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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 consequences for U.S. security? By exploiting plausibly exogenous time variation in global levels of U.S. military aid and cross-national time-series variation in the relative importance of the various military aid programs for recipient countries, we provide causal estimates of the effect of U.S. military aid on anti-American terrorism for a sample of 174 countries between 1968 and 2018. We find that higher levels of military aid increase the likelihood of anti-American terrorism in recipient countries. At the sample mean, doubling U.S. military aid increases the risk of anti-American terrorism by 12.9 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, thus creating anti-American resentment among those parts of the population that do not have access to the benefits from military aid.

Keywords: U.S. military aid, anti-American terrorism, instrumental variable estimation
JEL: D74, F35

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1. Introduction

There is substantial evidence that U.S. military interventionism, despite ostensibly being initiated to counter insurgents that oppose U.S. interests, can produce unintended 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 United States. 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. 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.[1]

In this paper, we advance the literature on the unintended consequences of U.S. interventionism by examining, with a novel instrumental-variable approach, the causal effect of *U.S. military aid* on the emergence of *anti-American terrorism* in aid-receiving countries.[2] That is, we ask whether military aid makes the United States safer or—consistent with the notion of unintended consequences of aid—contributes to insecurity. Answering this question is highly relevant, given that between 1968 and 2018, the world saw over 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).[3] What is more, the United

[1]This is not to say that U.S. aid will always result in increased conflict. For instance, Berman et al. (2011) study patterns of violence 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 had the intended effect of reducing 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.

[2]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), 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, 2019b, p. 278).

[3]Terrorist incidents that affect more than one country are referred to as transnational terrorism. For

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States responded to this threat from transnational terrorism by providing—among other measures—substantial military aid to foreign governments: between 1968 and 2018, the U.S. 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).

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).[4] For another, there is the *strategic logic channel*, which predicts that U.S. military aid leads to *more* anti-American terrorism. According to this argument, a terrorist group resorts to anti-American terrorism in the hopes of reducing U.S. military aid for a domestic government that the terrorist group opposes; a reduction in aid will weaken this domestic government and thus make terrorist success more likely (Neumayer and Plümper, 2011).

Our first contribution to the literature is to introduce and empirically evaluate a third, so far unexplored connection between aid and anti-American terrorism. We argue that U.S. military aid allows recipient country's politicians and bureaucrats to act as gatekeepers and create 'winners' and 'losers,' with the former disproportionately benefiting by sharing in the rents from aid. At the same time, the same provision of U.S. military aid creates anti-American resentment among the 'losers', i.e., those parts of the population that do not have access to political and economic benefits arising from aid. For these population groups, U.S. military aid instead constrains the 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*.[5]

In addition to introducing this hitherto unappreciated pathway, another main contribution of this paper is to provide—for the first time—a thorough identification of the *causal* effect of U.S. military aid on anti-American terrorism. Here, we employ an instrumental-

instance, the 9/11 attacks were transnational because foreign terrorists attacked on U.S. soil, with non-Americans being also victimized.

[4]For instance, 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).

[5]Our argument taps into the literature on the role of grievances in political violence, in particular civil wars (e.g., Fearon and Laitin, 2003; Collier and Hoeffler, 2004; Blattman and Miguel, 2010; Djankov and Reynal-Querol, 2010). However, the role of grievances in anti-American terrorism has so far been disregarded.

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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 also *respond* to terrorist activity (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 time variation in *global levels of U.S. military aid* associated with three distinct military aid programs (concerning foreign military financing, military education and other aid programs). Second, we exploit cross-national time-series variation in the *relative importance* of these various 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 dependent on economic, political, and geo-strategic considerations *within the United States* (e.g., Irwin, 2000; Newhouse, 2009). 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, or the influence of the U.S. military-industrial complex that lobbies 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 in recipient countries. In our preferred IV-specification, at the sample mean, doubling U.S. military aid increases the risk of anti-American terrorism by 12.9 percentage points, which, in turn, is approximately 82.5% of the mean-incidence of anti-U.S. terrorism. Our main finding holds when subjected to various robustness checks, including those that probe the validity of our identification strategy: 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

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production of anti-American terrorism in the country of interest. Other tools that we employ to bolster a causal interpretation of our empirical results include the use of placebo instruments, the selection-on-observables framework of Oster (2019) and the plausibly-exogenous framework of Conley et al. (2012) and van Kippersluis and Rietveld (2018).

Finally, we explore through which transmission channels U.S. military aid can affect anti-American terrorism. In detail, we ask whether higher levels of aid lead 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 instead suffer from reduced economic and political participation. These aggrieved groups direct their dissatisfaction against the U.S. as the perceived linchpin of an unfavorable status quo.

In addition to adding to the empirical literature on the unintended consequences of U.S. military policy, 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 study buttresses 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 violence (e.g., Berman et al., 2011; Findley et al., 2011; Nunn and Qian, 2014; Crost et al., 2014; Strange et al., 2017). In general, there is a debate on whether foreign aid serves as a rent that has a *destabilizing* effect (e.g., because a predatory political elite fight over it, as in Grossman, 1992; Djankov et al., 2008) or a *stabilizing* effect because it helps the government to keep rebels in check (e.g., Collier and Hoeffler, 2002; De Ree and Nillesen, 2009). Our findings are in line with notions of a destabilizing effect of aid. Third, we contribute to the literature on the potentially undesired institutional and socio-political consequences of aid. For instance, next to the more general question of how effective aid is in achieving development outcomes (e.g., poverty reduction) under different aid regimes (e.g., Mosley et al., 2004; Bourguignon

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and Sundberg, 2007), various papers have investigated the detrimental effect of aid on variables such as democracy (e.g., Djankov et al., 2008). By showing that U.S. military aid promotes corruption and exclusionary policies, we provide further evidence that aid can undermine institutional quality in recipient countries. Finally, 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. Here, the evidence regarding the effectiveness of various counter-terrorism policies is generally mixed (for surveys, see, e.g., Mueller and Stewart, 2014; Schneider et al., 2015; Gaibulloev and Sandler, 2019b). Indeed, by showing that U.S. military aid encourages anti-American terrorism, our study emphasizes the ineffectiveness of U.S. military aid as a counter-terrorism tool in light of backlash effects.

The rest of this paper is organized as follows. In Section 2, we provide a more detailed theoretical explanation about how U.S. military aid may be related to anti-American terrorism. We introduce our data and empirical model in Section 3. Our baseline OLS- and IV-estimates are reported in Sections 4 and 5, respectively. These sections also include a battery of robustness checks. 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 (e.g., 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, 2019b). 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). By supplementing local state capacity, U.S. military aid is expected to raise the material costs of operating a terrorist group, consequently lowering terrorist activity against the U.S.

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Thus, the policy-maker argument suggests that U.S. military aid bolsters local state capacity, which, in turn, raises the operating costs of terrorism. Our hypothesis is:

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

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

2.2.1. The Strategic Logic Channel

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*. Here, we consider 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 (e.g., Neumayer and Plümper, 2011; Savage and Caverley, 2017; Biddle et al., 2018). Consequently, the local terrorist group may resort to anti-American terrorism in the hope of forcing the United States to withdraw its support from the local government (e.g., due to political pressure from the American public). This, 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 very success.[6]

2.2.2. The Grievances Channel

According to the *grievances channel* we highlight in this paper, military aid affects the terrorists' calculus by lowering the *opportunity costs of terrorism*. Indeed, several theoretical contributions suggest that poor country-specific conditions that constrain socio-economic and political participation create *grievances* (e.g., exclusion and discrimination). This, in turn, makes violence—as an alternative way to achieve such participation—comparatively more attractive (Blomberg et al., 2004; Kurrild-Klitgaard et al., 2006; Enders et al., 2016; Gaibulloev and Sandler, 2019b; Jetter and Stadelmann, 2019; Auer and Meierrieks, 2021).

[6]Terrorism may also produce additional incidental benefits, e.g., in form of 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, potentially further incentivizing anti-American terrorism.

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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.[7] 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 the case of 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 among politicians and bureaucrats in order to secure access to them (Alesina and Weder, 2002, see also Fisman and Golden 2017; Dimant and Tosato 2018; Andersen et al. 2020). Furthermore, the same access to such rents may disincentivize investment into public goods that would curtail rent-seeking (e.g., Svensson, 2000).

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 gatekeepers and create 'winners' and 'losers,' with the former disproportionately benefiting by sharing in 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).[8] 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.[9]

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

[7]Indeed, there is evidence that the allocation of U.S. foreign assistance is driven by U.S. strategic, economic and political interests (e.g., Poe and Meernik, 1995; Kuziemko and Werker, 2006; Dreher et al., 2008; Boutton and Carter, 2014).

[8]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).

[9]This refers to the issue of *aid fungibility*, i.e., the ability of the recipient country to spend 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 be used to benefit government supporters outside of the military sphere.

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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. Here, 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. in a more positive light.

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). These arguments lead to the following hypothesis:

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

3. Data

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 Appendix A. A country list is also provided in Appendix A.

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 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. As part of

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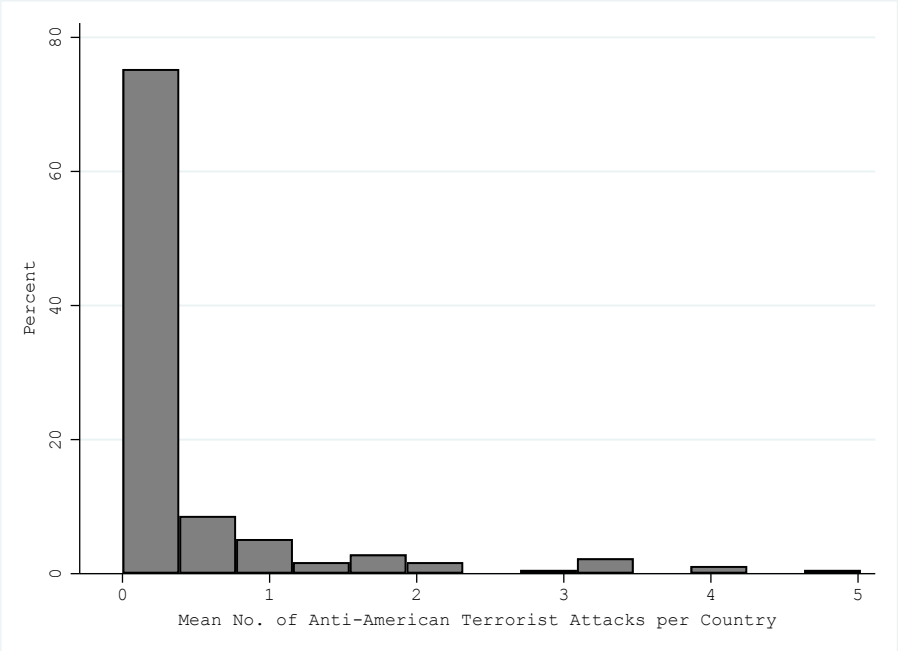
our robustness checks below, however, we also use alternative measures of anti-American terrorism, such as the count of anti-American terrorist attacks per country-year.

Anti-American terrorist attacks are carried out by non-U.S. actors and directed at U.S. diplomatic (e.g., embassies), military, commercial or non-official (e.g., American tourists) targets.[10] 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.

Figure 1: Histogram of Anti-American Terrorism



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 could originate from a third country, leading us to wrongly assign some cases.[11] 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

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

[11] 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.

