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Abstract

This paper studies the effects of bilateral foreign aid on conflict escalation and de-escalation. We make three major contributions. First, we combine data on civil wars with data on low level conflicts in a new ordinal measure capturing the two-sided and multifaceted nature of conflict. Second, we develop a novel empirical framework. 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 that are exogenous to recipients. We establish that the effect of foreign aid on the various transition probabilities is heterogeneous and can be substantial. Receiving bilateral aid raises the chances of escalating from small conflict to armed conflict, but we find no 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 one of the main obstacles to development. Research on the causes of civil war has found that economic growth, commodity shocks, weak institutions or states, and various forms of ethnic heterogeneity are all correlated with conflict.[1] Poor and badly governed states that are prone to experience conflict both need and receive substantial amounts of development assistance. While a large and growing literature examines the effect of foreign aid on civil conflict, it has failed to generate a consensus on whether aid fuels or appeases conflict. Both sides have theoretical and empirical backing. On the one hand, there is the well-established notion that foreign aid increases rents and thus raises the value of capturing the state (Haavelmo, 1954; Hirshleifer, 1989; Grossman, 1991). On the other hand, foreign aid may increase the level of public good provision which raises the opportunity cost of violent activity (Becker, 1968; Grossman, 1991; Collier and Hoeffler, 2004b), or aid may strengthen the military capability of recipients (Fearon and Laitin, 2003).

Most of the extant literature considers conflict to be a binary state but distinguishes between the onset and continuation of conflict, as these may be driven by different factors. However, studying the onset and continuation of conflict separately is an imperfect substitute for analyzing an inherently dynamic problem (Beck et al., 1998). Splitting the sample is not only inefficient and does not exploit known restrictions on the switching probabilities, it is also not a good approach for dealing with persistent dependent variables such as civil war. More fundamentally, there is no empirical sense of escalation or de-escalation among different conflict intensities when the ordinal nature of conflict is not incorporated. Only the trivial case of a switch from peace to conflict and vice versa is usually accounted for. These distinctions matter. Small scale conflicts below the usual minimal threshold of 25 battle-related deaths often start a cycle of violence. In contrast, as we show below, a civil war never broke out in a society that was completely at peace in the year before.

In essence, we conjecture that neglecting smaller conflicts pollutes 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

[1] See, for example, Fearon and Laitin (2003), Collier and Hoeffler (2004b), Besley and Persson (2011b), Esteban et al. (2012), Bazzi and Blattman (2014), and Berman and Couttenier (2015).

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 they provide vital information about the government's repressive capabilities (Acemoglu and Wolitzky, 2014; Bueno de Mesquita, 2013). Foreign aid, in turn, may exacerbate violent tendencies in such environments but not when society is truly at peace. Establishing how this *causal* effect differs across conflict histories is the key objective of this paper.

Our empirical analysis introduces three novelties in terms of measurement, estimation and identification. First, we propose a new measure of conflict which depicts 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, we identify the effect of aid on conflict using characteristics of the electoral system of donor countries. Specifically, 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 can have 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. We draw several conclusions from this analysis.

Aid does not harm recipient countries by causing conflict across the board. While all estimates suggest that bilateral aid tends to fuel conflict, we find no 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 and seem to support the state-as-a-prize perspective, although it remains difficult to delineate the exact channels.

Aid is 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. Much like Burnside and Dollar (2000), who argue that aid is not effective in countries with bad policies, our findings suggest that aid is counterproductive when there is violent turmoil.

Our results underscore the importance of carefully modeling the dynamics of conflict. This echoes the recent literature (e.g., Nunn and Qian, 2014; Bazzi and Blattman, 2014; Berman and Couttenier, 2015) but our analysis goes significantly further and generates new insights. Escalation and de-escalation, 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 assistance

Economists usually think of foreign aid and civil conflict in terms of two opposing hypotheses. One hypothesis is that aid appeases, the other that aid fuels conflict. The direction of the overall effect boils down to how aid changes the calculus of citizens and governments. For citizens, aid may alter the opportunity costs of fighting (Becker, 1968; Grossman, 1991; 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 (Hirshleifer, 1988, 1989; 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 while having more to lose (e.g., Fearon, 2007; Besley and Persson, 2011b). 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 effect of export price shocks on conflict onset at the country-level, while Berman and Couttenier (2015) show that positive income shocks have a stabilizing effect 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 budget constraint if aid is sufficiently fungible (Collier and Hoeffler, 2007). While higher military spending should theoretically reduce the risk of conflict (Fearon and Laitin, 2003), empirical evidence on this channel is divided. Collier and Hoeffler (2006) find that increased military spending in post-conflict states raises the likelihood of renewed rebellion, while Dube and Naidu (2015) provide strong evidence of the state capacity effect at the micro-level. They find that U.S. military aid increases violence of paramilitary organizations that function as complements to government forces in Colombia, especially in election years, but has no discernible effect on guerrilla warfare.

