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Does development aid increase military expenditure?

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Abstract:

The diversion of development aid to the recipient's military may be one explanation why aid is often found to be ineffective in promoting economic growth and development. Previous studies have not derived the causal effects of development aid on military expenditure. 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. While development aid has a positive effect on military expenditure in the full sample, the effect vanishes when we exclude outliers. However, we find that aid provided by coordinated market economies increases military expenditure in the full sample of recipient countries, even after controlling for outliers. Coordinated market economies have been found to deliver more government-to-government aid, which has a higher risk of capture compared to aid delivered through non-state development actors.

Keywords: aid, military expenditure, fungibility, instrumental variables, causality
JEL codes: F35, H56, O11

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1. 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, Dreher and Langlotz 2015). It is conceivable that aid is ineffective because it is not used for its intended purpose, indicating that aid is fungible. 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."[1] Finding evidence on whether "aid buys guns" therefore helps to explain why aid is often found to be ineffective.

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. (2016) find that extending mid- and long-term arms imports lead 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.

Previous studies examine correlations between aid and military expenditure and provide mixed evidence on the nexus between the two (Cashel-Cordo and Craig 1990, Khilji and Zampelli 1994, Feyzioglu et al. 1998). Two studies have advanced empirical research by using instrumental variables (IV) for aid (Collier and Hoeffler 2007, Kono and Montinola 2012). The results show that aid increases military expenditure, especially in autocracies. We are sceptical, however, as to whether these studies' IVs are excludable and therefore do not believe that their evidence reflects the causal effect of aid on military expenditure.

We apply a new instrumental variable for aid that has been proposed by Dreher and Langlotz (2015) in their study of aid and growth. More specifically, we use the interaction of donor government

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

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 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 2015). We use the resulting excludable IV to identify the causal effects of development aid on 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 by definition 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. Analysing the fungibility of aid implies a focus on development aid, which is given with explicitly non-military purposes.[2] In fact, 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.

We proceed as follows: Section 2 discusses previous studies on the effect of aid on military expenditure. Section 3 describes the data and identification strategy, while Section 4 presents the main results. We discuss robustness tests and heterogeneous effects in Sections 5 and 6. Section 7 concludes.

2. 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.[3] One example for the fungibility of aid is to divert development aid to the military. Some previous studies examined 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).

[2] On the relationship between military and non-military aid see, for example, Deger and Sen (1991).

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

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 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 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.[4]

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

[4] Dube and Naidu (2016) 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 (2016) 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.

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 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 in the course of 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 in the course of 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). 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. 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 in the course of 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 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.[5] 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).

3. Method and data

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

$$Military\ expenditure_{i,t}=\beta_1 Aid_{i,t} + \beta_2 X_{i,t} + \beta_3 \eta_i + \beta_4 \tau_t + \varepsilon_{i,t}, \quad (1)$$

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 Committee (DAC) recipient countries over the 1975-2012 period.[6] $Aid_{i,t}$ $(Aid_{i,t-1})$ is the amount of net ODA as a share of GDP

[5] 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.

[6] 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

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.

In the baseline model, 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 long-run contracts with arms manufacturers, we also investigate results using averaged data over four- and five-year periods.[7]

Following related studies examining determinants of military expenditure in developing countries, all regressions contain a vector of contemporaneous control variables $X_{i,t}$, 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). 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. 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).[8] 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, and gross national expenditure (as a share of GDP). 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

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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.

[7] 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.

[8] Aid may also increase the likelihood of conflict (e.g., Nunn and Qian 2014, Ahmed 2016b). The results of Ahmed (2016b) show, for example, that after the end of the Cold War, US aid increased the likelihood of conflict in "most repressive Cold War regimes."

expenditure in industrialized countries, scholars also use government ideology as an explanatory variable (see, for example, Bove et al. 2016 and Kauder and Potrafke 2016). Rightwing governments are often expected to spend more on the military than leftwing governments.[9] Lastly, countries having higher government expenditure (as a share of GDP) are also likely to spend more on the military (Gupta et al. 2001).[10]

As in Dreher and Langlotz (2015) 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 Dreher and Langlotz's (2015) 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. Following Dreher and Langlotz (2015), 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. Results remain robust when excluding Canada and the United States.[11] Similar to previous studies (Nunn and Qian 2014, Dreher and Langlotz 2015, Ahmed 2016a), 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.

[9] Government ideology is, however, difficult to measure in developing countries and has hardly been examined as an explanatory variable of military expenditure in developing countries. We include government ideology in our robustness tests to avoid potential omitted variable bias.

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

[11] 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.

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 effects of aid on military expenditure.[12] In this step we revert to the usual 2SLS procedure with the only difference being that we first constructed the instrument from the bilateral regression equation (2).[13] The dataset of our baseline specification includes 124 recipient countries over the 1975-2012 period.[14] Standard errors are clustered at the recipient-level.

Dreher and Langlotz (2015) explain in detail why the interaction of government fractionalization with the probability of receiving aid is likely to be a powerful IV. While government fractionalization is likely 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 favored 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 completes the channel from fractionalization to bilateral aid disbursements by showing that higher aid budgets translate into higher bilateral aid disbursements.[15] On the other hand, the probability of receiving aid is likely to influence the extent to

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

[13] 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.

