# Aiding War: Foreign Aid and the Intensity of Violent Armed Conflict

MICHAEL FINDLEY
*University of Texas, USA*

JOSEPH K. YOUNG ⓘD
*American University School of International Service, USA*

DANIEL STRANDOW
*Independent Researcher, Sweden*

AND

OLGAHAN CAT
*The University of Texas at Austin, USA*

Does foreign aid reduce violence during ongoing wars? In the policy community, there has been growing optimism about the prospect for aid to improve conflict-affected and fragile areas. We investigate whether foreign aid decreases, or even increases, violence *during* ongoing armed conflict. We advance a theoretical argument that concentrated foreign assistance allocated during ongoing armed conflicts increases military fatalities but decreases civilian fatalities. Using geographically coded data on all sub-Saharan African countries in conflict between 1989 and 2008, within a matching frontier design and supplemented by instrumental variable analysis, we find strong substantive and statistical support for our expectations about military conflict intensity though less support for the expectations about civilian fatalities. The paper provides novel insights about the effects of concentrated aid on military versus civilian conflict intensity, characterizes the effects at a sub-national level, and expands the spatial-temporal period of the analysis. We also probe the plausibility of the causal mechanism using interview evidence drawn from ex-commanders of the Lord's Resistance Army and generals of the Ugandan People's Defence Forces in northern Uganda. The paper offers both academic and policy insights, including that foreign aid allocated during ongoing wars may be more problematic than it is helpful.

¿Reduce la ayuda exterior la violencia durante las guerras en curso? En la comunidad política ha crecido el optimismo sobre la posibilidad de que la ayuda mejore las zonas frágiles y afectadas por conflictos. Investigamos si la ayuda exterior disminuye, o incluso aumenta, la violencia *durante* los conflictos armados en curso. Presentamos un argumento teórico según el cual la ayuda exterior concentrada, asignada durante los conflictos armados en curso, aumenta el número de víctimas militares, pero disminuye el de víctimas civiles. Utilizando datos codificados geográficamente sobre todos los países del África subsahariana en conflicto entre 1989–2008, dentro de un diseño de frontera de emparejamiento y complementado con un análisis de variables instrumentales, encontramos un fuerte apoyo sustantivo y estadístico para nuestras expectativas sobre la intensidad de los conflictos militares, aunque encontramos menos apoyo para nuestras expectativas sobre las víctimas civiles. Este artículo ofrece nuevas perspectivas sobre los efectos de la ayuda concentrada en la intensidad de los conflictos militares frente a los civiles, caracteriza los efectos a nivel subnacional y amplía el periodo espacio-temporal del análisis. También comprobamos la plausibilidad del mecanismo causal mediante entrevistas a ex comandantes del Ejército de Resistencia del Señor (ERS) y generales de las Fuerzas de Defensa del Pueblo de Uganda (FDPU) en el norte de Uganda. Este artículo ofrece perspectivas tanto académicas como políticas, incluida la de que la ayuda exterior asignada durante las guerras en curso puede resultar más problemática que útil.

L'aide étrangère réduit-elle les violences dans les guerres en cours ? La communauté politique est de plus en plus optimiste : l'aide a le potentiel d'améliorer la situation des zones fragiles ou subissant un conflit. Nous cherchons à établir si l'aide étrangère fait décroître, ou même accroît, la violence pendant un conflit armé. Nous proposons un argument théorique selon lequel l'assistance étrangère concentrée allouée lors de conflits armés fait augmenter le nombre de militaires morts, mais diminuer le nombre de civils tués. À l'aide de données codées géographiquement sur tous les pays d'Afrique subsaharienne en conflit entre 1989 et 2008, au sein d'un modèle de correspondance des frontières et avec l'aide d'une analyse des variables instrumentales, nous observons que de nombreux éléments étayent fortement, de façon substantielle et statistique, notre hypothèse quant à l'intensité du conflit militaire, mais plus faiblement notre hypothèse quant aux pertes civiles. L'article apporte de nouvelles informations sur les effets de l'aide concentrée sur l'intensité du conflit militaire ou civil, définit les effets à un niveau sous-national et élargit la période spatiotemporelle de l'analyse. Nous examinons également la plausibilité

du mécanisme causal à l'aide de données d'entretiens avec des ex-commandants de l'Armée de résistance du Seigneur (ARS) et généraux de l'armée ougandaise dans le nord de l'Ouganda. L'article apporte des informations académiques et politiques, notamment le fait que l'aide étrangère allouée lors de conflits en cours pourrait se révéler plus problématique qu'utile.

---

### Introduction

Does foreign economic aid reduce violence? Many aid workers, policymakers, and scholars believe that aid can be a boon (e.g., de Ree and Nillesen 2009), a sentiment that is increasingly reflected in the global conflict and fragility agenda. But what if aid makes violence worse? Based on numerous prominent examples of the destabilizing effects of humanitarian 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* (Uvin 1998; Andersen 1999, 2000; Maren 2009; Polman 2010). 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 (or decrease) the risk that aid sparks the onset of new conflicts. With few exceptions, the question of how foreign aid affects the intensity of conflict during ongoing wars has received little attention.

In this paper, we focus on conflict-affected areas and examine whether concentrated aid funding is more likely to produce higher-intensity conventional military violence in these areas relative to diffuse aid funding. We expect that the first situation, where the warring parties fight more decisive battles, should result in more short-term military fatalities than the latter.[1] We also consider the question of whether concentrated or diffuse aid affects civilian fatalities in local areas, arguing that, if anything, concentrated aid should be associated with fewer civilian fatalities, though they likely still occur as collateral damage, making the relationship imprecise.

An expansive literature examines aid and conflict onset typically at a cross-national level (Addison and Murshed 2001; Arcand and Chauvet 2001; Collier and Hoeffler 2007; Blattman and Miguel 2010; Collier 2011; Grossman 1992; Nielsen et al. 2011; Sollenberg 2012). At the same time, most case studies and policy reports articulate *sub-national processes* through which aid positively (or negatively) affects local *violence intensity*. Beyond anecdotal studies, the few rigorous sub-national aid-conflict analyses (Berman et al. 2011a, b, 2013; Beath et al. 2017; Sexton 2016) focus on *US military-administered* foreign aid during US counterinsurgency campaigns in Iraq and Afghanistan, which represent exceptional types of aid and exceptional contexts (Findley 2018). The few remaining attempts to broaden the study of aid and conflict dynamics examine the sub-national dynamics of other individual countries, including the Philippines (Crost et al. 2014) or Colombia (Weintraub 2016), or focus exclusively on humanitarian assistance cross-nationally (Nunn and Qian 2014; Narang 2015) or sub-nationally (Wood and Molfino 2016).

In this paper, we advance novel theoretical discussions about the impact of aid on military versus civilian fatalities and articulate expectations about their sub-national variation. We also carry out an empirical investigation that is at the same time sub-national and cross-national and covers two decades. In particular, we consider the effects of economic development and humanitarian assistance on the intensity of violence in already-contested sub-national areas[2] for all sub-Saharan African countries for the 1989–2008 period.

We combine sub-nationally geocoded aid commitments (Findley et al. 2011) with newly coded data on territorial control (similar to Aronson et al. 2022) and military and civilian fatalities (based in Sundberg and Melander 2013). Using a *Matching Frontier* approach (King et al. 2017) to address the selection of units and to generate balance on observables, as well as fixed effects and instrumental variable analysis to address unobserved confounding and endogeneity, our results show that when funding is concentrated, rather than diffused, military fatalities increase substantially, but civilian fatalities do not.

We briefly consider possible mechanisms underlying the statistical results by referencing qualitative case evidence from interviews with ex-Lord's Resistance Army (LRA) commanders in Uganda (introduced in Findley et al. 2023), and we further contextualize the results with respect to other sub-national studies of aid and conflict dynamics. Taken together, the paper advances new theoretical ideas, broadens the empirical scope, and utilizes new data and multi-method approaches to demonstrate the deleterious effects of foreign aid on the military intensity of ongoing armed conflict.

The argument and findings of this paper reflect critically on the global agenda prioritizing aid to *conflict-affected and fragile* contexts, which has permeated donor discussions over the past several decades. The fragility agenda lays out best practices for delivering aid in ways that are sensitive to ongoing conflict dynamics and that do not exacerbate the underlying reasons why conflict began in the first place. Indeed, over the past several decades, donors have placed a grand, but perhaps foolish, bet that they can use aid not only to improve development but also to promote stabilization of ongoing conflict dynamics. Our results are consistent with some critical arguments about aid for stabilization (Zürcher 2019) and provide rigorous empirical evidence that casts doubt on this approach. Indeed, our results indicate that foreign aid provision during wars may substantially increase localized military violence in ways that add up to substantial national-level instability.

### Aid and Conflict Intensity

In the year following the September 11 terrorist attacks, former President George W. Bush famously asserted that "we fight against poverty because hope is an answer to terror." This assertion reflects the optimism of many foreign aid proponents that aid can improve economic, political, and social circumstances, which in turn reduce the motivation to engage in political violence (Collier and Hoeffler 2004b). The

---

[1] If anything we would expect that if diffused aid makes low-intensity violence more viable, then 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] 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.

rockstar economist, Jeffrey Sachs, and the economist rockstar, Bono, have been leading proponents of aid as a broad solution to many challenges in low-income countries (Sachs 2006). Others are critical of this hope citing aid as the very problem (Grossman 1992; Easterly and Levine 1997; Moyo 2009), binding countries with massive sovereign debt, creating perverse incentives for rent-seeking, and even providing the very resources that pay for horrendous violence (Uvin 1998). Still, others take the perspective that aid's effects are conditional on a variety of factors and can have mixed effects (Collier 2007; Darden 2019).

