Aiding Victims, Abetting Violence: The Influence of Humanitarian Aid on Violence Patterns during Civil Conflict

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Abstract:

Annual allocations of bilateral and multilateral humanitarian assistance to conflict-affected states total billions of dollars. Humanitarian assistance plays a vital role in sustaining vulnerable populations. However, inflows of such aid may also exacerbate violence by both threatening insurgents and creating incentives for these groups to extend or deepen control over the areas in which it concentrates. Insurgent efforts to ameliorate threat and coopt resources ultimately raise the risk of conflict between insurgent and counterinsurgent forces. We evaluate our argument using recently constructed geo-located data on both aid commitments and conflict events for a sample of twenty sub-Saharan African countries during the post-Cold War era. Even after accounting for the non-random assignment of aid within conflict zones, we find that humanitarian aid increases the frequency of subsequent violent engagements between rebel and government forces in the areas in which it concentrates. Importantly, however, we find no evidence that other forms of foreign development aid exacerbate or prolong violence in the areas in which they are allocated.

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Since the end of the Cold War, donor states' humanitarian aid commitments have totaled nearly $160 billion dollars.[2] Moreover, in recent years, bilateral and multilateral humanitarian assistance has typically surpassed $10 billion annually (GHA 2013, 19).[3] Much of this aid is directed to states hosting refugees displaced by conflicts in their home countries. However, a substantial portion of overall humanitarian assistance goes to support the immediate needs of internally displaced persons (IDPs) and other civilians adversely impacted by conflict violence. Indeed, during the previous decade, more than half of all international humanitarian assistance was explicitly devoted to states involved in violent internal conflict (GHA 2013, 41, 79).[4] Advocates of humanitarian assistance argue that such aid is critical to sustaining populations aversely impact by war. In addition, governments have increasingly viewed development and humanitarian aid programs as important tools for countering violent extremism and combatting and containing insurgent threats (e.g., Fishstein 2010; USAID 2011). Yet, some scholars, policymakers, and activists contend that humanitarian aid may contribute to conditions of moral hazard, inadvertently prolong or exacerbate violence, or create incentives for predation by armed actors, thereby increasing the risk to civilian populations (e.g., Cooley and Ron 2002; Hoffman 2004; Willamson 2011).

Despite the billions of dollars of annual aid flows into conflict zones, relatively few existing studies have attempted to systematically examine the impact of humanitarian aid on subsequent patterns of violence within recipient states. Careful evaluation of its impact within

[2] This figure is computed from information in the AidData2.0 dataset (Tierney et al. 2011). It excludes aid given to international organizations or other non-state actors.
[3] The majority of humanitarian assistance comes from member states of the OECD Development Assistance Committee (DAC).
[4] This figure is consistent with aid commitments reported in the AidData2.0. According to that dataset, roughly 54% of humanitarian aid was devoted to states that experienced armed conflicts within their borders. This figure excludes humanitarian aid to states that hosted refugees from external armed conflicts but did not experience such violence within their own borders.

conflict zones is therefore important given the number of deaths and casualties produced by violent domestic conflicts and the large volumes of humanitarian assistance dollars that often flow into countries involved in or at risk for such conflicts. Consequently, in this manuscript we address the following question: Does humanitarian aid exacerbate or ameliorate violence in the areas in which it is allocated?

In addressing this question, we first examine the existing empirical literature on the relationship between and foreign aid and conflict. In general, we find that this literature produces a mixed picture regarding the overall impact of foreign development and humanitarian assistance on violence. Inconsistencies in these findings stem in part from important differences in the types aid evaluated in various studies, different levels of data aggregation (e.g., subnational versus state-level), differences in study scope (e.g., all recipients versus active conflict cases), or idiosyncrasies in the specific cases analyzed (in single-case studies). On balance, however, existing literature suggests that humanitarian aid may be most susceptible to producing unintended negative externalities. We therefore focus our theory and analysis explicitly on the comparative impact of humanitarian aid on subnational violence patterns relative to other forms of foreign aid. We argue that large inflows of humanitarian assistance may exacerbate violence by creating incentives for armed groups to extend or deepen control over the areas in which it concentrates. Ultimately, armed groups' efforts to acquire or maintain access to these valuable resources, coupled with their adversaries' (e.g., counterinsurgent forces) efforts to deny that access, contribute to an increase in violent confrontations between competing groups.

A central advantage of our analysis is that we rely on recently constructed geo-located data on both aid commitments and conflict events available for a sample of twenty sub-Saharan African countries to evaluate our argument. These data allows us to examine the influence of

different types of aid at the subnational level. Because we expect that recent violence patterns influence aid allocation decisions, we adopt a quasi-experimental empirical approach that allows us to more appropriately account for the non-random assignment of aid within conflict states as well as underlying temporal trends in violence that may bias our results. The results of our analyses provide support for our primary argument: the presence of humanitarian aid positively correlates with more frequent violent engagements between rebel and government. More specifically, results from a matched-sample difference-in-differences analysis suggests that violence declines more rapidly in areas that receive humanitarian aid resources compared to those that did not receive such resources. In other words, the presence of humanitarian aid resources appears to prolong violence in the areas in which it is dispersed. However, we find no evidence that the allocation of non-humanitarian forms of foreign aid influences subsequent violence patterns.

Helping or Harming?

Humanitarian aid helps sustain millions of civilians each year (GHA 2013). Yet, a wealth of anecdotal evidence and numerous qualitative studies have noted the potential negative externalities associated with the allocation of humanitarian aid in conflict zones (e.g., Anderson 1999; Cooley and Ron 2002; de Walle 2014; Fast 2010; Lischer 2005). These studies often highlight the role of refugee camps and the aid resources devoted to them in providing camouflage and protection to extremists and serving as a source of recruits and other resources for armed groups, thereby contributing to the perpetuation of violence or placing local civilians at risk. The potential for humanitarian aid to generate such unanticipated consequences should

not overshadow the benefits that aid provides to vulnerable populations, but it nonetheless deserves close scrutiny.

While numerous previous studies have noted the potential for aid to exacerbate violence, comparatively few studies have subjected this potential relationship to systematic empirical scrutiny. Fewer still have addressed this relationship at the subnational level or compared the impact of different types of aid on subsequent violence. Recent cross-national studies find general support for the argument that foreign aid can effectively suppress violence in recipient states. Young and Findley (2011), for example, find that foreign devoted to education, civil society, health, and conflict prevention reduce the risk of terrorism within recipient states. De Ree and Nillesen (2009) similarly find that development aid shortens the duration of civil conflicts in Sub-Saharan African states but has no impact on conflict onset.

In addition to these state-level analyses, a few country-specific studies have also noted the potential ameliorative effects of development, reconstruction, and humanitarian aid. One recent analysis found that both security and support for the Afghan government improved among civilians in villages that received higher levels of aid resources through the National Solidary Program (NSP), a nationwide program that distributes block grants to communities for specific development projects (Beath, Christia and Enikolopov 2012). Importantly, however, the observed positive impact of aid on security occurred only in areas that had experienced low levels of violence at the onset of the program, suggesting that may aid help prevent violence but is less likely to reduce violence where it is already severe. An analysis of conditional cash transfer programs—at type of aid—in the Philippines found that this program reduced violence by increasing civilian support of the government and reducing rebel influence in the areas in which it was implemented (Crost, Felter, and Johnson 2016). Similarly, Berman, Shapiro and

Felter (2011) find that insurgent violence declined in Iraqi neighborhoods that received higher levels of specific types of reconstruction aid. Specifically, projects funded through the Commander's Emergency Reconstruction Program (CERP) reduced insurgent violence by promoting the information gathering capability of counterinsurgent forces. However, they find no evidence that other types of reconstruction aid ameliorate violence.

