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Paying Them to Hate US: The Effect of U.S. Military Aid on Anti-American Terrorism, 1968-2018

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Abstract

Does U.S. military aid make the United States safer? Or does it have unintended and adverse consequences for U.S. security? We provide causal estimates of the effect of U.S. military aid on anti-American terrorism for a sample of 174 countries between 1968–2018 by exploiting plausibly exogenous time variation in global levels of U.S. military aid associated with distinct aid programs and cross-national time-series variation in the relative importance of the various military aid programs for recipient countries. We find that higher levels of military aid lead to an increased likelihood of the recipient country to produce anti-American terrorism. For our preferred instrumental-variable specification, doubling U.S. military aid increases the risk of anti-American terrorism by 2.7 percentage points. Examining potential transmission channels, we find that more U.S. military aid leads to more corruption and exclusionary policies in recipient countries. We argue that U.S. military aid results in anti-American terrorism by undermining institutions in recipient countries and creating anti-American resentment among those parts of the population that do not have direct or indirect access to benefits arising from aid.

Keywords: U.S. military aid, anti-American terrorism, instrumental variable estimation

JEL: D74, F35

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1. Introduction

Even though they are ostensibly initiated to counter insurgents that oppose U.S. interest, there is substantial evidence that U.S. interventionism, in particular U.S. *military* interventions, can produce unintended and adverse consequences. For instance, Dell and Querubin (2018) study the effect of the U.S. air campaign in Vietnam, finding that aerial bombing increased military and political activities of insurgents, while weakening local governance. Similarly, Kocher et al. (2011) find that U.S. aerial bombardment during the Vietnam War shifted local control in favor of insurgents opposing the U.S. In addition to strategies that emphasize overwhelming firepower, the provision of U.S. aid to prop up local governments that sympathize with the United States can likewise have inadvertent effects.[1] As an example, Nunn and Qian (2014) show that U.S. food aid, rather than having a stabilizing effect, increases the incidence and duration of civil conflict in recipient countries. Similarly, Sexton (2016) finds that the provision of U.S. counterinsurgency aid can lead to a significant increase in insurgent violence when allocated to contested Afghani districts.[2]

In this paper, we advance the literature on the unintended consequences of U.S. interventionism by examining the effect of *U.S. military aid* on the emergence of *anti-American terrorism* in aid-receiving countries.[3] That is, we ask the politically crucial question whether military aid actually makes the United States safer or—consistent with the notion of potential unintended consequences of aid—rather contributes to insecurity. We ask this question against the backdrop that between 1968 and 2018, the world saw over

[1]Such effects are, of course, not limited to U.S. policy. For example, Felter et al. (2014) investigate the effect of a large development program on conflict in the Philippines, showing that municipalities receiving development aid experience a large increase in conflict compared to municipalities that receive no aid.

[2]This is not to say that U.S. aid will always result in increased conflict. For instance, Berman et al. (2011) study violence against Coalition and Iraqi forces in the aftermath of the U.S. occupation of Iraq. They find that reconstruction spending provided by the United States to improve public service provision indeed reduces insurgent violence. Dell and Querubin (2018) also highlight that U.S. efforts to win local hearts and minds in Vietnam led to better outcomes compared to more aggressive military approaches.

[3]Throughout our paper, military aid refers to assistance that subsidizes or substantially enhances the military capability of the recipient country (e.g., through U.S. grants facilitating the training of military personnel of the recipient country), consistent with the definition given by the United States Agency for International Development (USAID, 2019). Moreover, terrorism is defined as "the premeditated use or threat to use violence against noncombatants by individuals or subnational groups to obtain a political objective through the intimidation of a large audience beyond that of the immediate victims" (Gaibulloev and Sandler, 2019, p. 278).

3,600 transnational terrorist attacks against American interests, most notably the 9/11 attacks on New York City and Washington, D.C. (Mickolus et al., 2019),[4] and that the United States responded to this threat from transnational terrorism by providing—among other measures—substantial *military aid* to foreign governments.[5]

In order to answer this question, we examine the impact of U.S. military aid on anti-American terrorism as well as the transmission channels for a sample of 174 countries between 1968 and 2018. Simple correlations will be insufficient in this challenging analysis because of relevant concerns of endogeneity, which is why we resort for identification to novel instrumental-variable strategies and various robustness checks to provide *causal* evidence on the nexus between military aid and anti-American terrorism.

Drawing on the surprisingly small existing literature, there are two competing predictions about the effect of military aid on (anti-American) terrorism. For one, military aid may lead to *less* anti-American terrorism by bolstering state capacity in recipient countries. We call this way of thinking the *policy-maker argument* as it corresponds to the American policy-makers' thoughts about the efficacy of military aid (e.g., Obama, 2013; The White House, 2013). On the other hand, there is the *strategic logic channel* which predicts that U.S. military aid leads to *more* anti-American terrorism. According to this argument, by resorting to anti-American terrorism, a terrorist group hopes to reduce U.S. military aid for a domestic government that the terrorist group opposes; a reduction in U.S. military aid will weaken this domestic government and thus make terrorist success in the future more likely (Neumayer and Plümper, 2011).

Our first contribution is to introduce and empirically investigate a third, so far unexplored connection between aid and anti-American terrorism. We argue that U.S. military aid has the—unintended—consequence of undermining institutions in recipient countries and creating anti-American resentment among those parts of the population that do not have direct or indirect (via governmental redistribution) access to political and economic rents arising from aid. For these population groups, U.S. military aid rather constrains the

[4]Terrorist incidents that affect more than one country are referred to as transnational terrorism. For instance, the 9/11 attacks were transnational because foreign terrorists attacked on U.S. soil, with non-Americans being also victimized.

[5]E.g., in 2013 then-U.S. president Obama noted that such aid "is fundamental to our [i.e., U.S.] national security [and] fundamental to any sensible long-term strategy to battle extremism" (Obama, 2013). Indeed, between 1968 and 2018, the United States gave approximately 600 billion US$ (inflation-adjusted) in foreign military assistance; in 2018 alone, the U.S. spent about 12 billion US$ on military aid (USAID, 2019).

means of economic and political participation, which encourages anti-American terrorism. We call this third pathway from more military aid to more anti-American terrorism the *grievances channel*.[6]

In addition to introducing this hitherto unappreciated pathway, the second and main contribution of this paper is to inform the literature on the unintended effects of U.S. interventionism by providing—for the first time—a thorough identification of the *causal* effect of U.S. military aid on anti-American terrorism. That is, we employ an instrumental-variable (IV) strategy to estimate the effect of military aid on anti-American terrorism in the presence of endogeneity concerns due to reverse causality or joint determination; for instance, such concerns may be rooted in the notion that military aid may not only determine terrorism, but aid may also *respond* to it (e.g., Balla and Reinhardt, 2008; Bapat, 2011; Boutton and Carter, 2014; Bezerra and Braithwaite, 2016; Lis, 2018).

For our IV approach, we rely on two sources of variation: First, we exploit plausibly exogenous time variation in *global levels of U.S. military aid* associated with three distinct military aid programs (on foreign military financing, military education and training, and other aid programs). Second, we exploit cross-national time-series variation in the *relative importance* of these various military aid programs for recipient countries, which we measure as the proportion of aid associated with specific military aid programs as a share of total U.S. military aid a recipient country obtains per year. Using both sources of variation at the same time, we construct an instrumental variable for the amount of military aid a country receives from the United States in a given year. This instrument is relevant as recipient countries will benefit from an increase in U.S. military aid associated with programs to other parts of the world (or lose due to corresponding decreases in aid). At the same time, our instrument is exogenous because aid recipient countries have no leverage to influence U.S. military aid patterns or the distribution of aid associated with specific military aid programs in other parts of the world. Rather, both the level and distribution of military aid is due to economic, political, and geo-strategic considerations *within the U.S.* For instance, they may be affected by budgetary considerations in the U.S., the relative political power of hawkish/dovish or isolationist/internationalist policy-makers, as well as the influence of

[6]Our argument taps into the broader literature on the role of grievances in political violence (e.g., Fearon and Laitin, 2003; Collier and Hoeffler, 2004; Blattman and Miguel, 2010; Djankov and Reynal-Querol, 2010; Gaibulloev and Sandler, 2019) which has so far disregarded anti-American terrorism as one prominent example of such violence.

the U.S. military-industrial complex that lobby for military aid to bolster foreign sales.

For a sample of 174 countries between 1968 and 2018, we use terrorism data from the *International Terrorism: Attributes of Terrorist Events* (*ITERATE*) dataset (Mickolus et al., 2019), showing that higher levels of military aid result in *more* anti-U.S. terrorism produced by recipient countries. In our preferred specification, at the sample mean, doubling U.S. military aid increases the risk of anti-American terrorism by 2.7 percentage points, which, in turn, is approximately 17.5% of the mean incidence of anti-U.S. terrorism. Also, our results indicate that non-IV approaches to the aid-terrorism relationship are downward biased, underestimating the role of U.S. military aid in anti-U.S. terrorism. Our main finding holds when subjected to various robustness checks, including those that probe the validity of our identification strategy. For instance, as part of our robustness checks we control for economic, political or demographic shocks that are correlated across regions (thus potentially affecting the construction of our IV), which could simultaneously influence the provision of military aid to and the production of anti-American terrorism in the country of interest. Furthermore, we assess whether U.S. military aid affects transnational terrorism against U.S. allies (e.g., the United Kingdom) or geo-political opponents (e.g., Iran), which we conclude is not the case. Here, finding that U.S. military aid does not incentivize transnational terrorism against U.S. foes also serves as an important placebo test, providing further evidence that our identification strategy is indeed valid.

Finally, we study whether U.S. military aid leads to increased state capacity (consistent with the strategic logic channel) and/or increased grievances (in line with our grievances channel). We find no evidence that U.S. military aid contributes to increased local state capacity. However, more U.S. military aid leads to more corruption and exclusionary policies in recipient countries. This supports our grievances argument: The inflow of military aid induces politically and economically detrimental behavior of local groups that have access to this inflow (such as rent-seeking behavior), which, in turn, encourages terrorism by other groups that have little access to it and suffer from reduced economic and political participation. These aggrieved or disenfranchised groups direct their dissatisfaction against the U.S. as the perceived linchpin of an unfavorable status quo in the recipient country.

In addition to adding to the empirical literature on the unintended consequences of U.S. military policy and interventions, our study also contributes to several other strands of the literature. First, we add to existing studies that correlate measures of U.S. foreign and military policy (e.g., U.S. arms exports, U.S. support for Israel) to the genesis of anti-American terrorism (Neumayer and Plümper, 2011; Gries et al., 2015; Krieger and

Meierrieks, 2015; Saiya et al., 2017). While these studies provide no causal estimates, they nevertheless indicate that stronger U.S. involvement tends to be associated with increased anti-American terrorist activity . Our paper is able to validate and strengthen these findings through an instrumental-variable strategy accounting for previously unappreciated endogeneity concerns. Second, we add to the broader literature on the potentially perverse effects of aid on conflict [7] and the (potentially undesired) institutional and socio-political consequences of aid[8]. Third, to the extent that military aid is used as a counter-terrorism tool, we extend the research on the evaluation of counter-terrorism spending and policies[9]

We proceed as follows. In Section 2, we provide a more detailed theoretical explanation as to how U.S. military aid may be related to anti-American terrorism. We introduce our data and empirical model in Section 3. Our baseline and IV-estimates are reported in Section 4, followed by various robustness checks and extensions in Section 5. In Section 6, we examine a number of transmission channels potentially accounting for the relationship between U.S. military aid and anti-American terrorism. Section 7 concludes.

