Fueling Conflict? (De)Escalation and Bilateral Aid

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Abstract

Civil conflicts undergo cycles of escalation. Beginning with riots, purges, and other violent acts of aggression, they escalate further and often culminate in outright civil war. This paper studies the effects of foreign aid on the escalation and de-escalation of conflict. We make three major contributions. First, we combine data on civil wars with data on low level conflicts in a new ordinal measure that captures the two-sided nature of conflict. Second, we study the effect of development aid on escalation and de-escalation. This allows us to give a rich description of how conflicts evolve dynamically, and to highlight the different roles played by bilateral aid in these transitions. We stress that low level conflicts matter since they are a violent expression of discontent over the distribution of rents (including aid) or of repression by the state. Third, we employ a new instrumental variable, which we then use to predict bilateral aid of DAC donor countries to 125 recipient countries over the period of 1975 to 2010. This solves the endogeneity concerns which have so far plagued the aid-conflict relationship. Our results show that the effect of foreign aid on the various transition probabilities is heterogeneous and sometimes very large. For example, receiving bilateral aid raises the chances of escalating from peace to small conflict, and from small conflict to armed conflict, but does not affect the transition from peace to civil war. Our main findings are robust to different estimation methods, controls and measures of conflict or foreign aid.

Keywords: conflict, foreign aid, political economy

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1 Introduction

Civil conflict is one of the main obstacles to development. Conflict injures and kills people, destroys material and immaterial infrastructure (Collier et al., 2003), limits trade and investment (Qureshi, 2013), decreases public health (Iqbal, 2006), and erodes state institutions (World Bank, 2011; Collier, 2009). In the worst case, civil conflict pushes economies into a downward spiral, generates profitable war economies (Kaldor, 2012), and spills over to neighboring countries (Bosker and de Ree, 2014). Research on the causes of civil war has found that low opportunity costs, slow or negative economic growth (Collier and Hoeffler, 2004b), weak institutions or states (Fearon and Laitin, 2003; Besley and Persson, 2011), various forms of ethnic heterogeneity (Esteban et al., 2012; Corvalan and Vargas, 2015), and ethno-nationalist politics which disenfranchise large parts of societies (Wucherpfennig et al., 2012; Cederman et al., 2013) are all correlated with conflict.

In this paper we study the effects of foreign aid on the escalation or de-escalation of civil conflict. Poor and badly governed states both need and receive substantial amounts of development aid.[1] A large and growing literature examines the effect of foreign aid on civil conflict but has failed to generate a consensus on whether aid is appeasing or fueling conflict. Both sides have theoretical and empirical backing. On the one hand, there is a well-established literature arguing that foreign aid increases rents and thus raises the value of capturing the state (Grossman, 1992). On the other hand, the appeasement camp brings forth the argument that aid increases the level of public good provision which, in turn, raises the opportunity cost of violent activity (Becker, 1968; Azam, 1995; Collier and Hoeffler, 2004b). Foreign aid may also increase the military capabilities of the government (Azam, 1995; Fearon and Laitin, 2003). According to this view, aid and other positive economic shocks thus either result in a reduction of the level of conflict, or a suppression of potential violence by the government.

Common to the entire literature is that it mostly relies on binary conflict measures (apart from Besley and Persson, 2011) but tries to distinguish between onset and continuation of conflict as these might be driven by different determinants (Collier and Hoeffler, 2002). What has been neglected so far are small scale conflicts which fall below the usual minimal threshold of 25 battle-related deaths per annum (Gleditsch et al., 2002) and the dynamics these small conflicts entail.

Why is this an important omission? Take for instance the argument that foreign aid enhances the military capability of the government. Aid could quite possibly enable the government to suppress violence so that we do not see an outbreak of a civil war. Yet it may also alienate its citizens with such acts. Civil discontent is fueled by the fact that some groups inevitably profit more from the extra financial flows than others. **?** and

[1] In fact, conflict might be one of the main reasons why research on the effect of aid on growth is inconclusive (see e.g., Boone, 1996; Burnside and Dollar, 2000; Rajan and Subramanian, 2008; Ahmed, 2012; Werker et al., 2009; Clemens et al., 2012; Ahmed, 2012; Brückner, 2013; Dreher et al., 2015).

Dreher et al. (2015), for example, show that funds tend to disproportionately flow to the birth region of the current ruler. Such feelings of dissatisfaction can find their expression in smaller acts of violent acts, such as riots or guerrilla activities. Any violent behavior of course questions the state's monopoly of violence, satisfying what can be considered the most basic definition of civil conflict. Low level conflicts have the potential to escalate into more violent states, whereas truly peaceful societies are most likely to remain peaceful. In fact, as we show below, a civil war never broke out in a society that was truly peaceful the year before. Exactly these type of escalation and de-escalation dynamics and how they are affected by aid have not been analyzed so far.

We may also interpret the role of civil conflict from a different angle. Small conflicts could be signal to the government and other potential rebels that a part of society is not content with the current provision, or division, of public goods. Hence, they can help potential rebels to get an estimate of how easily they can mobilize broader public and overcome collective action problems. As argued earlier by Collier and Hoeffler (2004b), one idealist is not enough to start a rebellion and needs a group of fighters mostly motivated by basic needs. Small acts of violence committed by citizens against the government can also provide information about repressive capabilities; both parties observe the government's capability or resolve to contain eruptions of violence. In essence, we argue that a neglect of small scale conflicts can seriously pollute estimates of aid on conflict, since an important dimension is ignored and should not be coded as "peace".

Our study makes three larger contributions to the literature. First, we combine data on civil wars with data on low level conflicts in a new ordinal measure that captures a whole range of conflict experience from small conflict to outright civil war. Second, we explicitly model the history-dependence and dynamics of civil conflict in an unprecedented fashion. This allows us to analyze the effect of development aid on escalation and/or de-escalation of conflict; that is, not only a binary transition from peace to conflict but also from low level conflict to higher levels and vice versa. We are thus able to highlight the different roles played by foreign aid in these transitions and examine the persistence of conflict. Third, we propose a new instrumental variable, which we then use to predict bilateral aid of all major donors to 125 recipient countries over the period of 1975 to 2010. This solves the rampant endogeneity concerns which have so far plagued the aid-conflict relationship. Our results show that the effect of foreign aid on the various transition probabilities is heterogeneous and sometimes very large. For example, receiving bilateral aid raises the chances of escalating from peace to small conflict, and from small conflict to armed conflict, but does not affect the transition from peace to civil war. Our main findings are robust to different estimation methods, controls and measures of conflict or foreign aid.

We proceed 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. Section 6 concludes.

2 Civil conflict and aid

Economists usually think of civil conflict in terms of two models. The contest model argues that the state is a price that rebels want to capture (Hirshleifer, 1988, 1989; Grossman, 1992), whereas the opportunity cost approach (Azam, 1995) argues that conflict is mainly determined by the calculus of "would-be rebels". Both models acknowledge that a well defended state is harder to conquer. Greater state capacity reduces the likelihood of successful capture and hence the expected value of rebellion (Fearon and Laitin, 2003). Furthermore, both perspectives focus on conflict onset rather than conflict dynamics more generally. Recent theoretical work has tried to account for different conflict dynamics. The main goal of this work is to explain onset, escalation and de-escalation cycles in a unified framework (Besley and Persson, 2011; Acemoglu and Wolitzky, 2014).[2] These theories highlight several channels through which foreign aid can have an heterogeneous effect on conflict.

