Fueling Conflict? (De)Escalation and Bilateral Aid

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Abstract

This paper studies the effects of bilateral foreign aid on conflict escalation and deescalation. First, we develop a new ordinal measure capturing the two-sided and multifaceted nature of conflict. Second, we propose a dynamic ordered probit estimator that allows for unobserved heterogeneity and corrects for endogeneity. Third, we identify the causal effect of foreign aid on conflict by predicting bilateral aid flows based on electoral outcomes of donor countries exogenous to recipients. Receiving bilateral aid raises the chances of escalating from small conflict to armed conflict, but we find little evidence that aid ignites conflict in truly peaceful countries.

Keywords: conflict, foreign aid, political economy, dynamic ordered panel data

JEL Classification: D74, F35, O11, C25

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1. Introduction

Civil conflict is not only one of the main obstacles to development, it also tends to be concentrated in poor countries. About half of all developing countries experienced an armed conflict in which at least 25 people died in a given year over the past four decades – directly or indirectly affecting close to four billion people. At the same time, poor and badly governed states prone to conflict need and receive substantial amounts of development assistance. Bilateral aid averaged about 5% of recipient GDP over the same period, but does this aid appease or fuel conflict?

A large and growing literature examining this question has failed to generate a consensus. Theoretically, the relationship is ambiguous as rising opportunity costs, increasing state capacity, and greater gains from capturing the state are all plausible consequences of development assistance. The empirical evidence is equally divided: several studies find that aid helps, while others maintain that it obstructs peace. Credible evidence is usually limited to specific regions or countries (e.g., the Philippines, Crost et al., 2014), specific types of aid (e.g., U.S. food aid, Nunn and Qian, 2014) or both (e.g., U.S. military aid in Columbia, Dube and Naidu, 2015). Devising a convincing identification strategy for bilateral aid has proven difficult given the well-known limitations of cross-country data.

Another notable divide between the theoretical and empirical literature is that the latter pays little attention to the dynamics of conflict. Empirically, conflict is usually considered to be a binary state, although recent theory stresses the importance of smaller conflicts (e.g., Bueno de Mesquita, 2013), different types of violence (e.g., Besley and Persson, 2011b), and conflict cycles (e.g., Rohner et al., 2013; Acemoglu and Wolitzky, 2014). Most papers distinguish between the onset and continuation of conflict, but studying these two transitions separately is an imperfect substitute for analyzing an inherently dynamic problem (Beck et al., 1998). More fundamentally, there is no empirical sense of escalation or deescalation among different conflict intensities when the ordinal nature of conflict is disregarded. Only the case of a switch from peace to conflict and vice versa is usually accounted for. These distinctions matter. As we show in the following, small scale conflicts below the usual minimal threshold of 25 battle-related deaths often start a cycle of violence. In contrast, a civil war never broke out in a society that was completely at peace in the year before.

Establishing the *causal* effect of bilateral aid on the escalation and deescalation of conflict is the key objective of this paper. In essence, we conjecture that neglecting smaller conflicts pollutes most existing estimates of the effect of aid on conflict. To see this, consider the argument that foreign aid incites violence because some groups inevitably profit more from the added financial flows than others. Hodler and Raschky (2014) and Dreher et al. (2015), for example, show that funds tend to disproportionately flow to the birth region of the current ruler. This is likely to translate into civil discontent which can find its expression in smaller acts of violence with comparatively low opportunity costs. Any violent behavior questions the state's monopoly of violence, satisfying what can be considered the most basic definition of civil conflict. Small conflicts thus act as a signal to the government that some part of society is not content with the current provision, or division, of public goods. In addition, they help potential rebels to get an estimate of how easily they can overcome collective action problems and provide information about the government's repressive capabilities. Foreign aid, in turn, may exacerbate violent tendencies in such environments but not when society is truly at peace.

Our empirical analysis introduces three novelties in order to identify these dynamics. First, we propose a new measure of conflict which captures the gradations of civil violence from peace over intermediate categories to fully fledged civil wars. Second, we develop a dynamic ordered probit framework which allows us to estimate escalation and deescalation probabilities for multiple states. In our approach, the onset, continuation, and the duration of each realization of civil violence are all well defined. We then extend this basic framework to account for unobserved heterogeneity (quasi fixed effects) and correct for the endogeneity of aid (based on Rivers and Vuong, 1988; Wooldridge, 2005; Giles and Murtazashvili, 2013). Third and most importantly, we identify the effect of aid on conflict using characteristics of the electoral system of donor countries. We interact political fractionalization of each donor with the probability of receiving aid to predict bilateral aid flows in a "gravity-style" aid equation (Frankel and Romer, 1999; Rajan and Subramanian, 2008; Dreher and Langlotz, 2015). This type of identification strategy is now common in the trade and migration literature but usually relies on structural characteristics of both partner countries. We solely use the variation arising from electoral outcomes in donor countries combined with the likelihood of receiving aid.

Our main results show that the causal effect of foreign aid on the various transition probabilities is heterogeneous and, in some instances, sizable. Foreign aid has a very different effect on the probability of experiencing conflict, depending on whether a society was entirely peaceful, already in turmoil, or mired in major civil conflict.

Aid does not seem to harm recipient countries by causing conflict across the board. While all estimates suggest that bilateral aid tends to fuel conflict, we find scarce evidence suggesting that foreign aid leads to new eruptions of conflict or that it drives the escalation towards (or the continuation of) civil wars. At face value, the positive signs are also at odds with rising opportunity costs, although it remains difficult to delineate the exact channels.

Our findings suggest that aid can be harmful when given to countries already experiencing violent turmoil just short of the conventional definition of civil conflict. In those cases we find *i)* a strong negative effect on the probability of transitioning back to peace, *ii)* an elevated risk of continued violence, and *iii)* a non-trivial probability of escalating into armed conflict. Donor countries have to be aware of the unintended consequences of giving aid to countries with lingering conflicts.

Our results underscore the importance of carefully modeling the dynamics of conflict. This echoes the recent literature (e.g., Bazzi and Blattman, 2014; Nunn and Qian, 2014; Berman and Couttenier, 2015) but our analysis goes several steps further and generates new insights. Escalation or deescalation, i.e., the switching among different conflict intensities, is a dynamic process and the established binary peace-war typology hides important heterogeneity. What is often coded as peace is not actually peaceful and what influences the decision to fight differs in these situations.

The remainder of the paper is organized as follows. Section 2 discusses the related literature and provides the theoretical background. Section 3 introduces our new ordinal conflict measure. Section 4 outlines our empirical model and identification strategy. Section 5 presents the empirical results and Section 6 discusses a battery of robustness checks. Section 7 concludes.

2. Related literature

A. Civil conflict and foreign aid

The direction of the overall effect of aid boils down to how it changes the calculus of citizens and governments. For citizens, aid may alter the opportunity costs of fighting (e.g., Becker, 1968; Collier and Hoeffler, 2004b). For governments, aid may increase state capacity (Fearon and Laitin, 2003; Besley and Persson, 2011a) and/or increase the value of capturing the state (e.g., Grossman, 1991). Variants of these theories incorporate both channels and try to distinguish between two opposing income effects: having less to fight over but fewer outside options versus fighting over a larger pie but having more to lose. As a result of this heterogeneity, the overall sign of the effect of aid remains theoretically ambiguous. We now briefly discuss these channels one by one.

Foreign aid affects the opportunity costs of fighting. If aid improves the provision of public goods, then it directly decreases the incentives of engaging in violent activities (Becker, 1968). Aid may also alter opportunity costs indirectly through economic growth. However, the large empirical literature on aid and growth finds little or at best weak evidence in favor of this channel (e.g., Rajan and Subramanian, 2008; Clemens et al., 2012; Dreher and Langlotz, 2015). The literature on income shocks and conflict is also instructive. Bazzi and Blattman (2014) find no evidence of an effect of export price shocks on conflict at the country-level, while Berman and Couttenier (2015) add that negative income shocks predict conflict at the subnational level.

Foreign aid may increase state capacity. When aid improves public resources, the government is likely to put more effort into controlling these resources (Fearon and Laitin, 2003). Greater control over resources increases its capability to suppress conflict and higher state capacity lowers the risk of conflict by reducing the likelihood of successful capture (Besley and Persson, 2011a). It thus diminishes the expected value of rebellion. Part of the state capacity effect could run through military spending. Although official development aid excludes military aid by definition, receiving aid relaxes the government's budget constraint if aid is sufficiently fungible (Collier and Hoeffler, 2007).

Foreign aid raises the stakes. Standard contest theory argues that the state is a price that rebels want to capture (e.g., Grossman, 1991). It predicts that conflict becomes more likely when aid receipts are higher as the expected gains from fighting increase. Such arguments are pervasive in the literature on conflict over natural resources and many other contests. However, the equilibrium level of conflict may be independent of the income level if the revenue and opportunity cost effects cancel out (Fearon, 2007). Dal Bó and Dal Bó (2011) show that the relative size of these effects depend on the labor and capital intensity of production, while Besley and Persson (2011b) introduce a model where they depend on the cohesiveness of political institutions. When aid acts like a resource windfall in weak states, it raises violence and repression in equilibrium. Hence, it matters where development aid actually goes and how easily it can be appropriated by rebels, either directly by intercepting aid deliveries or indirectly by imposing "revolutionary taxation."

Most studies in the literature on civil conflict find that aid appeases (e.g., de Ree and Nillesen, 2009; Savun and Tirone, 2011; Ahmed and Werker, 2015). Recently, however, evidence to the contrary has been accumulating (e.g., Besley and Persson, 2011b; Nunn and Qian, 2014; Dube and Naidu, 2015). Nunn and Qian (2014), for example, argue that food aid can be used as rebel financing since it can be captured almost instantly. Their results show that U.S. food aid prolongs the duration of conflict but does not predict conflict onset. Rising opportunity costs can also lead to an adverse effect of aid. Crost et al. (2014) show that municipalities in the Philippines which are about to receive more aid experience increased rebel activity. Rebels anticipating the impending change in incentives sabotage aid, since successful aid programs reduce support for their cause.

B. Cycles of violence

The cyclical nature of conflict is receiving increasing attention. Recent theories aim to account for escalation and deescalation cycles in a unified framework. Besley and Persson (2011b) emphasize that one-sided violence by an incumbent aiming to stay in power gives rise to multiple states of violence, ranging from peace over repression to civil war. Rohner et al. (2013) and Acemoglu and Wolitzky (2014) present models where recurring conflicts can happen by accident but are often started when there is a break down of trust or signals are misinterpreted. They only end when beliefs are updated accordingly. Once such a cycle starts, persistence may simply be the product of continuously eroding outside options which suggests that stopping violence becomes more difficult as conflicts intensify. The empirical literature lags behind this development. Even if studies account for different intensity levels, they usually analyze them separately and thus cannot deliver a full description of the underlying dynamics.

Small conflicts matter for a proper understanding of conflict cycles. They are often the starting point for further escalation and can be an integral part of rebel tactics. Political economy models highlight the importance of collective action and information problems that have to be overcome to engage in organized violence, revolution, or civil war (Esteban et al., 2012; Bueno de Mesquita, 2013). Small conflicts can help to overcome these problems by delivering an estimate on how many others are willing to fight the government. Theoretically, small conflicts can be considered a signaling device, where potential rebels try to determine the type of their government or vice versa (Acemoglu and Wolitzky, 2014). Minor violent actions do not have the same opportunity costs as civil war. They allow groups of individuals to question the monopoly of violence without investing too much into the fight and may be strategic substitutes to conventional warfare in a long standing rebellion (Bueno de Mesquita, 2013). Empirically, these situations are very different from peace. Without accounting for small scale conflicts, estimates of onset probabilities are likely to be biased by mixing truly peaceful societies with already violent and volatile environments.

A neglect of small conflicts is particularly worrying when it comes to the impact of aid on conflict. The effect of aid may very well be heterogeneous and depend on the level of violence.[1] This could be the case for at least two reasons. First, aid is not distribution-neutral (see, e.g., Dreher et al., 2015, who show that Chinese aid disproportionately flows to the birth region of African leaders). Greater aid flows may increase pre-existing discontent over the allocation of resources. Due to logistical reasons aid is given more often to peaceful regions or regions of low conflict intensity. If aid is primarily targeted at such regions, resentment may fortify in unprivileged areas, where violence persists. Opportunity costs erode and rebels controlling such a region may be able to recruit others more easily. Second, if a country is entirely peaceful, the government is less likely to divert development aid or freed-up funds to the military. If there is a lingering conflict, on the other hand, the incumbent government might continue to invest in the military to repress or discourage rebellion (Besley and Persson, 2011a). Hence, the effect of aid on state capacity differs depending on the level of violence.

[1]For instance, Collier and Hoeffler (2004a) argue that aid is especially effective in post-conflict scenarios.

C. Causal identification

The simultaneity of aid and conflict makes causal identification notoriously difficult. The strong correlation of low GDP per capita and civil strife is one of the most robust findings in the literature (e.g., Fearon and Laitin, 2003; Blattman and Miguel, 2010). Underdevelopment – with all that it entails – is the *raison d'être* of development aid. As a result, the effect of aid is likely to be biased upwards if aid is primarily given to countries in need, or biased downwards if donors are driven by political motives (as documented by, e.g., Kuziemko and Werker, 2006) or reduce aid in light of the logistical challenges created by conflict. Biases could also result from third factors influencing aid and conflict simultaneously, such as political and economic crises, or (systematic) measurement errors.