By contrast, other studies find that aid exacerbates violence in some contexts. Recent cross-national studies find that foreign food aid extends the duration of civil conflict in recipient states and that inflows of humanitarian assistance can inadvertently prolong civil wars in recipient states (Nunn and Qian 2014; Narang 2014). Sub-national analyses likewise suggest that some types of aid increase the likelihood of violence in the areas in which they are distributed. For example, Wood and Sullivan (2015) find that humanitarian aid commitments to conflict zones in sub-Saharan African appear to increase the risk of rebel violence against local civilians. Similarly, a recent study of development aid programs in Mindanao concluded that aid promoted insurgent violence in the communities to which it was allocated because it threatened rebel control in those areas (Crost, Felter and Johnston 2014). Weintraub's (2016) analysis of cash-transfer programs in Colombia likewise concludes that cash transfers exacerbated rebel violence because they represent direct threats to rebel authority. Interestingly, his findings directly contradict Crost, Felter, and Johnson's (2014) similar study of the Philippines discussed above.

These disparate conclusions call for greater scrutiny into the causal mechanisms through which aid may influence violence dynamics. Variations in the types of aid assessed across the aforementioned analyses as well as differences in scope conditions may partly explain this lack of clarity. Different types of aid are likely to present different challenges and opportunities to armed actors and may therefore exert different—perhaps opposing—influences on their

behaviors. Moreover, aid flows to countries *at risk* of conflict may produce different effects than aid directed to countries actively *engaged* in armed conflict. In this analysis, we therefore focus our attention explicitly on the impact of humanitarian aid flows on violence dynamics in countries already involved in civil conflict. We build upon previous studies by developing an argument that specifically links humanitarian aid inflows with violent contestation between rebel and government forces over territory. As we discuss in greater detail below, we link the characteristics of humanitarian aid—particularly the ease with which armed groups can expropriated it relative to other forms of aid and the potential benefits it provides—to armed groups' incentives to exert control over the areas in which it accumulates. By augmenting the value of controlling a given territory, larger inflows of humanitarian aid perpetuate violent conflict between state and rebel forces.

Aid Distribution within Conflict Zones

Considering both the motives behind the allocation of aid to specific areas as well as the political and logistical factors that influence its distribution on the ground represents an important first step in assessing aid's potential impact on violence patterns. First, humanitarian aid resources are not distributed randomly within a conflict state. To the extent that humanitarian aid serves as a component of larger conflict alleviation and harm reduction strategies, it should accrue to areas that are either at-risk for or have recently experienced violence and instability and areas with high concentrations of displaced persons. Armed groups are also aware of this relationship and may use it to their advantage. For example, rebels in Sierra Leone and Liberia intentionally escalated violence in some areas in order to provoke inflows of international humanitarian aid, which they then expropriated (Hoffman 2004). Decisions regarding humanitarian aid allocation

are therefore likely to be linked to recent patterns of violence and population displacement. We revisit this discussion when we present our research design because of the empirical challenges created by the non-random assignment of aid resources within conflict zones.

Second, political instability and violence often influence the logistics of humanitarian aid distribution within recipient states. Unlike other forms of foreign aid, which typically flow from donors directly to recipient state governments, humanitarian aid projects are often managed by third party aid organizations and NGOs (Addison, Le Billon and Murshed 2002, 382-383). Donors are particularly likely to adopt this approach when they perceive the government as corrupt, unreliable, or incapable of effectively delivering resources to needy areas (Collier and Hoeffler 2002, 437; Ofstad 2002). However, relying on third parties to provide humanitarian assistance often introduces new principle-agent problems that can hamper the ability of donors to exert control over aid resources once they are allocated to agents.

The patterns of relationships that develop between aid providers and militants illustrate the disconnect between principles' interests and agents' actions. In order to maintain operations in unstable areas and to protect their staff, aid providers often develop complex relationships with militants (Anderson 1999; Lischer 2005). Rebels and aid providers frequently negotiate over the scope, location, and distribution of aid projects (e.g., Anderson 1999; Ofstad 2002). In Afghanistan, for example, many aid agencies have held "talks" with rebels, and some have reportedly registered with and paid taxes to the Taliban in order to conduct aid operations in the areas the group control (Jackson and Giustozzi 2012, 6, 15). Similarly, while aid agencies operating in Somalia faced intense political (and legal) pressures from donors to avoid cooperating with Al-Shabab, many engaged in clandestine negotiations with the rebels in order to continue providing services to needy civilians (Tran 2013). These activities run counter to the

goals of donor states (as well as the government) and reflect a core principle-agent problem common to unstable conflict environments. Such principle-agent problems hamper donor states' ability to control where aid flows within conflict zones and which local groups benefit from it (e.g., Cooley and Ron 2002). Moreover, as these examples suggest, the inability of donors to directly oversee aid distribution may create opportunities for armed groups to gain access to valuable pools of resources despite donor objections.

This discussion highlights two important aspects of aid provision in conflict zones. First, aid is already more likely to flow to volatile and violent areas. Second, international NGOs and aid agencies often supplement or replace donor or recipient governments as the primary agents of humanitarian assistance distribution within conflict zones, thereby reducing the ability of donors to oversee and control aid distribution practices. Both observations are relevant to analyses of aid's impact on violence patterns because they highlight the manner in which humanitarian aid is explicitly connected to ongoing conflict processes.

Humanitarian Aid and Sub-national Violence Patterns

In this section we develop an argument that connects humanitarian assistance to subnational patterns of violent contestation between rebel and government forces. In line with previous studies, we assume that civil conflicts are typically asymmetrical and that rebels generally possess fewer resources, control less (and often no) territory, and are militarily much weaker than the governments they challenge. As such, we orient our argument toward the role of humanitarian aid in shaping rebel incentives to contest government control over a given territory. In other words, we assume that rebels, as the weaker actor, must garner substantial resources and wrest territory away from government control in order to achieve their ultimate political

objectives. By contrast, the central goal of the government is to prevent the rebellion from expanding and posing a serious threat to the regime. We therefore consider the role of humanitarian aid in shaping rebel decisions about which areas they attempt to control and why. [5]

We highlight two inter-related mechanisms through which aid promotes conflict violence. First, we argue that aid projects increase competition for civilian loyalty and represent a potential threat to rebel authority, leading rebels to attempt to subordinate these competitors. Second, aid resources provide both material and non-material benefits that augment rebel capabilities and sustain the organization. The presence of these resources encourages rebel groups to engage in predation, rent seeking, and the cooptation of aid agency activities as a means of securing both short- and longer-term benefits. Both competition from aid suppliers and desire to coopt resources increase rebel incentives to challenge the government for control over territory in which aid accumulates, thus leading to an increased risk of violence.

Competition and Challenge

During periods of large-scale instability and violence, donor states, international organizations, and recipient governments often see development and humanitarian aid as tools for reducing support for militants and bolstering regime capacity. The ultimate goal of this strategy is suppression of insurgent activity through the provision of public goods (e.g., Fishstein 2010; USAID 2011). Yet, even where such strategies succeed in diminishing support for rebels, they necessarily present a direct challenge to rebel authority and control. The successful application of

[5] While our argument is oriented toward rebel incentives to assert control over territory, we acknowledge that the same logic may apply to government decisions in some contexts. Incumbents are likely to view aid programs that operate without their consent and outside their zones of control as potential threats to their authority and as potential sources of benefits for rebels. Their forces are therefore likely to attempt to exert control over those areas.

these strategies may therefore produce short-term spikes in violence. Moreover, where rebels view aid providers as threats to their authority, these groups may become targets of violence (Fast 2010; Murdie and Stapley 2014). This outcome is most likely where aid projects and government counterinsurgency activities directly overlap and where local insurgents are unable to effectively differentiate between them (Jackson and Giustozzi 2012; Williamson 2011). Even when rebels are able to distinguish between aid workers and government forces, they may still view aid providers as competitors. In this situation, rebels are likely to either attempt to expel the interlopers or subordinate them in order to gain access to their resources.

