The Impact of Terrorist Designations on Foreign Aid

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Abstract

How do terrorist designations influence foreign aid allocation? While terrorism is often associated with increased aid flows, critics argue that counterterrorism (CT) measures, particularly formal terrorist listings, can suppress aid to conflict-affected regions. This paper seeks to adjudicate between two competing hypotheses: the strategic aid hypothesis, which predicts increased aid as donors seek to stabilize fragile contexts, and the chilling effect hypothesis, which anticipates aid reductions due to legal and operational constraints. Using a multi-method research design, we combine a cross-national event study of 132 developing countries (2001–2021) with subnational difference-in-differences analyses in five countries where designated groups held territorial control. Our results provide stronger support for the strategic aid hypothesis. Aid often increases following designations, particularly from institutional donors such as development banks. However, we find limited evidence of spatial targeting based on group control and identify donor-type heterogeneity. Civil society organizations (CSOs) and U.S. humanitarian flows appear more susceptible to chilling effects. Methodologically, we advance the study of aid and CT policy by integrating cross-national, subnational, and permutation-based inference using geocoded data from GODAD. The results call for closer attention to donor heterogeneity and subnational conditions to understand when counterterrorism designations enable aid recalibration and when they suppress humanitarian response.

Keywords: Terrorist Designations, Aid, Subnational, Counterterrorism, Development

JEL classifications: F35, O19, D74

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Introduction

Terrorism and counterterrorism have become defining features of the international system. Over the past three decades, the global incidence of terrorism has increased sharply, prompting governments to adopt a growing arsenal of counterterrorism (CT) tools (LaFree and Dugan 2007; Crenshaw and Pimlott 2019). Among these, the formal designation of armed groups as terrorist organizations has emerged as a prominent instrument. In the early 1990s, fewer than 5% of armed conflicts involved a designated group; by the early 2020s, that figure exceeded 50% (Lundgren et al. 2024). While the primary aim of designations is to isolate and degrade the capability of violent actors, humanitarian practitioners have warned that they also cause collateral consequences, including the disruption of aid deliveries to territories controlled by proscribed groups. This raises a fundamental yet unresolved question: Do terrorist designations reduce international aid to affected populations?

This question lies at the intersection of two seemingly contradictory claims in the literature. On one hand, large-N studies show that terrorism is often followed by increased foreign aid, as donors seek to stabilize fragile states and contain transnational threats (e.g., Dreher and Fuchs 2011; Bezerra and Braithwaite 2016). On the other hand, case-based research suggests that counterterrorism responses, specifically formal terrorist designations, can suppress and disrupt aid flows (e.g., Mackintosh and Duplat 2013). A possible resolution to this puzzling contradiction would be that terrorism, as measured in the large-N literature, is not correlated with terrorist designations. However, empirical research has found that terrorist attacks are a strong predictor of formal terrorist designations (Lee and Tominaga, 2024), suggesting that both claims cannot be true at the same time.

We address this puzzle by developing and testing two competing perspectives on how terrorist designations influence international aid allocation. The *strategic aid perspective*, rooted in political economy accounts, holds that designations prompt increased aid as donors respond to foreign policy concerns and humanitarian needs. Donors use aid to stabilize fragile contexts, undercut radicalization and recruitment, and counter the reputational costs of CT. If this logic holds, we expect aid levels to rise in locations affected by designated groups. The *chilling effect perspective*, by contrast, draws on qualitative research and argues that designations suppress aid flows by introducing legal uncertainty, operational barriers, and reputational risks. These constraints raise the cost of delivering aid and lead donors to reduce, redirect, or delay assistance. If this view is correct, we expect aid levels to decrease following the imposition of terrorist designations.

To adjudicate between these two perspectives, we implement a two-pronged research design. First, we conduct a cross-national event study covering 132 developing countries from 2001 to 2021 to assess how aggregate aid flows respond to terrorist designations over time, distinguishing between aid and donor types. Second, we exploit spatial variation in aid allocation at the subnational level, combining data from the Geocoded Global Official Development Assistance Dataset (GODAD) with data on listed groups' territorial control to examine how aid allocation varies within countries following terrorist designations. We focus on five cases, Yemen, Mali (two designation episodes), Nigeria, and Sri Lanka, where designated terrorist groups exercised territorial control at the time of listing. These cases allow us to test whether aid responses differ across regions with and without designated groups, providing a more granular view of donor responses.

We find that terrorist designations are generally followed by increases in foreign aid, particularly development aid, consistent with the strategic aid hypothesis. This pattern is evident both across countries and within them, as aid to designated-group areas often rises post-designation. However, we find little evidence that donors spatially target aid based on territorial control, and responses vary across donor types and sectors. Humanitarian aid flows are less consistent, with some indications of disruption in specific cases, most notably Yemen.

These findings have four main implications, further elaborated in the conclusion. First, they speak directly to a core debate on the effects of terrorism and counterterrorism on aid. While concerns about chilling effects have dominated policy discussions, our results show that formal designations more often prompt strategic engagement, particularly from institutional donors. Second, the results reveal that donor responses vary systematically by both nationality and institutional type. U.S. donors and civil society organizations appear more susceptible to chilling effects, while development banks and other donors are more likely to scale up aid. Third, our study shows that territorial control by listed groups is not, in itself, a consistent barrier to aid flows, though exceptions such as the Yemen case highlight the potential for localized chilling effects. Finally, the paper demonstrates the value of integrating cross-national and subnational designs to detect both aggregate and spatially differentiated effects of counterterrorism measures on aid, offering a framework for future work to assess when such measures disrupt aid and when they prompt strategic recalibration.

Terrorism, Counterterrorism, and Aid Allocation

A standard assumption in the aid literature is that donors allocate aid in ways that optimize their interests within constraints (Alesina and Dollar 2000; Dreher et al. 2009; Hoeffler and Outram 2011). The interests may include donor-centric aims such as securing diplomatic influence, advancing military cooperation, or reinforcing national identity, as well as recipient-oriented goals such as alleviating poverty, promoting public health, and responding to disasters. We argue that terrorism and counterterrorism measures may alter the incentives and constraints that shape donor decision-making. On one hand, terrorist violence may accentuate the strategic value of aid, prompting donors to increase engagement in affected areas. On the other hand, counterterrorism, including terrorist designations, may introduce legal, operational, and reputational constraints that limit donors' ability or willingness to provide aid. These contrasting dynamics give rise to two rival expectations: the *strategic aid* hypothesis and the *chilling effect* hypothesis.

Strategic aid hypothesis

The first perspective builds on the idea that aid can be used strategically to manage security threats. Several studies have found that terrorism tends to increase aid flows. Azam and Delacroix (2006) and Dreher and Fuchs (2011) find that terrorism leads to higher official development assistance (ODA), while Lis (2013, 2018) finds heterogeneous effects across bilateral and multilateral donors. Research focusing on specific donors, particularly the U.S., demonstrates that terrorism directly threatening donor interests prompts increased aid. Heinrich et al. (2017) and Boutton and Carter (2014) both find that U.S. aid increases in response to terrorism when it poses a threat to U.S. interests, while Bezerra and Braithwaite (2016) note that local terrorist violence in Sub-Saharan Africa leads to higher aid.

These patterns are consistent with a strategic aid logic in which donors use aid to mitigate the negative externalities of terrorism and preserve their influence in unstable environments (Bermeo 2017). We expect this logic applies not only to terrorism but also to terrorist designations. First, when a major government labels an armed group as terrorists, it elevates the strategic salience of the listed group and its area of operation by signaling increased risks of spillover, instability, or security threats. Aid is then deployed to contain those risks (Bermeo 2017; Heinrich et al. 2017). Second, designations enhance the political viability of aid by framing it as part of a counterterrorism strategy. This framing can mobilize domestic support and justify aid to fragile areas and controversial actors (Boutton and Carter 2014). Third, donors may seek to undercut local grievances that contribute to radicalization and recruitment into terrorist networks. Aid targeting a designated group's main territory can help erode its social appeal and recruitment base (Savun and Tirone 2018; Young and Findley 2011). Taken together, these mechanisms support an expectation that terrorist designations should be followed by increased aid. We formulate this as the strategic aid hypothesis:

H1: Terrorist designations are associated with increased foreign aid commitments to territories where designated groups operate.

Chilling effect hypothesis

In contrast, the second perspective emphasizes how counterterrorism instruments such as terrorist designations constrain aid provision. A growing body of qualitative research argues that these measures complicate or reduce aid flows. O'Leary (2021) and Nadel and Walton (2024) find that terrorist designations have a negative effect on donor willingness to provide aid. Mackintosh and Duplat (2013), drawing on case studies from Somalia and the occupied Palestinian territories, suggest that several donors, including the U.S. and European governments, reduced or withdrew humanitarian funding from areas under the control of designated groups due to legal risks. Similarly, Eckert (2021) finds that financial institutions engage in "de-risking," limiting transactions with actors in conflict zones tied to terrorism financing. At the implementation level, Burniske and Modirzadeh (2017) report that 69 percent of surveyed humanitarian workers felt that counterterrorism measures had curtailed their work.

These patterns are consistent with a chilling effect logic in which terrorist designations generate additional constraints on donors' aid calculus. This logic centers on three key mechanisms. First, designations create legal uncertainty. Counterterrorism laws such as the US Foreign Terrorist Organization (FTO) designation may prohibit or restrict engagement with designated groups, exposing donors and implementing partners to penalties, including potential criminal liability (McKeever 2020; Debarre 2019). Second, designations disrupt logistical and financial operations. Banks and implementing organizations may delay or block transactions, while heightened compliance requirements can slow or interrupt program delivery (Mackintosh and Duplat 2013; Burniske and Modirzadeh 2017). Third, designations raise political and reputational risks. Donors may face criticism for supporting areas controlled by proscribed actors, and local civil society groups may be stigmatized or excluded from aid partnerships (Lind and Howell 2010; Bolleyer and Gauja 2017; O'Leary 2021; see Haspeslagh 2021 for a similar argument pertaining to peace negotiations). Together, these constraints support an expectation of greater risk aversion in the wake of terrorist designation. This leads us to formulate a chilling effect hypothesis:

H2. Terrorist designations are associated with reduced aid commitments to territories hosting designated groups.

Data and Empirical Design

The strategic aid hypothesis and the chilling effect hypothesis offer starkly different perspectives on the likely impact of terrorist designations on aid allocation. To adjudicate between them, we implement a research design comprised of two complementary studies: (1) a cross-national event study tracing aggregate donor responses to terrorist designations, and (2) a subnational difference-in-differences (DiD) analysis assessing spatial variation in aid allocation within countries where designated groups hold territorial control. Below, we set out the data sources, operationalization, and estimation strategies of each. Summary statistics for the outcome variables and covariates used in both the cross-national and subnational analyses are provided in Appendix A1.

Cross-National Event Study

For the cross-national component of the study, we construct an annual, country-level panel dataset spanning 132 developing countries from 2001 to 2021.

Our key independent variables capture terrorist designations based on data from the Proscribed Armed Actors Dataset (PAAD) (Lundgren et al., 2024), which codes formal listings of armed non-state actors between 1979 and 2022. In our main analysis, we focus on designations issued by the United States, distinguishing between FTO listings and a broader category encompassing all U.S. designation types. FTO designations, issued under Section 219 of the Immigration and Nationality Act, carry far-reaching legal and financial consequences, including criminal penalties for material support and mandatory asset freezes. The broader category also includes designations under Executive Order 13224 (Specially Designated Global Terrorists), the Terrorist Exclusion List (TEL), and Executive Order 12947. We construct two binary indicators at the country-year level: one capturing any new U.S. designation (regardless of legal instrument), and one isolating FTO listings. In our robustness checks, we also examine designations by the European Union and the United Nations, which differ in legal reach but may reflect wider international consensus.

Our dependent variable is logged foreign aid commitments, measured using data from the OECD Creditor Reporting System (CRS). We focus on two aid categories: humanitarian assistance and development assistance (i.e., non-humanitarian ODA), measured in constant 2014 U.S. dollars. For each, we construct variables for total aid from all donors and aid from the U.S. specifically. For our additional tests, we also disaggregate aid flows by donor country and donor type, distinguishing between bilateral, multilateral, development bank, and civil society organization (CSO) donors.

To control for possible confounders, we incorporate covariates capturing conflict intensity (battle-related deaths) from the Uppsala Conflict Data Program (UCDP) (Davies et al., 2025), terrorism-related fatalities (START 2022), democratic quality (from the Varieties of Democracy dataset (Coppedge et al., 2024), and economic conditions (including GDP per capita and trade as a share of GDP), sourced from the World Bank's World Development Indicators).

To capture dynamic responses to designation events, we estimate an event study within a two-way fixed effects (TWFE) framework. The core specification is:

$$Aid_{ct} = \alpha_c + \delta_t + \sum_{k \neq -1} \beta_k \cdot 1[EventTime_{ct} = k] + \chi'_{c,t-1}\gamma + \epsilon_{ct}$$

Where Aid_{ct} is the aid outcome for country c in year t; α_c and δ_t are country and year fixed effects; $1[EventTime_{ct} = k]$ is a set of dummies equal to 1 if year t is k years relative to the terrorist designation; the omitted category is k=-1, the year immediately prior to designation; and $\chi'_{c,t-1}$ is a vector of lagged controls.

This approach tests both hypotheses by tracing the temporal evolution of aid before and after designations. Pre-trend coefficients (k<0) allow us to examine whether aid flows were already shifting prior to the designation year. The post-treatment coefficients (k≥0) indicate the magnitude and direction of aid responses. This approach is particularly suited for our context, as both anticipatory responses to designation threats and lagged implementation effects are theoretically plausible. All models are estimated using OLS with robust standard errors clustered at the country level. While treatment timing is staggered, we follow recent work suggesting that traditional TWFE event study estimators remain valid when treatment heterogeneity is limited (Goodman-Bacon 2021; Sun & Abraham 2021).

Subnational DiD Study

To complement our cross-national analysis, we examine four countries where ADM1-level aid (e.g., province or district, depending on the country) and territorial control data allow us to test whether aid shifts more sharply in areas under the control of groups formally designated as terrorists by the U.S. Each case implements a DiD strategy, enabling us to assess spatial heterogeneity in donor responses and evaluate whether aid behavior aligns more closely with the strategic or chilling effect hypotheses as applied to subnational variation in territorial control.

The selected cases span a range of conflict and designation contexts, all involving U.S. FTO listings (see Table 1). In Yemen, the Houthis (Ansar Allah) were designated in 2021. The group controls large parts of northern Yemen, including the capital Sana'a, providing a sharp territorial treatment contrast. Because the designation occurred in 2021, only a short post-treatment window is available. In Mali, we evaluate two separate designations: Ansar Dine, designated in 2013 during the Northern Mali insurgency, and Islamic State in the Greater Sahara (ISIS-GS), designated in 2020. In Nigeria, Boko Haram was designated in 2013, and its sustained territorial presence in the northeast offers a stable treated region over time.

Table 1. Cases in subnational DiD study

Country	Designated Group	Designation Year
Sri Lanka	Liberation Tigers of Tamil Eelam (LTTE)	1997
Mali	Ansar Dine (part of JNIM)	2013
Nigeria	Boko Haram	2013
Mali	Islamic State in the Greater Sahara (ISGS)	2020
Yemen	Houthis (Ansar Allah)	2021

Across all five cases, we estimate DiD models using ADM1-year panels tailored to the timing of each group's designation. Rather than applying a uniform sample, we adjust the estimation window in each case based on available data, ensuring sufficient pre- and post-treatment observations. This flexibility is necessitated by data limitations, particularly the length of post-treatment periods and the start of consistent geocoded aid coverage in some countries. Our aim is to preserve comparability while leveraging the maximum feasible variation around each designation year. For Sri Lanka (LTTE, 1997), we draw on the longest available horizon, estimating models with up to 5-year pre- and post-treatment windows (1992–2001). For Mali (Ansar Dine, 2013) and Nigeria (Boko Haram, 2013), treatment occurs mid-panel, allowing for symmetric 3- and 5-year windows on either side of the designation. In contrast, Mali (ISIS-GS, 2020) and Yemen (Houthis, 2021) offer limited post-treatment data, so we restrict the analysis to shorter panels beginning in 2013 or 2015 and use asymmetric windows of 1–2 years post-treatment.

Aid data are drawn from the GODAD dataset, which provides georeferenced project-level aid information from 18 OECD donors, China, India, and the World Bank from 1973 to 2021 (Bomprezzi et al., 2025). While the full dataset spans nearly five decades, our subnational models use country-specific panels ranging from the early 1990s to 2021, with most estimation windows falling within the 2000–2021 period. We focus on two primary outcome variables: logged commitments and logged disbursements, both measured in constant 2014 U.S. dollars and aggregated at the ADM1-year level.