2. U.S. Military Aid and Anti-American Terrorism

2.1. From U.S. Military Aid to Less Anti-American Terrorism

The rational-economic model of terrorism (Landes, 1978; Sandler et al., 1983; Enders and Sandler, 1993) assumes that terrorists rationally weigh the expected (opportunity) costs and benefits of violent and non-violent behavior when they consider employing terrorism as a means to achieve their goals (Schneider et al., 2015; Gaibulloev and Sandler, 2019). Against this backdrop, the *policy-maker argument* stresses that terrorism is more likely to emerge in countries characterized by low levels of state capacity. Low state capacity reduces the material costs of terrorism, making violence a more attractive option. For instance, it is less costly for terrorist organizations to operate when there is little retaliation by the government (Lai, 2007). Indeed, there is evidence that weaker states tend to see more terrorism (e.g., Lai, 2007; Hendrix and Young, 2014; George, 2018).

According to the policy-maker argument, by supplementing local state capacity, U.S.

[7] E.g., Grossman (1992); Collier and Hoeffler (2002); De Ree and Nillesen (2009); Felter et al. (2014); Nunn and Qian (2014); Boutton (2021).
[8] E.g., Mosley et al. (2004); Bourguignon and Sundberg (2007); Roodman (2007); Djankov et al. (2008); Fleck and Kilby (2010); Findley et al. (2011); Strange et al. (2017).
[9] E.g., Enders and Sandler (1993); Zussman and Zussman (2006); Mueller and Stewart (2014); Schneider et al. (2015); Cesur and Sabia (2016); Gaibulloev and Sandler (2019).

military aid is expected to raise the material costs of operating a terrorist group, consequently lowering terrorist activity also against the United States.

Governments of weak states may also have incentives to engage in negotiations with terrorist groups (e.g., Bapat, 2011). Assuming a rational calculus on the part of these governments, it may be less costly for them to arrive at an agreement with a terrorist group than to engage in a conflict that they cannot win militarily. For instance, a weak state's government may allow a terrorist group to operate from the state's territory in exchange for non-aggression against the local government. As a consequence of such tacit agreement, the terrorist group may use the state's territory to launch terrorist attacks against foreign actors (e.g., the United States). This creates an additional incentive to provide foreign military assistance: The provision of military aid may lead a weak governments to forego negotiations when the benefits from aid are larger than the benefits from negotiations with the terrorists (Bapat, 2011).

In sum, the policy-maker argument suggests that U.S. military aid bolsters local state capacity, which in turn raises the operating costs of terrorism. In addition, it reduces local governments' incentives to negotiate with terrorists. If such mechanisms are relevant, we expect to find empirical support for the following hypothesis:

Hypothesis 1 (H1): *More U.S. military aid leads to less anti-American terrorism originating from the country receiving the aid.*

2.2. From U.S. Military Aid to More Anti-American Terrorism

In contrast to the policy-maker argument, existing empirical evidence suggests that measures of U.S. military policy positively correlate with anti-American terrorism (Neumayer and Plümper, 2011; Gries et al., 2015; Krieger and Meierrieks, 2015; Saiya et al., 2017). One way to explain this positive link is the *strategic logic argument*, which we discuss below. Additionally, in this paper we posit that U.S. aid may also promote anti-American terrorism by fostering grievances (*grievances channel*), an argument that has hitherto received no attention for the nexus between U.S. military aid and anti-American terrorism.

2.2.1. The Strategic Logic Channel

The strategic logic channel considers a three-way interaction between a local government, a local terrorist opposition that wants to extract concessions from this government and a foreign actor (the U.S.) that provides military aid to the local government. The provision of military aid shifts the local balance of power in favor of the local government or military

(e.g., Savage and Caverley, 2017; Biddle et al., 2018), thus representing a brake on the terrorists' goal of achieving concessions from the local government (Neumayer and Plümper, 2011). Consequently, the local terrorist group may resort to anti-American terrorism, as it hopes that terrorism against the United States can force the U.S. to withdraw its support from the local government (e.g., due to political pressure from the American public), which, in turn, ought to shift the local balance of power in favor of the terrorist group. In other words, by successfully bolstering local state capacity, the United States perversely incentivizes anti-American terrorism designed to negate this success.[10]

2.2.2. The Grievances Channel

According to our *grievances channel*, military aid affects the terrorists' calculus by lowering the *opportunity costs of terrorism*. Indeed, a number of theoretical contributions suggest that poor country-specific conditions that constrain socio-economic and political participation create *grievances* (e.g., exclusion and discrimination) that make violence—as an alternative way to achieve such participation—comparatively more attractive (e.g., Blomberg et al., 2004; Kurrild-Klitgaard et al., 2006).

Bueno de Mesquita and Smith (2009) argue that the provision of aid yields benefits to the donor country, e.g., in the form of military and market access or a voting behavior favorable to the donor at international organizations.[11] While aid incurs costs for the recipient country as a consequence of making concessions to the donor (e.g., in form of a loss of political autonomy), the provision of monetary benefits may still make it worthwhile. In particular, aid may enable *rent-seeking behavior* so that politicians in power and other interest groups can appropriate aid and exclude other social groups from it (for economic aid, see, e.g., Svensson, 2000; Hodler, 2007; Keefer and Knack, 2007; Djankov et al., 2008). Rents due to military aid may incentivize corrupt behavior by politicians and bureaucrats to secure access to them (Alesina and Weder, 2002, see also Fisman and Golden 2017; Dimant and Tosato 2018). Furthermore, the same access to such rents may disincentivize investment into public goods that would curtail rent-seeking (e.g., Svensson, 2000).

[10]Terrorism may produce additional incidental benefits, e.g., in form of increased political leverage, media attention and peer acknowledgement among potential terrorist sympathizers (e.g., Melnick and Eldor, 2010; Neumayer and Plümper, 2011; Montalvo, 2011; Jetter, 2017). These benefits are expected to be disproportionately large when the United States are targeted.

[11]Indeed, evidence suggests that the allocation of U.S. foreign assistance is driven by U.S. strategic, economic (e.g., trade benefits) and political (e.g., crucial votes at the United Nations) interests (e.g., Poe and Meernik, 1995; Kuziemko and Werker, 2006; Dreher et al., 2008; Boutton and Carter, 2014).

It is worth noting that the recipient country usually has some leeway with respect to the *distribution* of aid resources. This makes it possible for the recipient country's politicians and bureaucrats to act as as gatekeepers and create 'winners' and 'losers,' with the former disproportionately benefiting by sharing the rents from aid. For the government, this is beneficial because the 'winners' of the distribution process ought to help the government stay in power (e.g., Bueno de Mesquita and Smith, 2009).[12] The recipient country may also use U.S. military aid to supplement its own military spending, using associated savings to finance other projects that disproportionately benefit its supporters.[13]

Consequently, the provision of U.S. military aid creates grievances among the 'losers' of the aid provision process, i.e., among those who (1) suffer from poorer institutions due to U.S. military aid and (2) have no direct or indirect (via governmental redistribution) access to the rents arising from aid. Applying the rational-economic model of terrorism, for these 'losers' the opportunity costs of terrorism are expected to decrease as non-violent economic and political participation becomes more constrained (e.g., due to increased corruption). This, in turn, ought to make alternatives to non-violence more attractive to achieve socio-political change. We expect aggrieved 'losers' to turn against the United States, blaming the United States for the political survival of the local government and unfavorable local conditions. As an example, Tokdemir (2017) shows that political 'losers' in the recipient countries of U.S. aid are more likely to express negative attitudes toward the U.S. as the amount of aid increases, while the 'winners' view the U.S. more positively.

In sum, U.S. military aid may (1) tip the balance of power between local governments and terrorist groups in favor of the former, consequently creating additional benefits from anti-American terrorism (strategic logic channel) and/or (2) affect the opportunity costs of anti-American terrorism in ways that make it more attractive (grievances channel). If these channels are relevant, we expect support for the hypothesis:

***Hypothesis 2 (H2)**: More U.S. military aid leads to more anti-American terrorism originating from the country receiving the aid.*

[12]This logic may also work in the opposite direction when severe negative aid shocks limit the ability of local governments to transfer resources to these groups (Nielsen et al., 2011).

[13]This refers to the issue of *aid fungibility*, i.e., the ability of the recipient country to spend categorical or targeted aid on non-targeted programs (e.g., Pack and Pack, 1993; Feyzioglu et al., 1998). Deger and Sen (1991) and Khilji and Zampelli (1994) find that military aid is as fungible as economic aid, so that military assistance by the United States may indeed also benefit the selectorate outside of the military sphere. Furthermore, recent evidence by Andersen et al. (2020) indicates that elite capture of foreign aid does indeed take place to a substantial amount.

3. Data and Empirical Model

To empirically investigate how U.S. military aid affects anti-American terrorism, we use data for 174 countries between 1968 and 2018. The summary statistics are reported in Table 1. A country list is provided in the appendix (Table A.1).

Table 1: **Summary Statistics**

Variable	N*T	Mean	Stand. Dev.	Min.	Max.
Anti-American Terrorism (Attacks, Location)	7,381	0.489	2.262	0	88
Anti-American Terrorism (Binary, Location)	7,381	0.156	0.363	0	1
Anti-American Terrorism (Attacks Per Capita, Location)	7,381	0.054	0.319	0	10.46
Anti-American Terrorism (Attacks, Origin)	7,381	0.264	1.287	0	23
Anti-American Terrorism (Binary, Origin)	7,381	0.099	0.300	0	1
Anti-American Terrorism (Attacks Per Capita, Origin)	7,381	0.024	0.160	0	4.58
Anti-American Terrorism (Attacks, Location, GTD)	7,173	0.357	1.880	0	49
Anti-American Terrorism (Binary, Location, GTD)	7,173	0.116	0.320	0	1
Anti-American Terrorism (Attacks Per Capita, Location, GTD)	7,173	0.033	0.224	0	7.4
Military Aid (in 100,000,000 USD)	7,381	0.706	4.829	0	115.6
Civil Conflict	7,381	0.192	0.394	0	1
Child Mortality	7,381	73.62	73.00	2.2	387.3
Regime Type	7,381	1.340	1.079	0	3
Population Size (logged)	7,381	15.96	1.554	11.69	21.05
Transnational Terrorism against France (Binary)	7,330	0.036	0.187	0	1
Transnational Terrorism against the UK (Binary)	7,330	0.046	0.210	0	1
Transnational Terrorism against Israel (Binary)	7,330	0.028	0.165	0	1
Transnational Terrorism against Turkey (Binary)	7,330	0.022	0.146	0	1
Transnational Terrorism against Iran (Binary)	7,333	0.017	0.129	0	1
Transnational Terrorism against USSR/Russia (Binary)	7,355	0.024	0.154	0	1
Transnational Terrorism against Syria (Binary)	7,330	0.009	0.095	0	1
Military Capacity	5,909	-0.044	0.904	-0.516	8.79
Territorial Control	7,379	91.09	10.36	31.11	100
Quality of Bureaucracy	7,381	0.252	1.538	-3.752	4
Equal Distribution of Resources	7,381	0.566	0.287	0.016	0.98
Corruption	7,347	0.505	0.297	0.002	0.96
Clientelism	7,381	0.497	0.265	0.018	0.98

Note: Summary statistics for baseline sample.