[14] Following Dreher and Langlotz (2015), 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.

[15] See Dreher and Langlotz (2015) for a detailed discussion on the choice of the IV, its excludability, and the channels from government fractionalization to bilateral aid. Dreher and Langlotz (2015) discuss this channel from

which changes in government fractionalization affect bilateral aid disbursements. Nunn and Qian (2014), Dreher and Langlotz (2015), and Ahmed (2016a) 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 (2016a), who use plausibly excludable IVs by interacting a time-variant variable with a country-variant variable.[16] 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). We deal with the endogenous level of the interaction term (the time-invariant probability of receiving aid) by including donor-recipient-fixed effects in the zero-stage regression.[17] In this regard our approach differs from the studies we have criticised for their IV approaches relying on distance or proximity measures. While the probability of receiving aid resembles such a proximity measure, we capture the level of the probability by including fixed effects and we are left with the interaction term only.[18] 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

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government fractionalization to bilateral aid in more detail and provide empirical testing of the hypotheses on the channel.

[16] 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 (2016a) investigates the effect of US aid induced by changes in US legislature fractionalization on repression in the recipient country.

[17] After aggregating over all donors, we are still left with the exogenous variation introduced through the time-varying interaction term only. The levels of the interaction term are captured by the fixed effects at the first and second-stage since donor government fractionalization is the same for all recipients and the probability of receiving aid is still perfectly multicollinear to the country-fixed effects.

[18] 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.

remains high. Apart from military aid, we are not aware of other variables that are likely to violate the exclusion restriction.[19]

Following Dreher and Langlotz (2015), 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.

4. Main results

Panel A of Table 1 shows the results with the OLS estimates for comparison using contemporaneous aid in columns (1)-(3) and lagged aid in columns (4)-(6).[20] 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). This result is in line with Collier and Hoeffler's finding (2007, Table 1, column (3)). However, in columns (2), (3), and (5) there is a slightly negative effect, statistically significant at the 5- to 10-percent-level. A one-percentage point increase in the aid to GDP ratio is associated with a 0.011-0.015-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 provides the reduced form estimates, and Panel D presents the first-stage results. 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-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

[19] See Dreher and Langlotz (2015) for a detailed discussion on 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. Dreher and Langlotz (2015) do not find evidence for such an impact through the tested channels.

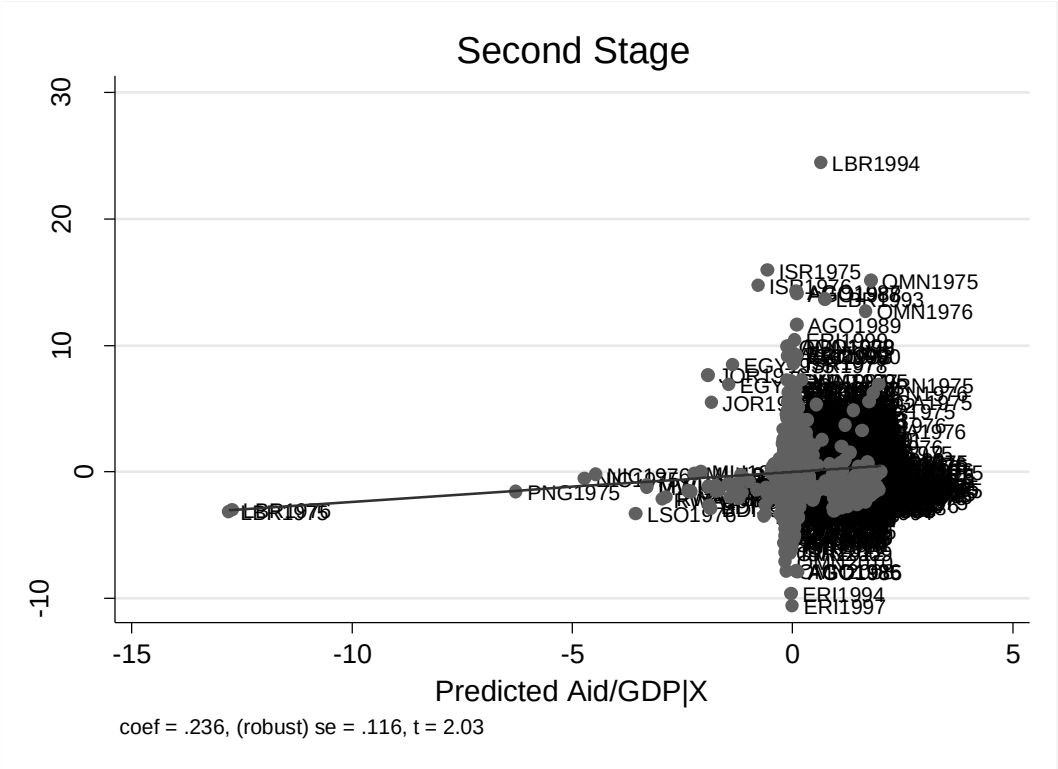
[20] We do not show the covariates in the tables in order to reduce clutter. Full regression results for our main specifications (Table 1) are presented in Appendix B (Table B9 and B10).