For Yemen, where GODAD does not cover aid projects beyond 2020, we applied AidData's Tracking Underreported Financial Flows (TUFF) methodology to manually assign ADM1 locations for aid disbursed in 2021 (Custer et al., 2023; Goodman et al., 2024). This extension was necessary because the Houthis were designated in 2021, and subnational aid data for that year were otherwise unavailable. TUFF geocoding combines open-source reporting with validation against OpenStreetMap features to ensure spatial accuracy. Of the 1,014 projects reported in relevant sectors in 2021, 412 were successfully geocoded at the ADM1 level using this approach. Because our subnational analysis focuses exclusively on humanitarian aid in the Yemen case, we limited geocoding to the ten OECD-defined sectors most relevant to humanitarian response. To ensure spatial comparability, we restrict pre-2021 GODAD records to the same ten sectors. Full details of the 2021 geocoding process are provided in Appendix A.

Treated regions are defined as ADM1 units that were under the control or sustained influence of designated groups at the time of designation. We adopt a consistent classification strategy across cases, relying on publicly available territorial control datasets (including those from the Assessment Capacities Project [ACAPS] 2021), UN Security Council reports, and secondary sources. In Yemen, ADM1s where more than 50% of the territory was under De Facto Authority (Houthi) control (as of February 2021) are categorized as treated. In Mali, the Kidal region is treated in the Ansar Dine case, and the Gao, Timbuktu, and Kidal regions are in the ISIS-GS case. In Nigeria, Borno, Yobe, and Adamawa are treated. In Sri Lanka, the Northern and Eastern provinces are treated, following mappings of LTTE territorial control during the US 1997 designation. Untreated ADM1s serve as comparison units. Specific case-level coding decisions are detailed in Appendix A.

To account for variation in development need and economic activity across ADM1s, we include two subnational covariates obtained from AidData's GeoQuery platform (Goodman et al., 2019): (1) population density, from the Gridded Population of the World (Version 4, Revision 11), and (2) nighttime lights, using DMSP-OLS calibrated composites. These variables proxy for local need and economic conditions. Because they are highly collinear with ADM1 fixed effects, we estimate models with covariates in separate specifications that exclude fixed effects and use conventional standard errors. Full models with controls are reported in the appendix for each case, while our main results rely on fixed effects and wild cluster bootstrapping and are presented without covariates.

We estimate two-way fixed effects DiD models of the following form:

$$y_{it} = \alpha_i + \delta_t + \beta_1 Post_t + \beta_2 Treated_i + \beta_3 (Post_t \times Treated_i) + \chi'_i \ \gamma + \epsilon_{it}$$

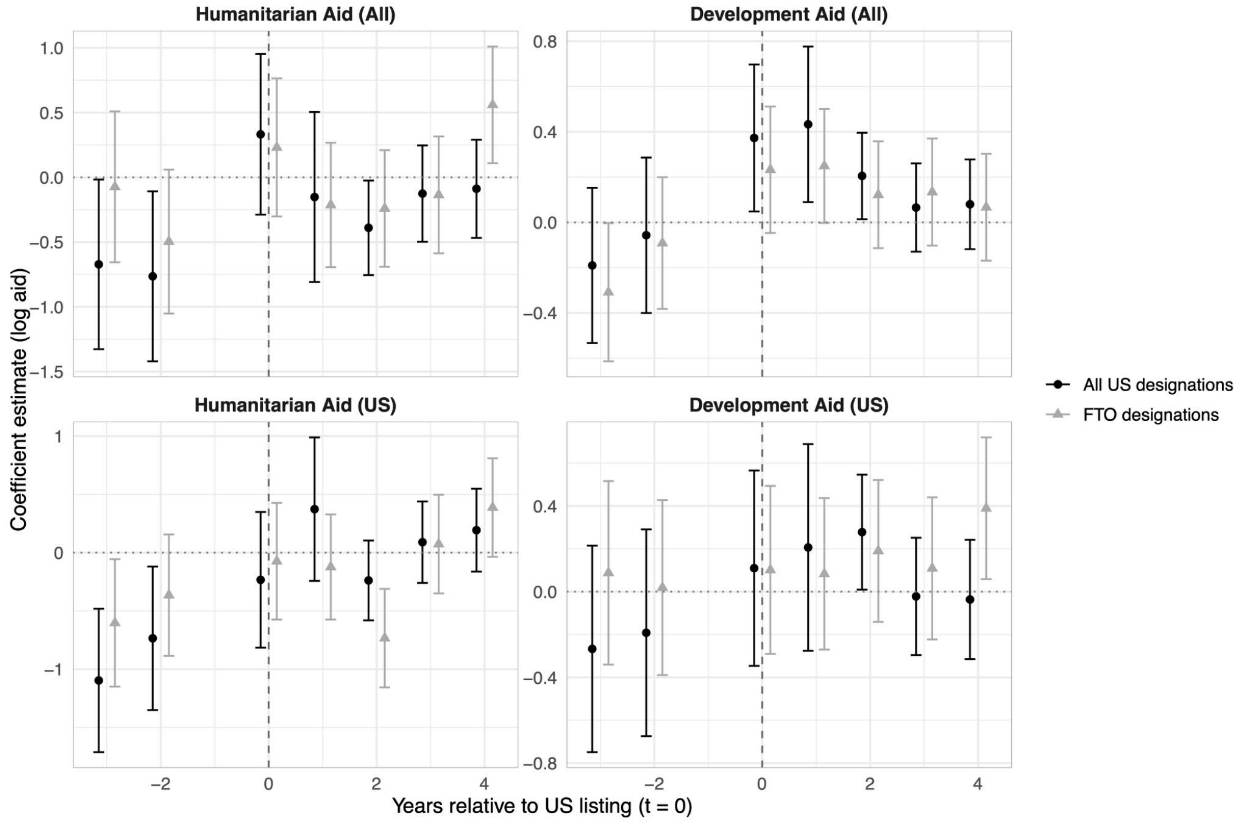
Where y_{it} is the aid outcome in ADM1 i, year t (commitments or disbursements); $Treated_i$ is a binary indicator for designated-group control; $Post_t$ captures the post-designation period; $\alpha_i$and δ_t are ADM1 and year fixed effects; χ'_i includes the subnational covariates described above. The coefficient of interest is $\beta_3$captures the differential change in aid flows in treated areas following designation. Standard errors are computed using wild cluster bootstrap procedures, which provide more reliable inference when the number of clusters is small (Cameron et al. 2008). This is particularly important for our analysis, as several case studies include fewer than 20 ADM1 units, which is below the threshold at which conventional cluster-robust standard errors are considered valid.

Results

Cross-national Results

Figure 1 presents results from our cross-national analyses of the impact of US terrorist designations on aid. We report estimates for four outcome variables: humanitarian and development aid from all donors and from the US specifically. For each outcome, the figure displays event-study coefficients for all US designation types combined and for FTO listings only. Full results are reported in the online appendix.

Figure 1. US Terrorist designation and aid allocation, event study coefficients



Notes: Estimated coefficients from event study models with country and year fixed effects. Points show changes in log aid commitments relative to the year of US designation (t = 0), with 95% confidence intervals. Full results in Tables B1 and B2 of the online appendix.

As shown in the top-left panel, humanitarian aid from all donors increases modestly in the year of designation (t = 0), though this change is not statistically significant relative to the baseline year (t = –1). However, compared to two and three years prior to designation (t = –2 and t = –3), where aid levels were significantly lower, the increase appears more substantive. A decline in year t+2 is consistent with a potential chilling effect, although the pattern is not sustained. Estimates for FTO listings follow a similar trajectory.

As shown in the lower-left panel, U.S. humanitarian aid rises modestly from the depressed levels observed at t–3 and t–2, reaching higher levels around the designation year. While the increases at t = 0 and t + 1 are not statistically significant, the pattern suggests an upward trend in aid flows. This shift is not linear, however, as a sharp and significant decline occurs at t+2, only to suggest increases thereafter.

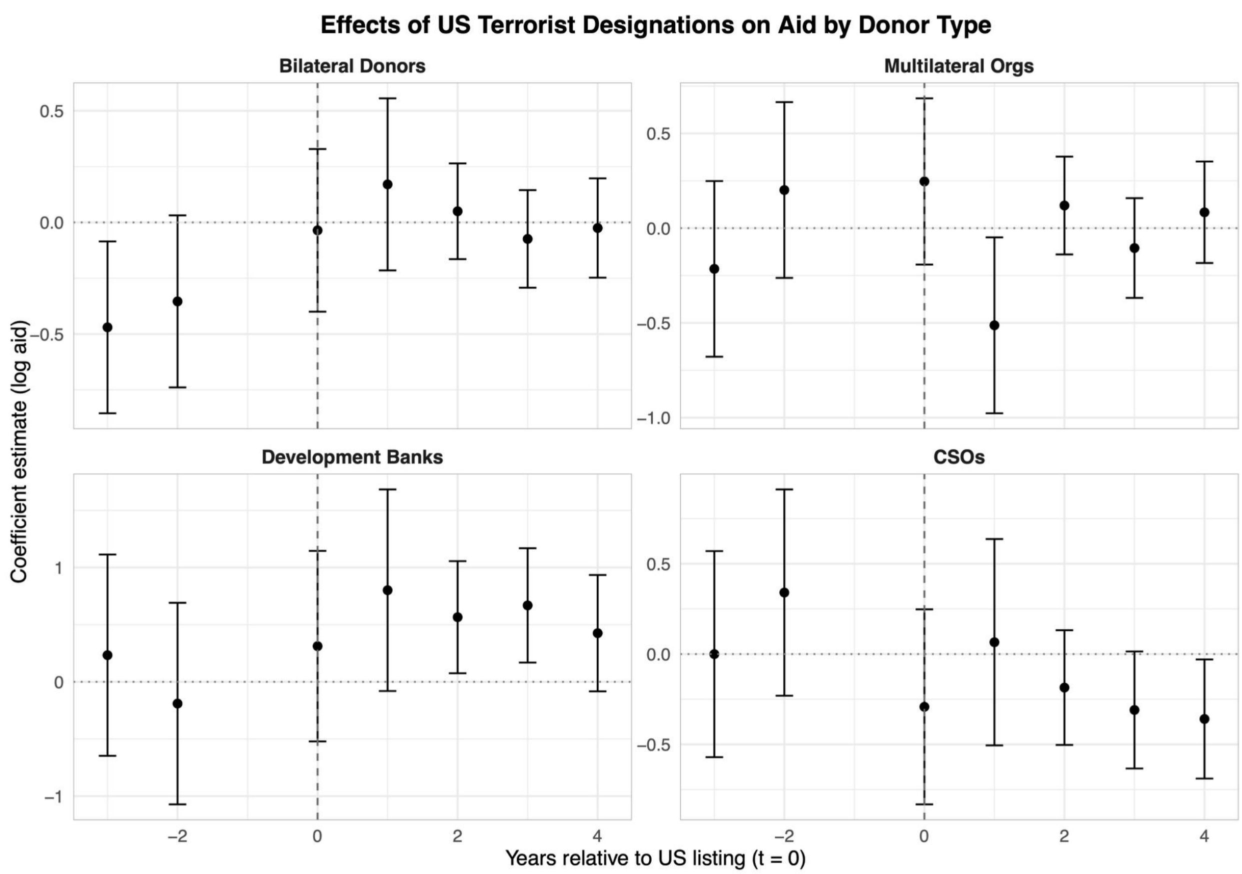
Development aid exhibits a clearer pattern. As is shown in the top-right panel, development aid from all donors rises in the year of designation and remains significantly elevated through years t+2, with a stronger effect from the combined designation category.

US development aid, plotted in the lower-right panel, follows a similar trajectory but with larger confidence intervals, indicating greater variability in effects across countries.

Taken together, these cross-national findings provide stronger support for the strategic aid hypothesis (H1) than for the chilling effect hypothesis (H2). Increases in development aid, especially by all donors, are consistent with the argument that designations raise the strategic salience of affected territories and prompt donors to use aid as a tool to contain spillover and preserve influence. Evidence of a chilling effect on humanitarian aid is weaker and mostly limited to the US response to FTO listings at select time points.

Our data allow us to probe how these results vary across different donor types. Figure 2 examines how different donor types respond to U.S. terrorist designations. These results suggest a divergent response between development banks and CSOs. Whereas development banks show a marked and sustained increase in aid commitments starting at t+1 and continuing through t+3, with all coefficients positive and statistically significant, aid from CSOs shows a consistent downward trend across the post-treatment period, culminating in statistically significant declines at t+3 and t+4. This divergence indicates that while institutional donors like development banks ramp up engagement, CSOs may reduce their involvement, possibly due to heightened legal risk, operational barriers, or funding constraints. Aid from bilateral and multilateral donors shows less consistent responses, with only isolated significant estimates and no clear post-treatment trajectory.

Figure 2. US terrorist designation and aid allocation, by donor type



Notes: Event study estimates of the effect of U.S. terrorist designations on aid flows, disaggregated by donor type. Points show estimated coefficients (log aid) with 95% confidence intervals. Models include country and year fixed effects. The vertical dashed line indicates the year of designation (t = 0).

In Figure B1 of the online appendix, we display estimated associations between terrorist designations by the EU and UN and subsequent aid allocation. The overall pattern resembles the results for U.S. designations and provides further support for the strategic aid hypothesis. For both EU and UN listings, development aid increases gradually beginning at t = 0, with statistically significant effects at t + 1 and t + 2. Humanitarian aid also spikes sharply in the listing year for both EU and UN designations, but this effect is short-lived, with no sustained increases or clear post-treatment decline. There is little indication of a chilling effect on humanitarian aid, even in the short term. One interpretation is that the stronger legal and financial constraints attached to U.S. FTO designations may deter humanitarian engagement more than multilateral listings by the EU and UN.

Taken together, the cross-national evidence points to a fairly consistent pattern: development aid tends to increase following terrorist designations, particularly from institutional donors such as development banks, and across both U.S. and multilateral listings. This supports the strategic aid hypothesis, suggesting that designations elevate the geopolitical salience of affected areas and trigger compensatory aid flows. In contrast, evidence for a chilling effect on humanitarian aid is limited and concentrated in specific donor types, especially U.S. aid and CSOs, where legal or operational constraints may be more binding.

Subnational Results

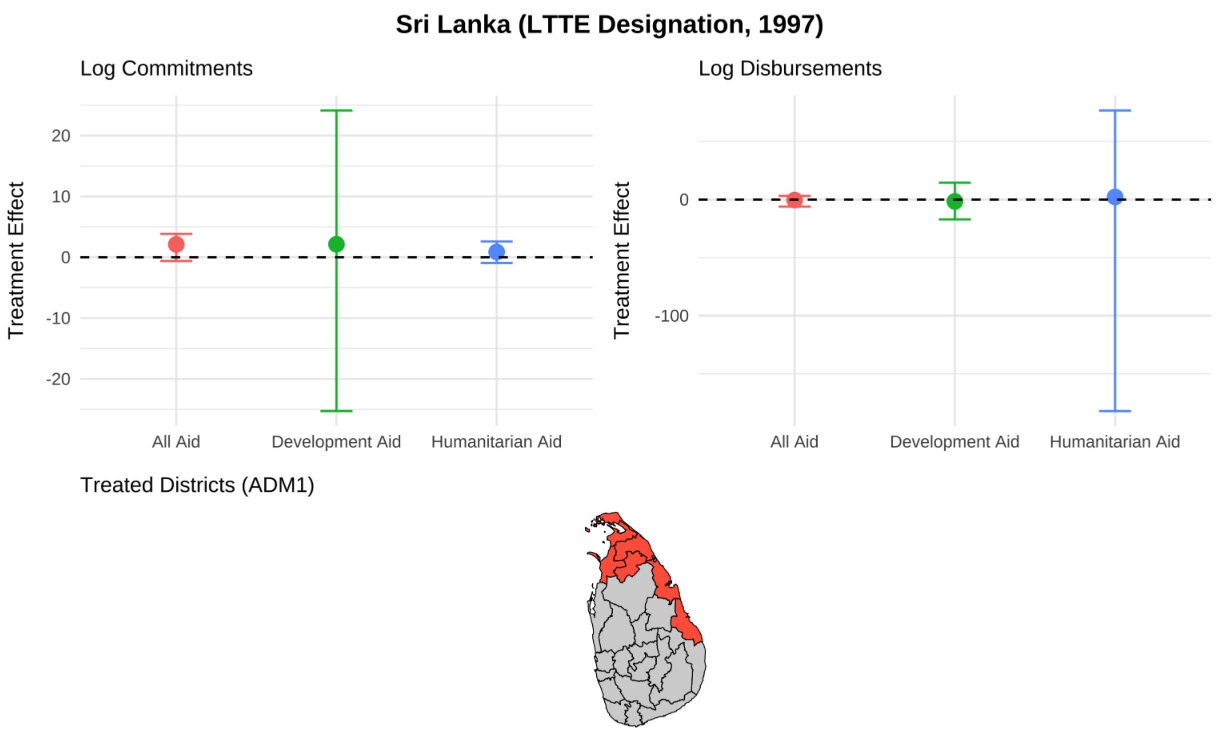
To assess whether designation effects vary across space, we examine subnational aid patterns in five countries where designated groups maintained territorial control. Across these cases, we estimate DiD models with ADM1 and year fixed effects, using wild cluster bootstrapped inference with 999 repetitions. We follow standard practice in the applied econometrics literature in selecting 999 repetitions, as this number allows for p-values to be calculated with precision to three decimal places without incurring excessive computational cost (Cameron, Gelbach, and Miller 2008). Figures 3 through 7 map treated districts and present the estimated average treatment effect on log commitments and disbursements, with full regression tables provided in Appendix C.

