Aid Shocks and Political Conflict: Evidence from the Disruption to United States Foreign Aid

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Abstract

How do foreign aid shocks affect political conflict? I answer this question by (1) leveraging the exogenously timed disruption to United States foreign assistance in 2025, (2) building a novel dataset of subnational locations of foreign assistance projects, and (3) conducting a pre-registered analysis. I find that the disruption caused a substantial increase in conflict, including conflict that is typically driven by armed groups (such as battles) as well as conflict typically driven by civilians (such as riots). I provide evidence for the most likely mechanisms for each type of actor. To explain the increase in conflicts involving armed groups, I propose a theory in which the abrupt loss of aid signals short-term state weakness, which creates incentives for rebels to attack while they are in a relative state of power. The paper expands the evidence-base and theory for how aid impacts conflict, with important implications for policy.

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1 Introduction

In January of 2025, the Trump administration announced a pause on all United States (US) foreign assistance. In the following months, the administration went on to cancel billions of dollars of aid and cause a massive disruption to the execution of aid programs. Although the scale of this disruption to the aid system was likely unprecedented, negative aid shocks are in fact common. Indeed, in a recent introduction to a special issue on aid withdrawal, Cheeseman et al. (2024) claim that "aid suspensions and withdrawals are a fundamental part of the political economy of foreign aid." Furthermore, Raddatz (2007) estimates that aid shocks account for 25 percent of the variation in GDP in low-income countries caused by external shocks. It is therefore no surprise that a large academic literature has been dedicated to examining both the causes (Nielsen 2013; Molenaers et al. 2015; Heinrich et al. 2016; Swedlund 2017; Von Borzyskowski and Vabulas 2019; Attia and Grauvogel 2023; Cheeseman et al. 2024) and effects (Bulíř and Hamann 2008; Arellano et al. 2009; Chauvet and Guillaumont 2009; Agénor and Aizenman 2010; Kodama 2012; Hudson 2015; Iannantuoni 2025) of aid withdrawals and shocks. However, only a small subset of this literature has focused on the impact of aid shocks on political conflict (Nielsen et al. 2011; Gutting and Steinwand 2017; Szabó 2022). In this paper, I build on the literature by advancing the theory for how aid impacts conflict, building an original dataset, and using a novel identification strategy to estimate the effects and unpack mechanisms.

From a research perspective, the 2025 disruption to US foreign aid is unique because the timing of the disruption was exogenous to political conflicts in aid-receiving countries and it involved a near-total pause in the implementation of US foreign aid. These factors allow for a credible causal estimate of the impact of negative aid shocks on conflict, which is typically difficult to achieve due to the endogenous relationship between the targeting of aid and conflict.

To leverage the unprecedented disruption, I build a novel dataset of the subnational locations of US foreign aid programs in 19 conflict-affected, aid-dependent countries for the fiscal year prior to the disruption. The dataset yields information on the locations of aid projects worth a total value of 5 billion US dollars in funding obligations for the period prior to the disruption.

I use these data to conduct a pre-registered event-study analysis. Essentially, I compare trends in conflict in subnational administrative units that were dependent upon US foreign aid prior to the disruption (treatment units) to administrative units that were not dependent (control units). I show that the treatment and control units experienced largely parallel trends in political conflict in the 10 years leading up to the event—and that the treatment units experienced a sharp relative spike in conflict in the first 6-month period following the disruption to aid. Specifically, for aid-dependent units with an average number of violent events in the pre-disruption period, I find that the disruption caused a 40 percent increase in violent events relative to non-aid-dependent units.. I provide evidence against a range of alternative explanations.

I find that the aid disruption caused an increase in conflict events that are typically driven by armed groups (such as battles and explosions) as well as events typically driven by civilians (such as riots and protests). The disruption also increased the incidence of conflict involving various types of armed groups, including state forces, rebel groups, and militias. Correspondingly, the impact was felt across a range of different countries characterized by distinct types of conflict.

I provide suggestive evidence that the disruption may have impacted different actors through distinct mechanisms. The increase in civilian-driven conflict is consistent with the aid withdrawal causing an increase in resentment or dismantling progress in improving attitudes toward the state. The increase in conflict between militias is consistent with the disruption increasing economic incentives for engaging in conflict. To explain the increase in state-rebel and state-militia conflict, I propose a novel theory in which the abrupt loss of aid signals short-term state weakness, which creates strategic incentives for non-state armed groups to initiate attacks while they are in a relative state of power.

I find that the effects of the disruption on conflict faded over time, becoming statistically insignificant about 6 months after the start of the disruption. I argue that the short duration of effects could be explained by the fact that most aid projects had resumed after about 5 months of the initial disruption. Alternatively, it is possible that some of the mechanisms through which shocks impact conflict are most likely to operate in the immediate aftermath of the shocks.

Previous studies have made important theoretical and empirical advancements in our under-

standing of the effects of aid shocks on conflict (Nielsen et al. 2011; Gutting and Steinwand 2017; Szabó 2022). My research builds on this literature in two main ways. First, it provides an arguably more credible causal estimate of the impact of aid shocks on conflict. Previous studies have relied on either matching methods (Nielsen et al. 2011; Gutting and Steinwand 2017), which do not overcome the problem of endogenously targeted aid, or instrumental variable approaches (Szabó 2022), which rely on strong exclusion restrictions. These studies have also used countries as the unit of analysis. In contrast, my event study approach leverages an exogenously timed shock, and I create novel data to leverage subnational variation in aid-dependence, which allows me to control for country-level trends and thus strengthen the plausibility of the parallel trends assumption. Furthermore, the credibility of my estimates is enhanced by the fact that I pre-registered my study and thus committed to design specifications prior to analysis.

Second, departing from previous studies that have examined the impact of shocks at the yearly level, my design allows me to examine impacts at a more granular level, including 6-month and 3-month periods. Furthermore, by focusing on a more contemporary time frame, I am able to use data that disaggregates violent events in terms of type and actor. Together, these two advancements allow me to examine the duration of effects and test a variety of different mechanisms in ways that previous studies could not. I also propose and test a novel mechanism for how aid shocks increase conflict. As a result, I present a more complete picture of the impact of aid shocks and advance our understanding of the means by which aid shocks impact conflict.

2 Literature and theory

2.1 Existing mechanisms

A rich literature has established numerous mechanisms through which foreign aid might either decrease or increase conflict (Zürcher 2017; Findley 2018). There are at least four mechanisms through which it might *decrease* conflict. First, aid can alter the economic incentives to engage in conflict, including by increasing the opportunity cost of engaging in conflict or lowering the marginal

benefit of conflict. If aid creates livelihood opportunities through, for example, business training, agricultural support, or cash-for-work programs, then it raises the opportunity cost of joining armed groups (Blattman and Annan 2016; Crost et al. 2016; Dasgupta et al. 2017). If it raises incomes, then it lowers the marginal benefits of any material benefits gained by fighting (Humphreys and Weinstein 2008). Second, aid can be used to reduce grievances against the state, win over "hearts and minds" of civilian populations, or generally improve attitudes toward the state. If aid provision achieves these goals, then civilians should be less likely to join non-state armed groups, participate in violence against the state, or support non-state armed groups, and more likely to support the state in countering these groups (Berman et al. 2011; Berman et al. 2013; Sexton and Zürcher 2024; Beath et al. 2025). Third, foreign aid can strengthen the capacity of the state to counter armed groups, including through funding or technical assistance to security institutions or, in the long run, by boosting GDP (Collier and Hoeffler 2002). Fourth, peacebuilding aid projects have the explicit goal of reducing violence and conflict. These include dispute resolution training (Blattman et al. 2014) and anti-violence campaigns around elections (Collier and Vicente 2014).

There are at least two main mechanisms through which foreign aid might *increase* conflict. First, if aid has the potential to improve attitudes toward the state and deter recruitment into armed groups, then insurgents have an incentive to violently sabotage aid projects (Crost et al. 2014; Sexton 2016). Similarly, it may compel them to use violence to retake territory (Weintraub 2016). Second, aid can encourage predation. Many aid programs, such as food assistance, involve the distribution of lootable resources. Armed actors may fight over these resources for personal gain or to fund insurgencies (Nunn and Qian 2014; Premand and Rohner 2024). When aid is provided directly to the government, violent actors may be more likely to fight over control of the state or political office (Grossman 1992; Dreher et al. 2025).

All six of these mechanisms could be relevant to the sudden disruption to foreign aid. For example, if aid supporting livelihoods is terminated, then the opportunity cost of participating in violent conflict may fall. If humanitarian aid disappears, so too do opportunities for predation. In addition to these mechanisms related to decreased *levels* of aid, scholars have also identified additional mechanisms that are specifically relevant to aid *shocks*. Whereas the levels mechanisms could apply

to situations where aid is either suddenly or gradually withdrawn, the shock mechanisms depend on aid volatility or the sudden withdrawal of aid. First, Nielsen et al. (2011) argue that negative aid shocks create commitment problems that lead to a breakdown in bargaining between the state and rebel groups. They theorize that aid withdrawals may shift the balance of power, which allows rebels to demand more resources from their weakened government opponent. But aid withdrawals also weaken government finances, which means the government cannot afford to pay the rebel demands in the short term and instead have to promise payments from future revenues. However, since the balance of power may shift in the future in favor of the government, the government cannot commit to future payments. The rebels therefore have an incentive to initiate violent conflict while in a relative position of power. Importantly, the authors use this theory to explain conflict onset; it is less relevant to explaining conflict intensity. We would also not expect this mechanism to lead to conflict in the very short term, as it first requires a breakdown in bargaining.

Second, Lyall et al. (2020) provide evidence from an experiment that aid programs that generate short-term windfalls might create resentment toward the state after the program ends. Thus, it is possible that the act itself of withdrawing aid—rather than just the absence of aid, as suggested by the attitudes mechanism—can create animosity that manifests as participation in violence against the state.

2.2 A novel mechanism: Signaling a weakened state

I also propose a novel mechanism through which a negative aid shock might increase conflict. The sudden withdrawal of aid could provide a signal to armed groups about the strength of the state. Witnessing the salient and widespread termination of aid programs, armed groups might perceive that the state has lost both funding and external support, which could in turn indicate that state forces have been weakened. At the same time, armed groups may be uncertain about the duration of this lost support. Over time, the government may re-establish strong ties with the donor country or recover funding from other sources. As a result, armed groups may seek to strike the state while in a position of relative strength, thus improving their bargaining position in future negotiations.

This is essentially a commitment problem; if armed groups believe their position of strength is temporary and the government cannot commit to a bargain made under this temporary distribution of power, then armed groups have an incentive to escalate conflict to exploit their fleeting advantage (Powell 2006). The logic of this mechanism is also consistent with work by Malis and Smith (2021), who demonstrate that challenges to state leaders are less likely following salient signals of external support. It should be clear that this mechanism shares many similarities with the one proposed by Nielsen et al. (2011). But it also differs in important ways. First, it does not require an actual shift in the balance of power, only a perceived shift. Second, it can help to explain changes in intensity of conflict, not just onset. Third, it is more likely to lead to conflict on a short time scale.

This mechanism could have varying effects subnationally. Crucially, we would expect the signal about weakened state strength to be stronger in areas with more disruption to aid. If the state seems weaker in areas with more disruption, it may be more vulnerable to attack in these areas. Furthermore, among rebel groups with regional rather than national presences, the disruption will likely provide a stronger signal of state weakness to groups based in areas with more disruption.

2.3 Evidence and hypothesis

The evidence for the overall effects of aid on conflict is mixed, with studies providing evidence that aid decreases conflict (Berman et al. 2011; Berman et al. 2013; Blattman and Annan 2016; Crost et al. 2016; Dasgupta et al. 2017; Chu et al. 2017; Lyall 2019; Mary and Mishra 2020; Beath et al. 2025; Mary 2026), increases conflict (Crost et al. 2014; Nunn and Qian 2014; Weintraub 2016; Premand and Rohner 2024; Dreher et al. 2025), or has no effect on conflict (Gehring et al. 2022; Sexton and Zürcher 2024). Research on the effects of negative shocks or sudden withdrawals of aid is much more limited, but also in greater agreement. In addition to the micro-level evidence from Lyall et al. (2020), both Nielsen et al. (2011) and Szabó (2022) provide evidence that negative aid shocks cause an increase in conflict. Gutting and Steinwand (2017) also document a conflict-increasing effect of negative shocks, which they find is ameliorated by greater donor proliferation. I therefore propose the following pre-registered hypothesis:

H1: Disruption to aid will cause an increase in conflict in aid-dependent areas relative to non-aid-dependent areas.

The above discussion makes clear that different mechanisms may impact different violent actors and types of conflict. Therefore, to unpack mechanisms and the scope of impact, I explore effects by different actors and event types.

3 Background

3.1 Disruption to US foreign aid

On the first day of his second term, January 20, 2025, President Donald Trump issued an executive order calling for a 90-day pause on all US foreign assistance (The White House 2025). In practice, this meant that no money could be obligated or disbursed to the contractors that implement foreign assistance programs. The executive order also called for a review of all foreign aid programs to determine which were aligned with American interests. Four days later, the administration issued a stop-work order for all US foreign assistance programs (USAID 2025). This meant that all ongoing programs—for which about $63 billion are spent annually (CSIS 2025)—were forced to cease, with exceptions only for military financing to Israel and Egypt and emergency food assistance.

The disruption to foreign assistance strongly affected all aid-implementing agencies, the largest being the United States Agency for International Development (USAID), followed by the Department of State (DoS), and the Department of Agriculture. But USAID was the only one of these agencies to be dismantled by the Trump administration. On January 27, massive USAID lay-offs were announced. On February 4, all direct-hire USAID staff were put on leave. In March, plans were announced for parts of USAID to be absorbed into DoS by July 1—and the rest eliminated (Jerving 2025; Sablich and Ainsworth 2026).

From January 24 to February 4, several waivers to the stop-work order were issued, including for "life-saving humanitarian assistance" (Miolene 2025a). In theory, the initial exceptions and additional waivers should have allowed for the continuation of activities related to food assistance,

HIV treatment, and malaria prevention, among others. However, in practice, even the programs that were in theory eligible for waivers remained paused due to a lack of payment, the massive staffing disruptions at USAID, and unclear guidance (Human Rights Watch 2025a; Miolene 2025a). The magnitude of federal expenditures to USAID during this period are illustrative. From the beginning of the stop-work order to March 10, expenditures to USAID totaled 245 million—just 3 percent of the average expenditures during the same period for the previous two years.[1]

On March 10, the Trump administration announced the cancellation of thousands of aid contracts. These included over 5,000 USAID contracts and over 4,000 DoS contracts (Human Rights Watch 2025b; Kates 2025). The list of canceled USAID contracts was leaked and, later that month, a separate list of retained contracts was leaked. Analyzing these lists, Kenny and Sandefur (2025) estimate that, in terms of dollar value, 34 percent of USAID programming was canceled. Most sectors suffered major cuts. For example, Kenny and Sandefur (2025) estimate that at least 85 percent of the value of programs in the agriculture, infrastructure, education, and conflict mitigation and reconciliation sectors, respectively, was canceled. The areas least-affected by the cuts were sectors related to humanitarian support, health, and macroeconomic growth, though they still suffered major cuts (e.g., a 20 percent reduction in HIV/AIDS programs).

For the programs whose contracts that were not canceled, the stop-work and funding freeze were officially lifted as of March 10. However, in practice, they continued to face disruptions. Although many payments for ongoing contracts issued by USAID and other aid organizations resumed in March, they slowed to a trickle in April, largely due to the actions of the Department of Government Efficiency (DOGE) and its government-wide "Defend the Spend" initiative. As a result, though many organizations resumed implementation in late March, they were again left without funds for implementation starting April. This resulted in a second *de facto* freeze on funding, albeit less comprehensive than the first (Miolene 2025b), which lasted until mid June (Brookings 2026). By July 20, 6 months after the start of the aid disruptions, 8.64 billion had been disbursed to USAID contracts (including those absorbed by DoS on July 1)—just 52 percent of the average for the same

[1]Author's calculations based on data from Brookings (2026). Data are available for 2023, 2024, and 2025.

period in the previous two years.[2]

Even as payments resumed, the US aid system continued to be disrupted by a lack of new contracts. In the third quarter of 2025, DoS obligated about $4.7 billion to contracts, which is about a fifth of the total obligated by DoS and USAID for the same period in previous years (Miolene and Tamonan 2025). Thus, even following the initial 6-month disruption ended, many programs remained canceled and few new ones were being created, so the US aid system continued to operate at a strongly diminished capacity.

