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Mostly harmless? A subnational analysis of the aid-conflict nexus

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Mostly Harmless?
A Subnational Analysis of the Aid-Conflict Nexus

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Mostly harmless? A subnational analysis of the aid-conflict nexus*

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November 2017

Abstract

Although most aid projects are aimed at local development, most research on the aid-conflict nexus is based on the country-year as unit of analysis. In contrast, this study examines the link between aid commitments and conflict intensity at the local level for three African countries between 1999-2008, using data from a unique dataset containing information on local aid allocations. The data shows that in general the spatial interdependence between aid and conflict is low, as aid is allocated relatively close to the capital and conflicts tend to occur in the peripheral areas. Fitting a Bayesian linear regression model the empirical analysis finds that there is no strong correlation between changes in lagged aid commitments and changes in conflict intensity. Looking at the extensive margin the results do show that fungible aid is correlated with increased conflict risk, in line with rent-seeking behaviour, but the estimated magnitude of the coefficient is very small. The results are stronger at the district level compared to the province level, suggesting that the possible link between aid and conflict is highly localised.

JEL-Classification: C11, D74, F35, O55
Keywords: Foreign aid, armed conflict, Africa

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Introduction

Annually billions of dollars of foreign aid flow from developed to developing countries with the aim to improve development outcomes such as reducing malnutrition and poverty. Setting development policy, in recent years there has been increased attention for the possible security risks associated with underdevelopment, as poor areas are perceived to be breeding grounds for insurgencies, and as such aid can be used as a tool to increase stability. However, the empirical literature on the aid-conflict nexus has produced diverging results concerning the potentially violence-reducing effect of foreign aid, which might not be surprising given the mixed results on aid effectiveness (Roodman, 2007; Easterly and Pfutze, 2008; Doucouliagos and Paldam, 2008, 2011). This study does not aim to address the general debate on foreign aid effectiveness in general but instead focuses on the aid-conflict nexus using microlevel data, in contrast with the majority of the literature which uses the country-year as unit of analysis. Using such a relatively coarse level of aggregation means that useful information on the dynamics of aid and conflict is potentially lost as most aid projects are targeted at local development (Findley et al., 2011; Berman et al., 2013) and violence tends to be highly localised (Buhaug and Gleditsch, 2008). Using a more disaggregated approach is therefore more advantageous as it takes into account these local dynamics, which could help improve our understanding of how aid possibly influences conflict.

There is a small number of studies using subnational data such Berman et al. (2013) on Iraq, Tahir (2015) on Pakistan, and work by Arcand et al. (2011) and Crost et al. (2014) on the Philippines as well as the studies by Strandow et al. (2014) and Wood and Sullivan (2015) focusing on Africa. Most of these studies focus on a particular conflict in a specific country which might make their results hard to generalise. This study extends the current literature by providing a cross-country analysis examining the link between aid allocations and conflict intensity at the local level in the Democratic Republic of the Congo (DRC), Ethiopia, and Sudan between 1999-2008. Using a unique dataset on local aid allocations the data is fitted, aggregated at province and district level, using a Bayesian linear regression model. This study is most similar to the work of Strandow et al. (2014), the main difference is that their analysis focuses on the effect of aid commitments in contested areas whereas I examine the more general effect of aid allocations on conflict intensity. In both cases though the focus is on the potential local rent-seeking incentives that foreign aid provides. This study also contributes to the literature on conflict intensity O'Loughlin et al. (2012); Hendrix and Salehyan (2012); Costalli and Moro (2012); Maystadt et al. (2014); Hegre et al. (2009); Raleigh and Kniveton (2012). Focusing on conflict intensity arguably provides better

insights concerning conflict dynamics, keeping the full information of the conflict data, this in contrast with commonly used cruder binary measures for violent armed conflict.

The statistical analysis provides little empirical evidence for a strong correlation between foreign aid commitments and conflict; pushing the results hard a two standard deviation increase in aid commitments is correlated with a 0.4% decrease in conflict intensity, measured by battle-related fatalities. This potential negative link seems stronger for nun-fungible aid compared to fungible aid. Moving from the intensive to the extensive margin does show a positive correlation between foreign aid and conflict, linking aid with increased conflict risk, echoing earlier results Strandow et al. (2014); Wood and Sullivan (2015) but the magnitude of the effect is relatively small. Given the quality of the available data on foreign aid, there are some drawbacks concerning the statistical inference, this means that caution needs to be taken with drawing too strong conclusions about the possible link between aid and conflict and the possible mechanisms. Nonetheless, the results hold using a number of different model specifications, measurements for the outcome variable, and across different levels of spatial aggregation.

Linking foreign aid and conflict

The quantitative research on the effect of foreign aid on conflict has produced some inconclusive results, particularly in relation to the direction of the effect.

One strand of the literature argues that aid flows possibly help improve stability, for instance by increasing public spending and thereby reducing grievances towards the government as well as increasing the opportunity costs of conflict, which makes rebel recruitment more difficult; or by increasing military expenditures thereby providing a strong deterrent (Collier and Hoeffler, 2002, 2007). Following Collier and Hoeffler (2002), foreign aid induced improvements in government capacity could reduce conflict risk as aid i) augments the government budget, relaxing budget constraints, ii) affects economic growth, although this is heavily contested (Doucouliagos and Paldam, 2008, 2011), and iii) helps diversify the economy, making it less dependent on primary commodities. Focusing on the direct channel between development assistance and stability, where aid relaxes the government's budget constraints, de Ree and Nillesen (2009) indeed find that higher levels of foreign aid are correlated with a reduction in conflict duration; possibly the result of increased government capacity according to the authors.[1] In similar vein, Savun and Tirone (2011) show that stability improves in countries

[1] They are unable to establish a causal link however as they discuss the results.

during a democratic transition when receiving foreign development assistance; so called democracy aid helps reduce the commitment problems of the government that occur during the democratisation process as the authority of the central government weakens and uncertainty increases; subsequently the likelihood of conflict decreases due to democracy aid.

In contrast, the other literature strand provides a more pessimistic perspective linking aid to increases in conflict risk and intensity. In a seminal study Grossman (1992) analyses the insurgents' objective to capture the state for financial advantages and argues that foreign aid will increase the prize associated with capturing the state and thus increase rent-seeking incentives, something reflected in the results by Addison and Murshed (2001). Volatility in aid flows plays an important role in explaining its potential destabilising effect. For instance Arcand and Chauvet (2001) find that although aid can have a stabilizing effect, the uncertainty of aid flows will actually increase conflict likelihood as the volatility in aid money leads to higher uncertainty levels which fosters instability. Similarly, Nielsen et al. (2011) show that large negative shocks in aid money are linked to shifts in the domestic power balance which again increases conflict likelihood while Djankov et al. (2008) find that negative aid shocks are related to a deterioration of institutional quality.