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 C 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 (Panel D). The Kleibergen-Paap first-stage F-statistics 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 the outlying countries see Appendix C: Case studies on Liberia and Israel). We therefore run the baseline 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



5. Robustness

In a first robustness test, we replace ODA with a measure of total flows consisting of ODA and Other Official Flows (OOF).[21] 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 in Appendix B shows that the results remain robust to the inclusion of OOF.[22] F-statistics clearly remain above the critical value of ten in all specifications. The effects in Table B1 are similar to those reported in Table 1 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 add log of population, gross national expenditure/GDP, and government ideology as additional covariates. Since the major outlying observations (Liberia 1975, 1976, and 1994) are not included in these specifications because of missing values in the additional covariates, the coefficients of

[21] 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.

[22] Results for arms imports (Table B5) remain robust to using total flows (ODA+OOF) as an explanatory variable.

development aid again lack statistical significance but remain positive. We can thus not confirm that aid is diverted to the military.[23]

In Table B3 we control for US military aid, which may be one major omitted variable. What is more, the exclusion restriction could 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 Table 1.

In another robustness test in Table B4 we replace the Polity IV democracy variable with Cheibub et al.'s (2010) democracy indicator as data availability differs for the two indices. The data by Cheibub et al. (2010) is available for more countries than we have in the sample when we use the Polity IV index, but does not include the years from 2009 to 2012. Results remain robust (Table B4), with the significantly positive effect being driven by Liberia only.[24]

Finally, in Table B5 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- to 10-percent-level. However, the effect lacks statistical significance when using the IV approach.

6. 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, corrupt versus less corrupt countries, conflict-ridden versus peaceful countries, low versus middle and high income countries, and across different types of donor countries.

6.1 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

[23] Table B11 includes full regressions of the IV results when including additional covariates.

[24] Results also remain robust when we use Rode and Bjørnskov's (2016) extended dataset including Cheibub et al.'s (2010) democracy indicator for the years from 2009 to 2012.

recipient countries.[25] To investigate heterogeneous effects of aid on military expenditure in democracies and non-democracies, we estimate our models for democracies and non-democracies separately (Table B6a and B6b) – as measured by the Polity IV index.[26] At first glance, our results seem to confirm Kono and Montinola's (2012) findings. While we mainly find no significant effects (and, if significant, negative effects) in the democratic country sub-sample, the effects turn significantly positive in the non-democratic country sub-sample. When we instead only include autocratic countries with a Polity IV index of smaller than or equal to -6 rather than including both, autocratic and so-called anocracies, the coefficient estimates of development aid are positive and statistically significant in columns (1) to (4) but lack statistical significance in columns (5) and (6). As in the baseline specification in Table 1, the significantly positive effect in the non-democratic sub-sample vanishes when we exclude Liberia. Though, the sign of the coefficient remains positive in non-democracies. When having a closer look at the democratic sub-sample we again find that an individual country drives the results, namely Israel. When excluding Israel from the democratic sub-sample, the coefficient estimates of development aid do not turn out to be statistically significant.[27] Consequently, we cannot confirm Kono and Montinola's (2012) findings.

6.2 Corruption

Following Gupta et al. (2001) corrupt recipients are likely to spend a higher share of their GDP on military expenditure than less corrupt countries. For example, arms imports are likely to increase when governments in arms-receiving countries are prone to bribes. Military affairs are usually dealt with in secrecy, therefore making corruption difficult to reveal (see Gupta et al. 2001 for a more detailed

[25] 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.

[26] Kono and Montinola (2012) use three different indices: the Polity IV index, the Unified Democracy Score (UDS) and Bueno de Mesquita and Smith's (2010) measure of the winning coalition size. It is worth noting that their findings vary according to the choice of the index. In order to split the sample, we use the proposed thresholds of the Polity IV index of larger than/equal to 6 for democratic and smaller than/equal to -6 for autocratic countries. As there are no broadly accepted thresholds for the two other continuous measures used by Kono and Montinola (2012), we do not want to overemphasize these results. When splitting the sample according to the UDS measure at zero (larger than zero for democratic and smaller than/equal to zero for autocratic countries), we find evidence for a significantly positive effect in three out of six specifications in the non-democratic sub-sample even after excluding Liberia. When we split the sample according to a winning coalition size of larger than 0.5 for democratic and smaller than/equal to 0.5 for non-democratic countries, we find no significant effects, neither in non-democracies nor in democracies when we exclude Liberia. We prefer to use the Polity IV democracy index, as it has a clear threshold.

[27] In two out of six specifications, we find a positive effect, statistically significant at the 10-percent-level. The effect is driven by an additional recipient country: Sri Lanka. When excluding both Israel and Sri Lanka, the coefficient estimate of aid lacks statistical significance in all six specifications.

discussion). 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.

We therefore split the sample according to the ICRG corruption index. Since there is no clear cut-off value for high and low corruption, we split the sample according to the median with higher values indicating less corruption. Countries above the median are classified as less corrupt and countries below the median are classified as corrupt countries. Tables B7a and B7b show significantly positive effects in four out of six specifications irrespective of the sub-sample. While the significant effects in corrupt countries vanish when excluding Liberia, the coefficient estimates of development aid remain positive and statistically significant in the less corrupt sub-sample after having controlled for outliers. However, the average corruption score in the two sub-samples differs only by one point, with a mean value of 2.1 for the corrupt sub-sample and 3.1 in the less corrupt sub-sample. This indicates that splitting the sample at the median leads to two sub-samples, which have on average rather similar corruption scores. We therefore tested the effect of ODA on military expenditure in countries with a corruption score of higher than 4 to capture less corrupt countries more clearly. As expected, the positive coefficient estimates lack statistical significance in this sub-sample. Taken together, we do not find clear evidence that aid is diverted to the military to a higher extent in corrupt recipient countries compared to less corrupt countries.