Much of the literature follows Clemens et al. (2012) and addresses the endogeneity problem by lagging aid. This is meant to rule out reverse causality and avoid bad-quality instruments (arguably without much success). Others follow the advice of Blattman and Miguel (2010) and focus on causal identification with single instruments. However, most instruments proposed so far are either weak or not exogenous: de Ree and Nillesen (2009), for example, use donor country GDP as an instrument for bilateral aid flows which could work through a variety of other channels, such as trade or FDI. A noteworthy exception are Nunn and Qian (2014) who use lags of U.S. wheat production interacted with each recipient's frequency of receiving aid as an instrument for U.S. food aid.[2] We extend the spirit of their identification strategy to all major bilateral donors, with the explicit aim of drawing conclusions that go beyond the (limited) effects of food aid given by one large donor. Much of the ground work has been done in Dreher and Langlotz (2015) who first introduce political fractionalization interacted with the probability of receiving aid as an instrument for bilateral aid flows in the context of growth regressions. We describe this strategy in more detail below.

3. Data

We study the occurrence of civil violence in 125 developing countries over the period from 1975 to 2010. We first discuss our measure of conflict, and then the operationalization of aid and the covariates. A list of the included countries and summary statistics of all variables can be found in Appendix A (Tables A-1 to A-3).

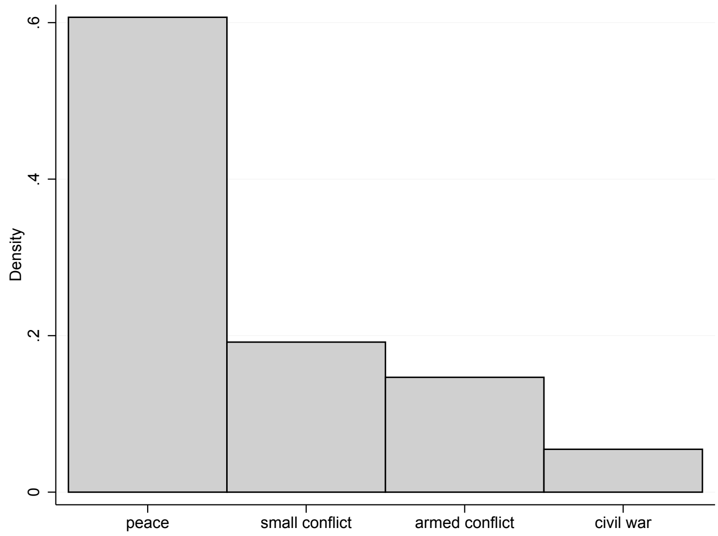
A. An ordinal measure of conflict

A distinct feature of the civil conflict literature is its crude measurement of conflict. The industry standard is to first count the number of battle-related deaths (BDs) and

[2] A different strategy is proposed by Werker et al. (2009) and Ahmed and Werker (2015), who use oil prices to instrument aid flows from oil-producing Muslim to non-oil producing Muslim countries.

then to create dummy variables indicating the surpassing of one of two thresholds (25 or 1,000 BDs) for the first time (conflict onset) or for any given year other than the first (continuation or ending). Clearly, a key concern motivating this choice is noise in the underlying raw data and theoretical ambiguity about what constitutes "conflict."

FIGURE I
Distribution of conflict intensities



Notes: Illustration of the unconditional distribution of the ordinal conflict measure. There are 3,014 peace years, 739 small conflict years, 544 armed conflict years, and 203 civil war years in our sample.

We propose a new ordinal measure of conflict with four states. For comparability, we begin with the standard UCDP-PRIO measure of civil conflict (Gleditsch et al., 2002). UCDP-PRIO defines civil conflict as a contested incompatibility that concerns the government or a territory in which armed force between two parties, one of which is the government, and results in at least 25 BDs per annum. We call conflicts that reach this state but do not exceed 1,000 BDs in a given year 'armed conflict.' At the top, we add a category called 'civil war' if there are more than 1,000 BDs. At the bottom, we complement the data with observations from the Cross-National Time-Series Data Archive (CNTS) on government purges, assassinations, riots and guerrilla warfare (Banks and Wilson, 2015).[3] All of these categories are manifestations of civil conflict, albeit on a lower intensity level. We only include observations of the CNTS data that are comparable to the type of conflict we consider in the above categories, i.e., conflicts

[3]The precise definitions of our variables from the Databanks User's Manual are as follows. Purges: Any systematic elimination by jailing or execution of political opposition within the ranks of the regime or the opposition. Assassinations: Any politically motivated murder or attempted murder of a high government official or politician. Riots: Any violent demonstration or clash of more than 100 citizens involving the use of physical force. Guerrilla Warfare: Any armed activity, sabotage, or bombings carried on by independent bands of citizens or irregular forces and aimed at the overthrow of the present regime. Note that Besley and Persson (2011b) took a similar approach when they added one-sided state repression (purges) as an intermediate category to what we define as civil war.

between two parties one being the state (two-sided, state-centered).[4] Only a truly peaceful society is coded zero. As a whole, the countries in our sample spend about one third of all years in conflict at various intensities and about two thirds of all years in peace. Figure I shows a histogram of the intensity distribution.

A key advantage of our approach is that the number of armed conflicts and civil wars in our sample are identical to the UCDP-PRIO measure. Hence, our results are comparable with existing studies and differ mainly due to the definition of peace. We distinguish between truly peaceful observations and those with irregular violence below the conventional thresholds. This conservative approach of changing existing measures implies that our ordinal measure is comparable and easy to understand. We avoid weighting procedures such as those used by the composite index of the CNTS data set. We also deliberately refrain from mixing flow and stock variables to measure different conflict intensities, such as taking the cumulative amount of BDs to create intermediate levels of armed civil conflict (e.g., Esteban et al., 2012; Bazzi and Blattman, 2014). Measures including both flow and stock variables do not allow us to study escalation and deescalation since they have absorbing terminal states. Appendix B presents the case of Sri Lankan Civil War to illustrate the benefits of our coding in more detail.

TABLE I
Unconditional transition matrix (in %)

	To State			
From State	Peace	Small Conflict	Armed Conflict	Civil War
Peace	87.26	10.69	2.06	0.00
Small Conflict	43.85	48.13	6.78	1.24
Armed Conflict	11.28	8.46	70.30	9.96
Civil War	1.49	5.97	23.88	68.66

Notes: The table reports the raw transition matrix estimated using the same balanced sample of 125 countries over 36 years that is used in the main analysis (4,500 observations imply 4,375 transitions). Rows sum to 100%.

Table I shows the unconditional transition probabilities as they are observed in our data. This simple exercise already allows us to make three worthwhile points. First, the cyclical nature of conflicts is clearly visible but there is not a single country in our data set where peace immediately preceded civil war. Second, our coding of small conflict achieves a credible and important separation of the lower category. Peace is now very persistent and, if anything, a transition to a small conflict is most likely. Small conflict is

[4] In the case of riots this may not be obvious from the variable definition, but the large riots recorded in the CNTS data usually involve violent clashes between anti-government protesters with (pro-)government forces. They are what incumbents react to with repression. For a prototypical example, see Yemen in 2011 (http://www.nytimes.com/2011/02/15/world/middleeast/15yemen.html).

a fragile state which often reverts back to peace, is not particularly persistent, but does sometimes erupt into more violent states. Third, higher intensity conflicts are once again more persistent. These observations match up well with the literature, in particular, the use of irregular means to increase mobilization for a future conventional campaign and increased persistence as outside opportunities erode (Bueno de Mesquita, 2013).

B. Bilateral aid flows and controls

Our main independent variables are two types of flows disbursed by 28 bilateral donors of the OECD Development Assistance Committee (DAC): Official Development Aid (ODA) and Other Official Flows (OOF). ODA refers to flows that are *i)* provided by official agencies to developing countries and multilateral institutions, *ii)* have economic development and welfare as their main objective, and *iii)* have a concessional character. The last condition reflects that the grant element should be at least 25%. OOF includes flows by the official sector with a grant element of less than 25% or flows that are not primarily aimed at development. We use net ODA flows which include loan repayments since these reduce the available funds. We do not examine multilateral aid which is typically a bit less than one third of all aid.

The data for government and legislative fractionalization (in donor countries) are from Beck et al. (2001). For the set of core controls, we follow Hegre and Sambanis (2006) by including the log of population to capture the scale effect inherent in conflict incidence and the log of GDP. We later also use the Polity IV score to account for institutional quality and a democracy dummy indicating if the Polity score is equal or above six. We control for a measure of political instability, that is, a dummy coded one if a country has experienced a change in its Polity IV score of at least three points. We also include the regional Polity IV score to proxy for the democratic values of the neighborhood (Gates et al., 2006) and allow for spillovers from neighboring countries with dummies indicating if at least one neighbor had a small conflict, armed conflict or war during a given year (Bosker and de Ree, 2014).

4. Empirical strategy

A. Conflict histories

We now develop an empirical framework that captures the ordinal nature of conflict, allows for a rich specification of conflict histories and includes variables that have history-dependent effects.

Dynamic switches among multiple states cannot be meaningfully estimated with linear models. Beck et al. (1998) show that separately specifying models of onset and ending of war is equivalent to a dynamic model of war incidence. However, many more linear models would be needed to study the transition among multiple states. The result would be unstable parameter estimates that are inefficiently estimated, potentially biased, and difficult to interpret. Further, if we believe that there is an underlying latent variable ('conflict') which is observed as an ordered outcome, then separate regressions can violate known parameter restrictions.[5] Hence, a non-linear framework is needed.

Some notation is in order to help fix ideas. As typical in an ordered setting, we observe a conflict outcome c_{it} which takes on $J+1$ different values in country i at time t. A specific outcome is $j \in \{0, 1, \ldots, J\}$. The outcomes are ordered by intensity (i.e., peace, small conflict, armed conflict, civil war) and are generated by a continuous latent variable c^*_{it} with J cut points $\alpha_1 < \cdots < \alpha_j < \cdots < \alpha_J$ to be estimated later. The first outcome is $c_{it} = 0$ if $-\infty < c^*_{it} < \alpha_1$, the intermediate outcomes are $c_{it} = j$ if $\alpha_j < c^*_{it} < \alpha_{j+1}$ with $0 < j < J$, and the last outcome is $c_{it} = J$ if $\alpha_J < c^*_{it} < \infty$.

Next, define the associated $J \times 1$ vector of one period conflict histories as $\mathbf{h}_{i,t-1} \equiv (h_{1,i,t-1}, \ldots, h_{j,i,t-1}, \ldots, h_{J,i,t-1})'$. The typical element of $\mathbf{h}_{i,t-1}$ is $h_{j,i,t-1} \equiv 1[c_{i,t-1} = j]$, that is, an indicator of whether the past outcome is identical to outcome j.

Contrary to the standard approach, our latent variable model of interest has a full set of history dependent effects

$$c^*_{it} = \mathbf{x}'_{it}\boldsymbol{\beta} + \mathbf{h}'_{i,t-1}\boldsymbol{\rho} + (\mathbf{x}_{it} \otimes \mathbf{h}_{i,t-1})'\boldsymbol{\gamma} + \mu_i + \epsilon_{it} \tag{1}$$

where $\mathbf{x}_{it}$ is a column vector of regressors without a constant, $\mathbf{h}_{i,t-1}$ is defined above, and the Kronecker product simply accounts for all possible interactions between $\mathbf{x}_{it}$ and $\mathbf{h}_{i,t-1}$. We include country level unobserved effects, μ_i, whose identification we discuss below. Typically we will partition the vector $\mathbf{x}_{it} = (\mathbf{x_1}'_{it}, \mathbf{x_2}'_{it})'$, so that some variables are history dependent and others are not (e.g., proxy controls and time dummies). We are only interested in the estimated coefficients inasfar as they define the relevant probabilities.

Conditional on the covariates and the conflict history we have three different types of outcome probabilities: $\Pr[c_{it} = 0|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}] = \Pr[c^*_{it} \leq \alpha_1|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}]$, $\Pr[c_{it} = j|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}] = \Pr[\alpha_j < c^*_{it} \leq \alpha_{j+1}|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}]$, and $\Pr[c_{it} = J|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}] = \Pr[c^*_{it} > \alpha_J|\mathbf{x}_{it}, \mathbf{h}_{i,t-1}]$. We have to be more explicit in the notation since we are interested in the transition and continuation probabilities of the various states. For simplicity, just focus on the j-th intermediate outcome where $0 < j < J-1$, then w.l.o.g. we can define continuation,

[5] This is a version of the misnamed "parallel regression assumption" in ordered probit models. If the outcome is an ordered response, then the predicted probabilities of falling below a certain cut point *must* be increasing in the outcome j for all values of the covariates (Wooldridge, 2010, p. 658). If all the coefficients can vary in each state, then this meaningless result cannot be ruled out.

escalation and deescalation from an initial state $j+p$ to outcome j as:

$$\begin{aligned}\Pr[c_{it}=j|\mathbf{x}_{it},h_{j+p,i,t-1}=1]=&F\left[\alpha_{j+1}-\mathbf{x}'_{it}\boldsymbol{\beta}-\rho_{j+p}-(\mathbf{x}_{it}\times h_{j+p,i,t-1})'\boldsymbol{\gamma}_{j+p}-\mu_i\right]\\&-F\left[\alpha_{j}-\mathbf{x}'_{it}\boldsymbol{\beta}-\rho_{j+p}-(\mathbf{x}_{it}\times h_{j+p,i,t-1})'\boldsymbol{\gamma}_{j+p}-\mu_i\right]\end{aligned} \tag{2}$$

where we have escalation if $p<0$, continuation if $p=0$ and deescalation if $p>0$. The case of $p=0$ is often also called 'persistence.' $F(\cdot)$ is some continuous symmetric c.d.f. which is defined by the distribution of the error terms, ϵ_{it}.