At subnational level an important implication of local aid allocations is that it provides a lootable resource as aid can be appropriated by insurgents in order to supplement income or as material support for their operations (Blouin and Pallage, 2008), both of which will potentially increase conflict duration (Findley et al., 2011). An example of this is the theft by al-Shabaab in Southern Somalia between late 2011 and early 2012 of about $500,000 worth of humanitarian aid (Department for International Development, 2013), while back in 1984-85 aid money to help the famine victims in Ethiopia was used by rebels to buy weapons (Central Intelligence Agency, 1985). Concerning the looting of aid and conflict, using data at the country level Nunn and Qian (2014) show that increases in U.S food aid correspond withs increases in both the incidence and duration of civil conflict, an effect that is more pronounced in countries with a recent spell of conflict.[2]

Most studies discussed so far use data aggregated at the country-year level

[2]Collier and Hoeffler (2002) argue that food aid is the only type of aid that can be appropriated by insurgents during a conflict. Also note that Christian and Barrett (2017) provide evidence that the results as presented by Nunn and Qian (2014) are due to a spurious trend.

which means a loss of information concerning the local dynamics of the aid-conflict nexus. In recent years there have been a number of studies trying to disentangle the relation between aid and conflict at the local level using microlevel data.[3] Using data on Iraq, Berman et al. (2013) find that U.S. military development spending per district reduces violence intensity, although the estimated effect is contingent on a number of other factors such as the presence of a large number of U.S. troops and the availability of development expertise. Studying the Philippines, Arcand et al. (2011) show that between 2003-2006 increases in the intensity of violence around aid projects are related to the insurgents' ideology and not just an effect of the level of aid itself, results consistent with their theoretical rent-seeking model. Crost et al. (2014) examine the effect of a large development programme in the Philippines on conflict intensity between 2002-2009 and find that municipalities that are barely eligible for receiving aid from this programme experience large increases in fatalities as the authors argue the insurgents try to sabotage the project. For Pakistan Tahir (2015) finds that aid increases conflict risk as it erodes the fiscal capacity of the state.

Closely related to the study presented in this paper is the work by Strandow et al. (2014) who examine the effect of aid distribution in contested areas during ongoing wars in Sub-Sahara Africa and find that concentrated aid increases military fatalities. Similarly Wood and Sullivan (2015) find that aid commitments increase conflict probability for a number of African countries.

From the literature a number of mechanisms emerge linking foreign aid and conflict, including rent-seeking behaviour, sabotage, and changes in state capacity. Of interest to this study is how these mechanisms influence conflict at the local level. For many developing countries foreign aid is an important source of income for the national government, augmenting state budget, which means that foreign aid can help relax some of the budget constraints a government faces. As such it could lead to an increase in public spending thereby increasing the government's visibility in underdeveloped areas lacking public services, thereby improving its legitimacy. As increased public spending will likely improve the government's standing with the local population, at the same time it will harm the support for local insurgency groups. Local aid projects might therefore become the subject of sabotage Arcand et al. (2011); Crost et al. (2014), such as for instance health projects which have been targeted by Islamist insurgents in Nigeria and Pakistan. Indeed, data from the Aid Worker Security Database shows that over the

[3] Böhnke and Zurcher (2013) study the impact of aid on perceived security in Afghanistan and is therefore not directly comparable with the other works discussed here.

years aid workers have been increasingly the target of violent attacks killing 1651 people in 2250 separate incidents (Humanitarian Outcomes, 2017). We would therefore expect that local aid projects are correlated with higher conflict risk.

Foreign aid can also be linked to a reduction in the provision of public goods (Svensson, 2000) with the exception of defence expenditures (Collier and Hoeffler, 2007) which translates to an increase in government deterrence. Given that foreign aid provides rent-seeking incentives this also entails that whomever controls the government will control the distribution of foreign aid, which means that rebels might try to capture the capital or areas that receive substantial amounts of aid and indeed Addison and Murshed (2001) finds at the cross-country level that aid is associated with increased conflict risk. In terms of the location of conflict events, Strandow et al. (2014) argue that violence will be more likely to occur in locations at a distance from the capital, out or reach of the government, as the government will concentrate its forces around the capital and rebels will prefer to avoid direct confrontations with a stronger adversary. Additionally, given the allocation of aid to different regions, this implies that rebels do not necessarily have to capture government control to appropriate aid as they can make use of the opportunity of access to aid in locations closer to where they normally operate. Foreign aid provides additional income or material support for insurgents (Blattman and Miguel, 2010) which means that it creates a local rapacity effect whereby aid can be looted. Given these rent-seeking incentives we would expect to observe conflict events close to the foreign aid source. Appropriating aid also entails that it will free up rebel resources, the same way as it did for the government, which means a possible increase in conflict risk (Anderson, 1999).

Data and measurement

First and second level administrative divisions, corresponding to provinces and districts, are used as unit of analysis as they capture the social heterogeneity that follows sub-national boundaries (Østby et al., 2009; Aas Rustad et al., 2011).[4] Two different levels are used as the statistical results could be driven by the level of aggregation as a result of modifiable areal unit problem (MAUP) (Gehlke and Biehl, 1934; Openshaw, 1983; Fotheringham and Wong, 1991) and also to account for possible displacement effects (Maystadt et al., 2014).

[4]Data source: Global Administrative Unit Layers (GAUL) from the FAO), reference year 1999.

Foreign aid

Measurements on local foreign aid allocations are taken from the UCDP/AidData dataset constructed by Findley et al. (2011) which includes detailed information on the location of about 70,000 aid projects for the period 1989-2008 covering 22 countries in Sub-Sahara Africa; it is currently the most comprehensive geocoded aid dataset available. This dataset is based on AidData (Tierney et al., 2011) which contains detailed information on development finance (loans or grants) allocated to developing countries with the intend to promote economic development. It includes data on finance by governments, official government aid agencies, and inter-governmental organisations but not from non-governmental organisations, the private sector or military assistance. The information in the dataset is compiled from a wide range of sources such as annual donor reports and project documents from bilateral and multilateral aid agencies as described in Tierney et al. (2011). For each region the aid allocations for the different projects, measured in the log of constant U.S. dollars, are aggregated to the annual level.