6.3 Conflicts and low-income countries

We have also split the sample according to whether the recipient country experiences a domestic or interstate conflict because countries involved in international and civil wars 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. Results in the two sub-samples, however, do not differ, suggesting that aid is not more likely to be diverted to the military in conflict-ridden countries than in countries at peace.

Moreover, 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.[28]

6.4 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 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).[29] On the contrary, France, Germany, Japan, South Korea, Austria, Netherlands, Switzerland, and Belgium are classified as coordinated market economies (CME). LMEs have been found to provide much 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 B8a and B8b). 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 and after controlling for the longer list of covariates from Table B2.[30] The results on contemporaneous effects do not depend on individual observations (columns (1)-(3) in Table B8b).[31] In columns (5) and (6), however, where we measure aid in year $t-1$, results lack statistical significance when we exclude Angola, Eritrea, and Liberia at the same time. The numerical meaning of the estimated effects is that when the aid to

[28] Results are available on request. We have used the country classification of the World Bank to split the recipients into low- and middle/high-income countries.

[29] 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.

[30] When we bootstrapped standard errors based on pairwise recipient country clusters, the effects in columns (1)-(5) remain statistically significant at the 10-percent-level.

[31] 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.

GDP ratio increases by one-percentage point, military expenditure as a share of GDP increases by about 0.15- to 0.26-percentage points in the most stringent specifications (columns (3) and (6) in Table B8b). 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 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 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.

7. Conclusion

Many empirical studies suggest that aid is not effective, for example, in increasing economic growth or supporting governance (e.g., Doucouliagos and Paldam 2008, 2011, Dreher and Langlotz 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. Two studies advanced empirical research on development aid and military expenditure by employing IV approaches to deal with the likely endogeneity of development aid (Collier and Hoeffler 2007, Kono and Montinola 2012). We are, however, sceptical as to whether the previous studies derived causal effects of development aid on military expenditure, since the IVs they used are arguably not excludable (cultural and political proximity between donor and recipient countries). We have therefore used a new IV strategy and new data on military expenditure for 124 recipient countries over the 1975-2012 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 (2015).

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.

Investigating heterogeneous effects according to autocracies and democracies corroborates the finding that two outliers – Liberia in the sample of non-democratic countries and Israel in the sample of democratic countries – drive the results. Considering our finding that including/excluding Liberia and/or Israel drastically changes the results, and therefore the conclusions that can be drawn, we posit that any study examining whether development aid influences military spending should explicitly state whether either of these two countries are included/excluded.

We have split the sample by donor types as proposed by Dietrich (2016). Liberal market economies (LMEs, e.g., the United States, the United Kingdom, Australia) have been found to provide much larger shares of bypass aid than coordinated market economies (CMEs, e.g., France, Germany, Japan). Our results show that aid provided by CMEs increases military expenditure, an effect being robust to excluding outlying countries. A one-percentage point increase in the aid-to-GDP ratio leads to about a 10 percent increase in military expenditure (share of GDP) for the average recipient country. When we measure aid in year $t-1$ instead of year t, the aid-induced effect is smaller and does not turn out to be statistically significant in some specifications. We propose that because 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.

Since specific types of aid including aid through different delivery channels are likely to affect military expenditure in different ways, future research should investigate which individual types 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)
			C. 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)
			D. First-Stage			
Fitted Aid/GDP	1.132***	1.121***	1.191***	1.207***	1.224***	1.305***
	(0.086)	(0.094)	(0.099)	(0.085)	(0.088)	(0.079)
Cragg-Donald F stat.	59.793	61.889	58.887	66.309	67.963	65.866
Kleibergen-Paap F stat.	175.144	141.646	146.255	201.296	192.043	272.038
Kleibergen-Paap LM stat.	4.273	3.667	3.280	4.177	4.052	3.711
K-P LM stat. p-val.	0.039	0.056	0.070	0.041	0.044	0.054
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, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level in Panel A, B and C and at the donor-recipient-country-level in Panel D; 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***
	(0.117)	(0.136)	(0.139)	(0.120)	(0.119)	(0.111)
Cragg-Donald F stat.	34.865	33.958	30.620	38.996	39.190	36.828
Kleibergen-Paap F stat.	81.979	58.453	66.682	88.635	90.045	123.850
Kleibergen-Paap LM stat.	17.606	21.438	16.227	17.519	17.970	14.279
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, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level; significance levels: * 0.10, ** 0.05, *** 0.01).