Foreign aid raises the stakes. Standard contest theory argues that the state is a price that rebels want to capture (Haavelmo, 1954; Hirshleifer, 1988, 1989; Grossman, 1991). It predicts that conflict becomes more likely when aid receipts are higher as the expected gains from fighting increase (Grossman, 1991). Such arguments are pervasive in the literature on conflict over natural resources and many other contests. However, as Fearon (2007) points out, the equilibrium level of conflict may be independent of the income level if the revenue and opportunity cost effects cancel out. Dal Bó and Dal Bó (2011) show how this depends on the labor and capital intensity of production. Besley and Persson (2011b) introduce a model where these effects 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 examining the onset or continuation of civil conflict find an appeasing effect of aid (e.g., de Ree and Nillesen, 2009; Savun and Tirone, 2011; Ahmed and Werker, 2015). However, evidence to the contrary has been accumulating (e.g., Besley and Persson, 2011b; Dube and Naidu, 2015; Nunn and Qian, 2014). Nunn and Qian (2014), for example, suggest that food aid in particular can be used as rebel financing since it can (almost instantly) be captured. Their results show that U.S. food aid prolongs the duration of civil conflict but does not predict its onset. Finally, even rising opportunity costs can be compatible with 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 this change in incentives may want to sabotage aid if 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 de-escalation 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 small conflict 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.[2]

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 (Bueno de Mesquita, 2013). 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 (Acemoglu and Robinson, 2006; Esteban et al., 2012). 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.[3] 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

[2] This also applies to Besley and Persson (2011b). Their theory generates multiple states and their empirical analysis uses ordered outcome models, but does not account for conflict histories, history-dependent effects and persistence.

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

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.

C. Causal identification

The nexus 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; Dreher et al., 2016) 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 to instrument 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.[4] 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 as an instrument for bilateral aid flows in the context of growth regressions. We describe this strategy in detail below.

[4] A different strategy had been proposed by Ahmed and Werker (2015), who use oil prices to instrument aid flows from oil-producing Muslim to non-oil producing Muslim countries.

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 the Appendix.

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 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."

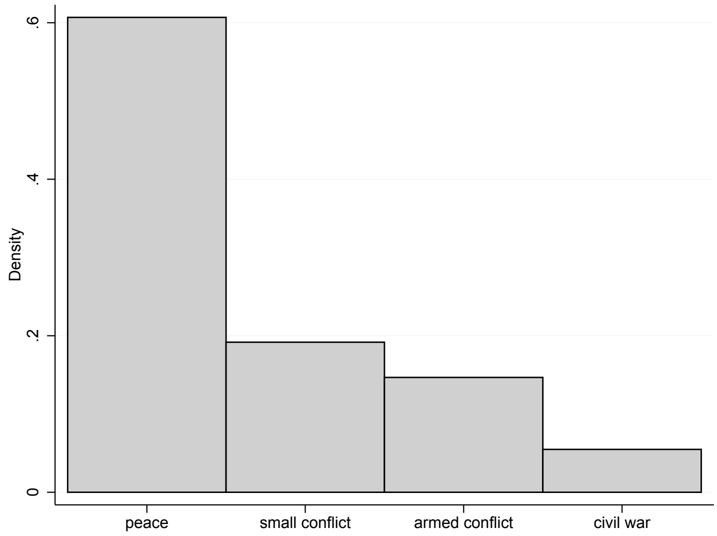
We propose a new ordinal measure of conflict with four states. For comparability, we begin with the standard UCDP-PRIO measure of civil conflict (Gleditsch et al., 2002). UCDP-PRIO defines civil conflict as a contested incompatibility that concerns the government or a territory in which armed force between two parties, one of which is the government, 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 were 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).[5] 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 between two parties one being the state (two-sided, state-centered).[6] Note that Besley and Persson (2011b) took a similar approach when they added one-sided state repression as an intermediate category to what we define as civil war.