Foreign aid increases public finances and hence the value of controlling the government. The contest model predicts that conflict becomes more likely with higher aid receipts as the expected gains from fighting increase (Grossman, 1992). At the same time, the government is likely to put more effort into controlling these rents. Empirical evidence for this hypothesis is rather scarce. Crost et al. (2014) show that municipalities in the Philippines which are about to receive more aid experience increased rebel activity. Crost et al. (2014) argue that rebels have a motive to sabotage aid since the success of the aid programs would reduce the support of the people for their course (but this interpretation is not consistent with the contest model).

Greater public resources increase the government's capability to suppress conflicts. If aid shifts the government's budget constraint outward (as a consequence of fungibility), it allows for increased military spending (Collier and Hoeffler, 2007; Kono and Montinola, 2013). Theoretically, this should reduce the risk of conflict (Azam, 1995; 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. Nielsen et al. (2011) find that negative aid shocks increase the risk of civil conflict outbreak reflecting that less resources available lead to more conflicts. Note that it is not clear, whether this effect runs through military spending of the government or opportunity costs of the rebels.[3] On the micro level Dube and Naidu (2014) find

[2]Such models often focus on specific actor constellations, such as group cohesion and composition (Esteban and Ray, 2011a,b; Esteban et al., 2012) or the institutional setting and beliefs (Acemoglu et al., 2003; Acemoglu and Wolitzky, 2014; Besley and Persson, 2011).

[3]For example, Strange et al. (2014) show that Nielsen et al.'s (2011) result only holds if there is not

that US military aid increases violence of paramilitary organizations that function as complements to government forces in Columbia, especially in election years, while it has no discernible effect on Guerrilla warfare.

Foreign aid affects the opportunity costs of fighting. Aid can increase the provision of public goods and thus raises the opportunity costs of fighting (Becker, 1968; Azam, 1995; Esteban and Ray, 2011a,b). Studies examining the onset of civil conflict often find support of this hypothesis (Collier and Hoeffler, 2002; Savun and Tirone, 2011, 2012). De Ree and Nillesen (2009) add that aid decreases the probability of armed conflict continuation, but find no effect of aid on conflict onset. Nunn and Qian (2014) argue that food aid can also reduce the opportunity costs of fighting since it can be almost immediately captured by the rebels. They show that US food aid prolongs the duration of civil conflict, but has no significant effects on conflict onset

Conflict comes in many forms and spirals through different stages. Recently, the ordinal and cyclical nature of conflict has received increasing attention (World Bank, 2011). Besley and Persson (2011) and Acemoglu and Wolitzky (2014) provide theoretical frameworks that allow for escalation and de-escalation dynamics of conflict. However, the empirical literature still lags behind in this development and tends to focus on one stage (onset, continuation, ending or post-conflict) of either civil conflict or civil war. Even if studies account for more stages or more intensity levels of conflict, they usually analyze these effects of interest separately and thus cannot give a full description of the underlying dynamics.[4] Collier and Hoeffler (2004a), for example, argue that aid is especially effective in preventing civil conflict in post conflict scenarios.

Small conflicts matter. Nevertheless, their role has been largely neglected in the literature. Theoretically, small conflicts can be seen as a signaling device, where the government tests how well it can quench rebellion with repressive actions. Potential rebels, in turn, may use smaller conflicts to determine the exact type of their government (Azam, 1995). Small conflicts also help to overcome and assess collective action problems. Riots deliver an estimate on how many others are willing to fight the government. Minor violent actions do not face the same opportunity costs as civil war. They allow groups of individuals to question the state's monopoly of violence without investing too much into the fight at first. Political economy models often 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). Empirical research tends to neglect conflicts below the 25 battle-related deaths threshold. Hence, whether a society is entirely peaceful or not is not actually captured by the typical coding of conflict. The neglect of small conflicts is especially problematic when analyzing the

sufficient alternative funding through Chinese aid, supporting the capability argument.

[4] Besley and Persson (2011) specify ordered logit regressions but do not account for conflict histories (lags) or allow for history-dependent effects (interactions with lags).

effect of aid on different manifestations of conflict. This is the case for two major reasons. First, aid is not distribution-neutral (Dreher et al., 2015), which increases discontent over the distribution of resources. Second, it is unlikely that foreign aid is a large contributor to a major outbreak of civil war, but it is quite plausible that it sets off a cycle of violence by inciting lower intensity conflicts and making them more persistent.

The simultaneity of aid and conflict makes causal identification notoriously difficult. The effect is likely to be biased upwards if aid is given to countries in need. A downward bias could instead occur if donors are driven by political motives or reduce aid due to logistical problems frequently arising during conflict. A bias could also result from third factors influencing aid and conflict simultaneously such as political and economic crises (Nunn and Qian, 2014; De Ree and Nillesen, 2009). Along these lines, many studies find that aid is often given for political reasons (Kuziemko and Werker, 2006; Dreher et al., 2014, among others). Without addressing these problems, simple OLS estimates could be biased in either direction. Many articles attempt to address the endogeneity problem by lagging aid to at least ensure contemporaneous exogeneity or by using instrumental variables. However, most instruments proposed in the literature are either weak or not excludable. The noteworthy exception is Nunn and Qian (2014) who use lagged US wheat production interacted with the probability of recipients to receive US food aid. Yet this identification strategy only allows drawing conclusions with respect to U.S. food aid.[5]

3 Data

3.1 An ordinal measure of conflict

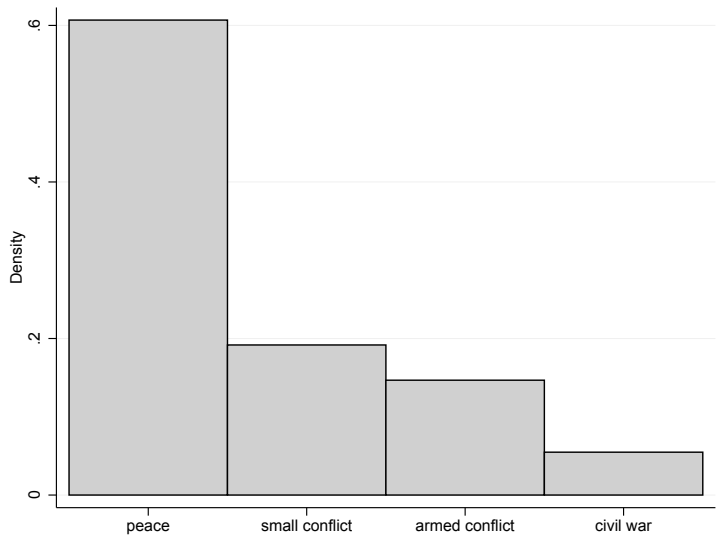
One of the often criticized features of the civil war/civil conflict literature is its crude measurement of conflict. The industry standard is to take one of two thresholds (25 or 1,000 battle related deaths) and create dummy variables indicating the surpassing of the respective threshold either for the first time (i.e., conflict onset) or for any given year (i.e., conflict duration). We try to improve upon this by creating a multi-faceted ordinal measure building on the standard Uppsala Conflict Data Program/Peace Research Institute Oslo (UCDP/PRIO) civil conflict measure (Gleditsch et al., 2002). The 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 annual battle-related deaths (BD) (Gleditsch