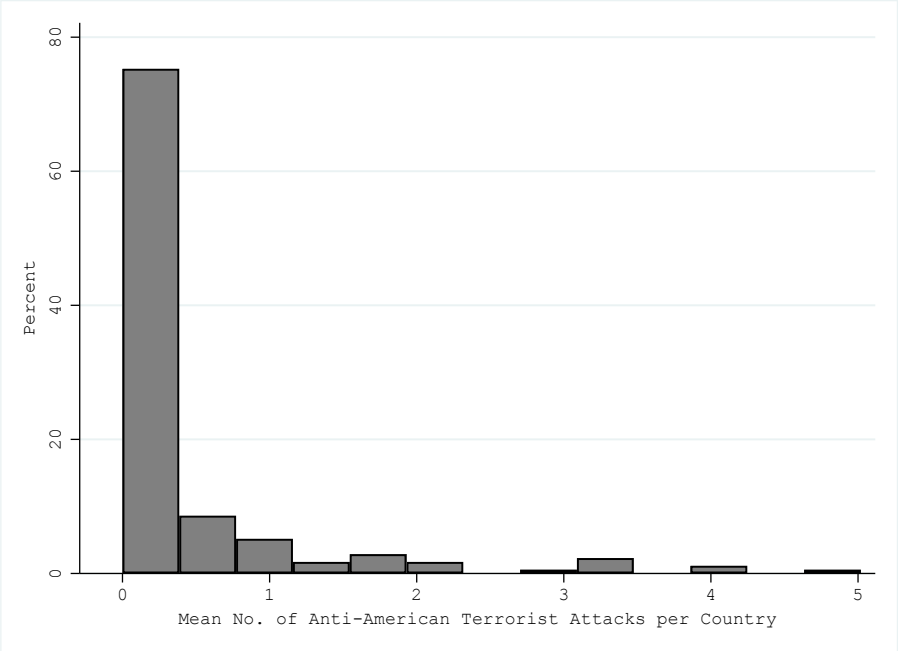
3.1. Measuring Anti-American Terrorism

Data on terrorism come from the *International Terrorism: Attributes of Terrorist Events (ITERATE)* dataset of Mickolus et al. (2019), which is the most extensive and consistent dataset capturing transnational terrorist activity (Enders et al., 2011). It defines transnational terrorism as "the use (or threat of use) of anxiety-inducing, extra-normal violence for political purposes by any individual or group (acting for or in opposition to established governmental authority) when such action is intended to influence the behavior of a target group wider than the immediate victims and when, through the nationality or foreign ties

of its perpetrators, its location, the nature of its victims or the mechanics of its resolution, and its ramifications transcend national boundaries" (Mickolus et al., 2019, p.1).

Our dependent variable, *anti-American terrorism*, is a binary variable that is equal to unity when a country-year pair sees at least one transnational terrorist attack against U.S. interests; it is equal to zero if there is no anti-American terrorist activity. Anti-American terrorist attacks may be directed at diplomatic (e.g., embassies), military, commercial or non-official (e.g., American tourists) targets.[14] As shown in Figure 1, anti-American terrorism is a rather rare event: Most countries see between zero and one anti-American attack per year. Therefore, little information is lost when we use a dichotomous terrorism indicator. Rather, by using a binary dependent variable, we provide a conservative estimate of the effect of military aid on anti-American terrorism. However, as a robustness check, below we will also use alternative measures of anti-American terrorism such as the count of anti-American terrorist attacks per country-year.

Figure 1: Histogram of Anti-American Terrorism



[14] *ITERATE* excludes terrorist attacks against combatants (e.g., U.S. troops that act as an occupying force in Iraq).

We use the *location definition* of transnational terrorism, where an attack is assigned to the terrorism venue country, i.e., the country in which the anti-U.S. attack occurs. Potentially, the perpetrators of an anti-U.S. terrorist attacks originate from a third country, leading us to wrongly assign some cases.[15] However, in many cases the nationality of the perpetrator is not known; in fact, Abrahms and Conrad (2017) report that only one in seven terrorist attacks is actually claimed. In addition, there may be multiple claims related to a single attack, again making it difficult to correctly assign an attack (Abrahms and Conrad, 2017). Using the location definition, we therefore avoid under-counting anti-U.S. terrorism. As a robustness check, below we also use an alternative definition of anti-U.S. terrorism where attacks are assigned to the terrorists' country of origin (if known) regardless of the venue country (*origin definition* of transnational terrorism).

Figure 2 illustrates the patterns of anti-U.S. terrorism for the countries in our sample. There are noticeable spikes in the early 1970s, early 1990s and the mid-2000s as well as relative lulls in the mid-1970s, mid-1990s and after 2005. Between 10% (after 2005) and over 30% (early 1970s) of all countries produced at least one anti-U.S. terrorist attack per year, pointing to a large geographical and temporal variation in anti-American terrorism.

3.2. U.S. Military Aid

USAID (2019) defines military assistance as aid that primarily benefits a recipient government's armed forces or that subsidizes or substantially enhances local military capability. In detail, the U.S. provides military aid associated with foreign military financing (*milfin*), foreign military training (*miltrain*) and further aid programs (*milother*), the latter including aid for counter-narcotics initiatives, peacekeeping and the cooperative threat reduction initiative, among others (USAID, 2019). Most military aid is spent on foreign military financing (approximately 55% of all military aid). Our main explanatory variable, *U.S. military aid*, is the sum of all individual aid programs provided to country i at year t:

$$aid_{it} = milfin_{it} + miltrain_{it} + milother_{it} \tag{1}$$

Military aid is measured in 100 million constant (inflation-adjusted) US$. The data are drawn from the *United States Agency for International Development* (USAID, 2019). As a robustness check, we also use alternative operationalizations of U.S. military aid below.

[15]For instance, in 1988 members of the *Japanese Red Army* attacked a U.S. military recreational club in Naples, Italy. Using the location definition of transnational terrorism, this attack is assigned to Italy.

Figure 2: Anti-American Terrorism, 1968-2018

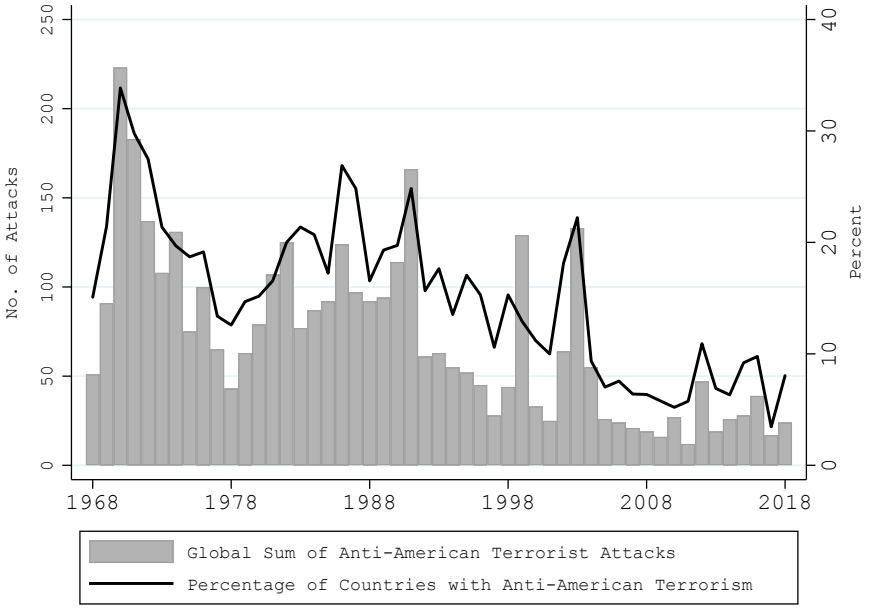


Figure 3 illustrates the global patterns of U.S. military aid over our observation period. There are some noticeable spikes in military aid during some years in the 1970s and 1980s and after 2005, with a relative lull after the end of the Cold War. There is a clear trend towards more countries receiving at least some U.S. military aid. For instance, in the 1970s 35% to 40% of all countries received military aid, while after the year 2000 between 70% and 80% of all countries received at least some aid in a given year.

3.3. Empirical Model

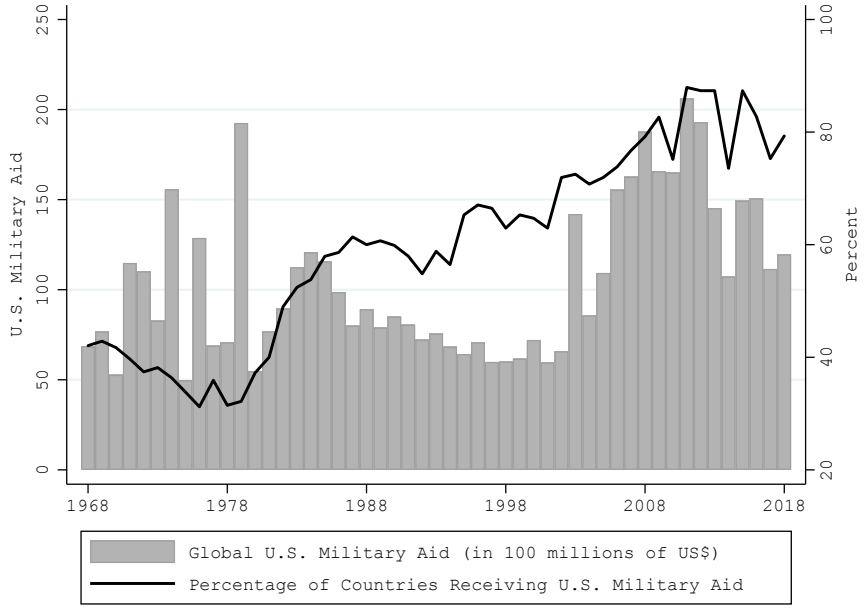
3.3.1. Baseline Linear Probability Model

To examine the effect of U.S. military aid on anti-American terrorism, we estimate the following model:

$$terror_{it} = \beta \times aid_{it} + \gamma \times X_{it} + \alpha_i + \tau_t + \epsilon_{it} \tag{2}$$

In equation (2), *terror* is an indicator variable that is equal to unity if country i experiences at least one incident of anti-U.S. terrorism in year t. Our independent variable of interest is U.S. military aid (aid). In addition to a well-behaved error term (ϵ), we also include country-fixed effects (α) to account for unobserved (time-invariant) heterogeneity as well as year-fixed effects (τ) to account for common shocks. Most importantly, the inclusion of

Figure 3: U.S. Military Aid, 1968-2018



time controls allows us to consider the influence of the Cold War and the War on Terror, which have been relevant to both the patterns of anti-American terrorism and U.S. military aid (e.g., Fleck and Kilby, 2010; Gaibulloev and Sandler, 2019).

Finally, our model includes a vector of additional controls (X) accounting for (1) a measure of *civil conflict* that is equal to unity when there is an incidence of large-scale domestic conflict (i.e., civil and ethnic warfare) drawn from the *Center for Systemic Peace*[16]; (2) *infant mortality* as a measure of economic development[17] and (3) (logged) *population size* to control for scale effects, with both variables drawn from the *World Development Indicators* (World Bank, 2019); and (4) a variable indicating *regime type* from Coppedge et al. (2021) which ranges from zero (closed autocracy) to three (liberal democracy).

We estimate equation (2) as a *linear probability model (LPM)* using the OLS estimator. For our analysis, the LPM has a number of advantages over alternatives that may also be employed when the dependent variable is a binary (such as the Probit model), including the

[16]See the Political Instability Task Force Dataset: http://www.systemicpeace.org/inscrdata.html.
[17]We use this measure of economic development as it maximizes the number of observations. Below, we also use per capita income as an alternative measure of economic development. Employing per capita income, however, substantially reduces sample size and means that we have to forego interesting and potentially influential country cases such as Afghanistan.

possibility to account for various fixed effects without encountering an incidental parameter problem and the availability of standard errors that are robust to cross-sectional dependence (for a more general discussion of advantages of the LPM over non-linear models, see Angrist and Pischke, 2008; Wooldridge, 2010).