The idea behind our ordinal variable is straightforward. Only a truly peaceful society is coded zero. Our measure takes on the value of one if at least one variable in the

[5]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.

[6]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).

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.

CNTS data set exhibits a positive value while there are less than 25 BDs. The next two categories follow the UCDP-PRIO measure. Conflicts with a minimum of 25 but less than 1,000 BDs are coded as two, while the civil war category, i.e., more than 1,000 BDs, takes on the value of three in our measure. 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. The Appendix presents the case of Sri Lanka as an illustration of our conflict measure.

A key advantage of this 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 which are 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 are not suited for our purpose. They do not allow us to study escalation and de-escalation since they have absorbing terminal states.

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

Table I
Unconditional Markov transition matrix (in percent)

	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%.

cyclical nature of conflicts is clearly visible. The highest switching probabilities are always into the next adjacent category, but chances of de-escalation into categories below are always greater than those of escalating to categories above. In fact, 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 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 discussion in the previous section, 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 percent. OOF includes flows by the official sector with a grant element of less than 25 percent or flows that are not primarily aimed at development. We do not examine multilateral aid which is typically a bit less than one third of all aid. The included donors and recipients are reported in Tables A-1 and A-2 in the Appendix.

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, or 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 score of at least three points (Gates et al., 2006). We also include the regional Polity score to proxy for the democratic values of the neighborhood (Gates et al., 2006) and an oil exporter dummy to partial out resource dependence. Last, we allow 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). Since the neighbor-in-conflict dummies and the regional polity score are undefined for island nations, we include an island dummy. Table A-3 contains summary statistics of all right hand side variables.

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.[7] 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 $h_{j,i,t-1} \equiv 1[c_{i,t-1} = j]$,

[7] 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.

that is, an indicator of whether the past outcome is identical to outcome j. We do not need a separate indicator for peace (i.e., $h_{0,i,t-1}$) since it is a linear combination of the other outcomes.

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 row 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, escalation and de-escalation 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 de-escalation 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}, but for now needs not be specified further.

We are also interested in the probabilities of de-escalating or escalation from the current state to *any* other lower or higher conflict state. These are

$$\begin{aligned}\Pr[c_{it} < j|\mathbf{x}_{it}, h_{j,i,t-1} = 1] =& \Pr[c^*_{it} \leq \alpha_j|\mathbf{x}_{it}, h_{j,i,t-1} = 1] = \\ & F\left[\alpha_j - \mathbf{x}'_{it}\boldsymbol{\beta} - \rho_j - (\mathbf{x}_{it} \times h_{j,i,t-1})'\boldsymbol{\gamma}_j - \mu_i\right]\end{aligned} \tag{3}$$

and

$$\begin{aligned}\Pr[c_{it} > j|\mathbf{x}_{it}, h_{j,i,t-1} = 1] =& \Pr[c^*_{it} > \alpha_{j+1}|\mathbf{x}_{it}, h_{j,i,t-1} = 1] = \\ & 1 - F\left[\alpha_{j+1} - \mathbf{x}'_{it}\boldsymbol{\beta} - \rho_j - (\mathbf{x}_{it} \times h_{j,i,t-1})'\boldsymbol{\gamma}_j - \mu_i\right].\end{aligned} \tag{4}$$

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

$$\begin{aligned} \frac{\partial}{\partial x_k}\left(\Pr[c_{it}=j|\mathbf{x}_{it},h_{j,i,t-1}=1]\right) =&(\beta_k+\gamma_{j,k})\left(f\left[\alpha_j-\mathbf{x}_{it}'\boldsymbol{\beta}-\rho_j-(\mathbf{x}_{it}\times h_{j,i,t-1})'\boldsymbol{\gamma}_j-\mu_i\right]\right. \\ &\left.-f\left[\alpha_{j+1}-\mathbf{x}_{it}'\boldsymbol{\beta}-\rho_j-(\mathbf{x}_{it}\times h_{j,i,t-1})'\boldsymbol{\gamma}_j-\mu_i\right]\right), \end{aligned} \tag{5}$$