In some cases, rebels have used violence against aid providers whose activities were perceived as threats to their control or authority. In Somalia, for example, Al-Shabab banned foreign NGOs and aid agencies from operating in the areas they controlled because the militant group viewed their activities as contrary to its interests (Ahmed and Migrio 2011). Yet, driving out humanitarian aid projects may prove costly, both because it can delegitimize rebels in the eyes of the international community and because it often imposes additional hardships on the local population. In this sense, rebels choose between coexisting with competitors and suffering the reputational costs associated with expelling them. Given these potential costs, rebels may attempt to subordinate or exert nominal influence over the activities of these competitors rather than drive them out. Al-Shabaab eventually adopted this strategy. Facing growing international condemnation and worsening famine, the group reversed its ban on foreign aid organizations. However, in order to manage the perceived threat aid groups presented, the rebels developed a sophisticated apparatus to monitor and influence the activities of aid organizations, including levying taxes of up to $10,000 on organizations conducting work in areas under its control (Tran 2013). Taliban forces in Afghanistan have similarly manipulated and attempted to control aid

projects where they exercise power in order benefit from the resources that the group provides (Jackson and Giustozzi 2012). Thus, where rebels are able to establish control or influence over aid groups, they are able to both effectively mitigate the threats the group provides as well as extract valuable resources from them.

Expropriation, Cooptation and Control

Successful rebellion—as well as basic group survival—depends on both continual inflows of resources and the ability to exert nominal control over some territory in which to organize these resources. The pursuit of these resources brings rebels into direct confrontation with state forces. For instance, numerous previous studies link the presence of lucrative resources to the locations of conflict events and motives for rebel violence (e.g., Le Billon 2001; Lujala 2010). The logic underlying these findings is that rebels benefit militarily or economically from capturing or exploiting conflict resources. Profits derived from these resources allow the group to purchase equipment, maintain the allegiance of existing troops, and recruit new troops, thereby expanding their operations. The presence of these resources therefore increases rebels' estimation of the value of controlling or maintaining access to a given territory and thus justifies some of the risk of engaging regime in these areas.

Similar to other conflict resources, rebels desire to capture humanitarian assistance or influence its distribution because it represents a source of both wealth and political power (Anderson 1999, 38; Grossman 1992). However, unlike conflict resources such as gems or drugs, humanitarian aid resources are often readily consumable and do not require labor-intensive extraction, processing, or access to large markets to provide benefits to rebels. For instance, commandeered food aid feeds hungry soldiers and civilian supporters and medical facilities heal

wounded troops, both of which benefit rebels in their war against the government. These features also make humanitarian aid more valuable to rebels than other forms of development assistance. Physical infrastructure projects such as school, irrigation systems, or roads are unlikely to create the same incentives for predation because these projects are more difficult to directly translate into benefits for the rebels. The presence of easily exploitable humanitarian aid resources therefore creates incentives for predatory looting that other resources do not.

Predation is a common strategy of rebel resource acquisition, and rebels often engage in looting as a means to compensate for short-term resource constraints (e.g., Weinstein 2007). For example, rebel factions looted food aid, medical supplies, communications equipment, and vehicles from aid organizations operating in Monrovia during the Liberian Civil War—all told, some $20 million worth of aid may came under the control of armed factions (Anderson 1999; Bryer and Cairns 1997). De Walle (2014, 352) asserts that the SPLM/A elevated looting to the status of "military strategy" in 1990s. Similarly, Tuareg and Islamist rebels have captured food and medical supplies from aid providers working in Mali (Irin 2012). Such predatory behavior is often transient and focused on the immediate acquisition of material goods that rebels can easily consume or divert directly to their war effort.

As an alternative to short-term predation and looting, rebels may attempt to co-opt or exert some (nominal) level of control over aid sites and accompanying resources in order to further their longer-term strategic interests. Whereas looting provides rebels with an immediate (but temporary) infusion of resource, the ability to control aid project sites or influence local aid distribution potentially provides a substantial source of recurring rents through which rebels can fund their operations and augment their capabilities. Unlike looting strategies, cooptation requires rebels to bargain with aid providers and local civilians, resulting in the formation of

complex, iterative relationships among these actors. In Afghanistan, for example, Taliban leaders closely scrutinized the actions of aid agencies and determined which groups were allowed to conduct operations in the areas they controlled—a position that allowed them to divert resources to their local allies (Jackson and Giustozzi 2012). As the example of Al-Shabaab noted above highlighted, rebels also bargain with aid providers over the terms of their operations, including negotiating fees and taxes on their operations. Similarly, in the Sudan, the SPLM/A siphoned off significant amounts of aid through taxes imposed on NGOs and international development agencies (Branch and Mampilly 2005). Taliban rebels reportedly implemented a tiered system in some provinces in which "authentic" humanitarian aid was exempted but other NGO activities were taxed at a rate of 10%, and private company projects at 20% (Jackson and Giustozzi 2012, 15). The imposition of such taxes—paid in either cash or goods—can serve as a significant source of recurring resources for rebels, allowing them to pay troops, recruit supporters, and replace depleted resources.

In addition to providing pecuniary and material benefits, the successful cooptation of aid activities can also help rebels establish legitimacy and influence the loyalty of the local population (Anderson 1999; Mampilly 2011). The SPLM/A in Sudan and the LTTE in Sri Lanka successfully incorporated the activities of aid agencies into their local governance structures, providing them influence over the agencies' activities (Mampilly 2011; Metelits 2009). Similarly, the Eritrean People's Liberation Front (EPLF) and Tigrayan People's Liberation Front (TPLF) in Ethiopia successfully used their influence over aid to bolster both domestic and international support (Duffield and Pendergast 1994; Reno 2011, 148-153). More recently, in some areas of Afghanistan, the Taliban have required aid organizations to provide food, medical aid, and other services to their members and have reportedly pressured aid groups to hire Taliban

members onto their local staff (Jackson and Giustozzi 2012, 15). Rebels in Rwanda and Cambodia likewise successfully influenced the distribution of aid and provision of relief services where they maintained a strong presence, thereby ensuring that their supporters maintained access to these valuable resources (Leriche 2004; Lischer 2005). This strategy directly benefits rebels by promoting loyalty among troops as well as civilian supporters.

Contestation and Conflict

The above discussion highlights the material and non-material benefits that successful control and cooptation of aid sites can provide to resource hungry rebels. This observation has two important implications related to patterns of violence within a conflict state. First, rebel activity is likely to increase in the areas in which aid resources concentrate. Where rebels have already established themselves, the introduction of aid resources creates incentives for the group to deepen its control over the territory. Similarly, the possibility of access to new aid resource encourages rebels to expand into new areas.