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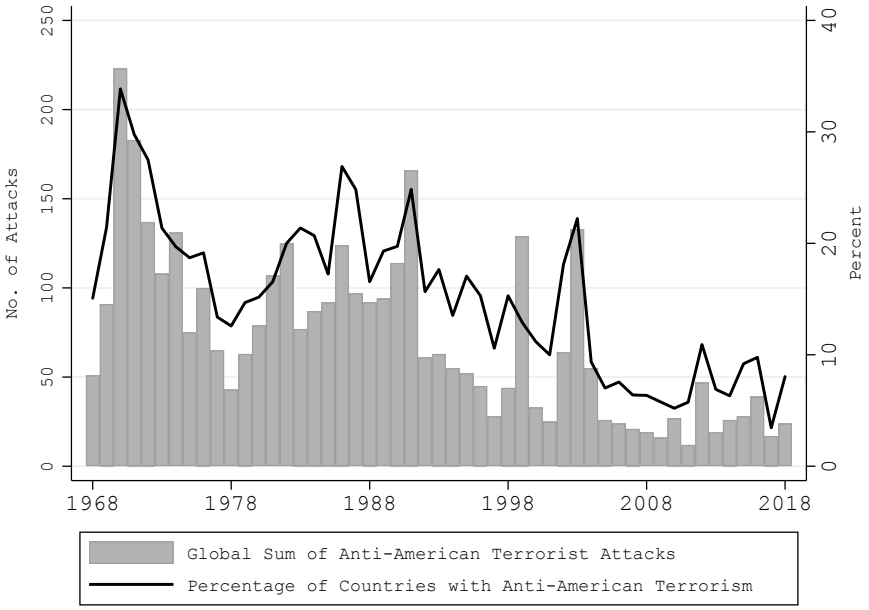
to a single attack, again making it difficult to correctly assign an attack (Abrahms and Conrad, 2017). Using the location definition, we therefore avoid under-counting anti-U.S. terrorism. As a robustness check, we use an alternative definition of anti-U.S. terrorism where attacks are assigned to the terrorists' country of origin (if known) regardless of the venue country. This latter definition of terrorism is known as the *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.

Figure 2: Anti-American Terrorism, 1968-2018



3.2. Measuring U.S. Military Aid

The data on U.S. military aid are drawn from the *United States Agency for International Development* (USAID, 2019). 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

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programs (*milother*), the latter including aid for counter-narcotics initiatives and peace-keeping, among others (USAID, 2019).[12] 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:

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

Aid is measured in 100 million constant (inflation-adjusted) US$. We apply the inverse hyperbolic sine transformation to the aid variable to accommodate for the influence of outliers; importantly, and in contrast to the log transformation, the inverse hyperbolic sine transformation is also defined for country–year observations that see no military aid provision (e.g., Burbidge et al., 1988). As part of our robustness checks below, we also use alternative operationalizations of U.S. military aid.

Figure 3 illustrates the global patterns of official 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 in a given year: 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.

[12]Note that the foreign military aid under consideration does not include financial support of foreign governments and military forces by U.S. intelligence agencies. Because of their clandestine nature, we cannot consider such aid flows in our paper. However, it is reasonable to assume that secret and official foreign aid are positively correlated for the majority of recipient countries. Anecdotal evidence of flows of (CIA) 'ghost money' lends some support to this assumption (for the case of Afghanistan, see, e.g., Rosenberg, 2013). Thus, the aid figures we use can be seen as a conservative estimate of the 'true' amount of U.S. involvement, also leading us to provide a rather conservative estimate of the impact of this type of aid on anti-American terrorism below.

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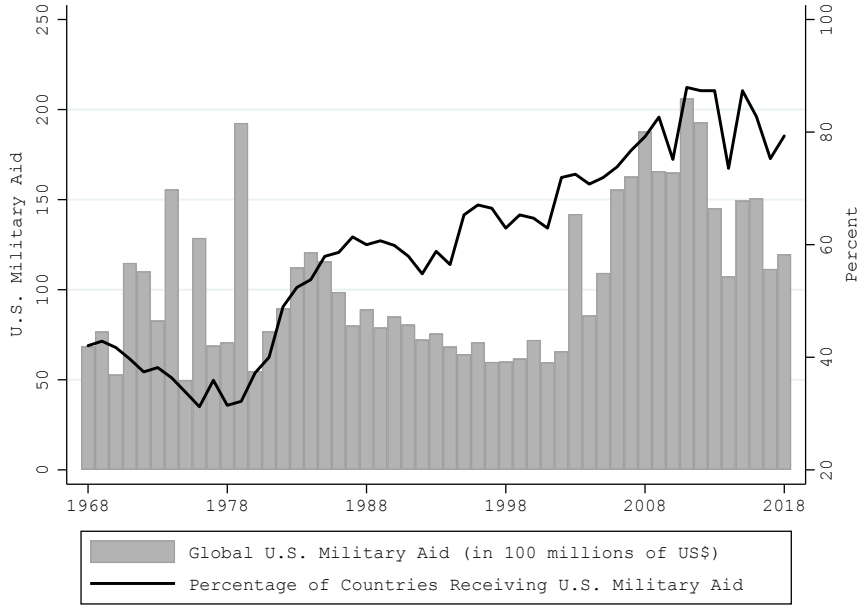
Figure 3: U.S. Military Aid, 1968-2018



4. OLS Estimates

4.1. Empirical Model

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

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

In Equation (2), *terror* is the binary measure of anti-American terrorism in country i and year t. Our independent variable of interest is U.S. military aid (*aid*). 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. The inclusion of time controls allows us to consider the influence of the Cold War and the War on Terror, which have been found to be relevant to both the patterns of anti-American terrorism and U.S. military aid (e.g., Fleck and Kilby, 2010; Gaibulloev and Sandler, 2019b).

The vector X accounts for time-varying covariates and a set of further fixed effects and trends. Concerning the latter, a first concern is that the local distribution of U.S. military aid may be correlated with U.S.-specific conditions. That is, U.S. economic and political cycles may differently affect the aid-terrorism nexus depending on the local distribution

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of military aid. For instance, Republican presidents may favor the provision of financial military aid, while Democratic presidents may prefer different types of aid. This, in turn, implies potentially different patterns of U.S. military aid and anti-American terrorism depending on which political party controls the White House. To account for such factors, we amend our model with (1) three interaction terms, where Democratic incumbency is interacted with the mean-probability of a country to receive financial, education or other types of military aid, respectively, and (2) three interactions terms, where the U.S. economic growth rate is interacted with the same mean-probabilities.[13] Data on U.S. economic growth comes from the *World Development Indicators* (*WDI*) of the World Bank (2019).

Similar to Nunn and Qian (2014), we also acknowledge that recipients of U.S. military aid tend to be more likely to (1) receive U.S. economic aid and (2) house U.S. troops.[14] Following Nunn and Qian (2014), to address the concern that such correlations may matter to our estimates, the vector X includes interactions of the year-fixed effects with the (1) average number of U.S. troops stationed in the country of interest and (2) the average level of U.S. economic aid the country receives over the observation period. Data on U.S. troops comes from an update of Kane (2012), while data on economic aid is from USAID (2019).

Finally, the vector X also includes four time-varying controls. First, we control *civil conflict*. This variable is equal to unity when there is an incidence of large-scale domestic conflict (i.e., civil and ethnic warfare); it is drawn from the *Major Episodes of Political Violence Dataset* (Marshall, 2020). It is plausible that both the emergence of terrorism and provision of aid may be driven by the presence of larger-scale conflict. Second, we control for *infant mortality* as a measure of economic development.[15] The data come from the WDI. Third, we control for a country's level of political development, using an index of *electoral democracy* from the *Varieties of Democracy Dataset* (*VDEM)* of Coppedge et al. (2021). Higher values of this index indicate that a country is more democratic (accounting for the extent of suffrage, freedom of association, freedom of expression, electoral competition etc.). Both economic and political development may be important confounders, given that

[13]Note that the direct (uninteracted) effects associated with these interactions are captured by the country- and year-fixed effects.

[14]However, U.S. military aid does not include payments for U.S. troops in aid-receiving countries, meaning that there is no danger of double-counting.

[15]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.

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both the anti-American terrorism and the provision of U.S. military aid may be affected by these conditions. Finally, to consider scale effects in both terrorism and aid provision, we also control for (logged) *population size* (WDI data).

4.2. Regression Results

Our OLS regression results are reported in Table 1. In short, regardless of which specification we run, the OLS-results suggest that higher levels of U.S. military aid are associated with stronger anti-American terrorist activity. This lends support for *H2* but not *H1*. Considering economic significance, using the baseline specification (specification (4) of Table 1), we find that a ten-percent increase in U.S. military aid increases the likelihood of the aid-receiving country to generate at least one incident of anti-American terrorism by 1.15 percentage points. 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 11.5 percentage point increase in the incidence of anti-American terrorism, which is approximately 73% of the mean incidence of anti-American terrorism.

The results for the controls are as expected. First, civil conflict is associated with more anti-American terrorism. This is in line with evidence that large-scale political instability facilitates terrorism, e.g., by undermining counter-terrorism effectiveness (Gaibulloev and Sandler, 2019b). Second, higher levels of economic development (i.e., lower child mortality) correlate positively with anti-American terrorism. This may be due to the fact that higher levels of wealth are likely to multiply the availability of American targets by attracting American business and tourism. Third, there is no obvious relationship between democracy and anti-American terrorism. Finally, anti-American terrorism is more likely in more populous countries, e.g. due to scale effects (e.g., Gaibulloev and Sandler, 2019b).

4.3. Selection on Observables and Bias from Unobservables

Even though we control for a number of fixed effects, U.S.-specific trends and other confounders, our OLS-estimates may still be biased by unobservable factors that correlate with a country's selection into receiving military aid and subsequent production of anti-American terrorism. In other words, the correlation between U.S. military aid and anti-American terrorism may be due to omitted variables, which, in turn, would threaten a causal interpretation of the results in Table 1.

To assess the impact of unobservables, we use the method developed by Oster (2019). Following her approach, we evaluate the sensitivity of our OLS-estimates to the inclusion of

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Table 1: **OLS Regression Results**

	(1)	(2)	(3)	(4)
U.S. Military Aid	0.117***	0.115***	0.111***	0.115***
	(0.030)	(0.035)	(0.031)	(0.035)
Civil Conflict			0.123***	0.123***
			(0.027)	(0.026)
Child Mortality			-0.001***	-0.001***
			(0.000)	(0.000)
Democracy			0.059	0.062
			(0.053)	(0.052)
Population Size			0.102***	0.098***
			(0.035)	(0.032)
Country-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Average U.S. Troops * Year-FE	No	Yes	No	Yes
Average U.S. Economic Aid * Year-FE	No	Yes	No	Yes
U.S. Democratic President * p(finance)	No	Yes	No	Yes
U.S. Democratic President * p(education)	No	Yes	No	Yes
U.S. Democratic President * p(other)	No	Yes	No	Yes
U.S. Economic Growth Rate * p(finance)	No	Yes	No	Yes
U.S. Economic Growth Rate * p(education)	No	Yes	No	Yes
U.S. Economic Growth Rate * p(other)	No	Yes	No	Yes
Observations	8,001	7,942	7,381	7,373

Notes: OLS-estimates reported. FE=Fixed effects. p(finance), p(education) and p(other)=Mean-probability to receive any financial, education or other U.S. military aid. Results for other covariates in first-stage regression not reported. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

observable control variables, using the associated regression results to derive bounds for the possible bias arising from unobservable variables. We can compare the change in the R^2 from the case without controls to the case with controls; if the R^2 increases due to the inclusion of controls, but the coefficient of interest (in our case, the regression coefficient associated with U.S. military aid) is not affected, it is expected that the inclusion of unobservables would have a similarly negligible effect. We can formally evaluate this expectation by making assumptions about (1) R_{max}, which is equal to the maximum value of the R^2 if all potential determinants (observables and unobservables) would be included, and (2) δ, which refers to the degree of selection on observed and unobserved variables. Based on these assumptions, we can estimate bias-corrected regression coefficients associated with U.S. military aid (Oster, 2019).