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:

$$S_j=(NT)^{-1}\sum_i^N\sum_t^T\left(\Pr[c_{it}=j|\mathbf{x}_{it},h_{j,i,t-1}=1]-\sum_{r\neq j}\omega_{rj}\Pr[c_{it}=j|\mathbf{x}_{it},h_{r,i,t-1}=1]\right), \tag{6}$$

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 ordered setting. Our specifications are basically first-order Markov switching processes which allow persistence to vary with each conflict outcome and permit history-dependent effects of the endogenous regressor (aid). 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

$$c^*_{1it} = \mathbf{z}'_{1it}\boldsymbol{\beta}_1 + \beta_2 a_{2it} + \mathbf{h}'_{1i,t-1}\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + u_{1it} \tag{7}$$

where $\mathbf{z}_{1it}$ is a 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 $t = 0, \ldots, T$. We always lose the first period, so in eq. 7 and from now on estimation runs over $t = 1, \ldots, T$.

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

$$a_{2it} = \mathbf{z}'_{1it}\boldsymbol{\alpha}_1 + \mathbf{z}'_{2it}\boldsymbol{\alpha}_2 + \mu_{2i} + \lambda_{2t} + u_{2it} \tag{8}$$

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).

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. 8 gives

$$a_{2it} = \mathbf{z}'_{1it}\boldsymbol{\alpha}_1 + \mathbf{z}'_{2it}\boldsymbol{\alpha}_2 + \bar{\mathbf{z}}'_i\boldsymbol{\psi} + b_{2i} + \lambda_{2t} + u_{2it} \tag{9}$$

$$a_{2it} = \mathbf{z}'_{1it}\boldsymbol{\alpha}_1 + \mathbf{z}'_{2it}\boldsymbol{\alpha}_2 + \bar{\mathbf{z}}'_i\boldsymbol{\psi} + \lambda_{2t} + \nu_{2it} \tag{10}$$

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. 9 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

$$c^*_{1it} = \mathbf{z}'_{1it}\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}'_{1i,t-1}\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + \omega u_{2it} + \epsilon_{1it}, \tag{11}$$

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 $\boldsymbol{\nu}_{2i}$ (Wooldridge, 2005; Giles and Murtazashvili, 2013)? Assuming that the heterogeneity only relates to the reduced form errors gives rise to a random effects specification with Mundlak terms for the first stage residuals. Assuming that the composite heterogeneity is a linear function of all three gives rise to a dynamic correlated random effects approach. The initial conditions are not ignorable when T is small and have repercussion towards how flexibly we must treat the unobserved heterogeneity (Wooldridge, 2005). Hence, independence of $\mathbf{h}_{i0}$ and $\mathbf{z}_i$ is unlikely.

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. 11 gives the final equation

$$\begin{aligned} c^*_{1it} = \; & \mathbf{z}'_{1it}\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}'_{1i,t-1}\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \omega\nu_{2it} \\ & + \lambda_{1t} + \mathbf{z}'_i\boldsymbol{\delta}_0 + \mathbf{h}'_{i0}\boldsymbol{\delta}_1 + \boldsymbol{\nu}'_{2i}\boldsymbol{\delta}_3 + d_{1i} + \epsilon_{1it}, \end{aligned} \tag{12}$$

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. 9, 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. 12. The standard errors are bootstrapped. 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 relatively large (35) which has two major implications. First, adding a new time-varying control variable means adding T additional regressors. Second, the initial conditions problem is most likely not very severe (although currently no Monte Carlo studies exists for our setting). 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.[8]

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 (Nunn and Qian, 2014).

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 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

[8] We conserve degrees of freedom by splitting the two vectors, so that in the case of the exogenous variables we have $\mathbf{z}_i^+ = (\mathbf{z}_{i1}', \mathbf{z}_{i2}', \dots, \mathbf{z}_{iR}', \bar{\mathbf{z}}_i^{+'})'$ 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).

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.[9] For the few donors with FPTP systems – Canada, UK, and the U.S. – we use legislative fractionalization as our preferred source of exogenous variation.[10] This is also in line with Ahmed (2016) who shows that higher fractionalization in the U.S. House of Representatives leads to increased aid disbursements.