Figure 1 shows the annual number of aid projects aggregated at country level. One serious concern using this data is the bias it introduces due to sample selection as only countries in Sub-Sahara Africa are included that experienced conflict between 1989-2008, and moreover in most cases only conflict-years are included. Two studies using the same dataset, Strandow et al. (2014); Wood and Sullivan (2015), exploit all of the available data using a matching design to try and account for the selection bias. Although this approach does allow one to exploit within-country variation in aid allocations, it does omit information on aid commitments in most non conflict years, since is not available for the majority of countries. This could be problematic because some countries experience breaks in conflict such as for instance Angola, the Central African Republic, Rwanda, and others. Additionally, although this is the most comprehensive dataset available it is unlikely that it includes the total number of aid projects, as highlighted in Strandow et al. (2014). An inspection on data availability shows that potentially missing data might not be random in terms of temporal coverage; the number of aid projects per year between 1989-1997 is considerably lower, only 16% of the total, compared to the later period from 1998 onwards. This study therefore takes a more conservative approach to deal with the sample selection bias, and the presence of measurement error, by limiting the analysis to the period 1999-2008 and focusing on three countries: the Democratic Republic of the Congo (DRC), Ethiopia, and Sudan. These countries have good temporal coverage, are some of the main recipients of foreign aid in Africa, receiving about 30 billion US dollars in foreign aid between 2000-2011, and display

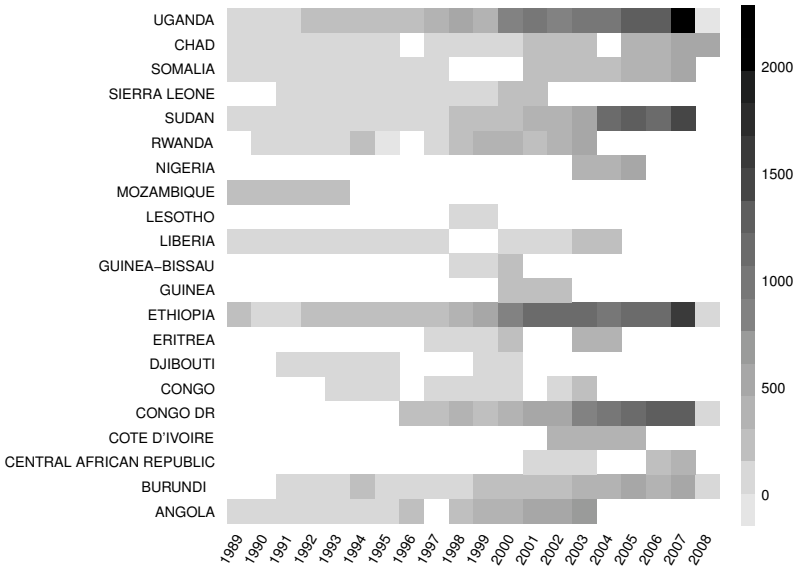

Figure 1: Data availability AidData. Figure shows the number of aid projects per country-year.

substantial within-country variation in both conflict and aid allocation.[5]

Finally, one last shortcoming of the data is that it only contains information on aid commitments and does not track, or provide information, on aid disbursements. Given that there likely is a delay between commitment and the actual disbursement in the intended region, the estimation will use lagged aid commitments to account for this delay Due to the absence of information on disbursements I cannot account for longer delays than one year between aid commitments and disbursements or for cases where there is not a one to one relation between commitments and disbursements. Nonetheless, aid commitments likely shape expectations of insurgents and provide an incentive for them to try and control a particular region (Strandow et al., 2014) and they are the best proxy for actual disbursements available Wood and Sullivan (2015). The limitations of the data does imply a constraint concerning the estimation of the effect of aid on conflict which ultimately relies on the assumption that aid commitments will have a short term effect on conflict intensity.

[5]Additionally, they are comparable in size, particularly the number of sub-national administrative units. A reason why Uganda (proliferation of new districts from 2000 onwards) and Burundi (too small) are not included.

Civil conflict

Data for the outcome variable is taken from the UCDP Georeferenced Event Dataset v.3 (Sundberg et al., 2010; Sundberg, 2013) which is the most accurate geocoded dataset on conflict currently available (Eck, 2012; Weidmann, 2015, 2016). A conflict event is defined as "a phenomenon of lethal violence occurring at a given time and place" and the dataset provides information on the location of the event, given in longitude and latitude coordinates, the time of occurrence, and the number of fatalities.
Given that according to the definition a conflict event has to be lethal, this entails that conflict types at the lower end of the violence spectrum, such as riots or protests, are not accounted for. Additionally, the dataset generally only includes events that are associated with a conflict that has generated at least 25 battle-related deaths in a year meaning that incidental fatal events are omitted.

This study uses the number of fatalities, aggregating the point data to regional level, measuring the intensity of conflict. There are two possible caveats concerning the fatality data. First, the conflict data relies for a large part on information from media reports, although it is supplemented with other information, which means that it could be subject to reporting bias as smaller events might not be picked up by the media as they might not be news-worthy enough. Second, the data only includes battle-related fatalities which might be a conservative estimate of the true number of fatalities associated with a conflict event, although research has shown these numbers to be reasonably accurate (Weidmann, 2015). In any case, this possible measurement error pertains to the variable on the right hand side of the equation, therefore the estimates will be unbiased but the uncertainty associated with the estimate will probably be larger.

Estimation framework

To estimate the relation between conflict and aid a first-differences model is used similar to Berman et al. (2013). The model has the following functional form:

$$\Delta C_{it} = \gamma \Delta A_{it-1} + \rho \Delta \sum_k W_{ikt} C_{kt} + \beta \Delta C_{it-1} + \theta_t \tag{1}$$

Outcome variable C_{it} is the change in the log count of the number of fatalities in region i at time t which is linked to, among other, the change in aid

commitments between $t-2$ and $t-1$. The model therefore estimates the relation between changes in aid commitments and conflict fatalities, captured by γ; testing the presence of local rapacity effects where we expect higher conflict intensities to follow increases in aid commitments. As discussed in the literature section aid projects could also be the target of sabotage, but unfortunately in this case there is no information on the initiator of conflict events, unlike the study by Crost et al. (2014), meaning that that particular hypothesis cannot be tested. Additionally, note that this empirical framework only accounts for violent appropriation of aid as there is no data available on non-violent means such as taxation for instance.

A main concern trying to estimate the effect of foreign aid on conflict is endogeneity, specifically reverse causality where conflict intensity influences the amount of aid committed to a region. As a donor has to balance risk and rewards a risk-adverse donor might decide not to commit aid to a conflict-struck region, instead allocating it to other locations, thereby diverting aid away from the conflict region. This means that conflict will create what are called aid orphans. On the other hand, conflict might actually attract aid when it is the donor's intend to ameliorate conditions. Using the same dataset as this study, Bezerra and Braithwaite (2016) find that conflict-struck regions are more likely to see an increase in foreign aid; specifically conflict in year t corresponds to an increase in aid commitment in year $t+1$. To account for this endogeneity the model therefore includes lagged changes in aid commitment, linking changes between $t-2$ and $t-1$ to predict the change in conflict intensity between $t-1$ and t. This means that in this framework for conflict to influence changes in aid commitments, donors should be able to anticipate conflict. This seems unlikely given the paucity of information on how aid actually influences conflict as discussed by Strandow et al. (2014) who argue that in the case of donor anticipation this probably leads to an increase in variation in aid commitments; and in the absence of a systematic effect across donors this will not bias the results.[6] In general development organisations tend to follow rather than preempt disasters (Ó'Gráda, 2009).

Lagging the aid variable is largely an ad hoc measure, in the absence of a good instrument, to deal with simultaneity bias and as a consequence this means that the potentially violence reducing effect of aid will be understated. Practically this means that caution needs to be taken interpreting the estimates, which will represent correlations between changes in aid commitments and conflict intensity, as given the limitations of the data it is hard to identify causality.