The designation of the LTTE provides the longest post-treatment window in our study, enabling an assessment of medium-run shifts in donor behavior. The main results presented in Figure 3 rely on a five-year pre- and post-treatment window (1992–2001), with estimates disaggregated by aid type. Estimated treatment effects on commitments are positive across all aid categories, ranging from 0.9 log points for humanitarian aid to just over 2 log points for both development and all aid. While none of these effects reach conventional levels of statistical significance, the consistent direction suggests potential strategic re-engagement with LTTE-held areas. It is worth noting, however, that observed commitment values in some sectors are sparse, particularly for development aid, which contributes to less precise estimates.

Disbursement effects are more mixed: near zero for all aid, modestly positive for humanitarian aid (+2.25 log points), and negative for development aid (−1.6 log points), though these estimates are also imprecise, with extremely wide confidence intervals. In particular, the wide interval for humanitarian disbursements reflects a high share of zero or near-zero values in treated districts post-designation. This data sparsity, combined with the log transformation and small number of treated units, results in unstable point estimates and inflated bootstrap variance.

Appendix Table C1 reports additional estimates using three alternative windows: 3 years pre/3 post, 3 pre/5 post, and 5 pre/3 post. Results for commitments remain positive across these alternative specifications and generally fall within the same range (1.5 to 2.3 log points), suggesting that the patterns observed in the main specification are robust. Disbursement effects are similarly imprecise and fluctuating across time windows, underscoring continued uncertainty in how designation shaped actual aid delivery on the ground. Taken together, these results point to a possible post-designation increase in donor commitments to LTTE-controlled areas, particularly for development sectors, though uncertainty remains high.

Figure 3. Map of Treated Districts and Estimated Effects of LTTE Terrorist Designation on Aid to Sri Lanka



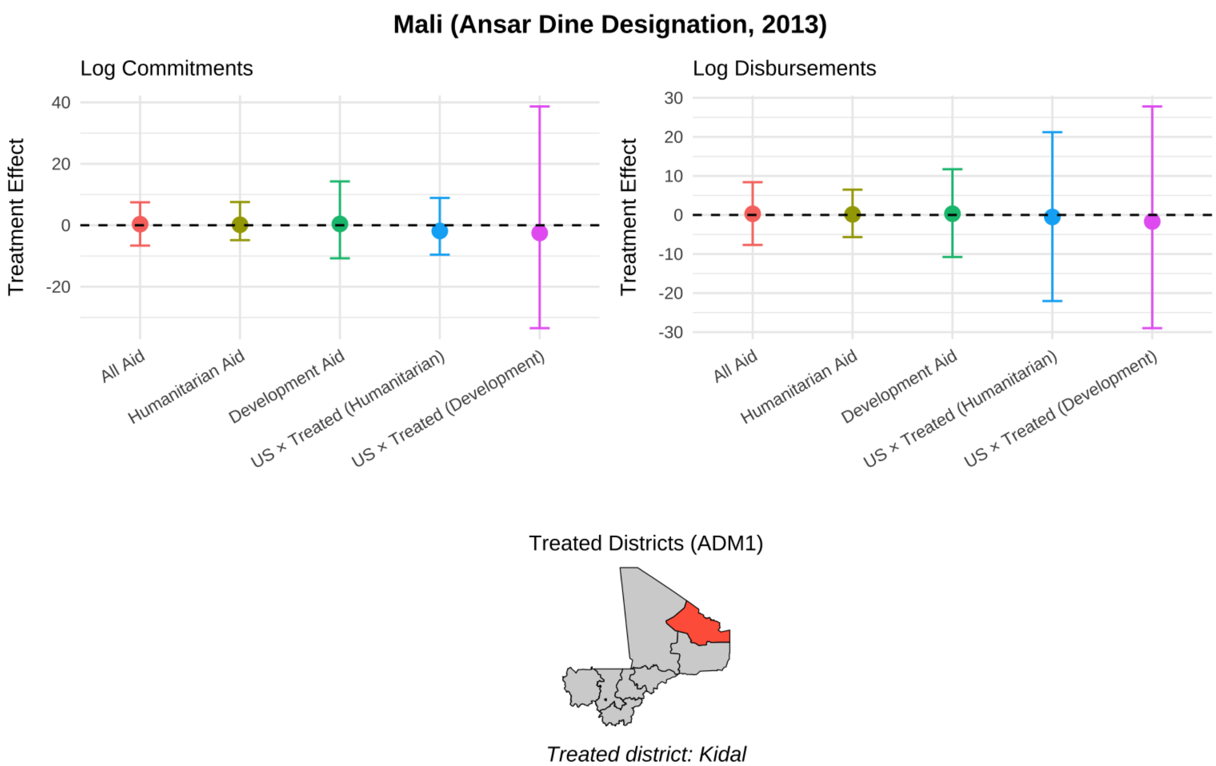
Treated: Jaffna, Kilinochchi, Mannar, Mullaitivu, Vavuniya, Batticaloa, Trincomalee

Notes: Top left panel shows bootstrapped DiD estimates (999 repetitions) of the treatment effect on log commitments; top right shows estimates for log disbursements. Estimates are disaggregated by aid type: all aid (red), development aid (green), and humanitarian aid (blue), with 95% confidence intervals clustered at the ADM1 level. Models use a five-year pre- and post-treatment window (1992–2001) and include no covariates. Bottom panel maps the treated ADM1 districts under LTTE control at the time of the 1997 U.S. FTO designation. See Table C1 in the Appendix for full regression results.

The U.S. designation of Ansar Dine in Mali in 2013 coincided with the group's territorial control in Kidal, providing an opportunity to assess spatial shifts in aid allocation. Figure 4 presents treatment effect estimates using a five-year pre- and post-treatment window (2008-2017), disaggregated by aid type. Across all aid types, point estimates for both commitments and disbursements are small and positive, ranging from 0.16 to 0.43 log points, but none reach statistical significance. The direction of effects is broadly consistent across humanitarian and development aid, as well as across shorter and asymmetric time windows (reported in Appendix Table C2), with all estimates for commitments falling between -0.14 and +0.57 log points.

Disaggregated models using a three-way interaction for U.S. aid (designation × post × U.S. donor) show consistently negative point estimates for both humanitarian and development aid, suggesting that the muted or slightly positive aggregate trends may be driven primarily by non-U.S. donors. However, these U.S.-specific effects are also imprecisely estimated, with wide confidence intervals and p-values far from conventional thresholds. Taken together, these findings do not provide evidence of a chilling effect in Mali. At the same time, while aggregate estimates are modestly positive, the donor-disaggregated models offer only limited support for a consistent strategic reallocation of aid, primarily among non-U.S. donors. While the direction of effects is relatively stable across specifications, the magnitude and precision of estimates suggest that any designation-related shift in aid flows to Kidal was modest at best.

Figure 4. Map of Treated Districts and Estimated Effects of Ansar Dine Terrorist Designation on Aid to Mali



Notes: Estimates as in Figure 3 . Models use a five-year pre- and post-treatment window (2008–2017) with no covariates. U.S. aid effects (designation × post × U.S. donor) are shown separately for humanitarian (blue) and development (pink) aid. Bottom panel maps the treated district (Kidal), under Ansar Dine control at the time of the 2013 U.S. FTO designation. See Appendix Table C2 for full regression results.

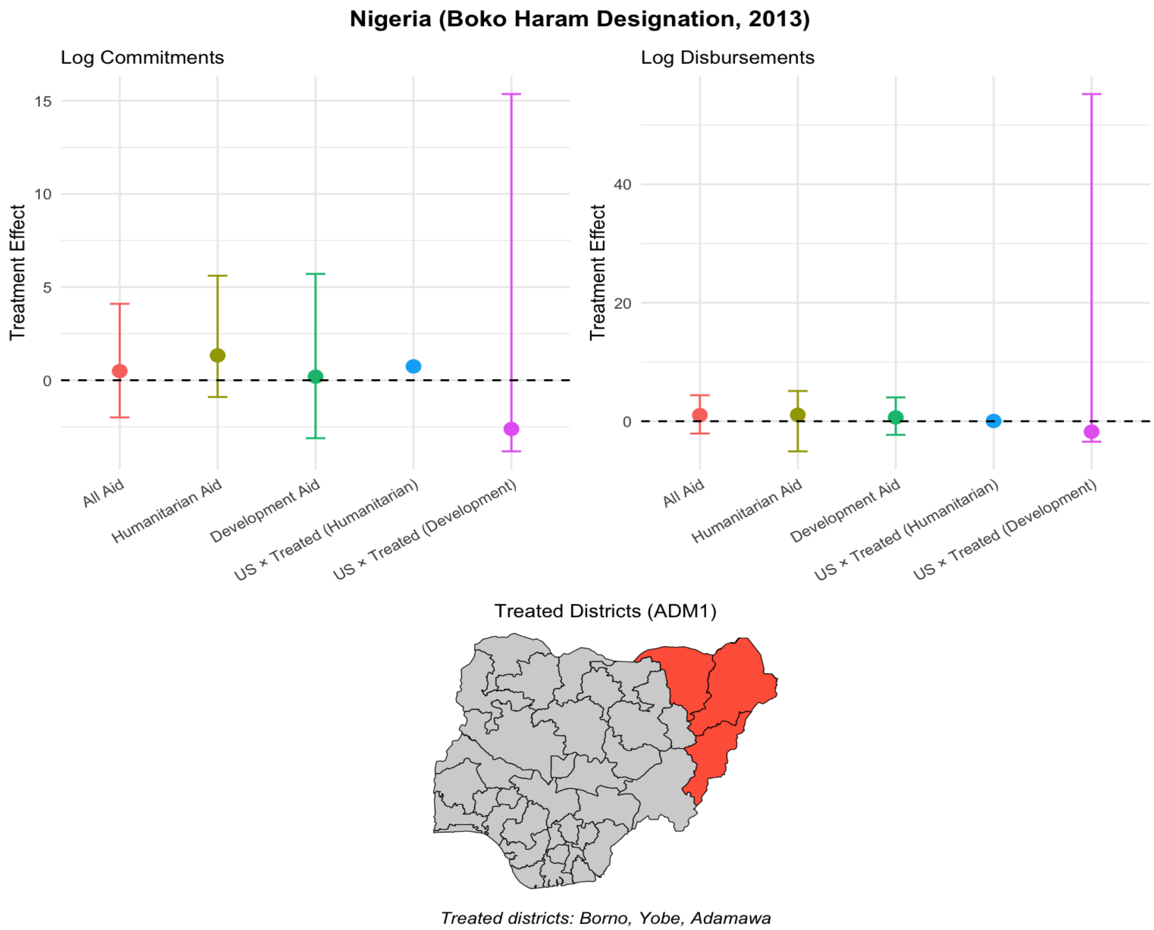
The U.S. designation of Boko Haram in Nigeria in 2013 allows us to examine changes in aid allocation to the conflict-affected northeastern states of Borno, Yobe, and Adamawa. Figure 5 presents treatment effect estimates using a five-year pre- and post-designation window (2008–2017). In this specification, disbursements of all aid to treated districts increase by 1.04 log points, while commitments rise by 0.49 log points. However, neither estimate reaches statistical significance, and confidence intervals are wide. These patterns are consistent across other time windows, such as three years before and five years after the designation, or symmetric three-year windows before and after, as reported in Appendix Table C3. Across these models, point estimates remain generally positive but imprecise.

Humanitarian and development aid flows show similar trends. Humanitarian aid commitments increase by 1.33 log points and disbursements by 1.08 log points, while development aid effects remain smaller and closer to zero. None of these are statistically significant, and the effect sizes vary across specifications, suggesting that any donor response to the designation was likely limited in scope.

U.S.-specific effects, estimated using a three-way interaction between designation, post-designation period, and donor identity, are mixed. In contrast to earlier cases, U.S. humanitarian aid commitments and disbursements show small positive point estimates in the five-year model (+0.74 and +0.05 log points, respectively). However, confidence intervals could not be estimated due to convergence failure in the wild cluster bootstrap procedure, caused by sparse outcome data and a limited number of treated clusters, which limits the reliability of these results. By contrast, U.S. development aid effects are consistently negative, with several models yielding extremely wide confidence intervals. For example, one model covering three years before and three years after the designation produces an interval spanning nearly 230 log points (from -223 to +6), while another ranges from -61 to +69. This asymmetry reflects a highly skewed bootstrap distribution, driven by sparse outcome data and the influence of individual treated clusters in resampled estimates. Although some U.S. development estimates approach conventional significance thresholds (e.g., $p = 0.084$), the magnitude of uncertainty limits clear interpretation.

Taken together, the results indicate that overall aid flows to Boko Haram–affected areas did not decline following the designation. While point estimates are generally positive, particularly for humanitarian aid, the wide confidence intervals and lack of statistical significance suggest considerable uncertainty. There is no strong evidence of a U.S. chilling effect on humanitarian aid, but the persistently negative, though imprecise, estimates for U.S. development aid raise the possibility of more selective disengagement in that sector.

Figure 5. Map of Treated Districts and Estimated Effects of Boko Haram Terrorist Designation on Aid to Nigeria



Notes: Estimates as in Figure 3, using a five-year pre- and post-treatment window (2008–2017) with no covariates. U.S. aid effects (designation × post × U.S. donor) are shown separately for humanitarian (blue) and development (pink) aid. Bottom panel maps treated districts (Borno, Yobe, Adamawa), under Boko Haram control at the time of the 2013 U.S. FTO designation. See Appendix Table C3 for full regression results.

We next turn again to Mali, where the Islamic State in the Greater Sahara (ISIS-GS) was designated by the U.S. as a Foreign Terrorist Organization (FTO) in 2020. The treated governorates include Gao, Kidal, Tombouctou, and Timbuktu, which are regions with an established ISIS-GS presence at the time of designation. Figure 6 presents the results for the 5-year pre- and 1-year post window, our main specification for Mali, given the timing of the designation and the limited post-treatment data.

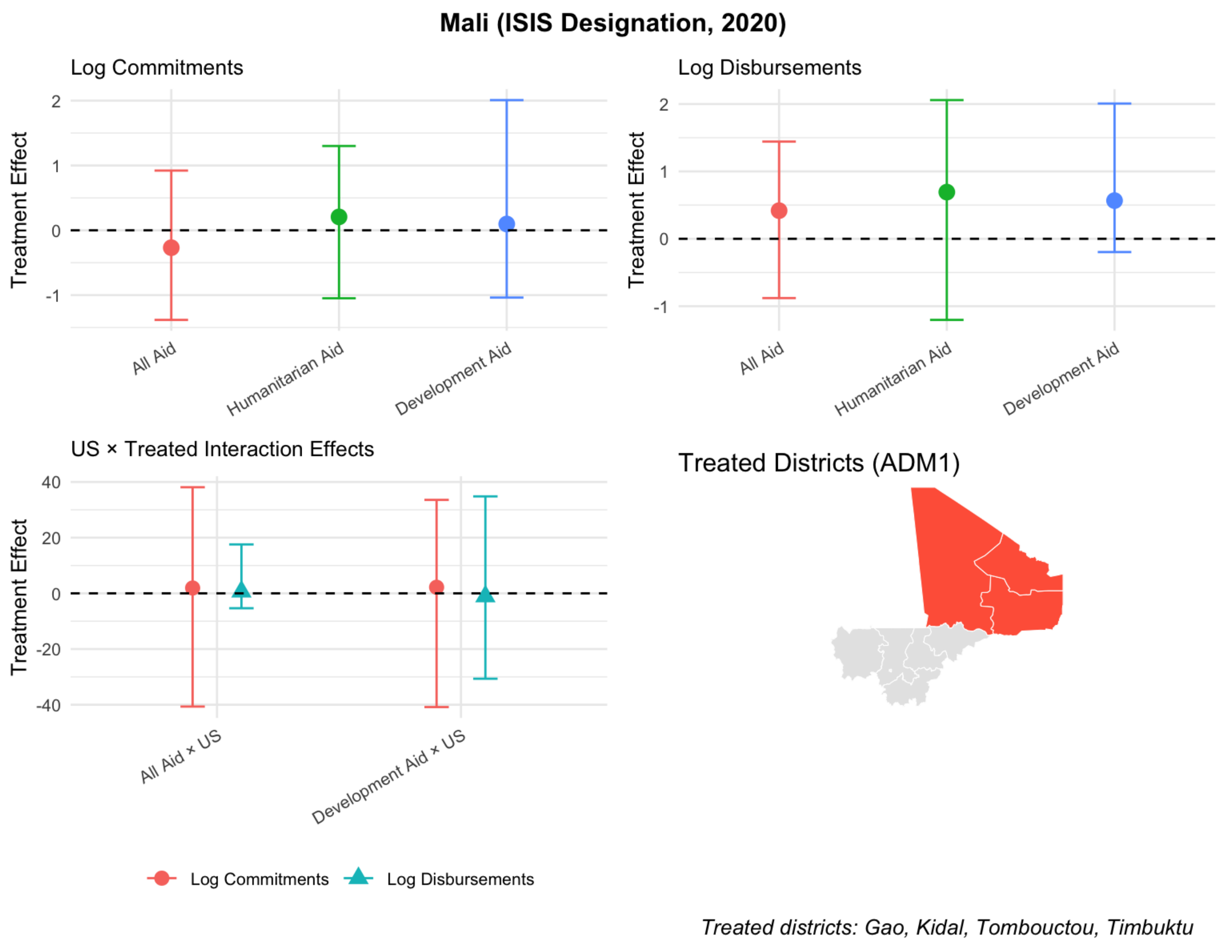
Across aid types, we find no evidence of a chilling effect. Point estimates for all aid, humanitarian aid, and development aid are generally positive, but statistically indistinguishable from zero. For example, log disbursements of humanitarian aid increased by an estimated 0.69 log points (95% CI: –1.20, 2.06), while development disbursements rose by 0.57 log points (95% CI: –0.20, 2.01). None of these results are significant at conventional levels, but the direction of change is consistent with the strategic aid hypothesis.