Similar to Nunn and Qian (2014) our identification strategy can be related to a difference-in-difference approach. We essentially compare the effects of aid induced by changes in political fractionalization in regular and irregular recipient countries. In fact, the probability is significantly correlated with the amount of aid receipts (Nunn and Qian, 2014; Dreher and Langlotz, 2015; Ahmed, 2016). On average, countries with a higher probability of receiving aid are also those to whom more aid is sent by the donors. Note that this does not trivially follow by definition since donors could also be continuously giving aid at a low level.

Applying this in a bilateral setting requires us to either *i)* compute aggregate probabilities of receiving aid, or *ii)* predict aid bilaterally and aggregate afterwards. We opt for the latter and proceed in two steps. First, we predict aid from donor j to recipient i in year t in a bilateral regression:

$$a_{3ijt} = \theta_0 g_{3jt} + \theta_1 (g_{3jt} \times \bar{p}_{3ij}) + \xi_0 l_{3jt} + \xi_1 (l_{3jt} \times \bar{p}_{3ij}) + \mu_{3ij} + \lambda_{3t} + \varepsilon_{3ijt} \tag{13}$$

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 FTPT observations of $g_{3jt} = 0$. Analogously, we set $l_{3jt} = 0$ in non-FPTP systems. Hence, we utilize only the system-specific 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}. Second, we aggregate the predicted bilateral aid from eq. 13 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. 9.

We may worry about what variation actually ends up in our constructed instrument.

[9] 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.

[10] 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, UK, and the U.S. without a material impact on the results.

To be clear, it consists of three different components: *i)* the estimated donor-recipient 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.

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 (as a share of recipient GDP) 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.[11] 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 are reported parentheses below):

$$\hat{a}_{3ijt} = \cdots - \underset{(0.014)}{0.043} g_{3jt} + \underset{(0.058)}{0.227} (g_{3jt} \times \bar{p}_{3ij}) + \underset{(1.407)}{2.564} l_{3jt} - \underset{(1.426)}{2.936} (l_{3jt} \times \bar{p}_{3ij}). \tag{14}$$

The coefficients on the interaction terms are highly significant. In both cases, increasing political fractionalization leads to more aid disbursements for nearly all of the sample. Interestingly, fractionalized parliamentary systems give more to regular recipients, whereas divided majoritarian systems give more to irregular recipients. Note that the effect of legislative fractionalization is not as large as a cursory glance at the coefficients may suggest. To see this, consider a 10 percentage points increase of political

[11] 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.

fractionalization in all donor countries when a recipient receives aid about two thirds of the time. Eq. 14 predicts that this increases the aid to GDP ratio by about one percentage point for aid from proportional systems ($0.1\times[-0.043+0.227\times 2/3] \approx 0.01$) and about six 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. Since this is our primary source of exogenous variation, it may be compared to the conventional rule-of-thumb level of ten. However, the constructed instrument will turn out to be considerably stronger at the unilateral country level.

We proceed by summing over all donors to get each recipients' predicted share of aid in GDP as described above. We repeated this estimation using net aid including Other Official Flows (OOF). The results are qualitatively and statistically similar (not reported, available on request).

B. Reduced form of endogenous 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. 9. 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 $T = 35$ 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 biggest 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 in all columns are always slightly above 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 closer to unity. As expected, this suggests that our instrument captures aid quite broadly. Second, our instrument is highly relevant. The cluster-robust F-statistics always exceed the conventional level of about ten by an order of magnitude. Hence, it seems safe to conclude that aggregating changes in aid induced by electoral outcomes in donor countries interacted with the probability of receiving aid makes for a powerful instrument of development aid.

An obvious 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 in

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. 14. Cluster robust standard errors are in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

recipients with many active donors. To investigate this, we measure donor fragmentation by the Herfindahl index and by the sum of the shares of the three largest donors (Gehring et al., 2015). We then split the sample into below and above average fragmentation based on both indices and re-run the first stage in each sub-sample. As expected, the coefficient of the instrument is slightly higher (by about 0.3-0.4) when the donor pool is more fragmented than the average, but the first stage F-statistics in all sub-samples are still many multiples of the critical threshold. Hence, our instrument does not only draw its power from 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 investments. 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 a heterogeneous effect on regular and irregular aid recipients.[12]

Table A-4 in the Appendix includes UNGA voting alignment, 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 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 positive 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 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 a zero effect of aid on conflict, if we would not correct for endogeneity (this is indeed the case if we run these regressions without the control function). In control function methods, testing the null that the coefficient on the

[12] 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.