Our results are reported in Table 2. They can be summarized as follows. First, while the

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inclusion of control variables (such as fixed effects, trends and time-varying confounders) substantially increases the R^2, this has little effect on the coefficient associated with U.S. military aid. Second, working with the assumption proposed by Oster (2019) that R_{max} ought to be 1.3 times larger than the R^2 from a regression with controls to confidentially assess the role of selection, we still find that the bias-adjusted regression coefficient associated with military aid is largely unchanged. Third, we have to assume equal selection (i.e. $\delta = 1$) and an R_{max} that is highly implausible for a linear probability model (i.e., $R_{max} > 0.65$) to obtain a bias-adjusted coefficient that is smaller than zero. In sum, the results in Table 2 indicate that a common source of endogeneity—selection/omitted variable bias—does not matter strongly to our OLS-estimates.

Table 2: **Selection on Observables**

	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(7a)
Panel A: With Only Country- and Year-Fixed Effects as Controls							
Value of δ	Baseline Effect [R^2]	Controlled Effect [R^2]	Bias-adjusted β R_{max}=1.3*R^2	Bias-adjusted β R_{max}=0.25	Bias-adjusted β R_{max}=0.35	Bias-adjusted β R_{max}=0.5	Bias-adjusted β R_{max}=0.7
δ=0.5	0.116 [0.02]	0.117 [0.07]	0.119	0.120	0.122	0.125	0.128
δ=1	0.116 [0.02]	0.117 [0.07]	0.120	0.123	0.128	0.133	0.141
Panel B: With Baseline Controls, Trends and Fixed Effects							
	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)	(7b)
Value of δ	Baseline Effect [R^2]	Controlled Effect [R^2]	Bias-adjusted β R_{max}=1.3*R^2	Bias-adjusted β R_{max}=0.25	Bias-adjusted β R_{max}=0.35	Bias-adjusted β R_{max}=0.5	Bias-adjusted β R_{max}=0.7
δ=0.5	0.122 [0.02]	0.115 [0.13]	0.114	0.110	0.105	0.094	0.072
δ=1	0.122 [0.02]	0.115 [0.13]	0.112	0.104	0.088	0.037	-0.550

Notes: *Baseline effect* refers to the OLS-estimate of U.S. military aid on anti-American terrorism without any controls. *Controlled effect* refers to the OLS-estimate of U.S. military aid on anti-American terrorism with controls as indicated. Columns (3) through (7) show the bias-adjusted β-coefficient associated with the effect of U.S. military aid on anti-American terrorism under various assumptions for R_{max} (i.e., an R^2 from a hypothetical regression of the outcome on the treatment and both observed and unobserved controls). The term δ refers the relative degree of selection on observed and unobserved variables, with δ=1 implying equal selection.

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5. Instrumental-Variable Estimates

5.1. Empirical Model

In addition to omitted variable bias, *endogeneity* concerns associated with estimating Equation (2) may also arise due to measurement error and reverse causality. Concerning the former, given that the military aid data comes from official U.S. budgetary data, measurement error in the main explanatory variable seems unlikely. However, the issue of endogeneity due to reverse causation remains. 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. To address these concerns, we estimate the following *two-stage instrumental-variable system*:

$$aid_{it} = \beta_1 \times globalaid_{it} + \mu_1 \times X_{it} + \alpha_{1,i} + \tau_{1,t} + \epsilon_{1,it} \tag{3a}$$

$$terror_{it} = \beta_2 \times \widehat{aid}_{it} + \mu_2 \times X_{it} + \alpha_{2,i} + \tau_{2,t} + \epsilon_{2,it} \tag{3b}$$

In Equation (3a), we predict the potentially endogenous variable *aid* using our instrumental variable, *globalaid*, the baseline controls, trends and fixed-effects. We use these predicted values of *aid* to explain anti-U.S. terrorism in Equation (3b). As above, we apply the inverse hyperbolic sine transformation to both *aid* and *globalaid* to account for skewness.

Instrument Construction. Our instrumental variable (IV), *globalaid*, is the weighted sum of U.S. military aid to other parts of the world. It is constructed as follows:

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.

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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 its neighboring countries (subscript n). 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.

4. For each aid program, we multiply each share from Step 1 by the corresponding global total that is corrected for neighborhood effects from Step 3. We weigh by local aid shares to make sure that especially those trends in global military aid matter to our instrument that are also relevant to local aid receipts.[16]

5. We aggregate all products from Step 4 into one variable that is our IV *globalaid*. This instrument varies over time and is distinct for each country in the sample.

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

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

In Figure 4, we provide an example of the construction of our instrument. In 2002, Turkey received approximately 100 million US$, where approximately 67% went to military financing, 4% to military education and the remaining 29% to other aid programs. In the same year, the United States provided approximately 760 million US$ in military financing aid to countries outside of the Middle East and Northern Africa (which includes Turkey), 76 million US$ in military education aid and almost 800 million US$ to other aid programs. Before applying the inverse hyperbolic sine transformation, the value of our instrument for local U.S. military aid for Turkey in 2002 is thus given by:

$$\underbrace{(0.67 \times 759.9)}_{\text{Military Financing}} + \underbrace{(0.039 \times 75.8)}_{\text{Military Education}} + \underbrace{(0.291 \times 797.28)}_{\text{Other Aid}} \approx 730.62 \quad (5)$$

[16] Below, we also construct alternative instruments that forego the use of the local aid share weights.

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Figure 4: Example of Instrument Construction

Instrument Relevance. Concerning 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. For instance, geo-strategic considerations within the United States are expected to determine the global level and distribution of military aid, meaning that, 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 and statistically significant 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 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 bud-

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getary 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 recipients 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. For instance, 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 United States (e.g., Irwin, 2000; Newhouse, 2009).[17]

Several concerns can be raised regarding our IV-strategy. 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 which potentially affect 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*

[17]The complexities of the U.S. congressional budget process add to the difficulties for foreign governments to exert influence on U.S. policy-makers. Because of the low popularity of foreign policy issues among constituents, foreign and military aid appropriations are typically folded into omnibus, multipurpose spending bills in Congress (Obey and Lancaster, 1988; Morgenstern, 2019; Lawson and Morgenstern, 2020), which is why its members have to balance various issues of—mainly—domestic policy simultaneously (Irwin, 2000).

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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.

5.2. IV-Regression Results

We report our IV-estimates in Table 3. In line with the OLS-estimates in Table 1, we find that higher levels of U.S. military aid result in a greater likelihood of anti-U.S. terrorism. The inclusion of additional trends and controls does not meaningfully influence the size or direction of the effect of aid on anti-U.S. terrorism, where the results for the confounders also mirror our OLS-results. In sum, these findings again speak to *H2* but not *H1*.

Compared to the OLS-results, the effect of military aid on anti-American terrorism is modestly larger. This may suggest that the OLS-estimates are downward biased and underestimate the role of U.S. military aid in 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 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.

With respect to the economic significance of our findings, using the baseline specification (specification (4) of Table 3), we find that a ten-percent increase in U.S. military aid increases the likelihood of the aid-receiving country to generate at least one incident of anti-American terrorism by 1.29 percentage points. 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 12.9 percentage point increase in the incidence of anti-American terrorism, which is approximately 82.5% of the mean incidence of anti-American terrorism.

Finally, we assess the trustworthiness of our IV-estimates by considering a number of diagnostic checks. Here, 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

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Table 3: **Instrumental-Variable Regression Results**

	(1)	(2)	(3)	(4)
U.S. Military Aid	0.139***	0.127***	0.142***	0.129***
	(0.039)	(0.041)	(0.043)	(0.044)
Civil Conflict			0.121***	0.123***
			(0.026)	(0.026)
Child Mortality			-0.001***	-0.001***
			(0.000)	(0.000)
Democracy			0.056	0.060
			(0.054)	(0.052)
Population Size			0.094**	0.096***
			(0.037)	(0.033)
First-Stage Regression Result				
Weighted U.S. Military Aid to Rest of World	0.086***	0.083***	0.084***	0.080***
	(0.016)	(0.014)	(0.016)	(0.014)
Civil Conflict			0.082*	0.053
			(0.049)	(0.039)
Child Mortality			-0.001	0.001
			(0.001)	(0.001)
Democracy			0.006	0.022
			(0.156)	(0.152)
Population Size			0.273**	0.200**
			(0.124)	(0.089)
Country-FE	Yes	Yes	Yes	Yes
Year-FE	Yes	Yes	Yes	Yes
Average U.S. Troops * Year-FE	No	Yes	No	Yes
Average U.S. Economic Aid * Year-FE	No	Yes	No	Yes
U.S. Democratic President * p(finance)	No	Yes	No	Yes
U.S. Democratic President * p(education)	No	Yes	No	Yes
U.S. Democratic President * p(other)	No	Yes	No	Yes
U.S. Economic Growth Rate * p(finance)	No	Yes	No	Yes
U.S. Economic Growth Rate * p(education)	No	Yes	No	Yes
U.S. Economic Growth Rate * p(other)	No	Yes	No	Yes
Observations	8,001	7,942	7,381	7,373
First-Stage F-Stat.	29.37	35.76	26.31	30.70
AR Test p-value	(0.00)***	(0.00)***	(0.00)***	(0.00)***
AR 95% Confidence Interval	[0.062; 0.228]	[0.047; 0.218]	[0.062; 0.251]	[0.046; 0.232]

Notes: IV-estimates reported. AR=Anderson-Rubin. FE=Fixed effects. p(finance), p(education) and p(other)=Mean-probability to receive any financial, education or other U.S. military aid. Results for other covariates in first-stage regression not reported. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

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 (Stock et al., 2002). Reassuringly, these diagnostics are also satisfactory. For one, 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

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endogenous regressor in the structural equation is not equal to zero. For another, the constructed Anderson-Rubin confidence intervals are similar to the standard IV-intervals, firmly indicating statistical significance at conventional levels.

5.3. Robustness of Instrumental-Variable Approach

In this sub-section, we assess the robustness of our IV-approach—and thus the causal claims associated with our regression results—in various ways, probing the instrument relevance and exogeneity assumptions. To conserve space, we only briefly discuss these robustness checks below but present them in more detail in Appendix B.

First, we consider *alternative constructions of our instrument*. For instance, we use different world regions to account for neighborhood effects or do not use the specific shares of U.S. military aid the country of interest receives as weights when constructing the instrument. We discuss these alternative instruments in-depth in Appendix B.1. As shown in Table B.1, using these alternative instruments yields results that are similar to our baseline ones, where higher levels of U.S. military aid lead to more anti-American terrorism.

Second, we employ a variety of *placebo instruments*. This approach is inspired by Christian and Barrett (2017). We discuss the construction of these placebo instruments in more detail in Appendix B.1. To give an example, we randomly assign the values of the instrument to another country for a specific year. Due to randomization, the instrument should consequently no longer be helpful in identifying the impact of U.S. military aid on anti-American terrorism. By contrast, the finding that the association between global and local U.S. military aid survives the randomization may indicate that this association is spurious, e.g., driven by (non-linear) background trends (e.g., Christian and Barrett, 2017). Indeed, as shown in Table B.1, the placebo instruments are unable to identify the effect of military aid on anti-American terrorism, where the associated IV-diagnostics point to instrument weakness. This raises confidence that our initial identification strategy is sound and that previously reported estimates of U.S. military aid on terrorism are not spurious.

