Foreign Aid and the Intensity of Violent Armed Conflict

Daniel Strandow, Michael G. Findley, Joseph K. Young

Abstract:

Does foreign aid increase or decrease violence during ongoing wars? Although answers to this question are almost surely found at local levels, most research on this topic is performed at much higher levels of analysis, most notably the country level. We investigate the impact of foreign aid on the intensity of violence during ongoing armed conflict at a microlevel. We examine the influence that concentrated aid funding has on political violence within war zones that are contested among combatants. Using new geographically coded data within a matching design, we find that multiple measures of funding concentration are associated with increased military fatalities, but not with civilian fatalities.

Keywords: Foreign Aid; Conflict; Africa

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AidData

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AidData is a research and innovation lab located at the College of William & Mary that seeks to make development finance more transparent, accountable, and effective. Users can track over $40 trillion in funding for development including remittances, foreign direct investment, aid, and most recently US private foundation flows all on a publicly accessible data portal on AidData.org. AidData's work is made possible through funding from and partnerships with USAID, the World Bank, the Asian Development Bank, the African Development Bank, the Islamic Development Bank, the Open Aid Partnership, DFATD, the Hewlett Foundation, the Gates Foundation, Humanity United, and 20+ finance and planning ministries in Asia, Africa, and Latin America.

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1. Introduction

Does foreign aid reduce violence? Many aid workers, policy makers, and scholars believe so. What if this intended aid actually makes violence worse? Based on numerous prominent examples of the destabilizing effects of foreign aid in countries such as Somalia, Rwanda, and the Democratic Republic of Congo, some have argued that a primary consideration in granting foreign aid is to *do no harm* (Andersen, 1999, 2000; Maren, 2009; Polman, 2010; Uvin, 1998). The academic and policy communities have spent much effort identifying how levels and changes in aid funding, as well as intervening political contexts, can increase the risk that aid sparks or fuels violent conflicts (Addison & Murshed, 2001; Arcand & Chauvet, 2001; Blattman & Miguel, 2010; Collier & Hoeffler, 2007; Collier, 2009; Grossman, 1992; Nielsen, et al, 2011; Sollenberg, 2012a). An expansive literature examines aid and conflict onset at a cross-national level, and yet most case studies and reports propose *sub-national processes* through which aid positively or negatively affects local *violence intensity*. The purpose of this paper is to investigate how foreign aid committed to violently contested areas[1] affects the subsequent intensity of violence in those areas.

There is a key area of tension in the previous literature between the size of aid as increasing the benefit of holding government power; versus aid as increasing the government's ability to deter rebellion; versus aid as rents driving conflict at the local level. We argue that, in already contested areas, concentrated aid funding is more likely to motivate conventional contests over territorial control, whereas diffused aid funding should promote low-intensity irregular operations. We expect that the first situation, where the warring parties fight more decisive battles, should result in more short-term military fatalities then the latter. In the second situation we do not expect to observe short-term spikes in military fatalities.[2]

In this paper, we introduce a new dataset that combines geocoded aid commitments (Strandow 2011) with data on territorial control (Strandow 2012) and military fatalities (Sundberg & Melander, 2013). Using propensity-score matching to better isolate the causal effect of our key variable, our results show that if funding is concentrated, instead of diffused, military fatalities increase substantially. In addition to a theoretical contribution, we offer new data and tools to examine subnational aid and conflict. This paper begins with an examination of the literature, after which we discuss our theoretical claims and specify a hypothesis. Following this discussion, we outline the research design, display the results, and consider some limitations and conclusions.

[1] By the term *contested areas* we refer to areas within countries that suffer conflict where there is ongoing violence between warring parties. This is a crucial distinction as it determines what population of cases that our results can be generalized to. The cases that we cover are warring parties in Africa South of the Sahara, 1989–2008.

[2] If anything we would expect that if diffused aid makes low-intensity violence more viable it would promote longer rebellions and lead to greater violence duration or long-term aggregates of military fatalities. This study does however focus only on short term impacts of aid.

2. Aid and Violence Intensity

We investigate the impact of foreign aid on the intensity of violence during *ongoing armed conflict*. To be clear, we are not concerned with the influence of aid on conflict onsets or recurrence. Neither do we devote effort to understanding how foreign assistance to peaceful areas impact warring parties' behaviors. Previous research covers broad theoretical ground with different independent and dependent variables and causal mechanisms. We arrange our review by first going through indirect relations between aid and conflict and then turning to more direct impacts, including the key concept of interest in this paper: *funding concentration*.[3]

2.1 Indirect Impact of Aid on Conflict

Policymakers and academics alike recognize that sending funds and resources to conflict areas can increase conflict risks (Addison & Murshed, 2001; Anderson, 1999; Collier & Hoeffler, 2007; Collier, 2009; Maren, 2009; OECD, 2001; Sida, 2013). The overall debate concerning the influence of foreign aid on conflict begins with the question of whether aid actually improves development (Collier, 2007; W. R. Easterly, 2006; Sachs, 2006). On a superficial level it seems obvious that more resources should improve the economy and a country's development trajectory (Sachs, 2006). This healthier economic path should then counteract some of the most important drivers of conflict, such as low growth, poverty, and associated unemployment (Collier & Dollar, 2002; Collier & Hoeffler, 2002b, p. 10).

One of the proposed causal paths linking a poor economy to increased conflict is that a decreased unemployment rate increases the opportunity cost of recruitment into military organizations. Increased opportunity cost in this context means that income is lost by engaging in a military organization compared to doing civilian work (Grossman, 1991; Collier & Hoeffler, 2004).[4] Not only could aid potentially increase the opportunity cost of recruitment but it may also improve security by influencing the population's interest in sharing information about insurgents (Berman, Shapiro, & Felter, 2011).

These causal paths between foreign aid and conflict are largely contingent on the first crucial step, that aid actually improves a country's economy. The contrast between Africa and Southeast Asia is the typical illustration that the relation between aid and development is quite complicated. Southeast Asia has developed rapidly with comparatively little foreign aid whereas most parts of Africa still

[3] By indirect impacts we refer to those that affect groups' behaviors via the country's economy. By direct impacts we mean the influence that aid can have on violence unmediated by other factors. For example, the competition for aid by warring parties can lead to increased violence between them.
[4] Note that there is also research suggesting that there are alternative causal paths between unemployment and violence, implying that it is difficult to find a causal effect of unemployment (Berman, Callen, Felter, & Shapiro, 2011).

struggle despite vast amounts of aid sent from the West, and other donors, over the years (W. Easterly & Levine, 1997; W. R. Easterly, 2006).

If it is used by a recipient government to pay off a narrow constituency instead of being leveraged to support growth promoting policies, aid may even inhibit long-term development (Wright, 2010). Foreign assistance may also impede development due to the so-called *Dutch Disease*, which implies that the development of some sectors is stunted when aid is a large part of a country's economy (Rajan & Subramanian, 2011; Younger, 1992).

2.2 Direct Impact of Aid on Conflict

We now turn to the more immediate determinants of how aid can affect violence intensity. Academics have begun specifying how the size of aid and changes in funding levels can increase the risk that aid sparks or fuels violent conflicts (Addison & Murshed, 2001; Arcand & Chauvet, 2001; Collier & Hoeffler, 2007; Collier, 2009; Grossman, 1992; Nielsen et al., 2011; Sollenberg, 2012b). Besides problems with high levels of funding there are also problems associated with shortfalls in aid. Aid is often a big part of recipient governments' economies, and the provision of aid tends to be volatile (Nielsen et al., 2011, p. 220). If governments use foreign assistance to pay off narrow constituencies, or elites from opposition parties, or potential rebel groups, then sharply decreased aid could destabilize such arrangements and increase the risk of conflict (Nielsen et al., 2011, p. 222; Sollenberg, 2012b, pp. 112–113).[5]

Increased aid can influence conflict propensity in at least three ways. Firstly, aid that is disbursed via the government (Addison & Murshed, 2001) and that could be diverted into private hands would increase individuals' value of holding government power. Rebels could therefore expect to gain access to such aid rents by capturing the center of state power (Azam, 1995; Grossman, 1992). The attraction to accessing rents by holding government power might depend on whether prospective coup or rebel leaders stand to gain greater rents relative to their pre-war access to rents. If the expected payoff of gaining access to state benefits outweighs the costs, then potential rebels may choose to engage in violent rebellion (Grossman, 1992).

