749)	(0.765)
Interstate conflict	0.287	0.138	0.106	0.274	0.170	0.170
	(0.333)	(0.161)	(0.156)	(0.324)	(0.158)	(0.147)
Domestic conflict	0.547*	0.044	0.006	0.474	0.073	0.032
	(0.308)	(0.168)	(0.182)	(0.358)	(0.177)	(0.193)
Government ideology	-0.162	-0.043	-0.068	-0.076	0.005	-0.015
(right-wing)	(0.306)	(0.076)	(0.087)	(0.230)	(0.053)	(0.061)
Military exp./GDP (t-1)		0.736***	0.708***		0.712***	0.685***
		(0.109)	(0.103)		(0.104)	(0.098)
Neighbor ME (t-1)			0.092			0.073
			(0.068)			(0.051)
	B. First-Stage					
Fitted Aid/GDP	0.970***	1.003***	1.113***	0.983***	0.980***	1.099***
	(0.218)	(0.269)	(0.310)	(0.235)	(0.232)	(0.218)
Kleibergen-Paap F stat.	19.874	13.871	12.854	17.441	17.781	25.410
K-P LM stat. p-val.	0.006	0.001	0.009	0.007	0.008	0.015
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	1789	1737	1480	1697	1697	1450
No. of Countries	85	85	75	85	85	75

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table B4 Additional covariates II, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	0.219	0.147	0.080	0.202	0.063	0.068
	(0.240)	(0.121)	(0.082)	(0.191)	(0.047)	(0.043)
Polity IV	-0.021	-0.013	-0.009	-0.024	-0.012	-0.011
	(0.026)	(0.011)	(0.009)	(0.027)	(0.008)	(0.008)
Log GDP per capita	-0.290	0.372	0.229	-0.429	0.053	0.109
	(1.178)	(0.463)	(0.311)	(0.990)	(0.236)	(0.196)
Interstate conflict	-3.055	-0.469	-0.170	-3.196*	-0.736	-0.460
	(1.885)	(0.673)	(0.576)	(1.744)	(0.488)	(0.512)
Domestic conflict	1.337*	0.813	0.786	1.357*	0.806	0.814
	(0.752)	(0.618)	(0.608)	(0.782)	(0.612)	(0.615)
Conscription	0.519**	0.104	0.095	0.554**	0.155	0.141
	(0.246)	(0.121)	(0.117)	(0.272)	(0.114)	(0.117)
Active force	0.937***	0.431**	0.464**	1.012***	0.405*	0.459**
	(0.320)	(0.211)	(0.224)	(0.356)	(0.211)	(0.228)
Military exp./GDP (t-1)	-0.567	-0.005	0.601	-0.724	0.062	0.560
	(0.491)	(0.309)	(0.501)	(0.447)	(0.318)	(0.508)
Neighbor ME (t-1)		0.738***	0.714***		0.712***	0.692***
		(0.065)	(0.071)		(0.065)	(0.070)
	B. First-Stage					
Fitted Aid/GDP	1.139***	1.077***	1.143***	1.190***	1.199***	1.264***
	(0.125)	(0.160)	(0.157)	(0.122)	(0.119)	(0.115)
Kleibergen-Paap F stat.	82.669	45.147	52.947	95.248	101.185	120.835
K-P LM stat. p-val.	0.000	0.000	0.000	0.000	0.000	0.000
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	2946	2857	2523	2788	2788	2466
No. of Countries	121	121	109	120	120	109

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table B5 US military aid, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	0.324	0.147	0.066	0.285	0.063	0.056
	(0.215)	(0.094)	(0.067)	(0.175)	(0.045)	(0.044)
US Military Aid/GDP	0.428**	0.027	0.034	0.487***	0.083*	0.072
	(0.167)	(0.046)	(0.038)	(0.181)	(0.046)	(0.046)
	B. First-Stage					
Fitted Aid/GDP	1.096***	1.092***	1.192***	1.161***	1.175***	1.275***
	(0.121)	(0.143)	(0.146)	(0.122)	(0.122)	(0.113)
Kleibergen-Paap F stat.	81.553	58.047	66.963	89.979	93.044	126.415
K-P LM stat. p-val.	0.000	0.000	0.000	0.000	0.000	0.000
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3412	3309	2906	3233	3233	2845
No. of Countries	120	120	106	120	120	106