Third, as discussed above, an important 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 anti-American terrorism in the country of interest. For instance, the flaring up of 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). At the same time, this anti-Americanism may also motivate anti-American terrorism in the country of interest, e.g.,

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by means of migrant diasporas. To account for such factors, we run additional robustness checks where we control for a variety of regionally correlated shocks such as regional levels of economic growth. Their construction is discussed in Appendix B.2. As shown in Table B.2, we do not find that controlling for such trends changes our main empirical conclusions, which further raises confidence in the validity of our IV-approach.

Finally, we also probe the assumption of instrument exogeneity by means of the *plausibly exogenous method* of Conley et al. (2012) and further developed by van Kippersluis and Rietveld (2018). This method relaxes the assumption of perfect instrument exogeneity, instead allowing for violations of the exclusion restriction. These violations, in turn, are derived from the data. We discuss the methodology and its application to our case in much more detail in Appendix B.3. Employing the plausible exogenous method (Table B.3), we find that even after allowing for plausible amounts of instrument endogeneity, there is still robust support for our main empirical conclusion that the provision of U.S. military aid results in more anti-American terrorism.

5.4. Further Robustness Checks

We also probe the robustness of our empirical results in additional ways. For the sake of brevity, we only briefly discuss these robustness checks below but present them in greater detail in Appendix C.

As a first set of robustness checks, we examine how various empirical choices (the measurement of baseline controls, the selection of standard errors, the inclusion of further controls etc.) affect our results. First, we study how changes to our baseline model affect our estimates. For instance, we replace our democracy indicator with an alternative democracy measure from a different data source. Here, we also study whether different standard error choices affect our results concerning statistical significance. The various changes to our baseline model are discussed in Appendix C.1. Second, we add further time-varying controls and U.S.-specific trends to our main specification. For instance, we also consider the roles of globalization and international conflict in U.S. military aid and anti-American terrorism. More information on the additional controls is given in Appendix C.2. Third, we consider the influence of specific foreign policy variables on our results. For instance, such policy variables include the deployment of U.S. troops, the provision of U.S. economic aid or the voting behavior at the United Nations. More information on these policy variables is given in Appendix C.3. As reported in Table C.1 (changes to baseline model), Table C.2 (additional covariates) and Table C.3 (foreign policy variables), the various alternative

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empirical choices do not matter to our main empirical finding that more U.S. military aid induces more anti-American terrorism.

Furthermore, our results may be driven by our measurement of anti-American terrorism or U.S. military aid. To investigate this possibility, we first use alternative measurements of anti-American terrorism (e.g., in absolute terms), employ the origin rather than location definition of transnational terrorism and draw data from the *Global Terrorism Database* (LaFree and Dugan, 2007) as an alternative data source. Second, we employ different measurements of U.S. military aid (e.g., in per capita terms); here, we also consider the extensive margins by comparing country-year observations that receive any U.S. military aid to those that do not receive it. Further discussion of the alternative measurements of terrorism and aid is given in Appendix C.4 and Appendix C.5, respectively. As shown in Table C.4 (measurement of terrorism) and Table C.5 (operationalization of military aid), there remains a terror-inducing effect of U.S. military aid on anti-American terrorism regardless of how we operationalize aid or terrorism.

It is also possible that specific sub-samples drive our main results. To study this possibility we sequentially drop from our sample sub-sets of countries that share certain economic, political or geographical characteristics. For instance, we run a specification where we drop all countries located in the Middle East and Northern Africa. We further discuss the various sub-samples in Appendix C.6. Moreover, there were two important international events that may have affected both U.S. military aid and anti-American terrorism: the end of the Cold War and the beginning of the War on Terror (e.g. Fleck and Kilby, 2010). We therefore also study whether there are systematic difference in the nexus between U.S. military aid and anti-American terrorism before and after these events. Finally, we also consider whether potentially influential country-cases (e.g., Israel, Iraq, Afghanistan or Pakistan) affect our estimates by also sequentially dropping them from our sample. As shown in Table C.6, we do not find that specific sub-samples of countries or country-cases drive our results or that the Cold War or War on Terror were influential.

Finally, the use of a linear probability model to study a binary outcome can be criticized because, e.g., predicted probabilities may fall outside the range between zero and one (e.g., Hellevik, 2009). Thus, we also employ non-linear models (logit and probit models) and their instrumental-variable counterparts as another robustness check. These non-linear models are discussed in more detail in Appendix C.7. As shown in Appendix C.7, using non-linear (instrumental-variable) models still indicates that more U.S. military aid leads to more anti-American terrorism.

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5.5. Effects of Military Aid on Terrorism Against Other Nations & Domestic Terrorism

Finally, we study how U.S. military aid affects transnational terrorism directed against other nations. For one, this includes U.S. allies such as the United Kingdom, France and Israel. These countries also saw considerable transnational terrorist activity directed against their interests during our observation period. For instance, out of the approximately 15,000 transnational terrorist incidents recorded in *ITERATE* between 1968 and 2018, approximately 1,100 (7.5%) were directed against British targets.[18] 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 example, resentment against the U.S. may also translate into more general anti-Western resentment and thus lead to terrorism against Western U.S. allies.

For another, we also consider the effect of U.S. military aid on countries whose politico-military relationship to the United States was more ambiguous (Pakistan and Turkey) or outright hostile (Iran and the USSR/Russia) during most of the observation period. 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 create seven additional dependent variables from the *ITERATE* dataset that are equal to unity when a country-year pair sees at least one transnational terrorist attack against the United Kingdom, France, Israel, Pakistan, Turkey, Iran or the USSR (Russia after 1992), respectively. Furthermore, we create two additional dependent variables from the *GTD* dataset that are equal to unity when a country-year pair sees at least one terrorist attack (by domestic, transnational or unknown actors against domestic or transnational targets) or at least one domestic terrorist attack (i.e., a terrorist attack by domestic actors against domestic targets). This is to assess whether aid produces destabilizing effects beyond anti-American violence, e.g., as proposed by Boutton (2021). For instance, U.S. aid may produce grievances (e.g., corruption) that may not only lead to anti-American violence, but also terrorism that is directed against the local government itself (Boutton, 2021).

Employing our usual IV-approach, we report our findings concerning the effect of U.S. military aid on the different types of terrorism in Table 4.[19] Our results can be summa-

[18] Still, these countries are far less often attacked than the United States. Anti-U.S. terrorism accounts for approximately 27% of all incidents recorded during our observation period.

[19] Note that for this analysis, we always drop the country that is the respective target of transnational

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rized as follows. First, there is some evidence that U.S. military aid also affects attacks against British and French (but not Israeli) targets. This may speak to the notion of spillover effects, where U.S. involvement creates broader anti-Western sentiment. At the same time, these spillover effects are smaller than the effect of U.S. military aid on anti-U.S. terrorism. Second, there is no evidence that U.S. military aid affects the patterns of transnational terrorism directed against other countries. Particularly with respect to terrorism against Iranian and Soviet/Russian 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. provides further evidence that our identification strategy is indeed valid. Finally, there is also no evidence that U.S. military aid shares a generalized relationship with (domestic) terrorism. This speaks to the idea that U.S. aid-giving produces a nuanced response (anti-American terrorism) but does not contribute to an overall escalation of violence.

Table 4: **Effect of U.S. Military Aid on Other Types of Terrorism**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Target Country →	United Kingdom	France	Israel	Pakistan	Turkey	Iran	USSR/ Russia	Any Terrorism	Domestic Terrorism
U.S. Military Aid	0.045*	0.050**	0.024	0.008	0.020	0.020	-0.025	0.027	-0.028
	(0.024)	(0.023)	(0.019)	(0.007)	(0.016)	(0.014)	(0.016)	(0.057)	(0.055)
Civil Conflict	0.058***	0.059***	0.009	0.002	0.027**	0.017	0.064***	0.245***	0.228***
	(0.019)	(0.018)	(0.009)	(0.003)	(0.012)	(0.011)	(0.018)	(0.032)	(0.034)
Child Mortality	-0.000*	-0.000**	-0.000	-0.000	-0.000	0.000	0.000	-0.002***	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Democracy	0.069**	0.036	0.062***	0.002	0.067***	0.039**	0.044**	0.032	0.024
	(0.033)	(0.025)	(0.024)	(0.005)	(0.021)	(0.015)	(0.018)	(0.077)	(0.076)
Population Size	0.026	0.001	0.052***	-0.005	0.042***	0.015	0.036**	0.074	0.052
	(0.020)	(0.017)	(0.020)	(0.006)	(0.015)	(0.014)	(0.014)	(0.076)	(0.060)
Observations	7,322	7,322	7,322	7,322	7,322	7,325	7,347	7,165	6,837
First-Stage F-Stat.	31.25	30.72	30.08	28.52	28.84	30.71	29.92	31.79	31.82
AR Test p-value	(0.06)*	(0.03)**	(0.21)	(0.26)	(0.23)	(0.19)	(0.13)	(0.64)	(0.62)

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

terrorism from our sample. For instance, when we study anti-Israeli transnational terrorism, we do not include data on terrorism in Israel (e.g., carried out by Palestinian groups).

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6. Exploration of Transmission Channels

6.1. Model and Variables

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 provided two arguments explaining this effect. First, according to the strategic logic argument of Neumayer and Plümper (2011), military aid promotes 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. Second, in line with our grievances argument, U.S. military aid encourages anti-U.S. terrorism by those that lose out on direct and indirect access to rents or are otherwise adversely affected by institutions weakened by aid.

In this section, we explore whether U.S. military aid indeed affects state capacity (in line with the strategic logic argument) and/or grievances (in line with our grievances argument). Here, 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, trends 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* accounting for total local military spending and 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, only available for the 1968-2012 period).Furthermore, from the *VDEM Dataset* we extract a variable measuring a *state's control over its territory*. This control might be challenged by criminals, warlords or other sub-national actors. To indicate administrative capacity we draw a variable from the *VDEM Dataset* that indicates the *rigor and impartiality of public administration*. This variable reflects the quality of bureaucracy by evaluating the arbitrariness and biases in the administration of the law by public officials. Higher values of this measure indicate a sounder bureaucracy.

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Grievances Variables. We also employ three variables reflecting rent-seeking behavior and distributional conflict in aid-receiving countries. All variables come from the *VDEM Dataset*. First, we use an index reflecting the *provision of public goods*. It measures public access to basic health and education services, with higher values indicating a fairer distribution. Second, we measure the extent of *regime corruption* using an index of executive, judicial and legislative corruption. Higher values of this index mean higher levels of corruption. Both indicators describe the potential loss in institutional quality and the extent of rent-seeking induced by the inflow of U.S. military aid. Finally, we consider the extent of *clientelism* within a country. Higher values of the associated index mean that clientelism is more common. 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.

6.2. Empirical Results

We report empirical results concerning the effect of military aid on the various mediators in Table 5 (Panel A). In Table 5 (Panel B), we report the correlation between the mediator and the likelihood of anti-U.S. terrorism to make a clearer case for mediation (Panel B).

Concerning the state capacity variables, we find evidence that higher levels of military capacity, stronger territorial control and a sounder bureaucracy share a negative association with anti-American terrorism (Panel B). However, at the same time, we find no evidence of a causal effect of U.S. military aid on these mediators (Panel A). Thus, while state capacity—as expected—deters anti-American terrorism, we find no 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 the consequence of the very beneficial effect of U.S. military aid on local state capacity.