[5]Interestingly, evidence on the inverse relationship, i.e., how conflict affects aid, is rather scarce. Some insights are provided by Dreher and Fuchs (2011) who look at the impact of terrorism on aid. Their results indicate that countries where terror originates are not receiving aid more often but if they do receive they are getting higher amounts. Lis (2014) supports the findings of Dreher and Fuchs (2011) and ads that armed conflict on the contrary leads to lower aid disbursements. These results reveal the endogenous character of aid. The endogeneity problem arises due to reverse causality and joint determination.

et al., 2002). To capture conflicts below the 25 BD threshold, we complement the UCDP/PRIO data with observations from Banks and Wilson (2015) on government purges, assassinations, riots and guerrilla warfare.[6] All of these categories clearly are manifestations of civil conflict albeit on a lower intensity level. These low level violent conflicts are usually ignored in the civil war literature. Hence, any state with less than 25 (or 1,000) BD is coded as "peaceful", which might be far from the truth. As argued in the introduction this masks potential escalating and de-escalating effects and may invite ill-formed conclusions about the role of foreign aid. It is easy to imagine that aid has the opposite effect in absolutely peaceful societies compared to societies already in turmoil.

The creation of our ordinal variable is straightforward; a truly peaceful society is coded zero. Our measure takes on the value of one if at least one variable taken from Banks and Wilson (2015) exhibits a positive value but there are less than 25 BD. The next two categories follow the logic of the UCDP/PRIO measure: conflicts with a minimum of 25 but less than 1,000 BD are coded as two while the civil war category (i.e., more than 1,000 BD) takes on the value of three in our measure. This procedure yields a total of 1,486 conflict country year and 3,014 peace country year observations in our sample of 125 developing countries during the the years 1975 to 2010. Figure 1 shows a histogram of our conflict measure.

Figure 1: Distribution of Conflict Intensities



The main advantage is that the number of armed civil conflicts (544) and civil wars

[6]According to the User's Manual of Banks and Wilson (2015) the precise definitions of our utilized variables 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.

(203) are identical with the UCDP/PRIO measure. Hence, our results remain comparable to other studies and differ mainly due to the definition of peace. We distinguish between truly peaceful country year observations (3,014) and those with irregular violence, which are below the conventional thresholds (739). This conservative approach of changing existing measures implies that our ordinal measure is straightforward to understand and avoids awkward weighting procedures such as the composite index of Databanks International (Banks and Wilson, 2015). We also avoid mixing flow and stock variables to measure different conflict intensities, such as taking the cumulative amount of BD to create intermediate levels of armed civil conflict (e.g., Esteban et al., 2012; Bazzi and Blattman, 2014; Corvalan and Vargas, 2015). The problem with measures including both flow and stock variables is that they are unsuitable for our purpose. Consider, for example, the often used intermediate UCDP/PRIO conflict category (Gleditsch et al., 2002) where conflicts are coded in such a way that they can never de-escalate below armed conflict once a single war observation has been observed.

Table 1: 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

Note(s): Estimated using the same balanced sample of 125 countries over 36 years that is used later for the main results (4,500 observations imply 4,375 transitions). Rows sum to 100%.

Table 1 shows the unconditional transition probabilities as there are observed in our data. This simple table already allows us to highlight three major points. First, the 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 the chances 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 it is most likely to transition into a small conflict. 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.

3.2 Aid flows and controls

Our aid variable includes two types of flows disbursed by 26 bilateral donors of the OECD's Development Assistance Committee (DAC): Official Development Aid (ODA) and Other Official Flows (OOF). ODA refers to those flows that are i) provided by official agencies to developing countries and multilateral institutions; ii) with economic development and welfare as the main objective; and iii) having a concessional character. The latter condition reflects that the grant element should be at least 25 percent. Contrary to that, 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, however, also consider the effects of the two types separately as these flows can be differently motivated and could thus lead to different outcomes (Dreher et al., 2015). We gathered data on net ODA disbursements and ODA + OOF disbursements for 26 DAC donors to 125 recipients over the period of 1975 to 2010. The included donors and recipients are reported in Table A-1 and Table A-2 in the Appendix. The upper panel of Table 2 depicts the core variables for our bilateral sample.

The data for government and legislature fractionalization are retrieved from the Database of Political Institutions (Beck et al., 2001). Following Dreher and Langlotz (2015), we use them to construct a combined government fractionalization measure (government fractionalization), where we impute the missing government fractionalizaton for the United States and Canada with their respective legislature fractionalization. The two variables that capture the Probability to receive aid measure the fraction of years a recipient has received positive net ODA (ODA + OOF) disbursements from a given donor.

The lower panel of Table 2 describes our variables in the collapsed country panel data set, which we use for our core analysis. The two aid variables are obtained by first summarizing the bilateral Aid to GDP values by recipients over it's donors during each year. For the purpose of illustration our ordinal conflict variable is depicted in four dummies (Peace, Small Conflict, Armed Conflict and Civil War). For the set of core control variables, we follow Sambanis (2002) and Hegre and Sambanis (2006) by choosing the Log of population to capture the scale effect of the dependent variable, the Log of GDP and lagged conflict, which works similarly to including the number of peace years used in a pooled setting. Additionally we include the revised Polity IV score to account for institutional quality, as well as a democracy dummy equal to one if the revised Polity IV score is equal or above six Dixon (1994); Marshall et al. (2010). Furthermore we include a measure for recent political instability, which is a dummy coded one if a country has experienced a change in it's polity IV score of at least three (Gates et al., 2006). We also include the regional polity score to test for the democratic values of a country's neighborhood (Gates et al., 2006). To test for the resource curse (Collier and

Table 2: Summary statistics

Variables	Mean	SD	Min	Max	N
Panel A: Bilateral Data Set					
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	0.34	0.27	0.00	0.83	141,487
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 Level Data Set					
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
Peace	0.67	0.47	0.00	1.00	4,500
Small Conflict	0.16	0.37	0.00	1.00	4,500
Armed Conflict	0.12	0.33	0.00	1.00	4,500
Civil War	0.05	0.21	0.00	1.00	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 Dummy	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 Dummy	0.26	0.44	0.00	1.00	4,500

Note(s): 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.

Hoeffler, 2004b) we include an Oil Exporter Dummy for countries, in which fuel exports amount to 33 percent of their GDP (Fearon and Laitin, 2003). Lastly, we test for conflict spillovers from neighboring countries (Sambanis, 2002; Bosker and de Ree, 2014) with three dummies indicating if at least one neighbor had a small conflict, armed conflict or war during a given year. Since the neighbor-in-conflict dummies and the regional polity score exclude island countries, we include an Island Dummy to keep them in the sample.