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. All first- and second-stage regressions include as control variables: log of GDP/capita, Polity IV, and interstate and domestic conflict, and US military aid/GDP. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table B6 Main results with democracy (Cheibub et al. 2010), excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	0.250	0.140	0.059	0.213	0.047	0.046
	(0.232)	(0.100)	(0.071)	(0.182)	(0.041)	(0.040)
	B. First-Stage					
Fitted Aid/GDP	1.041***	1.014***	1.111***	1.112***	1.117***	1.224***
	(0.120)	(0.135)	(0.138)	(0.121)	(0.121)	(0.115)
Kleibergen-Paap F stat.	75.486	56.671	64.516	84.173	85.514	113.833
K-P LM stat. p-val.	0.000	0.000	0.000	0.000	0.000	0.000
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	3600	3486	3024	3412	3412	2964
No. of Countries	127	127	112	127	127	112

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. All first- and second-stage regressions include as control variables: log of GDP/capita, Cheibub et al.'s democracy index, extended by Bjørnskov and Rode (2019) and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table B7 Arms imports, excluding Liberia

	(1)	(2)	(3)	(4)	(5)	(6)
			A. OLS			
Aid/GDP	7.591*	10.764*	11.808*	13.024**	13.024**	14.552**
	(3.872)	(5.460)	(6.106)	(6.323)	(6.323)	(6.970)
Adj. R-squared	0.050	0.062	0.057	0.061	0.061	0.057
			B. Second-Stage			
Aid/GDP	4.022	4.642	7.304	19.636	15.516	20.197
	(21.407)	(13.105)	(24.906)	(30.188)	(12.534)	(17.589)
			C. First-Stage			
Fitted Aid/GDP	1.622***	1.709***	1.398***	1.632***	1.630***	1.410***
	(0.258)	(0.247)	(0.125)	(0.285)	(0.286)	(0.098)
Kleibergen-Paap F stat.	39.660	47.823	124.685	32.731	32.568	205.169
K-P LM stat. p-val.	0.017	0.043	0.000	0.011	0.011	0.000
Aid lagged?	No	No	No	Yes	Yes	Yes
Lagged dependent?	No	Yes	Yes	No	Yes	Yes
Lagged Neighbor ME	No	No	Yes	No	No	Yes
No. of Observations	2493	2048	1832	1985	1985	1779
No. of Countries	119	109	100	109	109	100

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. All first- and second-stage regressions include as control variables: log of GDP/capita, Polity IV, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level). Significance levels: * 0.10, ** 0.05, *** 0.01.

Table B8 Heterogeneous effects: LME, no covariates, including and excluding Liberia

	(1)	(2)	(3)	(4)
	A. Second-Stage			
Aid/GDP	0.131	0.150	-0.227	-0.170
	(0.230)	(0.193)	(0.527)	(0.413)
	B. First-Stage			
Fitted Aid/GDP	1.326***	1.344***	1.166***	1.188***
	(0.092)	(0.094)	(0.118)	(0.127)
Kleibergen-Paap F stat.	209.987	205.996	98.007	87.069
K-P LM stat. p-val.	0.104	0.102	0.007	0.007
Aid lagged?	No	Yes	No	Yes
Liberia excluded?	No	No	Yes	Yes
No. of Observations	3466	3282	3439	3258
No. of Countries	124	123	123	122

Table B9 Heterogeneous effects: CME, no covariates, including and excluding Liberia