We also examine U.S.-specific effects using a three-way interaction term (designation × post × U.S. donor). These estimates show extreme confidence intervals and remain statistically insignificant. For development disbursements, the U.S.-specific effect is estimated at –1.1 log points (95% CI: –30.7, 34.8), while commitments rise by 2.19 log points (95% CI: –40.8, 33.6). Due to the scale of these intervals, we plot U.S. interaction effects separately in the bottom-left panel of Figure X for visual clarity.

Our main estimates are broadly consistent across the alternative windows presented in Appendix Table C4. While estimates based on longer post-treatment periods (e.g., 5-year pre, 2-year post) are directionally similar, they remain statistically insignificant. Notably, development disbursements using a 3-year pre-, 2-year post window show a statistically significant increase of 0.54 log points (95% CI: 0.08, 1.01; $p = 0.020$), but this result is not robust across other windows or aid types.

Taken together, this case study lends modest support to the strategic aid hypothesis: rather than withdrawing from designated regions, donors, particularly in the development sector, appear to have maintained or slightly increased aid flows in the immediate aftermath of the designation.

Figure 6. Map of Treated Districts and Estimated Effects of ISIS Terrorist Designation on Aid to Mali

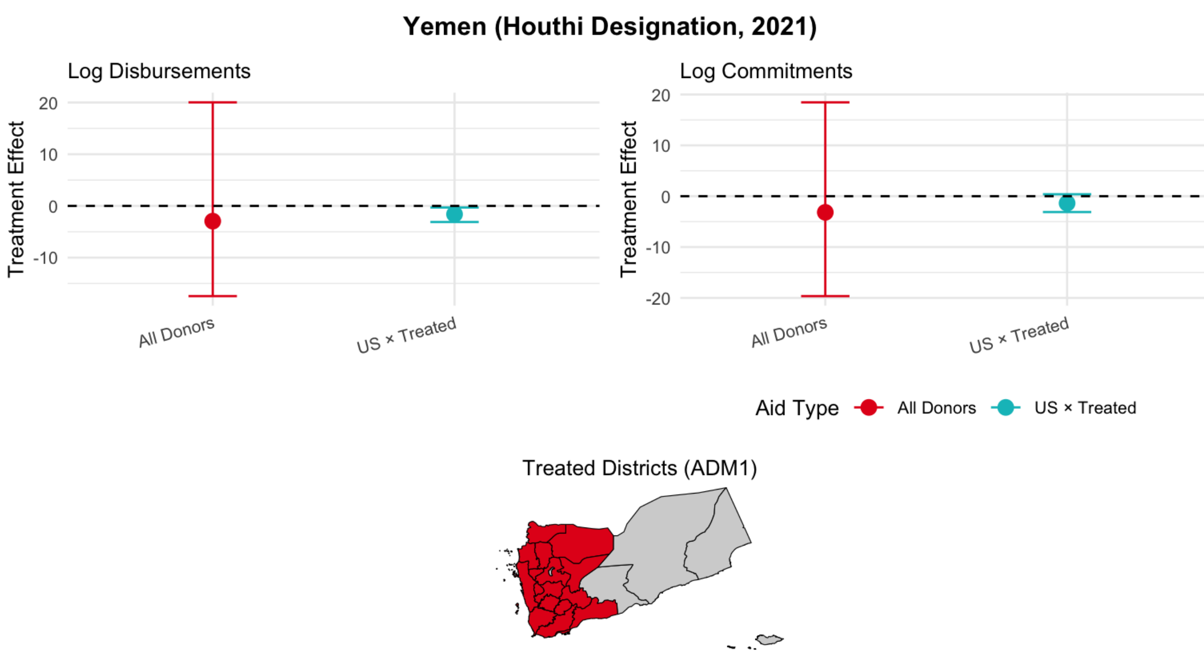


Notes: Estimates follow Figure 3, using a five-year pre-/one-year post-treatment window (2015–2020). U.S.-specific effects are shown for development and all aid only; humanitarian estimates omitted due to data gaps. Bottom-right panel maps treated districts (Gao, Kidal, Tombouctou, Timbuktu) under ISIS-GS presence at the time of the 2020 designation. See Appendix Table C4 for full results.

Yemen provides the strongest evidence of a chilling effect. Across all model windows, humanitarian aid commitments and disbursements to Houthi-controlled governorates decline in the year following the January 2021 U.S. designation (Table C5). Estimated effects range from -1.8 to -3.4 log points and are uniformly negative across three-, five-, and eight-year pre-treatment windows. Although these estimates do not reach conventional significance thresholds, the consistent direction and substantial magnitude of the coefficients are noteworthy, particularly given that the dataset captures only eleven months post-designation. Treated governorates are defined as those in which more than 50% of territory was under Houthi control at the time of designation, an approach that aligns with standard practice in territorial control and conflict event coding (e.g., Raleigh et al., 2010; Sundberg & Melander, 2013) and reflects the operational risks donors face when the majority of an administrative unit falls under a sanctioned authority. That said, this definition may slightly overstate Houthi presence in certain southern districts, such as parts of Aden or Lahij, where control was more fragmented.

Disaggregating by donor sharpens this picture. U.S. humanitarian disbursements to treated governorates fall by 1.6 log points in the five-year pre-/one-year post-treatment model (p = 0.020) and by 1.4 log points in the eight-year pre-/one-year post-treatment model (p = 0.036). U.S. commitments also decline, with the estimate from the eight-year window approaching significance at the 10 percent level (–1.7 log points; p = 0.056). These results indicate a swift and donor-specific contraction in aid flows, consistent with the chilling effect hypothesis. They also underscore the particular vulnerability of humanitarian channels to reputational and legal risks following an FTO designation.

Figure 7. Map of Treated Districts and Estimated Effects of Houthi Terrorist Designation on Aid to Yemen



Treated districts:
Abyan, Al Bayda', Ad Dali', Al Hudaydah, Al Jawf, Al Mahwit, 'Amran, Dhamar, Hajjah, Ibb, Lahij, Ma'rib, Raymah, Sa'dah, San`a`, Ta'izz, 'Adan

Notes: Estimates follow Figure 3, using a five-year pre-/one-year post-treatment window (2016–2021). Results shown for humanitarian aid only: all donors (red) and U.S.-specific effects (blue). Bottom panel maps treated governorates under Houthi control at the time of the 2021 designation. See Appendix Table C5 for full results.

Taken together, the subnational results lend conditional support to the strategic aid hypothesis. In four of the five cases, Sri Lanka, Mali (Ansar Dine), Nigeria, and Mali (ISIS-GS), estimated effects on aid commitments and disbursements are generally positive in the period following designation, suggesting that donors may increase or maintain engagement in areas under the control of designated groups. While most of these effects are statistically imprecise, the consistent direction across multiple aid types and model specifications, particularly among non-U.S. donors, points to a strategic recalibration of aid flows rather than wholesale disengagement.

Yemen emerges as a clear outlier. In the year following the January 2021 designation of the Houthis, humanitarian aid flows to Houthi-controlled governorates declined substantially. While aggregate estimates are not statistically significant, U.S.-specific models show disbursement reductions of 1.6 to 1.4 log points, with confidence intervals excluding zero in two of the three specifications. These results provide the strongest evidence in our study of a chilling effect, underscoring how legal and reputational risks can disproportionately impact humanitarian channels, particularly when donor governments are primary funders and implementing partners face high compliance burdens.

These patterns underscore the value of subnational analysis in identifying effects that may be masked in national aggregates. While spatially disaggregated differences are often modest, they reveal meaningful variation in how donors respond to terrorist designations. Contextual factors, including the timing of designation, donor composition, and legal exposure, shape whether aid is disrupted, redirected, or sustained. In this way, the subnational evidence both complements and qualifies the broader cross-national patterns, illustrating how strategic and chilling effects can coexist across different settings and sectors.

Cross-national Robustness

To assess whether the observed post-designation aid shifts could have occurred by chance, we conduct placebo tests using randomized inference. Specifically, we randomly assign designation years across the set of actually treated countries and re-estimate the t+1 event-study coefficient 10,000 times. This generates a distribution of placebo estimates under the null hypothesis of no causal effect. Appendix D1 presents the resulting placebo distributions for each of our four primary outcomes (Figures D1.1–D1.4).

For humanitarian aid from all donors, the actual t+1 estimate (–0.152) falls near the center of the placebo distribution (Figure D1.1). The empirical p-value is 0.4687, indicating that nearly 47% of placebo simulations produce effects of equal or greater magnitude. This suggests that the observed decline is statistically indistinguishable from random variation, offering no robust evidence of a chilling effect on aggregate humanitarian flows.

By contrast, the placebo test for development aid from all donors provides strong evidence of a non-random effect. The actual t+1 estimate (0.433) lies far outside the simulated distribution (Figure D1.2), with an empirical p-value of 0.000. This supports the interpretation that designations raise the strategic salience of affected countries and prompt compensatory increases in development aid.

For U.S. humanitarian aid, the t+1 estimate (0.373) lies in the upper tail of the placebo distribution (Figure D1.3), with an empirical p-value of 0.078. While this falls just outside conventional thresholds for statistical significance, the result suggests a potential short-term donor response rather than pure noise.

Finally, the placebo test for U.S. development aid yields an actual estimate (0.206) that is directionally consistent with a strategic response but not statistically distinct from the placebo distribution (empirical p = 0.189; Figure D1.4). Roughly one in five simulations produced effects of similar magnitude, suggesting that while the pattern aligns with the broader trend, it should be interpreted with caution.

Taken together, the placebo tests bolster our main conclusion: development aid flows, especially from the broader donor community, tend to increase following terrorist designations in a manner unlikely to be driven by chance. At the same time, the robustness checks underscore that evidence for systematic changes in humanitarian aid is more limited, particularly at the aggregate level, though patterns in U.S. flows hint at short-term adjustments that may merit further investigation.

Subnational Robustness

To assess the sensitivity of our baseline estimates, we conduct two complementary robustness exercises using alternative estimation strategies. The first approach estimates reduced-form DiD models that include subnational covariates (population density and nighttime lights) but exclude fixed effects and wild cluster bootstrapping. This stripped-down specification preserves degrees of freedom in cases with few treated units or limited post-treatment periods and allows us to test whether baseline results are robust to changes in model structure and inference procedures.

Across most cases, the direction of effects is broadly consistent with the main results, though magnitudes and significance levels vary. In Sri Lanka, estimates are small and statistically insignificant, reflecting the imprecision of long-run effects. In Mali (Ansar Dine), disbursement effects strengthen and reach marginal significance, while commitments become slightly negative, suggesting sensitivity to specification. For Nigeria, disbursements remain positive and become statistically significant, reinforcing the strategic aid interpretation. In Mali (ISIS-GS), effects remain positive but imprecise. Yemen diverges most clearly: the stripped-down models yield a marginally significant positive treatment effect on commitments and a positive but insignificant effect on disbursements, a marked contrast to the consistently negative baseline estimates. This reversal underscores the sensitivity of results to inference strategy in fragile contexts with short panels and high volatility. Full regression tables with controls and corresponding event study plots are available in Appendix D2.

The second approach implements randomized inference, relying on placebo tests to evaluate whether our estimated treatment effects could plausibly arise by chance. These models include ADM1 and year fixed effects and robust standard errors but exclude subnational covariates. We conduct two types of permutation tests: (1) spatial randomization, in which treatment labels are randomly assigned across ADM1 units while holding the number of treated units constant, and (2) temporal randomization, in which the treatment year is randomly reassigned across the available pre- and post-period. These tests generate a distribution of placebo estimates that serve as a non-parametric benchmark for evaluating the extremity of the observed treatment effect.

Randomized inference results are broadly consistent with the patterns observed in the reduced-form models. In Sri Lanka, the treatment effect on commitments remains strongly positive and unlikely to arise under random assignment, while the disbursement effect is indistinguishable from placebo outcomes. In Mali (Ansar Dine), both commitments and disbursements show strong year-level significance, reinforcing the possibility of a modest strategic response. Nigeria shows the clearest pattern: disbursement effects are large and relatively rare in the distribution of spatial placebo estimates, especially under ADM1 permutation. In Mali (ISIS-GS), the disbursement effect is again significant in the year-level test, lending support to a short-term post-designation increase in aid. Yemen, by contrast, shows a notable reversal. The stripped-down model produces positive treatment effects, and the randomized inference tests suggest these fall well within the distribution of placebo estimates. This inconsistency reinforces the need for caution in interpreting the Yemen results, particularly given limitations in sectoral coverage and the narrow post-treatment window. Full randomized inference results are presented in Appendix D3.

Taken together, these robustness checks suggest that our core findings are generally resilient to alternative estimation strategies. At the same time, they highlight the limits of inference in settings with recent treatment events or constrained data availability.

Conclusion

This paper has examined how terrorist designations influence foreign aid allocation, seeking to adjudicate between two competing perspectives: the strategic aid hypothesis, which predicts increased aid following designations, and the chilling effect hypothesis, which expects aid reductions due to legal and operational constraints. Our multi-method approach, combining cross-national event studies, subnational DiD models, and randomized inference, yields several important findings and broader implications.

First, the evidence across both the cross-national and subnational analyses aligns more consistently with the strategic aid hypothesis. We find that development aid tends to increase in the wake of terrorist designations. This should strengthen our belief that designations raise the geopolitical salience of affected areas and prompt donors to use aid as a tool to stabilize fragile environments, counter radicalization, or signal continued engagement. This also implies that previous findings on the impact of terrorism on aid (e.g., Azam and Delacroix 2006; Dreher and Fuchs 2011) extend to counterterrorism efforts.

Second, the evidence reveals only limited spatial targeting, implying that territorial control by terrorist groups is not systematically a barrier to aid distribution. This finding reinforces the strategic aid hypothesis and weakens the chilling effect hypothesis. It also suggests that strategic recalibration is more likely to occur at the national level than through spatially targeted aid allocation. As a possible exception to this pattern, Yemen presents the likeliest case of an observed chilling effect, with baseline models showing declines in humanitarian aid to Houthi-controlled areas following designation.

Third, we find several indications that terrorist designations affect donors differently depending on nationality. While our estimates are imprecise, they suggest that U.S. donors may reduce aid more than other donors. This lends itself to several possible interpretations: Either U.S. donors are more susceptible to chilling effects, or other donors are more likely to allocate aid to combat terrorism. The former interpretation would be consistent with theoretical mechanisms highlighting legal and political constraints—which are likely to bind U.S. actors more deeply—whereas the latter suggests that there is burden-sharing in strategic aid, where non-U.S. donors step in to increase their assistance to locations with U.S.-designated terrorist groups.

Fourth, we find divergent patterns across donor types, especially between development banks and CSOs. The former behave in line with the strategic aid hypothesis whereas the latter appear more likely to experience chilling effects and withdraw in the wake of terrorist designations. This suggests that donor types are varyingly insulated from the constraining effects of terrorist designations, adding important qualifications to the overall pattern.

Methodologically, our study advances a new framework for assessing the aid effects of counterterrorism policy by combining a cross-national event study of donor behavior with subnational difference-in-differences models that isolate spatial variation in aid to designated-group territories. We further implement permutation-based inference for the subnational analysis, a technique not widely adopted in the literature on aid and counterterrorism, despite its advantages in small-sample, clustered settings. Together, these innovations enable us to distinguish policy-induced shifts from underlying trends and to capture both aggregate and geographically targeted donor responses that would be obscured in national-level analyses alone.