Secondly, although aid that can be exploited by a government may increase the size of the prize – Addison and Murshed (2001) have found that it increases the size of military expenditures. A more recent study by Collier (2009) found that as much as 40% of African military expenditures were

[5] The proposed mechanism linking aid shocks and conflict focuses on personal networks and is more difficult to discuss in relation to geographically distinct contested areas, which means that the growing literature on aid shocks is engaging but outside the scope of this paper.

financed by aid. The improved military capacity should increase governments' success in deterring rebellion (Arcand & Chauvet, 2001, p. 30), potentially by reducing rebels' prospective gains from conquering the state.[6] Others argue that funding is non-appropriable by rebels as they would mainly be concerned with more easily available rents, such as diamonds or other lootable resources. And even if rebels would succeed in capturing government power, the probability of doing that is generally low (around 20%) and implies a lengthy struggle (on average seven years), suggesting that immediate resource rents would be preferred over heavily discounted aid rents (Collier & Hoeffler, 2002a, p. 437; Collier, 2009). Whatever the particular mechanism, the overall expectation of this second perspective is that *aid would decrease conflict risks*.

Thirdly, aid disbursements may bypass the government and the capital completely (Addison, Billon, & Murshed, 2002, pp. 382–383; Findley et al., 2011). Aid supplies could provide warring parties with greater incentives to engage in looting rather than attempting to govern the capital, analogous to lootable natural resources (Collier & Hoeffler, 2002b; Collier, 2000). Rebels would still be motivated by rents, but the main effect would be greater rent-seeking behavior in the areas of the country near where they tend to live and operate (Anderson, 1999, pp. 38–39; Blattman & Miguel, 2010, p. 11; Findley et al., 2011; Maren, 2009). Warring parties can exploit aid through theft and looting, and local elites with interests in maintaining violence, as in Somalia, can benefit from corruption or unfair business opportunities (Anderson, 1999, p. 39; Maren, 2009, pp. 94, 169; Webersik, 2006, p. 1467). Looted or embezzled aid can then be used to pay soldiers and buy arms, thus feeding on-going disputes (Anderson, 1999, p. 38; Blouin & Pallage, 2008; Maren, 2009, pp. 103–104, 260). Applying a rebellion as local rent-seeking logic recognizes that it is possible for warring parties to opportunistically exploit aid rents after the onset of conflict rather than initiating conflict solely with the goal of conquering the state.

One potential bridge between the perspectives was proposed by Findley et al. (2011) who suggested that if aid creates incentives for rebels to use violence, but government militaries become much stronger by diverting funding, then we would expect an increased risk of violence onset in the periphery, far from the reach of the central government. Rebels would then fight farther away from the capital and exploit local aid opportunistically until they gain sufficient strength to bring the violence closer to the institutions of the state. Regardless of the potential of bridging these perspectives a disaggregated approach that goes beyond country level aid flows and violence outcomes will help distinguish mechanisms at one or both stages.

[6] As an aside, there is recent work proposing how increased military spending may ignite regional arms races and thereby increase the probability of some conflicts (Collier & Hoeffler, 2007; Collier, 2009)

We propose that a difference between these three approaches is whether funding is assumed to be disbursed in a geographically *concentrated* or *diffused* manner. Aid is considered concentrated if that particular location attracts relatively higher levels of assistance compared to other sites.[7] As an example, aid that flows to a government's capital, which is often a comparably small area, could then be seen as a case of highly concentrated aid funding and rural disbursements represent diffused funding. We do not suggest that all aid projects committed to capitals imply highly concentrated values, and that all aid to local recipients represents diffused values. What we recognize is that, on average, international assistance going to capitals tends to be more valuable and concentrated to a smaller area compared to locally disbursed aid. And more crucially, funding could be concentrated beyond the capital and the government's control. This makes aid function as a local prize that attracts decisive attempts at conquest, without simultaneously increasing government deterrence. In what follows we introduce how funding concentration and diffusion may impact violence intensity.

Le Billon (2004) has already established that the concentration of a resource influences conflicts. Here, the notion of resource concentration is adapted to the special case of foreign aid funding. Whether aid funding is concentrated or diffused should influence warring parties' military decisions in already contested areas. It is more worthwhile to attempt to control points rather than large areas since the former are easier to defend and require less troops to dominate. When resources are valuable and spatially concentrated they should tip the scale in favor of attempting territorial control rather than casual raiding. Competing for territorial control (for instance control over the capital or another high value target) should hence be more likely with high concentration of aid values. A range of low-intensity irregular operations should be more likely if aid is diffused. We expect that the first situation, where the warring parties fight more decisive battles, should result in more short-term military fatalities then the latter. Previous research shows that conventional warfare tend to generate more fatalities compared to low-intensity operations, such as guerrilla and irregular warfare (Lacina, Gleditsch, & Russett, 2006, p. 678; Valentino, Huth, & Balch-Lindsay, 2004, p. 377). We consequently argue that in already contested areas, funding that is concentrated should motivate more committed battles, resulting in more military fatalities.

Some recent, disaggregated studies on Iraq support the counterfactual: small aid projects decrease military deaths (Berman, Felter, Shapiro, & Troland, 2013a, 2013b; Berman, Shapiro, et al., 2011, p. 804). Violence against coalition troops and Iraqi government forces is decreased when so-called CERP[8] projects are small (<$50,000). According to the authors' theory, one reason for this is that small programs are easier to revoke if they do not lead the local population to share more information.

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[7] As discussed in the reseach design section, we measure aid concentration as present if the value of aid per location in a given area is greater than the average funding to all areas and years in the dataset. This measure matches our theoretical construct as we are interested in locations that attract above average aid. We change some of the ways we measure this above average aid provision in the robustness section below.

[8] CERP is the US Army Corps of Engineers' *Commanders Emergency Response Program*.

If populations share more information, it is easier for government troops to increase security (Berman et al., 2013a, p. 515). Interestingly, another study on Iraq finds that a greater level of funding decreases civilian fatalities while increasing military fatalities. The purpose of that research was to investigate whether development projects aimed at increasing employment would decrease violence. The theory is that labor-intensive development programs should decrease labor-intensive insurgent violence. Rebel groups may then, if possible, substitute towards capital-intensive attacks. Capital-intensive attacks are likely to favor attacks against hard, military, targets over soft, civilian, targets (Iyengar, Monten, & Hanson, 2011, pp. 4–5).

An unrelated study of development aid in the Philippines found that whether villages received funding from a big project or not influenced fatalities. Generally, a location that received more aid saw increased military fatalities but the effect on civilian deaths was not as noticeable (Crost & Johnston, 2010, p. 37).

We thus hypothesize: If aid funding is concentrated rather than diffused, short-term *military* fatalities increase.

We assume that concentrated funding leads to more decisive battles as warring parties aim to exploit foreign assistance. Even if relaxing the assumption that warring parties always aim to exploit foreign aid our overall argument still applies. Warring parties, particularly governments, may be more inclined to violently defend locations with high funding concentration in order to fulfill their duties and improve the area's development. Warring parties may also be more interested in conquering areas with highly concentrated funding in order to take credit for improved development and to receive greater popular support. Whether warring parties engage in more decisive battles in order to directly exploit aid, or do so in order to reap more indirect benefits, may depend on the type of aid committed. Aid that cannot be physically exploited should increase competition for population's hearts and minds, and aid that can be appropriated should increase the likelihood that warring parties compete for more straightforward exploitation. In either case more concentrated funding would make asserting territorial control more worthwhile for at least one side of a conflict.