Appendix A: Data source and descriptive statistics

Table A1 Data source

Variable	Description	Data Source
Aid/GDP	ODA Net Total 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)
Corruption	Corruption, annual averages (ICRG), ranges from 0 to 6: most corrupt (0), least corrupt (6).	ICRG (2012)
Democracy index	Democracy index: dummy 1 for democracy.	Cheibub et al. (2010)
Domestic conflict	Dummy 1 for minor domestic conflict, between 25 and 999 battle-related deaths in a given year.	UCDP/PRIO Armed Conflict Dataset (2015)
Expenditure/GDP	Gross national expenditure (percent of GDP).	WDI (2015)
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)
Interstate conflict	Dummy 1 if the recipient country is involved in an interstate conflict.	UCDP/PRIO Armed Conflict Dataset (2015)
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)
Military expenditure/GDP	Military expenditure (percent of GDP).	SIPRI (2016)
Military personnel	Armed forces personnel, total, in millions.	WDI (SIPRI) (2015)
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)
Government ideology (left-wing)	Government ideology with respect to economic policy: Right (1); Left (3); Center (2); No information (0).	Database of Political Institutions (Beck et al. 2001)
Polity IV democracy	Polity IV democracy index, ranges from -10 to 10: Autocracies (-10 to -6); Democracies (6 to 10); Anocracies (-5 to 5).	Polity IV (2015)
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
Cheibub democracy	3035	0.37	0.48	0.00	1.00
Polity IV democracy	3466	0.56	6.80	-10.00	10.00
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
Neighbor ME	3059	3.05	2.91	0.00	42.91
Log population	3466	16.09	1.53	12.69	21.02
Corruption, annual averages (ICRG)	2029	2.62	1.01	0.00	6.00
Government ideology	3424	1.15	1.28	0.00	3.00
Gross national expenditure/GDP	3329	106.59	17.53	48.39	240.48

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: Additional regressions

Table B1 ODA and OOF

	(1)	(2)	(3)	(4)	(5)	(6)
	A. Second-Stage					
Aid/GDP	0.220*	0.130***	0.091***	0.186**	0.053**	0.052**
	(0.113)	(0.048)	(0.035)	(0.089)	(0.022)	(0.020)
	B. First-Stage					
Fitted Aid/GDP	1.004***	0.854***	0.882***	1.071***	1.090***	1.141***
	(0.112)	(0.138)	(0.151)	(0.103)	(0.096)	(0.107)
Cragg-Donald F stat.	45.272	41.201	37.571	49.993	51.819	48.548
Kleibergen-Paap F stat.	79.836	38.199	34.299	108.935	130.250	112.810
Kleibergen-Paap LM stat.	4.688	4.841	4.126	4.409	4.251	3.838
K-P LM stat. p-val.	0.030	0.028	0.042	0.036	0.039	0.050
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, 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 Additional covariates

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Second-Stage			
Aid/GDP	0.239	0.115	0.018	0.210	0.056	0.050
	(0.239)	(0.122)	(0.085)	(0.204)	(0.052)	(0.052)
			B. First-Stage			
Fitted Aid/GDP	1.005***	0.970***	1.043***	0.998***	1.013***	1.077***
	(0.109)	(0.143)	(0.166)	(0.126)	(0.122)	(0.124)
Cragg-Donald F stat.	29.984	24.021	21.076	28.777	29.622	26.728
Kleibergen-Paap F stat.	85.221	46.151	39.410	62.351	68.449	75.696
Kleibergen-Paap LM stat.	13.326	18.884	14.722	12.643	12.941	10.968
K-P LM stat. p-val.	0.000	0.000	0.000	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	3288	3199	2853	3109	3109	2779
No. of Countries	121	121	109	120	120	109

Notes: Data are at the recipient-year-level. Recipient- and year-fixed effects are included. First- and second-stage regressions include as control variables: log of GDP/capita, polity, log of population, gross national expenditure/GDP, party orientation, 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 B3 US military aid

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Second-Stage			
Aid/GDP	0.234**	0.116**	0.080***	0.197**	0.055**	0.053**
	(0.114)	(0.046)	(0.030)	(0.094)	(0.024)	(0.022)
US Military Aid/GDP	0.155	-0.111	-0.089	0.247	-0.024	-0.035
	(0.338)	(0.136)	(0.119)	(0.309)	(0.100)	(0.098)
			B. First-Stage			
Fitted Aid/GDP	1.152***	1.154***	1.212***	1.207***	1.237***	1.307***
	(0.113)	(0.129)	(0.148)	(0.103)	(0.100)	(0.107)
Cragg-Donald F stat.	63.309	67.828	63.034	66.902	70.302	66.741
Kleibergen-Paap F stat.	103.068	80.257	66.697	138.649	153.452	148.375
Kleibergen-Paap LM stat.	4.886	4.242	3.845	4.854	4.692	4.298
K-P LM stat. p-val.	0.027	0.039	0.050	0.028	0.030	0.038
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	3334	2931	3257	3257	2869
No. of Countries	122	122	107	121	121	107

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, 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 B4 Baseline with Cheibub et al.'s (2010) democracy index

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Second-Stage			
Aid/GDP	0.223*	0.142***	0.099***	0.231*	0.079**	0.078**
	(0.130)	(0.054)	(0.037)	(0.127)	(0.035)	(0.033)
			B. First-Stage			
Fitted Aid/GDP	1.078***	1.039***	1.107***	0.974***	0.982***	1.043***
	(0.091)	(0.101)	(0.109)	(0.128)	(0.123)	(0.134)
Cragg-Donald F stat.	52.558	51.418	48.473	50.371	51.062	49.224
Kleibergen-Paap F stat.	139.685	105.659	102.160	58.188	63.710	60.527
Kleibergen-Paap LM stat.	6.188	5.224	4.472	8.671	8.486	7.228
K-P LM stat. p-val.	0.013	0.022	0.034	0.003	0.004	0.007
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	3163	3059	2664	2983	2983	2603
No. of Countries	128	128	113	127	127	113