4 Empirical strategy

4.1 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. This part outlines on the quantities of interest, while the next two subsections discuss our estimation and identification strategies.

Differentiating between the onset and continuation of conflict has become the de facto standard in the extant literature (e.g., Collier and Hoeffler, 2004b; Esteban et al., 2012; Nunn and Qian, 2014; Corvalan and Vargas, 2015). These studies typically simplify the underlying dynamic structure by splitting the sample (e.g., studying only onsets by setting all continuations to missing), or use dynamic linear probability models instead of their non-linear counterparts (De Ree and Nillesen, 2009). Most fundamentally, the *ordinal nature of conflict* is usually not taken into account, in part because doing so would complicate the econometrics (ordered outcomes cannot be consistently estimated by OLS), and in part because the underlying theory did not deliver clear expectations about the effects (with the notable exception of Besley and Persson, 2011). Yet without ordering conflict outcomes by intensity and allowing for flexible dynamics, there is no empirical sense of escalation or de-escalation among different conflict levels. Only the trivial case of a switch from peace to conflict and vice versa is usually accounted for.

Dynamic switches among ordered responses cannot be meaningfully estimated with linear models. While Beck et al. (1998) show that separately specifying models of onset and ending of war is equivalent to a dynamic model of war incidence, many more linear probability models would be needed to study four different states. The result would be unstable parameter estimates that are inefficiently estimated and difficult to interpret. Furthermore, if the underlying latent variable ("conflict") is observed as an ordered outcome, then separate regressions can violate known parameter restrictions.[7]

Some notation is in order to help fix ideas. 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 (e.g., peace, low, mid, high). As typical in ordered settings, we assume that these outcomes are generated by a continuous latent variable c^*_{it} and J cut points $a_1 < \cdots < a_j < ... < a_J$ to be estimated later. The first outcome is $c_{it} = 0$ if $-\infty < c^*_{it} < a_1$, the intermediate outcomes are $c_{it} = j$ if $a_j < c^*_{it} < a_{j+1}$ with $0 < j < J$, and the last outcome is $c_{it} = J$ if $a_J < c^*_{it} < \infty$. 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 $h_{j,i,t-1} \equiv 1[c_{i,t-t} = j]$, that is, an indicator of whether the past outcome is identical to the current outcome. 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.

The latent variable model of interest includes variables that have history dependent

[7]This is a version of the misnamed "parallel regression assumption" is 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. If all the coefficients can vary in each state, then this meaningless result cannot be ruled out.

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 the possible interactions between $\mathbf{x}_{it}$ and $\mathbf{h}_{i,t-1}$. We include country level unobserved effects, μ_i, the identification of which we discuss later. 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., a full set of time dummies). We are only interested in the set of coefficients $\{\boldsymbol{\beta}, \boldsymbol{\rho}, \boldsymbol{\gamma}\}$ inasfar as they determine 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 $p > 0$. In words, the past state is $j+p$ and then a transition to j takes place, or no transition occurs and the state remains j when $p = 0$. $F(\cdot)$ is some continuous symmetric c.d.f. which is defined by the distribution of the error terms, ϵ_{1it}, but for now needs not be specified further.

We are also interested in the probability of de-escalation (or escalation) from the current 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 probabilities we define above. It should be

straightforward to see that these are just 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

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

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

The problem of state dependence is simple in dynamic binary models. State dependence is the average of the probability of an event happening when the event happened before minus the probability of the event when it did not happen before. 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 propose to estimate persistence as the difference between experiencing a particular state if it has occurred before and a weighted average of the different probabilities of experiencing this state when it has not occurred before.

Formally, we estimate 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 obtained via the observed transition matrix.[8] We expect (conditional) state dependence to increase with higher conflict intensities, so that the higher the level of conflict, the more persistent conflict is likely to be.

4.2 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. 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

[8]We weigh the probabilities of entering state j from any other state in $t-1$ by the normalized probability of switches from that state to the new state j. We normalize since the transition matrix is right stochastic, while we need the weights to sum to unity. Let the transition matrix be

$$\mathbf{p}=\begin{pmatrix} p_{0,0} & p_{0,1} & \cdots & p_{0,j} & \cdots \\ p_{1,0} & p_{1,1} & \cdots & p_{1,j} & \cdots \\ \vdots & \vdots & \ddots & \vdots & \ddots \\ p_{r,0} & p_{r,1} & \cdots & p_{r,j} & \cdots \\ \vdots & \vdots & \ddots & \vdots & \ddots \end{pmatrix}$$

then each weight is defined such that $\omega_{rj}=p_{rj}/\sum_{r\neq j}p_{rj}$.

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 the state dependence to vary with each conflict outcome and permit state-dependent effects of the endogenous regressor. Note that we need the panel to be balanced, since none of these approaches work with unbalanced panels. Later, in the robustness section, we contrast our approach to more traditional linear approaches.

We now incorporate two specific features into the general formulation from the preceding section. First, we add a contemporaneously endogenous regressor (the ratio of foreign aid to GDP) and, second, we interact the endogenous regressor with the lagged conflict states. We do not consider other interactions. Hence, our model of interest is

$$c_{1it}^{*} = \mathbf{z}_{1it}'\boldsymbol{\beta}_1 + \boldsymbol{\beta}_2 a_{2it} + \mathbf{h}_{1i,t-1}'\boldsymbol{\rho} + (a_{2it} \times \mathbf{h}_{1i,t-1})'\boldsymbol{\gamma} + \mu_{1i} + \lambda_{1t} + u_{1it} \tag{7}$$

where $\mathbf{z}_{1it}$ is a vector of strictly exogenous variables, a_{2it} is endogenous aid to GDP, 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 equation (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 the construction of the instrument in great detail in the next section. Note here 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 $b_{2i}|\mathbf{z}_i \sim \mathcal{N}(0, \sigma_{b_2}^2)$ 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 equation (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 equation (9) are numerically equivalent to the linear fixed effects model (Mundlak, 1978; Wooldridge, 2010, p. 332) making this a very robust specification.

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

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

since $u_{1it} = \omega u_{2it} + \epsilon_{1it} = \omega(\nu_{2it} - b_{2i}) + \epsilon_{1it}$ and we define $\omega = \tau/\sigma_{u2}$.

From equation (11) it is easy to see that 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 in different periods due to 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. Yet the whole point of the 'initial conditions problem' is that they are not ignorable when T is small and have repercussion towards how flexibly we must treat the unobserved heterogeneity (Wooldridge, 2005). Hence, assuming independence of $\mathbf{h}_{i0}$ and $\mathbf{z}_i$ is not very attractive.

Just as in 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 hetoergenity 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 equation (11) gives the estimation 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). Random effects ordered probit estimates scaled version of the parameters (e.g., $\boldsymbol{\beta}_1/\sqrt{(1+\sigma^2_{d_1})}$ and so on, assuming the usual normalization of $Var(\epsilon_{1it}) = 1$).

A two-step approach then implies that (1) we estimate the reduced form in equation (9), obtain an estimate of the residuals ($\hat{\nu}_{2it}$) and the reduced form errors in all periods ($\hat{\boldsymbol{\nu}}_{2i}$), and then (2) plug these into equation (12). Delta method standard errors

accounting for the first stage uncertainty can be derived, but using the panel bootstrap over both stages is much simpler.