A second implication of the above observation is that the pursuit of these resources increases the odds that rebel and government forces engage in violent conflict. In this sense, aid encourages both rebel and government violence in the area in which it is distributed. This observation represents a non-trivial component of our argument. While the arguments elucidated above focus primarily on the manner in which aid incentivizes rebel attempts to exert control over given areas, it is important to note that governments retain a keen interest in preventing the expansion of rebel forces, particularly into areas that contain valuable and easily exploitable resources. Even if the government does not engage in predation at similar rates to rebels, the knowledge that rebels can convert local resources into fuel for the rebellion compels the state to

attempt to deter rebel efforts to exert control over those resources. As a result, counterinsurgent forces are likely to forcibly respond to rebel efforts to extend or deepen control over a given territory, contributing to an overall escalation in the observed level of violence between the two forces within the area in which aid accumulates.

Taken together, the above arguments suggest that by increasing the value of a given territory, aid resources create incentives for rebels to contest government control over the areas in which they accrue and for governments to respond to respond to these efforts with force. Consequently, we expect that greater concentrations of easily exploitable resources in a given geographic area increase the frequency of violent interactions between rebel and government forces. This produces our central hypothesis: *The rate of violent confrontations between rebel and government forces in a geographic area increases in response to the distribution of humanitarian assistance.*

Data

In order to evaluate the hypothesis posited above, we rely on subnational data on both the locations of conflict violence and humanitarian aid projects during periods of active civil conflict. Data on conflict events comes from the Uppsala Conflict Data Program's (UCDP) Georeferenced Event Dataset (GED) (v1.5) (Melander and Sundberg 2013). The GED includes information on post-Cold War conflict events in Africa.[6] These events are based on information extracted from a variety of national and international media sources, human rights reports, and

[6] Sub-Saharan Africa represents a useful environment in which to examine the relationship posited above. Since the end of the Cold War, African states are both common sites of civil conflict and are consistently among the most aid dependent states in the international system.

documents from national and international governmental organizations.[7] Each event in the dataset is connected to a specific geographic location represented by longitude and latitude coordinates, though (as we discuss below) we aggregate these point data to subnational administrative units for the purpose of our analysis.

Data on the locations of humanitarian assistance is extracted from the recently constructed UCDP/AidData geo-referenced dataset (Findley et al. 2011; Strandow et al. 2012). These data originate from AidData 2.0 (Tierney et al. 2011) but have been subsequently spatially disaggregated for a sample of states in post-Cold War sub-Saharan Africa. This dataset captures annual project-level bilateral and multilateral aid commitments (in constant $US) for the years in which a state was involved in civil conflict as well as information on the type of aid project and the estimated location—given in latitude and longitude—of the project.

For all of our analyses, we rely on subnational first order administrative units (i.e., states, districts, communes, departments, etc.) as the unit of spatial aggregation. We adopt this level of analysis for multiple reasons. First, and most important, the dataset from which we take our information on humanitarian aid is arguably best mapped to this level of spatial aggregation. While many projects in this dataset are mapped to individual municipalities or other precise geographic locations, the most detailed location data available for most projects is the first order administrative unit (~70%).[8] Second, finer levels of spatial aggregation typically require more precise information regarding the locations of events. Consequently, the risk of error increases as

[7] Reporting bias is a common concern where data is based on media reports. We attempt to mitigate its impact in a several ways. First, by constraining the sample to African states, we examine less heterogeneous units. Second, we rely on events counts rather than estimates of casualties. Third, we control for several factors that reasonably account for variation in reporting such as population size, distance to the capital, etc.

[8] The UCDP/AidData dataset provides estimated geographic coordinates for all projects. However, where more detailed information is unavailable, the coordinates provided reflect the geographic center (centroid) of the first order administrative unit.

the level of disaggregation increases. Indeed, one recent study of the accuracy of event-level data collected from media sources suggests that such data are highly reliable down to the district level but less reliable at lower levels of aggregation (Weidmann 2014). Finally, political and administrative boundaries often reflect meaningful political, social, or geographic information that is overlooked in arbitrary grids structures or other formats. Relying on these units therefore preserves some of these important elements.

The temporal domain of our study is limited to episodes of civil conflict in sub-Saharan African States between 1990 and 2008. Our sample is constrained in this way primarily because the datasets from which we draw our information are limited to these cases. However, this sample poses no significant problems for our analysis given that our intention is to assess how aid shapes the dynamics of violence rather than investigate its role in conflict initiation or termination. Aggregating the data in the manner described above—and accounting for missing values in some of the covariates—produces a sample containing approximately 3,700 subnational unit years.

We use information contained in the GED to create our dependent variable as well as several relevant control variables. Because our argument focuses explicitly on the manner in which aid inflows shape rebels' willingness to violently contest control over specific geographic areas, our dependent variable captures violent interactions between rebel and government forces. Specifically, the variable *Battles* reflects the total number of discrete battlefield engagements between rebel and government forces observed within a given district during a calendar year as reported in the GED. The GED includes information on a range of different types of conflict events; however, because our hypothesis focuses specifically on rebel-government contests (as

opposed to attacks on targets) we filter our data to remove these types of events.[9] However, in the empirical models we control for recent intentional government and rebel attacks on civilian targets because both likely correlate with battlefield violence. For example, terrorism perpetrated by rebel forces may result in reprisal attacks by government forces and vice verse. The variable *One-sided Violence* therefore reflects the annual count of government and rebel one-sided violence events within a subnational unit.

Information on humanitarian aid is taken from the UCDP/AIDData geo-referenced dataset discussed above. We identify aid commitments to projects designated as "humanitarian" by their CRS codes (project purpose codes).[10] These projects provide food, shelter, water, sanitation, and health services as well as reconstruction and logistics and other short-term activities designed to promote the return and protection of civilians displaced by disasters and conflict. We remove aid allocated to all other sectors and use it to construct a control variable that we discuss in more detail below. We do this because our argument focuses primarily on the influence of easily exploitable resources rather than the impact of large-scale development projects, which are more difficult for rebels to exploit or expropriate.

We create three variables that capture the presence and concentration of humanitarian assistance within a subnational unit. As we argued above, areas with comparatively higher amounts of aid should create greater incentives for violence. While the total dollar amount of aid may be relevant to rebel decision-making, local differences in aid distribution are likely of greater strategic importance. Using a simple estimate of annual aid flows would not accurately capture the relative value of an area but rather would compare local aid levels to a global mean.

[9] This operationalization is consistent with other recent analyses (Berman, Shapiro and Felter 2011). The results are similar if we include battles between militias and rebel factions as well.
[10] We include projects with CRS codes between 70000 and 73010. More detailed descriptions are available at: http://www.oecd.org/dac/stats/purposecodessectorclassification.htm

For our purposes, it makes more sense to account for the relative distribution of aid within a defined geographic area. Our first variable, *Humanitarian Aid Concentration*, therefore represents the proportion of the humanitarian aid directed to a state that was allocated to the unit under observation[11]. It is constructed by dividing the dollar value of aid allocated to a given unit by the total amount of aid received by all subnational units within the same state. The variable is therefore bounded between 0 and 1. Our second variable accounts for both concentration and total value. *Weighted Humanitarian Aid* reflects the natural log of the total dollar value of all humanitarian aid allocated to a subnational unit during a given year weighted by the concentration variable discussed previously. Finally, because some of our models rely on quasi-experimental methods intended to evaluate the impact of a binary treatment variable on an observed outcome, we also construct the variable *Humanitarian Aid*. This dichotomous variable accounts for whether or not a unit received any level humanitarian aid during the year.

One important limitation of the dataset is worth noting before concluding the discussion of the aid data. Within the original dataset, some aid projects are coded as flowing directly to the central government (as opposed to specific areas within the state), and some project locations are simply coded as unknown. We exclude these cases because it is simply not possible to connect these projects to specific locations within the conflict state. In spite of this limitation, these data are the best available for analyzing the spatial distribution of aid within conflict states.