Critical foreign aid approaches highlight both the promise and pitfalls of aid, both in aid's broader societal impacts as well as in proximate effects on conflict, but critical approaches point to other sources of aid's typically deleterious effects. Critical approaches point to structural relations embedded in systems of state power and hierarchy (Hattori 2001), international development activities as a continued manifestation of neo-colonialism (Sabaratnam 2017), the illiberal role that foreign aid plays in propping up authoritarian regimes (Hagmann and Reyntjen 2016), fundamental mismatches between international intervention modalities and localized conflict dynamics (Autesserre 2010), and the self-interested hypocrisy that donors themselves come to believe and then in seeming good faith peddle to the world's poor (McMillan 2004; Weaver 2008; Woods 2014).

For the purposes of this paper, we are open to the possibility that aid can be effective in some respects, but we focus our investigation on its proximate impacts on violent conflict, in particular on the intensity of violence during *ongoing armed conflict*. We are mostly agnostic as to whether aid has distinct effects on conflict onset versus conflict intensity. Some theoretical expectations imply similar predictions across stages, but others apply more neatly to a single phase of conflict. We suspect that foreign aid creates some unique incentives when it is distributed during ongoing wars to conflict-affected regions. Coupling this suspicion with the practical consideration that our empirical strategy is best suited to examine intensity claims (logged counts as the dependent variable), we emphasize conflict intensity as our outcome of interest.[3]

### Approaches

Increased aid may influence conflict intensity in at least three ways, the first two of which emphasize national-level military conflict, and the third bridging the approaches by explicitly incorporating localized aid and conflict dynamics. First, aid that is disbursed via the government (Addison and Murshed 2001) and that could be diverted into private hands may increase the value of holding government power. Rebels could therefore expect to gain access to such aid rents by capturing the center of state power (Grossman 1992; Azam 1995). The attraction to accessing rents by holding government power might depend on whether prospective coups or rebel leaders stand to gain greater rents relative to their pre-war access to rents (Grossman 1992). In a complementary way, even if rebels do not seek to capture state power altogether, rebels may use violence to sabotage incumbent governments so that aid provision does not result in greater public or private support. To capture state power or sabotage incumbent governments from benefit-

ing, the overall expectation of this first perspective is that aid would *increase* conflict intensity.

Second, although aid may increase the size of the prize of holding governmental power, aid may also improve the ability of governments to deter and defeat rebellion (Arcand and Chauvet 2001). Indeed, aid appears to be associated with substantial levels of military expenditures (Addison and Murshed 2001; Langlotz and Potrafke 2019), accounting for as much as 40 percent of African military expenditures (Collier 2011). Aid funding may be non-appropriable by rebels, but more easily available rents, such as diamonds or other lootable resources, may also be present. Even if rebels could succeed in capturing government power, the probability of doing that is generally low and implies a lengthy struggle. Immediate natural resource rents would therefore be preferred over heavily discounted aid rents (Collier and Hoeffler 2002; Collier 2011). Whichever mechanism is in operation, the overall expectation of this second perspective is that aid would *decrease* conflict intensity.

Third, aid disbursements may bypass the government and the capital completely (Addison et al. 2002). Aid supplies could provide warring parties with greater incentives to engage in looting rather than attempting to govern the entire territorial state (Collier 2000; Collier and Hoeffler 2004a). Rebels may still be motivated by rents, but rent-seeking behavior would occur in the areas of the country near where they live and can loot localized aid (Anderson 1999; Maren 2009; Blattman and Miguel 2010; Findley et al. 2011). Warring parties can exploit aid through theft and looting, and local elites with interests in maintaining violence, as in Somalia, can then benefit from corruption or unfair business opportunities (Anderson 1999; Webersik 2006; Maren 2009). Looted or embezzled aid can then be used to pay soldiers and buy arms, thus feeding ongoing disputes (Anderson 1999; Maren 2009). 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 further from the reach of the central government. Rebels would fight further away from the capital and exploit high-value, localized aid opportunistically until the time at which they gain sufficient strength to bring the violence closer to the institutions of the state. Such an approach makes possible an examination of strategic decisions about when and where to target aid within a country. Sexton (2016), for example, provides some evidence that foreign aid increases violence in contested territories where rebels would mobilize, and reduces violence in locations controlled by the government where rebels prefer to avoid fighting. For our purposes, such strategic decisions manifest themselves in localized violence not isolated to the capital region.

Our approach connects the logic of approaches 1 and 2 by applying them to the sub-national level, where political capture or rent-seeking could motivate violence and where the state may use aid resources not to deter rebellion broadly, which would manifest as less violence, but rather to defeat rebellion in the periphery, which would manifest as more violence. We thus contend that a third approach, emphasizing localized aid and conflict, usefully adapts exist-

---

[3] Future work could provide more specific expectations and attempt to disentangle whether concentrated aid increases the probability of state versus one-sided conflict onset, intensity, recurrence, or some combination.

ing insights about aid and conflict to generate more precise expectations and analysis.

### *Geographically Concentrated Aid*

How aid funding is disbursed, whether in a geographically *concentrated* or *diffused* manner, is a critical consideration. We define aid as concentrated if a particular location receives relatively higher levels of assistance compared to other locations.[4] In this way, funding could be considered locally concentrated well beyond the capital or other areas of government's control. Aid may thus function as a localized prize that attracts attempts at conquest, without necessarily increasing government deterrence.

Whether aid funding is concentrated or diffused likely influences warring parties' military decisions in already contested areas. It is more beneficial to attempt to control specific locations rather than large areas because the former is easier to defend and requires fewer troops to maintain. When resources are valuable and spatially concentrated, they should tip the scale in favor of attempting territorial control rather than intermittent raiding. Competing for territorial control, for instance, control over the capital or another high-value target should hence be more likely with a high concentration of aid values. A range of low-intensity irregular operations should be more likely when 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 than the latter. Previous research shows that conventional warfare tends to generate more fatalities compared to low-intensity operations, such as guerrilla and irregular warfare (Valentino et al. 2004; Lacina et al. 2006). We consequently argue that in already contested areas, funding that is concentrated should motivate more committed battles, resulting in more military fatalities. We thus hypothesize:

**H1:** *Concentrated aid funding in a location increases the number of military fatalities, relative to military fatalities in locations with diffused aid funding.*

Even if warring parties do not always aim to exploit foreign aid directly, they may nonetheless be more inclined to defend violently the locations they control that have high funding concentration. By defending those areas, warring parties, particularly governments, can better fulfill their duties to the public and more broadly improve the area's development. Warring parties may also be more interested in conquering areas with highly concentrated funding to take credit for improved development and receive greater popular support. Whether warring parties engage in more decisive battles in order to exploit aid directly or reap more benefits indirectly may further depend on the type of aid committed. Aid that cannot be physically exploited should increase competition for the proverbial hearts and minds of the population, and aid that can be appropriated should increase the likelihood that warring parties compete for more straightforward exploitation. In either case, concentrated

aid funding makes military contests worthwhile for at least one side of a conflict.[5]

If concentrated aid increases military fatalities, it may not necessarily be associated with higher levels of civilian fatalities, relative to areas with diffused funding. As decisive military contests occur around concentrated aid, one implication is that conflict intensity should manifest itself primarily on the military stage. As a result, civilian fatalities could be lower in those areas. Regardless, we ask whether civilian fatalities are higher in areas with concentrated funding relative to civilian fatalities in areas with diffused funding. As the theoretical rationale above articulates, we expect that in areas with diffused funding, irregular conflict, such as guerrilla warfare, may be more likely and may be associated with higher levels of civilian fatalities. As such, we might expect that locations with concentrated aid experience *more military fatalities,* but those same areas experience *fewer civilian fatalities.* We thus hypothesize:

**H2:** *Concentrated aid funding in a location decreases the number of civilian fatalities, relative to civilian fatalities in locations with diffused funding.*

Still, even when conflict plays out primarily on the conventional military stage in locations with concentrated aid funding, civilian collateral damage often occurs and may attenuate the expected reduction in civilian fatalities. That is, in areas with both concentrated and diffused funding, there are good reasons why we might observe some level of violence against civilians in ways that no clear relationship emerges.

## Research Design

We now turn to our empirical strategy to investigate these hypotheses. 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 a product of complex selection and non-random assignment. To address these issues, we use a matching frontier approach together with instrumental variables, fixed effects, case analysis, and an array of other tests.

### *Cases and Data Structure*

Our sample consists of 479 warring parties against their entire oppositions in eighteen sub-Saharan countries between 1989 and 2008. We primarily use data from two original coding efforts. We first adapted and developed the UCDP geocoding methodology (Sundberg et al. 2011) so that it can be used to code the geographic coordinates of foreign aid projects (Findley et al. 2011; Strandow et al. 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. Figure 1 shows the foreign aid

---

[4]As discussed in the research design section, we measure aid concentration as present if the value of aid per location in each 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, including with a measure that considers amounts relative only to historical averages.