Indeed, we expect all variables of interest to be affected by cross-sectional dependence. Cross-sectional dependence refers to the interdependency of variables between countries. For instance, this interdependency may be due to spillover effects. If not accounted for, cross-sectional dependence in panel data may lead to correlation in the residuals that affects the validity of statistical inference (Sarafidis and Wansbeek, 2012). Pre-testing the variables in our data set for cross-sectional dependence using the test of Pesaran (2021), we indeed find that cross-sectional dependence is present for almost all variables employed in our analysis. Pre-tests similarly indicate the presence of autocorrelation and heteroskedasticity, as it is expected when panel data is employed. We therefore employ standard errors developed by Driscoll and Kraay (1998) which are not only robust to heteroskedasticity and autocorrelation, but also to general forms of cross-sectional dependence.

3.3.2. Two-Stage Instrumental-Variable Model

A major concern when estimating equation (2) is *endogeneity*. Apart from omitted variables or measurement error, endogeneity may be due to reverse causality or joint determination. That is, U.S. military aid may also respond to anti-American terrorism (e.g., Bapat, 2011; Boutton and Carter, 2014). For instance, anti-American terrorism in recipient countries may trigger the provision of additional U.S. military aid to foster local state capacity to curtail the future production of anti-American terrorism (e.g., Balla and Reinhardt, 2008; Bezerra and Braithwaite, 2016; Lis, 2018). In this case, estimating equation (2) would yield an upwards biased estimate of the effect of military aid on anti-American terrorism. Conversely, the results of equation (2) would be downward biased if additional anti-American terrorism from a recipient country leads to a reduction of U.S. military aid. For instance, the United States may reduce aid to pressure the aid-receiving country into intensifying its fight against terrorism.

Instrument Construction. To address these concerns, we estimate the following *two-stage instrumental-variable system*:

$$aid_{it} = \delta \times globalaid_{it} + \mu \times X_{it} + \alpha_i + \tau_t + \epsilon_{it} \tag{3a}$$

$$terror_{it} = \beta \times \widehat{aid}_{it} + \gamma \times X_{it} + \alpha_i + \tau_t + \epsilon_{it} \tag{3b}$$

Our instrumental variable (IV), *globalaid*, is constructed via the following steps:

1. For each country i, we calculate the share of the total amount of military aid a country receives annually. This aid is associated with one of the three main military aid programs for either (1) foreign military financing, (2) military training, or (3) other programs. This yields three shares (*smilfin, smiltrain, and smilother*) that add up to unity.
2. For each year t, we calculate the global amount of U.S. aid spent on each military aid program (subscript g).
3. From this global total, we subtract the military aid spent on each program that goes to each country i and those around it (subscript n).[18]
4. For each aid program, we multiply each share from Step 1 with the corresponding global total that is corrected for neighborhood effects from Step 3.
5. We aggregate all products from Step 4 into one variable that is our IV *globalaid*. This IV varies over time and is distinct for each country in the sample.

Formally, the IV *globalaid* is defined as follows:

$$globalaid_{it} = [smilfin_{it} \times (milfin_{gt} - milfin_{nt})] + [smiltrain_{it} \times (miltrain_{gt} - miltrain_{nt})] + [smilother_{it} \times (milother_{gt} - milother_{nt})] \quad (4)$$

Instrument Relevance. Intuitively, as the global U.S. military aid supply (approximated by *globalaid*) increases, the local military aid supply is also expected to increase. That is, ceteris paribus, countries can expect to see a larger inflow of resources (local military aid) when the total amount of resources available (global military aid) increases. Therefore, we expect a positive effect of our instrumental variable on local military aid in the first-stage regressions of our two-stage equation systems.

Instrument Exogeneity. We argue that U.S. military aid to non-neighboring countries (weighted by the relative importance of the various aid programs) is a plausibly exogenous instrument for local U.S. military aid. Our IV-approach is inspired by earlier work that

[18]Neighboring countries are located in the same region as the country of interest. The regions we consider are the Caribbean; Central Asia; Eastern Africa; Eastern Asia; Eastern Europe; Middle Africa; North and Central America; Northern Africa; Northern Europe; Oceania; South America; South-Eastern Asia; Southern Africa; Southern Asia; Southern Europe; Western Africa; Western Asia; and Western Europe.

similarly instrument local flows with global flows to other parts of the world. For instance, Dube and Naidu (2015) use aid flows to countries outside of Latin America to estimate the effect of U.S. military aid on conflict within Colombia, while Auer and Meierrieks (2021) use global arms trade flows to estimate the effect of local arms imports on local terrorist activity. We argue that the global level and distribution of the various forms of U.S. military aid are determined by economic, political, and geo-strategic considerations *within the United States*. For example, global levels of U.S. military aid may be affected by budgetary considerations in the United States, the relative political power of hawkish/dovish or isolationist/internationalist policy-makers and the political influence of the American military-industrial complex which may lobby for military aid to bolster foreign sales (e.g., Albouy, 2013; Boutton, 2021).

At the same time, we expect aid recipient countries to have no leverage to influence *global* U.S. military aid patterns. That is, aid recipient should not be able to systematically influence the aforementioned domestic conditions in the U.S. to the extent that they can sway the global level and distribution of U.S. military aid; potential aid recipients will not be able to systematically affect U.S. budgetary considerations, the composition of the U.S. Congress, U.S. industrial policy, or the geo-strategic outlook of the U.S.

Several concerns can be raised regarding our IV strategy. Below, we discuss these concerns and describe how we address them. First, it is possible that there are other changes over time that are spuriously correlated with both the instrument and anti-American terrorism. We account for this possibility by always including year fixed-effects. Second, U.S. military aid and anti-American terrorism may cluster in specific country neighborhoods (e.g., Israel-Egypt or Pakistan-Afghanistan). This would not only spuriously inflate the global level of U.S. military aid (which is part of our IV) but could also mean that our IV may not only affect anti-American terrorism through military aid but also other factors (e.g., cross-border ethnic grievances or inter-state rivalries). To rule out the influence of such neighborhood effects, when constructing our instrument we always exclude military aid to countries located in geographical proximity to the country of interest. Third, there may be economic, political or demographic shocks that are correlated across regions (and thus affect the construction of our IV) and might simultaneously influence the provision of military aid to and the production of anti-American terrorism in the country of interest. We therefore run additional specifications where we include time-variant covariates accounting for such shocks and thus potentially affecting the exclusion restriction. Finally, we consider it highly unlikely—given the intricacies of the U.S. political and bureaucratic

system—that recipient countries accumulate enough lobby power to not only affect aid decisions concerning their respective home country (which are *not* considered in our IV) but also the flow of U.S. aid to other parts of the world. Still, as part of our extensive sensitivity analysis, we identify countries that could exercise such lobby power (e.g., because they receive a substantial amount of aid or house U.S. troops) and assess whether our results are sensitive to their inclusion in our dataset.

4. Main Results

We report our main OLS and IV-OLS results in Table 2. Concerning the OLS estimates, we find that higher levels of military aid are associated with a greater likelihood of anti-American terrorism in both the parsimonious and baseline setting that includes a set of covariates. Here, the inclusion of the controls does not meaningfully influence the size or direction of the effect of aid on anti-American terrorism. The results for the controls speak to the empirical mainstream on the determinants of terrorism (e.g., Gaibulloev and Sandler 2019) in that anti-American terrorism is more likely in politically unstable, rich, and populous countries. However, regime type is not found to matter.

In sum, finding that more U.S. military aid is associated with more anti-American terrorism is in line with *H2*, while at the same time providing evidence against *H1*. The estimated effect sizes, however, are rather modest. Using the baseline specification (specification (2a) of Table 2), we find that an increase of U.S. military aid by 100 million US$ increases the likelihood of the country producing anti-American terrorism by 1%. For a country that receives the sample-mean quantity of U.S. military aid (approx. 71 million US$) and experiences the mean incidence of anti-American terrorism (15.6%), this implies that doubling U.S. military aid increases the likelihood of anti-American terrorism by 0.7%; this increase equals approximately 4.5% of the mean incidence of anti-American terrorism.

Table 2 also reports the IV-OLS results. Here, we again find that military aid exerts a positive effect on anti-American terrorism, implying evidence in favor of *H2*. Importantly, this effect is about four times *larger* compared to the non-IV estimates. This suggests that the OLS estimates are downward biased and underestimate the role of U.S. military aid in anti-American terrorism. Potentially, this may be due to classical measurement error, which leads to attenuation bias. Alternatively, the bias may result from simultaneity, where the United States reduces military aid to countries that generate anti-American terrorism. For instance, this may be due to bargaining between the United States and a potential aid recipient, with the U.S. withholding aid (which is desired by rent-seekers in the recipient

Table 2: **Baseline OLS- and IV-Estimates**

	(1a)	(1b)	(2a)	(2b)
Military Aid	0.010***	0.039**	0.010***	0.038***
	(0.002)	(0.016)	(0.002)	(0.014)
Civil Conflict			0.128***	0.123***
			(0.016)	(0.016)
Child Mortality			-0.001***	-0.001***
			(0.000)	(0.000)
Regime Type			0.006	-0.000
			(0.009)	(0.011)
Population Size			0.118***	0.085***
			(0.022)	(0.030)
First-Stage Regression Results				
Global Military Aid (IV)		0.010		0.012
		(0.002)***		(0.003)***
Civil Conflict				0.188
				(0.235)
Child Mortality				-0.008
				(0.003)***
Regime Type				0.207
				(0.083)**
Population Size				1.188
				(0.386)***
N*T	8,001	8,001	7,381	7,381
First-Stage F-Statistic		11.90		13.61
AR-Test (p-value)		(0.02)**		(0.00)***
AR 95% Confidence Interval		[0.003, 0.090]		[0.008, 0.084]

Notes: OLS estimates reported in Models (1a) and (2a) and IV-OLS estimates reported in Models (1b) and (2b). All models include country- and year-fixed effects. AR=Anderson-Rubin. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

country) in order to extract additional policy concessions (i.e., a tougher stance on anti-American terrorism) from the recipient country. As an alternative explanation, the U.S. may reduce aid due to political pressure within the U.S., e.g., as it may be politically costly to provide aid to countries that host anti-American terrorist groups.

Considering economic significance, using the baseline specification (specification (2b) of Table 2), we find that a one-unit increase in U.S. military aid (100 million US$) increases the likelihood of the aid-receiving country to generate at least one incident of anti-American terrorism by 3.8%. For a country at the sample-mean of U.S. military aid and anti-American terrorism, this estimate implies that doubling military aid (to approximately 142 million US$) results in a 2.7 percentage point increase in the incidence of anti-American

terrorism, which is approximately 17.5% of the mean incidence of anti-American terrorism.

In order for the IV-estimates to be trustworthy and interpretable in a causal way, they ought to pass a number of diagnostic checks. Indeed, the first-stage regression results and IV-diagnostics are consistently favorable to this. First, we find that our IV is a strong predictor of local U.S. military aid. As expected, we find that higher levels of global U.S. military aid (corrected for neighborhood effects and the relative importance of certain aid flows) result in higher levels of local military aid. Furthermore, the effective F-statistics surpass the usual threshold of $F = 10$ that would signal instrument weakness. However, this threshold has received some criticism for being potentially anti-conservative, meaning that instruments may be weak even if $F > 10$ (e.g., Lee et al. 2020). Thus, we also rely on diagnostics from weak-instrument robust inference (for an introduction to fully robust inference with weak instruments, see Stock et al., 2002). Reassuringly, these diagnostics are also satisfactory. First, the rejection of the null hypothesis of the Anderson-Rubin test, which is robust to arbitrarily weak instruments, tells us that the coefficient of the endogenous regressor in the structural equation is not equal to zero. Second, the constructed Anderson-Rubin confidence intervals are similar to the standard IV-estimates, firmly indicating statistical significance at conventional levels.