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, 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 B5 Arms imports

	(1)	(2)	(3)	(4)	(5)	(6)
			A. OLS			
Aid/GDP	7.668*	5.258**	5.543**	12.882**	5.711**	6.357**
	(3.932)	(2.556)	(2.786)	(6.260)	(2.568)	(2.714)
Adj. R-squared	0.049	0.541	0.551	0.060	0.543	0.555
			B. Second-Stage			
Aid/GDP	5.239	4.586	7.211	19.482	15.464	20.101
	(21.695)	(13.092)	(24.879)	(30.135)	(12.513)	(17.568)
			C. First-Stage			
Fitted Aid/GDP	1.594***	1.709***	1.398***	1.632***	1.631***	1.411***
	(0.261)	(0.254)	(0.129)	(0.294)	(0.294)	(0.102)
Cragg-Donald F stat.	215.079	209.594	78.745	229.934	229.763	110.370
Kleibergen-Paap F stat.	37.355	45.142	117.267	30.863	30.713	192.504
Kleibergen-Paap LM stat.	5.924	4.085	23.289	6.501	6.495	23.219
K-P LM stat. p-val.	0.015	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	2501	2051	1835	1988	1988	1782
No. of Countries	125	116	106	115	115	105

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, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level in; significance levels: * 0.10, ** 0.05, *** 0.01).

Table B6a Heterogeneous effects: Democratic countries

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Second-Stage			
Aid/GDP	-2.392	-0.344*	-0.474***	-1.939	0.056	0.023
	(1.811)	(0.197)	(0.157)	(1.423)	(0.062)	(0.052)
			B. First-Stage			
Fitted Aid/GDP	1.672***	2.362***	3.335***	1.722***	2.635***	3.877***
	(0.425)	(0.578)	(0.684)	(0.386)	(0.740)	(0.965)
Cragg-Donald F stat.	4.113	8.688	9.509	4.198	8.762	10.125
Kleibergen-Paap F stat.	15.500	16.692	23.748	19.937	12.674	16.142
Kleibergen-Paap LM stat.	5.425	4.032	3.279	5.254	4.065	3.750
K-P LM stat. p-val.	0.020	0.045	0.070	0.022	0.044	0.053
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	1298	1271	1034	1195	1195	973
No. of Countries	74	74	61	72	72	60

Table B6b Heterogeneous effects: Autocratic countries

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Second-Stage			
Aid/GDP	0.417**	0.251**	0.181***	0.365**	0.108**	0.113**
	(0.210)	(0.101)	(0.069)	(0.167)	(0.048)	(0.049)
			B. First-Stage			
Fitted Aid/GDP	1.037***	1.008***	1.053***	1.098***	1.105***	1.138***
	(0.109)	(0.146)	(0.163)	(0.110)	(0.109)	(0.111)
Cragg-Donald F stat.	31.488	28.955	25.520	34.323	34.476	30.956
Kleibergen-Paap F stat.	91.143	47.403	41.512	99.586	102.813	105.803
Kleibergen-Paap LM stat.	11.430	10.386	8.202	11.864	11.868	9.868
K-P LM stat. p-val.	0.001	0.001	0.004	0.001	0.001	0.002
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	2168	2089	1923	1997	1997	1846
No. of Countries	107	106	98	105	105	97

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, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level; significance levels: * 0.10, ** 0.05, *** 0.01). Democracy is measured with Polity IV index. Countries are classified as democratic (Panel A) if the index is larger or equal to 6 and non-democratic countries including transition countries (index smaller to 6).

Table B7a Heterogeneous effects: Corrupt countries

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Second-Stage			
Aid/GDP	0.134*	0.076***	0.083***	0.103*	0.027	0.033
	(0.068)	(0.026)	(0.028)	(0.056)	(0.021)	(0.023)
			B. First-Stage			
Fitted Aid/GDP	1.301***	1.302***	1.314***	1.379***	1.403***	1.415***
	(0.150)	(0.169)	(0.172)	(0.127)	(0.117)	(0.122)
Cragg-Donald F stat.	42.856	48.418	45.529	47.420	49.206	46.278
Kleibergen-Paap F stat.	75.527	59.437	58.188	117.534	143.994	133.893
Kleibergen-Paap LM stat.	2.921	2.501	2.290	2.765	2.648	2.479
K-P LM stat. p-val.	0.087	0.114	0.130	0.096	0.104	0.115
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	1447	1397	1313	1362	1362	1282
No. of Countries	52	52	48	51	51	48

Table B7b Heterogeneous effects: Less corrupt countries

	(1)	(2)	(3)	(4)	(5)	(6)
			A. Second-Stage			
Aid/GDP	0.707*	0.202*	0.184	0.475	0.197**	0.193**
	(0.388)	(0.117)	(0.121)	(0.339)	(0.085)	(0.091)
			B. First-Stage			
Fitted Aid/GDP	0.777***	0.797***	0.951***	0.876***	0.871***	1.103***
	(0.177)	(0.183)	(0.212)	(0.204)	(0.200)	(0.218)
Cragg-Donald F stat.	7.786	7.452	7.914	10.722	10.564	12.185
Kleibergen-Paap F stat.	19.353	18.899	20.058	18.412	18.947	25.554
Kleibergen-Paap LM stat.	10.753	10.578	9.466	11.025	11.869	10.664
K-P LM stat. p-val.	0.001	0.001	0.002	0.001	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	1940	1885	1587	1853	1853	1563
No. of Countries	72	72	61	72	72	61

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, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level; significance levels: * 0.10, ** 0.05, *** 0.01). Corruption is measured with the ICRG corruption index. We split the sample according to mean value of the index in the overall sample.