[5]The logic just outlined is consistent with a bargaining approach to war. From a bargaining perspective, there should always be some agreement that parties can reach to avoid war onset or continuation of war. And yet, combatants often do not reach agreements due to various strategic dilemmas, especially credible commitments. From the perspective of foreign aid distribution, large amounts of foreign aid may create large power shifts that make any commitments non-credible. Nielsen et al. (2011) apply this logic to the onset of the civil war, and here we contend that it may characterize the continuation and related intensity of the war. And notably, this logic would suggest that large amounts of aid create power shifts, thereby making future adherence to any agreement non-credible. In addition, resulting violence should be military in nature as the two core combatants fight now rather than reach a settlement.
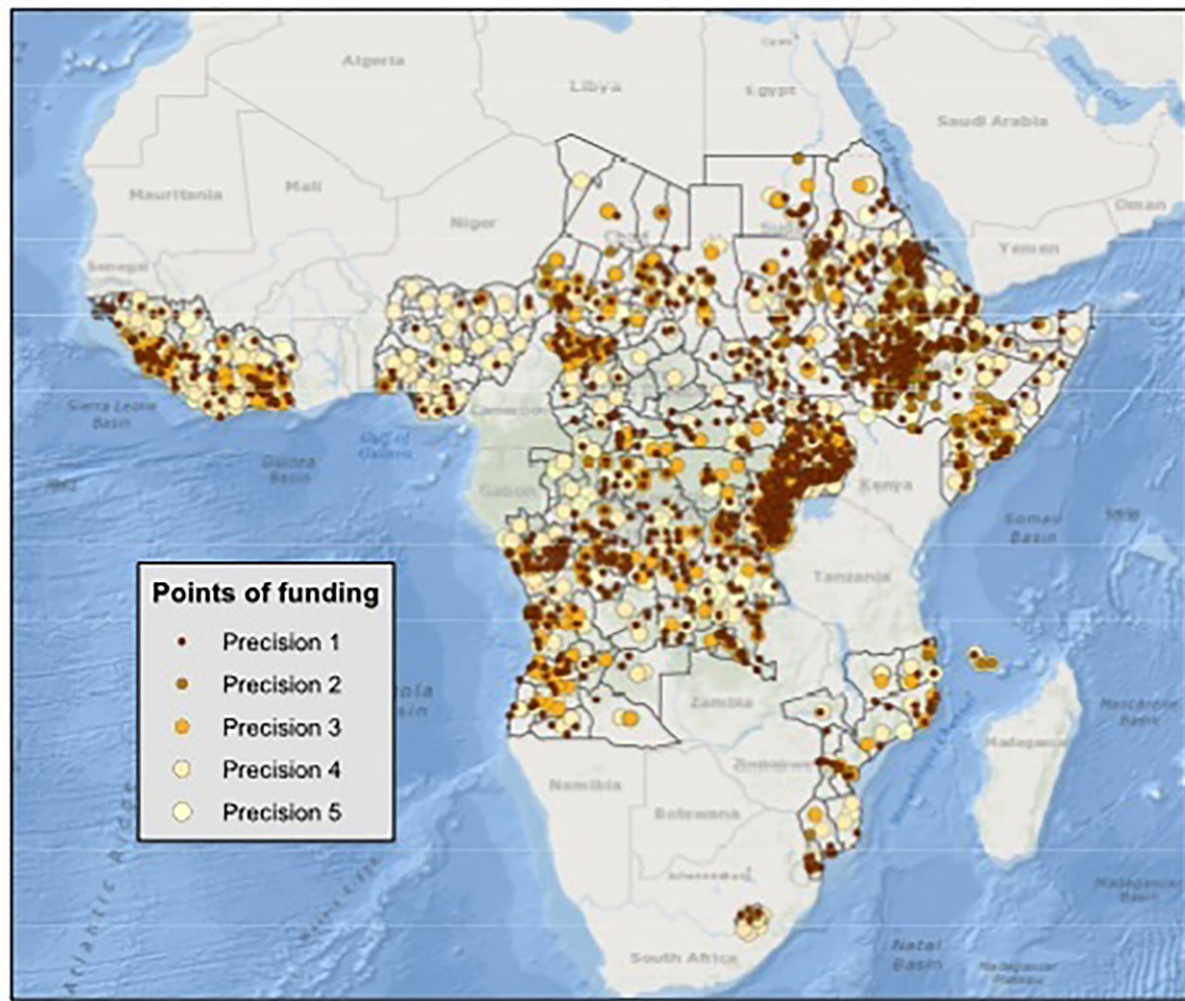
**Figure 1.** This map contains all aid projects (assigned geographic coordinates) for conflicts based on project descriptions in the AidData 2011 research release for the temporal period of our study (1989–2008). Each dot on the map represents a discrete aid project, and its size is scaled by the amount of aid it represents and colored according to the precision with which it is coded.

locations coded in the data set and online appendix B.3 explains the coding process. In short, we coded each discrete foreign aid project at the most local level possible given the available information and included a precision code identifying which level. We detail the full coding, including precision levels, in online appendix B.3, and note here that all of our analyses include projects with precision codes 1–5, which represent the majority of possible aid projects.

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 (Aronson et al. 2022). This events dataset is coded from, and is compatible with, the Uppsala Conflict Data Program's GED sub-Saharan Africa dataset (Sundberg and Melander 2013). Online appendices B.4 and B.5 explain the coding process. 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 to measure the dependent variables. Online appendix B.6 explains the aggregation process.

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 are further developed in online appendix B. Empirically, the dataset covers warring parties in sub-Saharan African states that have had 1 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 twenty-five annual deaths resulting from fighting between an organized warring party and a government.

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.
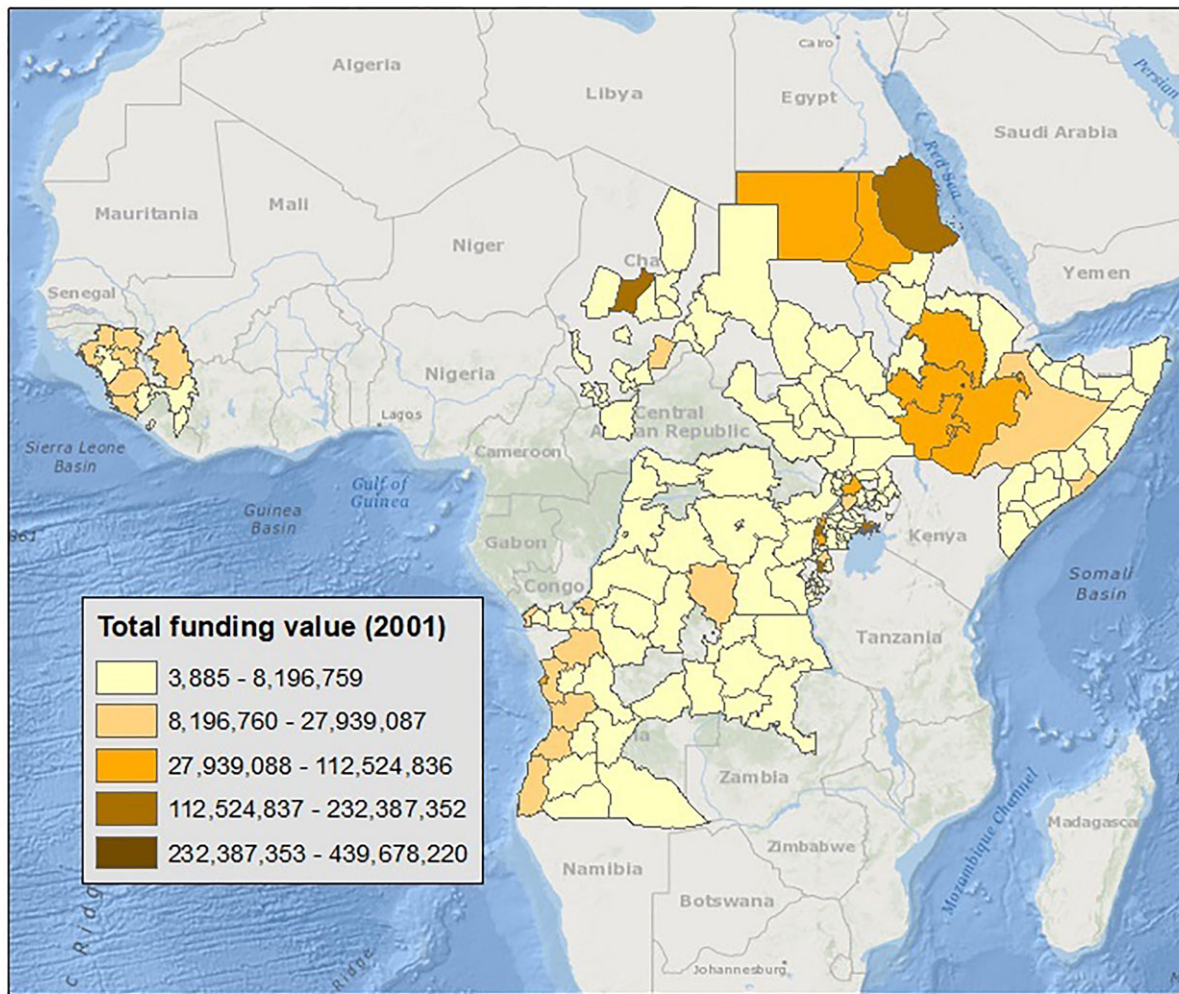
**Figure 2.** Total aid funding in constant USD in 2001 only.

Finally, we note that we aggregate these various data within administrative regions rather than grid cells for two main reasons. First, our key data are coded in relation to administrative regions and therefore do not necessitate the introduction of strong assumptions to divide measures over various cells. Second, our theoretical model assumes that warring parties develop strategies for politically relevant areas, and this analytical unit thus matches the theoretical argument better. Since the release of PRIO-GRID, studies increasingly define arbitrarily sized grid cells to serve as exogenous units of analysis, which can be important in the absence of other causal identification strategies, but they also rely on tenuous measurement choices that can introduce divergence between theoretical and empirical models. See online appendix B.2 for further rationale behind this decision.