The purpose of this entire exercise is to be able to define the partial effect of a particular $x_{k,it}\in\mathbf{x}_{it}$ on one of the transition probabilities defined above. It should now be straightforward to see that these are the derivatives of a particular probability with respect to $x_{k,it}$. For example, in the case of continuing in the past state j we have

$$\begin{aligned}\frac{\partial}{\partial x_k}\left(\Pr[c_{it}=j|\mathbf{x}_{it},h_{j,i,t-1}=1]\right)=&(\beta_k+\gamma_{j,k})\Big(f\left[\alpha_j-\mathbf{x}'_{it}\boldsymbol{\beta}-\rho_j-(\mathbf{x}_{it}\times h_{j,i,t-1})'\boldsymbol{\gamma}_j-\mu_i\right]\\&-f\left[\alpha_{j+1}-\mathbf{x}'_{it}\boldsymbol{\beta}-\rho_j-(\mathbf{x}_{it}\times h_{j,i,t-1})'\boldsymbol{\gamma}_j-\mu_i\right]\Big),\end{aligned} \tag{3}$$

where $f(\cdot)$ is the p.d.f. of $F(\cdot)$.

We still lack a formal definition of state-dependence. In binary models, state dependence is the probability of an event happening when the event happened before minus the probability of the event when it did not happen before net of all other observed and unobserved factors. With ordered outcomes it is no longer that simple. We need to account for the fact that there are several ways of entering into a particular state. Inspired by the labor literature (Cappellari and Jenkins, 2004), we estimate state-dependence as the difference between experiencing a particular state if it has occurred before and a weighted average of the ways of entering this state when it has not occurred before.

Formally, define state dependence in state j as follows:

$$S_j=(NT)^{-1}\sum_i^N\sum_t^T\left(\Pr[c_{it}=j|\mathbf{x}_{it},h_{j,i,t-1}=1]-\sum_{r\neq j}\omega_{rj}\Pr[c_{it}=j|\mathbf{x}_{it},h_{r,i,t-1}=1]\right), \tag{4}$$

where the weights, ω_{rj}, are the normalized class frequencies (the number of observations that can potentially make the switch, normalized to sum to unity). We expect state dependence to increase with higher conflict intensities. The higher the level of conflict, the more difficult it becomes to leave states that have a destructive nature.

B. Dynamic ordered probit with endogeneity

Identification of endogenous regressors and their partial effects under the presence of heterogeneity and first-order dynamics is tricky in non-linear settings. Researchers often opt for linear instrumental variable methods to keep things simple, but here we trade simplicity for a better understanding of the dynamics.

To model the ordered conflict outcome, we combine correlated random effects (CRE) and a control function (CF) approach with dynamic panel ordered probit models. Dynamic models with correlated random effects where all regressors are strictly exogenous have been studied by Wooldridge (2005), among others, and endogeneity was introduced into these types of dynamic binary choice models by Giles and Murtazashvili (2013). To the best of our knowledge, we are the first to employ a CRE approach with an endogenous regressor in an dynamic ordered setting. Note that this approach does not work with unbalanced panels. In the robustness section, we also specify linear models for comparison.

We incorporate two specific features into the general formulation from the preceding section. First, we add an endogenous regressor (the ratio of bilateral aid to GDP) and, second, we interact this variable with the one-period conflict history. We do not consider other interactions. Hence, our model of interest becomes

$$c_{1it}^{*} = \mathbf{z}_{1it}'\boldsymbol{\beta}_1 + \beta_2 a_{2it} + \mathbf{h}_{1i,t-1}'\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + u_{1it} \tag{5}$$

where $\mathbf{z}_{1it}$ is a column vector of strictly exogenous variables, a_{2it} is the endogenous aid to GDP ratio, λ_{1t} are time dummies, and everything else is defined as before. We added subscripts to each variable or vector if they belong to the main equation of interest (1) or the reduced form (2). We assume that the model is dynamically complete once the first-order dynamics are accounted for and that the error term is free of serial correlation. The process starts at $s < 0$ and is observed over $t = 0, \dots, T$. We always lose the first period, so in eq. 5 and from now on estimation runs over $t = 1, \dots, T$.

The endogenous aid to GDP ratio has the following linear reduced form

$$a_{2it} = \mathbf{z}_{1it}'\boldsymbol{\alpha}_1 + \mathbf{z}_{2it}'\boldsymbol{\alpha}_2 + \mu_{2i} + \lambda_{2t} + u_{2it} \tag{6}$$

where $\mathbf{z}_{2it}$ is a vector of instruments that is relevant and excluded from the main equation. Our instrument is generated from bilateral regressions. We discuss its construction in detail in the next section. Note that under mild conditions a generated instrument works just like a regular instrument: the parameters are estimated consistently and the limiting distributions are the same (see Wooldridge, 2010, p. 125). Hence the standard errors need not be adjusted, they are only likely to be noticeably biased in small samples.

We assume that the reduced form heterogeneity can be expressed as $\mu_{2i} = \bar{\mathbf{z}}_i'\boldsymbol{\psi} + b_{2i}$, where $b_{2i}|\mathbf{z}_i \sim \mathcal{N}(0, \sigma^2_{b_2})$ and $\mathbf{z}_i \equiv (\mathbf{z}'_{1it}, \mathbf{z}'_{2it})' \equiv (\mathbf{z}'_{i1}, \mathbf{z}'_{i2}, \ldots, \mathbf{z}'_{iT})'$ is a vector of all strictly exogenous variables in all time periods. Plugging this into eq. 6 gives

$$a_{2it} = \mathbf{z}'_{1it}\boldsymbol{\alpha}_1 + \mathbf{z}'_{2it}\boldsymbol{\alpha}_2 + \bar{\mathbf{z}}'_i\boldsymbol{\psi} + \lambda_{2t} + \nu_{2it} \tag{7}$$

where $\nu_{2it} = b_{2i} + u_{2it}$ is the new composite error term. It is well known that the coefficients on the time-varying covariates in eq. 7 are numerically equivalent to the linear fixed effects model, making this a very robust specification (Wooldridge, 2010, p. 332).

Following Rivers and Vuong (1988) and Giles and Murtazashvili (2013), joint normality of (u_{1it}, u_{2it}) conditional on $\mathbf{z}_i$ with $Var(u_{1it}) = 1$, $Cov(u_{1it}, u_{2it}) = \tau$, and $Var(u_{2it}) = \sigma^2_{u_2}$ implies that we can rewrite our model of interest as

$$c^*_{1it} = \mathbf{z}'_{1it}\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}'_{1i,t-1}\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + \omega u_{2it} + \epsilon_{1it}, \tag{8}$$

where we define $\omega = \tau/\sigma_{u_2}$.

Note that $u_{1it} = \omega u_{2it} + \epsilon_{1it} = \omega(\nu_{2it} - b_{2i}) + \epsilon_{1it}$, so our equation of interest is contaminated by both the first stage errors and the associated unobserved heterogeneity. The role of ν_{2it} is to "correct" for the contemporaneous endogeneity between the two equations, while b_{2i} allows for feedback from the unobserved effect in the reduced form.

If we let $b_{1i} = \mu_{1i} - \omega(\nu_{2it} - u_{2it})$ be the composite unobserved effect, then the key question in non-linear dynamic models is what assumptions do we make about how the composite heterogeneity relates to the initial conditions $\mathbf{h}_{i0}$, the covariates $\mathbf{z}_i$ and the reduced form errors in all periods $\boldsymbol{\nu}_{2i}$?

Following Giles and Murtazashvili (2013), we assume that $b_{1i}|\mathbf{z}_i, \mathbf{h}_{i0}, \boldsymbol{\nu}_{2i} \sim \mathcal{N}(\mathbf{z}'_i\boldsymbol{\delta}_0 + \mathbf{h}'_{i0}\boldsymbol{\delta}_1 + \boldsymbol{\nu}'_{2i}\boldsymbol{\delta}_3, \sigma^2_d)$. This homoskedastic normal distribution implies that the composite heterogeneity is a linear function: $b_{1i} = \mathbf{z}'_i\boldsymbol{\delta}_0 + \mathbf{h}'_{i0}\boldsymbol{\delta}_1 + \boldsymbol{\nu}'_{2i}\boldsymbol{\delta}_3 + d_{1i}$ where $d_{1i}|\mathbf{z}_i, \mathbf{h}_{i0}, \boldsymbol{\nu}_{2i} \sim \mathcal{N}(0, \sigma^2_d)$. Plugging this into eq. 8 gives the final equation

$$\begin{aligned} c^*_{1it} = \;& \mathbf{z}'_{1it}\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}'_{1i,t-1}\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \omega\nu_{2it} \\ & + \lambda_{1t} + \mathbf{z}'_i\boldsymbol{\delta}_0 + \mathbf{h}'_{i0}\boldsymbol{\delta}_1 + \boldsymbol{\nu}'_{2i}\boldsymbol{\delta}_3 + d_{1i} + \epsilon_{1it}, \end{aligned} \tag{9}$$

which can be estimated by standard random effects ordered probit along with the cut points α_j which will result in scaled parameters (e.g., $\boldsymbol{\beta}_1/\sqrt{(1+\sigma^2_{d_1})}$ and so on, assuming the usual normalization of $Var(\epsilon_{1it}) = 1$ is applied).

A two-step approach means *i)* we first estimate the reduced form in eq. 7, obtain an estimate of the residuals $(\hat{\nu}_{2it})$ and the reduced form errors in all periods $(\hat{\boldsymbol{\nu}}_{2i})$, and then *ii)* plug these into eq. 9. The standard errors are bootstrapped over both stages to account for the estimation of the residuals in the first step. Note that the CF approach does not require interactions with the residuals unlike IV methods, making it somewhat less robust but potentially much more efficient (Wooldridge, 2010, p. 128).

In our case T is large which has two major implications. First, adding a new time-varying control variable means adding T additional regressors. Second, the initial conditions problem is not likely to be severe. Rabe-Hesketh and Skrondal (2013) provide simulation results for different ways of specifying the conditional density of the unobserved effect in the dynamic binary probit model. Inspired by their study, we experimented with constraints that can be placed on the two sequences $\mathbf{z}_i$ and $\hat{\boldsymbol{\nu}}_{2i}$. Our results suggest that allowing only the first few periods to have an independent effect and constraining the rest to the time averages yields results that are almost indistinguishable from the full model.[6]

The average partial effects (APEs) are derivatives of the expectation of our specification with respect to the distribution of b_{1i} (see Blundell and Powell, 2004; Wooldridge, 2005). The APEs can be different for each t. We usually average across all observations to obtain a single estimate.

C. Identification

We use political fractionalization in donor countries interacted with the probability of receiving aid as our primary source of exogenous variation at the donor-recipient level. Dreher and Langlotz (2015) show that government fractionalization interacted with this probability is a strong instrument for bilateral aid. Government fractionalization is defined as the probability that any two randomly-chosen deputies of the parties forming the government represent different parties (Beck et al., 2001).

The motivation for this instrument comes from three different strains of literature. First, government or legislative fractionalization has been shown to positively affect government expenditures (Roubini and Sachs, 1989). Within a coalition government, logrolling during the budgeting process will lead to higher overall government expenditures. Second, higher government expenditures also imply higher aid budgets (Brech and Potrafke, 2014). Third, higher aid budgets translate into higher aid disbursements (Dreher and Fuchs, 2011). The interaction with the probability of receiving aid then introduces variation across recipients. An interaction of this endogenous probability with an exogenous variable is itself exogenous, provided we include country and time fixed effects.

Most studies analyzing the effects of political fractionalization on government spending focus on parliamentary systems with proportional representation. This is because coalition governments are more likely to be generated by some systems rather than others. Electoral rules, in particular first-past-the-post (FPTP) rules, define if

[6] We conserve degrees of freedom by splitting the two vectors, so that in the case of the exogenous variables we have $\mathbf{z}_i^+ = (\mathbf{z}'_{i1}, \mathbf{z}'_{i2}, \ldots, \mathbf{z}'_{iR}, \bar{\mathbf{z}}_i^{+'})'$ where $R < T$ and $\bar{\mathbf{z}}_i^+ = \frac{1}{T-R-1}\sum_{t=R+1}^{T} \mathbf{z}_{it}$ is the time average after period R. The residual sequence, $\boldsymbol{\nu}_{2i}^+$, is computed analogously. Our results are not sensitive to the choice of R, as long as the first period is allowed to have its own coefficients. We typically set $R = 4$. We also included $\mathbf{z}_{i0}$ to little effect (as suggested by Rabe-Hesketh and Skrondal, 2013).

government can be fractionalized at all or if there is a single-party government which negotiates the budget process in some form of reconciliation process with the legislative body. Persson et al. (2007) present a model along these lines where majoritarian elections usually lead to single party government and less spending in equilibrium than proportional elections. Hence, we prefer government fractionalization over fractionalization of the legislature as an instrument in parliamentary systems with proportional representation.[7] For the few donors with FPTP systems – Canada, the UK, and the U.S. – we use legislative fractionalization as our preferred source of exogenous variation.[8]

Just as in Nunn and Qian (2014), our identification strategy can be related to a difference-in-difference (DiD) approach. We essentially compare the effects of aid induced by changes in political fractionalization in donor countries among regular and irregular aid recipients. We later also examine the parallel trends assumption inherent in our approach.