The time dimension of our panel is comparatively large ($T = 36$), which has two major implications. On the one hand, adding a new time-varying control variable means adding T additional regressors. On the other hand, the initial conditions problem is not likely to be very severe (although there currently exist no Monte Carlo studies for the ordered setting). Rabe-Hesketh and Skrondal (2013) provide simulation results for different ways specifying the conditional density of the unobserved effect in the dynamic binary probit model. Inspired by their study, we experiment with interesting constraints that can be placed on the two sequences $\mathbf{z}_i$ and $\hat{\boldsymbol{\nu}}_{2i}$. For example, we conserve degrees of freedom by splitting the two vectors, so that in the case of the exogenous variables we have $\mathbf{z}_i^+ = (\mathbf{z}_{i1}', \mathbf{z}_{i2}', \ldots, \mathbf{z}_{iR}', \bar{\mathbf{z}}_i^{+'})'$ where $R < T$ and $\bar{\mathbf{z}}_i^+ = \frac{1}{T-R-1}\sum_{t=R+1}^{T} \mathbf{z}_{it}$ is the time average after period R. The residual sequence, $\boldsymbol{\nu}_{2i}^+$, is computed analogously. Our results are not sensitive to the choice of R, as long as the first period is allowed to have its own coefficients. We typically set $R = 4$ but report more liberal and more constrained results in the robustness section. We also included $\mathbf{z}_{i0}$ to little effect (as suggested by Rabe-Hesketh and Skrondal, 2013; Skrondal and Rabe-Hesketh, 2014).

The average partial effects are simply derivatives the expectation of our specification with respect to the distribution of c_{1i} (see Blundell and Powell, 2004; Wooldridge, 2005). Only the average partial effects (APEs) of time-varying covariates are identified and can be different for each t. We usually average across all $N \times T$ observations to obtain a single estimate of the APE.

4.3 The instrument

Our identification strategy is inspired by Nunn and Qian (2014) who estimate the effect of US food aid on conflict and propose a new instrument for US food aid. Their instrument for US food aid is the interaction between the probability of receiving aid from the US with US wheat production from the previous year. Multiplying a time-variant variable with a country-variant variable induces country-time variation in the instrument and is crucial for identification. Nunn and Qian (2014) argue that even though the probability of receiving aid from the US is itself not exogenous, the interaction of this probability with wheat production is exogenous conditional on the presence of country and time fixed effects. The interaction estimates the effects of aid on conflict among regular and irregular aid recipients of US aid when US wheat production changes. They liken this technique to a difference-in-differences approach "where the first-stage estimates compare US food aid receipts in countries that frequently receive US food aid to countries that rarely receive US food aid, in years following high US wheat production relative to years following lower production" (Nunn and Qian, 2014, p. 1638). A key contribution of our

paper is to extend the spirit of this approach to bilateral aid from multiple donors.

We use government fractionalization of donor countries in lieu of wheat production in the US as our primary source of exogenous variation. Dreher and Langlotz (2015) show that government fractionalization interacted with the probability of receiving aid is a powerful instrument for bilateral aid. Government fractionalization is defined as the probability that any two randomly-chosen deputies from among the parties forming the government represent different parties (Beck et al., 2001).[9] The motivation for this instrument stems from three different literatures. First, government or legislature fractionalization has been shown to positively affect government expenditures (Roubini and Sachs, 1989; Volkerink and De Haan, 2001; Scartascini and Crain, 2002). 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 (Round and Odedokun, 2004; Brech and Potrafke, 2014). Third, higher aid budgets translate into higher aid disbursements (Dreher and Fuchs, 2011).[10]

Similar to Nunn and Qian (2014) our identification strategy can be loosely compared to a difference-in-difference approach where we compare the effects of aid induced by changes in donor government fractionalization on conflict in regular and irregular recipient countries. We expect that the effect of aid on conflict depends on the recipient countries probability of receiving aid. In fact, it has been shown by Nunn and Qian (2014), Dreher and Langlotz (2015), and Ahmed (2012) that the probability is significantly correlated with the amount of aid receipts. On average, countries with a higher probability of receiving aid are also those to whom more aid is sent from the DAC donors. Note that this does not trivially follow from the definition of the probability since donors could also be giving aid continuously at a low level. Whereas the conclusions in Nunn and Qian (2014) are specific to food aid from the US only, our local average partial effect is representative of overall bilateral aid.

Applying this in a bilateral setting requires us to either a) identify aggregate probabilities of receiving aid, or b) predict aid bilaterally and aggregate afterwards. We opt for the latter and proceed in two steps. First, following Dreher and Langlotz (2015) we predict bilateral aid from donor j to recipient i in year t in a bilateral regression.[11]

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

where f_{3jt} is donor-specific government fractionalization and $\bar{p}_{3ij}$ is the donor-recipient-specific probability of receiving aid. Using donor-recipient fixed effects yields the same

[9]Note that we replace government fractionalization with legislative fractionalization for the United States and Canada, because their political systems lack the features that would allow for government fractionalization. The results remain robust to not replacing the measure for US and Canada, to omitting the two countries and to using legislature fractionalization for all donors instead.

[10]For a further discussion on the choice of the instrument see Dreher and Langlotz (2015).

[11]Also see Rajan and Subramanian (2008) or Frankel and Romer (1999) for similar approaches.

result as including two different levels of fixed effects, but is unnecessary since we are not interested in their estimates *per se* (see Abowd et al., 1999, p. 268). The time-invariant probability is defined as $\bar{p}_{3ij} = \frac{1}{T}\sum_t^T \mathbf{1}[a_{3ijt} > 0]$, so that it is 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 precedes the others. 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}.

Just as Nunn and Qian (2014), we argue that the interaction term of the probability of receiving aid and donor government fractionalization is conditionally exogenous. The exclusion restriction would be violated if there are variables that are correlated with donor fractionalization and affect conflict differently in countries that are regular and irregular recipients of aid. We find it hard to come up with such variables.

We then aggregate the predicted bilateral aid from equation (13) across all donors in order to get predicted aid as a share of GDP at the recipient-year level. Hence, $\sum_j \hat{a}_{3ijt}$ is used as the instrument above. Note that donor fractionalization is the same across all recipients and will be swept out by the time averages in the reduced form above. Similarly, everything but the time varying interaction will be swept out by the recipient effects and time effects.

5 Results

Our approach to estimating the ordered conflict outcome is very flexible. Although we only focus on a basic set of controls, we 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, hence we limit ourselves to only a basic set of additional controls championed in the recent literature. While doing so we fully account for the history-dependence and dynamics of conflict. This section presents our main results and then undertakes a battery of robustness checks.