By contrast, we find that more U.S. military aid adversely affects the distribution of public goods as well as the extent of corruption and clientelism (Panel A). Moreover, we also find that the unequal access to public goods as well as corruption and clientelism positively correlate with anti-American terrorism (Panel B). Consequently, this analysis provides evidence in favor of the grievances channel we proposed in this paper. That is, military aid induces rent-seeking behavior (as indicated by increases in corruption and clientelistic policies) and results in weaker institutions, corresponding to lower opportunity costs of terrorism for the 'losers' of these processes, encouraging anti-American resentment.

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Table 5: **Transmission Channels**

Panel A: IV-Estimates of Effect of U.S. Military Aid on Potential Mediators						
	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)
Dependent Variable →	Military Capacity	Territorial Control	Rigorous Public Administration	Public Goods Provision	Regime Corruption	Clientelism
U.S. Military Aid	-0.051	1.071	-0.059	-0.206**	0.069**	0.094***
	(0.057)	(1.172)	(0.142)	(0.100)	(0.029)	(0.032)
Civil Conflict	-0.026	-6.797***	-0.233***	-0.124**	0.006	-0.016
	(0.028)	(1.201)	(0.066)	(0.050)	(0.012)	(0.014)
Child Mortality	0.002***	-0.007	0.001	0.001	-0.001***	-0.000
	(0.001)	(0.012)	(0.001)	(0.001)	(0.000)	(0.000)
Democracy	-0.361***	2.359	2.963***	0.605***	-0.327***	-0.267***
	(0.097)	(1.489)	(0.261)	(0.193)	(0.041)	(0.049)
Population Size	-0.082	0.501	-0.138	0.512**	-0.048	-0.013
	(0.117)	(1.139)	(0.188)	(0.201)	(0.034)	(0.037)
Observations	5,909	7,371	7,373	7,373	7,373	7,373
First-Stage F-Stat.	31.25	30.70	30.70	30.70	30.70	30.70
AR Test p-value	(0.38)	(0.36)	(0.69)	(0.07)*	(0.02)**	(0.00)***
Panel B: Correlation between Potential Mediators and Anti-American Terrorism						
	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)
Mediator Variable →	Military Capacity	Territorial Control	Rigorous Public Administration	Public Goods Provision	Regime Corruption	Clientelism
Mediator	-0.072***	-0.008***	-0.023**	-0.037**	0.160**	0.193**
	(0.023)	(0.002)	(0.012)	(0.016)	(0.074)	(0.081)
U.S. Military Aid	0.113***	0.078***	0.123***	0.124***	0.127***	0.131***
	(0.028)	(0.029)	(0.027)	(0.027)	(0.027)	(0.027)
Civil Conflict	-0.000	-0.001***	-0.001**	-0.001**	-0.001*	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Child Mortality	0.075	0.092*	0.142**	0.095*	0.125**	0.123**
	(0.054)	(0.055)	(0.066)	(0.056)	(0.063)	(0.062)
Democracy	0.145***	0.125***	0.117***	0.137***	0.126***	0.119***
	(0.039)	(0.035)	(0.035)	(0.038)	(0.036)	(0.035)
Observations	5,909	7,371	7,373	7,373	7,373	7,373

Notes: IV-estimates (Panel A) and OLS-estimates (Panel B) reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

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7. Conclusion

We study the effect of U.S. military aid on the emergence of anti-American terrorism in recipient countries for a sample of 174 countries between 1968 and 2018 in both an OLS- and IV-setting. Our empirical analysis allows us to refute the argument—often voiced by U.S. policymakers—that U.S. military aid bolsters U.S. security. We find no evidence that more U.S. military aid makes the United States less vulnerable to terrorism. Instead, we show that higher levels of military aid cause higher levels of anti-American terrorist activity in recipient countries. This latter finding survives a battery of robustness checks.

Further exploring potential transmission channels through which U.S. military aid promotes anti-American terrorism, we find that more U.S. military aid does not contribute to state capacity in recipient countries, further refuting the policy-maker argument. Instead, we show that more military aid leads to more economic-political exclusion and 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. Thus, military aid can backfire, and this finding calls into question its effectiveness as a counter-terrorism tool.

The results from our preferred instrumental-variable specification suggest that at the sample-mean, doubling U.S. military aid increases the risk of anti-American terrorism by 12.9 percentage points, which is approximately 82.5% of the sample-mean incidence of anti-American terrorism. This points to substantial and adverse security effects of U.S. military aid, especially under those circumstances when the U.S. hands out hundreds of millions or even billions of US$ in aid.

Clearly, the United States provides military aid for a number of reasons other than counter-terrorism, e.g., to secure strategic positions, earn political favors at international organizations or help obtain market access for the U.S. defense industry. 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 have extremely high economic and political costs for the United States. For instance, in addition to human casualties, the costs of repairing the *USS Cole* (targeted by anti-U.S. terrorism in Yemen in 2000) amounted to more than

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250 million US$.[20] 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.

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

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Appendix A. Summary Statistics and Sample

Table A.1: **Summary Statistics**

	N*T	Mean	Std. Dev.	Min	Max
Anti-American Terrorism (ITERATE, binary, location)	7,373	0.156	0.363	0	1
U.S. Military Aid	7,373	0.706	4.832	0	115.6
Civil Conflict	7,373	0.191	0.393	0	1
Child Mortality	7,373	73.59	73.04	2.2	387.3
Democracy	7,373	0.442	0.287	0.007	0.92
Population Size (logged)	7,373	15.96	1.555	11.69	21.05
Anti-British Terrorism	7,322	0.046	0.21	0	1
Anti-French Terrorism	7,322	0.036	0.187	0	1
Anti-Israeli Terrorism	7,322	0.028	0.165	0	1
Anti-Pakistani Terrorism	7,322	0.005	0.068	0	1
Anti-Turkish Terrorism	7,322	0.022	0.146	0	1
Anti-Iranian Terrorism	7,325	0.017	0.129	0	1
Anti-Russian Terrorism	7,347	0.024	0.153	0	1
Terrorism (GTD, binary, domestic and transnational)	7,165	0.512	0.50	0	1
Terrorism (GTD, binary, domestic)	6,837	0.37	0.483	0	1
Military Capacity	5,909	-0.044	0.904	-0.516	8.80
State Control over Territory	7,371	91.12	10.34	31.11	100
Impartial and Rigorous Public Administration	7,373	0.255	1.537	-3.752	4.006
Public Goods Provision					
Regime Corruption	7,373	0.498	0.31	0.002	0.98
Clientelism	7,373	0.496	0.265	0.018	0.98

Note: N*T for sample with baseline controls, trends and fixed effects; see Table 1, models (3) and (4). Terrorists attacks against non-Americans are always from ITERATE, binary and use the country of location definition.

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Table A.2: **List of Countries**

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

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Appendix B. Robustness of Instrumental-Variable Approach

Appendix B.1. Alternative Construction of Instrument and Placebo Instruments

Alternative Instruments. We consider two alternative ways to construct our instrumental variable. 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 to assess whether our main empirical findings are due to the idiosyncratic construction of our baseline instrument.

Second, we do not weigh our IV with the relative importance of the various military aid programs for the country of interest. That is, in this case the alternative instrument simply equals the sum of U.S. military aid to other parts of the world (i.e., to countries that are not in the neighborhood of 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 use an instrument variant that is inspired by Nunn and Qian (2014). Here, our alternative instrument is equal to the sum of U.S. military aid to other parts of the world, where this sum is interacted with the mean-probability to receive any military aid over the observation period. For instance, for a country that receives U.S. military aid in 20 out of 50 years for which it is observed, the annual sum of U.S. military aid to other parts of the world will always be multiplied by a factor of 2/5. That is, we exploit plausibly exogenous time variation in U.S. military aid to other parts of the world and cross-sectional variation in a country's likelihood of being a recipient of U.S. military aid. According to Nunn and Qian (2014, p.1632), this instrument thus follows the same logic as a difference-in-differences estimator.

Our findings using the alternative instruments are reported in Table B.1. We find that regardless of which instrument variant we consider, there is always a positive effect of U.S. military aid on anti-American terrorism. Indeed, the point estimates show that this effect is stronger when these instrument variants are employed, suggesting that our baseline results are rather conservative. What is more, the associated IV-diagnostics are usually sound. Using the instrument variant that follows Nunn and Qian (2014), the first-stage F-statistic is borderline weak. However, the associated Anderson-Rubin test result—which is robust to potentially weak instruments—confidentially indicates that the coefficient of

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the endogenous regressor in the structural equation equals zero, which, in turn, supports the IV-estimates.

Table B.1: **Alternative Construction of Instrument and Placebo Instruments**

	(1)	(2)	(3)	(4)	(5)	(6)
Instrument for Military Aid →	IV I	IV II	IV III	Placebo I	Placebo II	Placebo III
U.S. Military Aid	0.192***	0.208***	0.275***	0.291	1.196	1.341
	(0.052)	(0.035)	(0.090)	(0.590)	(0.965)	(1.815)
Civil Conflict	0.120***	0.119***	0.116***	0.115***	0.075	0.025
	(0.026)	(0.026)	(0.025)	(0.038)	(0.064)	(0.141)
Child Mortality	-0.001***	-0.001***	-0.001***	-0.001**	-0.001	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.002)
Democracy	0.053	0.052	0.045	0.043	-0.052	-0.209
	(0.054)	(0.056)	(0.062)	(0.090)	(0.209)	(0.435)
Population Size	0.084**	0.081***	0.068*	0.065	-0.106	-0.041
	(0.033)	(0.031)	(0.038)	(0.118)	(0.189)	(0.217)
Observations	7,373	7,373	7,373	7,373	7,373	6,758
First-Stage F-Stat.	28.26	15.81	8.671	2.177	2.034	0.425
AR Test p-value	(0.00)***	(0.00)***	(0.00)***	(0.62)	(0.06)*	(0.11)

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. IV I=Uses different world regions to account for neighborhood effects. IV II=Does not account for local shares of specific military aid types but only uses military aid to other parts of the world. IV III=Military aid to rest of world interacted with the mean-probability to receive any military aid. Placebo I=Randomly assigns values of IV to another country for specific year. Placebo II=Randomly re-assigns values of IV within the same country. Placebo III=Randomly assigns all values of IV for one country to another country. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Placebo Instruments. For all placebo instruments, the idea is to undo the spatial or temporal ties between the instrument and the instrumented variable. We argue that these ties are essential to the relevance and validity of our baseline instrumental-variable approach. For instance, global U.S. military aid ought to be predictive of local U.S. military aid in the same year of observation, but not predictive of local aid patterns in other years. Consequently, removing the spatial or temporal ties—by construction—should create placebo instruments that share no association with local U.S. military aid and should consequently neither be valid nor relevant for identification.

In detail, we create three placebo instruments. First, we randomly assign the values of our baseline instrument to other countries for a specific year. For instance, this means that the values of the instrument associated with France for 1970 could be assigned to

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Nigeria, Frances's IV values for 1971 to Bolivia, and so on. Second, we randomly scramble the values of the instrument within the same country. For example, this means assigning the values of the instrument associated with France for 1970 to the year 1985, its values for the year 2010 to 1992, and so on. Third, we randomly assign the values of our baseline instrument to another country in our sample. For instance, we assign the values of the instrument for France for the 1970–2018 period to Egypt, while assigning the values for Egypt to India, and so on.

We show our findings using the placebo instruments in Table B.1. As expected, we find that the placebo instruments are weak, with the associated first-stage F-statistics being clearly smaller than ten. What is more, the associated Anderson-Rubin test results also do not indicate instrument soundness. Regardless of which randomly constructed instrument we employ, they do not allow us to identify the effect of local U.S. military aid on anti-American terrorism. This finding speaks to the notion that our initial identification strategy is sound and that our initial results have not been spuriously driven by background trends (Christian and Barrett, 2017).