Several questions remain unresolved and new puzzles emerge from this study. Looking ahead, future research should seek to deepen our understanding of the conditions under which terrorist designations lead to strategic engagement versus chilling effects. We highlight three priorities for advancing this agenda. First, the mechanisms driving donor heterogeneity should be more systematically analyzed. Differences in legal risk, institutional capacity, and political accountability may explain why some donors escalate aid while others withdraw. Second, more empirical work is needed on the design and implementation of humanitarian carve-outs and licensing regimes. It is possible that variation in their application explains some of the divergent results in our study. Third, further work is needed to unpack the mechanisms behind localized aid responses. Factors such as the governance capacity of terrorist groups, ideology, and patterns of civilian affiliation may help explain when and where strategic or chilling effects are more likely to occur.

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Appendix A: Data and Empirical Design

A1: Descriptive statistics

A1.1. Descriptive statistics, cross-national data

Variable	N	Mean	SD	Min	Max
Humanitarian Aid (log)	2,631	2.40	1.92	0.00	9.18
Development Aid (log)	2,631	5.81	1.47	1.46	9.89
US Aid (log)	2,631	3.47	1.99	0.00	9.19
Battle Deaths (log)	2,926	1.04	2.33	0.00	11.73
Terror Deaths (log)	2,926	1.13	2.05	0.00	9.55
Democracy (V-Dem)	2,528	0.33	0.21	0.00	0.86
GDP per capita (log)	2,780	7.82	0.97	5.54	9.43
Trade % GDP	2,476	77.82	35.78	4.13	348.00
Rule of Law	2,643	-0.49	0.66	-2.41	1.27

A1.2. Descriptive statistics, subnational data

Country	Variable	N	Mean	SD	Min	Max
Sri Lanka	Commitments (log)	103	14.12	2.06	6.83	19.00
Sri Lanka	Disbursement (log)	72	13.68	2.21	7.36	18.11
Sri Lanka	Population Density	105	678.14	839.45	49.71	2987.15
Sri Lanka	Nighttime Light	105	7.41	9.16	0.00	32.97
Nigeria	Commitments (log)	390	16.24	2.17	8.12	21.00
Nigeria	Disbursement (log)	392	15.77	2.08	8.12	20.96
Nigeria	Population Density	394	379.80	498.95	42.91	3087.85

Nigeria	Nighttime Light	394	2.64	3.88	0.05	17.15
Mali	Commitments (log)	271	15.48	3.63	0.00	19.86
Mali	Disbursement (log)	271	13.38	5.86	0.00	19.41
Mali	Population Density	122	1160.00	3346.32	0.48	13531.92
Mali	Nighttime Light	122	4.14	11.97	0.00	47.03
Yemen	Commitments (log)	122	19.00	2.86	9.18	23.97
Yemen	Disbursement (log)	126	18.36	2.91	9.18	23.59
Yemen	Population Density	121	444.61	970.62	1.91	4699.44
Yemen	Nighttime Light	121	3.39	6.74	0.03	28.70

A2: Creditor Reporting System (CRS) Data and TUFF Geocoding

Since CRS data for 2021 does not come pre-geocoded, we apply the TUFF methodology developed by AidData to assign geographic locations to aid projects manually. TUFF is a systematic, transparent, and replicable method for tracking aid and other financial flows from donors who do not publish comprehensive information about their overseas activities (Custer et al., 2023; Goodman et al., 2024). Originally designed to track Chinese and Saudi Arabian development finance, TUFF extracts project location details from publicly available sources such as government reports, donor documents, and media coverage. These location references are then validated against geospatial datasets, such as OpenStreetMap (OSM), to assign precise geographic coordinates.

In this study, we apply the TUFF methodology to geocode OECD CRS data for Yemen in the year 2021, ensuring consistency with pre-2021 aid data.

In this study, we apply the TUFF methodology to geocode OECD CRS data for Yemen in the year 2021, ensuring consistency with pre-2021 aid data. Given the scope of this study, we did not geocode all projects in the 2021 CRS dataset. Instead, we prioritized sectors most relevant to humanitarian aid: Emergency Response, Conflict, Peace and Security, Disaster Prevention and Preparedness, Water Supply and Sanitation, Development Food Assistance, Population Policies and Reproductive Health, Basic Health, Health (General), Reconstruction Relief and Rehabilitation, and Non-communicable Diseases (NCDs). This corresponds to OECD sector codes: 121, 122, 123, 130, 140, 152, 520, 720, 730, and 740.

Of the 1,014 projects identified in these sectors, 412 were geocoded at the ADM1 level (district or equivalent), following OSM's standard geospatial classification. As part of TUFF, we use the Geospatial Global Chinese Development Finance Dataset (GeoGCDF) method, which extracts project locations from textual descriptions (Goodman et al., 2024). The GeoGCDF methodology relies on OpenStreetMap (OSM) geospatial features to assign project locations, ensuring that CRS 2021 aid flows are spatially comparable to GODAD's pre-2021 data. This enables a harmonized spatial analysis of aid allocation before and after the designation.

To ensure consistency, we restrict the pre-2021 GODAD dataset to the same 10 sectors and aggregate project counts, commitments, and disbursements to the ADM1 level using the even-split allocation method provided in the dataset. This yields a harmonized panel covering 2000–2021.

The table below presents summary statistics for the five key outcome variables used in our extended analysis: commitments, disbursements, project counts, undisbursed amounts, and undisbursed share. All variables are aggregated at the district-year (ADM1-year) level, resulting in 5,191 observations for most outcomes.

A3: Case Selection

Mali (Ansar Dine)
We identify Kidal as the treated ADM1 in the Ansar Dine case. The group's origins are closely linked to Kidal, which served as a key base of operations during the 2012–2013 Northern Mali conflict. Both the United Nations and the United States (U.S. Department of State, n.d.) designated Ansar Dine as a terrorist organization in 2013, and UN Security Council documents confirm that Ansar Dine exerted territorial control in Kidal around the time of the UN designation in 2013.

Mali (ISIS-Greater Sahara)
We code Gao, Kidal, and Timbuktu as treated units in the ISIS-GS case. These areas were selected based on documented patterns of ISIS-GS activity around the time of the U.S. designation in March 2020. The United Nations and European Council on Foreign Relations (ECFR) report ISIS-GS territorial control and attacks in eastern Ménaka and northern Gao, with extensions into Timbuktu and Kidal. UN expert panels in 2023 confirmed the group's continued presence in these regions.

Nigeria (Boko Haram)
We designate Borno, Yobe, and Adamawa states as treated regions based on Boko Haram's known territorial control and operational base in northeastern Nigeria. This classification is supported by the European Union Agency for Asylum (EASO), which identifies these three states as the epicenter of Boko Haram and ISWAP activity around and after the 2013 U.S. designation.

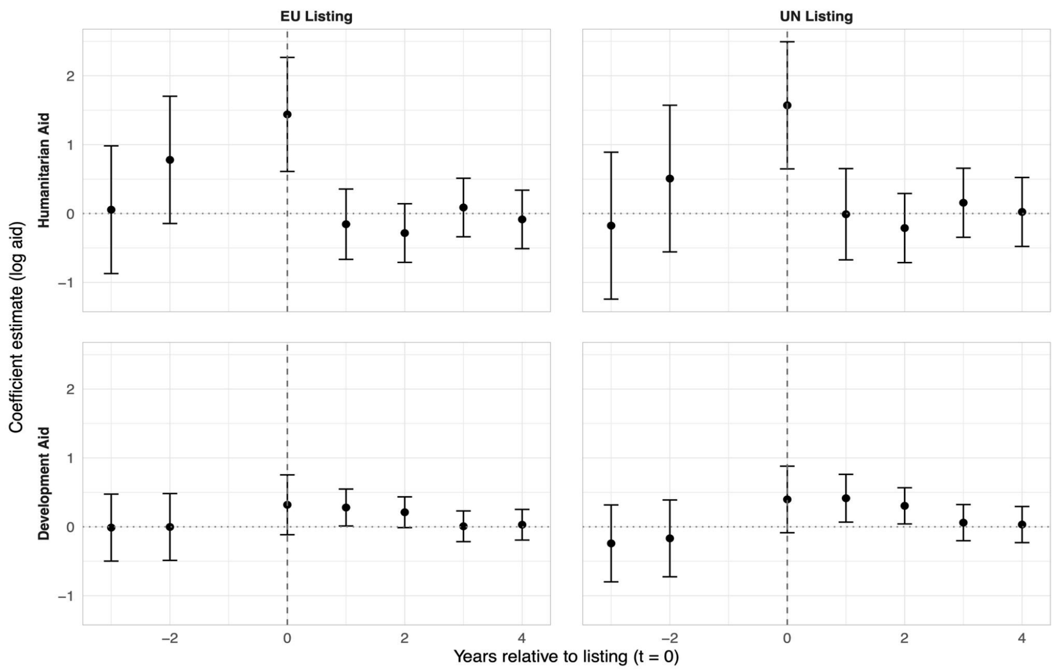
Sri Lanka (LTTE)
For Sri Lanka, LTTE-controlled areas are identified based on Sritharan (2023), who maps the group's de facto territorial control during the civil war. We code the Northern and Eastern provinces as treated, as they were the primary LTTE strongholds during the EU's 2001 designation. This classification is appropriate given the group's sustained control until the end of the war in 2009.

Yemen (Houthis)

Territorial control in Yemen is determined using the ACAPS dataset accessed via the Humanitarian Data Exchange (HDX). We rely on the February 11, 2021, update, closest to the January 2021 U.S. designation of the Houthis. An ADM1 unit is coded as Houthi-controlled if more than 50% of its area was classified as under De Facto Authority (DFA) control. This enables comparison of aid allocation patterns between Houthi and non-Houthi areas.

Appendix B: Cross-national regression analysis

Figure 1. EU and UN terrorist designations and aid allocation, event study coefficients



Note: Estimated coefficients from event study models with country and year fixed effects. Points show changes in log aid commitments relative to the year of EU and UN designations (t = 0), with 95% confidence intervals.

Figure 2. US terrorist designations and aid allocation, event study coefficients (no control variables)

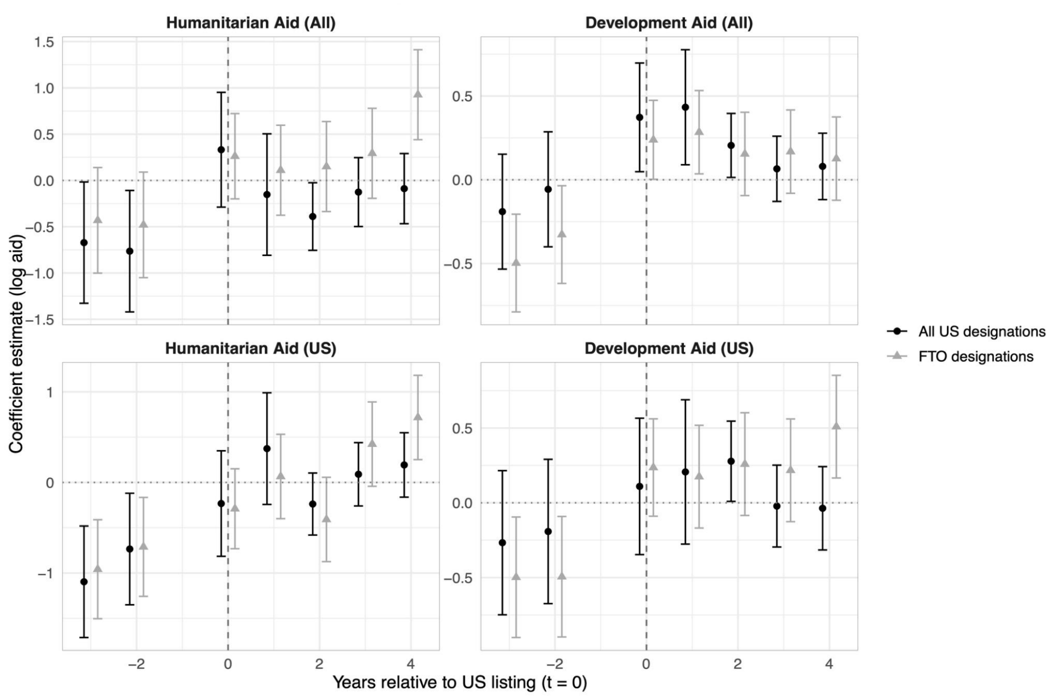


Table B1. US terrorist designations and aid allocation, all types

	Hum (All)	Dev (All)	Hum (US)	Dev (US)
	(1)	(2)	(3)	(4)
US Listing t-3	-0.672**	-0.190	-1.096***	-0.267
	(0.334)	(0.175)	(0.314)	(0.246)
US Listing t-2	-0.764**	-0.057	-0.735**	-0.192
	(0.335)	(0.175)	(0.314)	(0.246)
US Listing t0	0.332	0.373**	-0.233	0.110
	(0.316)	(0.166)	(0.297)	(0.233)
US Listing t+1	-0.152	0.433**	0.373	0.206
	(0.335)	(0.175)	(0.314)	(0.246)
US Listing t+2	-0.390**	0.205**	-0.238	0.278**
	(0.186)	(0.097)	(0.175)	(0.137)
US Listing t+3	-0.126	0.065	0.090	-0.022
	(0.190)	(0.099)	(0.178)	(0.140)
US Listing t+4	-0.088	0.080	0.193	-0.037
	(0.193)	(0.101)	(0.181)	(0.142)
Battle deaths (lag)	0.109***	0.001	0.087***	0.002
	(0.017)	(0.009)	(0.016)	(0.013)
Terrorism deaths (lag)	0.117***	0.017	0.140***	0.068***
	(0.023)	(0.012)	(0.022)	(0.017)
Liberal democracy (lag)	-0.400	0.855***	-1.051***	2.393***
	(0.349)	(0.182)	(0.327)	(0.256)
GDP per capita (lag)	-1.417***	-0.336***	-1.205***	-0.771***
	(0.185)	(0.097)	(0.174)	(0.136)
Trade (lag)	0.005***	0.002***	0.002	0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Rule of law (lag)	-0.401***	-0.041	-0.239**	-0.002
	(0.128)	(0.067)	(0.120)	(0.094)
Constant	12.933***	7.230***	10.280***	8.670***
	(1.633)	(0.854)	(1.533)	(1.201)
Country FE	Yes	Yes	Yes	Yes

Year FE	Yes	Yes	Yes	Yes
Observations	1,985	1,985	1,985	1,985
R^2	0.791	0.871	0.793	0.865

Note: **p<0.1;* ***p<0.05;* ****p<0.01*

Table B2. US terrorist designations and aid allocation, FTO listings

	Hum (All)	Dev (All)	Hum (US)	Dev (US)
	(1)	(2)	(3)	(4)
US FTO Listing t-3	-0.073	-0.308**	-0.603**	0.088
	(0.297)	(0.156)	(0.279)	(0.218)
US FTO Listing t-2	-0.496*	-0.091	-0.365	0.019
	(0.283)	(0.148)	(0.266)	(0.208)
US FTO Listing t0	0.231	0.232	-0.073	0.101
	(0.272)	(0.142)	(0.255)	(0.200)
US FTO Listing t+1	-0.213	0.249*	-0.123	0.083
	(0.245)	(0.128)	(0.230)	(0.180)
US FTO Listing t+2	-0.240	0.122	-0.734***	0.190
	(0.230)	(0.120)	(0.216)	(0.169)
US FTO Listing t+3	-0.135	0.134	0.074	0.109
	(0.230)	(0.121)	(0.216)	(0.169)
US FTO Listing t+4	0.560**	0.067	0.387*	0.389**
	(0.230)	(0.120)	(0.216)	(0.169)
Battle deaths (lag)	0.109***	0.001	0.093***	0.002
	(0.017)	(0.009)	(0.016)	(0.013)
Terrorism deaths (lag)	0.117***	0.016	0.139***	0.065***
	(0.023)	(0.012)	(0.022)	(0.017)
Liberal democracy (lag)	-0.417	0.798***	-1.086***	2.342***
	(0.349)	(0.183)	(0.327)	(0.256)
GDP per capita (lag)	-1.389***	-0.369***	-1.189***	-0.803***
	(0.185)	(0.097)	(0.174)	(0.136)
Trade (lag)	0.005***	0.003***	0.002	0.002
	(0.002)	(0.001)	(0.001)	(0.001)
Rule of law (lag)	-0.394***	-0.029	-0.235*	0.004

	(0.128)	(0.067)	(0.120)	(0.094)
Constant	12.587***	7.552***	10.020***	8.994***
	(1.627)	(0.852)	(1.527)	(1.196)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,985	1,985	1,985	1,985
R^2	0.790	0.870	0.793	0.865