Bilateral estimation. We begin by briefly discussing the bilateral regression which we use to construct the instrument. Recall, we regress aid received by each recipient from a particular donor (as a share of recipient GDP) on government fractionalization, government fractionalization interacted with the probability of receiving aid and a set of country and time fixed effects. The regression is estimated over 4,116 bilateral donor-recipient relations for which we have data, yielding a total of 129,348 observations. We do not constrain this estimation to the balanced sample we use later on for two reasons: 1) in order to get the best possible estimate of this relationship, and 2) unbalancedness is not a problem in fixed effects regressions as long as selection is ignorable. The results are not intended to be qualitatively interpreted on their own (apart from signs and significance),

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 are as follows (standard errors are in parentheses):

$$\hat{a}_{3ijt} = \underset{(0.019)}{0.164^{***}} - \underset{(0.014)}{0.036^{***}} f_{3jt} + \underset{(0.058)}{0.216^{***}} (f_{3jt} \times \bar{p}_{3ij}) + \hat{\mu}_{3ij} + \hat{\lambda}_{3t} \tag{14}$$

As expected the coefficient on the interaction term is positive and significant at the 1% level. We clustered the standard errors on the donor-recipient level. The cluster-robust F-statistic of the interaction term is about 13.76. Since this is our primary source of exogenous variation, it may be compared to the conventional rule-of-thumb level of ten. However, the instrument will turn out to be considerably stronger at the country level. Note that the negative coefficient on the base level of fractionalization is purely a scaling issue. Running the same regression without the interaction term yields significant positive coefficient of about 0.073 (cluster-t of 3.55). We proceed by summing over all donors to get the receiptients' predicted share of aid in GDP as described above.

We repeated this estimation using net aid including official sector flows (OOF). The results are qualitatively and statistically similar (not reported, available on request). Although we prefer government fractionalization on theoretical grounds (ruling parties usually make the budget), we also tried to obtain a second source of exogenous variation by using legislative fractionalization in lieu of government fractionalization. This yields considerably weaker results.[12]

Reduced form of endogenous aid. We now turn to the national level estimates of the first stage. Table 3 shows three reduced form regressions for aid to GDP which we obtain by estimating the equivalent fixed effects model of equation (9). The residuals from these models are used as control functions in the main specifications 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. Using this larger sample ensures that the most important conflicts are in the data set (e.g., Afghanistan, Iraq, Pakistan and many more).

Two things stand out in Table 3. First of all, the estimated coefficients on the instruments are very close to one in all columns. In fact, only in the first column the effect is marginally different from unity at the 5%-level. A one percentage point increase in the predicted aid to GDP ratio hence leads to about a one percentage point increase in actual aid to GDP. Adding other controls only moves the estimated coefficients closer to unity.

[12]While the interaction term is comparable in sign and magnitude, it is no longer significant at conventional levels. Likewise, legislative fractionalization is insignificant by a large margin in regressions without the interaction term.

Table 3: First stage regressions with generated IV

	Dependent Variable: Aid to GDP		
VARIABLES	(1)	(2)	(3)
Predicted aid to GDP ($\sum_j \hat{a}_{3ijt}$)	1.232***	1.104***	1.100***
	(0.110)	(0.076)	(0.076)
Selected Controls			
Log GDP per capita		-5.098***	
		(0.849)	
Log GDP			-5.128***
			(0.811)
Log Population			6.021**
			(2.311)
Additional Controls			
Country FE	Yes	Yes	Yes
Time FE	Yes	Yes	Yes
Summary Statistics			
Kleineberg-Paap F-statistic IV	125.8	212.7	211.9
$N \times T$	4375	4375	4375
T	35	35	35
N	125	125	125
Within-R^2	0.0383	0.0711	0.0734

Notes: Shows the result of the first stage regression using a linear two-way fixed effects model. The instrument is the sum over all donors from the bilateral aid regression($\sum_j \hat{a}_{3ijt}$), see equation (14). Cluster robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Note that we estimate these models directly in levels (not logs), since negative flows occur when loan repayments exceed new inflows. Second, the instruments are highly relevant. The cluster-robust F-statistics exceed the conventional level of about ten by an order of magnitude. It seems safe to conclude that aggregating the predicted contributions of all 26 individual DAC donors makes for a powerful instrument of development aid.

Main results. We now turn to answering our main research question. We present two sets of estimates. Table 4 reports the regression results and Table 5 shows the associated average partial effects of aid on different transitions.

Consider the regressions in Table 4 first. In columns (1) to (3), we show the estimates without additional controls, then add GDP per capita and then allow GDP and population to have different effects in the last column. The results are striking 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 that the positive effect of aid on conflict is stronger

Table 4: Second stage ordered probit regressions, CRE and CF

	Dependent Variable: Ordered Conflict		
VARIABLES	(1)	(2)	(3)
Aid to GDP (a_{2it})	0.093*	0.094*	0.094
	(0.050)	(0.057)	(0.057)
Residuals ($\hat{\nu}_{2it}$)	-0.105**	-0.108*	-0.108*
	(0.051)	(0.058)	(0.058)
Interactions with Lagged States			
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.022***	0.021**	0.021**
	(0.008)	(0.008)	(0.009)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.008	-0.010	-0.011
	(0.019)	(0.019)	(0.020)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.002	-0.001	-0.002
	(0.024)	(0.025)	(0.024)
Lagged States			
Small Conflict ($h_{1,i,t-1}$)	0.581***	0.578***	0.575***
	(0.074)	(0.075)	(0.080)
Armed Conflict ($h_{2,i,t-1}$)	2.109***	2.097***	2.106***
	(0.181)	(0.184)	(0.194)
Civil War ($h_{3,i,t-1}$)	3.428***	3.407***	3.424***
	(0.228)	(0.230)	(0.242)
Selected Controls			
Log GDP per capita		0.366	
		(0.387)	
Log GDP			-0.181
			(0.615)
Log Population			0.403
			(0.367)
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: Shows the result 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 (not shown). * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

if the country experienced a small conflict already and is not notably different from the base level for higher conflict intensities. We find strong evidence of the endogeneity of aid. The residuals from the first stage are significant with opposite sign and similar magnitude. This suggests that we would find a zero effect of aid on conflict, if we would not correct for endogeneity (which is indeed the case if we run this estimation without the control function). In control function methods, testing the null that the coefficient on the residuals is zero corresponds to a Hausman test of endogeneity which does not depend on the first stage, hence bootstrap standard errors will be conservative. Nevertheless, we can reject the null of endogeneity at the 5% significance level.

Somewhat surprisingly, none of the coefficients on the selected time varying controls are significant. The literature typically finds that GDP has large, positive, and significant effect (greater opportunity costs) and also finds evidence of scale effects. However, most earlier studies use pooled methods (including the sensitivity analysis by Hegre and Sambanis, 2006). Given that recipient level CREs are included in our specification and purge all time invariant characteristics, log GDP (per capita) and log population do not seem to contribute much additional information.

We have strong reasons to trust the effects presented in column one. We allow for quasi-fixed effects, first-order 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 described in section 4.2, which is why we limit ourselves to a basic set of controls. Each additional variable consumes several degrees of freedom due to how the unobserved heterogeneous is modeled. We return to the issue of additional controls in the robustness section.