[6] In the case of Ethiopia, the war with Eritrea has had very little influence on the commitments it received which actually increased over time (Borchgrevink, 2008).

As mentioned there is little known about how conflict influences donors' behaviour, both in the quantitative and qualitative literature ,and therefore to what extend violence influences local aid allocations in the sampled countries is not really clear We do know that in the past access to aid has been used as a weapon, for instance by the Ethiopian government in the wake of the 1983-85 famine, and it is therefore not unlikely that some form of regional favouritism exists where regions loyal to the regime receive more aid (Hodler and Raschky, 2014). Additionally, donors could be somewhat constraint by the recipient country's domestic policy concerning to which areas aid can be allocated; in the context of civil war a central government might obstruct aid allocations to restless regions. There is nonetheless little empirical evidence for this type of dynamic. Indeed in some cases the recipient country can be very cooperative such as Sudan which following the 1998 famine allowed international organisations to supply both emergency and development aid in contested regions as part of Operation Lifeline Sudan (Taylor-Robinson, 2002). Although in Sudanese case aid allocation decisions are influenced by humanitarian considerations, based on the need of the population, in other cases these decisions are shaped more in accordance with the paradigm that underdevelopment is seen as a security risk, for instance in the DRC where areas experiencing fighting received the majority of aid (Marriage, 2010).

Given that conflicts are often highly localised (Buhaug and Gleditsch, 2008) the outcome variable might exhibit spatial autocorrelation meaning that the observed change in the level of conflict intensity in region i could depend on changes in neighbouring regions. Therefore to account for possible spatial interdependence the spatial lag of the outcome variable - $\sum_k W_{ikt} C_{kt}$ - is included in the model. This spatial lag is a weighted conflict measure based on conflict in the k neighbouring regions of i. W is calculated using a binary spatial weights matrix based on first order contiguity, i.e. only including i's direct neighbours. Spatial-weights matrix W is not row-standardised as this would imply that the influence of region j on i decreases when the number of neighbour increases. This would entail that the effect of conflict in neighbouring areas is larger when a region has relatively few neighbours which is not theoretically justifiable in this case.[7] The sign and strength of the interdependence in the outcome is estimated by ρ.[8]

[7]Note though that LeSage and Pace (2014) show that the estimates and inferences from the regression model should not be sensitive to particular specifications of the spatial weights structure.

[8]Please see the work by Beck et al. (2006); Franzese and Hays (2007) for an extensive overview of model specification in the presence of interdependence.

Spatial autocorrelation is similar to temporal autocorrelation with the main difference that spatial autocorrelation can move in either direction. This means that when including a spatial lag, estimating the model using classic methods like OLS would lead to simultaneity bias where the errors are no longer independent. Therefore the data is fitted using Bayesian linear regression which has the advantage of producing consistent estimates in the presence of spatial interdependence (Lesage, 1997). For this study the model is fitted using a Gibbs sampler, JAGS (Plummer, 2014), which provides the advantage that it allows for non-constant variance over space. The conditional distribution of spatial parameter ρ can be integrated out solving the issue of multiple integration in the Bayesian setting (Lesage, 1997). Note that for the Bayesian model in general, all parameters are estimated using conditional probability which means that the parameters are returned as a probability distribution in the form of a posterior density (Jackman, 2000). To account for temporal dynamics the model also includes the lagged outcome variable capturing common trends (Plümper and Neumayer, 2010) and year indicators, θ_t, are included to account for common shocks.

Finally, to estimate the model a prior distribution needs to be specified for parameters such as γ and ρ, and in this case a standard noninformative or diffuse prior is used with $N(0, 10)$ distribution. The prior distribution should not influence the posterior (Gelman et al., 1995) and choosing a diffuse prior in this case means that the estimated coefficients will be similar to those obtained by maximum likelihood estimation.

Exploratory data analysis

Figure 2 shows the geographic location of aid projects and conflict events for each individual country. Based on the aid-conflict literature we would expect that i) aid and conflict cluster together in space and that ii) conflicts are located at a distance from the capital. In general the data does not show a high degree of overlap between aid and conflict, except maybe for Sudan; as a matter of fact Sudan is one of the countries where violence against aid workers is very common (Stoddard et al., 2009). In the Ethiopian case there is a certain degree of separation where most conflict occurs in the Ogaden whereas aid projects tend to be allocated in the Western part of the country; for the DRC aid allocations are quite disperse whereas conflict is more localised, specifically in the Kivus in the Eastern part of the country.

As a more formal test for spatial interdependence between aid allocations and

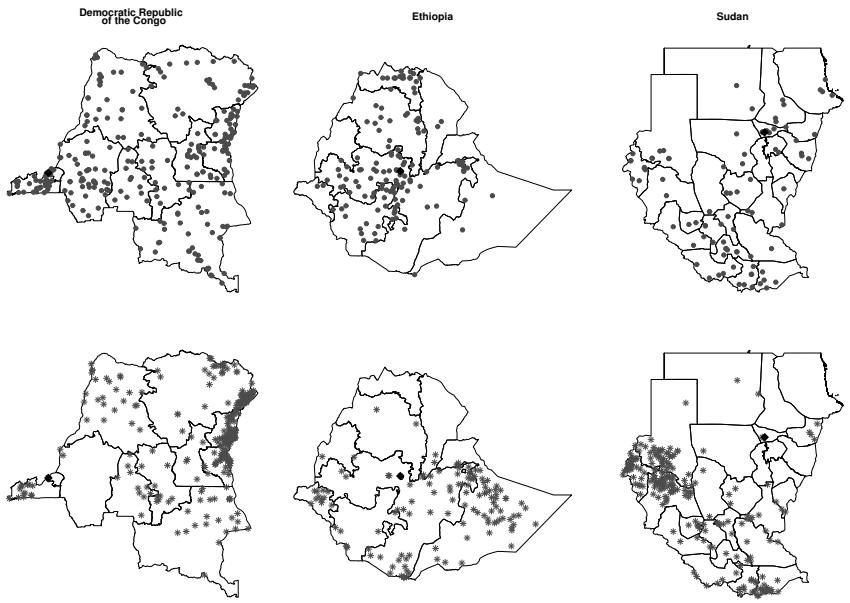

Figure 2: Unique locations for aid allocations (upper panel, dots) and conflict incidence (lower panel, asterisks) between 1999-2008. The black diamond indicates the capital of each respective country. Data source: UCDP/AidData.

conflict events the Nearest Neighbour Distance (NND) is used, calculated as the distance between an aid project and the nearest conflict event.[9] If aid indeed is correlated with conflict events because it either is the subject of sabotage or because it is subject to looting, then we expect the distance between local aid commitments and the nearest conflict event to be relatively small as a smaller distance corresponds to stronger interdependence. However, this is not what the data shows as the average distance between aid and conflict is about 270 Kilometer on average with a median distance of 185 Kilometer. These distances are relatively large compared with the values for aid and conflict separately, for instance the average distance between two conflict events is about 150 Km with a median of about 70 Km, the same applies to local aid commitments. For 23% of the observations (225 cases out of a 996 observations) the distance between an aid project and conflict is below 50 Km indicating some stronger interdependence and provides some support for the notion that aid might provide incentives for conflict.