Our focus is on variations in short-term military fatalities. Given the recent research on civilian targeting in civil war (Weinstein and Humphreys 2006, Weinstein 2006), we investigate civilian fatalities as well to ensure there are different processes to explain each. This work suggests that rebels will manhandle civilians when rebels do not depend on the citizens for resources. It is not clear, however, that aid operates on rebel behavior the same way as other resources, such as oil or diamonds. As such, we investigate the extent to which aid concentration and civilian fatalities relate to each other.

Previous research investigating conflict duration has found that humanitarian assistance and food aid may prolong conflicts (Narang, 2015; Nunn & Qian, 2014). This is significant because if aid can make conflicts endure longer, it will have an impact on long-term aggregates of fatalities, even if there are no short-term peaks in military fatalities. Introducing the type of aid as a dimension alongside its concentration may provide insights concerning foreign assistance's impact on a broader spectrum of violence. We do not further investigate the potential relationship between funding concentration and different types of aid, but realize that there is room to further expand the theory in the future. In the next section we discuss our data, cases and coding, and ultimately how to test our hypothesis.

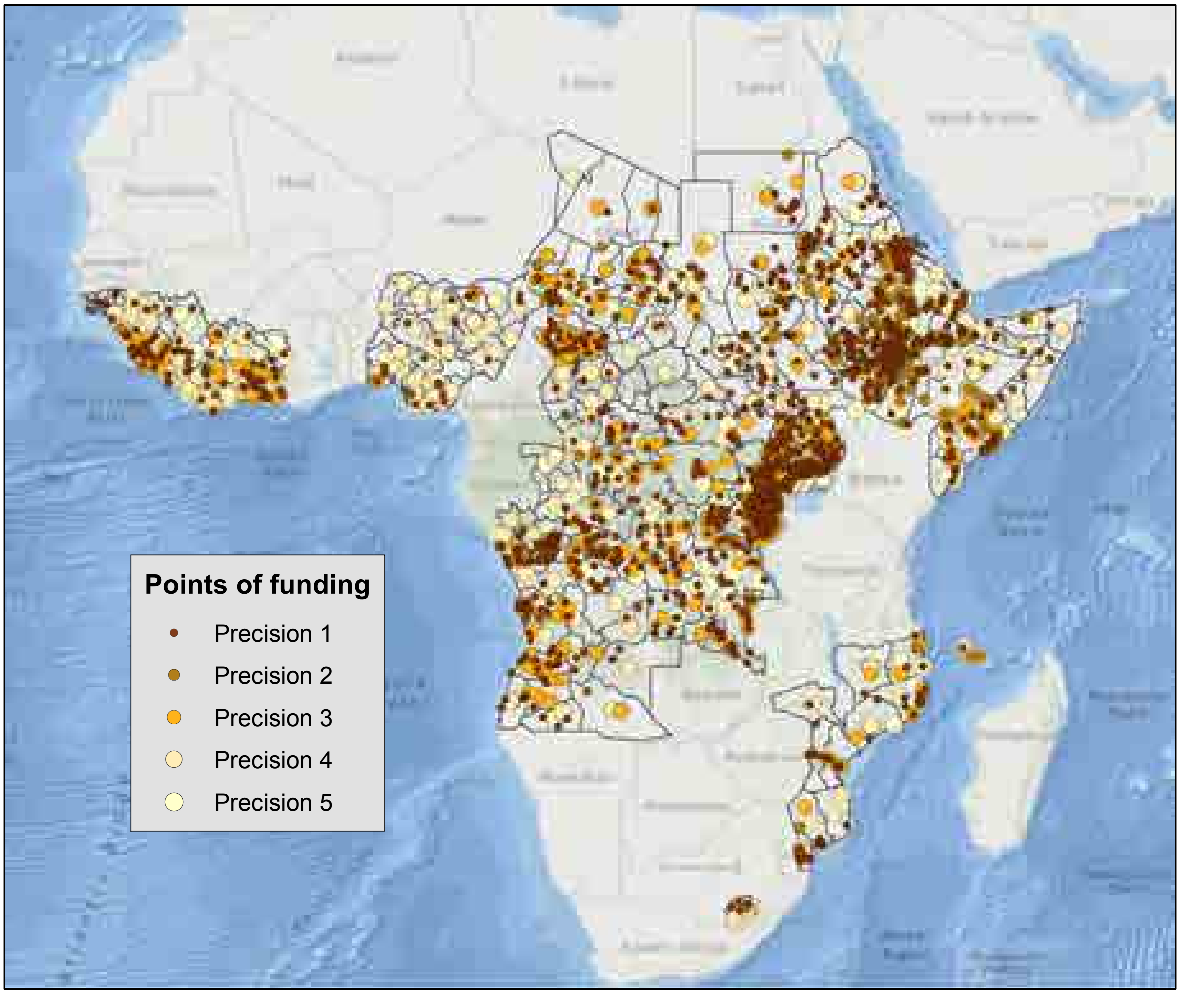
3. Research Design

Having established how aid could influence violence intensity in theory we now advance our strategy for hypothesis testing. We first introduce the structure of the dataset, the cases, and the independent and dependent variables. Following that we present the analytic challenge of dealing with foreign assistance that is not randomly assigned and how propensity score matching helps mitigate some of thoese issues. We also explain how we identify causal effects and which control variables we include.

3.1 Cases and Data Structure

In order to test the hypothesis it is crucial to discuss the measurement of foreign aid, violence, and a range of control variables. We primarily use data from two of our own original coding efforts. We first adapted and developed the UCDP geocoding methodology (Sundberg, Lindgren, & Padskocimaite, 2010) so that it can be used to code the geographic coordinates of foreign aid projects (Strandow, 2011). This methodology was applied to the most comprehensive collection of official development aid – AidData core (Tierney et al., 2011) – in order to code aid flows to conflict years in Africa South of the Sahara (Findley, 2011). Figure 1 shows the foreign aid locations coded in our data set.

Figure 1.



This map contains all aid projects that we had geo-referenced (assigned geographic coordinates) based on project descriptions by 2011. Each dot on the map represents a discrete aid project and is scaled by the amount of aid it represents as depicted in the legend.

Our second coding effort produced an events dataset, which contains information on which warring party initiated a particular clash, and which actor controlled a battle location after combat (Strandow, 2012). This events dataset is coded from, and is compatible with, the Uppsala Conflict Data Program's GED sub-Saharan Africa dataset (Melander and Sundberg, 2011). By aggregating these events in a yearly format, it is possible to use control variables that are crucial for specifying the impact of aid on violence intensity. These two independent coding efforts are then combined with the original UCDP-GED dataset in order to measure the dependent variables.

The resulting data structure has rows of warring party *A*'s actions versus the *B*-side in each *first order administrative division* (e.g., a province) each year. An administrative region is included if at least one person was killed in the area in the current year. Exactly how these datasets were collected and what they contain is further developed in the online appendix. Empirically the dataset covers warring parties in Sub-Saharan African states that have one year or more of state-based intra-state armed conflict since 1989. By state-based intra-state conflict we mean that there have been at least 25 annual deaths resulting from fighting between an organized warring party and a government (Harbom, Wallensteen, & Kreutz, 2007).

We include years of non-state violence between organized groups, as long as the country has already entered the dataset based on the state-based violence criteria. Warring parties become inactive and exit the dataset if the number of deaths falls below 25. Inactive parties can enter the dataset again after spells of inactivity. Warring parties associated with a conflict that started after 2007 are not included and for all warring parties 2008 is the last year that is coded.

3.2 Observing Foreign Aid

3.2.1 The Independent Variables

To test the hypothesis in a manner that makes it straightforward to interpret causal effects, we formulate dichotomous variables that are coded 1 if an observation receives *treatment*, and 0 if it does not, as shown in Table 1.