Applying this in a bilateral setting requires aggregating the bilateral variation in the instruments to the recipient-year level. We opt for a regression approach in which we predict aid bilaterally from the best linear combination of the two interacted instruments and then aggregate the bilateral predictions. Specifically, we predict aid from donor j to recipient i in year t in a bilateral regression:

$$a_{3ijt} = \theta_0 g_{3jt} + \theta_1 (g_{3jt} \times \bar{p}_{3ij}) + \xi_0 l_{3jt} + \xi_1 (l_{3jt} \times \bar{p}_{3ij}) + \mu_{3ij} + \lambda_{3t} + \varepsilon_{3ijt} \tag{10}$$

where g_{3jt} is government fractionalization, l_{3jt} legislative fractionalization and $\bar{p}_{3ij}$ is the pairwise probability of receiving aid. As discussed above g_{3jt} is typically zero in FPTP systems. For an identification consistent with our theoretical framework we set all FPTP observations of $g_{3jt} = 0$. Analogously, we set $l_{3jt} = 0$ in non-FPTP systems. Hence, we utilize only the system-relevant political fractionalization. The time-invariant probability is defined as $\bar{p}_{3ij} = \frac{1}{T} \sum_t^T \mathbf{1}[a_{3ijt} > 0]$, so that it contains the fraction of years in which recipient i received a positive amount of aid from donor j. We again added subscripts to indicate that this equation (3) precedes the others with index (2) and (1). We do not need to control for the endogenous level of $\bar{p}_{3ij}$ as it is captured by the recipient-donor fixed effects, μ_{3ij}. We then aggregate the predicted bilateral aid from eq. 10 across all donors in order to get predicted aid as a share of GDP at the recipient-year level. Hence, $\hat{a}_{2it} = \sum_j \hat{a}_{3ijt}$ is the instrument in eq. 7.

We may worry about what variation actually ends up in our constructed instrument. To be clear, it consists of three different components: *i)* the estimated donor-recipient

[7] Legislative fractionalization is defined similarly to government fractionalization. It gives the probability of randomly picking two deputies from the legislature that belong to different parties.

[8] France is an interesting case as it is a mixed system with two-round runoff voting. However, both government and legislative fractionalization vary for France. In a robustness test we also treat France in the same way as Canada, the UK, and the U.S. without a material impact on the results.

fixed effects aggregated over all donors, or $\sum_j \hat{\mu}_{3ij}$, *ii)* the estimated effects of those donor characteristics that do not vary across recipients and the time dummies aggregated over all donors, or $\sum_j \hat{\theta}_0 g_{3jt} + \sum_j \hat{\xi}_0 l_{3jt} + J\hat{\lambda}_{3t}$, and, finally, *iii)* the exogenous variation introduced by the two interaction terms aggregated over all donors, or $\sum_j \hat{\theta}_1(g_{3jt} \times \bar{p}_{3ij}) + \sum_j \hat{\xi}_1(l_{3jt} \times \bar{p}_{3ij})$. The first two are potentially endogenous, but we control for their influence in the estimation that follows. Donor fractionalization is the same across all recipients and will be swept out by the fixed effects (or time-averages) in the reduced form equation. Similarly, everything but the interaction terms will be swept out by the recipient effects and time effects.

Consider the influence of colonial ties for example. If a former colony receives more aid from its former colonizer, then this will be captured by a higher donor-recipient fixed effect and a higher probability to receive aid. Moreover, former colonizers may be more likely to intervene and act as "peacemakers." Both issues are no threat to our identification strategy, since these level effects are absorbed at the various stages. Our exclusion restriction would only be violated if a change in the political fractionalization of a former colonizer would lead to a different change in aid flows given to regular recipients as opposed to irregular recipients *and* this change in fractionalization would make the former colonizer more likely to intervene in one of these two groups. However, even this concern is mitigated by our exclusive focus on internal civil conflicts.

5. Results

A. Bilateral estimation

We begin by briefly discussing the bilateral regression which we use to construct the instrument. Recall that we regress aid received by each recipient from a particular donor on political fractionalization, its interaction with the probability of receiving aid, and a full set of country and time fixed effects. We estimate these models with the fraction of aid in GDP as the dependent variable (not in logs, since negative flows occur when loan repayments exceed new inflows).

The regression is estimated over 4,116 bilateral donor-recipient relations for which we have data, yielding a total of 129,348 observations.[9] These results are not intended to be interpreted causally on their own. They purely serve to "translate" the exogenous variation in donor characteristics into changes in aid disbursements at the recipient level, depending on how strongly a recipient depends on aid from each particular donor.

The estimated coefficients of our variables of interest are as follows (standard errors

[9]We do not constrain this estimation to the balanced sample we use later on for two reasons: *i)* in order to get the best possible estimate of this relationship, and *ii)* unbalancedness is not a problem in fixed effects regressions as long as selection is ignorable.

are reported parentheses below):

$$\hat{a}_{3ijt} = \cdots - \underset{(0.014)}{0.043} g_{3jt} + \underset{(0.058)}{0.227} (g_{3jt} \times \bar{p}_{3ij}) + \underset{(1.407)}{2.564} l_{3jt} - \underset{(1.426)}{2.936} (l_{3jt} \times \bar{p}_{3ij}). \quad (11)$$

The coefficients on the interaction terms are highly significant. Note that the negative sign on the second interaction coefficient is misleading. In both cases, increasing political fractionalization leads to more aid disbursements for nearly all of the sample. Interestingly, fractionalized parliamentary systems give more aid to regular recipients, whereas divided majoritarian systems give more aid to irregular recipients (which is in line with the result in Ahmed, 2016, for the case of the U.S.).[10]

The effects of political fractionalization are not as large as a cursory glance at the coefficients may suggest. To see this, consider a 10 percentage points increase of political fractionalization in a donor country when a recipient receives aid about two thirds of the time. Eq. 11 predicts that this increases the aid to GDP ratio by about 0.01 percentage point for aid from proportional systems ($0.1 \times [-0.043 + 0.227 \times 2/3] \approx 0.01$) and about 0.06 percentage points for aid from majoritarian systems ($0.1 \times [2.564 - 2.936 \times 2/3] \approx 0.06$). The increase in majoritarian systems tends to be larger, in part because it is estimated based solely on three of the biggest donors. We clustered standard errors at the donor-recipient level. The cluster-robust F-statistic of the interaction terms is about 10.83. Note that the constructed instrument will turn out to be considerably stronger once we aggregate to the country level, since we then add up many of these small changes in the aid to GDP ratio of recipients in any given year.[11]

B. Reduced form of aid

We now turn to country level estimates of the first stage relationship. Table II shows three reduced form regressions for aid to GDP which we obtain by estimating the equivalent fixed effects model of eq. 7. The residuals from these models are used as control functions in the main specifications which we estimate further below. The sample is now balanced at $T = 36$ (minus the initial period) and $N = 125$. This constitutes a much larger sample relative to the typical study in this field which often focuses exclusively on Sub-Saharan Africa or loses observations due to the inclusion of many controls. Our data contains countries experiencing some of the most severe and longest-running civil conflicts (e.g., Afghanistan, Iraq, Pakistan and many more).

Two things stand out in Table II. First, the estimated coefficients on the instruments

[10] An explanation could be that government fractionalization works mainly via its effect on the general budget and hence affects the volume of receipts of regular beneficiaries, while legislative fractionalization (e.g. divided government in the U.S.) results in amendments to the budget. The parties negotiating these amendments are likely to have different preferences over which countries should receive aid.

[11] We repeated this estimation using net aid including Other Official Flows (OOF). The results are qualitatively and statistically similar (not reported, available on request).

Table II
First stage regressions with generated IV

Variables	*Dependent Variable: Aid to GDP* (1)	(2)	(3)
Predicted aid to GDP ($\sum_j \hat{a}_{3ijt}$)	1.352***	1.234***	1.233***
	(0.088)	(0.067)	(0.068)
Selected Controls			
Log GDP per capita		-5.089***	
		(0.845)	
Log GDP			-5.114***
			(0.806)
Log Population			6.084***
			(2.306)
Additional Controls			
Country FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Summary Statistics			
Kleibergen-Paap F-statistic IV	233.5	336.2	331
$N \times T$	4375	4375	4375
T	35	35	35
N	125	125	125
Within-R^2	0.0412	0.0739	0.0763

Notes: The table shows the results of first stage regressions using a linear two-way fixed effects model. The instrument is the sum of predicted bilateral aid over all donors ($\sum_j \hat{a}_{3ijt}$) from eq. 11. Cluster robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

in all columns are always larger than one. Depending on the specification, a one percentage point increase in the predicted aid to GDP ratio leads to about a 1.3 percentage point increase in actual aid to GDP. Adding other controls moves the estimated coefficients a bit closer to unity. The size of the coefficient is not unusual. Related approaches in the trade and migration literature often yield coefficients that are sometimes below or near unity (Frankel and Romer, 1999) and sometimes considerably larger (Alesina et al., 2016). If the constructed instrument over-predicts the quantity in question, then the coefficient will be below unity, and *vice versa*. Not surprisingly, our aggregation of predicted bilateral flows tends to undershoot actual aid to GDP ratios and therefore has a multiplier above unity. Second, the aggregated instrument is highly relevant. The cluster-robust F-statistics always exceed the conventional level of about ten by an order of magnitude, which is also not unusual in comparable applications.[12]

[12] Without added controls Frankel and Romer (1999) report an F-statistic of 98.01 for their predicted trade shares. In a completely different context, Gordon (2004) reports F-statistics up to 291 when

Hence, it seems safe to conclude that aggregating many small changes in aid induced by electoral outcomes in donor countries interacted with the probability of receiving aid constitutes a powerful instrument of development aid.

No single donor or recipient is driving this result. Two graphs in Appendix A report the regression coefficients and the confidence intervals we obtain when we drop each donor (Figure A-1) or each recipient (Figure A-2) one at a time in the bilateral sample, aggregate the data to the country-level, and re-run the first stage regression. The estimates vary only within an extremely narrow band. A similar question regarding the strength of our instrument is whether this association is driven mainly by recipients with a highly fragmented donor pool. The variation of aid induced by changes in divided donor governments is likely to be higher for recipients with many active donors. To investigate this, we measure donor fragmentation by a Herfindahl index and the combined share of the three largest donors. We then interact predicted aid to GDP with a dummy indicating whether the recipient has a higher donor fragmentation than the sample mean. The coefficients on predicted aid to GDP and the first stage F-statistics are qualitatively similar to those in Table II. The interaction term itself is insignificant, irrespective of whether we use the Herfindahl or the share of the three largest donors. Hence, our instrument does not draw its power from any one donor, any one recipient, or settings where many donors are active at the same time.

A number of other concerns could be raised regarding the strength and validity of our identification strategy. Fractionalized governments and legislatures could be giving more aid to countries that are politically closer, more open to trade or that receive a lot of foreign direct investment. Any (conditional) correlation of our instrument with these variables might weaken the strength of our instrument and could violate the exclusion restriction in some circumstances. However, note that a violation of the exclusion restriction requires not only that fractionalization-induced aid disbursements vary in tandem with other variables and that these variables determine conflict, it also requires that these other variables have heterogeneous effects on regular and irregular aid recipients.[13]

Table A-4 in Appendix A includes UNGA voting alignment (based on ideal points as in Bailey et al., 2017), trade openness, and FDI inflows over GDP as additional controls into the first stage regressions. We now limit the sample to the subset of countries that is covered by the added variables. Column (1) re-estimates our base specification from above. Columns (2) to (4) progressively add the additional controls. The last column includes all added controls. The strength of our instrument is virtually unaffected. The F-statistic of the instrument varies between 30 to 70. Likewise, the estimated coefficients

instrumenting actual changes in Title 1 spending per pupil in U.S. districts with constructed values.

[13] Other factors, such as global economic crises, may both depress aid and lead to more fragmented governments in rich countries. However, if these factors uniformly affect all recipients in a given year, they are captured by the time effects.

of predicted aid are very stable around 1.3. Closer voting alignment and more openness increase aid flows, while the coefficient on FDI flows is not significant at conventional levels. Adding all variables increases the model fit by about six percentage points. While these measures clearly matter for aid allocation, they do not capture the exogenous variation that is contained in our instrument.

C. Baseline results

We focus on a basic set of controls in our main specifications but allow for (fixed) unobserved country heterogeneity, unobserved time effects, and instrument our time-varying variable of interest. All of these three measures take care of omitted variables and contemporaneous endogeneity. We present two sets of estimates for our baseline results. Table III reports the regression results and Table IV shows the associated average partial effects of aid on different transitions.