*Observing Foreign Aid*

INDEPENDENT VARIABLES

To test the hypotheses, we formulate dichotomous variables that are coded 1 if an observation receives *treatment*, meaning the value (constant USD) of committed aid in a location is above the mean, and 0 if it is not. For robustness checks, we vary the threshold by 0.05 times standard deviation above and below the mean within ±0.15 times standard deviation

range. We use commitment data primarily because disbursement data are spotty for much of our timeframe, and the OECD itself discourages users from relying on disbursement data for the pre-2002 period, which covers a substantial part of our timeframe (extending back to 1989).[6]

We specify a variable that captures whether warring parties would expect aid funding to be 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.[7]

An aid commitment is funding that a sender has pledged to disburse to the recipient. We make the assumption that sufficiently large sums of aid committed to a few enough locations capture 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 LRA in northern Uganda substantiate the assumption (Findley et al. 2023). It is, however, possible

---

[6] The OECD specifically instructs that "analysis on CRS disbursements … is not recommended for flows before 2002" (OECD 2006).

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

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 and 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 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). Whether resulting from earlier disbursements or current pledges, we therefore find it plausible that aid commitments reflect warring parties' expectations of future funding concentration. To illustrate, figure 2 shows the locations of aid funding in sub-Saharan Africa for the year 2001 only.

*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, both for the year 2001 only. The prevailing method for standardizing aid in national-level studies is the funding's share of the gross national income (Sollenberg 2012) or to divide aid by population. For subnational analyses, the quality and coverage of income data are not great enough to allow this type of transformation with local income data. Moreover, our theory is not related to 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, whatever the size of the national economy, is likely to capture warring parties' expectations.

### Dependent Variables

We consider two categories of violence intensity: short-term military and civilian fatalities.[8] There are a number of ways to operationalize these dependent variables. In online appendix A, we discuss how we define the two categories of intensity in relation to types of violence specified by the Uppsala Conflict Data Program (Eck et al. 2004) and Kalyvas (2006). 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 and Iyer 2010). Here, we conduct analyses using yearly data on fatality counts. Both dependent variables are measured in the year following the measurement of 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 and Melander 2013). The operational measures used in the study are logged values of each dependent variable, either military or civilian fatalities at time $t + 1$.

Military fatalities tend to be distributed over a high number of events each with fewer fatalities and a small share of events with exceptionally high fatalities (Clauset et al. 2007)

relative to civilian fatalities. 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, then 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 taking its log transformation.[9] There are a number of events with zero military fatalities.[10] The result is a *Military fatalities log* variable. Related, we compute a comparable *Civilian fatalities log* variable, which measures civilians killed in contested areas who are either caught in the crossfire or targeted in deliberate one-sided violence.

### *Challenges of Identifying Causal Effects*

There are two particular challenges for causal identification in this context: selection processes and unobserved covariation. In short, there may be consistent differences between our treatment and control groups due to observable and non-observable factors. We use matching procedures to address selection issues and fixed effects models to address some forms of unobservability. Matching allows us to create balance between treatment and control groups on observable variables, which allows us to mitigate these selection issues.[11] We also employ an instrumental variable approach to address endogeneity. These approaches do not solve all threats to valid causal inference, especially key unobservables, but they provide the most reasonable design for these particular data and research questions. And as noted, we supplement the statistical analysis with a case-based plausibility probe of aid and conflict in Uganda and discussion of several other conflict contexts.

We use a matching frontier approach to deal with selection effects (King et al. 2017). As mentioned above, matching allows us to reduce the imbalance between the treatment and control units on observable covariates. To do so, most matching methods require some amount of data pruning or throwing out observations that are non-comparable.[12] Pruning data often leads to increasing the variance of our estimates and leads to what is often called the balance-variance tradeoff. Unlike other matching methods, such as propensity scores, which optimize one side of the balance-variance tradeoff and leave the other to users, the matching frontier technique jointly optimizes solutions for each subset of the sample one by one.[13]

Matching cannot eliminate the influence of unobserved covariates and can therefore only achieve balance based on observed variables. We thus take a number of additional steps, which we report in the robustness section of the results. To account for unobserved covariates, at least in part, we estimate fixed effects models. We also note that we used an extensive set of observable covariates and, to the extent that they are correlated with confounding unobservables, then they may help account for unobservables. Finally, we

---

[8]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.

[9]We also rank transformed the dependent variable, and the results are qualitatively similar.

[10]Since the logarithm of zero is undefined, we add one to the fatality estimate before the log transformation.

[11]King et al. (2017) argue against using propensity scores when matching and attempting to create balance between treatment and control groups. Instead, we use their advice and use a matching frontier approach.

[12]These observations are usually control units.

[13]Our covariates consist of both binary and continuous variables, so we use Mahalanobis distance when matching them.
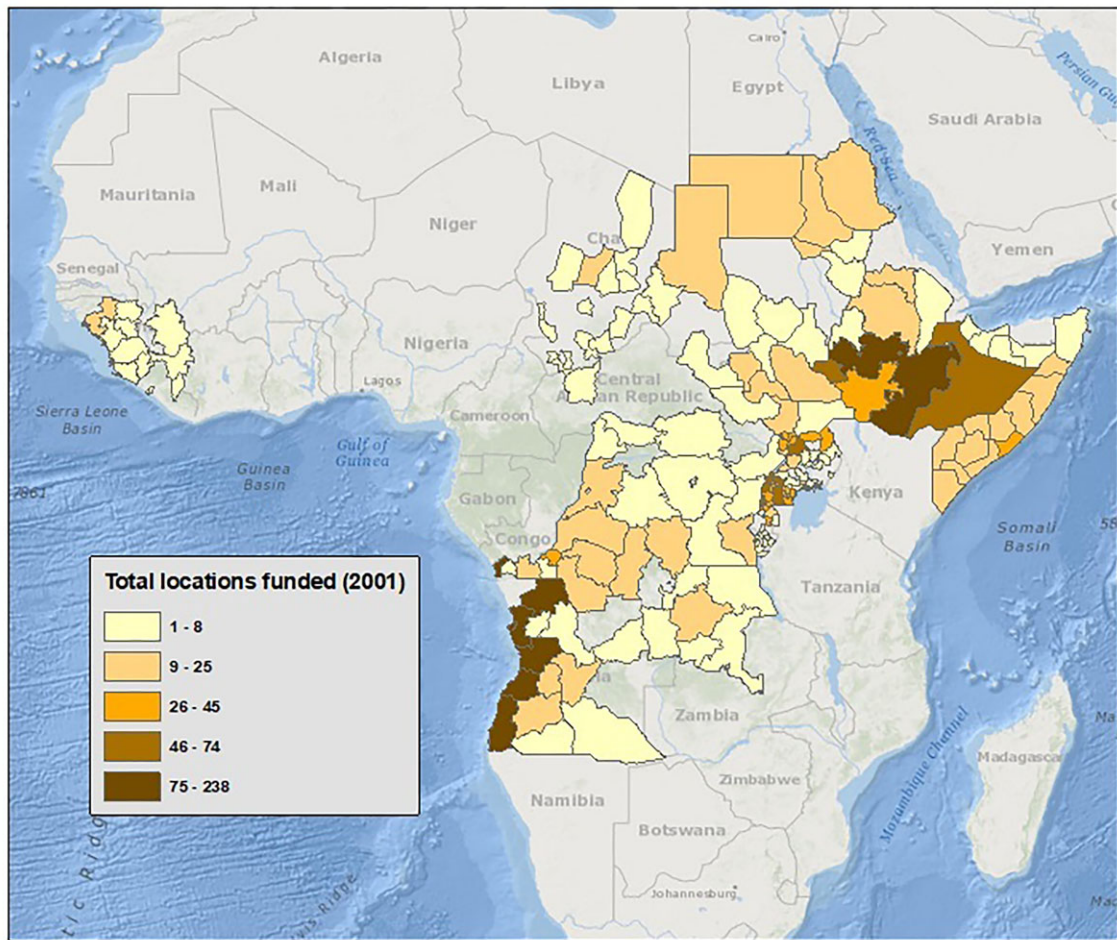
**Figure 3.** Total number of locations to which funding was committed in 2001 only.

conduct an instrumental variable analysis to address unobserved confounding, in particular, endogeneity.

### Estimation

We conduct ordinary least squares (OLS) regression together with the matching frontier approach. Although the matching frontier maximizes automation to reduce user dependence, sample size selection is still left to users. Continuing with the suggestion of King et al. (2017), although we select the sample size at which the iteration ends, the impact should not be dependent on the sample size choice. We also show that for every possible-sized matching solution, the estimates of the effects of aid concentration are positive and sizable (reported in the results below). Another choice is the threshold for our independent variable. The threshold of our main independent variable is the mean value of funding per location. In addition to that, we specify other models with six different thresholds nested in the range between 0.15 times the standard deviation above and below the mean. We also compute these mean values relative to historical aid in given administrative units rather than cross-sectionally across administrative units and show that the results are similar.

### Unobserved Covariation in Time and Space

Violence can spread over both time and space (Kalyvas 2008). Controlling for a lagged dependent variable can reflect both diffusion over time as well as space, as long as it is safe to assume that spatial diffusion is lagged (Beck et al. 2006). We include the treatment variables at $t-1$ as a way to increase the probability of pairing observations that have a similar history of receiving aid. There are many forms of unobserved covariates, some of which vary due to regional or global trends. By including year dummy variables, it is possible to take this variance into account. In table A4 in online appendix A, we include a summary of the temporal variables. As discussed more fully below, we supplement the matching approach with standard fixed effects models that incorporate many additional factors to address other inferential challenges.