[9] Aid allocations are lagged by one year to account for simultaneity and the data is subset to include only aid commitments and conflict events with the highest geoprecision limiting the point data to include 2405 aid projects and 1053 conflict events. See figure A1 for the distribution of distances.

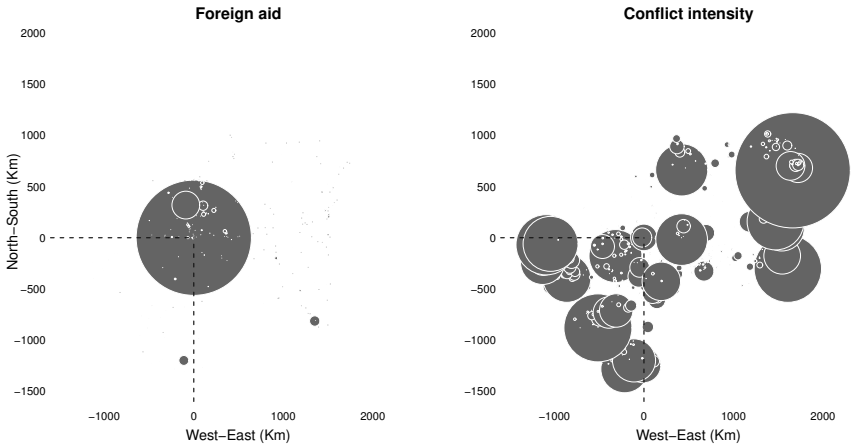

Figure 3: Distance to capital in Km. Per location the amount of aid committed or the number of battle-related fatalities is aggregated; the bubbles therefore represent the relative size of aid flow or conflict intensity.

Strandow et al. (2014) argue that aid will push conflict away from the capital as foreign aid flows will create incentives for rent-seeking but at the same time strengthen government capacity, although in the peripheral areas the government might still be relatively weak. This line of thought is not too different from the strategy applied in both Iraq and Afghanistan by the Americans where the capital was brought under control and thereafter stabilising greater neighbourhoods as security would spread as an oil-stain. Figure 3 illustrates the distance to the capital for both aid and conflict, where circle size represents either the amount of dollars or the number of fatalities.; it illustrates that aid commitments are centered around the capital whereas conflicts tend to occur in the more peripheral areas. One can only speculate about the reasons for this particular pattern. One the one hand it could indeed be the case as the literature suggests that aid strengthens the government positions and pushes conflict away from the capital, or that aid indeed fosters local development reducing conflict risk. One the other hand it could be that donors are risk-adverse, or constraint by the domestic policy of the recipient country, and commit aid only to relatively secure locations, although this would contrast with for instance Marriage (2010); Bezerra and Braithwaite (2016).

Besides the spatial correlation between aid and conflict we can also examine the temporal correlation as is done in figure 4 which plots the changes in aid commitments and number of fatalities, both aggregated at country level. From the figure no strong correlation between the two time-series emerges for any country, indeed correlations are low at 0.13 and 0.32 for the DRC and Ethiopia respectively, the correlation is only stronger for Sudan at -0.61 linking positive changes in aid with decreases in fatalities.

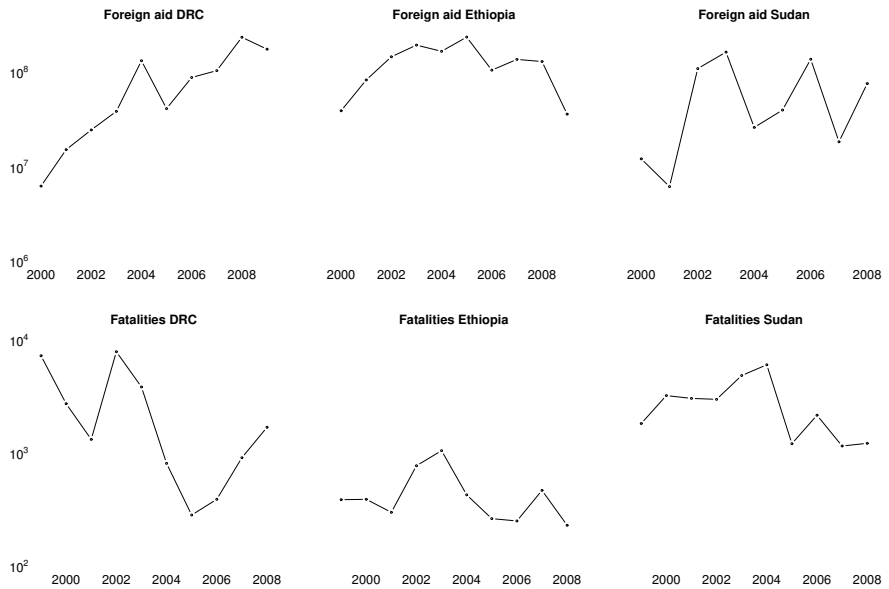

Figure 4: Time series for changes in aid commitments or number of battle-related fatalities aggregated for each country.

Regression results

Table 1: Fitting changes in conflict intensity (Bayesian linear regression)

Specifications	(1)	(2)	(3)	(4)	(5)
Panel A: Province level N = 432					
Foreign aid	−0.1		−0.1		
	(−0.4; 0.2)		(−0.4; 0.3)		
Foreign aid$_{t+1}$		0			
		(−0.4; 0.4)			
Foreign aid to government			0	−0.1	−0.1
			(−0.5; 0.5)	(−0.5; 0.4)	(−0.6; 0.4)
Fungible aid				0.1	0.1
				(−0.2; 0.4)	(−0.3; 0.4)
Non-fungible aid				−0.2	−0.3
				(−0.6; 0.1)	(−0.6; 0.1)
Spatial lag conflict	0.3	0.3	0.3	0.4	0.4
	(0; 0.7)	(0; 0.7)	(0; 0.7)	(0; 0.7)	(0; 0.7)
Temporal lag conflict	−1.2	−1.2	−1.2	−1.2	−1.4
	(−1.6, −0.9)	(−1.6; −0.9)	(−1.6, −0.9)	(−1.6; −0.9)	(−1.7; −1.0)
pD	13.6	13.1	14.0	15.8	72.2
DIC	1726.6	1726.4	1728.1	1729.4	1807.1
Region-specific year trend	No	No	No	No	Yes
	(1)	(2)	(3)	(4)	(5)
Panel B: District level N=2340					
Foreign aid	0		0		
	(−0.11; 0.10)		(−0.11; 0.10)		
Foreign aid$_{t+1}$		0			
		(−0.12; 0.09)			
Foreign aid to government	−0.05		−0.05	−0.05	−0.05
			(−0.20; 0.10)	(−0.20; 0.10)	(−0.20; 0.11)
Fungible aid				−0.01	−0.02
				(−0.11; 0.08)	(−0.13; 0.09)
Non-fungible aid				−0.02	−0.02
				(−0.13; 0.08)	(−0.13; 0.08)
Spatial lag conflict	0.51	0.51	0.51	0.51	0.50
	(0.40; 0.61)	(0.40; 0.61)	(0.41; 0.62)	(0.41; 0.61)	(0.38; 0.61)
Temporal lag conflict	−1.22	−1.22	−1.22	−1.22	−1.26
	(−1.32, −1.11)	(−1.32; −1.11)	(−1.32; −1.11)	(−1.32; −1.11)	(−1.37; −1.14)
pD	12.0	13.0	13.5	15.8	318.4
DIC	7801.1	7801.9	7803.1	7806.1	8299.98
Region-specific year trend	No	No	No	No	Yes