Consider the regressions in Table III first. In column (1) we show the estimates without additional controls, next we add GDP per capita, and then we allow GDP and population to have different effects in the last column. The results are interesting in a couple of respects. The coefficients of aid to GDP and its interactions with the lagged states are virtually the same across all three specifications (even though the underlying scale factors differ). The regressions suggest *i)* that the intensifying effect of aid on conflict is stronger if the country experienced a small conflict in the year before, and *ii)* that the effect is not statistically different from the base level (i.e., peace in the previous year) for higher conflict intensities. We also find reasonably strong evidence of the endogeneity of aid. The residuals from the first stage have the opposite signs and similar magnitudes as the coefficients on the base level. This suggests that we would find no evidence of an effect of aid on conflict, if we would not correct for endogeneity (this is indeed the case). In control function methods, testing the null that the coefficient on the residuals is zero corresponds to a Hausman test of endogeneity which does not depend on the first stage, hence the reported bootstrap standard errors will be conservative. Nevertheless, we can reject the null of endogeneity at the 10% significance level.

We prefer column (3) since it accounts for scale effects (conflicts with more battle-related deaths occur in larger countries) and measures the net effect of higher aid intensity at a given income level. Nevertheless, none of the coefficients on the selected time varying controls are significant. Most existing studies use pooled methods (including the sensitivity analysis by Hegre and Sambanis, 2006) which rely on between-country differences. Given that recipient level CREs and conflict histories are included in all of our specifications, log GDP (whether per capita or not) and log population do not seem to contribute much additional information. Note that we defer the discussion of the lagged states to the next subsection where we analyze the persistence and duration of conflicts

Table III
Second stage ordered probit regressions, CRE and CF

Variables	(1)	(2)	(3)
	Dependent Variable: Ordered Conflict		
Aid to GDP (a_{2it})	0.0728*	0.0729	0.0721
	(0.0432)	(0.0491)	(0.0468)
Residuals ($\hat{\nu}_{2it}$)	-0.0847*	-0.0865*	-0.0863*
	(0.0442)	(0.0501)	(0.0480)
Interactions with Lagged States			
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0220***	0.0209**	0.0212**
	(0.00792)	(0.00841)	(0.00866)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.00843	-0.0104	-0.0106
	(0.0187)	(0.0191)	(0.0191)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00229	-0.00139	-0.00229
	(0.0240)	(0.0252)	(0.0248)
Lagged States			
Small Conflict ($h_{1,i,t-1}$)	0.582***	0.578***	0.576***
	(0.0744)	(0.0752)	(0.0794)
Armed Conflict ($h_{2,i,t-1}$)	2.110***	2.098***	2.107***
	(0.181)	(0.185)	(0.190)
Civil War ($h_{3,i,t-1}$)	3.429***	3.406***	3.424***
	(0.227)	(0.230)	(0.241)
Selected Controls			
Log GDP per capita		0.253	
		(0.339)	
Log GDP			0.289
			(0.310)
Log Population			-0.0478
			(0.509)
Additional Controls			
Recipient CRE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Residual CRE	Yes	Yes	Yes
Initial States	Yes	Yes	Yes
Summary Statistics			
$N \times T$	4375	4375	4375
T	35	35	35
N	125	125	125

Notes: The table shows the results of an ordered probit model with correlated random effects and a control function approach. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

at various intensities.

We have strong reasons to trust the estimates presented in Table III. We allow for quasi-fixed effects, first-order multi-state dynamics, and correct for contemporaneous heterogeneity. In theory, additional controls may help justifying the identifying assumptions regarding the instrument but there is no *ex ante* reason to expect that our estimates are still biased. Including more variables also comes at a cost as we described earlier. Each additional variable consumes several degrees of freedom due to how the unobserved heterogeneity is modeled. We return to the issue of additional controls in the robustness section.

TABLE IV
Average partial effect of aid on transition probabilities

	To State			
From State	Peace	Small Conflict	Armed Conflict	Civil War
Peace	-1.639	1.154	0.475	0.010
	(1.056)	(0.743)	(0.317)	(0.009)
Small Conflict	-2.867**	1.439**	1.358**	0.070
	(1.359)	(0.701)	(0.646)	(0.048)
Armed Conflict	-1.379	-0.539	1.333	0.585
	(1.174)	(0.474)	(1.099)	(0.498)
Civil War	-0.401	-0.970	-0.618	1.989
	(0.387)	(0.734)	(0.551)	(1.494)

Notes: Based on column (3) in Table III. Panel bootstrap standard errors in parentheses, computed with 200 replications. Rows sum to zero. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

To assess the magnitude of the implied effects we have to turn to partial effects as opposed to estimated coefficients. Table IV reports estimates of APEs for a one percentage point change in aid on the various transition probabilities (see eq. 3 in Section 4). Note that – by definition – each row sums to zero. Although all estimates above the diagonal are positive and those below negative, we find no statistically significant evidence in favor of an effect of aid on conflict when countries are entirely at peace or engaged in a conflict with more than 25 BDs.

Aid has significant adverse effects in volatile environments which are not entirely peaceful but also not (yet) fully engaged in armed conflict. There, more aid makes peace less likely, but a continuation of small conflict and a transition to armed conflict more likely. A one percentage point increase in the ratio of foreign aid to GDP leads to about a 1.4 percentage point increase in the probability of transitioning from small conflict to armed conflict.[14] The same increase in aid also significantly increases the likelihood of

[14]We might be concerned that the effect of aid on the transition from small conflict to armed conflict is driven by a small subset of observations. However, there are about 50 switches behind this estimate and more than 300 observations behind each of two lower switches.

remaining in a small conflict (by about 1.4 percentage points) and makes a transition to peace much less likely (about -2.9 percentage points).[15]

The size of this effect is best understood in conjunction with a typical change in aid flows. The average aid to GDP ratio in our sample is about 5% and the within standard deviation is also close to 5% (when we exclude recipients who receive more than half their GDP in foreign aid, e.g., Liberia 2008, Palau 1994, 1995). Mali, for example, experienced a one standard deviation increase in its aid to GDP ratio in 1994 when the share of aid to GDP increased from about 8% to 13%. At the same time, there was an escalation from small conflict to armed conflict. Consistent with this observation, our model predicts an increase in the probability of transitioning from small conflict to armed conflict of about 7 percentage points. Aid increases of this magnitude are rare. Only in about 3% of the sample they exceed five percentage points but changes around one percentage point are more common (about 14% of the sample). In Uganda, for example, aid increased by about one percentage point on two occasions (1981 and 2002). In both cases, the country experienced an escalation of conflict.

D. Persistence, state-dependence and duration

Table V shows the average transition probabilities as they are predicted by our preferred specification.[16] The diagonal of this matrix shows the predicted persistence rates and the off-diagonal elements are the escalation and deescalation probabilities, respectively. Note that we define persistence and continuation in analogy, so that persistence is simply the estimated probability of remaining in a particular state. The matrix provides nearly all the terms needed to estimate state dependence as in eq. 4 apart from the weights. Recall that state dependence measures the effect of the state on itself after accounting for observed and unobserved differences in the population (e.g., the destructive effects of unemployment, after netting out that the unemployed may have different characteristics than the employed).[17] It is conceptually distinct from persistence which, in theory, could be entirely driven by observed and unobserved characteristics.

We find strong evidence of state dependence in each of the four states, even after controlling for observed and unobserved heterogeneity. The bootstrapped standard errors are many times smaller than the estimated effects of each state. State dependence in armed conflict and civil war is moderately high and very similar (we cannot reject the null that these two estimates are the same). For both types of conflict, the sheer fact that

[15] The size of the estimated effects are also in line with recent estimates by Besley and Persson (2011b), Crost et al. (2014), and Nunn and Qian (2014). However, de Ree and Nillesen (2009) find that an increase in aid flows by 10% decreases the probability of continuation of conflict by about eight percentage points.

[16] Table V can be directly compared to the observed data shown in Table I and the difference between these two is a basic measure of goodness of fit.

[17] The literature typically distinguishes between three sources of state dependence: heterogeneity, serial correlation, and true state dependence.

Table V
Estimated transition probabilities and state dependence

	To State			
From State	Peace	Small Conflict	Armed Conflict	Civil War
Peace	79.954***	16.344***	3.657***	0.045*
	(1.902)	(1.536)	(0.739)	(0.024)
Small Conflict	61.751***	27.463***	10.496***	0.290**
	(2.857)	(2.293)	(1.454)	(0.126)
Armed Conflict	21.783***	32.690***	39.749***	5.778***
	(4.412)	(2.268)	(4.388)	(1.246)
Civil War	3.485	13.835***	51.102***	31.578***
	(2.215)	(3.186)	(3.173)	(4.941)
State Dependence	40.794***	8.890***	32.380***	30.765***
	(2.693)	(1.635)	(4.326)	(4.872)

Notes: Based on column (3) in Table III. Panel bootstrap standard errors in parentheses, computed with 200 replications. The upper four rows sum to 100%. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

a country finds itself in conflict implies that the probability of remaining in conflict rises by about 30 percentage points. Comparing these estimates with the persistence probabilities shown on the diagonal is particularly instructive. State dependence accounts for the bulk of persistence in armed conflict and civil war, but much less so in small conflict and peace. Note that the literature typically combines armed conflicts and civil war which would increase our estimates of persistence (and probably also of state-dependence) in the combined state.

Taking a truly dynamic approach allows us to bridge another distinction that is often drawn in the conflict literature: event models versus duration models. First-order Markov models can be compared to discrete time duration models with a constant hazard rate (e.g., Cappellari and Jenkins, 2004).[18] The expected duration of peace is about five years. Most conflicts are relatively short-lived on average. Small conflicts last about 1.4 years, armed conflict about 1.7 years, and civil wars about 1.5 years. We are predicting conflicts that last longer than three years only after about the 95th percentile (and longer than five years after the 99th percentile). This may seem short compared to other findings in the literature but it is worth bearing in mind that we distinguish between different types of conflict that are often lumped together. A conflict cycle that goes from small over armed conflict to outright civil war and back is perfectly compatible with the duration typically found in the literature (e.g., Collier and Hoeffler, 2004b).[19]

[18] To see the equivalence, recall that the hazard rate is the probability that the current state will end, or $\Pr(T_i = t | T_i \geq t)$. A discrete time-homogeneous Markov chain has a constant hazard rate with a well defined expectation. The probability of exiting a particular state is geometrically distributed with $\Pr[T_i = t] = p_{ii}^{t-1}(1 - p_{ii})$. The expected survival time in state i is simply $\mathbb{E}[T_i] = 1/(1 - p_{ii})$ and the quantile function is $Q(r) = F^{-1}(r) = \ln(1 - r)/\ln(p_{ii})$ where r is the percentile of interest.

[19] Also note that our estimates under-predict persistence relative to the observed data, in part because

E. Identification assumptions and falsification

Our local average partial effect compares the effects of politically induced differences in bilateral aid between regular and irregular aid recipients. This raises the question whether the parallel trends assumption inherent in DiD approaches is satisfied, or if spurious non-linear trends are at work. Our identification strategy is only valid if the heterogeneous response to the instrument generated by the regularity of aid receipts is constant over time. This is not a concern in our case, although it is an issue in related work.[20]

Figure A-3 in the Appendix A show the time series of our political measures in the upper panel, and the time series of conflict and the aid to GDP ratios split by quartiles of the probability to receive aid. While our measures of political fractionalization, bilateral aid, and conflict are trending up towards the middle of the studied period, the trends are remarkably parallel at the different levels of aid dependency. Only non-linear trends of aid and conflict in highly aid-dependent countries which coincide with trends in donor fractionalization would be a threat to our identification strategy. Such trends are absent in our data, while the common trends are absorbed by the time dummies.

We also conduct several placebo tests. Our finding that aid leads to an escalation of conflict rests on the coincident timing of politically-induced aid flows and the observed conflict histories. Randomizing aid flows along various dimensions allows us to break this temporal structure. As an added advantage we also obtain Monte Carlo p-values for our coefficients of interest. We shuffle the data along four dimensions. We randomly re-assign the aid to GDP ratio by exchanging i) all observations in the sample, ii) the entire time series between countries, iii) years within countries, and iv) countries within years. The rest of the data is left unchanged. Note that we ignore the first stage variability. Randomizing this stage as well would introduce weak IV bias and lead to non-central distributions. All of the four randomizations break our causal chain. The aid flows of another country, time period, or both cannot possibly have caused the observed conflict but spurious trends along particular dimensions would persist.

Figure A-4 reports the results from 5,000 Monte Carlo simulations per randomization strategy. For each placebo test, we report the distribution of the coefficients on the interaction terms. The results are unambiguous. Our findings are not driven by global trends, cross-sectional dependence, or selection of countries into regular aid receipts. Our estimated coefficients on the interaction of aid with small conflict are far to the right of simulated distributions, with exact p-values that are considerably smaller than 0.05. Consistent with our main results, we find no evidence that the effect of aid is different in societies that experienced an armed conflict or a civil war in the preceding year.

we average out the effects of observed and unobserved heterogeneity.

[20] See Christian and Barret (2017) who show that non-linear trends could be driving the positive effect of food aid on conflict reported in Nunn and Qian (2014).