### Covariate Sets for Analysis and Matching

There is no consensus in the literature concerning exactly which control variables to include when matching. Although suggesting that matching performs well with many control variables, Rosenbaum and Rubin (1983) do not specify inclusion criteria. By contrast, Pearl (2009) recommends including slimmed covariate sets. One guideline is

not to include any post-treatment covariates as controls so as to not confuse what effect is measured (Gelman and Hill 2006, 188). 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 measured pre-treatment.[14]

We match on the following variables and use them in the analysis: *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 percentage, agriculture percentage, most petro locations, most diamond locations, most elevation, most forested, most agriculture, most attacks in current area, and greatest area.* These variables are defined and discussed in detail in tables A5–A7 in online appendix A.

### *Qualitative Context*

To contextualize the quantitative findings, we reference some insights gleaned from a set of interviews that Findley et al. (2023) conducted with former generals from the LRA as well as current generals in the Ugandan People's Defence Force (UPDF). We synthesize broad lessons about whether aid concentration motivated more violent conflict between the LRA and the Ugandan military, but not between the LRA or UPDF and Uganda civilians. This qualitative evidence thus serves the function outlined by Goertz (2017) of demonstrating that the causal mechanism indeed operated in an $X = 1, Y = 1$ case, where concentrated aid and higher levels of military conflict were both observed.[15]

### **Empirical Findings**

#### *Aid and Military Fatalities*

The results of our empirical investigation offer strong support for our hypotheses. We draw these inferences from OLS statistical models estimated on matched samples, and then contextualize the results relative to case-specific studies of aid and conflict. After presentation of the core and contextualized results, we report a set of robustness checks that corroborate the main findings.

In our primary analysis, we report dichotomized results in which aid concentration is defined as high when it is above the mean and low when it is below the mean. We first estimated the OLS model without covariates but based on the matching frontier pre-processing. In this analysis, the effect of aid concentration on military conflict fatalities is strongly positive. For our primary analysis, the estimated coefficient is 0.41 and is illustrated on the far right of figure 4 at 2,047 pruned observations. As the figure shows, the strong positive effect is consistent across the full range of matching frontier specifications and corresponding numbers of matched observations (based on pruning).

We then estimated the primary OLS model with control covariates included at the regression stage (reported below and in the online appendix). In all cases, the results again offer strong evidence that concentrated aid is associated with greater military conflict intensity. The estimated OLS
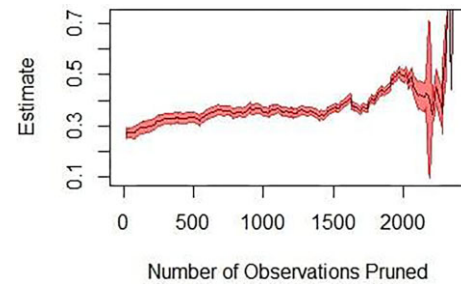


**Figure 4.** Estimated coefficient and confidence interval across the range of observation pruning. In all cases, the results are positive and significant.

coefficient for military fatalities regressed on concentrated aid is 0.419 with a standard error of 0.109 and a corresponding *p*-value of 0.000. (Raw regression results appear in table A.13 in online appendix A.) Substantively, this means that if aid is concentrated to a location, military fatalities in that location increase by 51 percent (converting from logged military fatalities).

This means that in conflict-affected areas, locations that receive concentrated aid experience ~96 more military deaths per year relative to locations that receive diffuse aid. The effect changes commensurate with the threshold for establishing concentrated aid, varying between ~56 and ~106 military deaths. Aggregating to the national level, when conflict-affected aid locations in a country receive concentrated aid, countries are predicted to experience 487 more military fatalities per year relative to aggregated fatalities in conflict-affected locations with diffuse funding. The middle line in figure 5 (with the triangle indicator and label "Mean") illustrates the main result.

To explore different possible thresholds for the definition of how much aid to a location constitutes aid concentration, we vary the threshold by changing the standard deviation around the mean. The results of these additional analyses (the six additional lines above and below the main result shown in figure 5) also strongly support the key conclusion that concentrated foreign aid is associated with higher military conflict fatalities. In addition to being positive in all specifications, we also observe a pattern in which increasing the threshold (e.g., only allowing for more concentrated funding) increases *military* casualties more, which is further consistent with the argument we articulated in Hypothesis 1.

The Uganda case example provides insights into the mechanisms through which concentrated aid can increase conflict intensity by causing more military fatalities. In Uganda, the LRA targeted aid extensively at various points during the war, especially outside of the window when it received support from al-Bashir in Sudan (roughly 1994–1999). The Ugandan military (UPDF) tended to protect concentrated aid more heavily than diffuse aid, which resulted in higher-intensity violent military clashes. The UPDF allocated substantial troops to accompany the disbursement of concentrated aid with full mandate to engage militarily to protect the aid. The UPDFs were also stationed in the north, based primarily near the village (now city) of Gulu, and maintained a longer-term presence near displacement camps where concentrated aid was frequently delivered and then distributed. Consequently, LRA efforts to obtain concentrated aid required higher levels of engagement to clash effectively with the Ugandan military. Sometimes the LRA

---

[14]We outline all covariate sets in the online appendices, especially in tables A5–A7 and A10–A12 and online appendix C. We also mention them in the empirical sections for each estimation separately.

[15]A full accounting of those interviews is beyond the scope of this paper, and we refer readers to Findley et al. (2023) for any additional context.
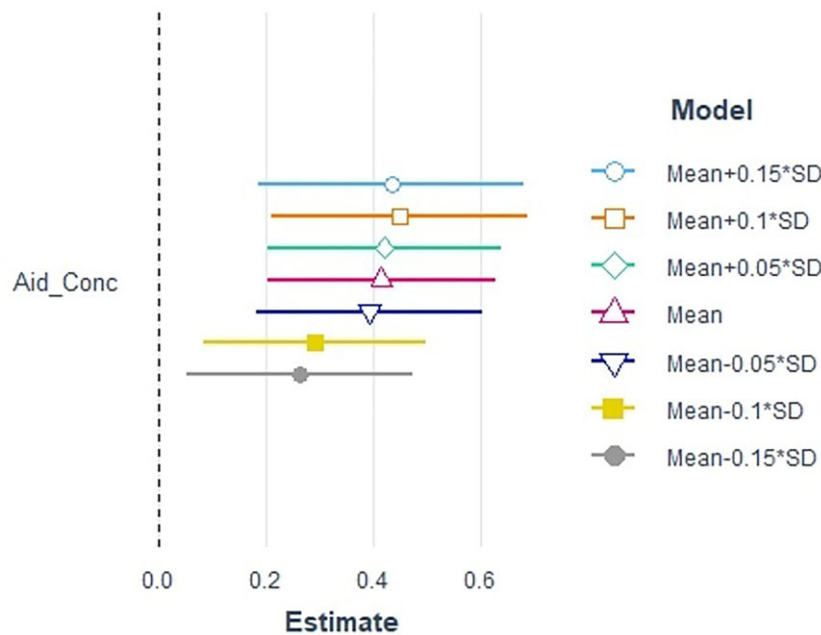
**Figure 5.** Estimated coefficients and confidence intervals for varying operationalizations of the aid threshold. In all cases, the results are positive and significant demonstrating that aid is associated with higher levels of military conflict intensity.

clashed with the military as the UPDF escorted the aid, whereas at other times the LRA and UPDF clashed upon distribution of the aid into warehouses and homes or in the time that followed distribution.

Ex-LRA commanders described a sophisticated aid targeting strategy that both bolstered its resources and also served to sabotage the government. For their part, UPDF generals described aid protection as a strategic objective to prevent the LRA from obtaining needed resources as well as a core humanitarian responsibility that could help the UPDF win hearts and minds. Although requiring greater effort and risk, military clashes made it possible to obtain valuable foreign aid resources. As it turns out, the LRA also directly looted from the military itself, all the way down to taking UPDF uniforms, which the LRA then used to clothe its own forces and build a military culture.

Some recent, disaggregated studies on Iraq and Afghanistan also lend support to our argument, but with emphasis on the counterfactual: smaller targeted and secured aid projects appear to decrease the intensity of violence (Berman et al. 2011b, 2013; Sexton 2016; Lyall 2019). When CERP[16] projects are small (<$50,000), violence against coalition troops and Iraqi government forces decreases. One reason may be that small programs are easier to revoke if they do not lead the local population to share more information. If populations share more information, then it is easier for government troops to increase security (Berman et al. 2013).

### Aid and Civilian Fatalities

We do not observe the same effect for civilian conflict fatalities. Indeed, there is no discernible increase or decrease in civilian fatalities in response to concentrated aid. The estimated coefficient for aid on civilian fatalities is 0.077 with a standard error of 0.059 and a corresponding $p$-value of

0.19, which is not consistent with most standards of statistical significance. Substantively, this means that if aid is concentrated to a location, civilian fatalities increase by 8 percent (converting from logged civilian fatalities), although we emphasize that the results are not statistically significant and thus cannot rule out that there is no effect. Aggregating to the national level, when conflict-affected aid locations in a country receive concentrated aid, countries are predicted to experience fifteen more civilian fatalities per year.

As with military fatalities, we conduct the analysis varying the operationalization of concentrated aid and find that the result is consistent in all but one of the specifications.