--Figure 1--

Figure 1 demonstrates the spatial distributions of humanitarian aid and government-rebel battles across the subnational units included in our sample. The total number of battles is shown in the left panel, while the right shows the value of humanitarian aid that accumulated to each

[11] Substituting the natural log of the dollar value of aid produces similar results.

unit in the sample. Darker shading reflects greater intensity of the specified variable. As the maps highlight, there is significant spatial variation in the distributions of both aid and violence across the units included in the sample. They also suggest some overlap between aid accumulation and conflict intensity.

We also account for a number of additional confounding variables in our empirical models. First, we create the variables *Non-humanitarian Aid Concentration* and *Weighted Non-humanitarian Aid* to account for the allocation of other forms of foreign aid in the area. We create these variables using the same data and in the same manner as the humanitarian aid variables discussed above. However, we include all aid that is not designated as humanitarian in nature according to its CRS code. These variables allow us to directly compare the impact of humanitarian assistance relative to other types of aid resources that flow into a given area. While we anticipate that humanitarian aid exerts a positive effect on subsequent violence levels, we are agnostic about the influence of non-humanitarian forms of aid. However, our argument implies that humanitarian may exacerbate violence by virtue of the ease with which it can be exploited or appropriated. To the extent that other forms of aid lack this characteristic, they may have little influence on the dynamics of violence in the areas in which they are allocated.

We include the estimated *Population* size within a subnational unit (CIESIN 2005). Population values are measured at five-year intervals, and we linearly interpolate between data points and log-transform the values. We also include the variable *Capital Distance*, which reflects the distance between the geometric center of the district (centroid) and the capital in logged kilometers. Similarly, we include a binary variable accounting for whether or not the district was located on an *International Border*. We also include a variable reflecting the *Area* of

the subnational unit measured in square kilometers. We include this measure because the size of subnational units varies significantly, and large units may offer more opportunities for violence.

Because the ethnic composition of a geographic area may also influence local conflict dynamics (e.g., Fjelde and Hultman 2014; Raleigh 2014), we include the dummy variable *Rebel Constituency*. This variable is coded 1 if the armed group challenging the incumbent regime extensively recruited among the dominant ethnic minority group that inhabited the subnational unit under observation. Information for this variable comes from the Ethnic Power Relations dataset (GeoEPR) and the ACD2EPR (Wucherpfenning et al. 2012). Previous studies also suggest that internal group fragmentation and competition among rival rebel factions shape broader conflict dynamics, including the severity of violence and its targets (e.g., Cunningham, Bakke and Seymour 2012; Wood and Kathman 2015). The presence of easily exploitable resources may also contribute to fragmentation and competition among rebel factions, leading to an escalation of violence. We therefore include the variable *Total Rebel Groups*, which reflects the total number of organized rebel groups active within the conflict state during a given year. Data for this variable is taken from the UCDP Dyadic Dataset (Harbom, Melander and Wallensteen 2008).

As we noted above, previous studies draw a connection between the presence of lootable resources and conflict violence. Because easily extractable diamond reserves have been particularly closely connected with violence, we include a variable indicating the presence of *Diamonds* in the unit (Gilmore et al. 2005). We also account for excessive rainfall. We include this variable because some studies have linked extreme weather patterns to political violence (e.g., Hendrix and Salehyan 2012) and because we believe severe weather is more likely to lead to inflows of aid. *Flooding* represents the estimated area (in log transformed square kilometers)

within a given district that received precipitation greater than two standard deviations above its ten-year average during the year of observation. We construct this variable from data available from the National Oceania and Atmospheric Administration (NOAA) (2011).

Finally, violence clusters both spatially and temporally. This occurs because spatially proximate units often possess similar underlying conditions that are likely to produce violence. Similarly, conflict events often experience spatial diffusion. To address these issues we include the variable *Neighbor Battles*, which captures the proportion of neighboring units that experienced battles during the year. Where appropriate, we also include a one-year lag of the dependent variable to account for temporal dependence.

Empirical Approaches and Results

We rely on multiple statistical approaches to evaluate the hypothesis proposed above. As we discussed in some detail above, the locations of aid projects within conflict states are likely related to recent conflict events. Consequently, project locations and violence patterns may not be independent of one another, presenting a challenge to accurately identifying a causal relationship between the two. We utilize conventional regression techniques as a first cut at evaluating our hypothesis. We then turn to a quasi-experimental research design—specifically a pre-matched difference-in-difference approach—through which we can more directly address the potential bias generated by the non-random assignment of aid to specific unit.

Our data are organized in a panel structure, which allows us to evaluate change in the dependent variable within specific units (e.g., districts) over time. In addition, the outcome variable in all analyses is the count of *Battles* observed during the year, which demonstrates an abundance of zeroes (~75% of observation). These features of the data require balancing among

methodological concerns. For the first set of models we select zero-inflated Poisson (ZIP) equations because this class of models addresses concerns related to the functional from of the data.[12] The ZIP model estimates the relationship between covariates and the dependent variable in two stages. First, the inflation step employs logistic regression to predict whether a given observation belongs to a population of "true zeroes". Thus, in the inflation model, a positive coefficient indicates that the variable predicts the absence of battles (i.e., always zero). The second step models the events count according to a Poisson distribution. Next, in order to address problems of unobserved heterogeneity among units, potential omitted variables bias, and to more appropriately account for over-time variations within panels, we employ a set of Poisson models that include unit fixed effects.[13] Both the ZIP and fixed effects Poisson models include one-year lags of the aid variables to minimize the possibility that current levels of violence influence aid allocation, which would bias our results. However, in the models that more appropriately account for selection effects, we examine the impact of contemporaneous aid.

--Table 1--

According to the result presented in Table 1, recent inflows of humanitarian are related to an increase in battlefield confrontation between rebel and government forces. In Model 1, the coefficient on *Humanitarian Aid Concentration* is positive and achieves statistical significance. Similarly, in Model 2 the coefficient on *Weighted Humanitarian Aid* is both positive and

[12] We include the control variables in both the count and inflation stages of the model. However, we only include the aid variables in the count stage. The results are the same if we include the aid variables in the first stage.

[13] Our dependent variable also exhibits over-dispersion, which suggests that negative binomial models may be more appropriate. We elect to use Poisson models primarily because the conditional fixed effects model available in most statistical packages is not a "true" fixed effects model and fails to adequately "condition out the fixed effects" (Allison and Waterman 2002). However, the results are robust to standard pooled or fixed effects negative binomial models.

significant. The results of the Poisson fixed effects models, which are presented in Models 3 and 4, return similar results. In contrast to the humanitarian aid indicators, the variables *Non-humanitarian Aid Concentration* and *Weighted Non-humanitarian Aid*, which account for all other forms of foreign aid allocated to the sub-national units, are negative and fall far short of statistical significance. Taken together, these results suggest that the frequency of violent engagements between rebel and government forces increases in response to recent inflows of humanitarian assistance but is largely unaffected by other types of aid.

Looking quickly at the results for the control variables, we find that only a few of the other covariates are significant predictors of the frequency of battles between rebel and state forces within a given subnational unit. Consistent with expectation, we find that the other violence variables (including one-sided violence, previous battles, and battles in neighboring units) consistently predict greater levels of battlefield violence. Interestingly, in the ZIP models, *Area*, *Capital Distance*, *Rebel Constituency*, *Total Rebel Groups* are significant predictors of structural zeroes (e.g., in the logit stage), but they do not appear to affect the count of rebel-government battles. However, in the fixed-effect Poisson specification, *Population*, *Rebel Constituency*, and *Total Rebel Groups* are all positively and significantly correlated with the rate of battles between government and rebel forces.