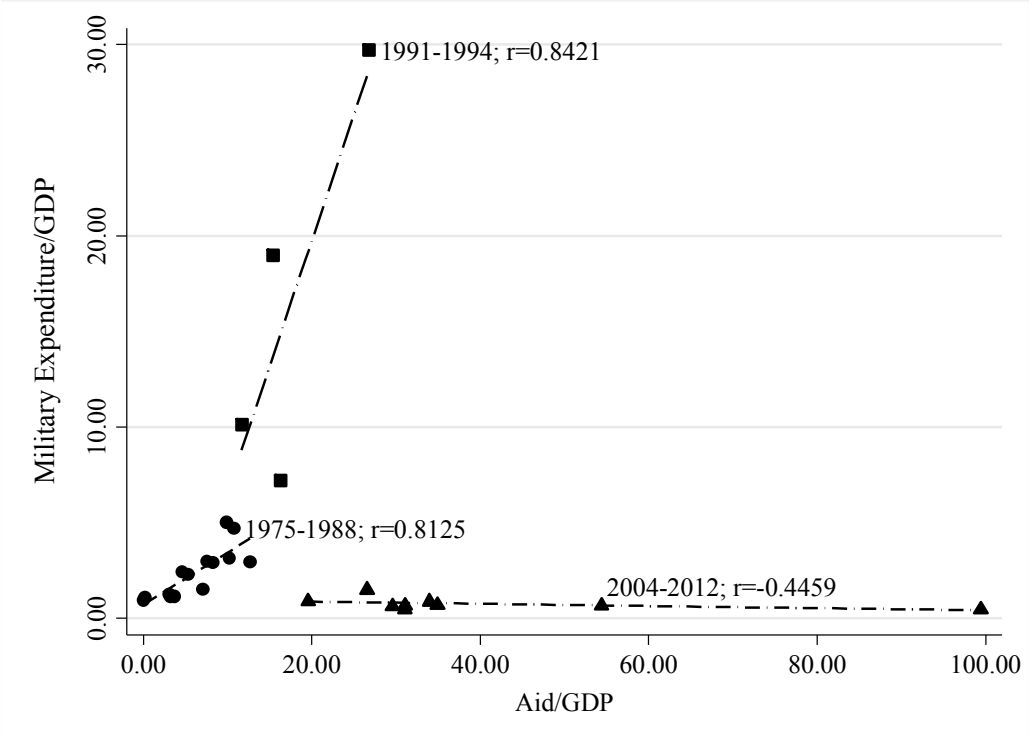
	(1)	(2)	(3)	(4)
	A. Second-Stage			
Aid/GDP	0.896***	0.794***	0.945**	0.821**
	(0.346)	(0.279)	(0.467)	(0.383)
	B. First-Stage			
Fitted Aid/GDP	1.462***	1.501***	1.428***	1.452***
	(0.111)	(0.119)	(0.146)	(0.152)
Kleibergen-Paap F stat.	173.842	160.214	95.319	91.103
K-P LM stat. p-val.	0.001	0.002	0.000	0.000
Aid lagged?	No	Yes	No	Yes
Liberia excluded?	No	No	Yes	Yes
No. of Observations	3466	3282	3439	3258
No. of Countries	124	123	123	122

Notes for Tables B8 and B9: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. Standard errors are in parentheses (clustered at the recipient-country-level). No covariates included. Donor countries are classified as LME or CME countries according to Dietrich (2016). Significance levels: * 0.10, ** 0.05, *** 0.01.

Appendix C: Case study on Liberia

We now discuss Liberia – the most outstanding outlier – in more detail. Liberia has been considered as being a fragile state since April 1979 (see Johnston 2004, Pham 2004, and Werker and Beganovic 2011 for detailed case studies on Liberia). When the government decided to increase the price of rice in spring 1979, citizens rebelled against the government. Samuel Doe, a master sergeant of the Liberian army, and confederates killed the President William Tolbert. Samuel Doe was ruling Liberia in the 1980s. Economic development declined, political instability and violence increased. In 1989 the warlord, Charles Taylor, and confederates rebelled. There was conflict and civil war over the 1989-2003 period, till the United States and Nigeria intervened. For Liberia, our dataset includes observations for the 1975-1988, 1991-1994, and 2004-2012 periods. There are missing values most likely because of the civil wars. Aid and military expenditure were strongly and positively correlated over the 1975-1988 (the correlation coefficient is 0.81) and 1991-1994 periods (the correlation coefficient is 0.84). By contrast, over the 2004-2012 period, the correlation coefficient is -0.45 (Figure 2). We are not aware of verified evidence proving the extent to which and the way in which rulers used ODA to increase military expenditure. However, aid is likely to have been diverted to the military in the 1980s under the government of Samuel Doe. In particular, "Doe looked further for diplomatic and other support" and was encouraged, for example, by left-wing intellectuals to "build closer ties with Libya and the Soviet bloc" (Pham 2004: 88). To prevent closer ties between Liberia and communist countries "U.S. economic and military assistance to Liberia between 1981 and 1985 totalled over $500 million" (Pham 2004: 89).

Figure 2: Correlation between ODA/GDP and military expenditure/GDP per period, Liberia



Notes: r is the correlation coefficient between military expenditure and aid (both as a share of GDP) for the respective period.