Table 5: Average partial effect of aid on transition probabilities

	To state			
From state	Peace	Small Conflict	Armed Conflict	Civil War
Peace	-2.121*	1.493*	0.615*	0.012
	(1.121)	(0.791)	(0.338)	(0.011)
Small Conflict	-3.556**	1.771**	1.698**	0.087
	(1.456)	(0.750)	(0.698)	(0.058)
Armed Conflict	-1.896	-0.758	1.840	0.814
	(1.278)	(0.495)	(1.175)	(0.531)
Civil War	-0.521	-1.266*	-0.813	2.601*
	(0.404)	(0.749)	(0.612)	(1.500)

Note(s): Based on column (1) in Table 4. Panel bootstrap standard errors in parentheses, computed with 200 replications. Significant at: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As expected, the coefficients on the lagged states increase with higher conflict

intensities. The coefficient on armed conflict is about four times as large as the coefficient on small conflict, whereas in the case of civil war it is about six times as large. Since the ratio of coefficients can be directly interpreted this points to an increasing importance of state-dependency. Combined with the positive coefficient on aid, this indicates that once a conflict spiral is in motion, a) it becomes harder and harder to end the conflict and transition back to peace, and b) aid seems to be a net accelerant of conflict, particularly for the transition from peace to small conflict and in turn to armed conflict. Nevertheless, these results are only indicative since none of these coefficients directly translate into interpretable relevant effect sizes (including state-dependence).

So does foreign aid really fight fire with fire? The estimated partial effects in Table 5 suggest that this indeed is the case at the lower end of the conflict scale. Most action occurs when aid is increased in societies that are not entirely peaceful but not (yet) fully engaged in armed conflict. For example, a one percentage point increase in the ratio of foreign aid to GDP leads to about a 1.7 percentage point increase in the probability of transitioning from small conflict to armed conflict. The same increase in aid also significantly increases the likelihood of remaining in a small conflict (by about 1.8 pp) and makes a transition to peace much less likely (about -3.5 pp). We also find evidence that aid can make peaceful societies transition to small conflict or armed conflict. However, we find no evidence in favor of the hypothesis that aid causes transition to civil wars. There is some indication that it prolongs civil wars, but this effect vanishes once we compute this matrix using the controls from the other two columns or simply drop the pacific island of Palau (which is a large outlier in terms of aid).

In a nutshell, we find that bilateral foreign assistance does indeed cause lower intensity conflicts but has little effect once a country is experiencing a civil war. Or even shorter, aid helps to ignite conflict cycles. This effect is only visible once we separate societies into those that are truly peaceful and those who are already experiencing violent turmoil. We find this to be a very plausible result. It supports the view that the state is a prize to be captured and highlights the importance of smaller disputes over resources. At face value, it does not support the opportunity cost perspective although we cannot precisely discriminate among the channels at this stage.

Robustness. We now subject this finding to several robustness checks and perturbations. We proceed in a top down fashion. First, we compare the ordered probit estimator to standard linear models, control function approaches and instrumental variable methods. Second, we examine the sensitivity of our results to the definition of the dependent variable and the independent variable. Finally, we include a variety of controls that have been championed in the recent literature to see if they change our basic conclusions.

The proposed dynamic ordered probit model is reasonably demanding to estimate

Table 6: 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})	-0.0007	0.0129**	0.0130**	-0.0008	0.0128**	0.0131**
	(0.0011)	(0.0060)	(0.0057)	(0.0009)	(0.0058)	(0.0060)
Residuals ($\hat{\nu}_{2it}$)		-0.0138**			-0.0138**	
		(0.0063)			(0.0063)	
Interactions with Lagged States						
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)				0.0063**	0.0063**	0.0075
				(0.0028)	(0.0032)	(0.0073)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)				-0.0103	-0.0103	-0.0125
				(0.0119)	(0.0120)	(0.0161)
Civil War ($a_{2it} \times h_{3,i,t-1}$)				-0.0028	-0.0027	-0.0104
				(0.0052)	(0.0125)	(0.0106)
Lagged States						
Small Conflict ($h_{1,i,t-1}$)	0.2544***	0.2538***	0.2472***	0.2288***	0.2279***	0.2166***
	(0.0308)	(0.0305)	(0.0304)	(0.0345)	(0.0356)	(0.0441)
Armed Conflict ($h_{2,i,t-1}$)	1.1243***	1.1232***	1.1225***	1.1729***	1.1715***	1.1839***
	(0.0788)	(0.0798)	(0.0785)	(0.0990)	(0.0993)	(0.1145)
Civil War ($h_{3,i,t-1}$)	1.7942***	1.7935***	1.7887***	1.8160***	1.8149***	1.8477***
	(0.0845)	(0.0937)	(0.0833)	(0.0868)	(0.0942)	(0.1026)
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. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

and we might wonder if our findings are driven by the structure we impose on the data. Table 6 below addresses this issue. Here we ignore the ordinal nature and estimate our main specification using different linear approaches. Recall that least squares is not really suitable for ordinal outcomes if the number of outcomes in not large and the error distribution is not approximately normal, among other issues. Nevertheless, this comparison is helpful in the sense that it allows us to show that the main results are not driven by our choice of estimator.

All first order effects of aid on conflict are similar to the non-linear models. Column (1) in Table 6 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. 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, the outcomes are almost identical in the case of one endogenous regressor.[13]

The models with interaction terms confirm our findings and generate interesting additional econometric insights. Contrasting columns (4) and (5) to column (3), it is easy to see that correcting for endogeneity again makes the estimated coefficient become positive and highly significant. The behavior of the estimated interaction terms is much more interesting. The coefficients on the three interaction terms are almost numerically identical, no matter if we use the control function estimator or not. However, in column (6), when we use a standard IV approach, their effects become much less precisely estimated while the signs and magnitudes are stable. This is perfectly in line with Bun and Harrison (2014), who recently showed that interaction terms of an endogenous and an exogenous regressor are often identified in linear models *without* instrumenting them separately. 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 less assumptions. However, there is good reason to belief that the CF is just as well suited as IV approaches to purge the remaining endogeneity, if any, from the interaction terms.

We now turn to sensitivity of our results to the operationalization of our key variables. In Table 7 we manipulate both the construction of our conflict measure and our dependent variable. 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 small scale

[13]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.

violence which we have so far neglect is that of terrorism. We chose to do so because in times of major civil conflicts the definition of what constitutes a terrorist act becomes very blurred. In fact as Campos and Gassebner (2013) show countries with a history of civil wars are the ideal training ground for (international) terrorists. Nonetheless terror attacks of course qualify as violent acts voicing disagreement with the government policies. We thus 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. This hardly changes the verdict of our empirical exercise. The only difference is that the statistical significance of the interaction between lagged small conflict and the aid variables loses significance. More importantly, however, the average partial effects of aid remain virtually unchanged.

Column (2) changes the composition of our dependent variable. In our main specification we have focused on ODA because this is typically what people mean when they talk about foreign aid. Here we include OOF to get a broader concept of foreign aid. As can be seen this does not affect our results.