Table 1. Independent (treatment) variables

Purpose	Name	Description
Hypothesis testing	Funding per Location	Coded 1 if the value (constant USD) per aid location is over the mean, 676,274
Robustness	Funding per Location, varying the threshold	Six thresholds to either side of the mean. Deviating from the mean with -0.15 to +0.15 standard deviations, in 0.05 increments
Robustness	Funding per Area	Based on constant USD per square km. Coded 1 if the value per square km, is over the average, 8303
Robustness	Total Funding	Coded 1 if the total value is over the average, 9,817,845

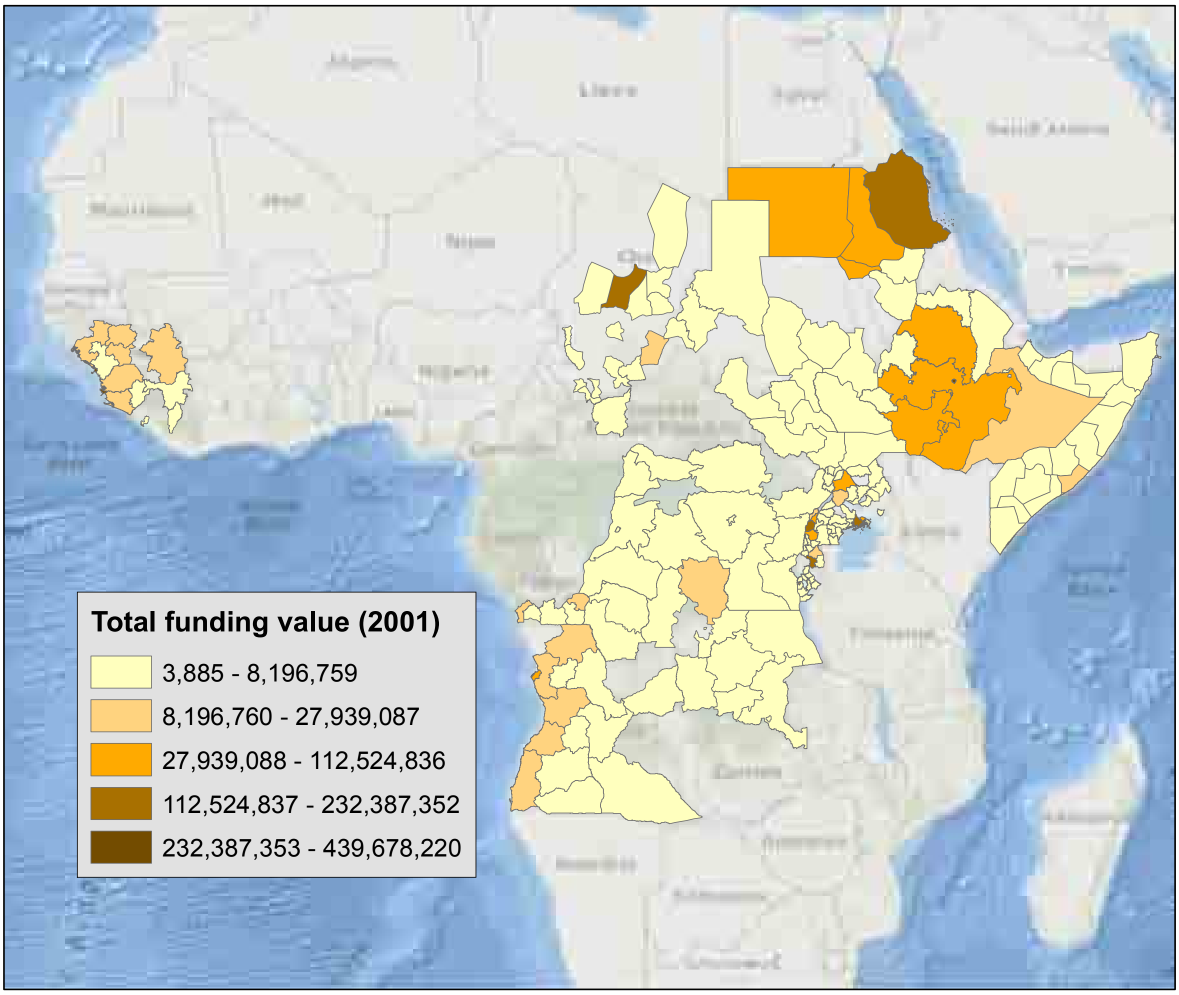
To test the hypothesis we specify a variable that captures whether warring parties would expect aid funding to be spatially concentrated. To formulate a treatment variable we specify a cut-off point between those areas that receive highly concentrated funding commitments and those that receive more dispersed, or no, funding.[9]

[9] If a region receives no aid at all, we manually code it as 0.

An aid commitment is funding that a sender has pledged to disburse to the recipient. We make the assumption that big enough sums of aid committed to few enough locations captures the attention of warring parties to the point that their contest strategy is affected. It might be an unrealistic claim that warring parties keep track of aid commitments, though we note that other studies make this assumption (Nielsen et al, 2011) and interviews with ex-generals from the Lord's Resistance Army in Northern Uganda substantiate the assumption (Narang, 2015). It is, however, possible that the parties observe actual aid disbursements and formulate expectations about future commitments and distributions from that information. In that case aid commitments would pick up on parties' expectations by being correlated with earlier aid disbursements. Commitments are likely to reflect earlier distributions because local needs for development aid change slowly over time and since donors can become attached to specific recipients, either due to earlier colonial relations or due to current foreign policy interests (McKinlay & Little, 1977).

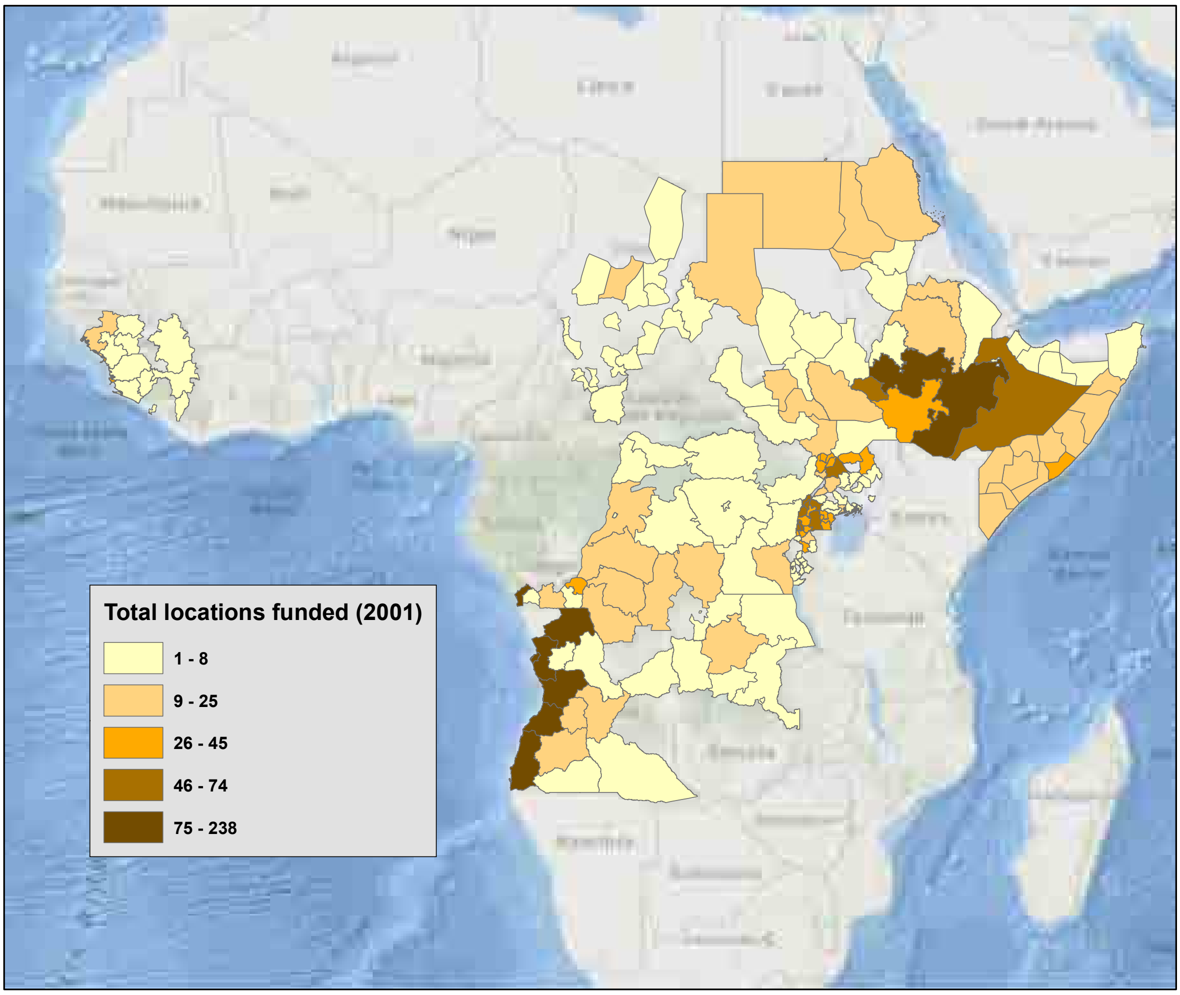
It is even possible that warring parties actively invite aid donors and gain knowledge of commitments through direct communications with implementing organizations. An example of this is when the Revolutionary United Front (RUF) in Sierra Leone reportedly invited Médecins sans Frontiéres and Action Contre la Faim to provide humanitarian aid in areas controlled by the rebel group (Polman, 2010, p. 103). Whether resulting from earlier disbursements or current pledges, we therefore find it plausible that aid commitments reflect warring parties expectations of future funding concentration. We currently do not have access to disbursement data of sufficient quality and therefore rely entirely on commitment data.