Interestingly, we find little evidence that other forms of lootable resources are systematically related to conflict patterns. In the ZIP models, the variable *Diamonds* is actually negatively related to the frequency of battles. This result holds when we substitute an indicator that includes all types of gemstones.[14] This result may initially appear surprising. However,

[14] We also assess the impact of drug cultivation (coca, opium, and cannabis). However, only cannabis is widely grown in sub-Saharan Africa (in Ethiopia, Mozambique, Nigeria, and

while resources such as diamonds have been linked to conflict outbreak and duration of conflict (e.g., Lujala 2010), other studies suggest that the location of diamond deposits are not strongly linked to the frequency of war events (Hegre, Ostby and Raleigh 2009). Consequently, our analysis suggest that humanitarian aid is significantly more likely to produce conflict violence than lootable resources. This is consistent with the argument we made above, which asserts that rebels would be more likely to expend the effort to exert control over aid-rich areas because, unlike gemstones, the accompanying resources are often readily consumable and do not require labor-intensive extraction, processing, or access to large markets to provide benefits.

While these results provide preliminary support for our hypothesis, we noted above that aid allocation decisions and previous violence are closely interconnected. It would therefore be unrealistic to assume that aid projects are randomly assigned within states experiencing domestic conflict. We supplement the conventional, control-based regression analyses with a quasi-experimental approach that allows us to account for the conditions that lead to the deployment of aid. In studies using observational data, the characteristics of the groups that received the treatment (treatment group) often differ significantly from those that did not receive the treatment (control group). Moreover, these differences often directly influence the process of selection into the treatment group, thereby introducing systematic bias into the analysis and producing incorrect statistical results. In our case, we are concerned that units are selected to receive aid largely because they possess specific qualities such as recent exposure to conflict violence or anticipation of future violence.

We adopt a propensity score-based matching method to address these selection issues. Propensity score matching estimators are widely used to evaluate average treatment effects in

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Uganda). Analyses including a "drugs" variable suggest that cannabis is not a significant driver of battlefield violence in Africa.

non-experimental data (e.g., Abadie and Imbens 2006; Dehejia and Wahba 2002; Rosenbaum and Rubin 1983).[15] The logic is straightforward. The goal of the matching process is to identify a sample of control cases that closely matches the treatment cases in all relevant pre-treatment characteristics. After these matched control cases are identified, any remaining significant differences in the outcome variables can be attributed to the intervention of the treatment rather than to the factors that lead to the assignment of the treatment.

Because of the high level of dimensionality in observational data, locating exact matches on each covariate is extremely challenging. Most matching methods therefore focus on improving the overall balance between treatment and control groups. Propensity score matching relies specifically on matching control and treatment units based on the their probability of assignment to the treatment group given a set of covariates (the propensity score) (Rosenbaum and Rubin 1983). The propensity score is determined by logistic regression. Following convention, in the model estimating the propensity score, we include covariates that predict selection for the treatment as well as those that likely predict the outcome (Caliendo and Kopeinig 2008). Specifically, we include each of the covariates we discussed above as well as additional covariates that likely influence the selection process. First, because aid clusters geographically and spatially, we include a variable representing the proportion of adjoining units that received humanitarian aid in the previous year. Second, we include one-year lags of each of the violence variables because conflict between armed groups or violence against civilians may generate greater aid inflows.

[15] We use the approach developed by Abadie and Imbens (2012), which takes into account that propensity scores are estimated rather than known. As a robustness check, we also employed nearest neighbor matching based on the Mahalanobis distance. Results using the matched dataset create through this process are highly similar.

Following the execution of the logit model, the matching algorithm matches each treatment group to its nearest neighbor based on its estimate propensity score. In order to improve the overall balance between control and treated units, we allow matching with replacement. Matching with replacement trades increased variance for reduced bias, and it is often preferred where treatment and control groups have dissimilar propensity scores (Caliendo and Kopeinig 2008). We take one additional step to help ensure the reliability of the matching process. Serial matching represents a potential problem in the context of time-series data. In many cases the best match for a given observation is that observation at another time point. During the matching process, we therefore exclude observations for one year prior to and one year following assignment to the treatment in order to prevent serial matching.[16]

The matching algorithm successfully locates 326 comparable control cases for the 659 original treatment observations.[17] This dissimilar number of control and treatment units occurs because we allow control units to be resampled in order to improve the overall balance in the matched sample. In subsequent regression analyses utilizing the matched dataset, we therefore rely on frequency weights based on the number of times a control unit was resampled to account for the asymmetry in the number of treatment and control units. We report balance statistics for the matched sample versus the original sample in Table 2. Overall, the matching process significantly improves the balance between the treatment and control groups. For most variables the mean difference between treated and control groups drops significantly following the matching process. Moreover, the standardized bias for each variable is ≤ 0.20, indicating that less

[16] Examining the matched data reveals that only 8 (roughly 1%) of treatment cases were matched to control cases from within the same panel, and none were matched in consecutive years.
[17] The number of treatment cases used in the matching is slightly lower than the total number of treatment cases in the total sample (n=700) because of missing values on the some of the pre-treatment matching variables.

than one quarter of a standard deviation difference remains between the groups.[18] Most importantly, following the matching process, the mean values of recent *Battles* across the control and treated group are nearly indistinguishable. In fact, the difference in means for each of the violence variables is statistically insignificant between control and treatment groups. This suggests a significant improvement in the overall similarity between the groups on this dimension and reduces the likelihood that our results are driven by systematic differences in recent exposure to conflict violence.

--Table 2--

We use this matched sample to estimate the causal effect of humanitarian aid allocation on the frequency of battles between the state and rebel forces. Results for the matched sample analyses are presented in Table 3. We conduct two types of analyses using the matched sample. The first is a simple treatment effects model that uses the propensity score-matching estimator to compute the Average Treatment Effect (ATE). Here, the ATE reflects the average of the difference between the observed and potential outcomes for each subject. In other words, we compute the impact that receiving the treatment had on members of the treatment group relative to members of the control group conditioned on the matched propensity scores. According to Model 1, after accounting for the process that drives selection into the treatment group, subnational units that received *Humanitarian Aid* during the year experienced approximately 0.5 additional battles than did those that did not receive the treatment.

In the second analysis we employ a difference-in-difference (DiD) estimator to control for any remaining bias created by secular trends in the data. While the matching process

[18] Standardized bias for continuous covariates is calculated by dividing the difference in means of the covariate between the treated group and the comparison group by the standard deviation of the treatment group. 0.25 is conventionally seen as the threshold for a reasonable match.

significantly reduced the overall differences between control and treatment observations, it does not allow us to directly account for trends in the trajectories of the control and treatment groups that would have continued in the absence of the treatment. The DiD method accounts for time trends unrelated to the treatment intervention by examining the difference in the change observed in the treatment group compared to the change observed in the control over a given window of time (Angrist and Pischke 2009). Specifically, we evaluate the difference between the pre- and post-assignment rate of *Battles* within the treatment group and compare this change to the observed change in the control group over the same temporal window. The difference in these changes reflects the causal effect of the treatment.