Several features of the disruption to the aid system are important for the research design. First, the initial disruption from January 20 to March 10 was near-total, which means that virtually all aid programs were affected. Second, the payment delays from April to June that were spearheaded by DOGE were part of a government-wide initiative, which means they likely affected most aid programs. Third, although the cancellations that began in March were more selective, they virtually eliminated entire sectors and thus had very broad effects.

3.2 The consequences of disruption

As of 2024, the US was the largest donor of foreign assistance in the world, contributing about 30 percent of global development assistance (CGD 2025), and USAID distributed the majority of US foreign assistance (DeSilver 2025). It is thus easy to imagine that a massive disruption to US foreign assistance and the termination of USAID would have major repercussions.

In the aftermath of the freeze, the impacts in many places were *immediate* and *salient*. For example, within one month of the freeze, one international NGO was forced to shut down the hospital and clinics it ran in refugee camps on the Myanmar-Thai border. Many people were discharged, leaving them unable to access medication and care, and there was at least one reported death as a result. In Haiti, a major HIV-treatment program was shut down, leaving pregnant women without access to medication that would prevent them from spreading HIV to their infants (Farge et al. 2025). In February, a US official leading the humanitarian response in northeast Syria,

[2]Author's calculations based on data from Brookings (2026).

which is estimated to serve millions of people, claimed that "We are seeing complete destruction of what once was a humanitarian response" (Konyndyk 2025).

Examples of specific programs that were terminated help to provide a deeper sense of the impact. In the eastern Democratic Republic of the Congo (DRC), a region beset by persistent armed conflict, the Partnership for the Development of the Eastern Congo (P-DEC) program was canceled. The program reportedly created livelihoods opportunities for thousands of young men, many of whom are no doubt vulnerable to recruitment into armed groups. The program also involved conflict resolution dialogues and an early warning system to protect civilians from outbreaks of violence (Aid Report 2025). Other illustrative program terminations include a $118 million program to procure and deliver HIV commodities in Uganda, a $76 million food assistance program for severely food-insecure households in Yemen, and an $8 million program designed to reduce gang violence in Haiti.[3]

Anecdotal accounts help to bring to light the human implications. In Dzaleka refugee camp in Malawi, the funding cuts forced aid agencies to reduce cash transfer rations by 50 percent. Aid workers reported in August that these reductions had pushed refugees to desperate measures, including theft and sex work (Kateta 2025). According to one refugee in Kenya's Kakuma Camp, as a result of cutbacks in humanitarian support "Desperation has increased, and with it lawlessness, suicide attempts, and families fleeing back to unsafe home countries." Another refugee claimed that "The security situation is out of hand. People are robbed in daylight near distribution centers. On 14 August, 2025, two people were killed with panga [machete] cuts in Kakuma" (NRG 2025, p. 12).

It is clear that the aid disruption could have potentially impacted conflict through each of the proposed mechanisms, including in the immediate aftermath of the initial disruption. The vast majority of social programs, which could have been improving attitudes toward the state, and livelihoods programs, which could have been increasing the opportunity cost of participating in conflict, were paused and eventually canceled. Peace and security aid, which was used to both bolster the state security sector and directly mitigate conflict, was virtually eliminated. Fewer aid projects meant fewer incentives to sabotage and fewer opportunities for predation. The massive

[3]Kenny (2025) and author's own data, described below.

and salient withdrawal of aid could have also signaled a weakened state, undermined the state's ability to pay off rebels, and created resentment.

4 Data

4.1 Constructing a dataset on subnational dependence on US foreign assistance

Existing subnational data on the locations of foreign aid projects are scarce. Moreover, they often cover select donors and time periods, and their coverage is often quite low. For example, one prominent recent source is the Geocoded Official Development Assistance Dataset (GODAD) (Bomprezzi et al. 2025). The dataset is impressive in scope, covering 19 donors, including the US, for 1973-2020. However, they geocode only about 20 percent of projects in terms of dollar value. The time range of the dataset also ends five years prior to the start of the aid disruption. Another major source is the International Aid Transparency Initiative (IATI 2026). The dataset relies on voluntary reporting from donors, and the US does not report for most countries. Other major sources include donor-specific datasets that do not include the US (AidData 2026); various country-specific datasets for periods prior to 2020, which geocode a smaller proportion of projects than the GODAD dataset (Bomprezzi et al. 2025); and a dataset constructed by Findley et al. (2011) for countries experiencing civil conflict in sub-Saharan Africa from 1989 to 2008, which geocodes about 5 percent of projects in terms of dollar value.[4] Although each of these datasets make valuable contributions to research, none allow for reliable estimates of subnational dependence on US foreign aid at the time the disruption began in 2025.

I therefore construct a novel dataset of the subnational locations of US foreign assistance. I pre-registered the procedures for data and variable construction prior to analysis.[5] In appendix G, I document any deviations from these parameters, and I show that the deviations have virtually no

[4]Author's calculations using data from Findley et al. (2011). I exclude funding to the national government (precision code 8) from the denominator.

[5]The pre-analysis plan can be viewed here: https://osf.io/mx3kr

effect on results.

As a starting point for dataset construction, I download data on all US foreign assistance obligations for fiscal year 2024—which runs from October 1, 2023 to September 30, 2024—from ForeignAssistance.gov. I focus on fiscal year 2024 because ForeignAssistance.gov provides only the fiscal year of obligations, not the month. Therefore, 2024 is the latest period for which all obligations are made prior to the aid disruption. ForeignAssistance.gov provides information on two types of financial transactions: obligations, which are legal commitments to disburse money for an aid project, and disbursements, which are payments for projects. I focus on obligations because they are more likely to be tied to projects that were ongoing when the disruption began.

I use the obligation data to determine subnational locations for countries in which (1) total obligations for 2024 are at least 0.5 percent of GNI for the most recent year with data and (2) there were at least 50 political conflict events and 50 fatalities from political conflict in the year prior to Trump's 2025 inauguration according to ACLED data (Raleigh et al. 2023). Since many of the mechanisms outlined above are related to intrastate conflict, I exclude countries for which conflict is primarily interstate. These criteria yield an initial sample of 25 countries.

I focus on specific types of foreign aid. In the final dataset, I include aid that could conceivably impact conflict in the short-to-medium-term and which is specific to certain areas of a given country. I refer to all aid included in my analysis as "relevant aid." Appendix A provides details on the types of aid that were omitted.

I determine the locations of aid projects tied to obligations through several steps. First, I use OpenAI to extract subnational location information from the activity title and description associated with each obligation, which are provided by ForeignAssistance.gov.[6] This approach is essentially the same as the approach employed for the GODAD dataset. In addition, for any obligations for which information was not extracted, I attempt to manually extract subnational location information from the activity title and description. In terms of dollar value, this step approach accounts for about 17 percent of the subnational location information in my final dataset.

Second, when no subnational location information is available through the activity descrip-

[6]I provide the OpenAI prompt in appendix A.

tions provided by ForeignAssistance.gov for an obligation, I search online for project documents that match the obligation in terms of activity description, country, and implementing organization name. I then extract subnational location information from these documents. Links to the source documents are provided in the replication code. This step accounts for about 24 percent of the location information in my final dataset.

Third, when no project documents can be located for an obligation, I extract subnational location information from documents found online that describe the areas of operation for the implementing organization associated with the obligation. In cases where these documents are lengthy reports, I use OpenAI to count the number of mentions of each subnational location.[7] Links to the source documents are provided in the replication code. This step accounts for the remaining 59 percent of the subnational location information in my final dataset.

I drop six countries from the sample for which I did not obtain sufficient location information (see appendix A for more details). The remaining 19 countries in the sample are displayed in table 1. Table 1 also displays the proportion of all relevant aid obligations for which I obtained location information. The proportions range from 0.27 in Burundi to 0.83 in the Central African Republic, with a median of 0.52. Across the entire sample of relevant aid obligations, I obtained subnational location information for 55 percent of the total value (5 billion out of 9.1 billion US dollars). Across the entire sample of *all* aid obligations (not just relevant aid), I obtained subnational location information for 41 percent of the total value. These rates are substantially higher than those of other datasets, for which, as discussed above, subnational location information is typically obtained for 20 percent or less of the total value.[8]

I distribute the total value of each obligation across all subnational locations that I extract for the obligation. In cases where OpenAI was used to count the mentions of each subnational location, I weight the amount assigned to each location based on the number of mentions. Afterwards, I assign the values associated with each location to the corresponding first-level administrative unit.[9]

[7] I provide the OpenAI prompt in appendix A.

[8] Note that comparisons across datasets should be made cautiously, as they cover different donors and time periods, both of which vary in terms of the potential availability of location information.

[9] I use lower-level administrative units for Malawi and Uganda. See appendix A for details.

These are the units of analysis in the study.

To estimate the total amount of aid per administrative unit, I scale the amount assigned to each unit based on the proportion of relevant aid in the country for which I obtained location information (see appendix A for details). The scaled values are used to calculate each unit's aid per gross regional income (GRI),[10] which I use as a measure of how heavily dependent it is on US foreign aid. I describe how I use this measure to construct the main independent variables in the following section.

If we assume that there is a correlation in the geographic areas targeted by different aid agencies, then there should be a correlation between my measure of aid dependence and the value of spending from other donors. As a validation test, I show that this is the case in appendix F.

I also merge my data to the lists of canceled and retained awards described in the background section above. Importantly, the accuracy of the lists is uncertain—both in terms of the status (i.e. canceled or retained) assigned to contracts and whether the lists constitute the universe of ongoing programs prior to disruption (Kenny and Sandefur 2025). Therefore, I do not use this data for most specifications; I instead use all 2024 obligations, as described above. However, the data from the lists provide at least a rough sense of the extent of cancellations by country and unit, so they are useful for exploratory and descriptive analysis.

Figure 1 presents the estimated percentage of USAID programming that was canceled among the countries in my sample. The table shows that the percentages canceled vary substantially, from as a little as 7 percent in Chad to as much as 75 percent in Malawi. However, the variation is driven in large part by the pre-disruption size of the sectors in each country. Indeed, aid outside the health and humanitarian sectors (labeled "Other" in the graph) was virtually eliminated everywhere. In contrast, cancellations to the humanitarian sector are in all cases below 40 percent.

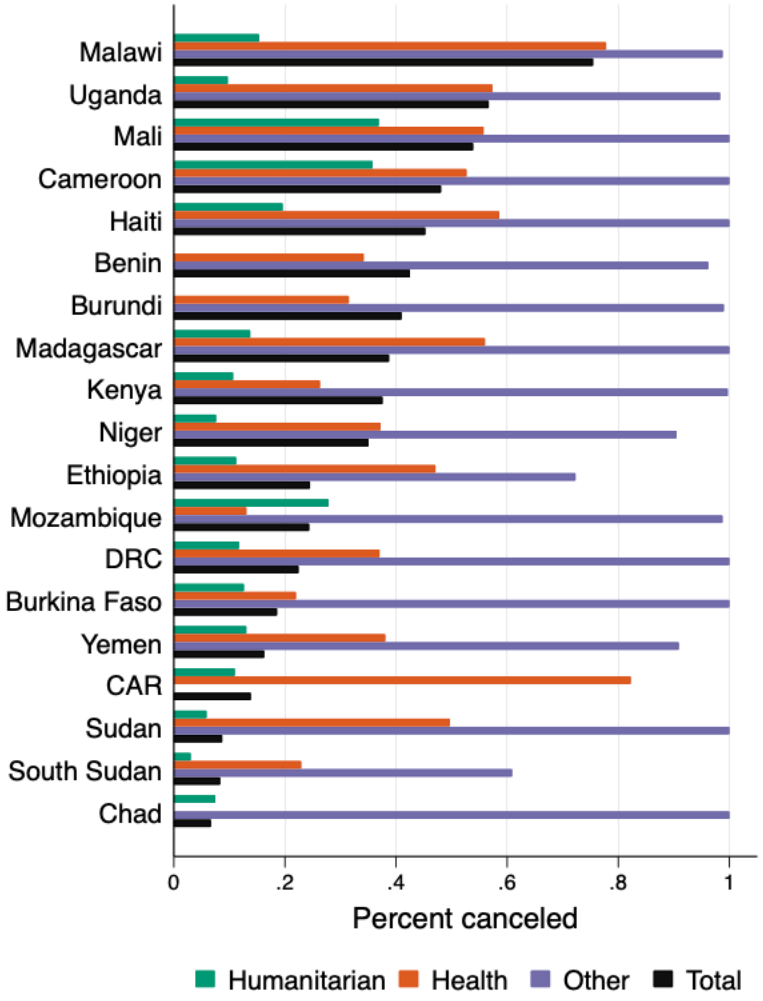
[10] I discuss my approach to calculating GRI in appendix A.

Table 1: Proportion of relevant aid for which subnational location information was obtained, by country

	Proportion extracted
Benin	0.43
Burkina Faso	0.71
Burundi	0.27
Cameroon	0.46
Central African Republic	0.83
Chad	0.33
Democratic Republic of the Congo	0.64
Ethiopia	0.47
Haiti	0.45
Kenya	0.56
Madagascar	0.56
Malawi	0.64
Mali	0.53
Mozambique	0.52
Niger	0.49
South Sudan	0.75
Sudan	0.54
Uganda	0.40
Yemen	0.41
Total	0.55

Note. Proportions are in terms of the dollar value of obligations.

Figure 1: Aid cancellations by country and sector



Note. The value of "Other" for the Central African Republic is missing because there was no "Other" aid on the list of either canceled or retained programs for the country. The same is true for Benin and humanitarian aid. The other bars that are not visible are 0s, not missing.

4.2 Dependent and control variables

The dependent variables for this study are based on ACLED data (Raleigh et al. 2023). I use dependent variables for both political conflict events and fatalities. Political conflict events include battles, violence against civilians, explosions/remote violence, riots, and protests.[11] Figure 2 summarizes the outcome data for 2024, the year prior to disruption.[12] The focus is on events, which is the main outcome used in analysis. The first panel of the figure shows the number of violent events varies substantially across the sample countries, ranging from 10,000 in Sudan to 139 in Malawi. The second panel shows that the type of conflict is also varied. For most countries in the sample, most events are either battles, explosions/remote violence, or violence against civilians—the type of events that most involved armed groups. In other countries, most events are protests or riots, which are typically carried out by civilians. Finally, the last panel shows the breakdown of actors involved in armed-group conflict. In most countries, either rebel of militia forces are involved in a substantial portion of events, and state forces are typically involved at a similar rate. In settings where none of the three actors are prominent, riots and protests are the dominant event types, which involve civilians. The sample thus includes countries facing a variety of different conflict types, of varying magnitudes, and including diverse actors.

I employ several time-varying controls in my study. These include a drought-impact index, an agricultural commodity demand index, a minerals price index, and an oil price index. I discuss data sources and the construction of these indices in appendix A.