Appendix B.2. Regional Trends

The validity of our instrumental-variable approach rests on the assumption that U.S. military aid to other parts of the world only matters to local anti-American terrorism by affecting the local provision of U.S. military aid. However, there may exist economic and political shocks that are correlated across regions. Such shocks could thus affect the construction of our instrumental variable. If these shocks also affect, at the same time, anti-American terrorism in the country of interest, this could violate the exclusion restriction.

Below, we control for several time-varying 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. In detail, we control for regional shocks in population size, civil conflict, economic and political development, economic growth, natural resource wealth, anti-American terrorism and political proximity to the United States.

With respect to those variables not already introduced in the main text, natural resource wealth refers to the sum of oil rents, natural gas rents, coal rents, mineral rents and forest rents as a share of GDP (WDI data). Data on the per capita rate of economic growth is also from the WDI. Finally, political proximity to the United States is operationalized as the difference between a sample country's ideal policy position and that of the United

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States, where this policy position follows from the country's voting behavior at the *United Nations General Assembly*. This voting data is from Bailey et al. (2018).

Formally, the shock variables are defined as the mean levels of (logged) population size, civil conflict incidence, child mortality, democracy, economic growth, natural resource wealth, anti-American terrorism (measured by the number of anti-American terrorist incidents) and the ideal policy position of countries located outside of the neighborhood of a country of interest. That is, they are constructed in the same way as our instrumental variable. Furthermore, we run a specification that includes regional trends (i.e., year-fixed effects interacted with a set of 18 regional dummy variables for the Caribbean, Central Asia, Eastern Africa etc.) as another way to account for any spurious correlation between both the instrument and anti-American terrorism.

Our results are reported in Table B.2. We find no evidence that specific regional shocks or trends affect our main empirical finding of a positive (causal) effect of U.S. military aid on anti-American terrorism in the aid-receiving countries. The associated IV-diagnostics and findings for the various controls are also stable and sound across the various specifications.

Appendix B.3. Plausibly Exogenous Framework

The main idea of the plausibly exogenous method of Conley et al. (2012) and further developed by van Kippersluis and Rietveld (2018) is to relax the assumption of perfect instrument exogeneity. It involves the following 2SLS model:

$$aid_{it} = \beta_1 \times globalaid_{it} + \mu_1 \times X_{it} + \alpha_{1,i} + \tau_{1,t} + \epsilon_{1,it} \tag{B.1}$$

$$terror_{it} = \beta_2 \times \widehat{aid}_{it} + \gamma \times globalaid_{it} + \mu_2 \times X_{it} + \alpha_{2,i} + \tau_{2,t} + \epsilon_{2,it} \tag{B.2}$$

In contrast to our main IV-approach, here we allow our instrument to enter the second-stage regression with a coefficient γ. That is, the level of U.S. military aid to other parts of the world (weighted by local aid shares) can now directly affect anti-American terrorism in the country of interest i, meaning that the exclusion restriction is violated. By contrast, in case of perfect instrument exogeneity, γ would be equal to zero and the exclusion restriction would hold.

By considering various values of γ, we can investigate how violations of the exclusion restriction matter to our IV-estimates (Conley et al., 2012). To determine reasonable values for γ, we would, ideally, consider the so-called 'zero-first-stage', i.e., a sub-sample of countries for which there is no effect of global on local military aid because there is no change in the local provision of military aid over time (van Kippersluis and Rietveld,

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2018); this would mean considering a set of countries that never receives U.S. military aid. Consequently, for this zero-first-stage case, in the reduced-form regression the estimated effect of global military aid on anti-American terrorism would be equal to γ. To understand this, note that the reduced-form relationship between global U.S. military aid and anti-American terrorism captures both γ and β_2, i.e., the effect of global military aid on local anti-American terrorism via local U.S. military aid. However, as there is no effect of global military aid on anti-American terrorism via local military aid in the zero-first-stage case (as these countries do not receive local aid), the associated reduced-form estimate only captures γ.

Unfortunately, for our sample there are only two country-cases (Hong Kong and Luxembourg) that never received military aid over the 1970-2018 period. Following van Kippersluis and Rietveld (2018), we thus construct three 'quasi zero-first-stage groups' of countries to approximate the zero-first-stage case. The first group considers the bottom 25% of countries with respect to the number of years during which they receive U.S. military aid. Countries that receive U.S. military aid for the fewest number of years include, e.g., Syria, North Korea and China. The second group of countries considers the bottom 25% with respect to the average level of U.S. military aid. For instance, countries that receive—on average—the smallest amount of aid per year include countries such as Bhutan, Cuba, Ireland and Switzerland. The third quasi zero-first-stage group consists of countries that are in both of the aforementioned zero-first-stage groups. We consider three zero-first-stage country samples to reduce concerns about spurious results due to idiosyncrasies related to the construction of a specific quasi zero-first-stage country group.

As shown in Table B.3, we always compare the reduced-form and first-stage regression results for the quasi zero-first-stage country groups with the remaining sample. We expect (1) that the effect of the instrument on anti-American terrorism (reduced-form estimates) ought to be weaker in the quasi zero-first-stage sample compared to the full sample and (2) that the effect of global aid on local U.S. military aid (first-stage estimates) ought to be similarly weaker. Also, the reduced-form estimates for the quasi zero-first-stage country groups give us values for γ that we can use to investigate how violations of the exclusion restriction affect our 2SLS estimates in the plausibly exogenous framework (Conley et al., 2012; van Kippersluis and Rietveld, 2018).

We report our empirical results in Table B.3. Concerning the reduced-form regressions that explore the effect of our instrument on anti-American terrorism (Panel A), we find that there is no statistically significant effect of global U.S. military aid on anti-American

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terrorism for the three quasi zero-first-stage country groups. We also find that the effect of the instrument on local U.S. military aid is much weaker for the quasi zero-first-stage countries (Panel B). In sum, these findings speak to the idea that the direct effect of global U.S. military aid on anti-American terrorism is negligible.

Indeed, if we use the reduced-from estimates (Panel A) to specify γ in the plausibly exogenous regressions, we find that the implied violations of the exclusion restrictions do not result in the inclusion of zero in the 95% confidence interval associated with β_2 (Panel C). Here, the confidence intervals associated with the plausibly exogenous approach are very similar to the baseline 2SLS regression intervals. In sum, the results of Table B.3 thus suggest that allowing for plausible amounts of instrument endogeneity still supports our main empirical conclusion that the provision of U.S. military aid results in more anti-American terrorism.

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Table B.2: **Role of Regional Shocks and Trends**

	(1)	(2)	(3)	(4)	(5)	(6)
U.S. Military Aid	0.119***	0.125***	0.122***	0.100**	0.096**	0.141***
	(0.043)	(0.044)	(0.045)	(0.043)	(0.045)	(0.043)
Civil Conflict	0.119***	0.127***	0.121***	0.119***	0.122***	0.119***
	(0.025)	(0.026)	(0.026)	(0.025)	(0.025)	(0.022)
Child Mortality	-0.000	-0.001**	-0.001***	-0.000	-0.000	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Democracy	0.110*	0.051	0.056	0.078	0.092	0.132**
	(0.063)	(0.052)	(0.053)	(0.049)	(0.062)	(0.059)
Population Size	0.079**	0.036	0.095***	0.083**	0.050	0.005
	(0.033)	(0.032)	(0.033)	(0.033)	(0.036)	(0.046)
Child Mortality (ROW)	0.012***				-0.000	
	(0.005)				(0.005)	
Democracy (ROW)	3.297**				1.411	
	(1.610)				(1.602)	
Population Size (ROW)		-1.342***			-0.726	
		(0.509)			(0.566)	
Civil Conflict (ROW)		1.130			0.906	
		(0.941)			(0.903)	
Economic Growth (ROW)			-0.033		-0.029	
			(0.027)		(0.027)	
Natural Resource Rents (ROW)			-0.005		-0.013	
			(0.022)		(0.022)	
Anti-American Terrorism (ROW)				-0.999***	-0.958***	
				(0.128)	(0.132)	
Political Proximity to U.S. (ROW)				-0.310	-0.247	
				(0.317)	(0.335)	
Observations	7,373	7,373	7,165	7,373	7,165	7,373
First-Stage F-Stat.	30.31	29.97	31.82	29.10	29.71	32.32
AR Test p-value	(0.00)***	(0.00)***	(0.00)***	(0.02)**	(0.04)**	(0.00)***
Regional Shocks	No	No	No	No	No	Yes

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. ROW=Rest of the world (i.e., mean of variable in countries outside of world region of country of interest). Regional shocks are created by interacting the year-fixed effects with the region dummy variables. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

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Table B.3: **Plausibly Exogenous Framework**

	(1)	(2)	(3)
Countries in quasi zero-first-stage group	37	37	15
(Number of Observations)	(N*T=1,774)	(N*T=1,715)	(N*T=709)
Countries in remaining sample	126	126	148
(Number of observations)	(N*T=5,599)	(N*T=5,658)	(N*T=6,664)
Panel A: Effect of Instrument on Anti-American Terrorism (Reduced Form)			
Full Sample	0.010***	0.010***	0.010***
	(0.003)	(0.003)	(0.003)
Quasi zero-first-stage group	0.002	-0.004	0.004
	(0.008)	(0.003)	(0.006)
Remaining sample	0.014***	0.013***	0.011***
	(0.004)	(0.004)	(0.004)
Panel B: Effect of Instrument on Local U.S. Military Aid (First Stage)			
Full Sample	0.080***	0.080***	0.080***
	(0.014)	(0.014)	(0.014)
First-Stage F-Stat.	30.70	30.70	30.70
AR p-value	(0.00)	(0.00)	(0.00)
Quasi zero-first-stage group	0.017**	0.002***	0.001*
	(0.007)	(0.001)	(0.001)
First-Stage F-Stat.	7.14	33.08	2.88
AR p-value	(0.80)	(0.20)	(0.65)
Remaining sample	0.089***	0.097***	0.086***
	(0.016)	(0.017)	(0.015)
First-Stage F-Stat.	33.01	31.22	31.21
AR p-value	(0.00)***	(0.00)***	(0.00)***
Panel C: Effect of U.S. Military Aid on Anti-American Terrorism (95% Confidence Interval)			
2SLS regression	[0.043; 0.216]	[0.043; 0.216]	[0.043; 0.216]
Plausibly exogenous regression	[0.047; 0.186]	[0.073; 0.240]	[0.025; 0.221]
Value of γ for plausibly exogenous regression	0.002	-0.004	0.004

Notes: OLS-estimates (Panels A and B) IV-estimates (Panel C) reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). *Full sample* refers to the baseline sample with 163 countries; these results are reported for comparison. Quasi zero-first-stage sample in column (1) are countries that rarely receive U.S. military aid (bottom 25%). Quasi zero-first-stage sample in column (2) are countries that receive relatively little U.S. military aid on average (bottom 25%). Quasi zero-first-stage sample in column (3) are countries included in both column (1) and (2). Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

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Appendix C. Further Robustness Checks

Appendix C.1. Changes to Baseline Model

We consider a number of modifications to our baseline model. First, we replace our baseline measurement of economic development, child mortality, with a country's per capita income. The GDP data comes from the WDI. Second, we replace our democracy index with an alternative democracy measure drawn from Krieger and Gründler (2016). Third, we replace the civil conflict indicator with a variable measuring the extent of state failure (i.e., incidences of revolutionary and ethnic warfare as well as coup d'etats) from the *Political Instability Taskforce* (PITF, 2019). Fourth, inspired by Enders et al. (2016) and Gaibulloev et al. (2017), we run two specifications that include quadratic terms for child mortality and democracy, respectively, to allow for non-linear effects of economic and political development on anti-American terrorism. Fifth, we run a specification that includes a lagged dependent variable to capture any dynamic effects. Sixth, in one specification we account for further U.S.-specific trends that may also incentivize specific forms and the volume of U.S. military aid. Here, we interact the respective mean-probability of a country to receive any financial, education or other military aid with annual realizations of U.S. arms exports, U.S. public debt (gross general government debt as a percentage of GDP) and U.S. electoral fractionalization. The former variable is from the WDI, while the latter two come from the *Comparative Political Data Set* of Armingeon et al. (2020). Finally, we employ two alternative standard errors. For one, we use Driscoll-Kraay standard errors that are robust to cross-sectional dependence (Driscoll and Kraay, 1998). 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). For another, we employ standard errors that are simultaneously clustered by country and the world region-year following Cameron et al. (2011), reflecting potential correlations at the level of the world region that could also matter to the construction of our instrumental variable.