Notes. Table presents point estimates with their 95% intervals between parentheses. All models estimated with year indicators. Estimates are taken as the mean from 3 parallel chains with 10,000 iterations each where the first 2,500 are discarded as burn-in, thinning rate was set to 10. Priors are $N(0, 10)$. All models converged based on a visual inspection of the traceplots for the parameters of interest and the values for the $\hat{R}$ statistic which was below the 1.05 threshold in all cases. Since the input variables are all placed on a common scale, centered around the mean and divided by two standard deviations, in order to facilitate easier comparison, they can be interpreted as the effect of moving from low to high values (Gelman, 2008).

Table 1 presents the posterior means along with the 95% interval (in parentheses). Column 1 reports the results of the model specified according to equation 1. Based on the recent literature the expectation is that positive

change in aid commitments correspond to positive changes in conflict intensity as aid money creates rent-seeking opportunities and aid projects could be subject to sabotage by insurgents. The results show that at the province level the posterior mean actually has the opposite sign, indicating a negative link between aid and conflict where increases in aid commitments are followed by a reduction in conflict intensity levels in the following year (panel a, col. 1, table 1). Although the probability of the direction of the effect is relatively high at 0.70, the estimated magnitude is small though; a two standard deviation increase in aid commitments is associated with just a 0.1% reduction in conflict intensity. Changing the unit-of-analysis to districts, to capture dynamics at a higher level of disaggregation, produces a posterior mean close to zero while the sign of the point estimate is about as likely to be negative as positive ($Pr(\gamma < 0)$=0.55). The results suggest that there is seemingly no strong link between aid commitments and changes in conflict intensity; the estimation at district level produces a null results whereas the magnitude of the province level estimate is very small.
The data shows no evidence for a particular strong link between conflict intensity and future aid commitments. Column 2 presents the results from a model specification including the lead of changes in aid commitments, between t and $t + 1$, rather than the lag; at both levels of aggregation the posterior mean is about zero while the posterior distribution does not exhibit a strong skew toward either negative or positive values.[10]

Aid commitments not allocated to a particular location but going straight to the central government could help increase state capacity, by overcoming budget constraints, which might influence conflict dynamics de Ree and Nillesen (2009). Therefore a variable is included in the model to account for this type of aid (col.3) and the results show that there is little correlation between changes in this type of aid and conflict intensity.[11]

The estimation so far has relied on a variable measuring changes in aid commitments that is agnostic about the fungibility of aid, or the ease with which it can be diverted from its intended purposes. Using this type of pooled aid variable rests on the assumption that all types of aid are equally likely to become fungible, as the donor is not able to monitor what actual will happen to the money (Devajaran and Swaroop, 1998), and rather than increasing net-expenditures it could be the case that aid money is used to substitute local government spending. Concerning aid fungiblity, Feyzioglu

[10] $Pr(\gamma < 0)$=0.46 at province level and 0.59 at district level.

[11] Government aid in this case refers to all the aid that goes directly to the government and is not allocated to others location in the country. It includes foreign aid for different sectors including general budget support.

et al. (1998) show that this depends on the particular sector to which the aid is committed; development loans or grants for agriculture, education, and energy lead to a reduction in government spending in these sectors whereas money earmarked for the transport and communication sector are fully spend on the intended purposes.[12] This means that at the local level aid commitments for instance in the agricultural sectors are easier targets for appropriation (Findley et al., 2011). To account for the fungibility of aid the model is therefore re-estimated including a variable for fungible and non-fungible aid. Here I follow Feyzioglu et al. (1998) and Findley et al. (2011) by coding aid going to agriculture, education, energy supply and generation (as well as general budget support) as fungible whereas aid going to transport and communication is coded as non-fungible.

Similar to the main model (col.1) the estimated magnitude of the coefficients for both variables is relatively small and with the 95% uncertainty interval centered around zero there is little indication for a strong correlation between change in either fungible or non-fungible aid and conflict intensity. This holds at both levels of aggregation. The posterior distributions do show that change in fungible aid commitments are more likely to be followed by increases in conflict intensity ($Pr(\gamma < 0) = 0.27$) while for non-fungible aid there is a stronger link with reductions in intensity ($Pr(\gamma < 0) = 0.92$). Since non-fungible aid tends to cover larger structural projects the negative effect on conflict could be explained by a mechanisms where the aid improves local welfare, and therefore increases the insurgents' opportunity costs. In this case we don't see an increase in violence as a result of insurgents trying to sabotage the project as was suggested in the Crost et al. (2014) study.

Although these results hold when including province-specific time trends, they do not hold when changing the level of aggregation to districts where $Pr(\gamma < 0)$ equals 0.58 and 0.65 for fungible and non-fungible aid respectively.

Note that with regard to the inclusion of the province or district specific year-trends (col.5) the fit of the model worsens given the increase in for instance the DIC. The results highlight a slight discrepancy in the estimate effect at the province level vis-a-vis the district level. There could be two possible explanation for this. One is that there is a displacement effect similar to the one discussed in Maystadt et al. (2014) where rebels can quite access the aid source in the district itself and fighting therefore takes place in the surrounding area. This would be consistent with the results reported by Strandow et al. (2014). However, estimating the model including a spatial

[12] For a synopsis on the debate on whether aid is fungible see Feridun (2014).

lag of aid the estimated effect is negative. In contrast, using the original model but allowing the coefficient to vary per country I find that changes in aid commitment are positively correlated with increases in conflict in Sudan with a probability of 0.26 at the province level but 0.65 at the district level, this would indicate that the aid-conflict nexus is highly localised in this particular country. Similarly for Ethiopia these probabilities are 0.16 and 0.41 for the province and district respectively. For the DRC the differences are somewhat smaller where a change in aid commitments is correlated with an increase in conflict with a probability of 0.41 at the province level and 0.33 at the district level.
One other explanation could be attenuation bias as a result of measurement error as moving to a more disaggregated level entails a loss of some aid and conflict observations due to the precision of the geocoding. For the conflict data the loss of information is not extremely substantial with a reduction of 18.5% in the number of observations, but it is considerably larger for the included number of aid projects which is reduced by 53.6%.
Across the board the model estimations provide little empirical support for a strong link between aid and conflict; in most cases the uncertainty interval is centered around zero and the magnitude of the effect is small. The results are similar using a number of other model specifications such as including the change in population or economic activity - using night light luminosity as a proxy and including a country-specific year trend to account for country-specific shocks. In addition the model is re-estimated changing the outcome variable using the changes in the number of events, to account for conflict quantity rather than intensity; this does not alter the general conclusion and these results are similar using levels rather than changes. Using the same dataset Strandow et al. (2014) and Wood and Sullivan (2015) did find a positive correlation between aid and conflict, but it seems that these results do not hold when using a more conservative sample of the data to deal with possible selection bias.