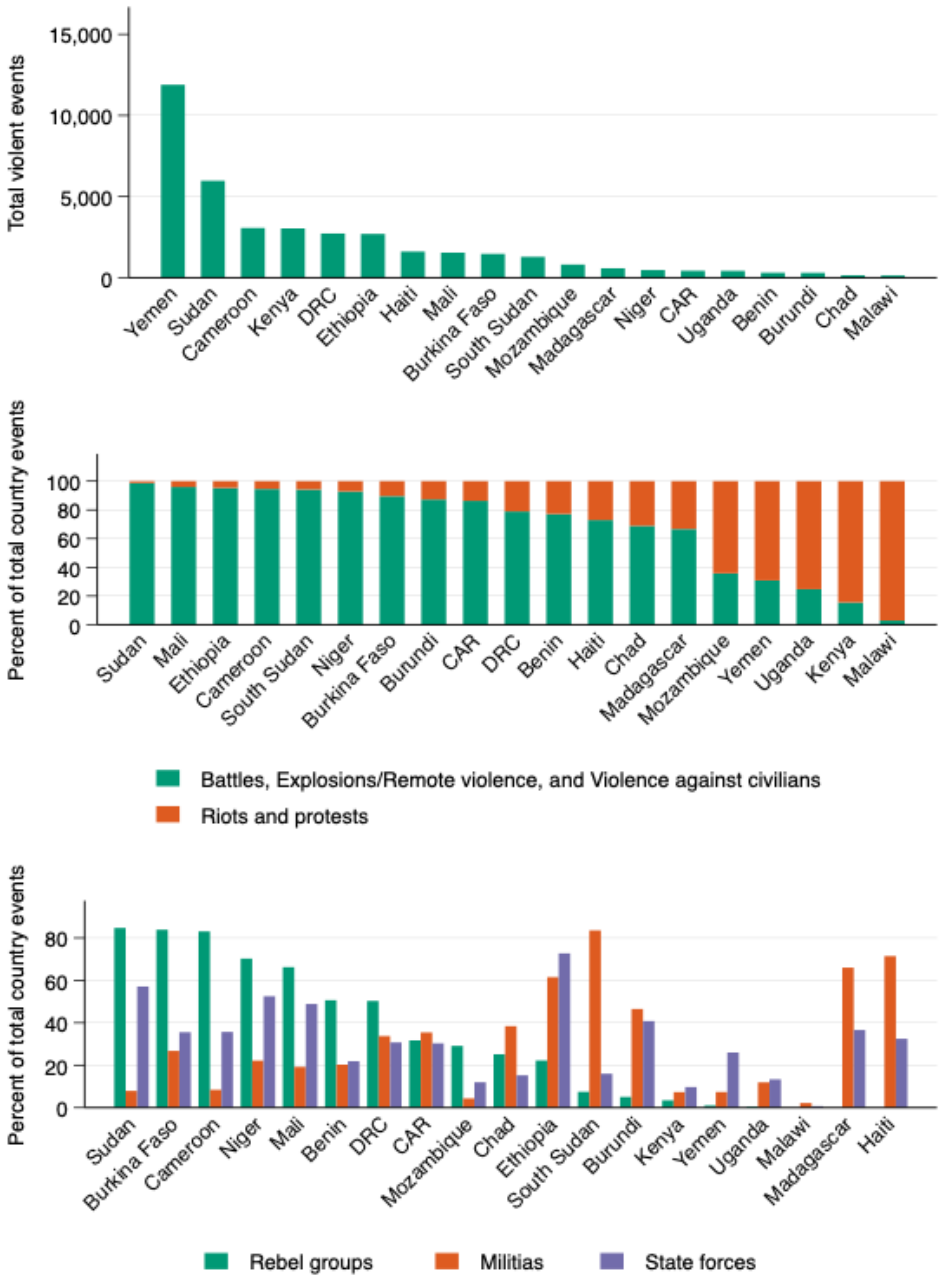
5 Research design

5.1 Identification strategy

To test my hypotheses, I conduct an event study design to compare administrative units that were heavily dependent on US foreign aid immediately prior to the cuts in fiscal year 2024 (aid-dependent units) to units that were less dependent on US foreign aid (non-aid-dependent units). The analysis

[11] Events labeled as "Strategic developments" are excluded from my dataset.
[12] Table C1 in the appendix summarizes the dependent variable numerically for the main sample.

Figure 2: Summary of violent events in 2024 in the sample countries



Note. For the third panel, the percents indicate the percent of the total events that involve the indicated actors *and* which are battles, explosions/remote violence, or violence against civilians.

was pre-registered following dataset construction and prior to data analysis. In appendix G, I document and justify minor deviations from the pre-analysis plan, and I show that the deviations do not meaningfully affect results.

The first assumption with the event study design, which is essentially a differences-in-differences design conducted over multiple periods, is that there would have been parallel trends in levels of conflict between aid-dependent units and non-aid-dependent units. A key fact that bolsters the credibility of this assumption is that the timing of the disruption was exogenous to subnational conflict dynamics in aid-receiving countries. The disruption was initiated on the first day of Trump's second term, suggesting that it was planned in advance and not in response to activities within the aid-receiving countries. I also provide evidence of parallel trends—or at least trends in the opposite direction of the effect—in the periods prior to the disruption.

The second assumption is that there was no anticipation of the massive aid disruption that affected trends in conflict in the period prior to the disruption. Anecdotal evidence supports this assumption. On the one hand, President Trump was openly critical of aid prior to taking office, so some level of funding cuts was to be expected (Lilly 2025). On the other hand, the scale, timing, and abruptness of the disruption was clearly not foreseen. Despite Trump's efforts to reduce the aid budget during his first term, Congress ensured that disbursements to USAID actually increased over his first four years in office (Tamonan 2024). Furthermore, in November 2024, one news outlet dedicated to the aid sector predicted that the new Trump administration would decrease funding for climate, reproductive health, and United Nations agencies, and *increase* funding for local organizations (Loy 2024). Thus, while some level of reorganization was expected, a total disruption was not. Indeed, according to one report, the funding freeze threw "USAID missions and their partners into chaos" (Escritt et al. 2025). Furthermore, I show below that the pre trends indicate little evidence of anticipation.

Importantly, my design does not require aid dependence or the targeting of disruption to be exogenous. Rather, it requires only parallel trends and no anticipation. If those two assumptions hold, the design yields unbiased causal estimates of the impact of the disruption event on conflict in aid-dependent units *relative to non-aid-dependent units*. One additional assumption is required for

these estimates to provide information about the impact of negative aid shocks on conflict: the aid-dependent units must have experienced a substantially sharper reduction in foreign assistance than non-aid-dependent units. In the background section, I demonstrated that the initial aid disruption was indiscriminate and near-comprehensive, so it almost certainly more heavily impacted units with more aid in 2024. Furthermore, for my main analysis sample, the value of aid per GRI canceled was 0.8 percentage points greater in aid-dependent units on average than non-aid-dependent units (classification of aid-dependence is described below). This is a large difference; 0.8 is about 1.1 standard deviations and twice the mean.

5.2 Estimation

To reduce issues with multiple testing, I pre-registered that I would have a single specification to test my main hypothesis (H1) and that all other specifications would be exploratory. The main specification is based on the following equation:

$$Y_{ict} = \alpha + \sum_{j=2}^{J} \beta_j (Lead\ j)_t \times Aid\text{-}dependent_i + \sum_{k=0}^{K} \gamma_k (Lag\ k)_t \times Aid\text{-}dependent_i + \lambda_i + \mu_{ct} + \epsilon_{ict} \tag{1}$$

Y is the inverse hyperbolic sine of political conflict events in administrative unit i, in country c, during period t. *Aid-dependent* equals 1 (aid-dependent) if the total scaled value of aid for administrative unit i is at least 2 percent of GRI. It equals 0 (non-aid-dependent) if the total scaled value of aid for the unit is less than 1 percent of GRI. The units for which aid is between 1 and 2 percent of GRI are left out of the sample. *Lead* j is an indicator that equals 1 for the jth period before the start of the aid disruption; *Lag* k is an indicator that equals 1 for the kth period after the start of the aid disruption and zero otherwise, with $k = 0$ being the first period including the aid disruption and *Lead* 1 omitted as the reference category; λ represents administrative unit fixed effects; and μ represents country-period fixed effects. The γ_k coefficients provide estimates of the disruption on conflict. The β_j coefficients test the plausibility of the parallel trends assumption. I cluster standard errors at the administrative unit level.

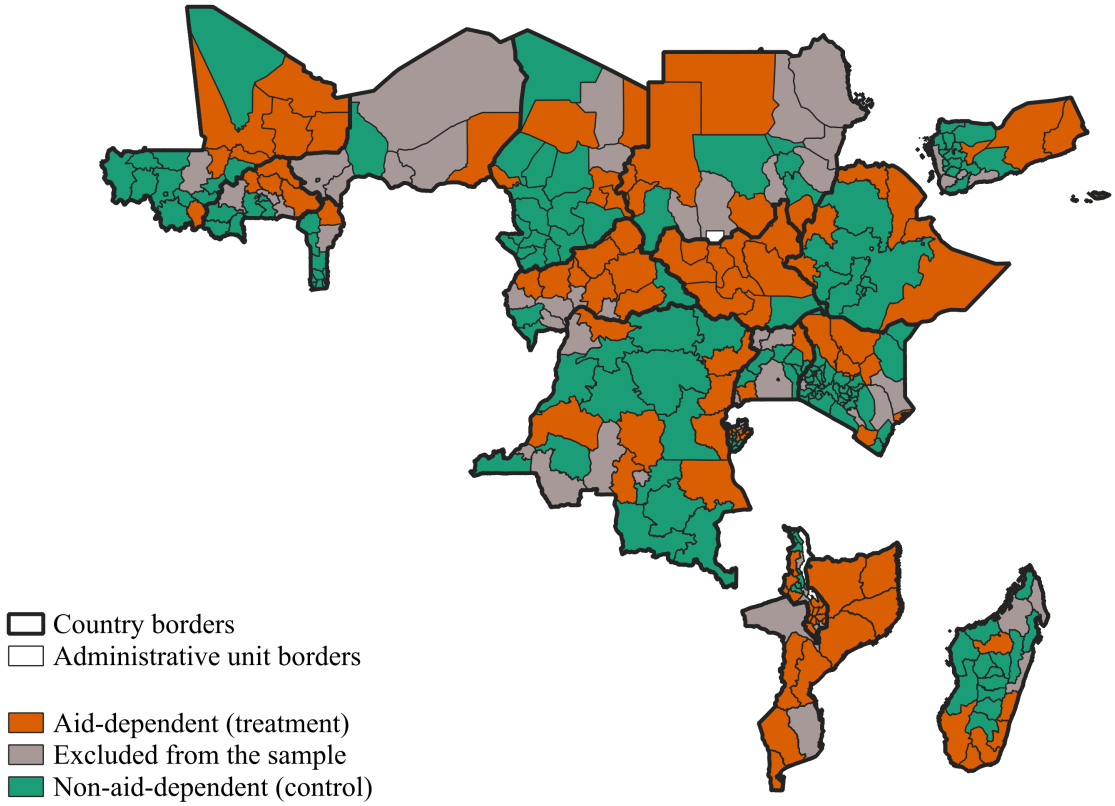
Periods are 6-calendar-month intervals. I use calendar months because they align with the coarsest temporal measurement of conflict in the ACLED data. Period $t = -1$ runs from July 1, 2024 to December 31, 2024. Period $t = 0$ starts on February 1, 2025, shortly after the initial pause to aid, and continues to July 31. Thus, January 2025 is omitted. There are no other gaps between periods. Note that period -1 is the first period prior to the disruption. Period 0 contains the entire initial near-total freeze, the cancellations in March, and the subsequent broad-based disruption, which continued at varying magnitudes until around mid June. By period 1, which starts on August 1, the initial disruption had ended but many programs were still canceled. I pre-registered that I would include periods from $t = -6$ to $t = 1$. As an additional exploration, I include pre periods that extend 10 years prior to the disruption, back to the start of 2015.

I use a binary classification for *Aid-dependent* to mitigate the effects of measurement error in the dataset and because it simplifies the event study analysis and makes interpretation clearer (de Chaisemartin et al. 2022). I leave out units for which aid is between 1 and 2 percent of GRI to further mitigate concerns around measurement error and ensure that aid-dependent units are compared to units that are substantially less dependent on aid, which makes them a suitable "control" group.

The administrative unit fixed effects control for time-invariant confounders and make it such that I am examining changes in the outcome within administrative units over time. The country-period fixed effects control for shocks over time that are common to each country and make it such that I am comparing changes within each administrative unit to other units within the same country. This approach strengthens the plausibility of the parallel trends assumption, as it only requires parallel trends among units within the same country.

My sample includes 115 aid-dependent (treatment) units and 163 non-aid-dependent (control) units. The units and their treatment status are displayed in figure 3. Note that two countries in the sample, Cameroon and Haiti, are not included in this main analysis. Since Cameroon has no treatment units and Haiti has no control units, the *Lead* $\times$ *Aid-dependent* and *Lag* $\times$ *Aid-dependent* variables for these countries are perfectly collinear with the country-period fixed effects. The countries are, however, included in other exploratory specifications that use different definitions of aid-dependence (see table C2 in the appendix for a breakdown of treatment assignments across

Figure 3: Dependence status for units in the main specification



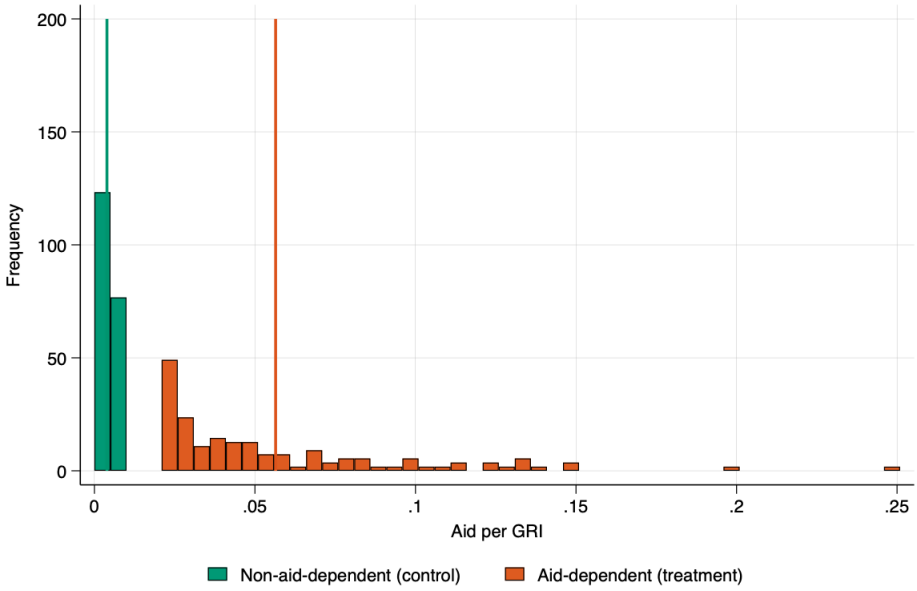
Note. The only control unit for Mozambique is Maputo City, which is not clearly visible on the map.

different definitions).

Figure 4 displays the pre-disruption aid per GRI for units in the sample, by treatment status. It shows that the average treatment unit had substantially more aid per GRI on average. Some treatment units, with aid accounting for over 10 percent of GRI, were clearly much more dependent than their control group counterparts.

In addition to my main specification, I conduct several exploratory specifications to unpack the results. These include specifications with longer pre-trends, shorter periods, alternative dependent variables based on event and actor type, and country-level estimates.

Figure 4: Pre-disruption aid per GRI for units in the sample, by dependence status



Note. The vertical lines are the means for each distribution.