Figure 2.



This map illustrates the total funding in constant USD. Darker color represents greater funding.

Figure 3.



Displayed in this map is the total number of locations that funding was committed to in 2001. Darker color represents a greater number of locations funded.

Funding per location is coded 1 if the value of aid per location in a given area is greater than the average funding to all areas and years in the dataset. Figure 2 displays the numerator and Figure 3 the denominator of the funding per location measurement. The prevailing method for standardizing aid in national level studies is the funding's share of the Gross National Income (Sollenberg, 2012a, p. 23) or to divide aid by population. For subnational analyses the quality, and coverage, of income data is not great enough to allow this type of transformation with local income data. Moreover, this theory is not proposing macro-level financial mechanisms, for which the size of aid would be relevant to relate to the size of the national income or to the population size. At the micro-level the value of aid in itself, whatever the size of the national economy, is likely enough to capture warring parties' expectations.

For robustness check we include the measure *Funding per area*, which records whether an area's funding per square kilometre exceeds the average of all areas. We also check the impact of *Total funding* exceeding the average total funding level.

3.2.2 Dependent Variables

We consider two categories of violence intensity: short-term military and civilian fatalities.[10] There are a number of ways to operationalize these dependent variables. Table 2 illustrates how we define the two categories of intensity in relation to types of violence specified by the Uppsala Conflict Data Program (Eck, Sollenberg, & Wallensteen, 2004) and Kalyvas (2006).

Table 2. The origins of the dependent variables from the Uppsala Conflict Data Program measurements

Violence Intensity	Casualties from	Measurement
Military Fatalities	Government, Rebel, or Militia Troops	Side A and Side B Deaths; Unknown Deaths
Civilian Fatalities	Civilians caught in Crossfire, Indiscriminately or Selectively Targeted	Civilian Deaths from State-based, Non-state, or One-sided Violence

Previous micro-level research with violence intensity as a dependent variable has either used fatality counts aggregated over several years, or fatality aggregates normalized by area population (Do & Iyer, 2010; Murshed & Gates, 2005). Here analyses are done using yearly data on fatality counts. Both dependent variables are measured the year after the independent variables.

We use arguably the most reliable, systematically collected, fatality data that is currently available, the Uppsala Conflict Data Program's Geo-Referenced Events Dataset, which covers conflict years in Africa South of the Sahara since 1989 (Sundberg & Melander, 2013). The operational measures used in the study are outlined in Table 3.

Table 3. Preparing the dependent variables for analyses

Name	Description
Military Fatalities Log	T+1, sum of best estimates of all fatalities minus civilian deaths (log-10 of value+1)
Civilian Fatalities Log	T+1, sum of all civilians killed by either side (log-10 of value+1)
Total Fatalities Log	T+1, sum of best estimates of all fatalities (log-10 of best estimate+1)

[10] Here we consider *short-term* to be effects that occur up to a year after a cause. This may appear to be an arbitrary cut-off point but when investigating yearly observations this is an intuitive representation of short-term effects.

The hypothesis is specific in that we expect funding concentration to mainly have an impact on military fatalities. Along a measure of military fatalities we therefore check if we can separate out the impact of funding concentration on military fatalities from civilian and total fatalities. Military fatalities tend to be distributed over a high number of events with few fatalities and a small share of events with exceptionally high fatalities (Bohorquez, Gourley, Dixon, Spagat, & Johnson, 2009; Clauset, Young, & Gleditsch, 2007). In addition to this heavy-tail distribution within cases there could potentially be differences in how best estimates of battle related deaths are coded between countries and warring parties. If parties to one conflict often inflate their fatality numbers, coders will be much more conservative in counting deaths compared to conflicts where the warring parties' information is more reliable.

To address this within and between cases variance we recode this variable by log transformation.[11] There are a number of events with zero military fatalities.[12] The result is *Military fatalities log*. *Civilian fatalities log* measures the total of civilians killed in contested areas either in crossfire or as a result of one-sided violence and *Total fatalities log* contains both military and civilian fatalities.

3.3 Challenges of Identifying Causal Effects

There are two challenges with causal identification in this context: selection problems and unobserved covariates. We use matching to address selection issues and fixed effects models to deal with unobserved covariates. These two approaches do not solve all threats to inference, but they provide the most reasonable design for these particular data and research question.

We use propensity score matching to address nonrandom assignment to treatment. When using exact matching, a subject under treatment is paired with a control subject if they share exactly the same value on all covariates except the key independent variable. Propensity score matching instead pairs subjects based on how likely they are to receive treatment, which occurs when their propensity scores are similar (Rosenbaum & Rubin, 1983; Sekhon, 2009). There are a number of model specifications that can be used to estimate the propensity score. Our treatment variables are dichotomous, and we use a logit specification (Caliendo & Kopeinig, 2008). The procedure is less dependent on model assumptions than equivalent procedures that achieve as-if random assignment in regression models (Rosenbaum & Rubin, 1983, pp. 48–49). This benefit does, however, come at the expense of using a coarse dichotomous independent variable. For robustness, we vary the threshold used to generate this variable and show all possible threshold ranges for which the effect holds.

[11] We also rank transformed the dependent variable and the results are qualitatively similar.
[12] Since the logarithm of zero is undefined, we add one to the fatality estimate before the log transformation.

Matching cannot eliminate the influence of unobserved covariates and can therefore only achieve balance based on observed control variables. Another potential issue is that observations that cannot be matched are not used to measure the causal effect. The causal effects that are estimated from matched pairs will therefore vary depending on how the matching is specified. To account for unobserved covariates, at least in part, we estimate fixed effects models, which we report in the robustness section.