The middle line in figure 6 (with the triangle indicator and label "Mean") illustrates the main result. Because the confidence intervals nearly all include zero, regardless of how we operationalize *aid concentration across these models*, we cannot rule out the possibility that *aid concentration* has no effect on civilian casualties.

The question of whether concentrated aid-motivated violence against civilians is more complex, as the LRA case shows, given that the LRA is well-known for abductions and broader civilian targeting. Ex-commanders from the LRA reported a sophisticated decision-making calculus that, at its core, centered on calibrating positive relations with the civilian population. The ex-LRA commanders described a strategy in which they recognized that their efforts to capture aid would harm the citizen population at least indirectly and sometimes even directly with violence. At the same time, they recognized that they relied on civilian support and needed to engage cautiously to the extent possible. This led them to engage violently against the UPDF and not the civilian population whenever possible. When the LRA needed to target concentrated aid once in villages, they described looting only a portion of the aid in order to preserve some semblance of good relations. Given the dependency of the LRA on those civilians and the heavy protection that government convoys provided to the foreign aid during distribution, the LRA needed to tread lightly, leading it to en-

---

[16]CERP is the US Army Corps of Engineers' *Commanders Emergency Response Program.*
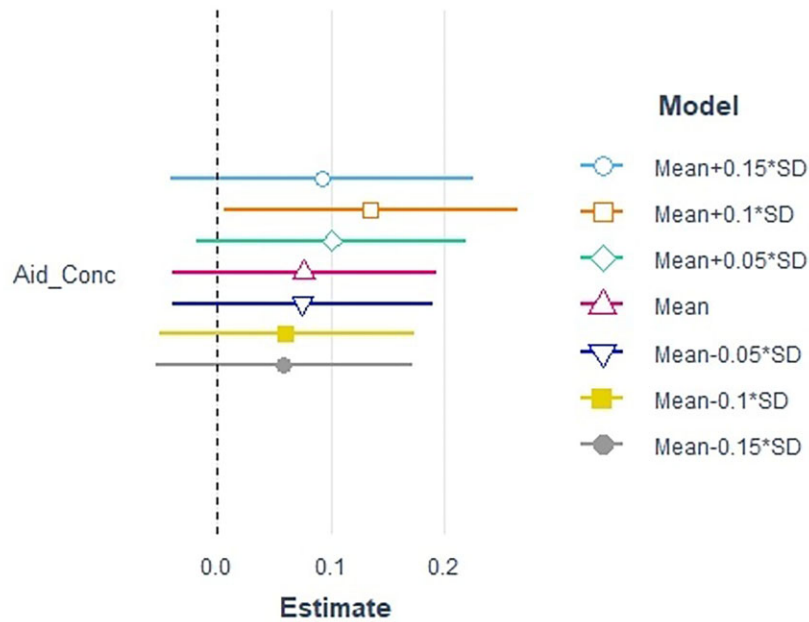
**Figure 6.** Estimated coefficients and distribution for varying operationalization of the aid threshold for civilian fatalities.

gage in violence more with the government military rather than with the civilian population.

Of course, at various points during the war when the LRA was particularly desperate, they turned to target civilians violently, but also in different ways than commonly described. Strikingly, ex-LRA leaders described a very different rationale for abduction than is commonly given—the LRA abducted people to transport commodity-based foreign aid back to LRA forces in the bush. In addition to some level of direct targeting, civilians also suffered when they were caught in the crossfire of military clashes between the UPDF and the LRA. Even the best attempts by both the UPDF and the LRA to limit violent effects on civilians were stymied both by necessity and circumstance.

These insights from the Uganda conflict help us understand the complexity of violence toward civilians in aid context and difficulties associated with making clear predictions. Despite the fact that war dynamics in Uganda point toward higher conflict intensity in military battle deaths, which would expectedly decrease civilian fatalities due to tactical changes in warfare as hypothesized, rebels' varying approach to civilians depending on their objectives and high collateral damage due to violent military contests make it harder to anticipate a clear direction in civilian casualties.

Our statistical analysis for sub-Saharan Africa did not reveal a clear relationship between aid concentration and civilian fatalities, perhaps for reasons illustrated by the Ugandan case. Evidence from a large community-driven development project in the Philippines corroborates this finding with aid's effects are far more pronounced for military rather than civilian fatalities (Crost et al. 2014). These violence-promoting effects are most pronounced for insurgent-initiated attacks against the government in the pre-program period, which may have been designed to sabotage prospective government gains from successful implementation.

## Robustness

### *Conceptualization and Measurement of Aid Concentration*

Our baseline specification of concentrated aid operationalizes aid in a way that most closely matches the underlying construct we developed earlier in the paper. In addition to varying the threshold for considering aid as concentrated or diffused, we can conceptualize aid concentration in substantively different ways, which implies different operational measures. First, it may be useful to consider the amount of aid in a location scaled by the size of the population to capture some of the concentration of aid per person or family. We standardize aid concentration with respect to population by dividing the aid value by population of administrative units. Second, it may be useful to consider the amount of aid in a location scaled by the areal size of the administrative unit to capture how aid may spread across larger or smaller areal spaces. Practically, we employ the same procedure as with the per capita analysis but for the geographic size of administrative unit. Third, the relevant comparison point may not be other areas at the same point in time but rather the same location at earlier points in time, especially if warring parties do not, in practice, understand how much aid is going to other locations at the same point in time. We calculated the cumulative averages of aid at country level and used these as our thresholds. This also helps us partially control for spatial diffusion among neighboring units given that they are usually located within the same country, except for the units that are neighbors but part of different countries. Finally, warring parties may consider aid to be concentrated in gradations rather than dichotomously concentrated or not. To do so, we created an ordinal variable with three levels as our independent variable. With the exception of the first measure, which is aid scaled by population size, statistical estimates are positive and significant for aid concentration's effect on military fatalities for these different pos-
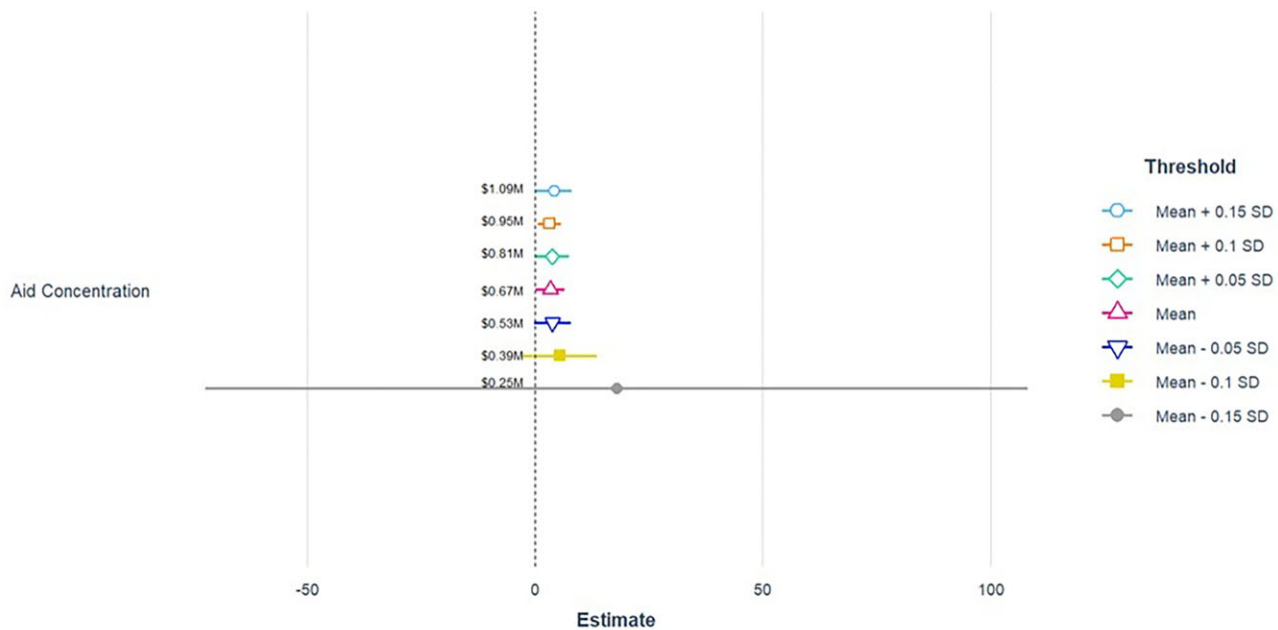
**Figure 7.** Coefficient plot for the instrumental variable models across a range of thresholds.

sible conceptualizations and corresponding operationalizations.[17] These models are presented in detail in online appendix D2.