Table III

Second stage ordered probit regressions, CRE and CF

Variables	*Dependent Variable: Ordered Conflict* (1)	(2)	(3)
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. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

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.

Somewhat surprisingly, none of the coefficients on the selected time varying controls are significant. The literature typically finds that GDP has a large, positive, and significant effect (greater opportunity costs) and also finds evidence of scale effects. However, 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 are included in all of our specifications and purge all time invariant characteristics, 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 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. Nevertheless, 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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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. 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.[13] The same increase in aid also significantly increases the likelihood of 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).[14]

The effect size 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). Afghanistan, for example, experienced a three standard deviation increase in its aid to GDP ratio in 2002 when the share of aid to GDP increased from about 9% to 24%. At the same time, it turned 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 20 percentage points. Aid increases of this magnitude are rare (only in about 3% of the sample they exceed five percentage points). 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. In both cases (1981 and 2002), the country experienced an escalation of conflict.

D. Persistence, state-dependence and duration

A distinct advantage of our dynamic approach is that it allows to quantify persistence and state dependence, study how development aid alters these relationships over time, and relate our quantitative predictions to duration models.

Table V shows the average transition probabilities as they are predicted by our preferred specification.[15] The diagonal of this matrix shows the predicted persistence rates and the off-diagonal elements are the escalation and deescalation probabilities,

[13] 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 49 switches behind this estimate and more than 300 observations behind each of two lower switches.

[14] The sign 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 8 percentage points.

[15] 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.

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. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

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. 6 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).[16] 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 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

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

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). To see the equivalence, note that the hazard rate for one period is just one minus the probability of remaining in a particular state if it has occurred before ($h_i = 1-p_{ii}$, where p_{ii} is the probability of staying in state i). 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 (just like the exponential duration model in continuous time).[17]

Applying these relationships to the conditional transition matrix estimated above gives the following results.[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 durations typically found in the literature (e.g., Collier and Hoeffler, 2004b).[19]

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. Finally, we include a variety of additional controls. We only briefly survey the results – the corresponding tables are relegated to the Appendix.

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. Table A-5 addresses this issue. Here we ignore the ordinal nature and estimate our base

[17] A bit of probability algebra suffices to show that the probability of exiting a particular state is geometrically distributed with $\Pr[T_i = t] = p_{ii}^{t-1}(1-p_{ii})$. The expected waiting or "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.

[18] We need to assume that the data actually satisfy the fundamental Markov property (memoryless) and the series is in a stationary equilibrium. This is true by construction for the estimated probabilities from our model, although this property may be violated in the raw data. We do not explore this issue further, since higher-order dynamics are usually not considered in the conflict literature.

[19] Also note that our estimates under-predict persistence relative to the observed data, in part because we average out the effects of observed and unobserved heterogeneity.

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 and instrumental variables approaches.[20]

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. However, 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 similarities of control function and instrumental variables approaches fully break down once interactions are involved. 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).

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. In times of major civil conflicts the definition of what constitutes a terrorist act becomes very blurred. In fact Campos and Gassebner (2013) show that countries with a history of civil wars are the ideal training ground for (international) terrorists. We now include country-year observations with a positive number of terror attacks but less that 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.

[20] In static models control function and instrumental variables approaches yield numerically identical results. However, here we specify the first stage of the control function estimator without controlling for the lagged states (to emulate the dynamic specification used for our non-linear estimator) which breaks this equivalence.

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. Armed conflict and civil war are classified as before. In column (3), neither the level estimates nor the interaction effects are statistically significant. This is also true for the APEs. The result is not surprising since small conflict events are rare compared to peaceful country-year observations. It is thus plausible that – on average – we find no effect of aid on the transition probabilities from "peace" (including small conflicts) to armed conflict or civil war. This comparison supports our argument that it is necessary to distinguish between peace and small conflicts.

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. (the biggest and potentially most politically-motivated donor in the world, e.g., Kuziemko and Werker, 2006, among many others). We do so for two reasons. First, for those three countries we use legislature fractionalization rather than government fractionalization as an IV for bilateral aid. In order to rule out that our results depend on this choice, we repeat 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.). Column (5) uses ODA, while Column (6) uses ODA with OOF. The estimated coefficients and APEs are in line with our preferred specification.