3.4 Determining Causal Effects

Comparing the effect of treatment and control observations – within matched pairs – on the outcome makes it possible to estimate the causal effect of aid concentration. Propensity score matching can be used in combination with regression for more accurate results, while introducing some dependence on regression model assumptions (Ho, Imai, King, & Stuart, 2007, pp. 200, 209–211). Rather than doing post-matching regression we trade some accuracy for fewer model assumptions, simplicity of analysis, and high transparency, by calculating the average treatment effect (ATE).[13]

The ATE gives the difference in expected values of outcomes between observations of treatment and observations of control (Morgan & Winship, 2007, pp. 36–37). Since we are interested in the average effect over both treatment and control, control observations are matched to treatment observations and vice versa (cross-matching; compared to the average treatment effect of the treated in Ho et al., 2007, p. 216).

The research design we have specified results in matched pairs of treatment and control observations that are more likely to, for instance, contain warring parties within the same area, the same year, the same type, or with the same amount of opponents. This means that on occasion an actor could be compared to itself at a later date, or possibly to its current opponent. We consider that this design is appropriate in order to correct for the time and space dependent effects that unobserved covariates might have on the ATE.

3.5 Controlling for Diffusion and Unobserved Covariate Trends

Violence can spread over both time and space (Bohara, Mitchell, & Nepal, 2006; Kalyvas, 2008). Neighbouring conflicts have been found to influence the prevalence of local conflict (Rustad, Buhaug, Falch, & Gates, 2011). Controlling for a lagged dependent variable can reflect both diffusion over time as well as space, as long as it is safe to assuming that spatial diffusion is lagged (Beck, Gleditsch, & Beardsley, 2006). We include the treatment variables at t-1 as a way to increase the probability of

[13] The decreased accuracy results from the remaining "imbalance in the matched sample [that is] is strictly unrelated to the treatment [..], or [that] has no effect on the outcome” (Ho, Imai, King, & Stuart, 2007, p. 213).

pairing observations that have similar history in receiving aid. This will help further reduce endogeneity problems. There are many forms of unobserved covariates, some of which vary due to continent- or world-wide trends. By including year dummies it is possible to take this variance into account. Table 4 contains a summary of the temporal variables.

Table 4. Time lags and trends control variables

Name	Description
1989	The first covariate year in the dataset, coded 1 if 1989
…	All years in between 1989 and 2008
2008	The last covariate year in the dataset, coded 1 if 2008
Funding Concentration, t-1	T-1 version of Funding Concentration, coded 1 if over 417,245 USD
Funding Concentration, t-1, varying the threshold	Six thresholds to either side of the mean of T-1 Funding Concentration. Deviating from the mean with -0.15 to +0.15 standard deviations, in 0.05 increments
Funding per Area, t-1	T-1 version of Funding per Area
Total Funding, t-1	T-1 version of Total Aid
Civilian Fatalities, lag1	First lag, sum of civilian deaths (log-10 of fatalities +1)
Military Fatalities, lag1	First lag, sum of best estimates of all deaths excluding civilian deaths (log-10 of fatalities+1)
Total Fatalities, lag1	First lag, sum of best estimates of all deaths, log-10 of value+1
Civilian Fatalities, lag2	Second lag, sum of civilian deaths (log-10 of fatalities +1)
Military Fatalities, lag2	Second lag, sum of best estimates of all deaths excluding civilian deaths (log-10 of fatalities+1)
Total Fatalities lag2	Second lag, sum of best estimates of all deaths, log-10 of value+1

3.6 Covariate Sets

There is no consensus in the literature concerning exactly what control variables to include when matching. While suggesting that matching performs well with many control variables, Rosenbaum and Rubin does not specify inclusion criteria (Rosenbaum & Rubin, 1983, pp. 48–49). By contrast, there are recommendations to include slimmed covariate sets (Pearl, 2009). One guideline is to not include any post-treatment covariates as controls so as to not confuse what causal effect that is measured (Gelman & Hill, Jennifer, 2006, p. 188; Ho et al., 2007, p. 202). A post-treatment variable in the model used here would for instance be the unobserved part of the causal mechanism, a warring party's decision to compete for territorial control or engage in low-intensity operations. We note here that the specific covariate sets are determined before the causal effects are measured. Tables 5, 6, and 7 display the covariates.

Table 5. Attacks, control, and spatial diffusion of attacks

Name	Description
Greater Battleground Control	*A* Preponderance in Control over Population. Coded 1 if *A* had a difference in population affected by control > 73580 (twice the average difference)
Greater Battleground Control, Alternative	For robustness. *A* more Control Counts. Coded 1 if *A* asserted control over more territory than *B* during current year and area
A is Challenger	Whether *A* is a challenger
Multiple Opponents	Coded 1 if multiple opponents in area
Attacks by A	Sum of all points attacked by *a* in administrative division
A over Peer Attacks	Dichotomous. Coded 1 if current area has as great, or greater, number of attacks by *A* than all other areas within the country that party *A* operates in
Population near Violence	Mean size of populations at battle locations

Table 6. Resource value control variables

Name	Description
Petro Locations	Number of petro locations within administrative division
Diamond Locations	Number of diamond locations within administrative division
Population Density	Population density
Rainfall	Rainfall in percentages
Agriculture	Agriculture land (land used for crops or pastures) coded in the following increments: 14%, 16%, 20%, 50%, 70%
Most Petro	Dichotomous. Coded 1 if current area has *greater* number of petro locations than all other areas within the country that actor a operates in
Most Diamonds	Dichotomous. Coded 1 if current area has *greater* number of diamond locations than all other areas within the country that actor a operates in
Most Agriculture	Dichotomous. Coded 1 if current area has as great, or greater, crops or pastures area percentage than the neighborhood max

Table 7. Geography control variables

Name	Description
Capital	Dichotomous, coded 1 if national capital in area
Mountainous	Real values of minimum elevation in meters
Forested	Percentages of forest cover
Most Mountainous	Dichotomous. Coded 1 if current area has as great, or greater, elevation than the neighborhood max
Most Forested	Dichotomous. Coded 1 if current area has as great, or greater, forest percentage than the neighborhood max
Area Size	Area in square kilometers
Greatest Area	Dichotomous. Coded 1 if current area has greater square kilometer area than all other areas within the country that actor a operates in

4. Results

We begin with some basic descriptive statistics and continue by evaluating the hypothesis. Most variables have high deviations around their means as is expected from count data. By relying on the mean of funding concentration to determine when funding is highly concentrated, as opposed to diffused, we are left with 16 % treated observations. See Tables 8 and 9.