6. Extensions

We present a number of extensions which subject our main findings to several robustness checks and perturbations. First, we compare the ordered probit estimator to standard linear models. Second, we examine the sensitivity of our results to the underlying definition of the key variables. Third, we include a variety of additional controls. Finally, we examine the role of humanitarian aid. We only briefly survey the results; all corresponding tables are relegated to Appendix A.

A. Linear estimation

The proposed dynamic ordered probit model is reasonably demanding to estimate and one might be concerned that our findings are driven by the structure we impose on the data ('identification by functional form'). Table A-5 addresses this issue. Here we ignore the ordinal nature and estimate our base specification using different linear approaches. Recall that least squares is not suitable for ordinal outcomes if the number of outcomes is not large and the error distribution is not approximately normal, among other issues.

All first order effects of aid on conflict are similar to the non-linear models. Column (1) in Table A-5 shows that, just as in the non-linear models, we find no effect if we estimate the fixed effects OLS counterpart to our dynamic specification when ignoring the endogeneity of aid. Column (2) then uses a control function approach to correct for the endogeneity of aid and recovers a positive first order effect of aid on all conflict outcomes. Column (3) illustrates the well-known equivalence of control function (CF) and instrumental variables (IV) approaches.[21]

The models with interaction terms confirm our initial findings. As columns (5) and (6) show, once we correct for the endogeneity of aid, the estimated coefficient is positive and significant. The coefficients on the three interaction terms are numerically similar, no matter if we use the control function estimator or not. In column (6), when we use a standard IV approach, the interaction effects become much less precisely estimated while the signs and magnitudes are broadly stable. The CF estimator requires only one first stage estimation to correct for popular transformations (such as squares or interactions) of the endogenous variable. The IV estimator instead requires us to generate many additional instruments to run as many additional first stage regression as we have interaction terms. As a result, the IV estimator is much less efficient but imposes fewer assumptions (Wooldridge, 2010, pp. 128–129). Given the stability of the estimated coefficients, this difference appears to be immaterial in our case.

[21] In static models CF and IV approaches yield numerically identical results. However, here we specify the first stage of the CF estimator without controlling for the lagged states.

B. Definition of variables

We now turn to the sensitivity of our results with respect to the operationalization of our key variables. In Table A-6 we alter the construction of our conflict and aid measures. Column (1) addresses the potential concern that while our newly developed measure is a step forward, we might not have gone far enough. One type of violence which we have so far neglected is terrorism. We now include country-year observations with a positive number of terror attacks[22] but less than 25 BD in the category one (small scale conflict) of our ordinal measure. In column (2) we combine categories two and three, since several studies only distinguish between peaceful countries and countries with more than 25 BDs. In both cases the results are qualitatively similar to our main findings.

Next, we compare our approach to the industry standard, where peace and small conflict are combined in one category. This eliminates the possibility to distinguish between truly peaceful countries and countries that experience small conflict. In line with our expectations, neither the level estimates nor the interaction effects are statistically significant in column (3). This is also true for the APEs.

Column (4) changes the definition of aid. So far, we have only focused on ODA. Here we include OOF to capture a broader concept of financial inflows from abroad, which does not affect our results. In columns (5) and (6) we exclude Canada, the UK and the U.S. We do so for two reasons. First, for those three countries we use legislative fractionalization rather than government fractionalization as an IV for bilateral aid. In order to rule out that our results depend on this choice, we estimate our preferred specification for the remaining 25 DAC donors. Second, these three donors could differ from the rest of the DAC donors in how they disburse aid to countries in conflict (e.g., if they are important to the U.S.).[23] Column (5) uses ODA, while Column (6) uses ODA with OOF. In each case, the estimated coefficients and APEs are in line with our preferred specification.

Last but not least, we code variants of the small conflict category by excluding one of the constituting variables each time (e.g., riots, assassinations). Our results are not driven by one single dimension of small conflict. As Table A-7 shows, we obtain quantitatively identical results for all four perturbations.

C. Additional controls

In Table A-8 we extend the set of control variables. Column (1) examines influence of conflict in the immediate regional neighborhood. We find little evidence of spillover effects, although such peer effects are generally difficult to identify. Columns (2) to (5) examine if political institutions affect the link between aid and conflict. This comes at

[22]From START (National Consortium for the Study of Terrorism and Responses to Terrorism) (2016).
[23]Our second stage results are also not driven by individual recipients.

the cost of a reduced sample.[24] None of the political variables alter our main results. Column (6) shows that GDP growth makes conflict less likely but does not affect the relationship between aid and conflict.

We strongly prefer our baseline estimates with country and time effects over the results reported in Table A-8. Many of the added variables can be considered "bad controls" in the sense that they themselves could be outcomes of development aid. As cases in point, political instability, classification as a democracy, or GDP growth have all been causally linked to aid in the past. The inclusion of outcomes on the right hand side creates a selection problem which can completely distort the estimated causal effect.[25]

D. Humanitarian and food aid

We now briefly examine the role of humanitarian aid and – its main component – food aid. Although humanitarian aid protects vulnerable populations, it is also easily captured by rebel groups and thus directly affects the opportunity costs of fighting.

Humanitarian aid represents about 6.5% of overall aid in our sample. To estimate its influence, we first calculate the correlation of humanitarian aid as a share of GDP with aggregated predicted aid to GDP (our instrument) and then the correlation with the part our instrument that is solely driven by exogenous variation.[26] The correlation of humanitarian aid to GDP with aggregated predicted aid to GDP is 0.12 but falls to 0.02 when the exogenous component is isolated. This leads us to conclude that our identification strategy does not rely on humanitarian aid.

Next we consider the role of food aid in particular. We focus on how the effect of U.S. food aid differs from the results of overall aid presented here. Table A-9 presents the results of simple replication and modification exercises using the data from Nunn and Qian (2014). Column (1) shows that our results are qualitatively similar in the matched sample of 103 recipient countries over the period from 1975 to 2007. In column (2), we then replicate a version of their main specification, where U.S. food aid is instrumented with U.S. wheat production interacted with the probability of receiving U.S. aid. However, we exchange their binary conflict indicator with our ordinal measure of conflict and include the appropriate interactions.[27] In line with their results, we find that U.S. food aid increases the probability of conflict across the board. Column (3) then removes the top

[24] The Polity IV score is not available for cases of foreign "interruption" (code -66) and lacks data for island countries. We lose, e.g., Afghanistan, Iraq, Cambodia, and Lebanon.

[25] See Angrist and Pischke (2008) for a discussion of this problem. A similar reasoning could be used to prefer the short specification in column (1) of Table III over the other two columns. Note that the inclusion of log GDP and log population hardly makes a difference in the estimates and both variables have insignificant coefficients, so that this distinction is immaterial for our main results.

[26] We regress our instrument on a full set of time and country fixed effects, and obtain the residual.

[27] Note that our framework does not allow for the large set of controls used in Nunn and Qian (2014). However, the corresponding OLS coefficients only vary in a narrow band, no matter if we specify the original long regression or the short regression (as in column (2) of Table A-9).

category from our dependent variable. This hardly affects our conclusions.

Last but not least, we conduct a falsification test to figure out if the identifying variation overlaps between our estimates of the effect of total ODA and the established effect of U.S. food aid. This should not be the case. Donor fractionalization of the 28 DAC donor countries should not predict U.S. food aid. Likewise, wheat production in the U.S. should not predict total ODA disbursed by the 28 DAC donors, but only a small part of U.S. overall aid. Table A-10 shows that this is reflected in the data. Hence, our primary finding that bilateral development aid promotes the continuation of small conflicts and an escalation of small to armed conflicts is quite different from the local average partial effect of U.S. food aid highlighted previously.

7. Conclusion

This paper studies the effects of development aid on conflict. While there is a large literature on the topic, it typically separates the onset of a conflict from its continuation and neglects smaller acts of violence. This misses important dynamics which our paper makes an effort to expose.

Our results show that the effects of bilateral aid are heterogeneous with respect to the different intensity levels of conflict. Whereas aid increases the probability that a conflict escalates from a low level of political violence to armed conflict, we find little evidence in favor of an adverse effect of aid in truly peaceful countries. Aid does also not seem to affect the transition probabilities once a country experiences armed conflict or civil war. These results underline the importance of separating truly peaceful situations from countries exposed to small conflict. If we do not account for this distinction, we would fail to detect an effect of aid on conflict.

These findings call for care when devising aid policies for countries affected by conflict. Particular care has to be exercised when aid is given to countries where turmoil is already present but armed conflict has not yet erupted. Our results suggest that aid might be more harmful than helpful in these situations, despite best intentions. Our analysis focuses on overall official development assistance. Future research could examine what type of assistance can be given to countries with persistent low-intensity conflicts so as to actually foster peace. Achieving this goal will require more research on the exact channels at play.

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Appendix A

Table A-1
Included donor countries, in alphabetical order

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

Table A-2
Included recipient countries, in alphabetical order

Afghanistan, Albania, Algeria, Angola, Antigua and Barbuda, Argentina, Bahamas, Bahrain, Bangladesh, Barbados, Belize, Benin, Bhutan, Bolivia, Botswana, Brazil, Brunei Darussalam, Burkina Faso, Burundi, Cambodia, Cameroon, Cape Verde, Central African Republic, Chad, Chile, China, Colombia, Comoros, Congo, Costa Rica, Cuba, Cyprus, Democratic Republic of Congo, Djibouti, Dominica, Dominican Republic, Ecuador, Egypt, El Salvador, Equatorial Guinea, Ethiopia, Fiji, Gabon, Gambia, Ghana, Grenada, Guatemala, Guinea, Guinea-Bissau, Guyana, Haiti, Honduras, India, Indonesia, Iran, Iraq, Israel, Ivory Coast, Jamaica, Jordan, Kenya, Kiribati, Lao, Lebanon, Lesotho, Liberia, Madagascar, Malawi, Malaysia, Maldives, Mali, Malta, Marshall Islands, Mauritania, Mauritius, Mexico, Micronesia, Mongolia, Morocco, Mozambique, Namibia, Nepal, Nicaragua, Niger, Nigeria, Oman, Pakistan, Palau, Panama, Papua New Guinea, Paraguay, Peru, Philippines, Rwanda, Saint Kitts and Nevis, Saint Lucia, Saint Vincent and the Grenadine, Samoa, Sao Tome and Principe, Senegal, Seychelles, Sierra Leone, Singapore, Solomon Islands, Somalia, South Africa, Sri Lanka, Sudan, Suriname, Swaziland, Syria, Tanzania, Thailand, Togo, Tonga, Trinidad and Tobago, Tunisia, Turkey, Uganda, Uruguay, Vanuatu, Venezuela, Vietnam, Zambia, Zimbabwe.

Table A-3
Summary statistics

Variables	Mean	SD	Min	Max	N
Panel A: Bilateral Data					
Aid to GDP (in percent)	0.19	1.40	-5.68	228.67	131964
Aid to GDP (with OOF, in percent)	0.19	1.49	-25.71	228.67	131964
Government Fractionalization	0.30	0.27	0.00	0.83	141789
Legislative Fractionalization (FPTP only)	0.06	0.17	0.00	0.69	151906
Probability to Receive	0.46	0.37	0.00	1.00	152208
Probability to Receive (with OOF)	0.45	0.36	0.00	1.00	152208
Panel B: Country Data					
Aid to GDP (in percent)	4.95	8.84	-2.95	241.69	4500
Aid to GDP (with OOF, in percent)	5.10	9.10	-10.89	241.69	4500
Log of GDP	16.19	2.10	11.39	22.97	4500
Log of Population	8.17	2.24	2.50	14.11	4500
Log of GDP per capita	7.96	1.12	5.08	11.49	4500
Polity IV (revised)	-0.14	6.79	-10.00	10.00	3670
Political Instability	0.18	0.39	0.00	1.00	3723
Regional Polity IV	-0.56	5.79	-9.00	10.00	3723
Neighbor in Small Conflict	0.40	0.49	0.00	1.00	4500
Neighbor in Armed Conflict	0.34	0.47	0.00	1.00	4500
Neighbor in War	0.16	0.36	0.00	1.00	4500

Notes: All measures of foreign aid to GDP have a maximum well in excess of 200%. This maximum is driven by Palau. Together with other pacific islands, Palau is part of the Compact of Free Association with the United States and receives foreign assistance greatly exceeding its GDP. Without Palau, the maximum falls to slightly above 100% (due to Liberia). Negative numbers are repayments of loans.