Despite concerns about the aid data quality the conclusions of the main model hold when changing the specification, however all estimations discussed so far rely on the assumption that the estimated effect is homogeneous across regions. As the results showed allowing for a different coefficient per country, there is a real possibility that the impact of aid allocations on conflict dynamics is instead heterogeneous across regions; something that will not be picked up by the pooled estimate as region-specific effects are averaged out. Therefore, the model is re-estimated allowing separate coefficients for each region, the results for which are shown in figure 5 which depicts the posterior mean along with the 50% interval. Unsurprisingly in most cases the estimated effect is similar to that of the pooled model, meaning close to zero, but there are some exceptions to this. There are a number of provinces in Sudan that

have a slightly larger estimated negative coefficients such as Western Darfur, Kasai Occidental and Northern Bahr El Ghazal. In contrast, a number of other provinces in Sudan, and some in what is now South Sudan, report a positive coefficient such as Blue Nile State, Jonglei, Northern Kordofan, Western Bahr El Ghazal, and Gadaraf. At the district level the distribution of the estimates is much the same only there is some more variation in the countries the districts are from at the upper and lower and of the scale. Here the largest positive coefficient is estimated for Kisangani (1.8, s.e.=0.5), the capital of the Orientale Province in the DRC, while the smallest coefficient is for Zalingei (-1.7, s.e.=0.4) located in Darfur, Sudan. There is no real pattern in terms whether districts in particular countries are more disposed towards a negative or positive effect. As an example, of the 16 districts for which the 50% interval of the estimated effect is below 0, five are in the DRC and Sudan, and six in Ethiopia. Similarly for the 23 districts where the 50% interval is positive, seven are in Ethiopia, and eight in the DRC and Sudan.

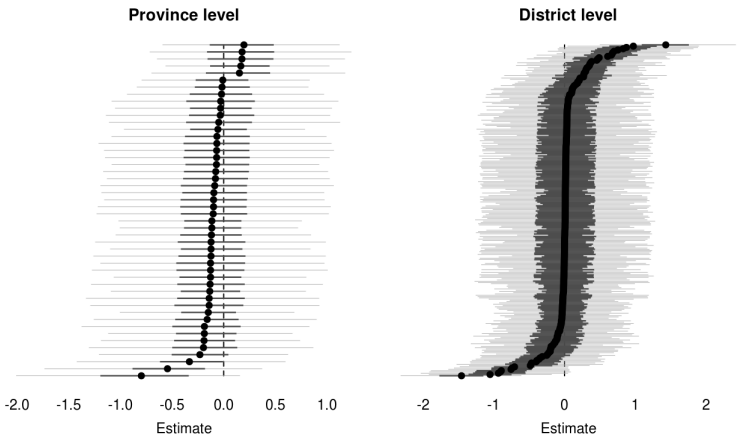

Figure 5: Estimated effect along with 50% interval for each individual province (*left*) or district (*right*).

So far the analysis has discussed how changes in conflict intensity are linked to lagged changes in aid commitments focusing on the possible nexus at the intensive margin, assuming that when aid commitments increase violence will increase as well. However, it could be the case that the aid-conflict nexus is not particularly strong at the intensive margin, as the results indeed seem to indicate, therefore it is worth examining the dynamics at the extensive margin. To test how the results hold up a model is specified using a binary outcome variable indicating the incidence of conflict at the local level; this outcome variable is linked to lagged changes in aid commitments as well as conflict dynamics (spatial and temporal lag), and other explanatory variables such as distance to the capital and ethnic polarisation.[13] Similar to the main

[13]Ethnic polarisation is here defined following Garcia-Montalvo and Reynal-Querol

regression analysis, two different model specifications are used to estimate the effect of changes in aid commitments, accounting for the fungibility of aid; all models are estimate using logit and figure 6 shows the posterior distribution of the aid variables.
At the province level, using the aggregate measure for aid the main results still hold, providing no strong support for the aid-conflict nexus, but changing the unit-of-analysis to districts the posterior distribution shows a positive link ($Pr(\gamma > 0)$=0.87) although the average estimated effect is small: a two standard deviation increase in aid commitments is correlated with a a 3% increase in conflict risk. For aggregate aid commitments this seems to suggest that the potential effect is very localised, i.e. the rapacity effects at the district level are not picked up at province level, entailing that the spillover effect is limited. Interestingly, splitting the aid variable to account for fungibility produces similar estimates for fungible aid but diverging for non-fungible aid. In the case of fungible aid, at both the province and district level this is linked to higher conflict probabilities, but again the estimated magnitude is relatively small: at the province level a two standard deviation increase in aid commitments is linked to a 10% increase in conflict risk, accounting for conflict dynamics. There is a large discrepancy for the non-fungible aid estimates; whereas the estimated effect is negative with a probability of 0.89 at the province level it is positive with a probability of 0.85 at the district level. Given that non-fungible aid consists of projects related to road infrastructure and communication, there is the possibility that the gains from these types of projects in terms of local development don't materialise at the district level but do at the province level, although this is speculative. In any case, this result does show that the potential negative effect of aid, in the sense that it correlates with increased conflict risk, is again very localised. The fact that fungible aid is more strongly correlated with conflict risk than non-fungible aid might suggest that rent-seeking through violent appropriation prevails over sabotage.

Conclusions

This study has provided an analysis on the link between aid and conflict at the sub-national level for a small sample of countries. Using subnational data helps overcome the potential information loss that has plagued other studies using the country-year as unit of analysis: microlevel data provides the advantage that it retains the full information on local development projects and within-country variation in violent armed conflict events. Important to note here is that this innovation is made possible by the release of a number of geocoded datasets which weren't available about half a decade ago. In contrast with much of the existing literature, the analysis provides

(2005). The variable is constructed using data from Weidmann et al. (2010).