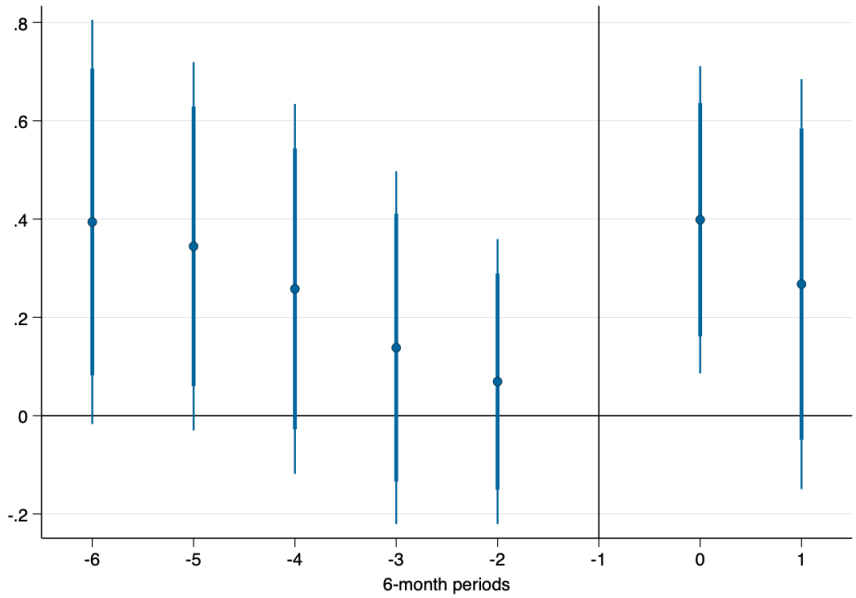
6 Results

6.1 Main test

The main results are presented in figure 5. They show that trends in aid-dependent and non-aid-dependent units were relatively parallel in the periods prior to the disruption. And although there is some violation of parallel trends at $p < 0.05$ in the pre period, the aid-dependent units were trending less violent relative to non-aid-dependent units. Then, immediately following the disruption, there was a sharp increase in conflict in aid-dependent units relative to non-aid-dependent units ($p = 0.001$). These findings provide evidence that the aid disruption caused an increase in conflict, which supports H1. However, the effects appear to persist only in the short term. In the second 6-month period, the effect attenuates and is only marginally significant ($p = 0.097$).

The coefficient for period 0 indicates that, relative to non-aid-dependent units, aid-dependent units experienced a 0.4 increase in the dependent variable, the inverse hyperbolic sine of violent events. For a unit with an average number of violent events in the pre-disruption period (48.6

Figure 5: Main event study results



Note. 99 and 95 confidence intervals are displayed. Results are based on equation 1. The β and γ coefficients are displayed. The outcome variable is the inverse hyperbolic sine of conflict events. The table version of the results are in table B1 in the appendix.

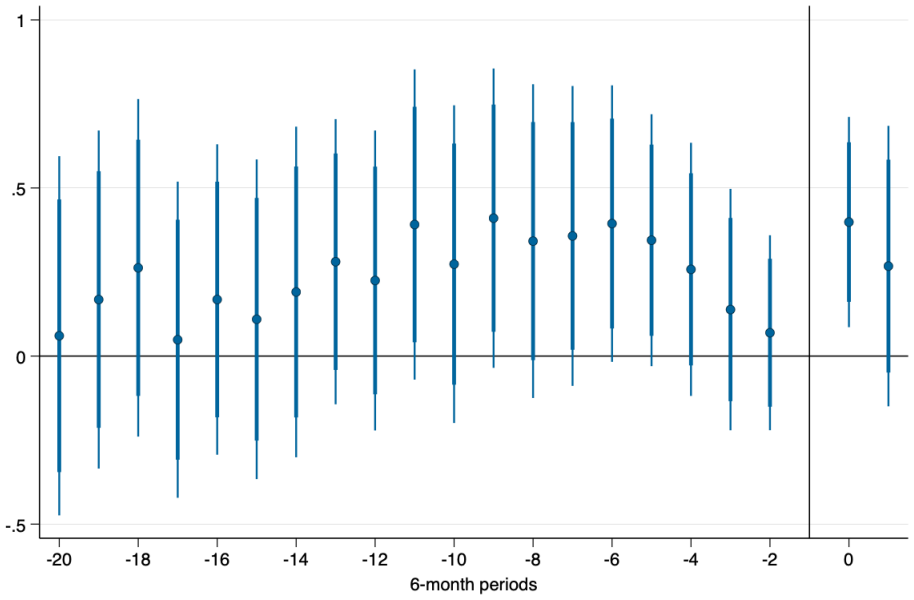
events), a 0.4 increase in inverse hyperbolic sine conflict events translates into an increase of 19.4 events—a 40 percent increase or 0.14 control-group standard deviations.

The confidence intervals for these results assume that parallel trends hold exactly, which is not likely. However, a sensitivity analysis following Rambachan and Roth (2023) indicates that even if we restrict the post-treatment violations of parallel trends to be no larger than the maximal pre-treatment violation of parallel trends, the result is still significant at $p = 0.05$, with a 95 percent confidence interval of [0.016, 0.801].

6.2 Exploratory tests

Figure 6 further strengthens the credibility of the findings. It displays pre-trends for 10 years prior to the disruption. It shows that, while trends were not always parallel, changes occurred smoothly over time. The clear relative increase in conflict following disruption marks a sharp departure from

Figure 6: Event study results with extended pre trends



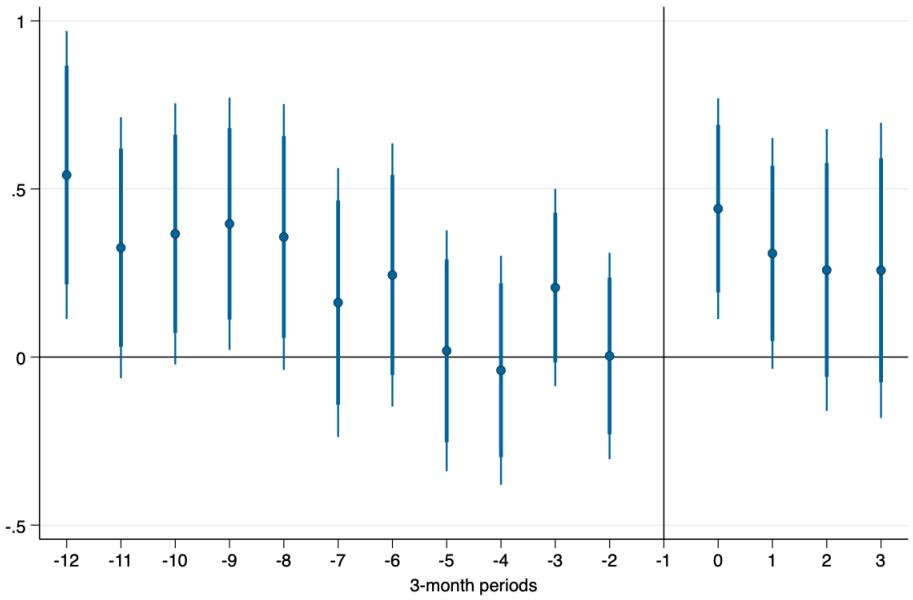
Note. 99 and 95 confidence intervals are displayed. Results are based on equation 1. The β and γ coefficients are displayed. The outcome variable is the inverse hyperbolic sine of conflict events. The table version of the results are in table B1 in the appendix.

previous trends.

Figure 7 presents results using 3-month periods. It makes clear that the effects were strongest immediately following the initial disruption and attenuated steadily over time. These findings indicate that the increase in conflict was driven more by the initial freeze and delays than the subsequent cancellations. Consistent with this interpretation, table D1 in the appendix shows that units that were more heavily affected by cancellations did not experience a relative increase in conflict. However, these results must be interpreted cautiously given the potentially endogenous nature of cancellations.

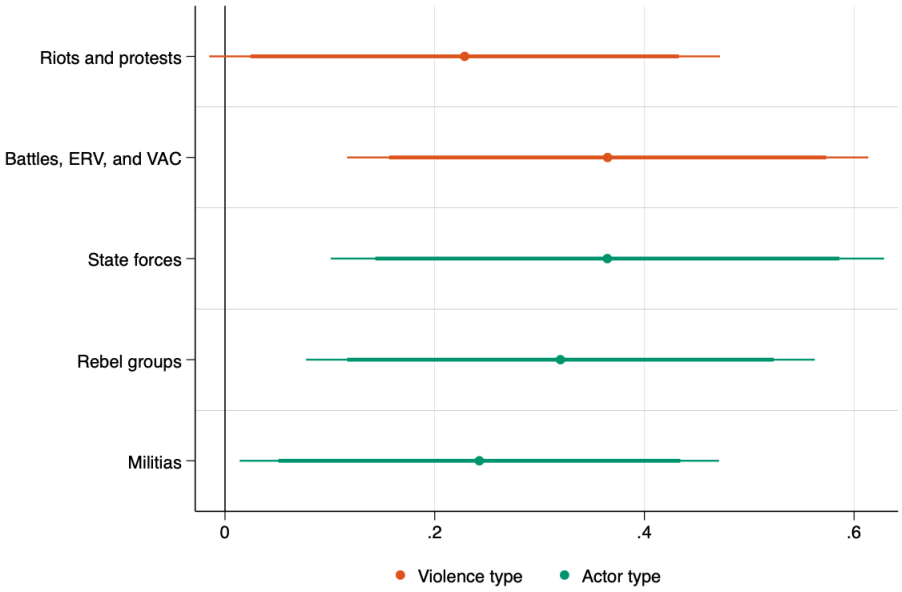
To explore what is driving the spike in conflict in period 0, figure 8 displays the results using various dependent variables. Each coefficient is for the *Lag* 0 × *Aid-dependent* variable for a distinct regression. For the orange coefficients, the dependent variable is the inverse hyperbolic sine of the count of specific types of conflict. The first coefficient indicates that aid-dependent units

Figure 7: Event study results using 3-month periods



Note. 99 and 95 confidence intervals are displayed. Results are based on equation 1. The β and γ coefficients are displayed. The outcome variable is the inverse hyperbolic sine of conflict events. The table version of the results are in table B1 in the appendix.

Figure 8: Disaggregation of effects by type of conflict and actor

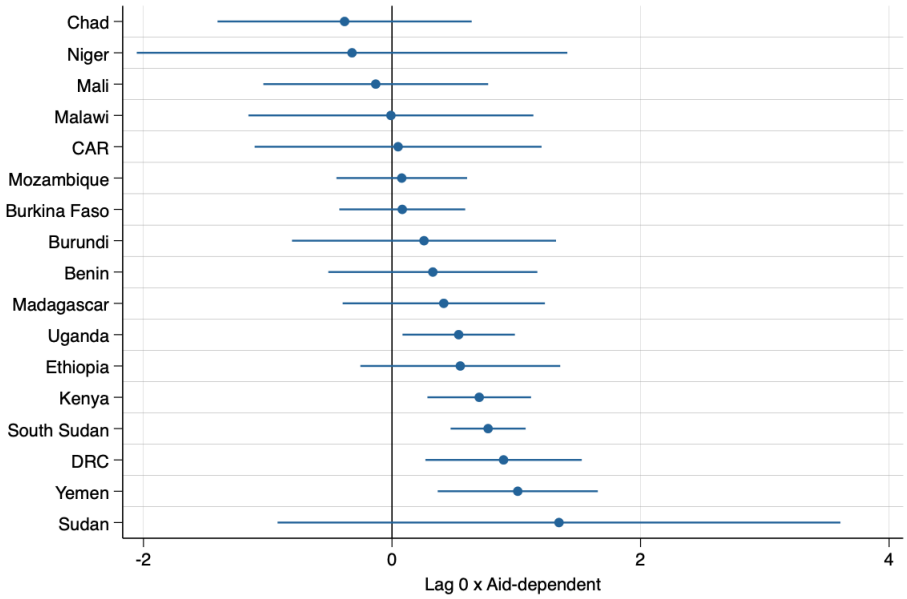


Note. 95 and 90 confidence intervals are displayed. Results are based on equation 1. Each coefficient is for the *Lag* 0 × *Aid-dependent* variable for a separate regression. Each regression uses a distinct dependent variable. For the orange coefficients, the dependent variable is the inverse hyperbolic sine of the type of violent events indicated on the y axis. For the green coefficients, the dependent variable is the inverse hyperbolic sine of battle, ERV, and VAC events involving the actor indicated on the y axis. ERV = explosions/remote violence. VAC = violence against civilians. The table version of the results are in table B2 in the appendix.

experienced a marginally significant ($p = 0.066$) relative increase in riots and protests—types of conflict that are typically instigated by civilians. The second coefficient indicates that aid-dependent units experienced a clearly significant ($p = 0.004$) relative increase in battles, explosions/remote violence, and violence against civilians—types of conflict that typically involve armed groups. To examine what is driving this increase, I breakdown the effect on armed group conflict by type of actor. For the green coefficients, the dependent variables are the inverse hyperbolic sine of battles, explosions/remote violence, and violence against civilians for a distinct actor. The results indicate that aid disruption caused a significant increase in violent activity involving state forces, rebel groups, and militias.

Figure 9 displays country-level estimates. The coefficients in the plot are for the *Lag* 0 × *Aid-dependent* variable, each from a separate regression restricted to a single country. Many of

Figure 9: Country-level estimates



Note. The coefficients are for the *Lag 0* × *Aid-dependent* variable, each from a separate regression restricted to the country labeled on the y axis. The regressions are based on equation 1, with period instead of country-period fixed effects. The table version of the results are in tables B3 and B4 in the appendix.

the coefficients are estimated on small samples and are thus imprecise, so the results should be considered strictly exploratory. As one would expect, the more violent countries at baseline tend to have larger coefficients, as they have more potential for large swings in levels of conflict. But there is not a clear correlation between the magnitude of the effect and the type of conflict in the country (as displayed in figure 2). For example, some of the largest effects are in Yemen and Kenya (where riots and protests are most prevalent) and Sudan and South Sudan (where armed-group conflict is most prevalent). I show in appendix E that results are robust to dropping one country from the sample at a time, which indicates that the effects are not driven by a fluke spike in conflict in aid-dependent units in a single country.

I also show in appendix E that the results are robust to a variety of other specifications. These include using the inverse hyperbolic sine of fatalities as the dependent variable; assigning aid dependence without a gap between thresholds, such that no units are dropped from the sample based on

treatment assignment; using period fixed effects instead of period-country fixed effects, which prevents any countries from being dropped from the sample; and including the time-varying controls discussed in the data section.

6.3 Discussion

Mechanisms

Of the seven mechanisms discussed in the theory section that could explain an increase in conflict following a negative aid shock, which are consistent with the results? Clearly, only mechanisms that operate in the short term are relevant, as the effects appear very shortly after the disruption began and diminished over time. Thus, the commitment problem mechanism proposed by Findley (2018) and mechanisms related to recruitment into armed groups are not consistent with results.

It is also unlikely that the relative increase in conflict was driven by a disruption to aid designed to build state capacity to counter armed groups. Much of this aid involves activities with longer-term effects, such as trainings. Other capacity-building aid involves the provision of resources. But disruptions in material provision would only explain the spike in conflict if security forces were dependent on the regular and consistent provision of materials to contain conflict. For the 2024 data for my sample, only two obligations involve the explicit provision of resources: armored personnel carriers in Niger and armored security vehicles in Kenya. It is unlikely that such provisions would be regular and consistent. Direct funding support in terms of weapons, ammunition, and salary might have a more short-term effect. However, the obligations in the data that could feasibly fit this category—such as a counter-narcotics project in Malawi to "strengthen capacity of front-line actors"—are present in only 5 countries and total 9.6 million dollars, about 0.1 percent of the total value of relevant aid. It is also possible that the aid shock caused a reorganization of government finances away from security and toward more severely affected sectors, but this would be unlikely on a very short time frame.

Another unlikely mechanism is the removal of peacebuilding aid, which is designed to directly reduce conflict rather than build the capacity of the government to do so. However, for my sample

of countries, every single Peace and Security aid contract was canceled. This fact is not consistent with the timing of effects. If disruption to peacebuilding projects drove the increase in conflict, we would expect the effects to persist beyond the initial pause since the projects did not resume.

I have already mentioned that the increase in conflict was not likely driven by recruitment into armed groups. Mechanisms related to recruitment could include economic incentives, improving attitudes toward the state, and resentment from withdrawal. However, each of these mechanisms could also drive *participation* in conflict, which could affect conflict in the short term. Specifically, if disruption to aid increases poverty and thus the marginal benefit of conflict, then young men that comprise informal community militias in places like South Sudan may be more willing to instigate raids to win access to land or cattle. And if disruption to aid creates resentment or undoes any progress in improving attitudes toward the state, civilians may be more likely to participate in riots and protests against the state. However, if we assume that membership in rebel groups is fixed in the short term and these groups are already committed to contesting the state, we would not expect changes in opportunity costs or attitudes to impact these groups' short-term actions.