Table B8a 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***
	(0.113)	(0.182)	(0.213)	(0.118)	(0.126)	(0.134)
Cragg-Donald F stat.	36.712	30.529	33.270	39.135	39.807	41.004
Kleibergen-Paap F stat.	76.298	34.932	34.039	75.924	68.057	71.809
Kleibergen-Paap LM stat.	5.762	14.736	12.923	5.821	5.861	5.872
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 B8b 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***
	(0.160)	(0.168)	(0.217)	(0.152)	(0.158)	(0.193)
Cragg-Donald F stat.	48.604	60.352	50.046	51.463	57.144	48.953
Kleibergen-Paap F stat.	64.230	63.651	44.113	72.748	76.206	60.376
Kleibergen-Paap LM stat.	20.046	19.321	16.643	20.329	21.363	19.546
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, and interstate and domestic conflict. Standard errors are in parentheses (clustered at the recipient-country-level; significance levels: * 0.10, ** 0.05, *** 0.01). Donor countries are classified as LME or CME countries according to Dietrich (2016).

Table B9 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 democracy	-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 ependiture/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; significance levels: * 0.10, ** 0.05, *** 0.01). These are the full 2SLS regression results of Table 1.

Table B10 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 democracy	-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 ependiture/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; significance levels: * 0.10, ** 0.05, *** 0.01). These are the full OLS regression results of Table 1.

Table B11 Additional covariates full regressions, IV

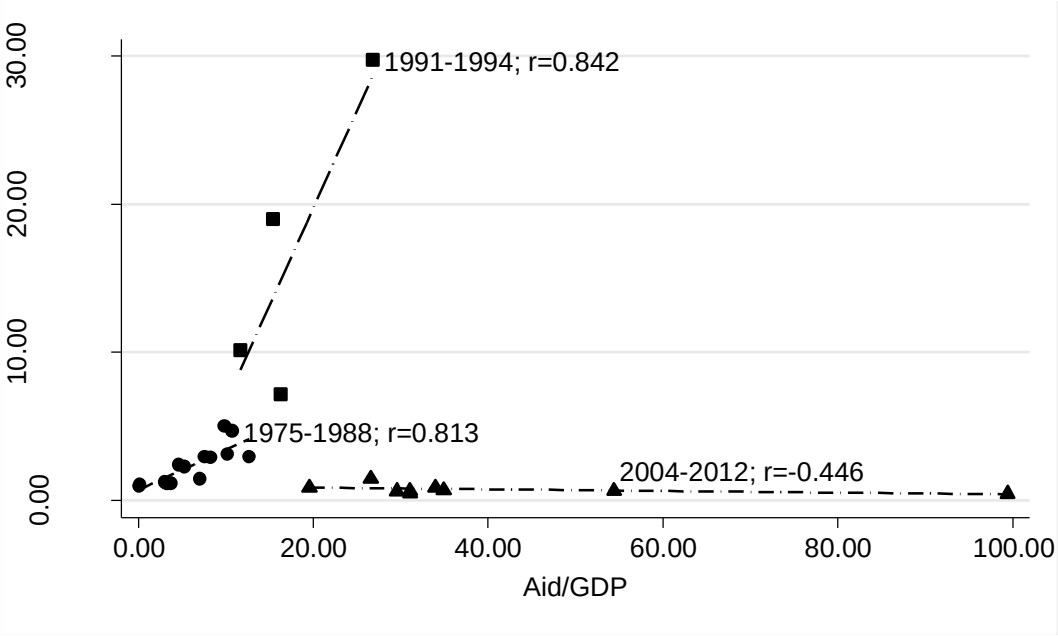
	(1)	(2)	(3)	(4)	(5)	(6)
Aid/GDP	0.239	0.115	0.018	0.210	0.056	0.050
	(0.239)	(0.122)	(0.085)	(0.204)	(0.052)	(0.052)
Polity IV democracy	-0.021	-0.011	-0.005	-0.024	-0.011	-0.007
	(0.028)	(0.010)	(0.007)	(0.028)	(0.008)	(0.008)
Log GDP per capita	-0.169	0.222	0.098	-0.315	0.096	0.103
	(0.724)	(0.282)	(0.224)	(0.651)	(0.175)	(0.165)
Log population	-1.735	-0.161	0.093	-1.892	-0.242	-0.017
	(1.361)	(0.445)	(0.423)	(1.311)	(0.372)	(0.410)
Gross national expenditure/GDP	0.001	-0.000	0.007	0.005	0.005	0.004
	(0.026)	(0.012)	(0.009)	(0.021)	(0.006)	(0.006)
Government ideology (left-wing)	-0.041	0.007	0.007	-0.035	0.000	0.004
	(0.099)	(0.032)	(0.030)	(0.094)	(0.028)	(0.033)
Interstate conflict	1.325*	0.778	0.748	1.279*	0.718	0.734
	(0.746)	(0.579)	(0.565)	(0.709)	(0.525)	(0.526)
Domestic conflict	0.568**	0.143	0.153*	0.599**	0.175*	0.167*
	(0.222)	(0.094)	(0.092)	(0.239)	(0.091)	(0.091)
Military ependiture/GDP (t-1)		0.753***	0.726***		0.722***	0.705***
		(0.062)	(0.066)		(0.062)	(0.067)
Neighbor ME (t-1)			0.050*			0.040
			(0.028)			(0.025)
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	3288	3199	2853	3109	3109	2779
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). These are the full regression results of Table B2.