Last but not least, we conduct one further robustness test. Since our small conflict category proves to be crucial and it is composed of several variables, we code variants of this category by excluding one of the constituting variables each time (e.g., riots). As the results in Table A-7 show, we obtain quantitatively identical results for all four perturbations. Hence, we conclude that our results are not driven by one single dimension of small conflict.

C. Additional controls

In Table A-8 we extend the set of control variables. Column (1) examines influence of the immediate regional neighborhood. We find little evidence of spillover effects of conflict. This finding is in line with Bosker and de Ree (2014) who find that only ethnic conflicts spill across borders. Column (2) addresses the potential concern that oil, quite literally, fuels conflict and that major oil exporters might exhibit different aid patterns (Fearon, 2005). We find no evidence in favor of either hypothesis. In columns (3) to (6) we examine if the political sphere affects aid and conflict. This comes at the cost of a reduced sample.[21] None of the political variables alter our main results. Political instability is associated with conflict and countries with a Polity IV score of greater or equal to six are less likely to engage in violent activities. Column (7) shows that GDP growth makes conflict less likely but does not affect the relationship between aid and conflict.

In a further robustness test, we take a closer look at humanitarian aid which represents about 6.5% of overall aid in our sample. Humanitarian aid could affect conflict differently than regular aid. 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 the exogenous variation. To be more precise, we regress our instrument on a full set of time and country fixed effects, and obtain the residual. 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. Hence, our identification strategy does not rely countries on receiving humanitarian aid.

All in all, we find that our results are robust and neither driven by our estimation technique, the operationalization of key variables, nor the set of control variables.

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. We make three major contributions to the measurement, estimation and identification of the effect of aid on conflict.

First, we propose a new measure of conflict by combining data on civil wars with data on low-level conflicts as measured by government purges, assassinations, riots, and guerrilla warfare. Second, contrary to previous studies, our ordinal conflict measure allows us to analyze the dynamics of conflict much more explicitly. Third, we use characteristics of the political system of donor countries to identify the causal effect of aid on conflict.

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 to armed conflict, we find no statistically significant effect of aid in truly peaceful countries. Aid does also not 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.

Our findings stress that donors have to be aware of unintended consequences when

[21] 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.

giving aid to countries with lingering conflicts. This could be of particular importance when fighting stops but the underlying grievances are not fully resolved. What we could not address in this analysis is potential heterogeneous effects of different types of aid flows. Future research could focus on what assistance can be given countries with lingering conflicts so as to actually help rather than harm.

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Appendix

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	131,964
Aid to GDP (with OOF, in percent)	0.19	1.49	-25.71	228.67	131,964
Government Fractionalization	0.30	0.27	0.00	0.83	141,789
Legislative Fractionalization (FPTP only)	0.06	0.17	0.00	0.69	151,906
Probability to Receive	0.46	0.37	0.00	1.00	152,208
Probability to Receive (with OOF)	0.45	0.36	0.00	1.00	152,208
Panel B: Country Data					
Aid to GDP (in percent)	4.95	8.84	-2.95	241.69	4,500
Aid to GDP (with OOF, in percent)	5.10	9.10	-10.89	241.69	4,500
Log of GDP	16.19	2.10	11.39	22.97	4,500
Log of Population	8.17	2.24	2.50	14.11	4,500
Log of GDP per capita	7.96	1.12	5.08	11.49	4,500
Oil Exporter	0.46	0.50	0.00	1.00	4,500
Polity IV (revised)	-0.14	6.79	-10.00	10.00	3,670
Political Instability	0.18	0.39	0.00	1.00	3,723
Regional Polity IV	-0.56	5.79	-9.00	10.00	3,723
Neighbor in Small Conflict	0.40	0.49	0.00	1.00	4,500
Neighbor in Armed Conflict	0.34	0.47	0.00	1.00	4,500
Neighbor in War	0.16	0.36	0.00	1.00	4,500
Island	0.26	0.44	0.00	1.00	4,500

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. 14. 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:					
	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	FE-OLS	CRE-CF	FE-2SLS	FE-OLS	CRE-CF	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