5. Robustness and Extensions

In the following, we assess the robustness of our main findings reported in the previous section to (1) modifications to our IV-approach, (2) the inclusion of additional covariates, (3) the use of alternative operationalizations of anti-American terrorism and U.S. military aid as well as (4) sample restrictions and alternative lag structures. As an important extension to our main empirical analysis, we (5) also study the role of U.S. military aid in terrorism directed against other nations.

5.1. Robustness of Instrumental-Variable Approach

We first explore the robustness of our IV-approach, given that any claims about causality concerning the effect of military aid on anti-American terrorism rest on the validity of our exclusion restriction that our instrumental variable (U.S. military aid to non-neighboring countries weighted by the relative importance of various military aid programs) has—conditional on the controls and fixed effects—no direct effect on local anti-U.S. terrorism.

As discussed above, the main threat to the validity of our IV-approach comes from the potential impact of economic or political shocks that are correlated across regions (thus

affecting the construction of our IV) and may influence the provision of military aid to and the production of anti-American terrorism in the country of interest. For instance, the flaring up of conflict and anti-Americanism in other parts of the world may influence the level and composition of U.S. aid-giving to those parts of the world (and thus impact our IV), while at the same time also motivating anti-American terrorism in the country of interest, e.g., by means of social media exposure or migrant diasporas. To account for such mechanisms, we include several time-varying covariates. These variables specifically consider the prevalence of conflicts as well as economic and political shocks in the rest of the world, i.e., in those countries that matter to the construction of our IV for a specific country of interest. Formally, these variables are defined as the mean levels of civil conflict incidence, child mortality and regime type as well as the sum of anti-American terrorism in countries located outside of the neighborhood of a country of interest. Furthermore, we run a specification that includes regional trends (i.e., year-fixed effects interacted with a set of regional dummy variables) as another way to account for any spurious correlation between both the instrument and anti-American terrorism due to the geographical concentration of the targets of U.S. military aid and of anti-U.S. terrorism in specific terrorism hot spots.

Furthermore, we consider two alternative ways to construct our IV. First, we employ an alternative definition of the instrument in which we alter the excluded region around the country of interest. In detail, rather than differentiating between 18 world regions we only consider seven (East Asia and the Pacific; Eastern Europe; Western Europe; the former Soviet Union; Sub-Saharan Africa; the Americas; Northern Africa and the Middle East). This is another way to assess whether shocks that affect U.S. military aid to other parts of the world do not systematically correlate with our instrument. Second, we do not weigh our IV with the relative importance of the various military aid programs for the country of interest. This is to explore to what extent our IV's identification comes from these weights, where these weights may potentially predict anti-American terrorism through channels other than the receipt of U.S. military aid.

Finally, we run a specification where we augment our baseline IV with a set of instruments following Lewbel (2012) who proposes to generate additional (internal) instruments in the form of a vector of regressors that are uncorrelated with the covariance of heteroskedastic errors; here, identification is achieved by having regressors that are uncorrelated with the product of heteroskedastic errors.[19] The use of additional instruments also

[19]Technically, this procedure involves (i) a regression of the military aid variable on the vector of controls

allows us to perform the Hansen over-identification test, which is useful to reduce concerns about misspecification.

Table 3: **Robustness of IV-Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Robustness Check→	Regional Trends	Terrorism and Conflict	Economic and Political Development	All Time-Variant Controls	Different Regions for IV	No Aid Weights for IV	Add Lewbel-type Instruments
Military Aid	0.030***	0.031***	0.038***	0.031***	0.043***	0.013***	0.007***
	(0.008)	(0.011)	(0.014)	(0.011)	(0.016)	(0.003)	(0.002)
Civil Conflict	0.116***	0.121***	0.118***	0.119***	0.122***	0.128***	0.128***
	(0.015)	(0.014)	(0.016)	(0.014)	(0.016)	(0.016)	(0.016)
Child Mortality	-0.000	-0.000**	-0.000	-0.000	-0.001**	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Regime Type	0.006	0.006	-0.003	0.000	-0.001	0.005	0.004
	(0.008)	(0.011)	(0.009)	(0.009)	(0.012)	(0.009)	(0.009)
Population Size	-0.025	0.086***	0.068**	0.078***	0.080**	0.114***	0.085***
	(0.039)	(0.025)	(0.032)	(0.027)	(0.031)	(0.022)	(0.017)
Anti-American Terrorism (ROW)		-0.007***		-0.007***			
		(0.001)		(0.001)			
Civil Conflict (ROW)		0.004		0.004			
		(0.005)		(0.005)			
Child Mortality (ROW)			0.001*	0.000			
			(0.000)	(0.000)			
Regime Type (ROW)			-0.005	-0.017			
			(0.019)	(0.018)			
N*T	7,381	7,381	7,381	7,381	7,381	7,381	7,381
First-Stage F-Statistic	34.16	13.88	13.85	14.05	11.85	11.50	123.1
AR-Test (p-value)	(0.00)***	(0.00)***	(0.00)***	(0.00)***	(0.01)**	(0.01)**	(0.00)***
Hansen-Test (p-value)							(0.11)

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. ROW=Rest of the World. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

In Table 3 we report our empirical results when (1) controlling for regional trends as well as economic or political shocks in the rest of the world, (2) using alternative definitions of our IV and (3) adding Lewbel-type instruments to aid identification. We find that the inclusion of regional trends and time-varying covariates is of little consequence to the effect (both in terms of direction and size) of U.S. military aid on anti-American terrorism compared to our baseline estimates reported above in Table 2. Using alternative definitions

and fixed effects, (ii) storing the residual from this regression, (iii) mean-centering all controls and (iv) multiplying the residual from (ii) with the respective mean-centered variables from (iii), where this vector of products are the generated internal instruments. While the residual from (ii) has zero covariance with each of the regressors used to construct it, its products with the centered regressors from (iv) will contain sizable elements if there is evidence of heteroskedasticity, thus allowing for identification via heteroskedasticity. Indeed, for our case a modified Wald statistic for group-wise heteroskedasticity on the residual from (i) strongly implies the presence of heteroskedastic errors necessary for identification.

of the IV, our findings also conform to our baseline results. Adding the Lewbel-type instruments yields smaller coefficients with respect to the effect of aid on terrorism, even as the effect is estimated precisely enough to again support the notion that aid promotes anti-American terrorism.[20] What is more, the Hansen over-identification test available when also employing Lewbel-type instruments provides no evidence of misspecification. In sum, these robustness checks probe various ways in which our baseline IV-approach and identification strategy could be invalidated: We are able to show that such concerns are not warranted.

5.2. Additional Confounders

We evaluate whether our main findings are robust to the inclusion of additional covariates. We amend our baseline model with additional variables measuring (1) a country's exposure to globalization, (2) its economic growth, (3) its natural resource wealth, (4) incidences of international conflict and (5) demographic conditions (the urbanization rate and male youth burden). The summary statistics, data sources and definitions of these additional variables are reported in Supplementary Table A.5. We also run a specification that includes a lagged dependent variable to capture dynamic effects. As shown in Supplementary Table A.2, the inclusion of these covariates does not affect our main conclusion: Higher levels of U.S. military aid robustly induce more anti-American terrorist activity in aid-receiving countries, with the associated IV-diagnostics indicating that these results are trustworthy.

Furthermore, we consider a number of modifications to our baseline model. In detail, we (1) replace our baseline measurement of economic development, child mortality, with a country's per capita income, (2) replace the regime type variable with an alternative measure of democracy and (3) substitute the variable measuring the incidence of civil conflict with one measuring the magnitude of civil conflict (summary statistics, data sources and definitions are provided in Supplementary Table A.5). Inspired by Enders et al. (2016) and Gaibulloev et al. (2017), we also run two specifications that include quadratic terms for child mortality and regime type, respectively, to allow for non-linear effects of economic and political development on anti-American terrorism. As shown in Supplementary Table A.3, however, these modifications to our baseline model are immaterial to our main finding of an adverse effect of U.S. military aid on anti-American terrorism in line with *H2*.

[20] In the case of the Lewbel-type instruments, inspection of the first-stage regression results indicate that some constructed instruments are not relevant. This likely contributes to the apparent downward bias.

Finally, it is possible that the provision of U.S. military aid correlates with other aid and military variables, which could affect identification of the effect of the former on anti-U.S. terrorism. For instance, regular recipients of U.S. military aid also tend to house larger U.S. troop contingents.[21] Thus, we also run a series of models where we control for (1) the total level of economic assistance a country receives (WDI data), (2) the presence of U.S. troops in aid-receiving countries using data from an update of Kane (2012) and (3) the existence of a collective defense agreement of the aid recipient with the United States.[22] Further information on these variables is provided in Supplementary Table A.5. As shown in the appendix (Supplementary Table A.4), these robustness checks also do not affect our baseline IV-findings concerning the role of military aid in anti-U.S. terrorism. Effect sizes are comparable to our baseline finding.

5.3. Alternative Dependent Variables

Furthermore, we assess whether our results are affected by the choice of the dependent variable. Our baseline dependent variable is a binary indicator equal to unity when a country experiences any kind of transnational anti-American terrorism using the location definition of transnational terrorism (i.e., an incident is assigned to the country in which the incident occurs). Using the same location definition, our first alternative dependent variable is the *count of anti-American terrorist attacks* per country-year pair. Furthermore, we weigh this count by local population size to consider scale effects, creating an additional dependent variable (the *per capita number of attacks*). For example, Jetter and Stadelmann (2019) show that measuring terrorism in per capita terms may yield different empirical results compared to measuring terrorism in absolute terms.

We also create three additional dependent variables (a binary indicator equal to unity when there is any anti-American terrorism, the number of terrorist attacks and the number of terrorist attacks per capita) using the *origin definition* of transnational terrorism. Here, an anti-American attack is assigned to the country of origin of the terrorist perpetrator, regardless of the venue country of the anti-American terrorist attack. A major problem of this approach, however, is that it cannot properly account for attacks that are not claimed

[21] Importantly, however, U.S. military aid does not include payments for U.S. troops in aid-receiving countries.

[22] As reported by the U.S. State Department, military alliances with the U.S. include *NATO, ANZUS, SEATO*, the *Rio Pact* as well as further bilateral U.S. alliances with Japan, South Korea, the Philippines and Pakistan. This variable varies over time, e.g., as countries join *NATO* or *SEATO* was disbanded.

or that are claimed by multiple terrorist organizations, a feature that tends to be the norm in transnational terrorism (Abrahms and Conrad, 2017). Indeed, while the *ITERATE* dataset reports approximately 15,000 transnational terrorist incidents between 1968 and 2018, for over 5,000 incidents we cannot determine a country of origin. This may introduce considerable uncertainty into our estimates, especially as we expect the distribution of cases where the origin of terrorist perpetrators is unknown to be non-random, clustering in countries that have low levels of press freedom and weak communication systems.