We report our empirical results in Table C.1. Our findings can be summarized as follows. First, the use of alternative indicators for economic development, democracy and political instability is immaterial to our main finding of an adverse effect of U.S. military aid on anti-American terrorism. Second, while there is no evidence that economic development is non-linearly related to anti-American terrorism, we find that semi-democratic countries are

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most strongly affected. This latter finding is in line with Gaibulloev et al. (2017). However, this non-linear relationship does not matter to our main finding. Third, our main finding does not change when we account for a lagged dependent variable or further U.S.-specific trends. Finally, using alternative standard errors is also of little consequence to statistical precision. For all specifications, the associated IV-diagnostics are sound.

Table C.1: **Changes to Baseline Model**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
U.S. Military Aid	0.110**	0.126***	0.139***	0.113**	0.125***	0.131***	0.095**	0.099**	0.129***	0.129***
	(0.047)	(0.043)	(0.046)	(0.050)	(0.043)	(0.044)	(0.039)	(0.044)	(0.028)	(0.041)
Civil Conflict	0.104***	0.126***			0.119***	0.119***	0.106***	0.116***	0.123***	0.123***
	(0.028)	(0.026)			(0.025)	(0.025)	(0.022)	(0.025)	(0.017)	(0.023)
Child Mortality		-0.001**	-0.001***		0.000	-0.000*	-0.001***	-0.001***	-0.001***	-0.001***
		(0.000)	(0.000)		(0.001)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Democracy	0.049		0.050		0.063	0.794***	0.050	0.116**	0.060	0.060
	(0.056)		(0.052)		(0.052)	(0.202)	(0.044)	(0.049)	(0.038)	(0.046)
Population Size	0.184***	0.099***	0.093***	0.167***	0.099***	0.052*	0.076***	0.081**	0.096***	0.096**
	(0.044)	(0.032)	(0.033)	(0.044)	(0.033)	(0.031)	(0.029)	(0.032)	(0.022)	(0.037)
Per Capita Income	0.044*			0.050**						
	(0.024)			(0.025)						
Democracy Index		0.076***		0.079***						
		(0.028)		(0.029)						
State Failure			0.028***	0.028***						
			(0.005)	(0.006)						
Child Mortality Squared					-0.000					
					(0.000)					
Democracy Squared						-0.834***				
						(0.217)				
Lagged Dependent Variable							0.154***			
							(0.022)			
Observations	6,833	7,388	7,489	6,922	7,373	7,373	7,234	7,373	7,373	7,373
First-Stage F-Stat.	30.57	31.13	32.31	29.81	31.24	32.17	30.68	31.09	80.60	18.39
AR Test p-value	(0.02)***	(0.00)***	(0.00)***	(0.03)**	(0.00)***	(0.00)***	(0.01)**	(0.03)**	(0.00)***	(0.01)**

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. Model (8) accounts for additional U.S. specific factors (where the respective mean-probability to receive financial, education or other military aid is interacted with annual realizations of U.S. arms exports, U.S. public debt and U.S. political polarization). Standard errors clustered at the country-level in parentheses. Driscoll-Kraay standard errors in Model (9) and standard errors clustered at both the country- and regional-level in Model (10). *p<0.1, **p<0.05, ***p<0.01

Appendix C.2. Further Control Variables

To 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 (indicating the extent of economic, political and social globalization), where the data is from the *KOF Globalization Index* (Gygli et al., 2020); (2) government size, i.e., government expenditure as a share of GDP (WDI data); (3) natural resource wealth (WDI data); (4) incidences of international warfare (Major Episodes of Political Violence Dataset); (5) urbanization, i.e., the share of the population living in cities (WDI data); (6) the male youth share, i.e., males aged 15-24 as a share of males aged 15-64 (WDI data); and (7) the rate of economic growth (WDI data).

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Our results reported in Table C.2 indicate that higher levels of U.S. military aid continue to robustly induce more anti-American terrorist activity in aid-receiving countries, with the associated IV-diagnostics indicating that these results are trustworthy. Concerning the additional controls, we find that there is a statistically significant and positive correlation between anti-American terrorism and economic growth and natural resource wealth, respectively. The latter finding speaks to the notion that greed for rents—which we also argue are at work with respect to the effect of U.S. military aid on anti-American terrorism—is relevant to the patterns of anti-American terrorism.

Table C.2: **Inclusion of Additional Covariates**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
U.S. Military Aid	0.117***	0.101**	0.102**	0.130***	0.136***	0.130***	0.099**
	(0.042)	(0.048)	(0.047)	(0.044)	(0.045)	(0.044)	(0.049)
Civil Conflict	0.121***	0.105***	0.109***	0.122***	0.123***	0.123***	0.102***
	(0.026)	(0.030)	(0.027)	(0.026)	(0.026)	(0.026)	(0.028)
Child Mortality	-0.001***	-0.001*	-0.001**	-0.001***	-0.001**	-0.001***	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Democracy	0.060	0.035	0.032	0.060	0.055	0.060	0.040
	(0.053)	(0.059)	(0.056)	(0.052)	(0.051)	(0.052)	(0.058)
Population Size	0.086***	0.141***	0.110***	0.096***	0.086**	0.096***	0.120***
	(0.032)	(0.049)	(0.037)	(0.033)	(0.034)	(0.033)	(0.044)
Globalization Index	-0.002						
	(0.002)						
Government Size		-0.000					
		(0.001)					
Natural Resource Rents			0.002**				
			(0.001)				
International Conflict				0.004			
				(0.013)			
Urbanization					0.002		
					(0.002)		
Male Youth Share						-0.000	
						(0.004)	
Economic Growth							0.001**
							(0.001)
Observations	7,121	6,242	6,706	7,373	7,373	7,373	6,775
First-Stage F-Stat.	34.42	28.24	29.82	30.64	36.59	32.40	28.35
AR Test p-value	(0.00)***	(0.04)**	(0.03)**	(0.00)***	(0.00)***	(0.00)***	(0.05)**

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

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Appendix C.3. Foreign Policy Variables

It is also possible that the provision of U.S. military aid correlates with other aid and foreign policy 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. Thus, we amend our model with additional foreign policy variables. First, we control for the total amount of economic development assistance provided by other countries. It is measured by the the net official development assistance for economic development purposes by other countries and multilateral institutions as a share of gross national income (WDI data). Second, we control for the per capita number of U.S. troops in aid-receiving countries using data from an update of Kane (2012). Third, we control for the existence of a collective defense agreement of the country of interest with the United States. 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. Note that this variable varies over time, e.g., as new countries join *NATO* or *SEATO* was disbanded. Fourth, we control for a country's political proximity to the United States. This proximity is operationalized as the difference between a sample country's ideal policy position and that of the United States, where this policy position follows from the country's voting behavior at the *United Nations General Assembly*. This voting data is from Bailey et al. (2018). Finally, we control for U.S. per capita economic aid, using data from USAID (2019). Note that we still control for average levels of U.S. economic aid (and troop presence) via the various fixed effects and trends already described in the main text.

As shown in Table C.3, controlling for these additional foreign policy variables does not affect baseline IV-findings concerning the role of military aid in anti-U.S. terrorism. Effect sizes are comparable to our baseline finding and the IV-diagnostics are sound. Concerning these additional controls, there is evidence that U.S. troop presence and political proximity to the United States correlate with more anti-American terrorism, which is consistent with Dreher and Gassebner (2008) and Gaibulloev and Sandler (2019a). For instance, citizens may attack U.S. interests to signal their displeasure with their country's foreign policy alignments with the United States.

Appendix C.4. Measurement of Anti-American Terrorism

To assess whether our results are robust to the operationalization of anti-American terrorism, we proceed as follows. First, using the location definition of transnational terrorism (as in the main text), we use the *count of anti-American terrorist attacks* per country-year

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Table C.3: **Role of Other Foreign Policy Variables**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
U.S. Military Aid	0.130***	0.118***	0.125***	0.144***	0.135***	0.121***	0.128***
	(0.045)	(0.044)	(0.044)	(0.045)	(0.046)	(0.045)	(0.047)
Civil Conflict	0.123***	0.113***	0.123***	0.122***	0.112***	0.123***	0.112***
	(0.026)	(0.026)	(0.026)	(0.026)	(0.027)	(0.025)	(0.026)
Child Mortality	-0.001***	-0.001**	-0.001***	-0.001***	-0.001**	-0.001**	-0.001**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Democracy	0.060	0.075	0.051	0.004	0.022	0.056	0.020
	(0.052)	(0.051)	(0.052)	(0.051)	(0.051)	(0.051)	(0.051)
Population Size	0.095***	0.099***	0.106***	0.108***	0.116***	0.097***	0.118***
	(0.033)	(0.033)	(0.033)	(0.032)	(0.034)	(0.033)	(0.034)
Official Development Aid	-0.000				0.000		
	(0.001)				(0.001)		
U.S. Troops		0.015**			0.013**		0.012**
		(0.007)			(0.006)		(0.005)
U.S. Ally			0.057**		0.022		0.023
			(0.024)		(0.027)		(0.027)
Political Proximity to U.S.				0.025**	0.019*		0.018
				(0.011)	(0.011)		(0.011)
U.S. Economic Aid						0.001	0.001
						(0.000)	(0.000)
Observations	7,373	6,725	7,373	7,180	6,536	7,373	6,536
First-Stage F-Stat.	30.98	31.81	30.44	33.38	32.85	29.95	30.86
AR Test p-value	(0.00)***	(0.01)**	(0.00)***	(0.00)***	(0.00)***	(0.00)***	(0.01)**

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

pair and the *per capita* number of anti-American terrorist 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.

Second, we use the 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. This allows us to create three additional dependent variables, namely (1) a binary indicator equal to unity when there is any anti-American terrorism, (2) the number of terrorist attacks and (3) the number of terrorist attacks per capita. A major shortcoming of the origin definition of transnational terrorism is, however, that it cannot properly account for attacks that are not claimed 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

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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.

Third, we 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 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. While it is clearly worthwhile to assess whether the choice of the dataset used to construct our dependent variables matters to our empirical conclusions, we ultimately prefer the ITERATE data for several reasons. For one, the GTD data only starts in 1970 (*ITERATE* starts in 1968), and GTD data for 1993 is incomplete and coding conventions changed over time, while *ITERATE* uses a consistent coding scheme (Enders et al., 2011). Furthermore, compared to *ITERATE*, *GTD* tends to undercount terrorist activity in the 1970s and overcount it in the 1990s (Enders et al., 2011).