Note: *p<0.1; **p<0.05; ***p<0.01*

Table B3. US terrorist designations and aid allocation, by donor type

	Bilateral	Multilateral	Dev Banks	CSOs
	(1)	(2)	(3)	(4)
US Listing t-3	-0.470**	-0.215	0.233	-0.0004
	(0.196)	(0.236)	(0.449)	(0.291)
US Listing t-2	-0.354*	0.201	-0.190	0.340
	(0.197)	(0.237)	(0.450)	(0.291)
US Listing t0	-0.035	0.247	0.312	-0.292
	(0.186)	(0.224)	(0.425)	(0.275)
US Listing t+1	0.170	-0.513**	0.801*	0.066
	(0.197)	(0.237)	(0.450)	(0.291)
US Listing t+2	0.050	0.120	0.565**	-0.186
	(0.109)	(0.132)	(0.250)	(0.162)
US Listing t+3	-0.074	-0.105	0.669***	-0.310*
	(0.112)	(0.134)	(0.255)	(0.165)
US Listing t+4	-0.025	0.084	0.426	-0.359**
	(0.114)	(0.137)	(0.260)	(0.168)
Battle deaths (lag)	0.013	0.018	-0.001	-0.004
	(0.010)	(0.012)	(0.023)	(0.015)
Terror deaths (lag)	0.040***	0.036**	-0.012	0.111***
	(0.014)	(0.016)	(0.031)	(0.020)
Liberal democracy (lag)	0.473**	1.057***	0.642	-0.781**
	(0.205)	(0.247)	(0.468)	(0.303)
GDP/cap (lag)	-0.340***	-0.232*	0.168	0.191
	(0.109)	(0.131)	(0.249)	(0.161)
Trade (lag)	0.001	0.002*	-0.002	0.005***

	(0.001)	(0.001)	(0.002)	(0.001)
Rule of law (lag)	-0.158**	-0.004	0.674***	0.259**
	(0.075)	(0.090)	(0.172)	(0.111)
Constant	7.180***	4.564***	-0.857	-2.158
	(0.959)	(1.155)	(2.194)	(1.421)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,985	1,985	1,985	1,985
R^2	0.856	0.781	0.738	0.704

Note: *p<0.1; **p<0.05; ***p<0.0 1

Table B4. EU and UN terrorist designations and aid allocation

	Hum (all) (EU)	Dev (all) (EU)	Hum (all) (UN)	Dev (all) (UN)
	(1)	(2)	(3)	(4)
Listing t-3	0.055	-0.012	-0.176	-0.241
	(0.473)	(0.248)	(0.544)	(0.285)
Listing t-2	0.779*	-0.002	0.507	-0.168
	(0.471)	(0.247)	(0.543)	(0.284)
Listing t0	1.439***	0.320	1.571***	0.397
	(0.422)	(0.222)	(0.471)	(0.247)
Listing t+1	-0.155	0.280**	-0.010	0.415**
	(0.260)	(0.137)	(0.338)	(0.177)
Listing t+2	-0.284	0.211*	-0.211	0.304**
	(0.217)	(0.114)	(0.256)	(0.134)
Listing t+3	0.088	0.007	0.157	0.061
	(0.217)	(0.114)	(0.256)	(0.134)
Listing t+4	-0.085	0.030	0.023	0.033
	(0.216)	(0.114)	(0.255)	(0.134)
Battle deaths (lag)	0.107***	0.0003	0.106***	0.0001
	(0.017)	(0.009)	(0.017)	(0.009)
Terror deaths (lag)	0.120***	0.018	0.120***	0.018
	(0.023)	(0.012)	(0.023)	(0.012)
Liberal democracy (lag)	-0.355	0.850***	-0.358	0.875***
	(0.349)	(0.183)	(0.350)	(0.183)
GDP/cap (lag)	-1.414***	-0.325***	-1.403***	-0.321***
	(0.187)	(0.098)	(0.186)	(0.098)

Trade (lag)	0.005***	0.003***	0.005***	0.002***
	(0.002)	(0.001)	(0.002)	(0.001)
Rule of law (lag)	-0.403***	-0.038	-0.394***	-0.034
	(0.128)	(0.067)	(0.128)	(0.067)
Constant	12.794***	7.142***	12.690***	7.114***
	(1.644)	(0.863)	(1.639)	(0.858)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Observations	1,985	1,985	1,985	1,985
R^2	0.791	0.870	0.790	0.870

*Notes: *p<0.1; **p<0.05; ***p<0.0* 1

Appendix C: Subnational regression analysis, Full tables

Table C1 . Sri Lanka: DiD Estimates (Wild Cluster Bootstrap, 999 reps)

Aid Type	Outcome	Window	Estimate	Std. Error	p-value	N	R-squared
All Aid	Log Commitments	5pre_5post	2.122	(0.951)	0.125	91	0.388
All Aid	Log Disbursements	5pre_5post	-0.449	(1.822)	0.733	61	0.511
All Aid	Log Commitments	5pre_3post	2.176	(1.092)	0.117	67	0.445
All Aid	Log Disbursements	5pre_3post	0.977	(1.786)	0.449	45	0.620
All Aid	Log Commitments	3pre_3post	1.503	(1.201)	0.262	54	0.501
All Aid	Log Disbursements	3pre_3post	0.666	(1.740)	0.603	38	0.731
All Aid	Log Commitments	3pre_5post	1.570	(1.087)	0.252	78	0.462
All Aid	Log Disbursements	3pre_5post	-0.929	(2.025)	0.591	54	0.577
Humanitarian Aid	Log Commitments	5pre_5post	0.871	(0.800)	0.206	48	0.452

Humanitarian Aid	Log Disbursements	5pre_5post	2.250	(0.835)	0.398	30	0.775
Humanitarian Aid	Log Commitments	5pre_3post	0.696	(1.404)	0.514	35	0.533
Humanitarian Aid	Log Disbursements	5pre_3post	-1.502	(0.000)	0.282	22	0.966
Humanitarian Aid	Log Commitments	3pre_3post	0.238	(1.395)	0.859	28	0.648
Humanitarian Aid	Log Disbursements	3pre_3post	-0.386	(0.000)	0.513	20	0.990
Humanitarian Aid	Log Commitments	3pre_5post	0.600	(0.984)	0.441	41	0.524
Humanitarian Aid	Log Disbursements	3pre_5post	3.299	(0.905)	0.213	28	0.782
Development Aid	Log Commitments	5pre_5post	2.137	(1.373)	0.543	60	0.510
Development Aid	Log Disbursements	5pre_5post	-1.568	(2.360)	0.471	41	0.679
Development Aid	Log Commitments	5pre_3post	2.284	(2.219)	0.611	45	0.567
Development Aid	Log Disbursements	5pre_3post	-0.493	(3.941)	0.798	30	0.714
Development Aid	Log Commitments	3pre_3post	2.255	(2.439)	0.618	37	0.591
Development Aid	Log Disbursements	3pre_3post	-0.658	(4.268)	0.652	24	0.875
Development Aid	Log Commitments	3pre_5post	2.143	(1.411)	0.517	52	0.545
Development Aid	Log Disbursements	3pre_5post	-1.486	(2.448)	0.516	35	0.830

Table C2. Mali, Ansar Dine: DiD Estimates (Wild Cluster Bootstrap, 999 reps)

Aid Type	Outcome	Window	Estimate	Std. Error	p-value	N	R-squared
All Aid	Log Commitments	5pre_5post	0.366	(0.219)	0.459	503	0.099
All Aid	Log Disbursements	5pre_5post	0.303	(0.219)	0.484	531	0.069

All Aid	Log Commitments	5pre_3post	0.388	(0.202)	0.479	432	0.113
All Aid	Log Disbursements	5pre_3post	0.336	(0.172)	0.473	456	0.074
All Aid	Log Commitments	3pre_3post	0.180	(0.207)	0.573	339	0.096
All Aid	Log Disbursements	3pre_3post	0.145	(0.161)	0.520	358	0.080
All Aid	Log Commitments	3pre_5post	0.379	(0.218)	0.470	558	0.064
All Aid	Log Disbursements	3pre_5post	0.345	(0.207)	0.488	591	0.052
US x Treated (All Aid)	Log Commitments	5pre_5post	-2.478	(0.065)	0.711	503	0.109
US x Treated (All Aid)	Log Disbursements	5pre_5post	-0.846	(0.052)	0.723	531	0.070
US x Treated (All Aid)	Log Commitments	5pre_3post	-1.817	(0.074)	0.750	432	0.119
US x Treated (All Aid)	Log Disbursements	5pre_3post	-1.216	(0.069)	0.758	456	0.077
US x Treated (All Aid)	Log Commitments	3pre_3post	-2.476	(0.073)	0.618	339	0.106
US x Treated (All Aid)	Log Disbursements	3pre_3post	-1.433	(0.066)	0.777	358	0.084
US x Treated (All Aid)	Log Commitments	3pre_5post	-2.137	(0.056)	0.774	558	0.073
US x Treated (All Aid)	Log Disbursements	3pre_5post	-0.739	(0.042)	0.754	591	0.053
Humanitarian Aid	Log Commitments	5pre_5post	0.161	(0.220)	0.555	312	0.085
Humanitarian Aid	Log Disbursements	5pre_5post	0.158	(0.206)	0.562	342	0.077
Humanitarian Aid	Log Commitments	5pre_3post	0.034	(0.201)	0.867	271	0.094

Humanitarian Aid	Log Disbursements	5pre_3post	0.235	(0.162)	0.504	294	0.083
Humanitarian Aid	Log Commitments	3pre_3post	-0.140	(0.212)	0.559	220	0.110
Humanitarian Aid	Log Disbursements	3pre_3post	0.252	(0.172)	0.477	240	0.096
Humanitarian Aid	Log Commitments	3pre_5post	-0.040	(0.205)	0.852	358	0.075
Humanitarian Aid	Log Disbursements	3pre_5post	0.084	(0.195)	0.691	390	0.057
US x Treated (Humanitarian)	Log Commitments	5pre_5post	-1.841	(0.065)	0.727	312	0.092
US x Treated (Humanitarian)	Log Disbursements	5pre_5post	-0.512	(0.103)	0.555	342	0.077
US x Treated (Humanitarian)	Log Commitments	5pre_3post	-1.759	(0.096)	0.828	271	0.100
US x Treated (Humanitarian)	Log Disbursements	5pre_3post	-0.863	(0.100)	0.777	294	0.085
US x Treated (Humanitarian)	Log Commitments	3pre_3post	-2.623	(0.111)	0.520	220	0.122
US x Treated (Humanitarian)	Log Disbursements	3pre_3post	-0.503	(0.101)	0.516	240	0.096
US x Treated (Humanitarian)	Log Commitments	3pre_5post	-2.090	(0.044)	0.637	358	0.085
US x Treated (Humanitarian)	Log Disbursements	3pre_5post	-0.615	(0.086)	0.535	390	0.059
Development Aid	Log Commitments	5pre_5post	0.427	(0.339)	0.460	380	0.152
Development Aid	Log Disbursements	5pre_5post	0.321	(0.274)	0.496	406	0.108
Development Aid	Log Commitments	5pre_3post	0.455	(0.340)	0.445	320	0.180
Development Aid	Log Disbursements	5pre_3post	0.019	(0.263)	0.945	342	0.116

Development Aid	Log Commitments	3pre_3post	0.079	(0.331)	0.810	247	0.168
Development Aid	Log Disbursements	3pre_3post	-0.512	(0.248)	0.461	268	0.128
Development Aid	Log Commitments	3pre_5post	0.570	(0.335)	0.452	423	0.112
Development Aid	Log Disbursements	3pre_5post	0.353	(0.247)	0.461	459	0.092
US x Treated (Development)	Log Commitments	5pre_5post	-2.492	(0.075)	0.680	380	0.164
US x Treated (Development)	Log Disbursements	5pre_5post	-1.662	(0.067)	0.773	406	0.114
US x Treated (Development)	Log Commitments	5pre_3post	-1.832	(0.092)	0.626	320	0.186
US x Treated (Development)	Log Disbursements	5pre_3post	-1.864	(0.089)	0.758	342	0.124
US x Treated (Development)	Log Commitments	3pre_3post	-2.381	(0.101)	0.602	247	0.178
US x Treated (Development)	Log Disbursements	3pre_3post	-2.236	(0.103)	0.730	268	0.138
US x Treated (Development)	Log Commitments	3pre_5post	-2.219	(0.067)	0.701	423	0.122
US x Treated (Development)	Log Disbursements	3pre_5post	-1.407	(0.056)	0.801	459	0.096

Table C3. Nigeria: Difference-in-Differences Estimates (Wild Cluster Bootstrap, 999 reps)

Aid Type	Outcome	Window	Estimate	Std. Error	p-value	N	R-squared
All Aid	Log Commitments	5pre_5post	0.492	(0.860)	0.618	772	0.149
All Aid	Log Disbursements	5pre_5post	1.043	(0.589)	0.360	803	0.167
All Aid	Log Commitments	5pre_3post	0.063	(0.842)	0.929	671	0.167
All Aid	Log Disbursements	5pre_3post	0.506	(0.586)	0.423	705	0.175

All Aid	Log Commitments	3pre_3post	-0.596	(1.423)	0.844	468	0.135
All Aid	Log Disbursements	3pre_3post	0.197	(1.064)	0.866	492	0.151
All Aid	Log Commitments	3pre_5post	0.695	(1.183)	0.587	813	0.099
All Aid	Log Disbursements	3pre_5post	1.183	(0.840)	0.323	834	0.118
US x Treated (All Aid)	Log Commitments	5pre_5post	-2.101	(0.748)	0.105	772	0.154
US x Treated (All Aid)	Log Disbursements	5pre_5post	-1.349	(0.848)	0.360	803	0.168
US x Treated (All Aid)	Log Commitments	5pre_3post	-2.713	(0.634)	0.111	671	0.175
US x Treated (All Aid)	Log Disbursements	5pre_3post	-2.093	(0.929)	0.114	705	0.180
US x Treated (All Aid)	Log Commitments	3pre_3post	-5.571	(0.391)	0.119	468	0.158
US x Treated (All Aid)	Log Disbursements	3pre_3post	-3.812	(1.369)	0.110	492	0.166
US x Treated (All Aid)	Log Commitments	3pre_5post	-1.669	(1.042)	0.345	813	0.102
US x Treated (All Aid)	Log Disbursements	3pre_5post	-1.202	(1.041)	0.325	834	0.119
Humanitarian Aid	Log Commitments	5pre_5post	1.332	(0.745)	0.284	330	0.355
Humanitarian Aid	Log Disbursements	5pre_5post	1.084	(0.781)	0.359	323	0.369
Humanitarian Aid	Log Commitments	5pre_3post	0.573	(0.734)	0.477	296	0.341
Humanitarian Aid	Log Disbursements	5pre_3post	0.300	(0.781)	0.783	285	0.374
Humanitarian Aid	Log Commitments	3pre_3post	-0.496	(1.894)	0.814	198	0.318

Humanitarian Aid	Log Disbursements	3pre_3post	0.475	(1.500)	0.795	201	0.401
Humanitarian Aid	Log Commitments	3pre_5post	0.439	(1.380)	0.776	329	0.303
Humanitarian Aid	Log Disbursements	3pre_5post	0.886	(1.121)	0.740	343	0.315
Development Aid	Log Commitments	5pre_5post	0.190	(1.114)	0.881	621	0.171
Development Aid	Log Disbursements	5pre_5post	0.645	(0.704)	0.434	623	0.223
Development Aid	Log Commitments	5pre_3post	-0.230	(1.008)	0.846	539	0.197
Development Aid	Log Disbursements	5pre_3post	0.255	(0.665)	0.702	544	0.240
Development Aid	Log Commitments	3pre_3post	-1.086	(1.668)	0.818	375	0.168
Development Aid	Log Disbursements	3pre_3post	-0.607	(1.368)	0.854	376	0.193
Development Aid	Log Commitments	3pre_5post	0.162	(1.552)	0.899	645	0.119
Development Aid	Log Disbursements	3pre_5post	0.507	(1.244)	0.804	640	0.141
US x Treated (Humanitarian)	Log Commitments	5pre_5post	0.743	(1.122)	0.891	330	0.351
US x Treated (Humanitarian)	Log Disbursements	5pre_5post	0.046	(1.219)	0.875	323	0.366
US x Treated (Humanitarian)	Log Commitments	5pre_3post	0.569	(1.002)	0.694	296	0.340
US x Treated (Humanitarian)	Log Disbursements	5pre_3post	0.291	(0.997)	0.766	285	0.374
US x Treated (Humanitarian)	Log Disbursements	3pre_3post	1.620	(0.381)	0.396	201	0.402
US x Treated (Humanitarian)	Log Commitments	3pre_5post	1.220	(1.138)	0.929	329	0.305