Finally, we create three dependent variables (a binary indicator equal to unity when there is any anti-American terrorism, the number of terrorist attacks, and the number of terrorist attacks per capita) using data from the *Global Terrorism Database* (*GTD*) first introduced by LaFree and Dugan (2007). As the GTD does not report information on the country of origin of perpetrators (Enders et al., 2011, p.323), we use the location definition of transnational terrorism when using the GTD data. The main advantage of the GTD is that it is available free of charge, which has led to it becoming the 'industry standard' in terms of terrorism data.[23] On the other hand, there are a number of problems associated with *GTD* detailed in Enders et al. (2011): For instance, the data only starts in 1970 (*ITERATE* starts in 1968), data for 1993 is incomplete and coding conventions changed over time (while *ITERATE* uses a consistent coding scheme). Furthermore, compared to *ITERATE*, *GTD* tends to undercount terrorist activity in the 1970s and overcount it in the 1990s (Enders et al., 2011). Following Enders et al. (2011), there are thus a number of reasons why we prefer to use the data on transnational terrorist activity reported in *ITERATE* over *GTD*. Still, it is clearly worthwhile to assess whether the choice of the dataset used to construct our dependent variables matters to our empirical conclusions.

We report our empirical findings in Table 4. Regardless of which alternative dependent variable we employ, we always find that more U.S. military aid leads to a higher likelihood of the aid-receiving country to produce anti-American terrorism. What is more, the associated regression diagnostics always indicate that the IV-results are sound. Finally, the point estimates are reasonably close across terrorism definitions and data sources, suggesting that our results are not sensitive to the choice of the dependent variable.

[23]The GTD data can be donwloaded at `https://www.start.umd.edu/gtd/`.

Table 4: **Alternative Measurements of Anti-American Terrorism**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Measurement of Terrorism →	Binary (Location)	No. of Attacks (Location)	Attacks Per Capita (Location)	Binary (Origin)	No. of Attacks (Origin)	Attacks Per Capita (Origin)	Binary (GTD)	No. of Attacks (GTD)	Attacks Per Capita (GTD)
Military Aid	0.038***	0.261**	0.040***	0.022**	0.098*	0.020**	0.030*	0.256***	0.029**
	(0.014)	(0.130)	(0.015)	(0.009)	(0.057)	(0.009)	(0.016)	(0.089)	(0.013)
Civil Conflict	0.123***	0.534***	0.108***	0.117***	0.388***	0.055***	0.110***	0.215	0.056***
	(0.016)	(0.117)	(0.027)	(0.019)	(0.080)	(0.015)	(0.018)	(0.156)	(0.019)
Child Mortality	-0.001***	-0.001	-0.000**	-0.000***	-0.001	0.000	-0.000	0.002*	-0.000
	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.001)	(0.000)
Regime Type	-0.000	-0.003	-0.008	0.002	0.048	-0.004	0.008	0.105	0.003
	(0.011)	(0.087)	(0.009)	(0.007)	(0.045)	(0.004)	(0.013)	(0.095)	(0.010)
Population Size	0.085***	0.432**	-0.113**	0.073***	0.311***	-0.021	0.098***	0.492*	-0.050
	(0.030)	(0.188)	(0.045)	(0.020)	(0.102)	(0.024)	(0.030)	(0.252)	(0.036)
N*T	7,381	7,381	7,381	7,381	7,381	7,381	7,173	7,173	7,173
First-Stage F-Statistic	13.61	13.61	13.61	13.61	13.61	13.61	11.79	11.79	11.79
AR-Test (p-value)	(0.00)***	(0.06)*	(0.00)***	(0.03)**	(0.09)*	(0.04)**	(0.03)**	(0.00)***	(0.02)**

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. GTD=Global Terrorism Database. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

5.4. Alternative Operationalizations of Military Aid

In our baseline model, U.S. military aid enters the model in a non-transformed form, allowing us to assess how the risk of anti-U.S. terrorism responds to a one-unit (i.e., 100 million US$) increase in U.S. military aid. As a robustness check, we consider whether the operationalization of our main explanatory variable of interest, U.S. military aid, matters.

First, we consider an inverse hyperbolic sine transformation of our military aid variable. Doing so potentially allows us to, e.g., reduce the influence of outliers, approximating the natural logarithm of the military aid variable while retaining zero-valued observations for which the natural logarithm itself would be undefined (e.g., Bellemare and Wichman, 2020). Our IV is transformed in the same way. Second, we weigh military aid by local population size to account for potential scaling effects. Here, we use both a non-transformed and inverse hyperbolic sine transformed version of the military aid per capita and the associated instrumental variable. Third, we winsorize the military aid variable at the 95th percentile of its distribution, i.e., we replace the largest values of the aid variable by their values at the 95th percentile. This also allows us to reduce the influence of outliers. Finally, we trim the aid variable at the 95th percentile of its distribution, meaning that observations with very large values of the military aid variable are dropped from our sample. This helps to assess whether our results are driven by extraordinary amounts of military aid given to some countries, e.g., to post-2001 Afghanistan or post-2003 Iraq.

We report our empirical findings in Table 5. Irrespective of how we operationalize mil-

itary aid, we find that more U.S. military aid results in more anti-American terrorism.[24] The associated IV-diagnostics are always sound. We conclude that our main finding of an adverse effect of U.S. military aid on anti-American terrorism is not due to the operationalization of the main explanatory variable.

Table 5: **Alternative Measurements of Military Aid**

	(1)	(2)	(3)	(4)	(5)	(6)
Measurement of Military Aid →	Baseline	Baseline, IHS	Aid Per Capita	Aid Per Capita, IHS	Winsorized, 95%	Trimmed, 95%
Military Aid	0.038***	0.145***	0.006***	0.053***	0.151***	0.185**
	(0.014)	(0.030)	(0.002)	(0.011)	(0.048)	(0.085)
Civil Conflict	0.123***	0.119***	0.127***	0.125***	0.119***	0.115***
	(0.016)	(0.015)	(0.016)	(0.016)	(0.016)	(0.016)
Child Mortality	-0.001***	-0.001***	-0.000	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Regime Type	-0.000	0.003	-0.006	0.003	0.004	-0.009
	(0.011)	(0.009)	(0.010)	(0.009)	(0.009)	(0.009)
Population Size	0.085***	0.090***	0.152***	0.117***	0.102***	0.076***
	(0.030)	(0.024)	(0.028)	(0.021)	(0.022)	(0.023)
N*T	7,381	7,381	7,381	7,381	7,381	6,688
First-Stage F-Statistic	13.61	81.48	35.58	148.1	72.34	46.47
AR-Test (p-value)	(0.00)***	(0.00)***	(0.00)***	(0.00)***	(0.00)***	(0.05)**

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. IHS=Inverse hyperbolic sine transformation. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

5.5. Lag Structure

In our baseline specification, we examine the contemporaneous effect of U.S. military aid on anti-American terrorism. We now consider an alternative lag structure, testing how lagged U.S. military aid affects the production of anti-American terrorism in recipient countries. As shown in Supplementary Table A.6, higher levels of U.S. military aid also lead to more anti-American terrorism at deeper lags. While we no longer detect a statistical significant effect at the fourth lag, especially for the second and third lag effect sizes tend to become—at the expense of statistical precision—somewhat larger, pointing to persistent effects that may need some time to fully materialize.

[24] As another robustness check, we also use a log transformation of the military aid variable, with unity being added to zero-valued observations. Applying this log transformation, we also find that higher levels of military aid induce more anti-American terrorism.

5.6. Sub-Sample Analysis

As a final robustness check, we assess whether specific world regions drive our results. In detail, we first drop all OECD countries from the sample. The ill effects of U.S. military aid may be less pronounced in these countries as they are more developed and democratic. Second, we successively drop from our sample all Middle Eastern, Latin American and sub-Saharan African countries. Third, we successively drop from our sample all countries that never produce anti-U.S. terrorism (roughly 25% of our sample) as well as countries where a large number of U.S. troops (more than 10,000 servicemen) were stationed (e.g., Germany, Iraq, and Afghanistan). In doing so, we expect to reduce the effect of influential outliers on our regression results in that some of these countries may have had enough leverage to affect the global patterns of U.S. military aid for political or geo-strategic reasons.

As reported in the appendix (Supplementary Table A.7), there is no evidence that dropping data from specific world regions or 'special' countries affects our findings. That is, more U.S. military aid continues to lead to more anti-American terrorism for all reduced samples we consider.

5.7. Effects of U.S. Military Aid on American Allies and Foes

As an extension to our main analysis, we finally study how U.S. military aid affects transnational terrorism directed against other nations. Here, we consider four American allies that were most affected by transnational terrorist activity. Using the location definition of transnational terrorism, our additional dependent variables are equal to unity when a country-pair sees at least one transnational terrorist attack against (1) the United Kingdom, (2) France, (3) Israel and (4) Turkey. Out of the approximately 15,000 transnational terrorist incidents recorded in *ITERATE* between 1968 and 2018, approximately 1,100 (7.5%) are directed against British, 800 (5%) against French, 680 (4.5%) against Israeli and 450 (3%) against Turkish targets. Still, these countries are far less often attacked than the U.S.; anti-U.S. terrorism accounts for approximately 27% of all incidents recorded during our observation period.

We may hypothesize that U.S. military aid creates spillover effects in the form of terrorism that is directed against American allies in aid-receiving countries. For instance, resentment against the U.S. due to aid may also translate into more general anti-Western resentment and thus lead to terrorism against Western U.S. allies such as the United Kingdom and France. At the same time, however, we expect such spillover effects—if they

are detectable—to be smaller than the effect of U.S. military aid on anti-U.S. terrorism in countries receiving U.S. military aid.

Furthermore, we expect U.S. military aid to induce *no* transnational terrorist activity against geo-political adversaries of the U.S. in aid-receiving countries. Indeed, it is an important placebo test to study the impact of U.S. military aid on transnational terrorism against nations that were not aligned with the United States during our observation period. Thus, we also create three additional dependent variables from the *ITERATE* dataset that are equal to unity when a country-pair sees at least one transnational terrorist attack against (1) Iran, (2) the USSR or Russia (after 1991), and (3) Syria. We choose these three countries for two reasons. First, they have been—at least at times—geo-political opponents of the U.S.. For instance, Iran became a prominent U.S. foe after the Islamic Revolution in 1979. Second, these countries experienced noticeable transnational terrorist activity, even though they were still far less likely to be affected by transnational terrorism than the U.S.: Between 1968 and 2018, there were approximately 230 (1.5% out of 15,000 total transnational terrorist incidents) transnational terrorist attacks against Iranian, 410 (2.5%) attacks against Soviet/Russian and 110 (0.75%) attacks against Syrian interests.

Employing our usual IV-approach, we report our findings concerning the effect of U.S. military aid on transnational terrorism directed against other nations in countries receiving U.S. military aid in Table 6.[25] For comparison, we also report our baseline results that relate U.S. military aid to the production of anti-American terrorism.

Our analysis shows that there is no comparable effect of U.S. military aid on the production of transnational terrorism against British, French and Turkish interests in aid-receiving countries. If anything, there is a statistically significant and positive effect on anti-Israeli terrorism. This may point to the purportedly close politico-military relationship between the U.S. and Israel resulting in adverse spillover effects for Israel, even though the estimated effect of U.S. military aid on anti-Israeli terrorism is rather small. Moreover, the AR-test results indicate that there may be a weak-instrument issue when estimating the former effect, casting doubt on the validity of these estimates. In sum, our results thus provide little evidence that U.S. military aid also results in terrorism spillover effects against American

[25]Note that for this analysis, we always drop the country that is the respective target of transnational terrorism from our sample. For instance, when we study anti-French transnational terrorism, we do not include data on France itself. This is to rule out that we would also erroneously consider terrorist activity by French perpetrators against foreign targets.