*Confounding*

To address possible confounding, we first estimate eight fixed effects regression models. These models attempt to deal with potential omitted variables that might affect the inferences we reach. We vary several factors, including year- and country-fixed effects with and without controls, as well as how we cluster the standard errors. In nearly all of these additional models presented in Table 1, we find that the results are consistent with the main results.[18]

Next, we employ a two-stage least squares (2SLS) model with an instrumental variable. Studies have shown that government expenditure as a share of GDP of donor countries determines aid amounts (Brech and Potrafke 2014) and therefore may be a suitable instrument. As such, we instrument our independent variable, aid concentration, with donor countries' government expenditures as a share of their GDPs. To operationalize the instrumental variable, we collapse these shares to the recipient-year level.[19]

The instrument requires donors to be states since there is no plausible equivalent of expenditure as a share of GDP for non-state donors like international organizations. One critical assumption we make here is that donors do not donate aid specifically to provinces. When executing our 2SLS

regression, we exclude fixed effects because the instrument does not have within-country variation and acknowledge the variation that fixed effect models sacrifice in order to control for unobserved confounders (Mummolo and Peterson 2018). Given that we have a theoretically plausible instrument and a large set of covariates, we choose not to give up variation in this trade-off. Also, the number of province-warring parties in our data is 1,033 for a total number of 2,378 observations, which suggests an already small number of temporal points (∼2 years per observation), which further bolsters this decision. We include the following control variables in all of the 2SLS models: *battleground control, multiple oppositions, area size, high population density, agriculture percentage, most petrol locations, most diamond locations,* and *most forested.*

The coefficient plot in figure 7 shows the size and sign of the impact for each instrumental variable model with seven thresholds. The results with all specifications are shown in table D1 in online appendix D. When the threshold lowers, the statistical significance attenuates slightly. There are jumps in the number of provinces with concentrated aid, particularly at the two lowest thresholds. These jumps might be explained by the insensitivity of lower thresholds; they might not as successfully select highly concentrated provinces. Beyond these caveats, most models are robust to different specifications, and they pass standard identification tests.

## Discussion and Conclusion

In this study, we examined the effects of concentrated foreign aid on conflict intensity, positing that concentrated aid to conflict-affected areas increases military fatalities, but not necessarily civilian fatalities. In contrast to most work on aid and conflict, which examines national-level aid flows and conflict dynamics, we articulated a theoretical argument emphasizing that localized aid distribution affects localized patterns of violence. This theoretical approach necessitated the use of sub-national geospatial data on aid and

---

[17]We also estimated the same models with civilian casualties as our dependent variable to test our second hypothesis. Unlike military casualties, the results are not consistently significant for civilian casualties as predicted. The results are presented in more detail in online appendix D4.

[18]Where the results are inconsistent (Model 4), there are neither year-fixed effects nor controls, which is not an ideal specification to deal with potential confounders.

[19]For example, if Angola receives aid from Belgium, Canada, Finland, France, Italy, Liechtenstein, the Netherlands, and Sweden in 1990, then we collapse government expenditure as a share of GDP of these countries in 1990, which yields the instrumented value for Angola. We use donor data from AidData and government expenditures as a share of GDP from the World Bank.

**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 | + | 0.001 |
| 3 | No | Yes | None | Side A | + | 0.012 |
| 4 | No | Yes | None | Country | + | 0.225 |
| 5 | Yes | Yes | None | Side A | + | 0.000 |
| 6 | Yes | Yes | None | Country | + | 0.000 |
| 7 | Yes | Yes | Full | Side A | + | 0.000 |
| 8 | Yes | Yes | Full | Country | + | 0.03 |

Year FE—Year-fixed effects.
Country FE—Country-fixed effects.
DV—Military fatalities (logged).
IV—Lagged aid concentration with mean as a threshold.
Controls—None or full (oil, rainfall, area size, percent of area agricultural, battleground control, capital city, minimum elevation, diamonds, population density, percent forested, average value of aid projects per location).

conflict, which we coded and then aggregated for analysis. As Blattman and Miguel (2010, 24) noted, these types of data are "likely to yield more convincing answers to theoretical questions."

The combination of high-resolution data, analytic strategies to address selection and endogeneity, and contextualization with aid and conflict dynamics in northern Uganda provides credible evidence about our theoretical expectations. Taken together, the evidence provides strong support for the expectation that concentrated aid increases military fatalities (Hypothesis 1) but weak evidence for the expectation that concentrated aid decreases civilian fatalities (Hypothesis 2), likely because of collateral damage on civilians during military contests.

The external validity of our inferences depends on the settings, treatments, outcomes, units, and time (STOUT) across which the mechanisms may apply (Findley et al. 2021). Our study likely has higher external validity than most other work on aid and conflict intensity, which examines aid during US counterinsurgency campaigns primarily (settings), with small military-administered aid programs (treatments), on US targeted violent conflict (outcomes), in Iraq and Afghanistan (units), which are not representative of where most conflict occurs in the world (sub-Saharan Africa and Asia; Pettersson et al. 2019), and military-administered aid aimed at immediate stabilization may play out over very different time periods relative to longer-term development assistance or even immediate humanitarian relief (time). The inferences about concentrated aid's effects on military fatalities most likely transport across STOUT in the following ways: within sub-Saharan Africa after 2008 in the same countries; within sub-Saharan Africa after 2008 in other countries; with similar types of aid after 2008 and perhaps with other forms of humanitarian or governance aid between 1989 and 2008; and to other measures of military and civilian violence although not necessarily to non-violent resistance.

Although our study primarily evaluated how concentrated aid influenced the intensity of violence, future work could examine whether the effect is the same for onset, duration, termination, or recurrence. Civil war studies that attempt to unpack this issue have found that some, but not all, factors play a similar role in all phases (e.g., Bleaney and Dimico 2011). Indeed, some phases of conflict may need separate logics to explain the type and severity of violence (Kalyvas 2006), and we expect that understanding the impact of con-

centrated aid on other conflict phases would be a fruitful direction for future research.

Over the past couple of decades, tremendous optimism about the welfare-improving possibilities of foreign aid has emerged as scholars and practitioners increasingly see the noble efforts of foreign aid donors and even participate in the programs as evaluators. While foreign aid undoubtedly has some positive effects, our argument and findings indicate that any beneficial effects are unlikely to be realized when aid is allocated to conflict-affected areas during ongoing wars. These results corroborate Zürcher's (2017) contention that expecting concentrated aid to improve conflict dynamics may be a fool's errand.

One implication may be that donors should emphasize the allocation of diffuse rather than concentrated aid, which could reduce military, and perhaps civilian, fatalities. Of course, many other considerations need to be weighed in making aid allocation decisions, but we suspect that donors would be wise to exercise caution well beyond what the global agenda on the allocation of aid to fragile and conflict-affected areas has articulated.

## Supplementary data

Supplementary information is available at the *International Studies Quarterly* data archive.

## References

ADDISON, TONY, PHILIPPE LE BILLON, AND S MANSOOB MURSHED. 2002. "Conflict in Africa: The Cost of Peaceful Behaviour." *Journal of African Economics* 11(3): 365–86.

ADDISON, TONY, AND S MANSOOB MURSHED. 2001. "The Fiscal Dimensions of Conflict and Reconstruction." *Discussion Paper No. 2001/49.* World Institute for Development Economics Research.

ANDERSEN, REGINE. 2000. "How Multilateral Development Assistance Triggered the Conflict in Rwanda." *Third World Quarterly* 21(3): 441–56.

ANDERSON, MARY B. 1999. *Do No Harm: How Aid Can Support Peace—Or War.* Boulder, CO: Lynne Rienner Publishers.

ARCAND, JEAN-LOUIS, AND LISA CHAUVET. 2001. "Foreign Aid, Rent-Seeking Behavior, and Civil War." In *Working Paper*

ARONSON, JACOB, MICHAEL FINDLEY, KYOSUKE KIKUTA, AND JAMESIGOE WALSH. 2022. "The Tyranny of Supply: Natural Resources and Rebel Territorial Control in Civil Conflicts." *Working Paper.*

AUTESSERRE, SÉVERINE. 2010. *The Trouble with the Congo: Local Violence and the Failure of International Peacebuilding.* Cambridge: Cambridge University Press.

Azam, Jean-Paul. 1995. "How to Pay for the Peace? A Theoretical Framework with References to African Countries." *Public Choice* 83(1): 173–84.

Beath, Andrew, Fotini Christia, and Ruben Enikolopov. 2017. "Can Development Programs Counter Insurgencies?: Evidence from a Field Experiment in Afghanistan." *Research Paper*, MIT Political Science Department, https://papers.ssrn.com/sol3/papers.cfm?abstract_id=1809677.

Beck, Nathaniel, Kristian Skrede Gleditsch, and Kyle Beardsley. 2006. "Space is More than Geography: Using Spatial Econometrics in the Study of Political Economy." *International Studies Quarterly* 50(1): 27–44.

Berman, Eli, Michael Callen, Joseph H. Felter, and Jacob N Shapiro. 2011a. "Do Working Men Rebel? Insurgency and Unemployment in Afghanistan, Iraq, and The Philippines." *Journal of Conflict Resolution* 55(4): 496–528.

Berman, Eli, Joseph H. Felter, Jacob N. Shapiro, and Erin Troland. 2013. "Modest, Secure, and Informed: Successful Development in Conflict Zones." *American Economic Review* 103(3): 512–7.

Berman, Eli, Jacob N. Shapiro, and Joseph H Felter. 2011b. "Can Hearts and Minds be Bought? The Economics of Counterinsurgency in Iraq." *Journal of Political Economy* 119(4): 766–819.

Blattman, Christopher, and Edward Miguel. 2010. "Civil War." *Journal of Economic Literature* 48(1): 3–57.

Bleaney, Michael, and Arcangelo Dimico. 2011. "How Different Are the Correlates of Onset and Continuation of Civil Wars?" *Journal of Peace Research* 48(2): 145–55.

Brech, Viktor, and Niklas Potrafke. 2014. "Donor Ideology and Types of Foreign Aid." *Journal of Comparative Economics* 42(1): 61–75.

Clauset, Aaron, Maxwell Young, and KristianSkrede Gleditsch. 2007. "On the Frequency of Severe Terrorist Events." *Journal of Conflict Resolution* 51(1): 58–87.

Collier, Paul. 2000. "Rebellion as a Quasi-Criminal Activity." *Journal of Conflict Resolution* 44(6): 839–53.

———. 2007. "The Bottom Billion." *Economic Review* 25(1): 17.