TABLE A-4

Robustness: First stage

	Dependent Variable: Aid to GDP				
VARIABLES	(1)	(2)	(3)	(4)	(5)
Predicted aid to GDP ($\sum_j \hat{a}_{3ijt}$)	1.319***	1.384***	1.244***	1.318***	1.307***
	(0.219)	(0.165)	(0.228)	(0.219)	(0.171)
Selected Controls					
Log GDP	-4.042***	-3.980***	-4.222***	-4.045***	-4.151***
	(0.968)	(0.962)	(0.907)	(0.966)	(0.913)
Log Population	4.855**	6.029**	5.531**	4.923**	6.505***
	(2.393)	(2.460)	(2.227)	(2.397)	(2.306)
UNGA Voting Alignment		2.084***			1.793***
		(0.525)			(0.473)
Trade Openness			0.045***		0.040***
			(0.010)		(0.009)
FDI Inflows / GDP				0.037	0.021
				(0.028)	(0.024)
Additional Controls					
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes
Summary Statistics					
Kleibergen-Paap F-statistic IV	36.12	70.39	29.76	36.34	58.22
Within-R^2	0.113	0.145	0.152	0.114	0.176
$N \times T$	3080	3080	3080	3080	3080
T	35	35	35	35	35
N	88	88	88	88	88

Notes: The table shows the results of first stage regressions using a linear two-way fixed effects model. The instrument is the sum of predicted bilateral aid over all donors ($\sum_j \hat{a}_{3ijt}$) from eq. 11. Cluster robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A-5

Robustness: Different linear estimation schemes

	Estimation Method:					
VARIABLES	(1) FE-OLS	(2) CRE-CF	(3) FE-2SLS	(4) FE-OLS	(5) CRE-CF	(6) FE-2SLS
Aid to GDP (a_{2it})	-0.0011	0.0104*	0.0114*	-0.0012	0.0103*	0.0116*
	(0.0011)	(0.0055)	(0.0058)	(0.0009)	(0.0054)	(0.0061)
Residuals ($\hat{\nu}_{2it}$)		-0.0117**			-0.0117**	
		(0.0059)			(0.0060)	
Interactions with Lagged States						
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)				0.0058**	0.0059*	0.0077
				(0.0028)	(0.0033)	(0.0073)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)				-0.0108	-0.0107	-0.0125
				(0.0120)	(0.0122)	(0.0162)
Civil War ($a_{2it} \times h_{3,i,t-1}$)				-0.0026	-0.0025	-0.0096
				(0.0054)	(0.0130)	(0.0104)
Lagged States						
Small Conflict ($h_{1,i,t-1}$)	0.2506***	0.2501***	0.2486***	0.2271***	0.2263***	0.2174***
	(0.0306)	(0.0308)	(0.0306)	(0.0342)	(0.0355)	(0.0439)
Armed Conflict ($h_{2,i,t-1}$)	1.1201***	1.1193***	1.1231***	1.1707***	1.1695***	1.1841***
	(0.0797)	(0.0813)	(0.0789)	(0.0996)	(0.1000)	(0.1144)
Civil War ($h_{3,i,t-1}$)	1.7902***	1.7896***	1.7899***	1.8116***	1.8105***	1.8457***
	(0.0856)	(0.0962)	(0.0835)	(0.0878)	(0.0962)	(0.1027)
Summary Statistics						
$N \times T$	4375	4375	4375	4375	4375	4375
T	35	35	35	35	35	35
N	125	125	125	125	125	125

Notes: All columns include recipient and time fixed effects. Clustered standard errors in parentheses for all columns but column (2) and (5), where we report panel bootstrap standard errors in parentheses, computed with 200 replications. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A-6

Robustness: Alternate measures of conflict and foreign aid

	Perturbations on LHS or RHS:					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	w/ Terror	only 25 BDs	UCDP-PRIO	w/ OOF	No Anglo Saxon	No Anglo Saxon w/ OOF
Aid to GDP (a_{2it})	0.0832*	0.0324	0.0571	0.0467	0.272	0.106
	(0.0453)	(0.0401)	(0.0426)	(0.0418)	(0.194)	(0.0655)
Residuals ($\hat{\nu}_{2it}$)	-0.0905**	-0.0442	-0.0539	-0.0607	-0.296	-0.122*
	(0.0443)	(0.0407)	(0.0433)	(0.0431)	(0.197)	(0.0671)
Interactions with Lagged States						
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0101	0.0197**		0.0209**	0.0308**	0.0234**
	(0.0100)	(0.00814)		(0.00836)	(0.0148)	(0.00954)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.0172	-0.00814	-0.0258	-0.0113	-0.0209	-0.0127
	(0.0202)	(0.0167)	(0.0201)	(0.0181)	(0.0329)	(0.0197)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00747		-0.0202	-0.00331	-0.0284	-0.00194
	(0.0254)		(0.0264)	(0.0178)	(0.0475)	(0.0215)
Lagged States						
Small Conflict ($h_{1,i,t-1}$)	0.741***	0.531***		0.575***	0.578***	0.573***
	(0.0775)	(0.0788)		(0.0809)	(0.0807)	(0.0819)
Armed Conflict ($h_{2,i,t-1}$)	2.448***	2.260***	2.088***	2.105***	2.120***	2.114***
	(0.220)	(0.189)	(0.173)	(0.185)	(0.196)	(0.185)
Civil War ($h_{3,i,t-1}$)	3.798***		3.334***	3.434***	3.478***	3.442***
	(0.266)		(0.229)	(0.239)	(0.253)	(0.240)
Summary Statistics						
$N \times T$	4375	4375	4375	4375	4375	4375
T	35	35	35	35	35	35
N	125	125	125	125	125	125

Notes: All columns include the log of GDP, log population, the initial states, CRE at the recipient level, residual CRE, time fixed effects. No Anglo Saxon excludes Canada, the UK and the U.S. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A-7

Robustness: 'Leave-one-out' test for small conflict coding

	Dependent Variable: Ordered Conflict			
	(1)	(2)	(3)	(4)
Variables	No Assassinations	No Guerrilla Warfare	No Purges	No Riots
Aid to GDP (a_{2it})	0.0774	0.0600	0.0933*	0.0630
	(0.0509)	(0.0434)	(0.0510)	(0.0469)
Residuals ($\hat{\nu}_{2it}$)	-0.0866*	-0.0688	-0.107**	-0.0695
	(0.0516)	(0.0446)	(0.0523)	(0.0479)
Interactions with Lagged States				
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0159*	0.0170**	0.0218**	0.0134*
	(0.00884)	(0.00797)	(0.00880)	(0.00785)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.0137	-0.00960	-0.0105	-0.0200
	(0.0181)	(0.0184)	(0.0191)	(0.0196)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00855	-0.00459	-0.00326	-0.0125
	(0.0288)	(0.0217)	(0.0255)	(0.0271)
Lagged States				
Small Conflict ($h_{1,i,t-1}$)	0.584***	0.383***	0.601***	0.766***
	(0.0773)	(0.0729)	(0.0785)	(0.0914)
Armed Conflict ($h_{2,i,t-1}$)	2.059***	1.953***	2.115***	2.157***
	(0.182)	(0.174)	(0.190)	(0.184)
Civil War ($h_{3,i,t-1}$)	3.391***	3.266***	3.431***	3.443***
	(0.232)	(0.227)	(0.240)	(0.245)
Summary Statistics				
$N \times T$	4375	4375	4375	4375
T	35	35	35	35
N	125	125	125	125

Notes: All columns include the log of GDP, log population, the initial states, CRE at the recipient level, residual CRE, time fixed effects. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table A-8
Robustness: Additional covariates

	Dependent Variable: Ordered Conflict					
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Aid to GDP (a_{2it})	0.0462	0.0422	0.0296	0.0401	0.0449	0.0413
	(0.0381)	(0.0387)	(0.0395)	(0.0391)	(0.0388)	(0.0374)
Residuals ($\hat{\nu}_{2it}$)	-0.0596	-0.0561	-0.0431	-0.0552	-0.0598	-0.0532
	(0.0392)	(0.0371)	(0.0392)	(0.0375)	(0.0373)	(0.0377)
Interactions with Lagged States						
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0212***	0.0226**	0.0251**	0.0239**	0.0242**	0.0198**
	(0.00803)	(0.0113)	(0.0102)	(0.0113)	(0.0113)	(0.00838)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.0127	-0.0111	-0.00683	-0.0107	-0.00775	-0.0165
	(0.0202)	(0.0205)	(0.0209)	(0.0192)	(0.0191)	(0.0170)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00432	-0.00278	-0.000690	-0.000633	-0.000977	-0.00462
	(0.0248)	(0.0275)	(0.0255)	(0.0270)	(0.0276)	(0.0239)
Added Controls						
Neighbor in Small Conflict	0.128*					
	(0.0673)					
Neighbor in Armed Conflict	0.0623					
	(0.0811)					
Neighbor in Civil War	0.165*					
	(0.0877)					
Political Instability		0.218***				
		(0.0769)				
Polity IV (revised)			-0.0102			
			(0.00830)			
Regional Polity IV				0.0118		
				(0.0159)		
Democracy					-0.334***	
					(0.124)	
GDP Growth						-1.043***
						(0.293)
Summary Statistics						
$N \times T$	4375	3708	3672	3708	3708	4375
N	125	103	102	103	103	125

Notes: All columns include the log of GDP per capita, the lagged states, the initial states, CRE at the recipient level, residual CRE, time fixed effects. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

TABLE A-9
Comparison: Our Results vs. Nunn and Qian (2014)

VARIABLES	*Dependent Variable: Ordered Conflict* (1) Aid to GDP	(2) U.S. Food aid	(3) U.S. Food aid
Aid (a_{2it})	0.0714	0.0129*	0.0114*
	(0.0614)	(0.00731)	(0.00663)
Residuals ($\hat{\nu}_{2it}$)	-0.0851	-0.0126*	-0.0112*
	(0.0587)	(0.00721)	(0.00656)
Interactions with Lagged States			
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0272**	-0.000720	-0.000711
	(0.0133)	(0.000808)	(0.000823)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.00235	-0.000322	0.00150
	(0.0234)	(0.000694)	(0.00108)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00211	0.000105	
	(0.0248)	(0.00112)	
Lagged States			
Small Conflict ($h_{1,i,t-1}$)	0.566***	0.701***	0.655***
	(0.0814)	(0.0774)	(0.0788)
Armed Conflict ($h_{2,i,t-1}$)	2.057***	2.041***	2.156***
	(0.176)	(0.149)	(0.157)
Civil War ($h_{3,i,t-1}$)	3.348***	3.268***	
	(0.216)	(0.209)	
Selected Controls			
Log GDP per capita	0.252	0.589	0.479
	(0.342)	(0.638)	(0.561)
Additional Controls			
Recipient CRE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Residual CRE	Yes	Yes	Yes
Initial States	Yes	Yes	Yes
Summary Statistics			
Kleibergen-Paap F-statistic IV	24.73	10.59	10.59
$N \times T$	3296	3296	3296
T	31	31	31
N	103	103	103

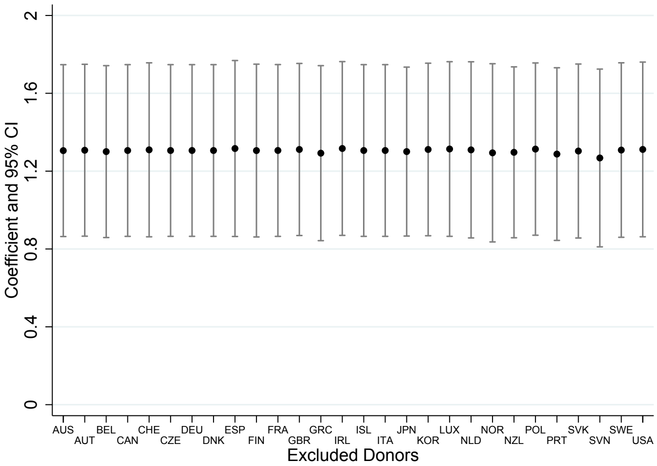
Notes: The table shows the results of an ordered probit model with correlated random effects and a control function approach. Panel bootstrap standard errors in parentheses, computed with 200 replications. All models also estimate J cut points and the variance of the random recipient effect.
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A-10
Falsification test

	Dependent Variable:	
	(1)	(2)
Variables	U.S. Food aid	Aid to GDP
Predicted aid to GDP ($\sum_j \hat{a}_{3ijt}$)	2.125	
	(3.126)	
Nunn and Qian (2014) IV		-0.0000156
		(0.0000327)
Selected Controls		
Log GDP per capita	-58.7451	-4.6827***
	(41.6526)	(0.8736
Additional Controls		
Country FE	Yes	Yes
Time FE	Yes	Yes
Summary Statistics		
Within-R^2	0.0460	0.1116
$N \times T$	3193	3193
T	31	31
N	103	103

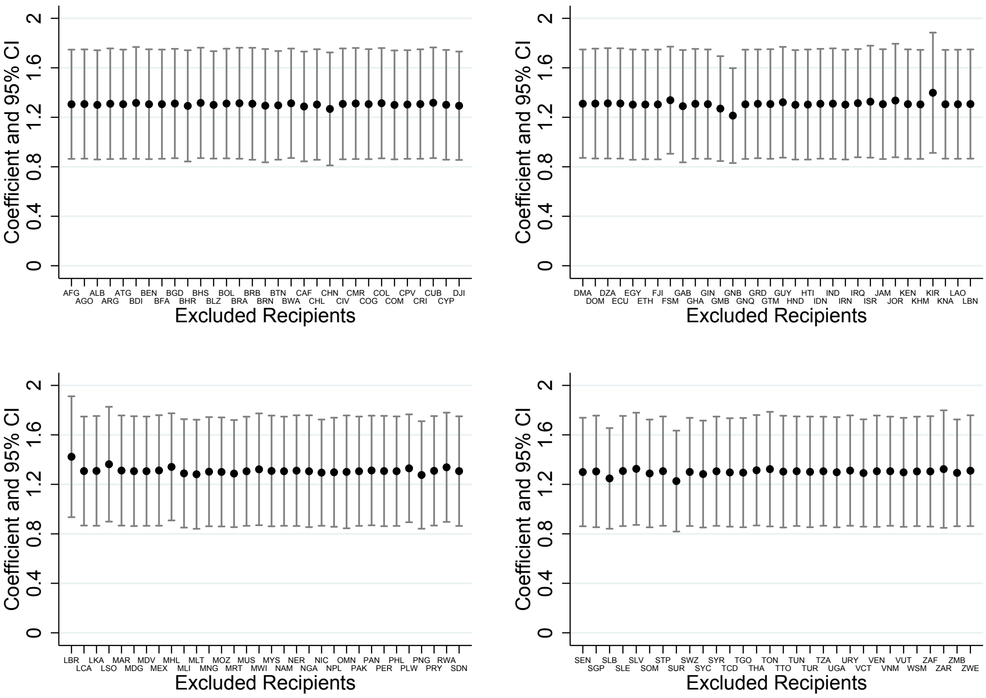
Notes: The table shows the results of first stage regressions using a linear two-way fixed effects model. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure A-1
Leave-one-out test: Donors