Appendix C: Case studies on Liberia and Israel

We now discuss the two most outstanding outliers in more detail, Liberia and Israel. 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.

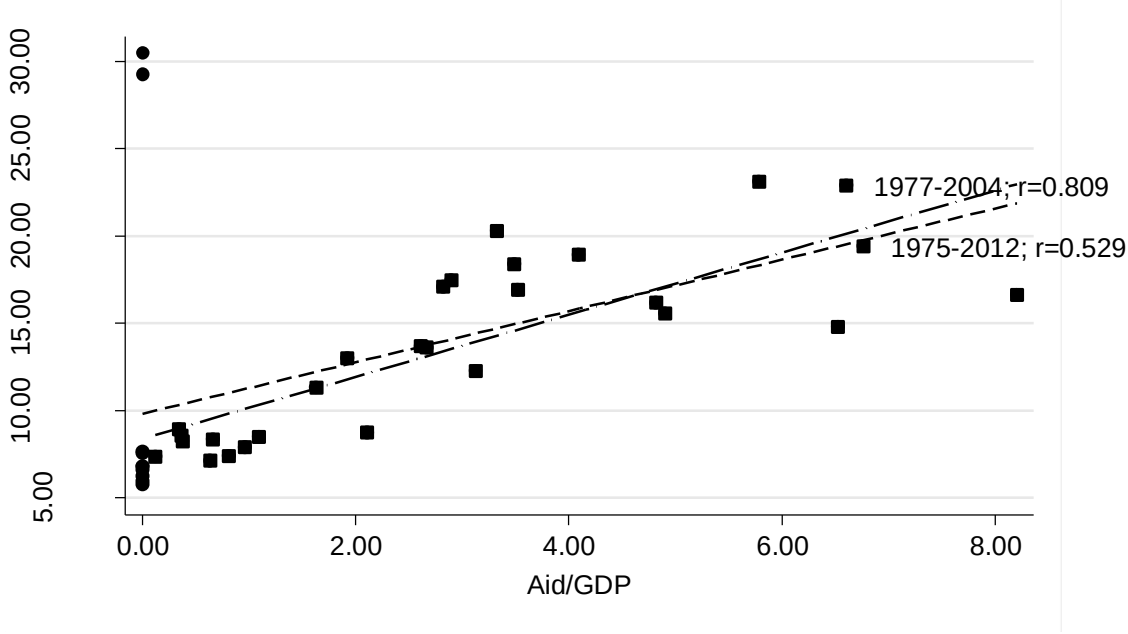
Our second major outlying country is Israel. Israel is a developed OECD country that has been a democracy with recurring elections since the establishment of the modern state in 1948. The aid-to-GDP-ratio was quite large in the 1980s, taking on the largest value of 8.2 percent in 1985 at the end of the first Lebanon war. At the same time, Israel spends a large share of its GDP on the military. Military expenditure (as a share of GDP) decreased, however, from about 30.5 percent in 1975 to about 5.7 percent in 2012. For the 1977-2004 period, the coefficient of correlation between aid and military expenditure is 0.81 (Figure 3).[32] For the overall sample period from 1975-2012 the correlation is, however, much lower (0.53), but still remarkably high.

The most important donor country providing aid to Israel is the United States. In our sample, about 96 percent of the aid Israel receives is provided by the United States. In the course of the 1977 peace agreement between Israel and Egypt, US development and military aid increased to both Israel and Egypt, with Egypt receiving two-thirds of any aid given to Israel. In general, US military aid to Israel has been larger than ODA throughout the observation period. In 2004, for example, US ODA was about 0.4 percent of Israeli GDP compared to a much higher share of US military aid, which reached about 2.1 percent of Israeli GDP. In order to avoid increasing the official amount of military aid even further and to

[32] We specifically look at the 1977-2004 period since aid has been quite small before 1977 and no aid was provided after 2004.

reduce the gap between military aid and ODA, the governments might have agreed to declare military aid as ODA. We conjecture that a large share of the ODA provided by the United States to Israel was intended as military aid from the very beginning.

Figure 3: Correlation between ODA/GDP and military expenditure/GDP per period, Israel



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

Both Israel and Liberia have been involved in conflicts for many years. We control for conflict in our econometric model, but may not have captured the true military threat that Liberia and Israeli face. Consequently, both recipient countries are likely to have diverted as many resources as possible (including ODA) to the military.