———. 2011. *Wars, Guns and Votes: Democracy in Dangerous Places.* New York, NY: Random House.

Collier, Paul, and Anke Hoeffler. 2002. "Aid, Policy and Peace: Reducing the Risks of Civil Conflict." *Defence and Peace Economics* 13(6): 435–50.

——. 2004a. "Aid, Policy and Growth in Post-Conflict Societies." *European Economic Review* 48(5): 1125–45.

——. 2004b. "Greed and Grievance in Civil War." *Oxford Economic Papers* 56(4): 563–95.

——. 2007. "Unintended Consequences: Does Aid Promote Arms Races?" *Oxford Bulletin of Economics and Statistics* 69(1): 1–27.

Crost, Benjamin, Joseph Felter, and Patrick Johnston. 2014. "Aid Under Fire: Development Projects and Civil Conflict." *American Economic Review* 104(6): 1833–56.

Darden, Jessica Trisko. 2019. Aiding and abetting: US foreign assistance and state violence. Redwood City, California, United States: Stanford University Press.

De Ree, Joppe, and Eleonora Nillesen. 2009. "Aiding Violence or Peace? The Impact of Foreign Aid on the Risk of Civil Conflict in sub-Saharan Africa." *Journal of Development Economics* 88(2): 301–13.

Do, Quy-Toan, and Lakshmi Iyer. 2010. "Geography, Poverty and Conflict in Nepal." *Journal of Peace Research* 47(6): 735–48.

Easterly, William, and Ross Levine. 1997."Africa's Growth Tragedy: Policies and Ethnic Divisions." *The Quarterly Journal of Economics* 11: 1203–50.

Eck, Kristine, Margareta Sollenberg, and Peter Wallensteen. 2004. "One-Sided Violence and Non-State Conflict." In *States in Armed Conflict 2003*, edited by Lotta Harbom, 133–42. Uppsala Publishing House: Uppsala, Sweden.

Findley, Michael G. 2018. "Does Foreign Aid Build Peace?" *Annual Review of Political Science* 21(1): 359–84.

Findley, Michael G., Trey Billing, and David Backer. 2023. "Rebel Sophistication in Aid Targeting: Insights from the Inner Circles." University of Texas at Austin. Unpublished Manuscript.

Findley, Michael G., Kyosuke Kikuta, and Michael Denly. 2021. "External Validity." *Annual Review of Political Science* 24: 365–93.

Findley, Michael G., Josh Powell, Daniel Strandow, and Jeff Tanner. 2011. "The Localized Geography of Foreign Aid: A New Dataset and Application to Violent Armed Conflict." *World Development* 39(11): 1995–2009.

Gelman, Andrew, and Jennifer Hill. 2006. *Data Analysis Using Regression and Multilevel/Hierarchical Models.* Cambridge: Cambridge University Press.

Goertz, Gary. 2017. *Multimethod Research, Causal Mechanisms, and Case Studies: An Integrated Approach.* Princeton, NJ: Princeton University Press.

Grossman, Herschel I. 1992. "Foreign Aid and Insurrection." *Defence Economics* 3(4): 275–88.

Hagmann, Tobias, and Filip Reyntjens, eds. 2016. *Aid and Authoritarianism in Africa: Development without Democracy.* London, UK: Bloomsbury Publishing.

Hattori, Tomohisa. 2001. "Reconceptualizing Foreign Aid." *Review of International Political Economy* 8(4): 633–60.

Kalyvas, Stathis N. 2006. *The Logic of Violence in Civil War.* Cambridge: Cambridge University Press.

———. 2008. "Promises and Pitfalls of an Emerging Research Program: The Microdynamics of Civil War." *Order, Conflict, and Violence* 41(8): 1043.

King, Gary, Christopher Lucas, and Richard A Nielsen. 2017. "The Balance-Sample Size Frontier in Matching Methods for Causal Inference." *American Journal of Political Science* 61(2): 473–89.

Lacina, Bethany, NilsPetter Gleditsch, and Bruce Russett. 2006. "The Declining Risk of Death in Battle." *International Studies Quarterly* 50(3): 673–80.

Langlotz, Sarah, and Niklas Potrafke. 2019. "Does Development Aid Increase Military Expenditure?" *Journal of Comparative Economics* 47(3): 735–57.

Lyall, Jason. 2019. "Civilian Casualties, Humanitarian Aid, and Insurgent Violence in Civil Wars." *International Organization* 73(4): 901–26.

McKinlay, Robert D., and Richard Little. 1977. "A Foreign Policy Model of US Bilateral Aid Allocation." *World Politics* 30(1): 58–86.

McMillan, John. 2004. "Avoid Hubris and Other Lessons for Reformers." *Finance and Development* 41(3): 34–7.

Maren, Michael. 2009. *The Road to Hell.* New York, NY: Simon and Schuster.

Moyo, Dambisa. 2009. *Dead Aid: Why Aid is not Working and How There is a Better Way for Africa.* London: Macmillan.

Mummolo, Jonathan, and Erik Peterson. 2018. "Improving the Interpretation of Fixed Effects Regression Results." *Political Science Research and Methods* 6(4): 829–35.

Narang, Neil. 2015. "Assisting Uncertainty: How Humanitarian Aid Can Inadvertently Prolong Civil War." *International Studies Quarterly* 59(1): 184–95.

Nielsen, Richard A., Michael G. Findley, Zachary S. Davis, Tara Candland, and Daniel L Nielson. 2011. "Foreign Aid Shocks as a Cause of Violent Armed Conflict." *American Journal of Political Science* 55(2): 219–32.

Nunn, Nathan, and Nancy Qian. 2014. "US Food Aid and Civil Conflict." *American Economic Review* 104(6): 1630–66.

OECD. 2006. *Development Co-Operation Report: Efforts and Policies of Members of the Development Assistance Committee.*

Pearl, Judea. 2009. "Myth, Confusion, and Science in Causal Analysis." Unpublished Paper, http://web.cs.ucla.edu/~kaoru/r348.pdf.

Pettersson, Therése, Stina Högbladh, and Magnus Öberg. 2019. "Organized Violence, 1989–2018 and Peace Agreements." *Journal of Peace Research* 56(4): 589–603.

Polman, Linda. 2010. *The Crisis Caravan: What's Wrong with Humanitarian Aid?.* New York, NY: Metropolitan Books.

Rosenbaum, Paul R., and Donald B Rubin. 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika* 70(1): 41–55.

Sabaratnam, Meera. 2017. *Decolonising Intervention: International Statebuilding in Mozambique.* Lanham, MD: Rowman & Littlefield.

Sachs, Jeffrey D. 2006. *The End of Poverty: Economic Possibilities for Our Time.* London, UK: Penguin.

Sexton, Renard. 2016. "Aid as a Tool against Insurgency: Evidence from Contested and Controlled Territory in Afghanistan." *American Political Science Review* 110(4): 731–49.

Sollenberg, Margareta. 2012. "A Scramble for Rents: Foreign Aid and Armed Conflict." *PhD Dissertation*, Department of Peace and Conflict Research. http://uu.diva-portal.org/smash/record.jsf?searchId=1&pid=diva2:512184.

Strandow, Daniel, Michael Findley, Daniel Nielson, and Josh Powell. 2011. *The UCDP and AidData Codebook on Georeferencing Aid: Version 1.1.* Uppsala: Uppsala University.

Sundberg, Ralph, Mathilda Lindgren, and Ausra Padskocimaite. 2011. *UCDP GED Codebook Version 1.0-2011.* Uppsala: Uppsala University.

Sundberg, Ralph, and Erik Melander. 2013. "Introducing the UCDP Georeferenced Event Dataset." *Journal of Peace Research* 50(4): 523–32.

TIERNEY, MICHAEL J., DANIEL L. NIELSON, DARREN G. HAWKINS, J. TIMMONS ROBERTS, MICHAEL G. FINDLEY, RYAN M. POWERS, AND BRADLEY PARKS et al. 2011. "More Dollars than Sense: Refining Our Knowledge of Development Finance Using AidData." *World Development* 39(11): 1891–906.

UVIN, PETER. 1998. *Aiding Violence: The Development Enterprise in Rwanda*. Boulder, CO: Kumarian Press.

VALENTINO, BENJAMIN, PAUL HUTH, AND DYLAN BALCH-LINDSAY. 2004. "Draining the Sea: Mass Killing and Guerrilla Warfare." *International Organization* 58(2): 375–407.

WEAVER, CATHERINE. 2008. *Hypocrisy Trap*. Princeton, NJ: Princeton University Press.

WEBERSIK, CHRISTIAN. 2006. "Mogadishu: An Economy without a State." *Third World Quarterly* 27(8): 1463–80.

WEINTRAUB, MICHAEL. 2016. "Do all Good Things Go Together? Development Assistance and Insurgent Violence in Civil War." *The Journal of Politics* 78(4): 989–1002.

WOOD, REED M., AND EMILY MOLFINO. 2016. "Aiding Victims, Abetting Violence: The Influence of Humanitarian Aid on Violence Patterns during Civil Conflict." *Journal of Global Security Studies* 1(3): 186–203.

WOODS, NGAIRE. 2014. *The Globalizers*. Ithaca, NY: Cornell University Press.

ZÜRCHER, CHRISTOPH. 2017. "What Do We (not) Know about Development Aid and Violence? A Systematic Review." *World Development* 98: 506–22.

ZÜRCHER, CHRISTOPH. 2019. "The Folly of 'Aid for Stabilisation'." *Third World Quarterly* 40(5): 839–54.