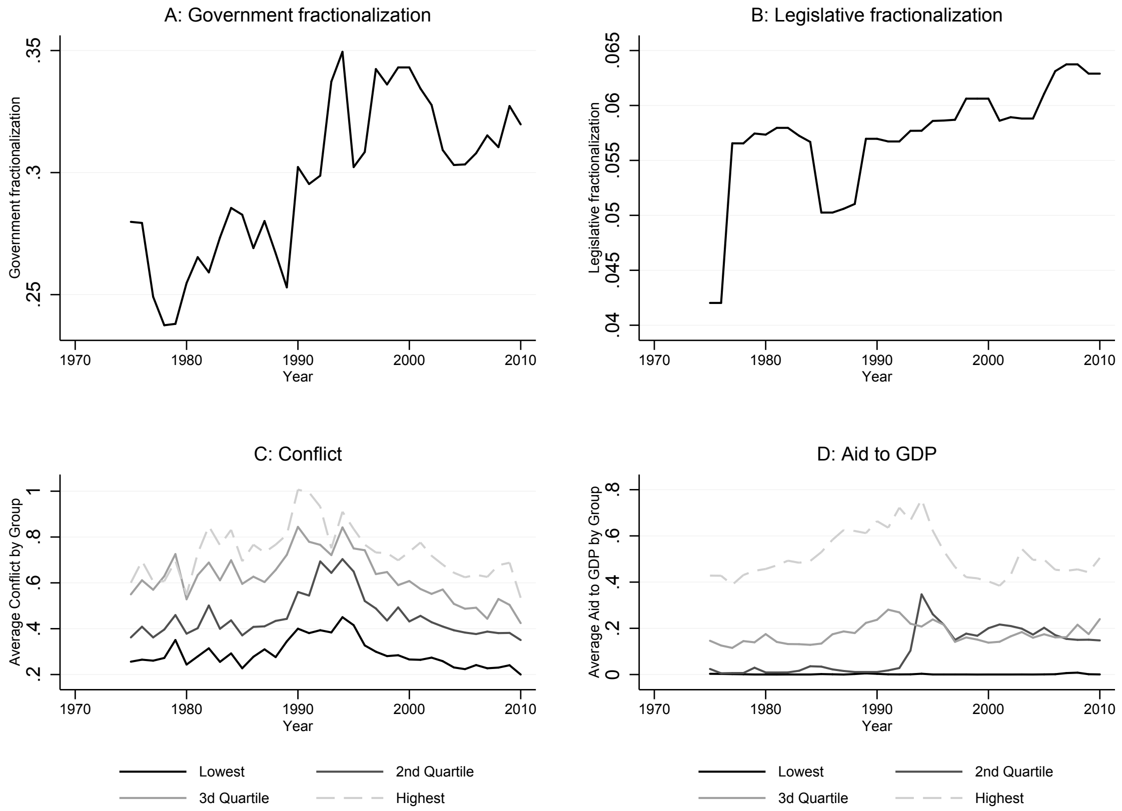
Notes: Each point in the figure represents the result of a regression of actual on predicted aid shares where one of the DAC donors has been excluded from the bilateral sample. Cluster robust standard errors are provided as error bars.

Figure A-2
Leave-one-out test: Recipients



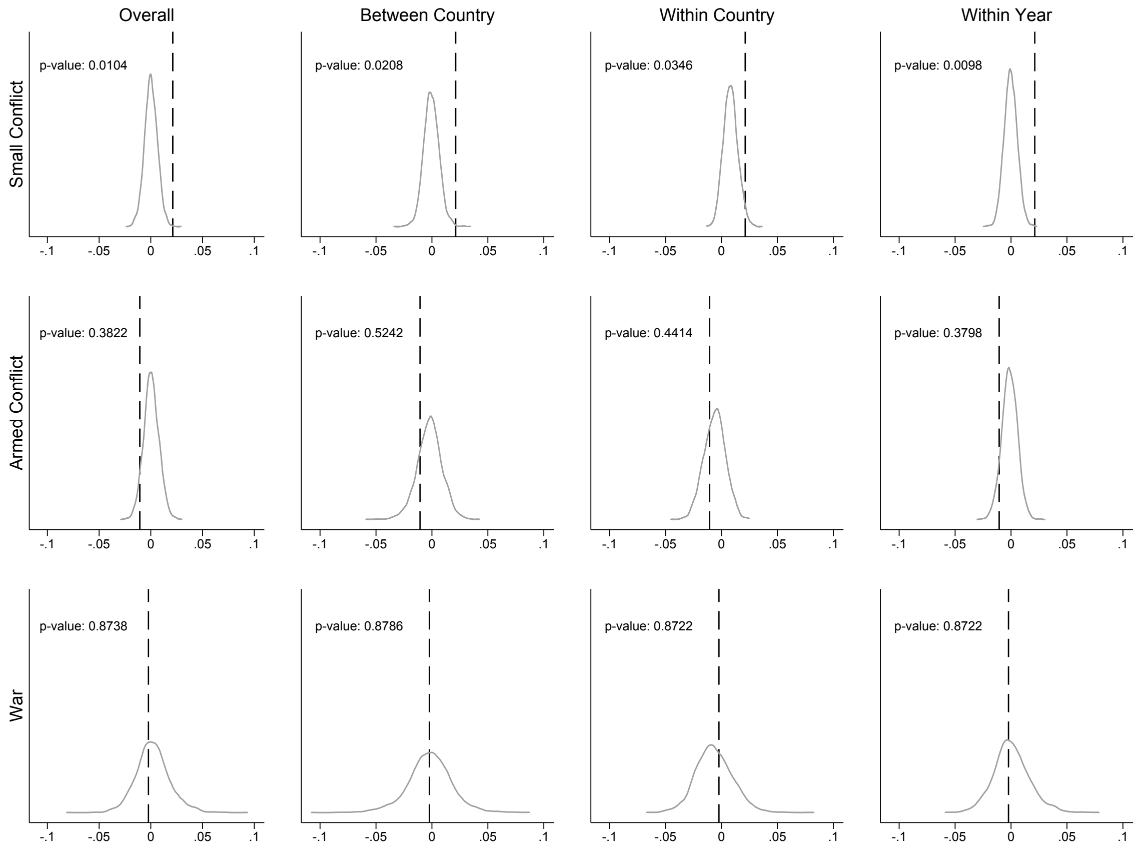
Notes: Each point in the figure represents the result of a regression of actual on predicted aid shares where one of the recipients has been excluded from the bilateral sample. Cluster robust standard errors are provided as error bars.

Figure A-3
Parallel trends



Notes: The figures shows the time series of average government fractionalization of donors (panel a), average legislative fractionalization of donors (panel b), conflict in recipient countries grouped by their probability to receive aid (panel c), and average aid to GDP ratios in recipient countries grouped by their probability to receive aid (panel d). Conflict measures the probability of experiencing any type of conflict (ranging from small conflict to civil war).

Figure A-4
Randomization Test

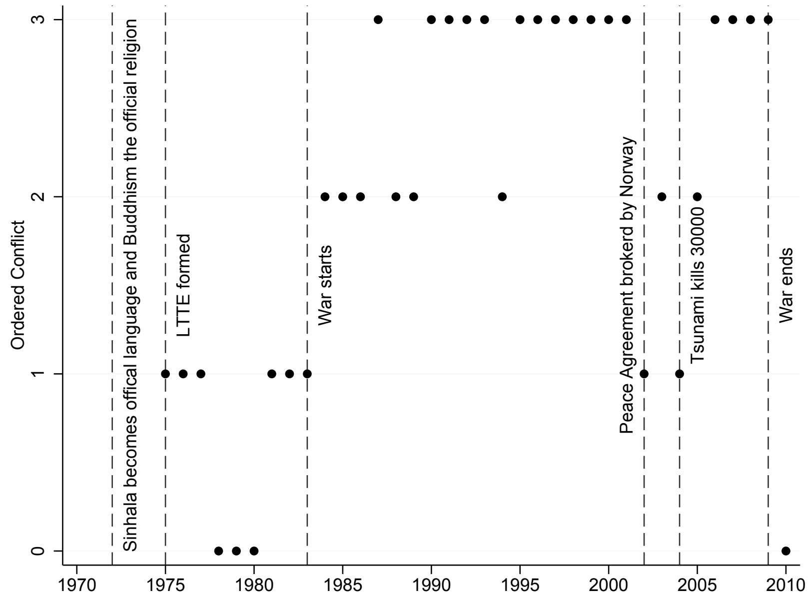


Notes: The figure shows the distribution of point coefficients based on 5,000 Monte Carlo replications per randomization strategy as described in the text. The *p*-values are estimated as the proportion of times that the absolute value of the *t*-statistics in replication data exceed the absolute value of the original *t*-statistic.

Appendix B: Short Case Study – Sri Lanka

Figure B-1 illustrates the dynamics of the civil conflict in Sri Lanka from 1975 to 2010 as captured by our measure. Sri-Lanka is an ideal case for two reasons: First, the conflict went through all conflict intensities. Second, the conflict turned violent in the mid-1970s right around the start of our sample and ended in 2010 at the end of our sample period.

FIGURE B-1
Conflict dynamics in Sri Lanka



The political conflict between the Sinhalese (about 73.8% of the population) and the Tamils (about 18% of the population, concentrated in the northeast of the country), has been lingering in Sri Lanka since the independence from the British Empire in 1948. The conflict started escalating in 1970 when the new constitution declared Sinhala as the official language and defined Buddhism as the official religion. The reaction of the Tamil (mainly Christians and Hindus with their own language) followed in 1972 when Ceylon became officially recognized as the Republic of Sri Lanka.[28] The Tamils formed the Tamil New Tigers Group to set up a separate homeland *Tamil Eelam* in the northeast of Sri Lanka which was accompanied by heavy riots (Banks and Wilson, 2015).[29]

In 1975, the New Tigers Group re-named itself the Liberation Tigers of Tamil Eelam (LTTE) spurring harsh responses by the government. Notice that while the UCDP-PRIO still codes the country as peaceful, our residual category of small conflicts already picks up the escalating violence. In 1978 the LTTE was outlawed. Interestingly, this coincides with a drop in our conflict measure to zero. The next escalation occurred in 1981, when riots erupted in Jaffna and a state of emergency was declared. Finally, in 1983 the first

[28]See http://www.cfr.org/terrorist-organizations-and-networks/sri-lankan-conflict/p11407.
[29]See http://www.aljazeera.com/focus/blanktemplate/2008/11/2008111061193133.html.

guerrilla attack, an ambush, was conducted by the LTTE, resulting in the death of 13 soldiers. The incident led to the eruption of riots and the killing of hundreds of people. The year 1984 then marks the first armed conflict observation in the UCDP-PRIO data set (category two in our measure).

The UCDP-PRIO data set does a good job for most of the following years in which the conflict is varying between armed conflict and civil war until the military defeat of the LTTE in 2009.[30] There are, however, two observations, one in 2002 and the other in 2004, in which UCDP-PRIO codes a peace observation. In both cases what follows is an armed conflict observation, and in 2006 a civil war observation. The two "peace" observations which in our approach fall into the small conflict category coincide the ceasefire mediated by Norway in 2002 and the split of LTTE, after which one part formed a pro-government party. The second slump in conflict intensity was 2004, in which more than 30,000 citizens died during the tsunami.[31] Yet in both cases violence never ceased but failed to reach the threshold of 25 BD. In 2002 there have still been several clashes between LTTE fighters and government soldiers, although both groups tried to adhere to the peace agreement.[32] In 2004 rioters burned down outlets of the government friendly splinter group who seceded from the LTTE (Banks and Wilson, 2015).[33]

Summing up, our measure captures the cyclical nature of the civil conflict between the LTTE and the government of Sri-Lanka rather well. Sri-Lanka was never actually completely at peace from 1981–2009 until the military defeat of the LTTE.

[30]New York Times 2009: http://www.nytimes.com/2009/05/19/world/asia/19lanka.html?_r=2&ref=global-home.

[31]See http://www.cfr.org/terrorist-organizations-and-networks/sri-lankan-conflict/p11407.

[32]Heidelberg Institute for International Conflict Research (HIIK) 2002: http://www.hiik.de/en/konfliktbarometer/pdf/ConflictBarometer_2002.pdf.

[33]HIIK 2004: http://www.hiik.de/en/konfliktbarometer/pdf/ConflictBarometer_2004.pdf.