Table 6: **Effect of U.S. Military Aid on Terrorism against Other Nations**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Terrorism Target →	USA (Baseline)	United Kingdom	France	Israel	Turkey	Iran	USSR/ Russia	Syria
Military Aid	0.038***	0.006	0.014	0.010**	-0.004	0.002	-0.007	0.003
	(0.014)	(0.009)	(0.009)	(0.005)	(0.006)	(0.003)	(0.005)	(0.003)
Civil Conflict	0.123***	0.062***	0.059***	0.016**	0.030***	0.017**	0.066***	0.011**
	(0.016)	(0.010)	(0.012)	(0.008)	(0.009)	(0.008)	(0.012)	(0.004)
Child Mortality	-0.001***	-0.000**	-0.000**	-0.000**	-0.000	0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Regime Type	-0.000	0.010**	0.008	0.013***	0.014***	0.008***	0.008	0.004
	(0.011)	(0.004)	(0.005)	(0.005)	(0.004)	(0.003)	(0.006)	(0.003)*
Population Size	0.085***	0.028	0.000	0.051***	0.050**	0.019**	0.039***	0.010
	(0.030)	(0.022)	(0.014)	(0.020)	(0.023)	(0.010)	(0.015)	(0.007)
N*T	7,381	7,330	7,330	7,330	7,330	7,333	7,355	7,330
First-Stage F-Statistic	13.61	13.40	13.60	14.09	11.26	13.19	13.84	13.66
AR-Test (p-value)	(0.00)***	(0.54)	(0.04)**	(0.11)	(0.44)	(0.60)	(0.20)	(0.21)

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. USSR/Russia=Data refers to USSR till 1991 and to Russia from 1992 onwards. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

allies. Importantly, there is also no evidence that U.S. military aid affects the patterns of transnational terrorism directed against American foes. That is, we do not find that countries receiving U.S. military aid are more likely to produce transnational terrorism against Iranian, Soviet/Russian, and Syrian targets. This is an important placebo test: Finding that U.S. military aid does not share the same relationship with transnational terrorism against nations hostile to the U.S. suggests that our identification strategy is indeed valid.

6. Exploration of Transmission Channels

In the previous section, we provided a broad battery of evidence that more U.S. military aid leads to more anti-U.S. terrorism. Above, we hypothesized that more aid may result in more anti-U.S. terrorism either due to (1) the strategic logic argument of Neumayer and Plümper (2011) who argue that military aid benefits local state capacity, which makes terrorist success less likely and thus creates a strategic incentive for terrorist groups to attack the U.S. to cut military aid, or (2) the grievances argument we developed above, where U.S. military aid induces rent-seeking behavior in the recipient country, encouraging terrorism directed against the U.S. as sponsor of the local government by those that lose out on direct and indirect access to rents or are otherwise adversely affected by weaker institutions as a consequences of aid inflows.

In this section, we aim to explore whether U.S. military aid indeed affects state capacity

(in line with the strategic logic argument) and/or distributional grievances (in line with our grievances argument).

6.1. Model and Variables

We consider the following two-stage equation system:

$$aid_{it} = \delta \times globalaid_{it} + \mu \times X_{it} + \alpha_i + \tau_t + \epsilon_{it} \tag{6a}$$

$$channel_{jit} = \beta \times \widehat{aid_{it}} + \gamma \times X_{it} + \alpha_i + \tau_t + \epsilon_{it} \tag{6b}$$

We employ our usual IV-approach to instrument military aid, controlling for the usual baseline covariates and country- as well as year-fixed effects. We use the first-stage regression results to estimate the causal effect of U.S. military aid on the various transmission variables (*channel*) introduced below.

State Capacity Variables. Hendrix and Young (2014) differentiate between two dimensions of state capacity: Military and administrative capacity. Following their example, by means of principal component analysis we first construct a composite indicator of *military capacity* by accounting for total local military spending, total military personnel as well as military spending in relation to the military personnel. The data are drawn from the *National Material Capabilities Dataset (NMC)* (updated from Singer, 1988).[26] Furthermore, from the *VDEM Dataset* we extract another variable that measures a *state's control over its territory*. This control might be challenged by criminals, warlords, insurgents or other sub-national actors. Finally, to indicate administrative capacity we use the *quality of bureaucracy index* from the *VDEM Dataset*. This index measures arbitrariness and biases in the administration of the law by public officials, with higher values corresponding to a sounder bureaucracy.

Grievances Variables. We also employ three variables reflecting rent-seeking behavior and distributional conflict in aid-receiving countries. First, we utilize an index of the *equal distribution of resources* from the *VDEM Dataset*. This variable reflects the distribution of public goods such as health and education, with higher values corresponding to a fairer distribution. Second, we measure the extent of *political corruption*, using a corruption index from the *VDEM Dataset*; higher values of this index mean higher levels of corruption. This

[26] Note that the NMC data are only available for the 1968-2012 period.

indicator describes both the potential loss in institutional quality and the extent of rent-seeking induced by the inflow of U.S. military aid. Finally, we use an index measuring the extent of *clientelism* within a country. Clientelism refers to the targeted distribution of resources (goods, services, jobs, money etc.) in exchange for political support, e.g., in the form of vote buying or particularistic politics. The clientelism variable also comes from the *VDEM Dataset*, where higher values mean that clientelism is more common.

6.2. Empirical Results

Concerning the effect of U.S. military aid on state capacity, we find no evidence that aid increases local military strength, government authority over its territory or the quality of its bureaucracy (see Table 7). There is thus little evidence that U.S. military aid bolsters local state capacity. In turn, this suggests that the positive relationship between U.S. military aid and anti-American terrorism is not due to the strategic logic channel, which posits that anti-American terrorism ought to be—somewhat paradoxically—the consequence of the beneficial effect of U.S. military aid on local state capacity.

Table 7: **Potential Transmission Channels**

	(1)	(2)	(3)	(4)	(5)	(6)
Dependent Variable (Transmission Channel) →	Military Capacity	Territorial Control	Quality of Bureaucracy	Distribution of Resources	Corruption	Clientelism
Military Aid	-0.012	-0.004	-0.040	-0.016***	0.016***	0.025***
	(0.013)	(0.321)	(0.041)	(0.004)	(0.004)	(0.008)
Civil Conflict	-0.039*	-6.722***	-0.275***	-0.024***	0.010**	-0.009
	(0.022)	(0.741)	(0.030)	(0.007)	(0.005)	(0.008)
Child Mortality	0.002***	-0.003	0.001	-0.000	-0.001***	-0.000
	(0.001)	(0.004)	(0.001)	(0.000)	(0.000)	(0.000)
Regime Type	-0.086***	0.999***	0.597***	0.034***	-0.042***	-0.043***
	(0.013)	(0.186)	(0.027)	(0.003)	(0.004)	(0.005)
Population Size	-0.121**	0.779**	-0.157***	0.097***	-0.026***	-0.013
	(0.052)	(0.342)	(0.059)	(0.010)	(0.008)	(0.015)
N*T	5,909	7,379	7,381	7,381	7,347	7,381
First-Stage F-Statistic	22.25	13.60	13.61	13.61	13.52	13.61
AR-Test (p-value)	(0.35)	(0.99)	(0.38)	(0.00)***	(0.01)**	(0.01)**

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

By contrast, we find that U.S. military aid induces a more unfair distribution of public goods as well as higher levels of corruption and clientelism in aid-receiving countries; the associated IV-diagnostics are sound and point to a causal interpretation of these findings. These findings provide evidence in favor of the grievances channel from military aid to anti-American terrorism we proposed in this paper. That is, military aid induces rent-seeking behavior (as indicated by increases in corruption) and results in weaker institutions as well as more exclusionary and clientelistic policies, corresponding to lower opportunity costs of terrorism for the 'losers' of these processes that may consequently encourage anti-American resentment and violence.

7. Conclusion

There is growing concern that U.S. interventionism—be it in the form of direct military engagement or through the provision of aid—can have unintended consequences and even backfire on the United States. Referring to this discussion, we ask whether U.S. military aid makes the United States safer from transnational terrorism. We investigate this question with data for 174 countries between 1968 and 2018. We exploit plausibly exogenous time variation in global levels of U.S. military aid associated with distinct aid programs and cross-national time-series variation in the relative importance of the various military aid programs for recipient countries to provide causal estimates of the effect of U.S. military aid on anti-American terrorism.

Our empirical analysis allows us to refute the argument—often voiced by U.S. policy-makers—that U.S. military aid bolsters U.S. security: We find (1) no evidence that U.S. military aid contributes to increased local state capacity and (2) no evidence that more U.S. military aid makes the United States less vulnerable to terrorism.

Rather, we show that higher levels of military aid cause higher levels of anti-American terrorist activity in recipient countries. This finding is robust to a variety of empirical modifications including, e.g., controlling for economic or political shocks that may correlate with our instrumental variable, alternative definitions of our IV, the use of alternative measures of anti-American terrorist activity and different measurements of U.S. military aid. As an important placebo test, we also show that U.S. military aid only matters to anti-American terrorism but not to transnational terrorism against U.S. allies and opponents.

We also examine the transmission channels through which U.S. military aid promotes anti-American terrorism. We find that more U.S. military aid leads to more economic-

political exclusion and rent-seeking (corruption). This latter finding speaks to the grievances channel we introduced in the paper. U.S. military aid induces rent-seeking and distributional conflict. Aggrieved social groups that are negatively affected by weakened institutions and have no direct or indirect access to the rents U.S. military aid entails, in turn, direct their dissatisfaction against the United States as the perceived linchpin of an unfavorable status quo in the recipient country.

The results from our preferred specification suggest that at the sample-mean, doubling U.S. military aid increases the risk of anti-American terrorism by 2.7 percentage points, which is approximately 17.5% of the sample-mean incidence of anti-American terrorism. This points to substantial and adverse security effects of U.S. military aid. Such effects are expected to be particularly pronounced under certain circumstances (e.g., in times of state collapse) when U.S. military aid handed out can easily amount to hundreds of millions or even billions of US$ (USAID, 2019).

In sum, our empirical findings support the notion that (1) foreign aid can have unintended consequences in aid-receiving countries (e.g., by encouraging rent-seeking and corruption), (2) aid can promote rather than curtail violence, and (3) military aid can backfire, which calls into question its effectiveness as a counter-terrorism tool.

Clearly, the United States provides military aid for a number of reasons other than counter-terrorism, e.g., to gain military access, secure strategic positions, earn political favors at international organizations, help obtain market access for the U.S. defense industry or acquire trade benefits from recipient countries. These benefits may very well be substantial. However, our study suggests that these benefits need to be weighed against the security risks the provision of military aid may entail, especially since even single acts of anti-U.S. terrorism can be extremely costly in social, political, and economic terms. For instance, in addition to human casualties, the costs of repairing the *USS Cole* (targeted by anti-U.S. terrorism in Yemen in 2000) were more than 250 million US$.[27] As another example, the political fallout from the 2012 terrorist attack on the U.S. consulate in Benghazi, Libya, was highly influential in both the 2012 and 2016 U.S. presidential elections.

[27] See https://news.usni.org/2017/07/27/repair-bill-uss-fitzgerald-collision-will-cost-fix%2Duss-cole-terror-attack.