--Table 3--

--Table 4--

In practice, we observe each observation in the year prior to the introduction of the treatment and the year in which the treatment was deployed. We consider three elements that account for any changes in the frequency of battlefield engagements between rebel and government forces within this window of time: 1) whether a unit was assigned to the treatment group or control group, 2) whether the observation is in the pre-treatment period or the post-treatment period, and 3) the interaction of the former factors. Each factor is included as a binary variable in the DiD models: *Humanitarian Aid*, *Post-treatment Period*, and an interaction term composed of these two factor variables. The interaction term effectively represents the average treatment effect on the treated units (ATET).[19] In other words, the coefficient on the interaction

[19] Formally, we estimate the following model: $y_{ist}=\alpha+\gamma T_s+\lambda P_t+\delta(T_s*P_t)+\varepsilon_s+\mu_t$, where y is the number of *Battles*, γT represents the *Humanitarian Aid* treatment group across the two periods, λP reflects the *Post-treatment Period,* and $\delta(T*P)$ represents the interaction term. The subscript i refers to unit-level variation, t refers to the time period under observation, and s refers to the treatment group.

term reflects the number of *Battles* that result from treated units' exposure to *Humanitarian Aid* compared to the number that these units would have been expected to receive if they had not received humanitarian aid. For the sake of consistency, we estimate this relationship via standard negative binomial models, but the results are robust to linear models.

As with the previous analyses, the results from the DiD model presented in Model 2 suggest a positive and statistically significant relationship between humanitarian aid and the subsequent frequency of conflict between rebel and government forces.[20] Given that the coefficient of interest is an interaction, we calculate the marginal effects in order to determine the impact of aid on the treated units (see Puhani 2012). We report the substantive impact of aid in Table 4. According to these results, pretreatment levels of violence between control and treatment units differed slightly, yet not in the manner that might be expected if aid goes to more violence areas. The mean number of *Battles* observed during the period prior to the implementation of the treatment is actually slightly higher in the control rather than the treatment group. Interestingly, the results also suggest that violence declines significantly in the control units between *t*-1 and *t.* By contrast, it increases slightly in treated units. In other words, while violence was slightly higher in the control group before the treatment, it declined at a sharper rate between pretreatment and treatment periods. By contrast, mean violence levels rose slightly during this period among groups that received aid. The overall difference in the rates of change between control and treated units is approximately 29%. The results therefore suggest that violence remained higher—and even marginally increased—in areas that received humanitarian aid compared to highly comparable units that did not receive aid.

[20] We also replicated the model using *Non-humanitarian Aid* as the treatment. As with the previous results, the results from a DiD model suggest that other types of aid are not systematically related to subsequent changes in the level of conflict violence.

The results from the DiD analysis are consistent with our argument and provide strong support for our primary hypothesis. They also potentially shed additional light on the manner through which aid influences violence. It appears that the introduction of aid contributes to the persistence of violence in the areas in which it is committed. This result might therefore suggest that rebels do not necessarily instigate new violence in the areas in which aid accrues; rather, they are more likely to fight harder and longer to maintain control over these areas. To put it another way, the absence (or retraction) of aid appears to reduce (or remove) some of the incentive for rebels to contest a given territory, thus contributing to a general decline in violence.

Implications and Conclusion

The implications of our findings are perhaps disappointing at first glance. While aid is intended to alleviate human suffering, we find that it often perpetuates violence in the areas in which it is employed. Despite this finding, we do not interpret our results as justifying the reduction of development and humanitarian aid to states threatened by internal conflict. Development and humanitarian aid have become important tools through which the U.S. and other developed states seek to achieve their national security interests. More importantly, humanitarian aid is often critical to sustaining vulnerable populations adversely impacted by conflict. Drastically reducing aid to vulnerable populations may therefore not represent a realistic option given the immediate and heavy human costs it would likely impose.

Despite these rather discomfiting results, we believe that it is premature to make policy recommendations based on these findings. Our results highlight the need to improve policies and practices of aid distribution in conflict zones with the recognition of potential negative consequences in the area. However, improvements in policy should be guided by rigorous

analysis. For example, acknowledging the potential role of humanitarian aid in the production or continuation of violence (as well as mechanisms that drive that relationship) is an important first step. It is also important, however, to evaluate whether other forms of foreign aid or assistance produce similar effects. For example, our results join a number of other studies that demonstrate the potential negative externalities associated with humanitarian aid (e.g., Anderson 1999; Cooley and Ron 2002; Narang 2014; Wood and Sullivan 2015). Other studies further suggest that some forms of foreign can successfully ameliorate conflict violence in active warzones (Berman, Shapiro and Felter 2011; Crost, Felter, and Johnson 2014). In addition, several studies have shown that some types of foreign aid may reduce the odds that a state experiences violence conflict in the first place (Savun and Tirone 2013; Young and Findley 2011). Our study helps clarify some of the ambiguity in previous results by showing that some types of aid (e.g., non-humanitarian forms) do not create the types of negative externalities associated with humanitarian aid. This represents an important distinction that future empirical studies should further explore. Specifically, it is important to identify what alternative types of aid may be most successful at ameliorating conflict or which are least likely to worsen instability and violence.

Examining the role of aid in exacerbating or ameliorating violence may is an important area of inquiry. In this manuscript we constrained our argument and analysis specifically to the relationship between humanitarian aid and rebel-government violence at the district (or equivalent subnational unit) level. However, future research using data disaggregated by location, donor, and sector will help further clarify how specific aid types, distribution methods, and donors influence the impact of aid on conflict. Scholars may also wish to investigate how aid interacts with peacekeeping operations. Given the wealth of studies that link peacekeeping with violence reduction (e.g., Hultman, Kathman and Shannon 2015), examining the impact of

humanitarian employed within this context would represent a fruitful avenue for future exploration. Each of these areas could provide important and policy relevant finding regarding the effectiveness of specific aid strategies and might also produce new insights about how resource inflows shape rebel decision-making.

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Table 1: Results from Poisson Models

	Model 1		Model 2		Model 3	Model 4
	ZIP		*ZIP*		*Poisson, Fixed Effects*	
	Count	Inflation	Count	Inflation		
Humanitarian Aid Concentration $_{(t\text{-}1)}$	0.674*				0.554*	
	(0.168)				(0.132)	
Weighted Humanitarian Aid$^{\dagger}{}_{(t\text{-}1)}$			0.044*			0.036*
			(0.011)			(0.009)
Non-humanitarian Aid Concentration $_{(t\text{-}1)}$	-0.108				-0.101	
	(0.272)				(0.290)	
Weighted Non-humanitarian Aid$^{\dagger}{}_{(t\text{-}1)}$			-0.005			-0.002
			(0.017)			(0.017)
One-sided Violence	0.007*	-0.135*	0.007*	-0.135*	0.006*	0.006*
	(0.001)	(0.055)	(0.001)	(0.056)	(0.002)	(0.002)
Area$^{\dagger}{}_{(km2)}$	0.040	-0.170*	0.041	-0.170*		
	(0.063)	(0.055)	(0.063)	(0.056)		
Flooding$^{\dagger}{}_{(km2)}$	-0.022	0.014	-0.026	0.013	-0.032	-0.032
	(0.019)	(0.026)	(0.020)	(0.026)	(0.020)	(0.020)
Capital Distance$^{\dagger}{}_{(km)}$	-0.023	0.115*	-0.025	0.114*		
	(0.031)	(0.051)	(0.031)	(0.051)		
Population$^{\dagger}{}_{(t\text{-}1)}$	0.076	-0.002	0.073	-0.002	1.324*	1.326*
	(0.079)	(0.067)	(0.079)	(0.067)	(0.498)	(0.499)
International Border	-0.007	-0.123	-0.011	-0.123		
	(0.183)	(0.193)	(0.183)	(0.193)		
Rebel Constituency	0.121	-0.964*	0.113	-0.965*	0.705*	0.691*
	(0.145)	(0.171)	(0.146)	(0.171)	(0.358)	(0.353)
Total Rebel Groups	0.020	-0.347*	0.021	-0.348*	0.130*	0.132*
	(0.058)	(0.069)	(0.057)	(0.069)	(0.046)	(0.046)
Diamonds	-0.117	-0.150	-0.107	-0.148		
	(0.164)	(0.209)	(0.165)	(0.210)		
Lagged Battles	0.022*	-0.232*	0.022*	-0.232*		
	(0.005)	(0.039)	(0.005)	(0.039)		
Neighbor Battles	0.641*	-2.591*	0.641*	-2.592*	2.784*	2.775*
	(0.206)	(0.265)	(0.206)	(0.266)	(0.240)	(0.241)
Wald X^2/F	658.09*		657.71*		315.35*	312.17*
N	3,558		3,558		2,560	2,560