Table 8. Descriptive statistics of the independent variable

	Treatment = 1	Mean	Standard deviation
Aid per location	370	0.16	0.36
N=2372			

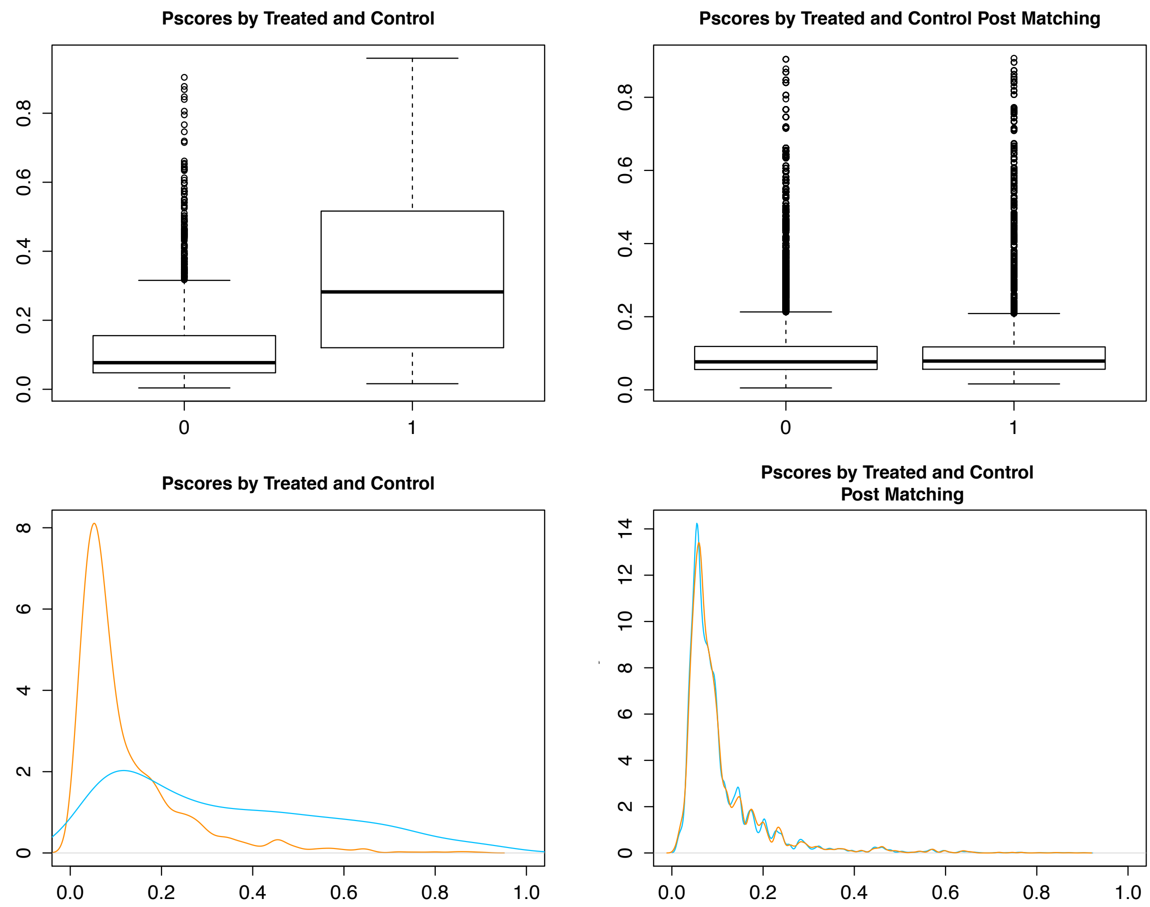
Table 9. Descriptive statistics of the dependent variables

	Mean	Standard deviation
Military fatalities, log	0.65	0.93
Civilian fatalities, log	0.34	0.66
Total fatalities, log	0.76	0.98
N=2372		

4.1 Aid Concentration and Violence Intensity

As discussed above, the treatment variable funding concentration, is operationalized as *Funding per location*. Figure 4 shows propensity scores before matching to the left and the propensity scores of observations that remain after matching (post matching) to the right.

Figure 4. Pre- and post-matching of funding per location, for military deaths



The graphs in the upper row show how the average propensity score (y-axis), and the spread in scores, varies between treatment and control observations (x-axis). The propensity to receive treatment is close to 0.3 for the treated observations. The lower row makes a similar point by showing the cumulative propensity score on the y-axis and the propensity score on the x-axis. The curve representing treated observations is colored blue and in the figure to the left it is the flatter of the two. The cumulative propensity score essentially adds together the number of observations of a certain propensity score so that it is possible to visualize which scores that are more common. The lower figure echoes the box-plot in showing that the propensity scores for the treated observations are more spread out than those of the untreated observations. After matching, the distributions of treatment and control observations are well balanced.

Table 10 displays the results of the post-matching difference tests. Control variables that are included in a model specification are indicated with check marks. Calculating treatment effects of control variables is irrelevant since they are most likely not as-if randomly assigned. We find that our hypothesis is supported. Specifically, if aid funding is expected to be concentrated rather than diffused, the short-term *military* fatalities increase. We find no effect on civilian deaths suggesting that in already violent areas more concentrated funding tends to shift the mode of warfare between armed groups, and not the intensity in one-sided violence. Aid per location is associated with an increase in total deaths (civilian plus military deaths) but we would expect this result to be driven by the impact of funding concentration on military deaths.

Table 10. Average treatment effect of aid value per location

Variable	Total Deaths	Military Deaths	Civilian Deaths
Funding per location	0.27***	0.31***	0.06
	(0.099)	(0.097)	(0.067)
Two-tailed p-value	0.006	0.001	0.395
Greater battleground control	✔	✔	✔
Number of petro locations	✔	✔	✔
Number of diamond locations	✔	✔	✔
Number of attacks committed by party A	✔	✔	✔
If A is challenger	✔	✔	✔
If A has multiple opponents	✔	✔	✔
Average population near battlegrounds	✔	✔	✔
Capital	✔	✔	✔
Area size	✔	✔	✔
Population density	✔	✔	✔
Precipitation	✔	✔	✔
Minimum elevation	✔	✔	✔
Forest-%	✔	✔	✔
Agriculture-%	✔	✔	✔
Most petro locations	✔	✔	✔
Most diamond locations	✔	✔	✔
Most elevation	✔	✔	✔
Most forested	✔	✔	✔
Most agriculture	✔	✔	✔
Most attacks in current area	✔	✔	✔
Greatest area	✔	✔	✔
Aid per location, t-1	✔	✔	✔
Total funding, t-1	✔	✔	✔
DV lag1	✔	✔	✔
DV lag2	✔	✔	✔
Year dummies (1989-2008)	✔	✔	✔
Obs.	2372	2372	2372
Treated obs.	370	370	370
Matched obs.	2362	2359	2359
Matched unweighted	4603	4526	4526
Caliper (SDs)	0.1	0.1	0.1
Obs. dropped by caliper	10	13	13

The Average Treatment Effect of 0.31 represents a 52% increase in military fatalities (log) in treated observations compared to control observations (mean=0.6 in unmatched sample). In actual numbers of fatalities that represents a 138% increase, or an additional 4.1 fatalities compared to the 2.98 fatalities if there is no, or low, funding concentration.[14]

After making our analyses we explored some randomly selected matched pairs in Angola, Sudan, Uganda, and Somalia to get a sense for whether the mechanism we propose is plausible. We did find that in observations where funding was concentrated, as opposed to diffused, there was a tendency over time towards more committed battles over territorial control – starting in the year of the treatment and continuing into the following year.