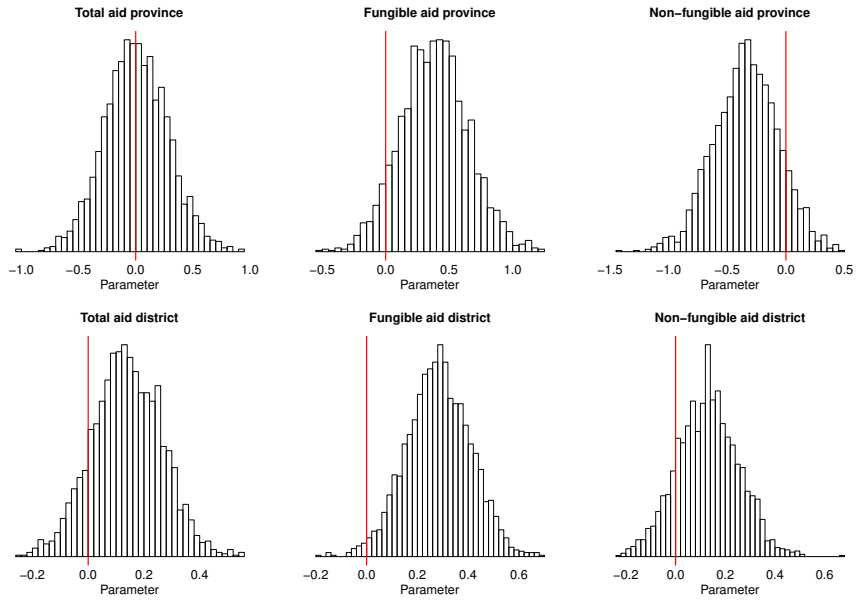

Figure 6: Posterior distribution logit estimates

no strong empirical evidence for a link between foreign aid and conflict in either direction, whereas most work on this topic tends to find an effect one way or another. The data does show that both aid commitments and conflict events tend to cluster, but there is no strong interdependence. Similarly, fitting a Bayesian linear regression model does not provide strong evidence for a correlation between lagged changes in aid commitments and changes in conflict intensity, or events. These results hold using a number of different model specifications and also across different levels of spatial aggregation; the latter robustness check is something that is often overlooked or ignored in the current literature. Moving from the intensive to the extensive margin does produce some stronger evidence for a correlation between aid and conflict, but even in this case the estimated magnitude of the effect is relatively small; a two standard deviation increase correlates with just a 10% increase in conflict risk. The results of this study therefore suggest that, at least for this sample, there does not seem to be a strong correlation between aid and conflict.

There are some caveats concerning the conclusions that can be drawn from the analysis, which are mainly the result of foreign aid data availability. Although the dataset used provides the most comprehensive geo-referenced information on aid allocations for a cross-section of countries, a main shortcoming is that there is no information available on disbursements; this means that the

estimation relies on the assumption that, in this case, there is a one-year lag between commitment and disbursement. This is an issue that faced other studies (Strandow et al., 2014; Wood and Sullivan, 2015) as well and has been dealt with in similar terms as the analysis provided here. In reality it could of course take longer for foreign aid money to reach the intended destination and the disbursement rate could be lower than what is committed, but in any case the reported amount committed is the best approximation we have at the moment. During the time it took to conduct this study a number of geocoded datasets have become available for a number of countries providing information on both commitments and disbursements which could be used for future research. The only comparable cross-country datasets is one containing information on World Bank projects, but this type of aid might not be entirely comparable to the foreign aid considered in this work. In analysing the aid-conflict nexus, testing whether increases in foreign aid correlates with increases in violence, this study, like many others, has relied on the assumption that foreign aid is appropriated by violent means, or that access to the aid source, e.g. local government, can only be achieved using violence. This assumption neglects that in certain cases insurgents prefer non-violent over violent means in order to either gain access or appropriate foreign aid, such as taxation for instance. This is a dynamic that is currently hard to test with the existing data and might be a subject worth closer scrutiny for future research. The result from the logit estimation where fungible aid is more strongly correlated with violence than non-fungible aid does seem to suggest that appropriation of aid could be an important motive in contrast with sabotage. Finally, this study has considered the nexus between development aid and conflict which is a choice driven largely by data availability as no comparable dataset exists on the provision of humanitarian aid at the local level, to the best of my knowledge. This is another area that deserves more attention in future research, certainly in the light of the increasing trend of violence against aid workers.

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Appendix

Table A2: Descriptive statistics

	Province level (*N=432*)	District level (*N=2340*)
Conflict intensity	113 (413)	18 (166)
Conflict intensity t-1	128 (437)	26 (132)
Conflict intensity W	607 (1023)	99 (369)
Foreign aid	$12{\cdot}10^6$ ($26{\cdot}\ 10^6$)	$1{\cdot}10^6$ ($8{\cdot}\ 10^6$)
Foreign aid fungible	$2{\cdot}10^6$ ($7{\cdot}\ 10^6$)	$0.2{\cdot}10^6$ ($3{\cdot}\ 10^6$)
Foreign aid non fungible	$8{\cdot}10^6$ ($20{\cdot}\ 10^6$)	$0.9{\cdot}10^6$ ($7{\cdot}\ 10^6$)
Foreign aid government	$161{\cdot}10^6$ ($520{\cdot}\ 10^6$)	$152{\cdot}10^6$ ($472{\cdot}\ 10^6$)
Distance to the capital	656 (408)	614 (410)
Ethnic polarisation	0.55 (0.27)	0.41 (0.36)

Notes. Standard deviation between parentheses.

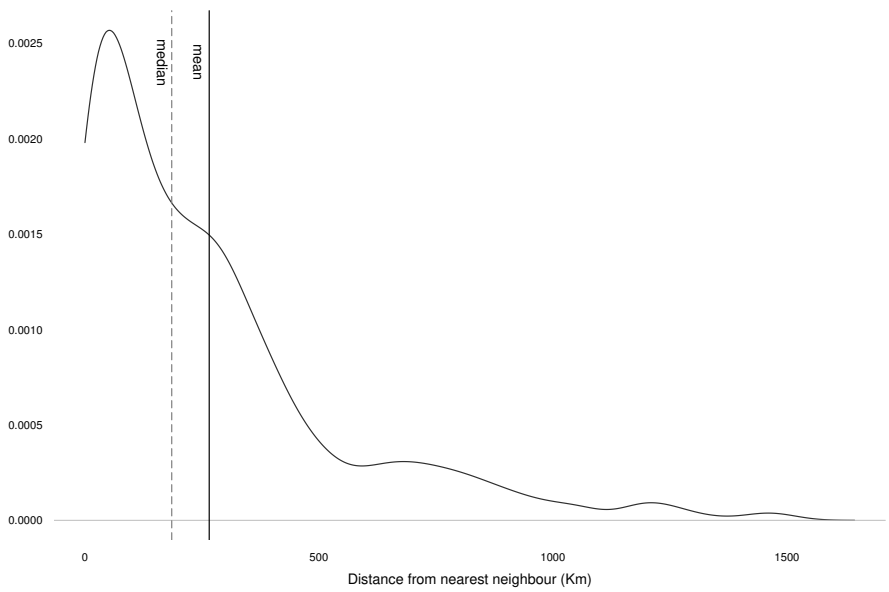

Figure A1: Density of nearest neighbour distance between conflict and aid. Black vertical line indicates the mean value, the red vertical dotted line indicates median value.

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