Thus, the most likely explanation remaining for the increase in rebel group conflict is the signaling mechanism. By this mechanism, the disruption creates the perception among armed groups that the strength is weak, which creates a strategic incentive to attack. This mechanism is consistent with the short-term spike in rebel-group conflict, as my theory suggests that they have an incentive to strike while their advantage is at a maximum. The decrease in conflict over time is also consistent with my claim above that the actual change to the capacity of state security was minimal. If rebel groups learn through confrontations that state strength is virtually unchanged, then we should expect levels of conflict to return to pre-disruption levels. This mechanism could also apply to militia groups in contest with the state.

Table 2 summarizes the mechanisms, their type (aid shock or change in aid levels), and the types of conflict for which they are consistent with the evidence. To be clear, my design does not establish causal evidence for any of these mechanisms. Rather, it provides suggestive evidence based on which mechanisms are consistent with the patterns of effects.

Table 2: Summary of mechanisms that can (or cannot) explain the increase in conflict

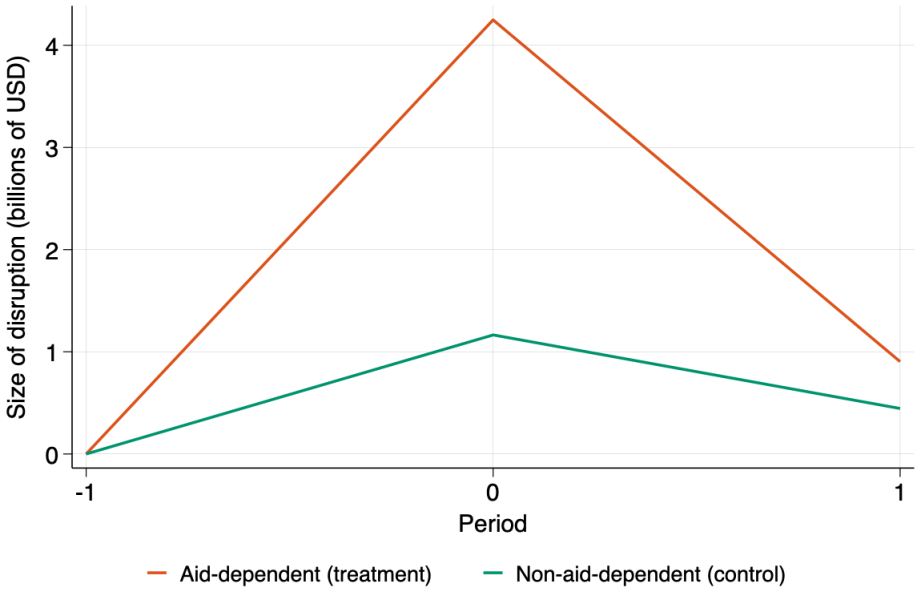
Mechanism	**Type of mechanism**	**Consistent with the evidence for which types of conflict?**
Economic incentives	Levels	Militia-based conflict over resources
Attitudes toward the state	Levels	Riots and protests
State capacity aid	Levels	None
Peacebuilding aid	Levels	None
Resentment of withdrawal	Shock	Riots and protests
Signal of state weakness	Shock	Armed group conflict with the state
Findley (2018) commitment problem	Shock	None

Magnitude and duration

I have estimated that aid-dependent units experienced a 0.4 relative increase in inverse hyperbolic sine violent events. Based on a counterfactual analysis, this means that had the aid-dependent units maintained the same trends in conflict as the non-aid-dependent units, there would have been 598 fewer violent events in my sample countries in the 6 months following disruption. But this is likely an underestimate of the overall impact of disruption in my sample country for two reasons. First, some administrative units are excluded from the sample (see figure 3), and these are not included in the counterfactual analysis. Second, most of the "control" group, units for which aid per GRI is less than one percent, still received substantial amounts of aid and were thus affected by the disruption. Therefore, we should think of the relative increase in conflict in aid-dependent units as an additional effect on top of some unknown effect in non-aid-dependent units.

To help make sense of the duration of effects, figure 10 presents estimates for the magnitude of the disruption by treatment group over time. For period 0, which includes the initial near-total freeze and subsequent broad-based disruption, I assume all programs were disrupted. To calculate the total value of disruption, I use the amount of all canceled and retained contracts. By period 1, most retained projects had resumed, so I calculate the total value of disruption as the value of all canceled programs. In period 0, the total estimated disruption to aid-dependent units was 3.1 billion dollars greater than the disruption to non-aid-dependent units. In contrast, in period 1, the

Figure 10: Magnitude of disruption over time, by treatment group



total estimated disruption to aid-dependent units was only 0.5 billion dollars greater.

Based on these trends, there are various possible explanations for the short duration of effects. For the mechanisms related to levels of aid, the short duration could be explained by the possibility, suggested by figure 10, that the size of the disruption in aid-dependent units converged over time to the size of the disruption in non-aid-dependent units. This would suggest that only large changes in levels of aid impact the magnitude of conflict. For the mechanisms related to shocks, one possibility is that the resumption of aid dampened the shocks by mitigating resentment or signaling a renewed strength of the government. Another possibility is that the shock mechanisms have a fleeting effect regardless of whether aid resumes. For example, for the signaling mechanism, misperceptions of weakened state capacity could be corrected over time through conflict. For the resentment of withdrawal mechanism, previous research has suggested that collective action for protests is most common in the immediate aftermath of shocks (Correa et al. 2025).

There are two additional potential explanations for the short duration that are less likely. One is that humanitarian aid, which was less affected by cancellations, was more relevant for conflict. Since humanitarian aid was still heavily affected in the short run but recovered more in the long

run, this explanation could account for the short-run impacts on conflict. However, I show in table D1 in the appendix that there was a short-run relative spike in conflict in both humanitarian-aid-dependent *and* non-humanitarian-aid-dependent units—despite the fact that I find no correlation between humanitarian-aid dependence and non-humanitarian-aid dependence.[13] These results also hold when I control for the other type of aid (e.g., the effect of humanitarian-aid dependence holds when controlling for levels of non-humanitarian aid). Another possibility is that, over time, other donors filled the gaps created by the withdrawal of US foreign aid. However, this explanation is unlikely because funding from other OECD countries, which comprise most major donors, also decreased in 2025 (albeit less abruptly and to a lesser extent) (Donor Tracker 2025). And although donations to private charities grew substantially, the total increase pales in comparison to the disruption to US aid (Beaty 2025).

6.4 Alternative explanations

In this subsection, I provide evidence against a range of alternative explanations for the relative increase in conflict in aid-dependent units. One possible explanation is that aid-dependent units experienced a spike in conflict precisely because they received more aid. In other words, the aid itself caused an increase in conflict. This explanation is consistent with findings from some previous studies (e.g., Nunn and Qian 2014). However, it is not consistent with the timing of effects. This alternative explanation would be most likely if the aid-dependent units in my study *became* aid-dependent in 2024. Although I cannot examine this possibility using my own data, I show in appendix F that (1) the aid-dependent units did not experience a relative spike in aid funding from other donors in 2024 and (2) there is a strong correlation between aid-dependence based on my data and the amount of funding from other donors. Moreover, even if the units in my study did become aid-dependent in 2024, this alternative explanation would still not explain the trends. If aid caused a spike in conflict, we would expect the spike to occur prior to disruption. Indeed, the main channels through which aid is theorized to increase conflict require that aid is actually present; we would

[13]The lack of correlation is consistent with the idea that development and humanitarian aid often target different areas (Findley 2018).

not expect lingering effects after aid has disappeared. However, we see no spike in relative conflict in 2024. We also see that the increase in conflict was most pronounced immediately following the initial disruption, during which US foreign aid was virtually eliminated.

A related explanation is that aid was targeted in 2024 at units that were expected to become more violent in 2025. However, the pre-trends in conflict suggest that the aid-dependent units were trending less violent prior to the disruption, so it is unlikely that an uptick in conflict was expected—particularly given the difficulty of predicting conflict (Bazzi et al. 2022). This alternative explanation is also undermined by the evidence presented in appendix F, which, as mentioned above, suggests that the aid-dependent units in my study did not start receiving relatively more aid in 2024.

Other alternative explanations relate to other actions taken by the Trump administration at the same time as the disruption. For one, Trump announced plans for new tariffs on the first day of his second term, the same day the aid disruption began. Throughout the course of March and April, the administration imposed a series of tariffs that impacted the countries in my sample to varying degrees. Importantly, my main specification controls for any country-level shocks caused by the tariffs via country-period fixed effects. But if the tariffs affected aid-dependent units in a given country more than other units within the same country, then they could potentially explain the relative increase in conflict. To account for this possibility, in table E1 in the appendix, I run a version of equation 1 that controls for unit-level openness to trade. The results are robust to including these controls, so it is unlikely that the tariffs drove the effect.

The administration could have also driven the effects by cutting funding to United Nations peacekeeping missions. Since peacekeeping has been shown reduce conflict (Walter et al. 2021), if peacekeeping missions were more likely to be in aid-dependent regions, then such funding cuts could explain the results. However, the administration did not cancel funds for peacekeeping until August 2025 (Nichols 2025). Figure 11 shows the number of peacekeeping personnel over time for the missions in the sample countries. Only three countries in the sample had large missions, and it is clear that the cuts did not cause a noticeable decrease in personnel until August, September, and October, respectively. Thus, cuts to peacekeeping cannot explain the relative spike in conflict

in early 2025.

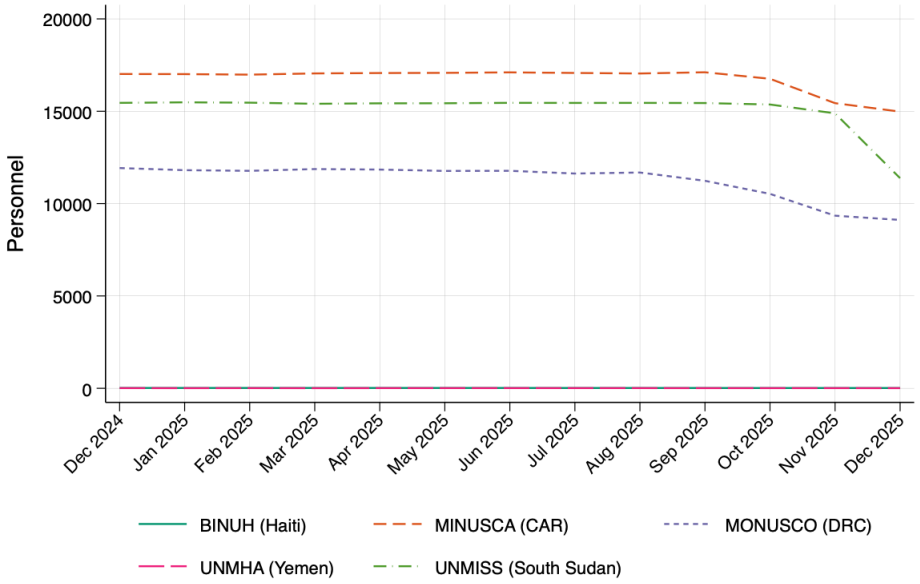
The Trump administration could have also driven the effects through direct military action. However, among the countries in my sample, the only country in which the administration conducted strikes in early 2025 was Yemen (McGowan et al. 2026). I show in appendix E that the results are robust to dropping Yemen from the sample. A related possibility is that the anticipation of military strikes influenced armed group actions. If units that were most likely to expect strikes were also more or less likely to be aid-dependent, this could explain the results. As a placebo test, in table D2 in the appendix, I conduct a version of equation 1 with a proxy of expectation of strikes substituted for *Aid-dependent*. The proxy is a binary measure of pre-sample armed-group conflict. I show that the areas that were most likely to expect strikes did not experience a change in conflict relative to less violent areas following the aid disruption. It is therefore unlikely that the results are driven by anticipation of military action by the Trump administration.

Finally, it is important to note that other donors also reduced their aid funding in 2025 (Vigersky 2025; Donor Tracker 2026; Brien and Loft 2025). If these cuts were more concentrated in my treatment units and happened to occur at the same time as the US disruption, it would amplify my estimates, but I would still be estimating the effect of aid cuts. If they were more concentrated in control units, it would only make my estimtes more conservative.

7 Conclusion

In this paper, I have provided causal evidence from the disruption to US foreign aid that large, abrupt negative aid shocks cause an increase in political conflict among a range of actors and in a range of different conflict-affected settings. I have provided suggestive evidence that the mechanisms are most likely related to worsened attitudes toward the state or increased resentment among civilians, economic incentives for militias, and, according to a novel theory presented in this paper, strategic incentives for rebel groups and in some cases militias. Finally, I have argued that the short duration of effects could be explained by either the resumption of many aid programs over time or the possibility that the effects of shocks are short-term by nature. As a whole, the

Figure 11: UN peacekeeping personnel over time, by mission



Note. Based on data from United Nations Peacekeeping (2026).

research creates a more complete understanding of how aid shocks impact conflict.

Although the scale of the 2025 disruption to foreign aid may have been unique, large negative aid shocks are common (Cheeseman et al. 2024; Raddatz 2007). They are also becoming increasingly relevant. For example, following peak levels of funding during the COVID pandemic, humanitarian funding from Germany, the world's second-largest humanitarian donor, fell by 76 percent from 2022 to 2025 (Vigersky 2025). In 2025, France reduced its aid budget by about 40 percent (roughly 2.7 billion dollars). In 2026, it reduced it by another 16 percent (about 820 million dollars) (Donor Tracker 2026). In early 2025, the United Kingdom announced it would reduce its aid budget from 0.5 percent of GNI to 0.3 percent by 2027 (about 7 billion dollars) (Brien and Loft 2025). Crucially, when many donors reduce funding simultaneously in this manner, it can amount to a very large shock. And although the shocks from these donors will likely be less sudden than the recent US disruption, they will still require abrupt withdrawals over a short period of time.

The clearest policy takeaway from this work is that sudden large withdrawals of aid by donors intensify various forms of political conflict in recipient countries. This indicates that using aid

suspensions as a foreign policy tool—a common approach by donor countries for goals ranging from democratization to sanctioning leaders (Cheeseman et al. 2024)—has tradeoffs beyond the direct humanitarian effects. It also suggests that large slashes to aid budgets, such as the recent and planned cuts to foreign assistance among various donors, could lead to spikes in political conflict.

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Appendix

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A: Dataset construction: Additional details

Relevant aid

I only include aid in my dataset that could conceivably impact conflict in the short-to-medium-term and which is specific to certain areas of a given country. I refer to this as "relevant aid." The following types of aid are not included:

- Overhead, such as program support aid
- Budget support
- Government capacity strengthening, except support to security agencies
- Support to national government agencies
- Activities designed to facilitate long-term private sector investment
- Aid for improving markets in the long-term
- System development (including for legal, health, WASH, agriculture, education, supply chain, and information systems), which is a long-term process
- Revenue mobilization
- Research

  For some obligations, the determination of whether it was relevant or not was somewhat subjective. Importantly, these decisions were made prior to pre-registration. To ensure that no changes were made following pre-registration, I included in the pre-analysis plan a table with the proportion of the value of aid for which subnational information was extracted from each country. If I had changed any coding decisions in the dataset construction, these proportions would have changed. Table A1 reproduces this table using the dataset used for analysis. For reasons discussed in appendix G, the table includes fiscal year 2025, but I drop fiscal year 2025 from analysis.

Table A1: Proportion of relevant aid for which subnational location information was extracted, by country, including fiscal years 2024 and 2025

	Proportion extracted
Benin	0.426
Burkina Faso	0.713
Burundi	0.269
Cameroon	0.464
Central African Republic	0.828
Chad	0.330
Democratic Republic of the Congo	0.640
Ethiopia	0.466
Haiti	0.451
Kenya	0.563
Madagascar	0.561
Malawi	0.638
Mali	0.533
Mozambique	0.516
Niger	0.486
South Sudan	0.750
Sudan	0.537
Uganda	0.403
Yemen	0.412

Note. Proportions are in terms of the dollar value of obligations.