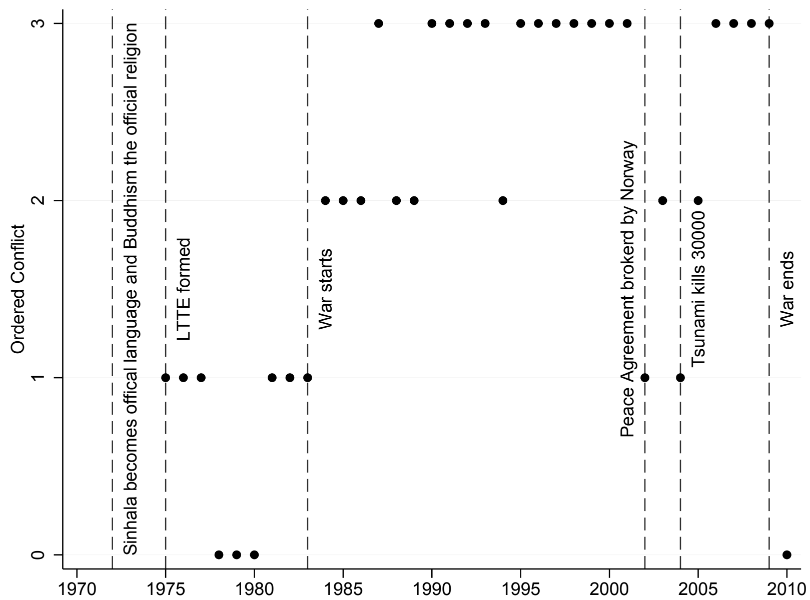
	Dependent Variable: Ordered Conflict						
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Aid to GDP (a_{2it})	0.0728	0.0717	0.0639	0.0571	0.0710	0.0692	0.0664
	(0.0473)	(0.0486)	(0.0472)	(0.0425)	(0.0505)	(0.0496)	(0.0438)
Residuals ($\hat{\nu}_{2it}$)	-0.0868*	-0.0860*	-0.0780*	-0.0705*	-0.0861*	-0.0839*	-0.0789*
	(0.0493)	(0.0498)	(0.0455)	(0.0417)	(0.0490)	(0.0479)	(0.0445)
Interactions with Lagged States							
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0218***	0.0214**	0.0235**	0.0254**	0.0241**	0.0246**	0.0203**
	(0.00829)	(0.00863)	(0.0112)	(0.0102)	(0.0112)	(0.0113)	(0.00831)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.0114	-0.0107	-0.0107	-0.00699	-0.0108	-0.00765	-0.0150
	(0.0198)	(0.0190)	(0.0205)	(0.0207)	(0.0191)	(0.0192)	(0.0174)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00352	-0.00212	-0.00309	-0.000925	-0.00100	-0.00129	-0.00318
	(0.0213)	(0.0257)	(0.0276)	(0.0257)	(0.0274)	(0.0280)	(0.0246)
Added Controls							
Neighbor in Small Conflict	0.125*						
	(0.0716)						
Neighbor in Armed Conflict	0.0620						
	(0.0822)						
Neighbor in Civil War	0.165*						
	(0.0933)						
Island	0.0247						
	(0.0364)						
Oil Exporter		0.0316					
		(0.152)					
Political Instability			0.217***				
			(0.0802)				
Polity IV (revised)				-0.0120			
				(0.00875)			
Regional Polity IV					0.0154		
					(0.0179)		
Democracy						-0.352**	
						(0.141)	
GDP Growth							-1.026***
							(0.321)
Summary Statistics							
$N \times T$	4375	4375	3708	3672	3708	3708	4375
N	125	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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Short Case Study: Sri Lanka

Figure A-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 the UCDP-PRIO 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 A-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.[22] The Tamils formed the Tamil New Tigers Group to set up a separate homeland *Tamil Eelam* in the north-east of Sri Lanka which was accompanied by heavy riots (Banks and Wilson, 2015).[23]

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

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

riots erupted in Jaffna and a state of emergency was declared. Finally, in 1983 the first guerrilla attack, an ambush, was conducted by the LTTE, resulting in the death of 13 soldiers. The incident led also 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.[24] 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 30000 citizens died during the tsunami.[25] 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.[26] In 2004 rioters burned down outlets of the government friendly splinter group who seceded from the LTTE (Banks and Wilson, 2015).[27]

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.

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

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

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

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