US x Treated (Humanitarian)	Log Disbursements	3pre_5post	-0.548	(2.458)	0.710	343	0.314
US x Treated (Development)	Log Commitments	5pre_5post	-2.617.	(0.503)	0.097	621	0.179
US x Treated (Development)	Log Disbursements	5pre_5post	-1.764	(0.620)	0.127	623	0.226
US x Treated (Development)	Log Commitments	5pre_3post	-2.920.	(0.653)	0.098	539	0.206
US x Treated (Development)	Log Disbursements	5pre_3post	-2.780	(0.657)	0.108	544	0.250
US x Treated (Development)	Log Commitments	3pre_3post	-5.817.	(0.468)	0.090	375	0.196
US x Treated (Development)	Log Disbursements	3pre_3post	-5.421.	(0.607)	0.084	376	0.225
US x Treated (Development)	Log Commitments	3pre_5post	-2.594	(0.451)	0.108	645	0.128
US x Treated (Development)	Log Disbursements	3pre_5post	-2.039	(0.674)	0.101	640	0.149

Table C4. Mali, ISIS: DiD Estimates (Wild Cluster Bootstrap, 999 reps)

Aid Type	Outcome	Window	Estimate	Std. Error	p-value	N	R-squared
All Aid	Log Commitments	5pre_1post	-0.268	(0.325)	0.496	439	0.027
All Aid	Log Disbursements	5pre_1post	0.418	(0.317)	0.273	474	0.054
All Aid	Log Commitments	5pre_2post	-0.160	(0.201)	0.497	458	0.042
All Aid	Log Disbursements	5pre_2post	0.320	(0.267)	0.303	492	0.062
All Aid	Log Commitments	3pre_1post	-0.132	(0.338)	0.732	309	0.036
All Aid	Log Disbursements	3pre_1post	0.449	(0.276)	0.203	334	0.080

All Aid	Log Commitments	3pre_2post	-0.025	(0.225)	0.910	328	0.052
All Aid	Log Disbursements	3pre_2post	0.359	(0.235)	0.270	352	0.089
US x Treated (All Aid)	Log Commitments	5pre_1post	1.923	(0.183)	0.595	439	0.028
US x Treated (All Aid)	Log Disbursements	5pre_1post	0.630	(0.702)	0.418	474	0.054
US x Treated (All Aid)	Log Commitments	5pre_2post	1.889	(0.191)	0.556	458	0.043
US x Treated (All Aid)	Log Disbursements	5pre_2post	0.638	(0.699)	0.418	492	0.062
US x Treated (All Aid)	Log Commitments	3pre_1post	2.063	(0.163)	0.559	309	0.038
US x Treated (All Aid)	Log Disbursements	3pre_1post	0.644	(0.673)	0.363	334	0.079
US x Treated (All Aid)	Log Commitments	3pre_2post	2.005	(0.176)	0.562	328	0.055
US x Treated (All Aid)	Log Disbursements	3pre_2post	0.653	(0.671)	0.375	352	0.089
Humanitarian Aid	Log Commitments	5pre_1post	0.207	(0.444)	0.676	283	0.064
Humanitarian Aid	Log Disbursements	5pre_1post	0.693	(0.525)	0.277	313	0.064
Humanitarian Aid	Log Commitments	5pre_2post	0.008	(0.469)	0.984	289	0.064
Humanitarian Aid	Log Disbursements	5pre_2post	0.634	(0.487)	0.293	319	0.063
Humanitarian Aid	Log Commitments	3pre_1post	0.386	(0.512)	0.504	198	0.081
Humanitarian Aid	Log Disbursements	3pre_1post	0.740	(0.505)	0.254	218	0.091
Humanitarian Aid	Log Commitments	3pre_2post	0.182	(0.550)	0.793	204	0.079

Humanitarian Aid	Log Disbursements	3pre_2post	0.692	(0.471)	0.246	224	0.089
Development Aid	Log Commitments	5pre_1post	0.097	(0.388)	0.803	355	0.037
Development Aid	Log Disbursements	5pre_1post	0.566.	(0.298)	0.074	394	0.066
Development Aid	Log Commitments	5pre_2post	0.098	(0.222)	0.681	374	0.063
Development Aid	Log Disbursements	5pre_2post	0.340	(0.214)	0.168	412	0.081
Development Aid	Log Commitments	3pre_1post	0.396	(0.366)	0.279	259	0.038
Development Aid	Log Disbursements	3pre_1post	0.777*	(0.275)	0.031	288	0.069
Development Aid	Log Commitments	3pre_2post	0.383	(0.208)	0.126	278	0.067
Development Aid	Log Disbursements	3pre_2post	0.537*	(0.198)	0.020	306	0.086
US x Treated (Humanitarian)	Log Commitments	5pre_1post	NA	(NA)	NA	283	0.064
US x Treated (Humanitarian)	Log Disbursements	5pre_1post	1.161	(0.585)	0.246	313	0.063
US x Treated (Humanitarian)	Log Commitments	5pre_2post	NA	(NA)	NA	289	0.064
US x Treated (Humanitarian)	Log Disbursements	5pre_2post	1.155	(0.591)	0.250	319	0.062
US x Treated (Humanitarian)	Log Commitments	3pre_1post	NA	(NA)	NA	198	0.079
US x Treated (Humanitarian)	Log Disbursements	3pre_1post	1.203	(0.522)	0.176	218	0.090
US x Treated (Humanitarian)	Log Commitments	3pre_2post	NA	(NA)	NA	204	0.079
US x Treated (Humanitarian)	Log Disbursements	3pre_2post	1.191	(0.533)	0.188	224	0.087

US x Treated (Development)	Log Commitments	5pre_1post	2.190	(0.211)	0.563	355	0.040
US x Treated (Development)	Log Disbursements	5pre_1post	-1.100	(0.167)	0.570	394	0.064
US x Treated (Development)	Log Commitments	5pre_2post	2.144	(0.222)	0.543	374	0.066
US x Treated (Development)	Log Disbursements	5pre_2post	-1.110	(0.170)	0.566	412	0.081
US x Treated (Development)	Log Commitments	3pre_1post	2.399	(0.163)	0.571	259	0.042
US x Treated (Development)	Log Disbursements	3pre_1post	-0.930	(0.142)	0.645	288	0.065
US x Treated (Development)	Log Commitments	3pre_2post	2.318	(0.186)	0.589	278	0.070
US x Treated (Development)	Log Disbursements	3pre_2post	-0.953	(0.150)	0.621	306	0.084

Table C5. Yemen: DiD Estimates (Wild Cluster Bootstrap, 999 reps)

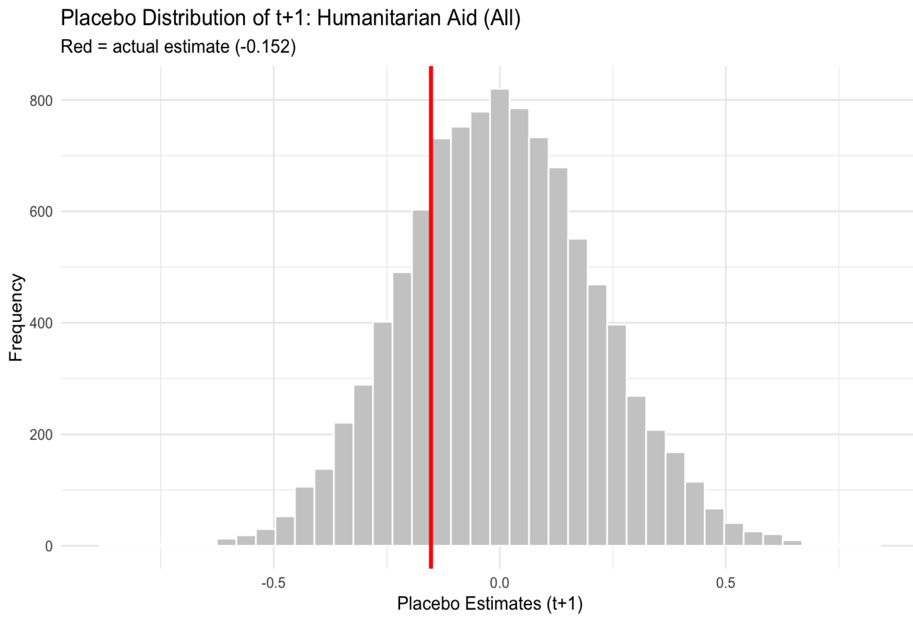
Aid Type	Outcome	Window	Estimate	Std. Error	p-value	N	R-squared
Humanitarian Aid	Log Commitments	3pre	-3.370	(0.141)	0.205	148	0.217
Humanitarian Aid	Log Disbursements	3pre	-2.827	(0.148)	0.184	161	0.255
Humanitarian Aid	Log Commitments	5pre	-3.173	(0.159)	0.338	217	0.269
Humanitarian Aid	Log Disbursements	5pre	-2.948	(0.142)	0.277	233	0.288
Humanitarian Aid	Log Commitments	8pre	-1.827	(0.111)	0.357	320	0.239
Humanitarian Aid	Log Disbursements	8pre	-1.805	(0.091)	0.242	328	0.272
US x Treated (Humanitarian)	Log Commitments	3pre	-1.014	(0.922)	0.280	148	0.232
US x Treated (Humanitarian)	Log Disbursements	3pre	-1.315	(0.798)	0.109	161	0.288
US x Treated (Humanitarian)	Log Commitments	5pre	-1.374	(0.787)	0.107	217	0.294

US x Treated (Humanitarian)	Log Disbursements	5pre	-1.607*	(0.607)	0.021	233	0.326
US x Treated (Humanitarian)	Log Commitments	8pre	-1.701.	(0.719)	0.058	320	0.281
US x Treated (Humanitarian)	Log Disbursements	8pre	-1.448*	(0.547)	0.031	328	0.307

Appendix D: Robustness Regressions

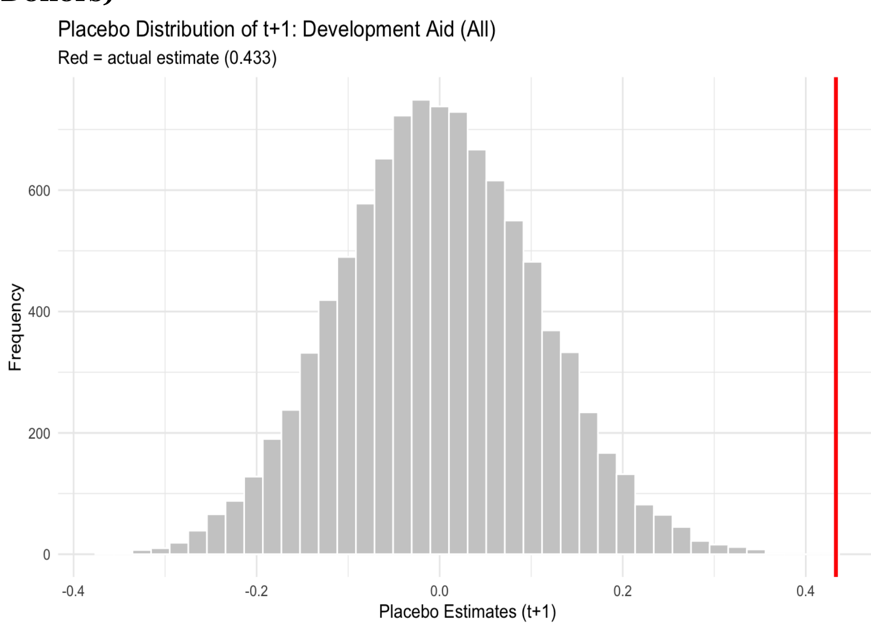
D1: Cross-national Robustness

Figure D1.1. Randomized Placebo Distribution of t+1 Coefficient: Humanitarian Aid (All Donors)



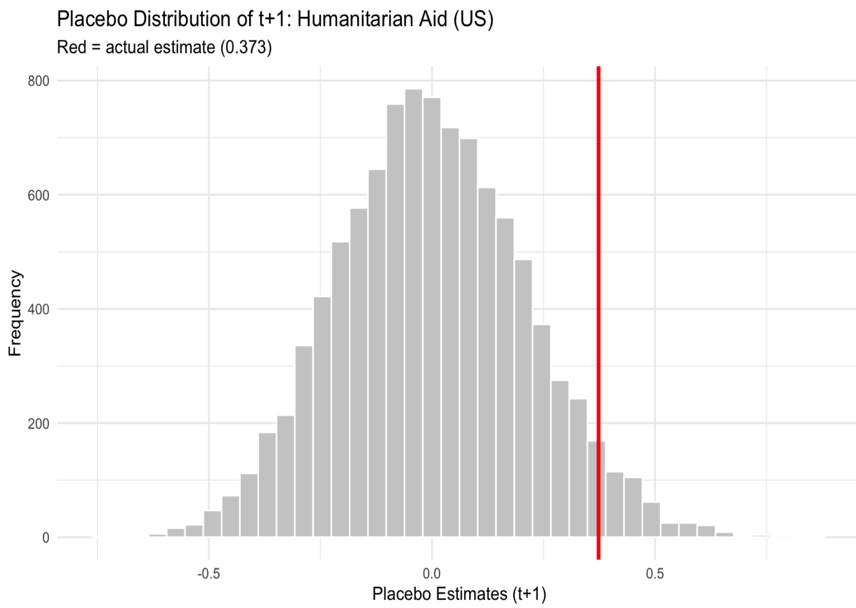
Notes: Histogram displays the distribution of placebo estimates for the t+1 coefficient on log humanitarian aid (all donors), based on 10,000 simulations with randomly assigned designation years across treated countries. The red vertical line marks the actual estimate from the event study (−0.152). The empirical p-value is 0.4687, indicating that the observed decline is not statistically distinguishable from random variation.

Figure D1.2. Randomized Placebo Distribution of t+1 Coefficient: Development Aid (All Donors)



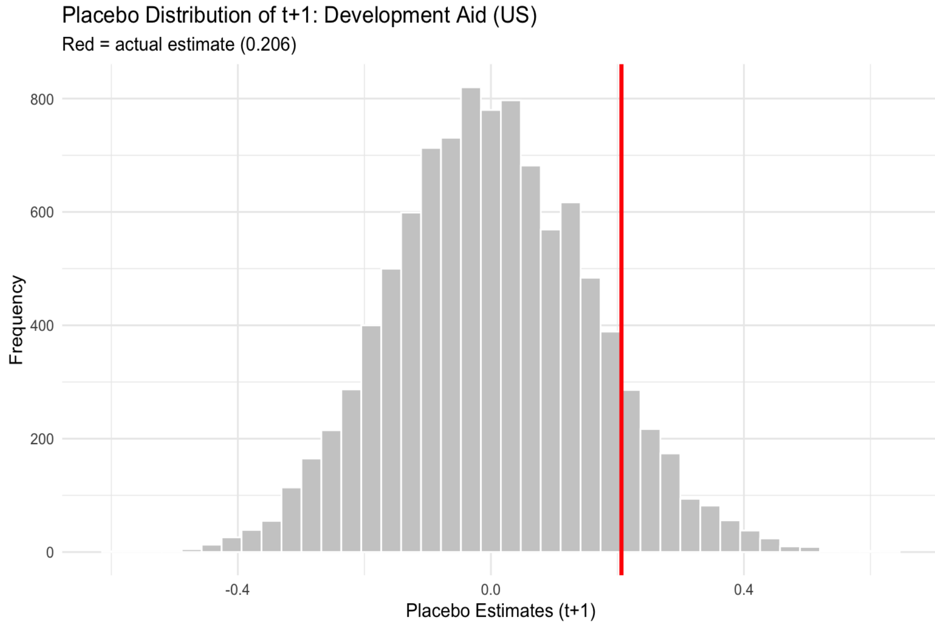
Notes: Histogram displays the distribution of placebo estimates for the t+1 coefficient on log development aid (all donors), based on 10,000 simulations with randomly assigned designation years across treated countries. The red vertical line marks the actual estimate from the event study (0.433). The empirical p-value is 0.000, indicating that none of the placebo simulations produced a coefficient as large as the observed estimate. This strongly suggests that the increase in development aid following designation is unlikely to be due to chance.

Figure D1.3. Randomized Placebo Distribution of t+1 Coefficient: Humanitarian Aid (US)



Notes: Histogram displays the distribution of placebo estimates for the t+1 coefficient on log humanitarian aid from the United States, based on 10,000 simulations with randomly assigned designation years across treated countries. The red vertical line marks the actual estimate from the event study (0.373). The empirical p-value is 0.078, indicating that fewer than 8% of placebo simulations produced an effect of this magnitude or larger. This suggests that the observed post-designation increase is unlikely to be due to chance, though the result falls just outside conventional significance thresholds.

Figure D1.4. Randomized Placebo Distribution of t+1 Coefficient: Development Aid (US)



Notes: Histogram displays the distribution of placebo estimates for the t+1 coefficient on log development aid from the United States, based on 10,000 simulations with randomly assigned designation years across treated countries. The red vertical line marks the actual estimate from the event study (0.206). The empirical p-value is 0.189, indicating that nearly 19% of placebo simulations produced estimates of this magnitude or greater. This suggests the observed increase is not statistically distinguishable from random variation.