OpenAI prompts

To extract subnational location information from the activity descriptions provided by ForeignAssistance.gov, I use the following OpenAI prompt: "Extract subnational place names (cities, states, etc.) and the type of place from the provided text. Return them as a semicolon-separated list. Place names and place types should be separated by a comma. For example, if one entry mentions that a program was conducted in Unity and Lakes states, the output should be 'unity, state; lakes, state'. If the type of place cannot be inferred, then the type of place name is 'unknown'. If no place names are found, return 'None'. Do NOT extract country names. If only country names are found, return 'None'."

To count the number of mentions of each subnational location in reports, I use the following OpenAI prompt:

- **Task:** Count the number of mentions of each place name in this document.
- **Definition of place name:** An explicitly written geographic unit, including but not limited to a region, city, or camp.
- **Required procedure:**
  1. Extract a verbatim list of all place-name mentions.
  2. Count occurrences using only that extracted list.
- **Inclusion criteria:**
  - Count mentions appearing in paragraphs or tables related to humanitarian or development assistance, broadly defined.
  - A mention qualifies if it appears anywhere within such a paragraph or table.
- **Exclusion criteria:**
  - Do not count mentions in headings, captions, maps, or figure titles.
  - Do not infer place names; only count those explicitly written.
  - Do not include country names.

- **Counting rules:**
  - Count each textual occurrence separately, even if repeated in the same paragraph.
  - Normalize possessives and plurals to one canonical form.
  - Do not include zero counts.
  - Do not count any place name not explicitly extracted in step 1.
- **Output format:**
  - One single line listing each place with its count indicated, formatted as:

    `place name, place type, #; place name, place type, #`
  - Do not end the list with a semicolon.
  - The output should be on ONE line.
  - Do not provide commentary.

Dropping countries

If the value of aid obligations for which I obtained location information was less than 20 percent of the total value of relevant aid for a given country, I dropped the country from the sample. If the Herfindahl–Hirschman Index for obligations for which I obtained location information was greater than 0.5 (which means a small number of obligations would drive treatment assignment), I dropped it from the sample. The countries dropped for either reason are Afghanistan, Guinea, Honduras, Somalia, Syria, and Venezuela.

Units of aggregation

For most countries in my sample, I aggregate subnational location obligations at the level of the first administrative unit. The two exceptions are Malawi and Uganda. For Malawi, I use the second administrative level because the first level is very coarse and the second level is frequently discussed in project documents. For Uganda, I use an informal level of administrative units (sub-regions) that falls between the first (very coarse) level and the second level. I use sub-regions over the second level because sub-regions are more often mentioned in project documents.

Calculating gross regional income

I calculate gross regional income as the country's gross national income times the proportion of the country's population in the unit. I use data from Malynowsky (2025) on subnational population for the first administrative unit, using the most recent year available. I do this for all countries except Mali (because the Malynowsky (2025) data uses coarser units) and Malawi and Uganda, since I do not use their first administrative levels as the unit of analysis. For Mali, I use data from the Malian National Institute of Statistics for 2023 (Institut National de la Statistique 2023). For Malawi, I use data from the 2018 census (National Statistical Office of Malawi 2019). For Uganda, I use data from the 2024 census (Uganda Bureau of Statistics 2024).

Scaling aid assigned to each administrative unit

To estimate the total amount of aid per administrative unit, I scale the amount assigned to each unit through the process of extracting location information. I use the following formula to obtained the scaled value:

$$Scaled_{ic} = Initial_i \times \frac{Extracted_c}{Total_c} \tag{2}$$

Where $Initial_i$ is the initial value of aid obligations assigned to unit i through the process of extracting location information, $Extracted_c$ is the value of relevant aid obligations in country c for which I obtained location information, and $Total_c$ is the total value of relevant aid obligations for country c. For analyses involving the cancellation data, $Initial_i$ is the initial amount of canceled aid obligations assigned to unit i through the process of extracting location information, $Extracted_c$ is the value of relevant aid obligations on either the canceled or retained list for country c for which I obtained location information, and $Total_c$ is the total value of relevant aid obligations on either the canceled or retained list for country c. For analyses involving sector-specific aid, $Initial_i$ is the initial amount of aid obligations for a given sector assigned to unit i through the process of extracting location information, $Extracted_c$ is the value of relevant aid obligations for the given

sector for country c for which I obtained location information, and $Total_c$ is the total value of relevant aid obligations for the given sector for country c.

Control variables

I use five different control variables in my robustness checks. The first four are time varying. The last is interacted with the lead and lag indicators.

First, I use a drought impact index. Following Harari and Ferrara (2018), I calculate the drought index for unit i in 6-month period t as follows:

$$D_{it} = \sum_{c} \sum_{m=1}^{12} SPEI_{i,t-m}\, \alpha_{i,c,t-m}\ \gamma_{ic}$$

where $SPEI_{i,t-m}$ is the Standardized Precipitation-Evapotranspiration (SPEI) index value averaged over the grid cells of unit i, m months prior to the start of period t; $\alpha_{i,c,t-m}$ is the share of that month that is the growing season for crop c in unit i (calculated using the modal crop planting start date and harvest end date for the grid cells in the unit); and γ_{ic} is the share of the area of unit i that is devoted to growing crop c. SPEI data are from Beguería et al. (2026). Crop areas are from Monfreda et al. (2008). Growing season data are from Sacks et al. (2010). Together the two sources cover 19 major crops. Due to SPEI data availability, I use the months of 2024 to calculate the index for periods 0 and 1.

Second, I use an index to capture demand for a unit's agricultural commodities. Following the general approach of Berman and Couttenier (2015), I calculate the index as follows:

$$A_{it} = \sum_{c} \delta_{i,c,\tau(t)}\ asinh(\nu_{c,\tau(t)})$$

where $\delta_{i,c,\tau t}$ is crop c's share of the total crop value for unit i, for the calendar year prior to the start of period t, denoted $\tau(t)$. ν_c is the global production of crop c in year $\tau(t)$, excluding production from the unit i's country, which represents global demand. I calculate crop values using data on crop volume production from Monfreda et al. (2008). I infer prices by calculating

the average value per metric ton in 2021, using data from UN Comtrade (2025). I use crops that account for at least 1 percent of the area for any units or at least 0.1 percent of the mean area across all units. This yields 37 crops. I use the lag year because Comtrade data for 2024 are incomplete at the time of writing.

Third, I use an index for mineral commodity prices. Following the general approach of Berman et al. (2017), I calculate it as follows:

$$M_{it} = \sum_{r} \mu_i \; asinh(\upsilon_{rt})$$

where μ is an indicator that equals 1 if mineral r is present in unit i. υ_{rt} is the mean monthly price of mineral r over period t. Price data is from World Bank (2026a). Mineral presence data is from ICMM (2026).

Fourth, I use an index for oil prices. I calculate it as follows:

$$O_{it} = \xi_i \; asinh(\phi_t)$$

where ξ is an indicator that equals 1 if oil extraction was active as of February 2026 in unit i. ϕ_t is the mean monthly oil price over period t. Price data is from World Bank (2026a). Extraction data is from GOGET (2026).

Finally, following Berman and Couttenier (2015), I calculate an exposure to trade variable as the distance to the nearest seaport from a unit's centroid. Distance is calculated as the inverse hyperbolic sine of kilometers. Port data are from OCHA (2026).

B: Table versions of results

Column 1 of table B1 provides the table version of the main results from figure 5. Column 2 provides the table version of the main results from figure 6. Column 3 provides the table version of the main results from figure 7. Table B2 presents the results from figure 8. Tables B3 and B4 present the results from figure 9.

Table B1: Table version of results (1)

	(1) asinh(VE)	(2) asinh(VE)	(3) asinh(VE)
Lead 14 × Aid-dependent		0.191	
		(0.189)	
Lead 13 × Aid-dependent		0.281*	
		(0.163)	
Lead 12 × Aid-dependent		0.225	0.542***
		(0.172)	(0.165)
Lead 11 × Aid-dependent		0.391**	0.325**
		(0.178)	(0.149)
Lead 10 × Aid-dependent		0.273	0.367**
		(0.182)	(0.150)
Lead 9 × Aid-dependent		0.410**	0.396***
		(0.171)	(0.144)
Lead 8 × Aid-dependent		0.342*	0.357**
		(0.180)	(0.152)
Lead 7 × Aid-dependent		0.357**	0.162
		(0.172)	(0.154)
Lead 6 × Aid-dependent	0.394**	0.394**	0.244
	(0.158)	(0.158)	(0.151)
Lead 5 × Aid-dependent	0.345**	0.345**	0.0186
	(0.144)	(0.144)	(0.138)
Lead 4 × Aid-dependent	0.258*	0.258*	-0.0393
	(0.145)	(0.145)	(0.131)
Lead 3 × Aid-dependent	0.138	0.138	0.207*
	(0.138)	(0.138)	(0.113)
Lead 2 × Aid-dependent	0.0693	0.0693	0.00339
	(0.112)	(0.112)	(0.118)
Lag 0 × Aid-dependent	0.399***	0.399***	0.441***
	(0.120)	(0.120)	(0.126)
Lag 1 × Aid-dependent	0.268*	0.268*	0.308**
	(0.160)	(0.161)	(0.132)
Lag 2 × Aid-dependent			0.259
			(0.161)
Lag 3 × Aid-dependent			0.258
			(0.169)
Fixed effects	Country-period	Country-period	Country-period
Period length	6-month	6-month	3-month
Controls	2112	5808	4224

All regressions are based on equation 1. Asinh(VE) is the inverse hyperbolic sine of violent events. Coefficients *Lead 20 × Aid-dependent* through *Lead 15 × Aid-dependent* are included in the regression for column 2 but not displaayed due to space constraints. Standard errors are clustered at the administrative unit level. * p<0.1, ** p<0.05, *** p<0.01.

Table B2: Table version of results (2)

	(1) asinh(RP)	(2) asinh(BEV)	(3) asinh(State)	(4) asinh(Rebel)	(5) asinh(Militia)
Lead 6 × Aid-dependent	0.126	0.503***	0.432**	0.426***	0.405***
	(0.163)	(0.167)	(0.176)	(0.146)	(0.147)
Lead 5 × Aid-dependent	0.281*	0.420***	0.381**	0.372***	0.311**
	(0.155)	(0.154)	(0.161)	(0.130)	(0.146)
Lead 4 × Aid-dependent	0.109	0.268*	0.219	0.102	0.0988
	(0.135)	(0.152)	(0.164)	(0.144)	(0.136)
Lead 3 × Aid-dependent	0.132	0.123	0.0883	0.0956	0.107
	(0.131)	(0.135)	(0.130)	(0.135)	(0.100)
Lead 2 × Aid-dependent	0.0310	0.190	0.0978	0.0591	0.00223
	(0.116)	(0.123)	(0.119)	(0.109)	(0.117)
Lag 0 × Aid-dependent	0.229*	0.365***	0.365***	0.320***	0.243**
	(0.124)	(0.126)	(0.134)	(0.123)	(0.116)
Lag 1 × Aid-dependent	0.136	0.380**	0.309*	0.260	0.235*
	(0.134)	(0.168)	(0.182)	(0.159)	(0.137)
Constant	1.967***	2.175***	1.548***	0.866***	1.585***
	(0.0380)	(0.0413)	(0.0420)	(0.0381)	(0.0340)
Fixed effects	Country-period	Country-period	Country-period	Country-period	Country-period
Observations	2224	2224	2224	2224	2224

All regressions are based on equation 1. Asinh(X) is the inverse hyperbolic sine of X. VE = violent events. RP = riots and protests. BEV = nattles, explosions/remote violence, and violence against civilians. State = events involving state forces. Rebel = events involving rebel forces. Militia = events involving militias. Standard errors are clustered at the administrative unit level. * p<0.1, ** p<0.05, *** p<0.01.

Table B3: Table version of results (3)

	(1) asinh(VE)	(2) asinh(VE)	(3) asinh(VE)	(4) asinh(VE)	(5) asinh(VE)	(6) asinh(VE)	(7) asinh(VE)	(8) asinh(VE)	(9) asinh(VE)
Lead 6 × Aid-dependent	-1.934***	0.0158	-0.0553	1.100	-0.815*	0.512	1.331	0.892**	0.235
	(0.395)	(0.383)	(0.580)	(0.787)	(0.403)	(0.472)	(0.777)	(0.414)	(0.531)
Lead 5 × Aid-dependent	-0.241	-0.763***	-0.675	0.976	-0.802	0.549**	1.032*	1.100***	-0.252
	(0.402)	(0.220)	(0.466)	(0.898)	(0.542)	(0.260)	(0.504)	(0.286)	(0.510)
Lead 4 × Aid-dependent	-0.893**	-0.504	0.129	0.372	-0.778	0.250	0.944	1.322***	-0.656
	(0.346)	(0.351)	(0.420)	(0.577)	(0.497)	(0.230)	(0.650)	(0.235)	(0.435)
Lead 3 × Aid-dependent	0.226	-0.517**	-0.270	0.221	-0.414	0.208	0.579	0.727*	-0.324
	(0.301)	(0.225)	(0.604)	(0.677)	(0.699)	(0.179)	(0.627)	(0.366)	(0.412)
Lead 2 × Aid-dependent	-0.375	-0.436***	-0.179	0.310	-0.613	0.268	0.758	0.771***	-0.447
	(0.365)	(0.0973)	(0.324)	(0.459)	(0.422)	(0.287)	(0.495)	(0.241)	(0.492)
Lag 0 × Aid-dependent	0.329	0.0826	0.258	0.0491	-0.381	0.898***	0.550	0.702***	0.417
	(0.377)	(0.224)	(0.495)	(0.525)	(0.489)	(0.302)	(0.365)	(0.206)	(0.387)
Lag 1 × Aid-dependent	-2.774***	0.144	0.283	0.0110	-0.235	0.469	0.692	0.749***	-0.234
	(0.405)	(0.342)	(0.626)	(0.915)	(0.436)	(0.293)	(0.418)	(0.246)	(0.810)
Constant	1.909***	4.255***	2.517***	2.521***	1.500***	2.920***	2.973***	3.537***	2.673***
	(0.0219)	(0.0610)	(0.214)	(0.339)	(0.0787)	(0.0567)	(0.145)	(0.0301)	(0.112)
Fixed effects	Period	Period	Period	Period	Period	Period	Period	Period	Period
Country sample	Benin	BurkinaFaso	Burundi	CAR	Chad	DRC	Ethiopia	Kenya	Madagascar
Observations	88	80	120	96	160	168	96	328	152

All regressions are based on equation 1. Asinh(VE) is the inverse hyperbolic sine of violent events. The regression is conducted for one country at a time, as indicated by the label. Standard errors are clustered at the administrative unit level. * p<0.1, ** p<0.05, *** p<0.01.