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Table A.1: **List of Countries**

Afghanistan	Egypt	Liberia	Saudi Arabia
Albania	El Salvador	Libya	Senegal
Algeria	Eq. Guinea	Lithuania	Serbia
Angola	Eritrea	Luxembourg	Seychelles
Argentina	Estonia	Madagascar	Sierra Leone
Armenia	Eswatini	Malawi	Singapore
Australia	Ethiopia	Malaysia	Slovakia
Austria	Fiji	Maldives	Slovenia
Azerbaijan	Finland	Mali	Solomon Islands
Bahrain	France	Malta	Somalia
Bangladesh	Gabon	Mauritania	South Africa
Barbados	Georgia	Mauritius	South Korea
Belarus	Germany	Mexico	South Sudan
Belgium	Ghana	Moldova	Spain
Benin	Greece	Mongolia	Sri Lanka
Bhutan	Guatemala	Montenegro	Sudan
Bolivia	Guinea	Morocco	Suriname
Bosnia and Herzegovina	Guinea-Bissau	Mozambique	Sweden
Botswana	Guyana	Namibia	Switzerland
Brazil	Haiti	Nepal	Syria
Bulgaria	Honduras	Netherlands	Tajikistan
Burkina Faso	Hong Kong	New Zealand	Tanzania
Burma/Myanmar	Hungary	Nicaragua	Thailand
Burundi	Iceland	Niger	The Gambia
Cambodia	India	Nigeria	Timor-Leste
Cameroon	Indonesia	North Korea	Togo
Canada	Iran	North Macedonia	Trinidad and Tobago
Cape Verde	Iraq	Norway	Tunisia
Central African Republic	Ireland	Oman	Turkey
Chad	Israel	Pakistan	Turkmenistan
Chile	Italy	Palestine/West Bank	Uganda
China	Ivory Coast	Panama	Ukraine
Colombia	Jamaica	Papua New Guinea	United Arab Emirates
Comoros	Japan	Paraguay	United Kingdom
Costa Rica	Jordan	Peru	Uruguay
Croatia	Kazakhstan	Philippines	Uzbekistan
Cuba	Kenya	Poland	Vanuatu
Cyprus	Kosovo	Portugal	Venezuela
Czech Republic	Kuwait	Qatar	Vietnam
DR of the Congo	Kyrgyzstan	Rep. of the Congo	Yemen
Denmark	Laos	Romania	Zambia
Djibouti	Latvia	Russia	Zimbabwe
Dominican Republic	Lebanon	Rwanda	
Ecuador	Lesotho	Sao Tome and Principe	

Table A.2: **Inclusion of Additional Control Variables**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Military Aid	0.032**	0.027*	0.029**	0.038***	0.043***	0.038**	0.029**
	(0.015)	(0.014)	(0.014)	(0.014)	(0.016)	(0.015)	(0.013)
Civil Conflict	0.123***	0.112***	0.107***	0.123***	0.123***	0.123***	0.108***
	(0.016)	(0.017)	(0.018)	(0.016)	(0.017)	(0.016)	(0.015)
Child Mortality	-0.001***	-0.001***	-0.001***	-0.001***	-0.000	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Regime Type	0.003	-0.000	-0.003	-0.000	-0.004	-0.000	-0.002
	(0.013)	(0.010)	(0.010)	(0.011)	(0.011)	(0.011)	(0.010)
Population Size	0.086***	0.133***	0.127***	0.085***	0.063*	0.085***	0.072***
	(0.032)	(0.038)	(0.029)	(0.030)	(0.037)	(0.030)	(0.028)
Globalization Index	-0.001						
	(0.002)						
Economic Growth		0.001*					
		(0.001)					
Natural Resource Rents			0.002***				
			(0.001)				
International Conflict				0.004			
				(0.008)			
Urbanization					0.004*		
					(0.002)		
Male Youth Burden						0.000	
						(0.004)	
Lagged Dependent Variable							0.139***
							(0.024)
N*T	7,121	6,780	6,711	7,381	7,381	7,381	7,241
First-Stage F-Statistic	11.36	11.54	9.191	13.69	11.65	11.87	10.94
AR-Test (p-value)	(0.04)**	(0.09)*	(0.06)*	(0.00)**	(0.01)***	(0.01)**	(0.03)**

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table A.3: **Robustness of Baseline Model**

	(1)	(2)	(3)	(4)	(5)	(6)
Military Aid	0.031**	0.038***	0.041***	0.034**	0.035**	0.036**
	(0.015)	(0.014)	(0.014)	(0.015)	(0.014)	(0.015)
Civil Conflict	0.114***	0.124***			0.120***	0.123***
	(0.017)	(0.016)			(0.016)	(0.016)
Child Mortality		-0.001***	-0.001***		0.000	-0.000***
		(0.000)	(0.000)		(0.000)	(0.000)
Regime Type	0.002		0.001		-0.000	0.055
	(0.010)		(0.011)		(0.011)	(0.040)
Population Size	0.205***	0.088***	0.086***	0.207***	0.091***	0.082***
	(0.034)	(0.029)	(0.031)	(0.034)	(0.031)	(0.028)
Per Capita Income	0.044*			0.046**		
	(0.023)			(0.023)		
VDEM Democracy Score		0.029		0.040		
		(0.040)		(0.039)		
Magnitude of Civil Conflict			0.034***	0.029***		
			(0.005)	(0.004)		
Child Mortality Squared					-0.000*	
					(0.000)	
Regime Type Squared						-0.022
						(0.015)
N*T	6,838	7,381	7,381	6,838	7,381	7,381
First-Stage F-Statistic	11.18	13.50	13.80	11.23	12.51	12.10
AR-Test (p-value)	(0.04)**	(0.01)**	(0.00)***	(0.04)**	(0.01)**	(0.02)**

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table A.4: **Additional Controls for Aid and Military Policy**

	(1)	(2)	(3)	(4)
Military Aid	0.042***	0.035**	0.038***	0.039**
	(0.016)	(0.014)	(0.014)	(0.016)
Civil Conflict	0.121***	0.114***	0.123***	0.112***
	(0.016)	(0.015)	(0.016)	(0.016)
Child Mortality	-0.001***	-0.000**	-0.001***	-0.000*
	(0.000)	(0.000)	(0.000)	(0.000)
Regime Type	0.000	0.004	-0.003	0.002
	(0.011)	(0.011)	(0.011)	(0.011)
Population Size	0.076**	0.105***	0.097***	0.108***
	(0.035)	(0.027)	(0.028)	(0.028)
Development Aid	-0.004			-0.003
	(0.002)			(0.002)
U.S. Troops		0.003		0.002
		(0.008)		(0.009)
Alliance with U.S.			0.067**	0.046
			(0.029)	(0.034)
N*T	7,381	6,725	7,381	6,725
First-Stage F-Statistic	8.820	14.34	13.62	9.436
AR-Test (p-value)	(0.00)***	(0.01)**	(0.00)***	(0.01)**

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table A.5: **Summary Statistics and Operationalizations of Additional Controls**

Variable	N*T	Mean	Stand. Dev.	Min.	Max.
Globalization Index	7,170	50.98	17.47	14.15	90.98
Economic Growth	6,831	2.051	6.163	-64.99	140.4
Natural Resource Rents	6,760	8.133	11.62	0	89.12
International Conflict	7,432	0.029	0.166	0	1
Urbanization	7,432	51.14	23.96	2.611	100
Male Youth Burden	7,432	16.65	4.737	2.677	31.83
Per Capita Income	6,770	8.186	1.508	5.067	11.663
VDEM Democracy Score	7,432	0.445	0.288	0.007	0.919
Magnitude of Civil Conflict	7,432	0.673	1.644	0	10
Development Aid	7,432	4.129	7.658	-0.643	94.95
U.S. Troops	6,725	0.147	1.091	0	39.78

Variable definitions:
1) Globalization Index – Definition: Globalization index accounting for de jure and de facto economic, political and social globalization.– Source: Gygli et al. (2019).
3) Economic Growth – Definition: Growth rate of real GDP per capita. – Source: World Development Indicators (WDI).
4) Natural Resource Rents – Definition: Sum of oil, natural gas, coal, mineral and forest rents as a share of GDP. – Source: WDI.
5) International Conflict– Definition: Dummy variable, equal to unity when there are incidences of international warfare or violence. – Source: Major Episodes of Political Violence Dataset (MEPV).
6) Urbanization – Definition: Share of total population living in urban areas. – Source: WDI.
7) Male Youth Burden – Definition: Share of males 15-29 as share of males 15-64. – Source: WDI.
8) Per Capita Income – Definition: Real GDP per capita, logged. – Source: WDI.
9) VDEM Democracy Score – Definition: Electoral democracy index, higher values mean higher levels of political development – Source: VDEM.
10) Magnitude of Civil Violence – Definition: Magnitude of civil war or civil violence. – Source: MPEV.
11) Development Aid – Definition: Net official development assistance received, as share of local GNI; missing replaced by zeroes. – Source: WDI.
12) U.S. Troops – Definition: Number of U.S. troops, divided by size of local population – Source: Update from Kane (2012).

Table A.6: **Lag Structure**

	(1)	(2)	(3)	(4)	(5)
Military Aid Lags →	Lag=0 (Baseline)	Lag=1	Lag=2	Lag=3	Lag=4
Military Aid	0.038***	0.032**	0.044**	0.041**	0.029
	(0.014)	(0.014)	(0.017)	(0.020)	(0.019)
Civil Conflict	0.123***	0.125***	0.121***	0.118***	0.124***
	(0.016)	(0.014)	(0.016)	(0.016)	(0.016)
Child Mortality	-0.001***	-0.001***	-0.001***	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Regime Type	-0.000	0.001	0.001	0.003	0.005
	(0.011)	(0.011)	(0.012)	(0.011)	(0.011)
Population Size	0.085***	0.091***	0.075**	0.076**	0.095***
	(0.030)	(0.031)	(0.036)	(0.037)	(0.036)
N*T	7,381	7,341	7,299	7,254	7,206
First-Stage F-Statistic	13.61	13.94	15.02	15.74	15.56
AR-Test (p-value)	(0.00)***	(0.03)**	(0.00)***	(0.04)**	(0.13)

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Table A.7: **Sub-Sample Analysis**

	(1)	(2)	(3)	(4)	(5)	(6)
Sub-Sample →	No OECD	No Middle East	No Latin America	No Sub-Saharan Africa	No Anti-American Terrorism	Only <10,000 U.S. Troops
Military Aid	0.052***	0.035*	0.036***	0.029**	0.042***	0.083**
	(0.018)	(0.018)	(0.011)	(0.011)	(0.014)	(0.038)
Civil Conflict	0.108***	0.126***	0.095***	0.117***	0.134***	0.072**
	(0.022)	(0.017)	(0.015)	(0.024)	(0.018)	(0.030)
Child Mortality	-0.000	-0.000	-0.001***	-0.001**	-0.000**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)
Regime Type	-0.011	0.007	0.005	-0.004	-0.001	-0.031**
	(0.012)	(0.010)	(0.011)	(0.013)	(0.014)	(0.015)
Population Size	0.003	0.120***	0.084***	0.048	0.110***	-0.032
	(0.033)	(0.035)	(0.028)	(0.035)	(0.036)	(0.048)
N*T	6,257	6,625	6,173	5,220	5,903	5,273
First-Stage F-Statistic	10.03	9.298	10.40	16.20	11.78	3.427
AR-Test (p-value)	(0.00)***	(0.09)*	(0.00)***	(0.03)**	(0.00)***	(0.02)**

Notes: IV-OLS estimates reported. All models include country- and year-fixed effects. AR=Anderson-Rubin. OECD=Organisation for Economic Co-operation and Development. Driscoll-Kraay standard errors in parentheses. *p<0.1, **p<0.05, ***p<0.01.