Coefficients and standard errors from ZIP models (Models 1 & 2) and Poisson models with panel fixed effects (Models 3 & 4). Time invariant covariates dropped in fixed-effects models. *=$p \leq 0.05$

Table 2: Balance Statistics

	Raw Data		**Matched Data**		
	Treated	*Control*	*Control*	*Mean Difference*	*Standard Bias*
Non-humanitarian Aid $_{(t-1)}$	0.081	0.738	0.070	0.011	0.069
Battles$_{(t-1)}$	3.632	1.303	4.226	-0.594	-0.073
One-sided Violence	5.238	1.294	3.551	1.687	0.114
One-sided Violence $_{(t-1)}$	6.234	0.204	5.353	0.881	0.039
Total Rebel Groups	1.266	8.979	1.173	0.093	0.105
Neighbor Battles	0.369	0.693	0.365	0.004	0.013
Neighbor Battles$_{(t-1)}$	0.398	0.346	0.457	-0.059	-0.190
Area (km^2)†	10.067	4.666	9.779	0.288	0.138
International Border	0.835	0.111	0.887	-0.052	-0.140
Flooding (km^2)†	0.836	0.158	0.341	0.495	0.177
Capital Distance (km)†	4.814	12.704	4.642	0.172	0.083
Diamonds	0.15	0.085	0.129	0.021	0.059
Rebel Constituency	0.289	12.704	0.293	-0.004	-0.009
Population† $_{(t-1)}$	13.68	0.085	13.53	0.150	0.107
Neighbor Humanitarian Aid $_{(t-1)}$	0.439	0.085	0.451	-0.012	-0.035

659 matched pairs. Matched sample statistics reflect frequency weighting used for repeated control units. *Note: Means of treatment group are the same in the raw and matched samples.

Table 3: Matched Sample Analyses

	Model 1 *ATE*	**Model 2** *Difference-in-Difference*
Humanitarian Aid	0.509 (0.216)*	-0.152 (0.349)
Post-treatment Period		-0.278 (0.132)*
Humanitarian Aid X Post-treatment Period		0.321 (0.161)*
Wald X^2/F		6.19*
N	3,015	2,636 (659 pairs)

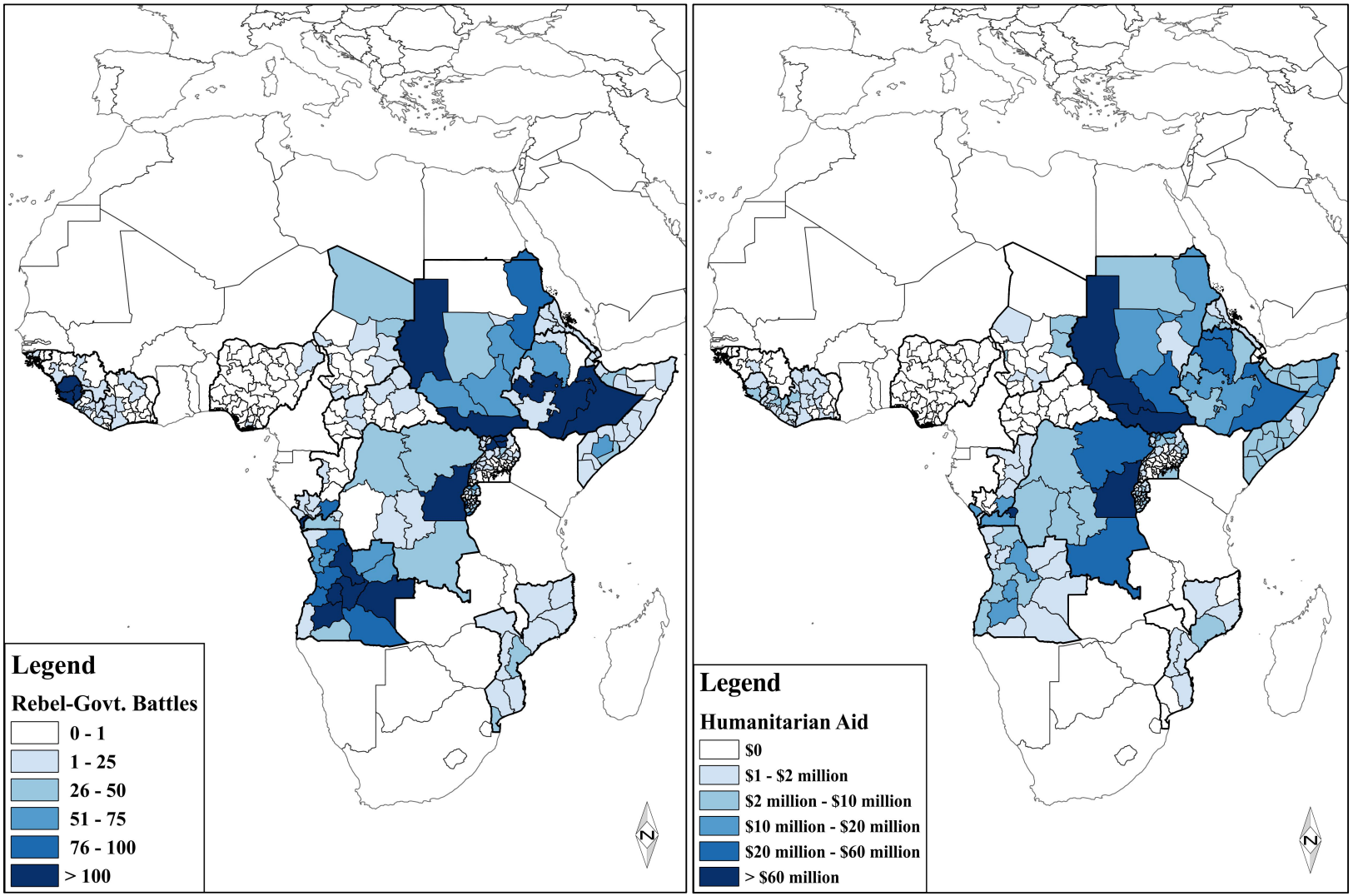
Coefficients from negative binomial models with standard errors (clustered on subnational units).*=$p \leq 0.05$

Table 4: Substantive Impact of Humanitarian Aid (Difference-in-Difference)

Humanitarian Aid	**Mean Battles (*t*-1)**	**Mean Battles (*t*)**	**Mean Difference**	**% Change**
Yes	3.63 (0.317)	3.80 (0.403)	+0.17	+4.38%
No	4.23 (1.428)	3.20 (0.845)	-1.03	-24.35%
ATET				**28.73%**

Point estimates with standard errors (computed via delta-method) in parentheses.

Figure 1: Rebel-government Battles and Humanitarian Aid in Sub-Saharan Africa, 1989-2008



Total number of rebel-government *Battles* (left) and accumulated value of *Humanitarian Aid* commitments ($US) within subnational units for the sample of African conflict states between 1989 and 2008 included in the analysis.