Table B4: Table version of results (4)

	(1) asinh(VE)	(2) asinh(VE)	(3) asinh(VE)	(4) asinh(VE)	(5) asinh(VE)	(6) asinh(VE)	(7) asinh(VE)	(8) asinh(VE)
Lead 6 × Aid-dependent	-0.583	0.0978	1.023*	0.889*	0.109	1.182*	0.538	1.752***
	(0.565)	(0.539)	(0.498)	(0.225)	(0.233)	(0.607)	(0.498)	(0.586)
Lead 5 × Aid-dependent	-0.233	0.184	-0.550	0.500	0.265	1.456**	0.608	1.879**
	(0.493)	(0.526)	(0.559)	(0.586)	(0.217)	(0.481)	(0.336)	(0.662)
Lead 4 × Aid-dependent	-0.271	-0.271	-0.306	0.910***	0.341	1.372	0.361	1.509***
	(0.548)	(0.682)	(0.304)	(0.0374)	(0.191)	(0.841)	(0.449)	(0.471)
Lead 3 × Aid-dependent	-0.803	0.516	-0.235	-0.731	-0.411	0.437	0.788**	1.278**
	(0.556)	(0.368)	(0.386)	(1.586)	(0.252)	(0.891)	(0.341)	(0.442)
Lead 2 × Aid-dependent	-0.432	0.313	-0.0911	0.486**	0.207	-0.426	0.482**	0.534
	(0.492)	(0.386)	(0.319)	(0.0868)	(0.198)	(0.599)	(0.165)	(0.373)
Lag 0 × Aid-dependent	-0.00875	-0.131	0.0786	-0.322	0.773***	1.344	0.537**	1.012***
	(0.553)	(0.431)	(0.228)	(0.403)	(0.133)	(1.016)	(0.200)	(0.305)
Lag 1 × Aid-dependent	-0.259	-0.506	0.672	0.616	0.171	1.846	0.363	1.228**
	(0.437)	(0.690)	(0.462)	(0.257)	(0.258)	(1.137)	(0.488)	(0.478)
Constant	1.532***	3.493***	2.830***	3.444***	4.525***	4.474***	3.048***	4.794***
	(0.275)	(0.135)	(0.233)	(0.0591)	(0.0946)	(0.311)	(0.0347)	(0.120)
Fixed effects	Period	Period	Period	Period	Period	Period	Period	Period
Country sample	Malawi	Mali	Mozambique	Niger	SouthSudan	Sudan	Uganda	Yemen
Observations	184	152	72	24	80	88	80	144

All regressions are based on equation 1. Asinh(VE) is the inverse hyperbolic sine of violent events. The regression is conducted for one country at a time, as indicated by the label. Standard errors are clustered at the administrative unit level. * p<0.1, ** p<0.05, *** p<0.01.

C: Additional descriptive statistics

Table C1: Summary statistics for the main dependent variable, asinh(Violent events)

	Mean	SD	Min	Max	N
All	3.16	1.71	0	8.47	2736
Not aid dependent	3	1.7	0	8.47	1304
Aid dependent	3.36	1.67	0	7.8	920

Note. Statistics are for all units and periods in the main specification. The dependent variable is the inverse hyperbolic sine of the count of conflict events

Table C2: Number of treatment and control units by country and type of aid dependence

	Dependent (treatment)					Non-dependent (control)				
	Main	No-gap	HA	NHA	Cuts	Main	No-gap	HA	NHA	Cuts
Benin	1	1	0	2	4	10	11	12	8	6
Burkina Faso	4	5	6	3	5	6	8	6	6	7
Burundi	9	11	0	12	4	6	7	17	1	12
Cameroon	0	1	0	2	3	8	9	10	6	4
Central African Republic	9	11	14	1	11	3	6	2	14	3
Chad	5	7	7	0	1	15	16	11	22	21
Democratic Republic of the Congo	8	10	11	4	7	13	16	12	15	14
Ethiopia	5	5	6	3	9	7	8	6	8	3
Haiti	7	8	8	5	7	0	2	0	3	3
Kenya	6	8	4	8	14	35	39	42	29	28
Madagascar	6	9	3	8	9	13	13	16	9	9
Malawi	18	19	2	19	18	7	9	20	9	7
Mali	7	8	5	5	7	12	12	15	12	12
Mozambique	6	7	1	7	6	3	4	9	2	4
Niger	1	2	1	1	2	2	6	5	3	4
South Sudan	9	9	9	8	1	1	1	0	1	4
Sudan	6	8	13	1	2	5	10	1	16	16
Uganda	2	3	1	5	7	8	11	10	5	6
Yemen	6	7	10	0	10	12	15	9	22	2
Total	115	139	101	94	127	166	203	203	191	165

Note. Main refers to the main treatment variable, *Aid-dependent. No-gap* refers to *Aid-dependent, no-gap*, for which there is no gap between the treatment and control thresholds. It is described in detail in appendix E. *HA* and *NHA* refer to *HAid-dependent* and *NHAid-dependent*, respectively, which measure dependence on humanitarian and non-humanitarian aid. Both are described in appendix D. *Cuts* refers to *Heavily-cut*, which measures exposure to the aid cancelations. It is described in appendix D.

D: Additional results

Table D1 presents the first set of additional results. Column 1 presents the results based on equation 1, with the variable *Heavily-cut* substituted for *Aid-dependent*. As pre-registered, *Heavily-cut* equals 1 if the value of aid cut per GRI is at least 0.2 percent, and 0 if it is less than 0.1 percent. The distribution of treatment assignments by country for this variable are displayed in table C2.

Columns 2 and 3 present the results based on equation 1, with the variable *HAid-dependent* and *NHAid-dependent*, respectively, substituted for *Aid-dependent*. As pre-registered, each of these variables equal 1 if the value of relevant sector's aid per GRI is at least 1 percent, and 0 if it is less than 0.5 percent. The distributions of treatment assignments by country for these variables are displayed in table C2.

Interestingly, the correlation between humanitarian and non-humanitarian aid dependence is not significant ($p = 0.43$ in a regression with one observation per unit). Despite the lack of correlation, the coefficients for period 0 are very similar both to one another and to the coefficient in the main results. It is notable that the effects seem more persistent for humanitarian aid. However, the parallel trends assumption looks less plausible for the humanitarian-aid-dependent units, so these results should be interpreted cautiously. I show in columns 4 and 5 that the results are robust to including interactions between each lead/lag indicator and a continuous variable for the aid per GRI for the other type of aid. For example, for the humanitarian aid-dependent regression, I include interactions with non-humanitarian aid per GRI. I use a continuous control measure rather than the main binary measure to avoid dropping additional countries from the sample. Including these controls helps to account for any correlation between humanitarian and non-humanitarian aid dependence, which could be confounding.

Table D2 presents the results of a placebo test based on equation 1, with *PSAG conflict* substituted for *Aid-dependent*. *PSAG conflict* is a measure of pre-sample armed-group conflict, which proxies for greater expectation of armed strikes from the US. The variable is an indicator that equals 1 if a unit's number of conflict events from battles, explosions/remote violence, and violence against civilians in 2022 is greater than the median for that year. I use 2022 as a reference because

it is the year prior to the start of the main sample, but it is still recent enough that it should provide a useful measure of the most-likely-to-target places. In column 1, the median for the full sample is used. In column 2, the median for a unit's country is used. The effects in the post-disruption period are null, suggesting that anticipation of armed US strikes is not driving the results.

Table D1: Additional results 1

	(1) asinh(VE)	(2) asinh(VE)	(3) asinh(VE)	(4) asinh(VE)	(5) asinh(VE)
Lead 6 × Treatment	0.137	0.533***	0.185	0.473**	0.130
	(0.123)	(0.187)	(0.166)	(0.187)	(0.172)
Lead 5 × Treatment	0.0181	0.459**	0.129	0.439**	0.0962
	(0.126)	(0.188)	(0.147)	(0.190)	(0.153)
Lead 4 × Treatment	0.0160	0.379**	0.0660	0.359**	0.00917
	(0.119)	(0.179)	(0.159)	(0.182)	(0.157)
Lead 3 × Treatment	0.140	0.323**	0.131	0.311*	0.144
	(0.129)	(0.158)	(0.150)	(0.161)	(0.155)
Lead 2 × Treatment	0.00467	0.148	0.147	0.110	0.142
	(0.112)	(0.138)	(0.130)	(0.134)	(0.131)
Lag 0 × Treatment	0.0640	0.407**	0.388***	0.375**	0.393***
	(0.107)	(0.157)	(0.127)	(0.157)	(0.130)
Lag 1 × Treatment	0.0523	0.402**	0.166	0.379**	0.159
	(0.134)	(0.155)	(0.177)	(0.156)	(0.181)
Constant	3.059***	3.058***	2.980***	3.034***	2.973***
	(0.0363)	(0.0396)	(0.0425)	(0.0409)	(0.0426)
Treatment	Heavily-cut	HAid-dependent	NHAid-dependent	HAid-dependent	NHAaid-dependent
Fixed effects	Country-period	Country-period	Country-period	Country-period	Country-period
Controls	None	None	None	NHA*leads/lags	HA*leads/lags
Observations	2336	1984	1936	1984	1936

All regressions are based on equation 1, with *Treatment* substituted for *Aid-dependent*. The treatment variables for each column are indicated in the labels and described in the text of the appendix. Asinh(VE) is the inverse hyperbolic sine of violent events. For controls, *NHA*leads/lags* indicates that a continuous measure of non-humanitarian aid per GRI (described in the text of the appendix) is interacted with each of the lead and lag indicators separately. *HA*leads/lags indicates* the same but for humanitarian aid. These 14 controls are not displayed due to space constraints. Standard errors are clustered at the administrative unit level. * p<0.1, ** p<0.05, *** p<0.01.

Table D2: Additional results 2

	(1) asinh(VE)	(2) asinh(VE)
Lead 6 × PSAG conflict	0.287*	0.334***
	(0.161)	(0.126)
Lead 5 × PSAG conflict	0.234	0.260**
	(0.160)	(0.114)
Lead 4 × PSAG conflict	0.201	0.256**
	(0.147)	(0.119)
Lead 3 × PSAG conflict	0.208	0.218**
	(0.139)	(0.108)
Lead 2 × PSAG conflict	0.0965	0.114
	(0.114)	(0.0941)
Lag 0 × PSAG conflict	0.0616	0.00507
	(0.119)	(0.102)
Lag 1 × PSAG conflict	-0.0837	-0.0813
	(0.158)	(0.123)
Constant	3.086***	3.087***
	(0.0348)	(0.0240)
PSAG conflict type	Full sample	Country
Fixed effects	Country-period	Country-period
Observations	2576	2576

All regressions are based on equation 1, with *PSAG conflict* (described in the main text of the appendix) substituted for *Aid-dependent*. For PSAG conflict type, *Full sample* indicates that *PSAG conflict* is calculated using the median of the full sample. *Country* indicates that *PSAG conflict* is calculated using the median of the unit's country. * p<0.1, ** p<0.05, *** p<0.01.

E: Robustness checks

Table E1 presents robustness checks. Column 1 presents the results based on equation 1, with the inverse hyperbolic sine of fatalities as the Y variable. Column 2 presents the results based on equation 1, with the variable *Aid-dependent, no-gap* substituted for *Aid-dependent*. As pre-registered, *Aid-dependent, no-gap* equals 1 if the value of aid per GRI is at least 1.5 percent, and 0 if it is less than 1.5 percent. The distribution of treatment assignments by country for this variable is displayed in table C2. Column 3 presents the results based on equation 1, with period instead of country-period fixed effects. Column 4 presents the results based on equation 1, with the addition of the following time varying controls described in detail in appendix A: a drought-impact index, an agricultural commodity demand index, a minerals price index, and an oil price index. Column 5 presents the results with an exposure to trade variable (described in appendix A) interacted with the lead and lag indicators. It is clear that the main results are robust in all cases. Tables E2 and E3 show that the results are robust to leaving one country out of the sample at a time.

Table E1: Robustness checks

	(1) asinh(Fatal)	(2) asinh(VE)	(3) asinh(VE)	(4) asinh(VE)	(5) asinh(VE)
Lead 6 × Treatment	0.665***	0.359***	0.318**	0.396**	0.392**
	(0.248)	(0.127)	(0.149)	(0.158)	(0.160)
Lead 5 × Treatment	0.592***	0.338***	0.297**	0.347**	0.350**
	(0.220)	(0.115)	(0.145)	(0.144)	(0.144)
Lead 4 × Treatment	0.334	0.230**	0.181	0.257*	0.261*
	(0.221)	(0.114)	(0.139)	(0.145)	(0.145)
Lead 3 × Treatment	0.277	0.199*	0.0883	0.133	0.131
	(0.207)	(0.107)	(0.126)	(0.138)	(0.142)
Lead 2 × Treatment	0.320*	0.194**	-0.0506	0.0682	0.0768
	(0.192)	(0.0908)	(0.115)	(0.112)	(0.112)
Lag 0 × Treatment	0.364**	0.317***	0.305***	0.407***	0.393***
	(0.171)	(0.0992)	(0.109)	(0.122)	(0.122)
Lag 1 × Treatment	0.254	0.271**	0.0532	0.276*	0.278*
	(0.232)	(0.127)	(0.143)	(0.161)	(0.158)
Drought-impact index				0.800	
				(1.112)	
Ag. demand index				-0.255	
				(0.223)	
Minerals price index				0.313	
				(0.190)	
Oil price index				0.752	
				(1.249)	
Constant	2.474***	3.257***	3.056***	7.706*	3.096***
	(0.0596)	(0.0313)	(0.0384)	(4.516)	(0.351)
Treatment	Aid-dependent	Aid-dependent, no gap	Aid-dependent	Aid-dependent	Aid-dependent
Fixed effects	Country-period	Country-period	Period	Country-period	Country-period
Controls	None	None	None	Time-varying	Port distance
Observations	2112	2576	2112	2112	2112

All regressions are based on equation 1, with *Treatment* substituted for *Aid-dependent*. The treatment variables for each column are indicated in the labels and described in the text of the appendix. Asinh(VE) is the inverse hyperbolic sine of violent events. Asinh(Fatal) is the inverse hyperbolic sine of fatalities. Standard errors are clustered at the administrative unit level. For controls, *Time-varying refers* to the four time-varying controls described in the text of the appendix. They are displayed. *Port distance* indicates that a measure of the inverse hyperbolic sine of the distance to the nearest seaport (described in the text of the appendix) is interacted with each of the lead and lag indicators separately. These 7 controls are not displayed due to space constraints. * p<0.1, ** p<0.05, *** p<0.01.

Table E2: Results leaving out country out at a time (1)

	(1) asinh(VE)	(2) asinh(VE)	(3) asinh(VE)	(4) asinh(VE)	(5) asinh(VE)	(6) asinh(VE)	(7) asinh(VE)	(8) asinh(VE)	(9) asinh(VE)
Lead 6 × Aid-dependent	0.333**	0.287*	0.260*	0.271*	0.372**	0.260*	0.227	0.117	0.259*
	(0.145)	(0.147)	(0.148)	(0.149)	(0.147)	(0.150)	(0.144)	(0.159)	(0.148)
Lead 5 × Aid-dependent	0.333**	0.354**	0.338**	0.304**	0.402***	0.298**	0.279*	0.133	0.343**
	(0.142)	(0.142)	(0.145)	(0.143)	(0.144)	(0.148)	(0.143)	(0.155)	(0.145)
Lead 4 × Aid-dependent	0.217	0.208	0.167	0.194	0.242*	0.173	0.146	0.0228	0.236*
	(0.135)	(0.136)	(0.139)	(0.137)	(0.135)	(0.142)	(0.136)	(0.148)	(0.138)
Lead 3 × Aid-dependent	0.103	0.114	0.101	0.119	0.129	0.0793	0.0671	-0.0330	0.127
	(0.123)	(0.123)	(0.124)	(0.120)	(0.121)	(0.129)	(0.123)	(0.133)	(0.126)
Lead 2 × Aid-dependent	0.0381	0.0115	-0.0190	-0.0150	0.0283	-0.0286	-0.0402	-0.107	0.0235
	(0.111)	(0.115)	(0.117)	(0.114)	(0.113)	(0.118)	(0.113)	(0.123)	(0.115)
Lag 0 × Aid-dependent	0.306***	0.315***	0.315***	0.299***	0.354***	0.255**	0.295***	0.299**	0.287***
	(0.107)	(0.109)	(0.109)	(0.108)	(0.108)	(0.111)	(0.109)	(0.120)	(0.109)
Lag 1 × Aid-dependent	0.0544	0.0107	0.00641	0.00597	0.0520	-0.0212	-0.0159	-0.0625	0.0305
	(0.138)	(0.142)	(0.144)	(0.140)	(0.145)	(0.147)	(0.141)	(0.155)	(0.138)
Constant	3.131***	3.047***	3.131***	3.108***	3.205***	3.105***	3.096***	3.044***	3.121***
	(0.0387)	(0.0380)	(0.0377)	(0.0365)	(0.0393)	(0.0399)	(0.0378)	(0.0455)	(0.0391)
Fixed effects	CP	CP	CP	CP	CP	CP	CP	CP	CP
Country sample	Benin	BurkinaFaso	Burundi	CAR	Chad	DRC	Ethiopia	Kenya	Madagascar
Observations	2136	2144	2104	2128	2064	2056	2128	1896	2072

All regressions are based on equation 1. Asinh(VE) is the inverse hyperbolic sine of violent events. The regression is conducted leaving one country out at a time, as indicated by the label. Country-period fixed effects are included. Standard errors are clustered at the administrative unit level. * p<0.1, ** p<0.05, *** p<0.01.