In column (3) we exclude the most interesting but also most politically and strategically-motivated donor in the world: the US (e.g., Kuziemko and Werker, 2006, among many others). Thus results could be affected if the US differs from the rest of the DAC donors substantially in disbursing aid to countries in conflict (if they are important to the US). While the uninteracted aid variable loses both in magnitude and significance the interaction with lagged small states increases in size (and remains highly significant). It is also very interesting to note, that the evidence for endogeneity of aid is weakened. This points indeed to the special role played by the US in disbursing strategically-motivated aid. Two things need to be kept in mind, however. First, the IV approach is not invalidated if endogeneity were truly absent it would only be inefficient to use it. Second, omitting the most important donor is not really a feasible option when analyzing foreign aid flows. Moreover and most importantly the calculated average partial effects of aid remain comparable with our main specification.

The complex and demanding estimation procedure persuaded us to opt for a parsimonious model regarding additional control variables. In Table 8 we extend the set of control variables. Before going into the details it is reassuring that or conclusions are hardly affected by the set of control variables employed. This is particularly true for the average partial effect of aid.

Column (1) sheds light on the role played by the immediate neighborhood. Interestingly we find little evidence of spillover effects of conflict. This finding is in line with Bosker and de Ree (2014) who only find spillover effects for ethnic conflicts. Column (2) addresses the potential concern that oil is a literal fuel for conflict and that major oil exporters might exhibit different aid patterns (Fearon, 2005). We find no evidence for either hypothesis. In columns (3) to (6) we check for the importance of the political

sphere in affecting aid and conflict. This comes at the cost of a reduced sample. We find an important role played by political instability as it escalates conflict. More relevant to our analysis is the fact that there does not seem to be relation to aid since our results are unaffected by the inclusion of political instability. Including a country's Polity IV score (to measure the level of democracy) is just as insignificant as taking the level of democracy in the neighborhood into account (columns 4 and 5). This verdict changes if we focus on the truly democratic countries in column (6). Following the established definition of Dixon (1994) we define democracies as countries with a Polity IV score greater or equal to six. While these countries are less likely to experience conflicts the role of aid is not affected by the inclusion of the democracy dummy variable.

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

Table 7: Robustness: Alternate Measures of Conflict and Foreign Aid

	Perturbations on LHS or RHS:		
	(1)	(2)	(3)
VARIABLES	With Terror	With OOF	Without USA
Aid to GDP (a_{2it})	0.158***	0.0989*	0.0761
	(0.0589)	(0.0539)	(0.0570)
Residuals ($\hat{\nu}_{2it}$)	-0.164***	-0.111**	-0.0938
	(0.0581)	(0.0550)	(0.0574)
Interactions with Lagged States			
Small Conflict ($a_{2it} \times h_{1,i,t-1}$	0.0108	0.0223***	0.0300**
	(0.00982)	(0.00781)	(0.0119)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.0154	-0.00933	-0.0157
	(0.0191)	(0.0180)	(0.0284)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.00781	-0.00330	-0.0259
	(0.0255)	(0.0172)	(0.0377)
Lagged States			
Small Conflict ($h_{1,i,t-1}$)	0.746***	0.577***	0.583***
	(0.0738)	(0.0758)	(0.0769)
Armed Conflict ($h_{2,i,t-1}$)	2.462***	2.115***	2.126***
	(0.204)	(0.181)	(0.189)
Civil War ($h_{3,i,t-1}$)	3.817***	3.446***	3.492***
	(0.251)	(0.227)	(0.242)
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

Note(s): Perturbations based on table 4 column 1. 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 (not shown). Significant at: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 8: Robustness: Additional Covariates

	Dependent Variable: Ordered Conflict					
VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
Aid to GDP (a_{2it})	0.0962*	0.0932	0.0792	0.0725	0.0858	0.0845
	(0.0571)	(0.0577)	(0.0571)	(0.0518)	(0.0597)	(0.0600)
Residuals ($\hat{\nu}_{2it}$)	-0.110*	-0.107*	-0.0934*	-0.0860*	-0.101*	-0.101*
	(0.0582)	(0.0584)	(0.0553)	(0.0506)	(0.0579)	(0.0581)
Interactions with Lagged States						
Small Conflict ($a_{2it} \times h_{1,i,t-1}$)	0.0223**	0.0212**	0.0237**	0.0255**	0.0245**	0.0260**
	(0.00868)	(0.00839)	(0.0112)	(0.0102)	(0.0113)	(0.0112)
Armed Conflict ($a_{2it} \times h_{2,i,t-1}$)	-0.00968	-0.00995	-0.0105	-0.00677	-0.0101	-0.00700
	(0.0191)	(0.0192)	(0.0205)	(0.0207)	(0.0195)	(0.0191)
Civil War ($a_{2it} \times h_{3,i,t-1}$)	-0.000997	-0.00107	-0.00296	-0.000748	-0.000420	0.000717
	(0.0248)	(0.0252)	(0.0277)	(0.0262)	(0.0273)	(0.0276)
Added Controls						
Neighbor in Small Conflict	0.122					
	(0.0782)					
Neighbor in Armed Conflict	0.0597					
	(0.0904)					
Neighbor in Civil War	0.161					
	(0.125)					
Island	0.247					
	(0.203)					
Oil Exporter		0.0429				
		(0.168)				
Political Instability			0.216***			
			(0.0832)			
Polity IV (revised)				-0.0129		
				(0.00935)		
Regional Polity IV					0.0176	
					(0.0190)	
Democracy						-0.355***
						(0.134)
Summary Statistics						
$N \times T$	4500	4500	3708	3672	3708	3708
N	125	125	103	102	103	103

Note(s): 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 (all not reported, detailed results available on request). 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 (not shown). Significant at: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

6 Conclusion

This paper has made three major contributions to the literature on the effects of development aid on conflict. First, we have proposed a new measure for conflict by combining data on civil wars with data on low-level conflicts as measured through government purges, assassinations, riots and guerrilla warfare. Second, contrary to previous studies, our ordinal conflict measure allows us to analyze escalation and/or de-escalation effects of conflicts with different intensity levels and thus to analyze conflict dynamics much more detailed and explicit. Third, we use a new instrumental variable to predict bilateral aid that combines cross-country variation and time variation through interacting donor government fractionalization with the recipient's probability of receiving aid. We propose a novel econometric strategy to investigate the causal effect of aid on conflict and conflict transition by using an ordered probit setting with history dependence, correlated random effects and a control function approach to take account of the endogenous character of aid.

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 country turns from peace to low-level conflict, there is no evidence that aid increases the probability of a direct shift from peace to outright civil war. One explanation for this finding is that aid is not distribution-neutral resulting in dissatisfaction among the people, which in turn can result in low-level conflict but is not likely to result immediately in civil war.

Moreover, we find evidence for a prolonging and even escalating effect of aid on conflict if a country is already experiencing a conflict of any intensity. This result indicates that aid might be used as a means to finance an ongoing conflict rather than for its intended purpose only. At the same time, the motivation – to keep the war going – increases when the potential gains from war are raised through aid. These findings stress that donor countries have to be aware of unintended consequences when giving aid to conflict-ridden countries. As the effects of aid are not alike for all types, future research should focus more on these differences in order to develop policy implications on what types of aid should be given to conflict-prone countries.

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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, Luxembourg, Netherlands, New Zealand, Norway, Poland, Portugal, Slovak Republic, 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, 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 Kittis 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.