D2: Subnational Robustness - Regressions with controls

Table D2.1. Robustness: Difference-in-Differences Estimates: Sri Lanka(LTTE, 1992-2002)

	Log Commitments	Log Disbursements
Treatment (Treated × Post)	-0.077	0.430
	(0.541)	(0.679)
Population Density	0.001	0.000
	(0.001)	(0.001)
Nighttime Lights	-0.008	0.088
	(0.074)	(0.080)
Num.Obs.	103	72

*Notes: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001*

Figure D2.1. Robustness: Event Study: Sri Lanka(LTTE, 1992-2002)

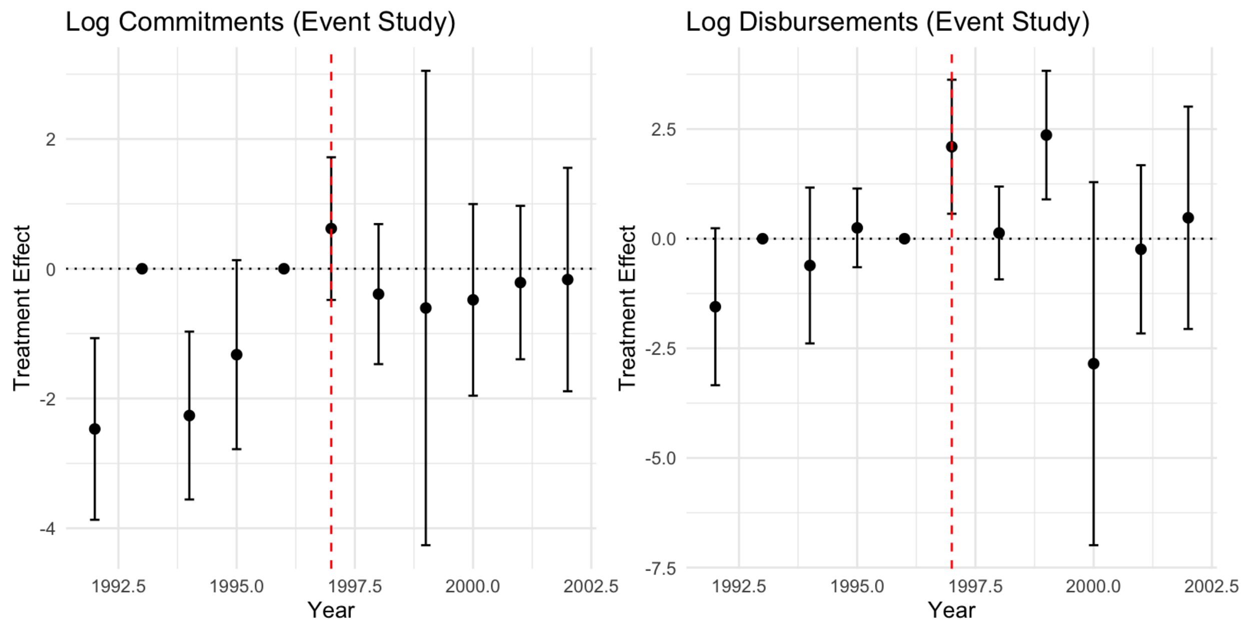


Table D2.2. Robustness: Difference-in-Differences Estimates: Mali (Ansar Dine, 2008–2018)

	Log Commitments	Log Disbursements
Treatment (Treated × Post)	-0.333	1.081+
	(0.357)	(0.583)
Population Density	0.000	0.001
	(0.000)	(0.001)
Nighttime Lights	-0.002	-0.346
	(0.033)	(0.330)
Num.Obs.	104	104

*Notes: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001*

Figure D2.2. Robustness: Event Study: Mali (Ansar Dine, 2008–2018)

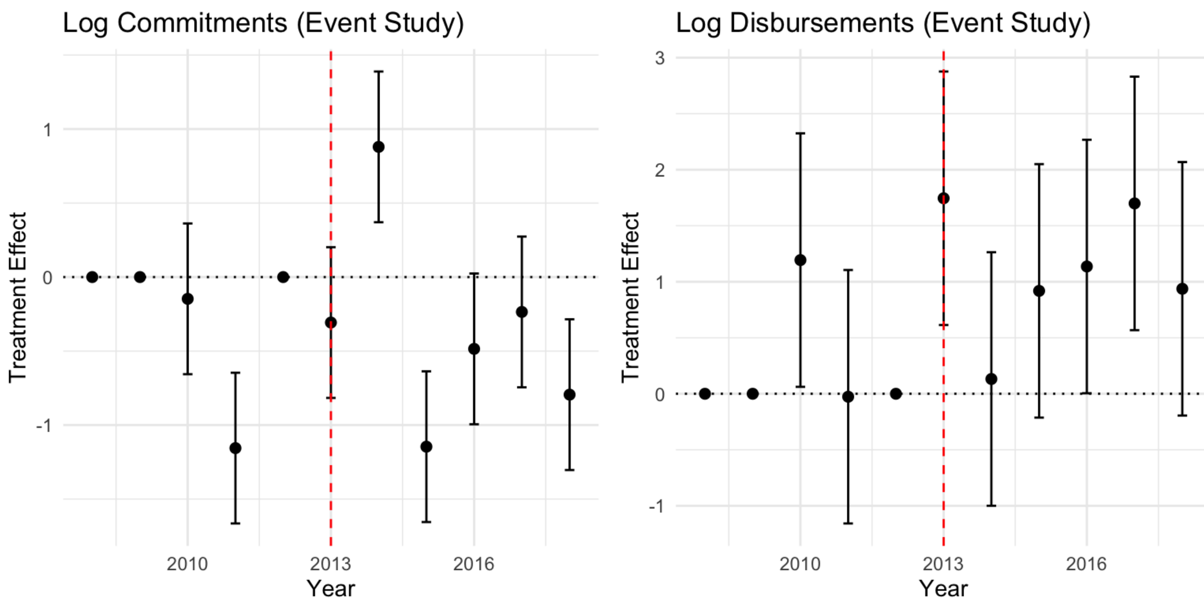


Figure D2.3. Robustness: Difference-in-Differences Estimates: Nigeria (Boko Haram, 2008–2018)

	Log Commitments	Log Disbursements
Treatment (Treated × Post)	0.634	0.947*
	(0.480)	(0.441)
Population Density	0.001**	0.001**
	(0.000)	(0.000)
Nighttime Lights	-0.037	-0.038
	(0.047)	(0.040)
Num.Obs.	390	392

*Notes: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001*

Figure D2.3. Robustness: Event Study: Nigeria (Boko Haram, 2008–2018)

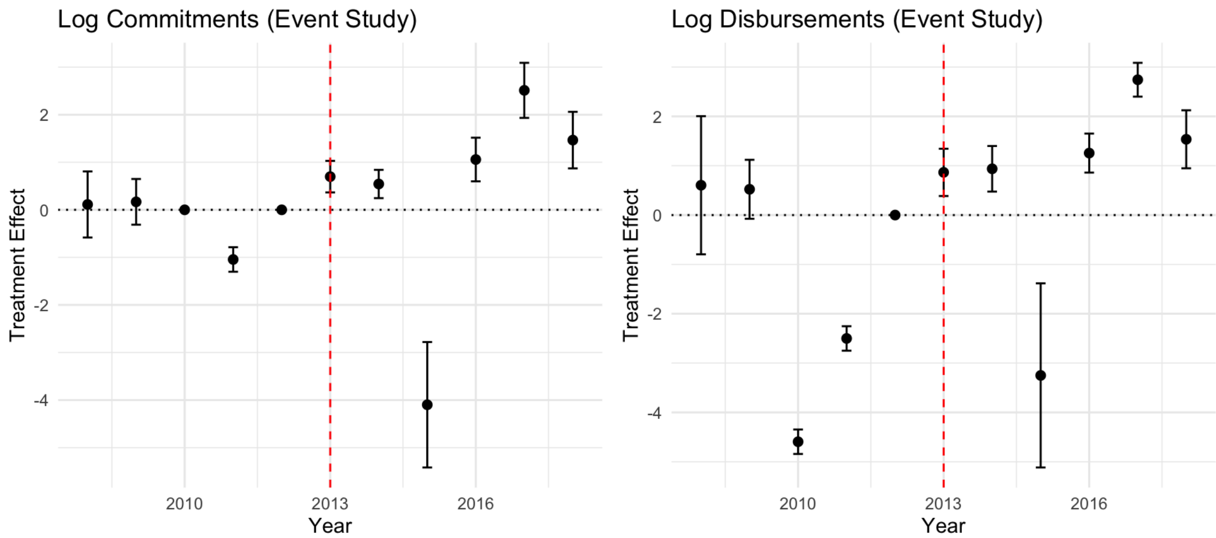


Table D2.4. Robustness: Difference-in-Differences Estimates: Mali, ISIS (2015–2020)

	Log Commitments	Log Disbursements
Treatment (Treated × Post)	0.287	0.998
	(0.585)	(0.818)
Population Density	0.001+	0.000
	(0.000)	(0.000)
Nighttime Lights	-0.149	-0.047
	(0.090)	(0.148)
Num.Obs.	56	56

*Notes: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001*

Figure D2.4. Robustness: Event Study: Mali, ISIS (2015–2020)

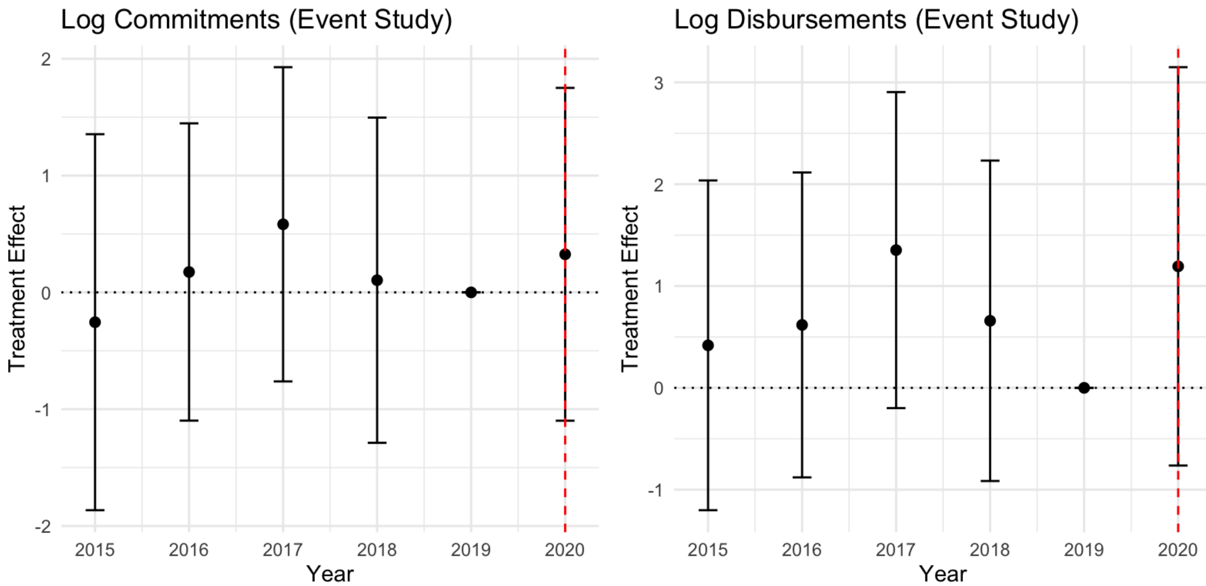
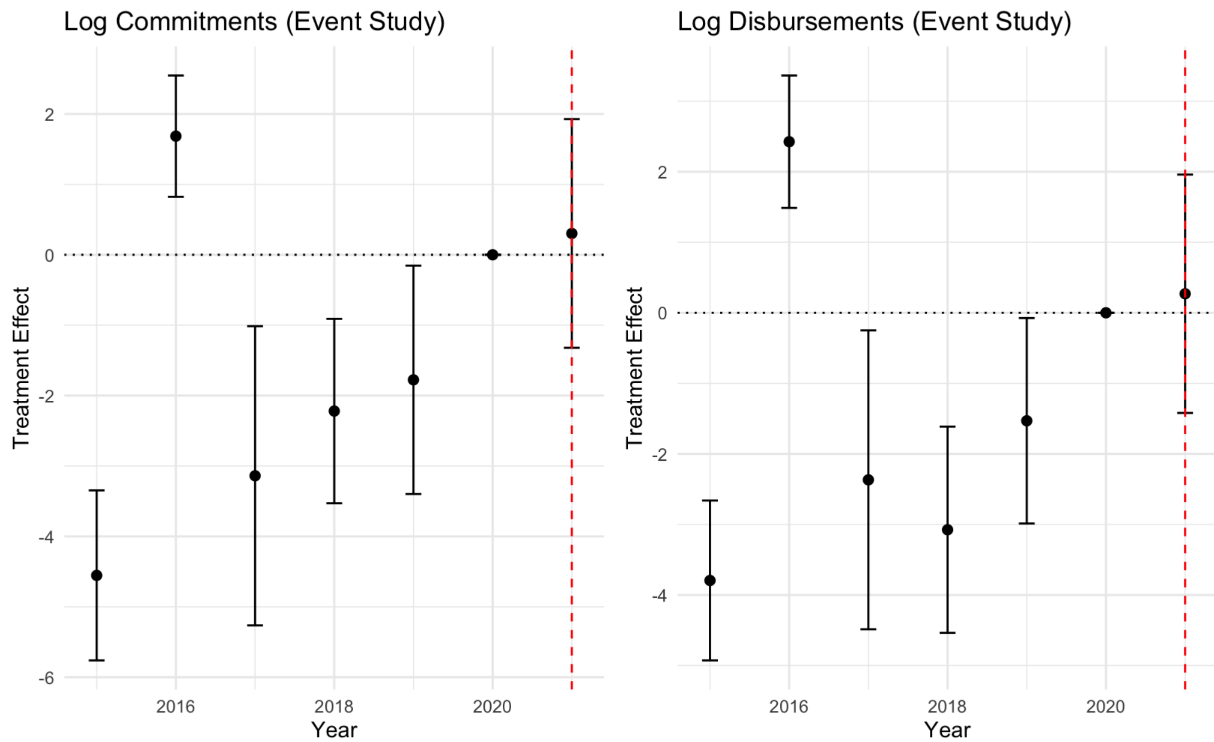


Table D2.5. Robustness: Difference-in-Differences Estimates: Yemen, Houthi (2015–2020)

	Log Commitments	Log Disbursements
Treatment (Treated × Post)	1.365+	1.155
	(0.781)	(0.795)
Population Density	-0.001	-0.003**
	(0.001)	(0.001)
Nighttime Lights	0.292*	0.369**
	(0.120)	(0.111)
Num. Obs.	115	119

*Notes: p < 0.1, * p < 0.05, ** p < 0.01, *** p < 0.001. The + symbol denotes marginal significance at the 10% level.*

Figure D2.5. Robustness: Event Study: Yemen, Houthi (2015–2021)



D3: Subnational Robustness - Randomized Inference

Table D3.1: Randomized Inference: Sri Lanka(LTTE 1997)

Outcome	Estimate	P-value (Year)	P-value (ADM1)
Log Commitments	1.893	1	0.111
Log Disbursements	-0.124	1	0.949

Notes: P-values based on 10,000 permutations. ADM1 and year fixed effects included; no subnational controls.

Table D3.2: Randomized Inference: Mali (Ansar Dine 2013)

Outcome	Estimate	P-value (Year)	P-value (ADM1)
Log Commitments	1.063	0	0.455
Log Disbursements	0.845	0	0.398

Notes: P-values based on 10,000 permutations. ADM1 and year fixed effects included; no subnational controls.

Table D3.3: **Randomized Inference: Nigeria (Boko Haram 2013)**

Outcome	Estimate	P-value (Year)	P-value (ADM1)
Log Commitments	0.882	0.000	0.134
Log Disbursements	1.376	0.254	0.024

Notes: P-values based on 10,000 permutations. ADM1 and year fixed effects included; no subnational controls.

Table D3.4: **Randomized Inference: Mali (ISIS 2020)**

Outcome	Estimate	P-value (Year)	P-value (ADM1)
Log Commitments	0.136	0.747	0.82
Log Disbursements	0.949	0.000	0.11

Notes: P-values based on 1,000 permutations. ADM1 and year fixed effects included; no subnational controls.

Table D3.5: **Randomized Inference: Yemen (Houthi 2021)**

Outcome	Estimate	P-value (Year)	P-value (ADM1)
Log Commitments	0.851	0.211	0.459
Log Disbursements	0.403	0.604	0.772

Notes: P-values based on 10,000 permutations. ADM1 and year fixed effects included; no subnational controls. Sectors restricted to those geocoded in 2021.