Table E3: Results leaving out country out at a time (2)

	(1) asinh(VE)	(2) asinh(VE)	(3) asinh(VE)	(4) asinh(VE)	(5) asinh(VE)	(6) asinh(VE)	(7) asinh(VE)	(8) asinh(VE)
Lead 6 × Aid-dependent	0.323**	0.295**	0.418***	0.270*	0.233	0.243*	0.284*	0.164
	(0.152)	(0.148)	(0.132)	(0.144)	(0.148)	(0.146)	(0.147)	(0.144)
Lead 5 × Aid-dependent	0.337**	0.327**	0.480***	0.313**	0.272*	0.276*	0.317**	0.188
	(0.148)	(0.144)	(0.125)	(0.140)	(0.145)	(0.143)	(0.142)	(0.137)
Lead 4 × Aid-dependent	0.154	0.218	0.344***	0.175	0.140	0.140	0.189	0.0787
	(0.140)	(0.134)	(0.121)	(0.134)	(0.138)	(0.135)	(0.136)	(0.136)
Lead 3 × Aid-dependent	0.107	0.0601	0.192	0.0988	0.0872	0.0778	0.0692	0.00271
	(0.125)	(0.126)	(0.117)	(0.121)	(0.125)	(0.122)	(0.123)	(0.124)
Lead 2 × Aid-dependent	0.0403	-0.0242	0.115	-0.00942	-0.0264	0.0243	-0.0245	-0.0430
	(0.116)	(0.115)	(0.104)	(0.112)	(0.116)	(0.112)	(0.114)	(0.116)
Lag 0 × Aid-dependent	0.343***	0.346***	0.379***	0.313***	0.269**	0.268**	0.305***	0.261**
	(0.104)	(0.107)	(0.105)	(0.106)	(0.110)	(0.104)	(0.108)	(0.110)
Lag 1 × Aid-dependent	0.0489	0.0629	0.153	0.00977	-0.00349	-0.0511	-0.000745	-0.0604
	(0.151)	(0.137)	(0.129)	(0.139)	(0.144)	(0.138)	(0.141)	(0.143)
Constant	3.252***	3.059***	3.056***	3.087***	3.046***	3.027***	3.092***	2.980***
	(0.0368)	(0.0382)	(0.0317)	(0.0374)	(0.0370)	(0.0370)	(0.0387)	(0.0381)
Fixed effects	CP	CP	CP	CP	CP	CP	CP	CP
Country sample	Malawi	Mali	Mozambique	Niger	SouthSudan	Sudan	Uganda	Yemen
Observations	2040	2072	2152	2200	2144	2136	2144	2080

All regressions are based on equation 1. Asinh(VE) is the inverse hyperbolic sine of violent events. The regression is conducted leaving one country out at a time, as indicated by the label. Country-period fixed effects are included. Standard errors are clustered at the administrative unit level. * p<0.1, ** p<0.05, *** p<0.01.

F: International Aid Transparency Initiative (IATI) data and tests

The IATI data

The International Aid Transparency Initiative (IATI) gathers data on foreign aid projects through the voluntary collaboration of aid agencies. For projects starting in 2024, the largest contributors of geocoded information include United Nations agencies, the European Commission, the African Development Bank, and national development agencies for Germany and the UK. US development agencies are not included in the dataset for recent years.

The dataset indicates the date range of projects, the amount spent on projects, and, in some cases, the geographic coordinates of projects, among other information. For projects with geographic coordinates starting in 2024 in my sample of 19 countries, the commitments total 4.2 billion dollars.

Validating using IATI

If we assume that there is a correlation in where different funding agencies target aid, then there should be a unit-level correlation between my measure of aid dependence and the value of commitments in the IATI dataset. Indeed, I find a strong correlation. For my sample of 19 countries, compared to non-aid-dependent units in my sample, aid-dependent units have on average 2.8 percentage points more IATI-aid per GRI for IATI projects starting in 2024 ($p = 0.004$ using a regression with robust standard errors and one observation per unit).

Alternative explanations test

If the aid-dependent units in my sample became aid-dependent in 2024, immediately prior to the disruption, it could be the case that donors were targeting these units because they expected an increase in conflict in them—which could explain the relative uptick in conflict in these units in 2025. It would also make it more likely that the aid itself caused an increase in conflict (though I argue in the main text that this explanation would still be unlikely). To test the possibility that aid-dependent units in my sample became aid-dependent in 2024, I run a regression based on the

following equation:

$$Y_{ict} = \alpha + \sum_{j=2}^{J} \beta_j (Lead\ j)_t \times Aid\text{-}dependent_i + \gamma_k (Lag)_t \times Aid\text{-}dependent_i + \lambda_i + \mu_{tc} + \epsilon_{ict} \quad (3)$$

Note that this is the a very similar setup to equation 1. However, the dependent variable in this case, $IATI$, is the inverse hyperbolic sine of IATI spending in unit i, in country c, for year t. Furthermore, *Lead* j is an indicator that equals 1 for the jth year before 2024, *Lag* is an indicator that equals 1 for 2024, and *Lead* 1 is omitted as the reference category.

The results are presented in table F1. They show that there was a relative increase in funding from non-US donors to the aid-dependent units in my study in 2023. However, crucially, there was no statistically significant relative change from 2023 to 2024. Combined with the fact that US funding correlates strongly with non-US funding, this finding provides evidence against the idea that my treatment units became dependent on US foreign aid in 2024. Thus, for the alternative explanation to hold, it would have to be the case that donors increased aid to the treatment units in 2023 in expectation of future conflict—but the anticipated spike in conflict did not appear until 2025, and only for a short period immediately following Trump's inauguration. It is therefore clear that the aid disruption is a much more likely reason for the conflict spike.

Table F1: Relationship between treatment status and IATI funding

	(1)
	IATI
Lead 9 × Aid-dependent	-1.539
	(1.005)
Lead 8 × Aid-dependent	-1.069
	(0.812)
Lead 7 × Aid-dependent	-2.149**
	(0.900)
Lead 6 × Aid-dependent	-2.739***
	(0.831)
Lead 5 × Aid-dependent	-1.922**
	(0.856)
Lead 4 × Aid-dependent	-1.733*
	(0.932)
Lead 3 × Aid-dependent	-1.952**
	(0.953)
Lead 2 × Aid-dependent	-1.977**
	(0.981)
Lag 0 × Aid-dependent	-1.143
	(0.938)
Constant	14.38***
	(0.260)
Fixed effects	Country-period
Observations	2780

The regression is based on equation 3. Standard errors are clustered at the administrative unit level. * p<0.1, ** p<0.05, *** p<0.01.

G: Deviations from the pre-analysis plan

Dataset construction

I deviate from the pre-registered dataset construction in the following ways:

1. I pre-registered that I would include obligations from fiscal years 2024 and 2025 in my dataset. My initial reasoning for including obligations from fiscal year 2025 was that they would be tied to projects that were most likely to be active at the time of the disruption, and most fiscal year 2025 obligations would have been made prior to Trump's inauguration. However, after pre-registration, I decided that including fiscal year 2025 was inappropriate because a non-negligible amount of obligations were made after inauguration, and their geographic targeting could be endogenous to the outcomes of interest. I therefore only use obligations from 2024 to determine subnational aid-dependence. Column 2 of table G1 presents the main results based on obligations from both 2024 and 2025, as pre-registered. Crucially, they are very similar to the main results based on obligations from only 2024, presented in column 1 of table G1 and in figure 5 in the main paper. If anything, the results in column 2 are more supportive of the main hypothesis.

2. I pre-registered that the initial sample (of 25 countries) was determined in part by whether total obligations for 2024 and 2035 are at least 0.5 percent of GNI. In this paper, I state that the initial sample was determined in part by whether total obligations for 2024 *only* were at least 0.5 percent of GNI. This change has no impact at all on the countries included in the sample.

3. I pre-registered that I determined subnational population using World Bank data. This was a typo. I had already decided not to use World Bank data because it is missing for the Central African Republic, Chad, the Democratic Republic of the Congo, Haiti, and Uganda (see World Bank (2026b)). It is thus not possible to use it for all of the countries I claimed in the pre-analysis plan.

Analysis

I did not deviate from the pre-registered analysis for the main specification in any way. I deviated from exploratory analyses in the following ways:

1. I include several robustness checks that were not pre-registered. These include the control for exposure to trade and leaving out one country from the regression at a time. I include these tests to provide evidence against alternative explanations.
2. I include several exploratory tests that were not pre-registered. These include extended pre trends, effects by actor type, country-level estimates, and the placebo tests using pre-sample armed-group conflict. I include them to further unpack the main pre-registered results and provide evidence against alternative explanations.
3. I did not pre-register the use of IATI data or the tests that rely on it. I include this data to help validate the data and provide evidence against alternative explanations.
4. For the sake of streamlining the paper, I do not include all robustness checks that were mentioned in the pre-analysis plan. Specifically, I exclude tests that involve calculating aid dependence using aid that most directly relates to the theorized mechanisms (*Aid-dependent, most relevant*) and dropping the least reliable types of location extractions from the dataset (*Aid-dependent, most reliable*). These tests are displayed in columns 3 and 4 of table G1. The results are clearly robust to these specifications.
5. Again, for the sake of streamlining, I do not include the exploratory hypotheses from my pre-analysis plan in the paper. However, I do still include some of the tests for these hypotheses as exploratory analyses. Below, I lay out the exploratory hypotheses and discuss which tests were included or excluded:
    - *H2a: Humanitarian aid-dependent administrative units will experience a relative increase in conflict following the aid cuts—but this increase will be smaller than the increase for non-humanitarian aid-dependent administrative units.* I provide tests of humanitarian aid-dependence and non-humanitarian aid-dependence in table D1. I do not explicitly

compare the effects, but it is clear they are not significantly different. Note that there was a mistake in the relevant equation in the pre-analysis plan (equation 2). A correctly specified triple interaction should include a term for *Treatment* $\times$ *Rebel conflict lag* (or $M_{i,y-1} \times AidDep_i$ using the variable names in the pre-analysis plan). It is included in the regressions below.

- *H2b: The relative increase in violence among aid-dependent administrative units—and especially humanitarian aid-dependent administrative units—will be weaker in units with rebel violence in the pre-cut period.* I do not include this test in the main paper because it is not well powered. The results are presented below in table G2, columns 1 and 2. The coefficients of interest ($Lag\ 1 \times Treatment \times Rebel\ conflict\ lag$ and $Lag\ 0 \times Treatment \times Rebel\ conflict\ lag$) are not significant.
- *H3a: The relative increase in violence among aid-dependent administrative units will be stronger for violence related to protests and riots compared to violence related to battles, violence against civilians, and explosions/remote violence.* I do not explicitly test this hypothesis, but the relevant regressions are presented in figure 8. The difference in effects for the two types of conflict are not significantly different.
- *H3b: The relative increase in violence related to battles, violence against civilians, and explosions/remote violence among aid-dependent administrative units will be weaker in units with rebel violence in the pre-cut period.* Again, the test for this hypothesis is not well-powered. The results are presented below in table G2, column 3. The coefficients of interest ($Lag\ 1 \times Treatment \times Rebel\ conflict\ lag$ and $Lag\ 0 \times Treatment \times Rebel\ conflict\ lag$) are not significant.

Table G1: Additional tests from the pre-analysis plan

	(1) asinh(VE)	(2) asinh(VE)	(3) asinh(VE)	(4) asinh(VE)
Lead 6 × Treatment	0.394**	0.362**	0.182	0.432***
	(0.158)	(0.159)	(0.171)	(0.157)
Lead 5 × Treatment	0.345**	0.319**	0.129	0.371**
	(0.144)	(0.147)	(0.152)	(0.144)
Lead 4 × Treatment	0.258*	0.227	0.0872	0.269*
	(0.145)	(0.146)	(0.165)	(0.143)
Lead 3 × Treatment	0.138	0.131	0.155	0.163
	(0.138)	(0.142)	(0.156)	(0.138)
Lead 2 × Treatment	0.0693	0.0836	0.164	0.0616
	(0.112)	(0.112)	(0.133)	(0.110)
Lag 0 × Treatment	0.399***	0.431***	0.385***	0.372***
	(0.120)	(0.122)	(0.134)	(0.118)
Lag 1 × Treatment	0.268*	0.275*	0.168	0.275*
	(0.160)	(0.165)	(0.186)	(0.158)
Constant	3.022***	2.987***	2.979***	3.003***
	(0.0399)	(0.0445)	(0.0411)	(0.0390)
Treatment	Aid-dependent	Aid-dependent	Aid-dependent, most relevant	Aid-dependent, most reliable
Fixed effects	Country-period	Country-period	Country-period	Country-period
Sample	2024	2024 and 2025	2024	2024
Observations	2112	2112	1888	2128

All regressions are based on equation 1, with *Treatment* substituted for *Aid-dependent*. The treatment variables for each column are indicated in the labels and described in the text of the appendix. Asinh(VE) is the inverse hyperbolic sine of violent events. The 2024 sample uses only obligations from fiscal year 2024. The 2025 sample uses obligations from fiscal years 2024 and 2025.
* p<0.1, ** p<0.05, *** p<0.01.

Table G2: Moderating effect of lagged rebel violence

	(1) asinh(VE)	(2) asinh(VE)	(3) asinh(BEV)
Rebel violence lag	-0.00587	-0.161	0.201
	(0.166)	(0.183)	(0.208)
Rebel violence lag × Treatment	-0.131	0.137	-0.205
	(0.238)	(0.272)	(0.270)
Lead 2 × Treatment	0.0209	0.135	0.187
	(0.149)	(0.192)	(0.150)
Lead 2 × Rebel violence lag	-0.106	0.0913	-0.183
	(0.177)	(0.199)	(0.217)
Lead 2 × Treatment × Rebel violence lag	0.139	-0.00326	0.0600
	(0.216)	(0.245)	(0.236)
Lag 0 × Treatment	0.384**	0.437**	0.252*
	(0.151)	(0.186)	(0.147)
Lag 0 × Rebel violence lag	0.264	0.400	-0.0750
	(0.212)	(0.246)	(0.247)
Lag 0 × Treatment × Rebel violence lag	-0.0465	-0.191	0.266
	(0.263)	(0.341)	(0.279)
Lag 1 × Treatment	0.193	0.586***	0.317*
	(0.195)	(0.180)	(0.186)
Lag 1 × Rebel violence lag	-0.0476	0.412*	-0.163
	(0.245)	(0.247)	(0.267)
Lag 1 × Treatment × Rebel violence lag	0.181	-0.522	0.187
	(0.334)	(0.351)	(0.341)
Constant	3.017***	3.015***	2.133***
	(0.0599)	(0.0595)	(0.0558)
Treatment	Aid-dependent	HAid-dependent	Aid-dependent
Fixed effects	Country-period	Country-period	Country-period
Observations	2112	1984	2112

The regression is adapted from equation 1, with *Treatment* substituted for *Aid-dependent* and interactions with *Rebel violence lag* added. *Rebel violence lag* is an indicator for whether the administrative unit experienced an event with a rebel group in the previous calendar year. *HAid-dependent* refers to dependence on humanitarian aid. Asinh(VE) is the inverse hyperbolic sine of violent events. Asinh(BEV) is the inverse hyperbolic sine of battles, explosions/remote violence, and violence against civilians. Standard errors are clustered at the administrative unit level. Interactions including leads 6 through 3 are included in the regression but not displayed due to space constraints. * p<0.1, ** p<0.05, *** p<0.01.