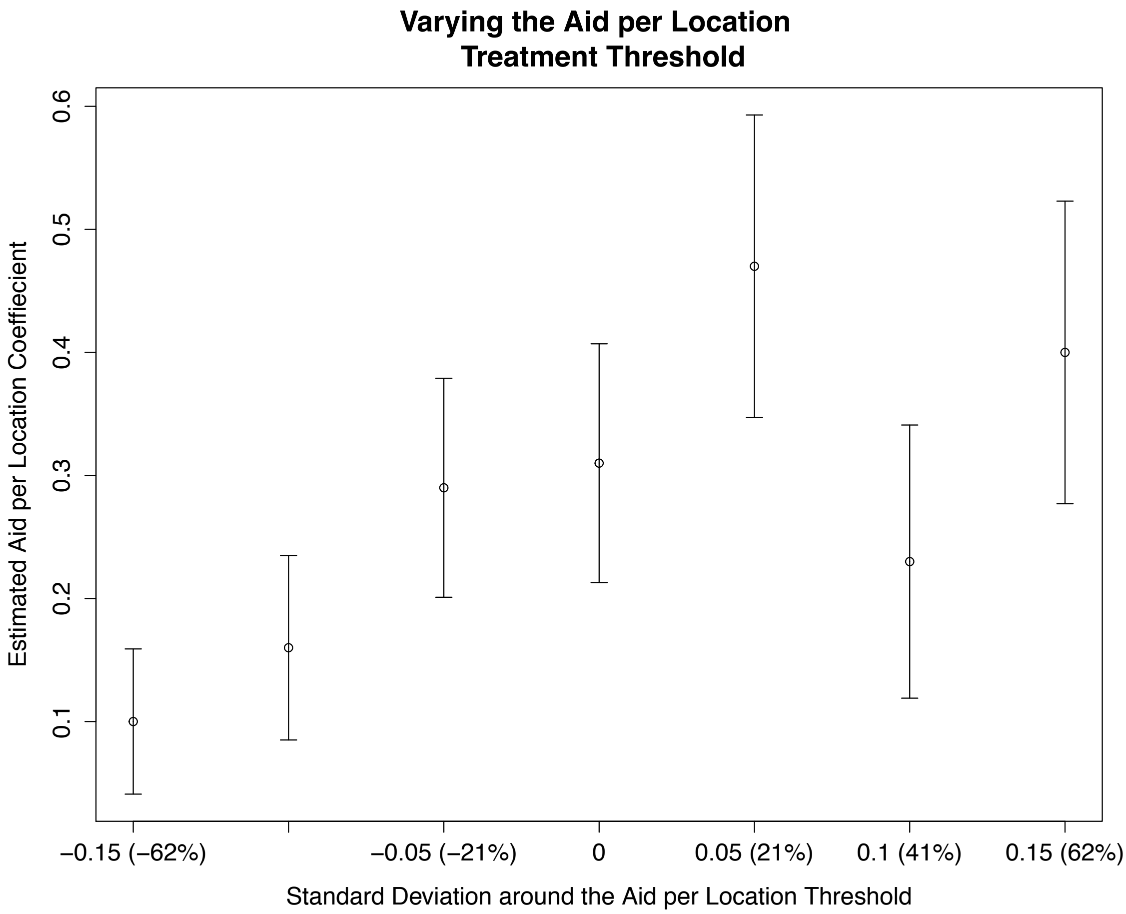
In the theory section we recognized that there are many ways beyond direct looting in which increased funding concentration could result in more conventional fighting. For instance that governments would be more interested in defending locations that are expected to receive highly valuable aid, and that rebels could be interested in attacking these types of locations even if they do not expect to be able to exploit certain types of aid. Interestingly, when examining treated observations we find that some of the shifts towards more conventional battles resulted from the opposite situation where the government engaged in offensives against rebels. It is possible that expectations of highly concentrated funding simultaneously have an impact on both the government's motivation to secure territory, as well as the rebels' vulnerability to being caught in pitched battles when sticking around for too long near valuable locations. There appears to be a range of possible ways that highly concentrated aid, of various types, can motivate more committed battles and greater military fatalities.

4.2 Robustness

The main result is robust to many alternative measures of the most important independent, dependent, and control variables. Starting with the independent variable we checked whether our findings were robust to shifting the threshold for when an observation is considered to be subject to treatment. The different thresholds vary between -0.15 and 0.15 standard deviations around the mean of Funding per location (see Table 1). Figure 5 illustrates that when analyzing funding concentration's effect on military fatalities based on alternative thresholds, the results remain but the effects and statistical significances decrease at 38% ($257,336) of Funding per location's main cut-off point ($676,274).

[14] We transformed the ATE from logarithms to numbers by first adding the ATE to the baseline (average DV for control observations, IV=0). We raised 10 to this sum (10^0.91) and from that figure subtracted 1 to arrive at the sum in actual fatalities (7.1). By subtracting the baseline in actual fatalities (7.7-2.98) we arrive at the ATE in fatalities rather than log of fatalities. It is then possible to calculate the percentage increase in actual fatalities from the baseline of fatality numbers (4.1/2.98=1.38).

Figure 5. Displaying the impact, and confidence interval, of funding per location on military deaths, as a result of varying the treatment threshold



We consider that the Funding per location measurement, of the currently available options, best represents the theoretical concept of funding concentration. We did however also check the results when using the alternative measures, Aid per square kilometre and Total funding. The directions of the effects remain but the statistical significance levels drop to the 90% level. The theoretical impact of Total funding, irrespective of its concentration, could be interesting to develop in its own right. For now the results are the strongest for the Funding per location measurement.

We also check whether the results are robust to an alternative specification of one of the most important control variables, Battleground control. When replacing this measure the results are essentially the same. Although we already control for whether an administrative division contains a capital we also checked whether the results hold when all capital regions are completely excluded from the dataset and found that the effect increased while remaining at the 99% level.

It is possible that how warring parties expect that aid will be implemented has changed over the years. If lessons were learned – for instance since the early 90's humanitarian operations in DRC (Polman, 2010), and food aid operations in Somalia and elsewhere (Addison et al., 2002, p. 383; Maren, 2009; Natsios, 1996) – we would then expect the impact of highly concentrated aid to be ameliorated later in the dataset. We selected 1997 as the starting point since by that time policy makers should have become aware of the problems resulting from when donors created pockets of highly valuable aid in the early 90's. Andrew Natsios (1996) for instance suggested that donors flood markets with food aid to diffuse its value to warring parties. We find that the direction of the effect remains, albeit lower (0.22). The statistical significance dropped to the 90% level. This might suggest that aid implementation has improved over time, but could also result for technical reasons. We for instance know that the quality of available aid data has increased over time. Although there are related studies supporting the idea that funding concentration can fuel violence, this field of inquiry is still in its infancy and will benefit greatly when more geocoded aid data becomes available from more sources, for additional time periods and regions.

In Table 10, we report the results of a series of fixed effects regression models. These models attempt to deal with potential omitted variables that might affect the causal inferences we report. We vary a number of factors including, year and country fixed effects models with and without controls and changing the way we cluster the standard errors and find that the results are consistent in all but the last model.

Table 1. Fixed Effects Regression Models

Model	Year FE	Country FE	Controls	Clustered SEs	Sign	P Value
1	Yes	No	None	side a	+	0.000
2	Yes	No	None	country	+	0.019
3	No	Yes	None	side a	+	0.006
4	No	Yes	None	country	+	0.051
5	Yes	Yes	None	side a	+	0.019
6	Yes	Yes	None	country	+	0.038
7	Yes	Yes	Full	Side a	+	0.055
8	Yes	Yes	Full	country	+	0.181

Key:
Year FE-Year Fixed Effects
Country FE-Country Fixed Effects
DV- Military fatalities (logged)
IV – Lagged continuous measure of the total value of aid entering an area
Controls – None or Full (Oil, rainfall, area size, % of area agricultural, battleground control, capital city, minimum elevation, diamonds, population density, % forested, Average value of aid projects per location)

5. Conclusion

We argued that if aid funding is expected to be spatially concentrated within contested areas, then the probability that warring parties engage in conventional battles over territorial control increases. By contrast, aid that is diffused will increase the probability that warring parties engage in irregular, dispersed, operations. Conventional battles over territorial control are in turn more likely to result in high military fatalities, as compared to irregular and guerrilla warfare.

The independent variable *Funding concentration* was operationalized as *Funding per location*, which measures the US dollars that were committed to each location receiving aid in a contested area. This dichotomous variable was coded 1 if the value per location was greater than the average of all contested areas in the dataset. The unit of observation was specified as a warring party versus its entire opposition in an administrative division per year.

The goal of the research design was to ameliorate the problem of non-random selection into treatment. We relied on propensity score matching where pairs of observations were matched based on how likely they were to receive treatment (i.e., aid per location coded 1). Observations that were similar (in for instance past aid commitments) were more likely to be matched.

We conclude that greater Funding per location increased military fatalities by 138%, or 4 individuals, compared to if there were low or no funding concentration. We caution readers not to overemphasize this result for three reasons: (1) It is impossible to know what percentage of the total population of aid projects that we have been able to geocode. This problem is not unique for this study but is common for this type of data. (2) We rely on the assumption that aid commitments are correlated with warring parties expectations about future aid disbursements, something that is potentially contentious. (3) Our results should only be generalized to contested areas where there have been reports of at least one military casualty during a year.

While there is important work to do, our approach fits with a growing movement in conflict studies to move to smaller units of spatial and temporal aggregation (e.g., Urdal, 2008, Raleigh et al., 2010, Sullivan, 2012). Combined with matching and other techniques to improve valid causal inference, these data allow researchers to get more micro-level tests of micro-level claims.

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