We report our empirical findings in Table C.4. Regardless of which alternative dependent variable or terrorism dataset 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. The results for the control variables are also consistent for different dependent variables and terrorism datasets.

Appendix C.5. Operationalization of U.S. Military Aid

As another robustness check, we consider whether the operationalization of our main explanatory variable of interest, U.S. military aid, matters. We use the following eight variants. First, we do not apply the inverse hyperbolic sine transformation. This allows us to assess how anti-U.S. terrorism responds to a one-unit (i.e., 100 million US$) increase in U.S. military aid. Second, we use the log-transformation of the military aid variable, which may be more familiar to some readers. For this transformation, we also add unity to all aid data-points to account for zero-valued observations. Third, we weigh military aid by local population size to account for potential scaling effects, where we use both a non-transformed and inverse hyperbolic sine-transformed variant of military aid per capita.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable →	No. of Attacks (Location)	No. Of Attacks Per Capita (Location)	Binary Anti-American Terrorism (Origin)	No. of Attacks (Origin)	No. Of Attacks Per Capita (Origin)	Binary Anti-American Terrorism (GTD)	No. of Attacks (GTD)	No. Of Attacks Per Capita (GTD)
U.S. Military Aid	0.913**	0.112**	0.110***	0.490**	0.068**	0.099**	0.737**	0.069*
	(0.362)	(0.056)	(0.035)	(0.194)	(0.027)	(0.050)	(0.292)	(0.040)
Civil Conflict	0.501**	0.116**	0.113***	0.365***	0.057**	0.105***	0.231	0.062**
	(0.221)	(0.051)	(0.025)	(0.116)	(0.025)	(0.027)	(0.241)	(0.026)
Child Mortality	-0.002	-0.001**	-0.000	-0.001	-0.000	-0.000	0.001	-0.000*
	(0.002)	(0.000)	(0.000)	(0.001)	(0.000)	(0.000)	(0.002)	(0.000)
Democracy	-0.184	-0.044	0.020	0.068	-0.026	0.100*	0.818**	0.056
	(0.569)	(0.055)	(0.042)	(0.281)	(0.025)	(0.056)	(0.363)	(0.034)
Population Size	0.442*	-0.086	0.066**	0.270**	-0.011	0.101***	0.574**	-0.029
	(0.248)	(0.055)	(0.029)	(0.126)	(0.024)	(0.036)	(0.229)	(0.034)
Observations	7,373	7,373	7,373	7,373	7,373	7,165	7,165	7,165
First-Stage F-Stat.	30.70	30.70	30.70	30.70	30.70	31.79	31.79	31.79
AR Test p-value	(0.01)**	(0.04)**	(0.00)***	(0.01)**	(0.00)***	(0.06)*	(0.02)**	(0.09)*

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. Location=Location definition of transnational terrorism. Origin=Origin definition of transnational terrorism. GTD=Global Terrorism Database. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Fourth, we use a binary aid measure to study the extensive margins associated with the military aid-terrorism relationship. Here, we consider two variants, where the binary variable is either equal to unity if a country receives any amount of military aid or if it receives aid in excess of five million US$ per year. Finally, we winsorize the military aid variable at the 95^{th} percentile of its distribution, i.e., we replace the largest values of the aid variable by their values at the 95^{th} percentile. Alternatively, we trim the aid variable at the 95^{th} percentile of its distribution, meaning that observations with very large values of the military aid variable are dropped from our sample. Both trimming and winsorizing ought to help 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.

The results are shown in Table C.5. Irrespective of how we operationalize military aid, we find that more U.S. military aid results in more anti-American terrorism. In some cases, the first-stage F-statistics fall moderately below $F = 10$. However, the Anderson-Rubin test results that are robust to weak instruments reassuringly still point to instrument soundness in these cases. Finally, using the non-transformed military aid variable (specification (1) in C.5) allows us to further think about the economic significance of the estimated effects.

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Here, we find that a one-unit increase in U.S. military aid (100 million US$) increases the likelihood of the aid-receiving country to generate at least one incident of anti-American terrorism by 4%. For a country at the sample-mean of U.S. military aid and anti-American terrorism, this estimate implies that doubling military aid (to approximately 142 million US$) results in a 2.88 percentage point increase in the incidence of anti-American terrorism, which is approximately 17.5% of the mean incidence of anti-American terrorism. While still substantial, this effect is smaller than the one associated with the inverse hyperbolic sine transformed aid data.

Table C.5: **Alternative Operationalization of U.S. Military Aid**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Military Aid Variable →	Not IHS transformed (in USD 100 million)	Log+1	Aid Per Capita	Aid Per Capita, IHS	Binary (1=any aid)	Binary (1=aid > USD 5 million)	Winsorized (95%)	Trimmed (95%)
U.S. Military Aid	0.040**	0.158***	0.007***	0.006**	0.071**	0.070**	0.194***	0.202**
	(0.016)	(0.054)	(0.002)	(0.003)	(0.031)	(0.031)	(0.064)	(0.098)
Civil Conflict	0.127***	0.123***	0.115***	0.115***	0.135***	0.127***	0.119***	0.116***
	(0.025)	(0.026)	(0.025)	(0.027)	(0.027)	(0.027)	(0.027)	(0.023)
Child Mortality	-0.000	-0.001***	0.001	0.001	-0.001**	-0.001**	-0.001***	-0.001***
	(0.000)	(0.000)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)	(0.000)
Democracy	0.029	0.059	-0.016	-0.015	0.047	0.056	0.062	-0.031
	(0.060)	(0.052)	(0.073)	(0.084)	(0.053)	(0.053)	(0.052)	(0.050)
Population Size	0.095***	0.097***	0.141***	0.141***	0.116***	0.120***	0.100***	0.076***
	(0.030)	(0.033)	(0.040)	(0.041)	(0.035)	(0.035)	(0.033)	(0.029)
Observations	7,373	7,373	7,373	7,373	7,373	7,373	7,373	6,682
First-Stage F-Stat.	9.42	31.47	10.81	8.87	276.90	279.70	63.31	79.74
AR Test p-value	(0.00)***	(0.00)***	(0.00)***	(0.02)**	(0.02)**	(0.02)**	(0.00)***	(0.04)**

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. IHS=Inverse hyperbolic sine. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

Appendix C.6. Sub-Samples and Influential Country-Cases

Certain world regions, sub-samples of countries or time periods may drive our results. To study this possibility, we proceed as follows. First, we drop all OECD countries (members before 1990) 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 drop from our sample either (1) all Middle Eastern and Northern African, (2) Latin American or (3) sub-Saharan African countries. For instance, some of these world regions (e.g., Latin America) saw rather high levels of anti-American terrorism, while others (e.g., the Middle East and

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Northern Africa) have tended to receive especially high levels of U.S. military assistance. Third, we drop from our sample either (1) all countries that never produce anti-U.S. terrorism (roughly 25% of our sample), (2) all U.S. allies (as defined in Appendix C.3) and (3) all countries where a large number of U.S. troops (more than 10,000 servicemen) were deployed (e.g., Germany, Iraq and Afghanistan). For instance, through the latter approach we expect to reduce the influence of direct U.S. military interventions and occupations. Finally, we run two specifications that include an interaction term for the Cold War era (1968–1992) and the post-9/11 era (2002–2018), respectively. Significant interaction terms would tell us that these two international events (which could have affected the provision of military aid and induced a shift in the hot-spots of terrorism) mattered significantly to the relationship between U.S. military aid and anti-American terrorism.

As shown in Table C.6, there is no evidence that dropping data from specific world regions or from "special" sub-set of countries affects our findings. Moreover, the interaction effects associated with the Cold War and post-9/11 eras are statistically insignificant, indicating that our findings are also not driven by not driven by specific time period. That is, more U.S. military aid continues to lead to more anti-American terrorism for all reduced samples we consider, with the IV-diagnostics also being satisfactory.

Table C.6: **Sub-Sample Results**

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Sample →	No OECD	No MENA	No Latin America	No SSA	Drop Countries with No Terrorism Against U.S.	Drop U.S. Allies	No Countries with >10,000 U.S. Troops	Cold War (1968-1992)	Post-9/11 Era (2002-2018)
U.S. Military Aid	0.190***	0.113**	0.131***	0.100**	0.125***	0.187***	0.136***	0.110**	0.121**
	(0.045)	(0.052)	(0.044)	(0.043)	(0.045)	(0.069)	(0.052)	(0.047)	(0.050)
Civil Conflict	0.110***	0.140***	0.095***	0.117***	0.137***	0.086***	0.116***	0.113***	0.117***
	(0.025)	(0.027)	(0.023)	(0.037)	(0.029)	(0.022)	(0.025)	(0.025)	(0.025)
Child Mortality	-0.001***	-0.001***	-0.001**	-0.001*	-0.001**	-0.001**	-0.001**	-0.001***	-0.001***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Democracy	0.009	0.039	0.148***	0.072	0.080	0.071	0.014	0.114**	0.091*
	(0.054)	(0.051)	(0.054)	(0.065)	(0.062)	(0.053)	(0.049)	(0.050)	(0.051)
Population Size	0.014	0.091*	0.089***	0.072*	0.124***	0.008	0.075**	0.082**	0.075**
	(0.031)	(0.048)	(0.031)	(0.042)	(0.038)	(0.031)	(0.032)	(0.033)	(0.036)
U.S. Military Aid * Cold War								-0.005	
								(0.102)	
U.S. Military Aid * Post-9/11 Era									0.014
									(0.156)
[Equality of Coefficients]								[0.01]	[0.01]
[p-value]								[0.96]	[0.93]
Observations	6,257	6,446	6,165	5,220	5,895	5,265	6,411	7,373	7,373
First-Stage F-Stat.	27.19	25.68	25.83	26.18	28.89	27.97	37.55	16.16; 10.22	16.07; 5.05
AR Test p-value	(0.00)***	(0.03)**	(0.00)***	(0.03)**	(0.00)***	(0.00)***	(0.00)***	(0.05)**	(0.02)**

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). AR=Anderson-Rubin. OECD=Organisation for Economic Co-operation and Development. MENA=Middle East and Northern Africa. SSA=Sub-Saharan Africa. For the two interaction models (8) and (9), we report two F-statistics for our instrument and the instrument interacted with the break year, respectively. For these models, we also test for equality of the military aid coefficient before and after the supposed break date. Not being able to reject the H_0 of coefficient equality implies that the effect of military aid on anti-American terrorism is similar before and after the break. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01

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Moreover, we study whether specific parts of our sample drive our results by considering how our estimates of the effect of U.S. military aid on anti-American terrorism change when we drop potentially influential country-cases from our dataset. In detail, we drop from our dataset ten countries that are potentially influential because they received substantial amount of U.S. aid and/or produced considerable anti-U.S. terrorism over our period of observations. As shown in Table C.7, we drop from our sample countries such as Afghanistan, Iraq, Israel, Egypt, Saudi Arabia and Pakistan. As also shown in this table, however, dropping these country-cases does not change the main empirical conclusion of our paper. More U.S. military aid leads to more anti-U.S. terrorism. The associated point-estimates are robust to the dropping of specific country-cases, indicating that these cases do not drive our empirical results.

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Table C.7: **Role of Potentially Influential Country-Cases**

Dropped Country	β-Coefficient Military Aid	IV-Diagnostics
(Full Sample)	0.129***	F=30.70
	(0.044)	AR=(0.00)***
Afghanistan	0.121**	F=31.67
	(0.048)	AR=(0.01)**
Iraq	0.114**	F=31.10
	(0.046)	AR=(0.01)**
Israel	0.127***	F=30.08
	(0.044)	AR=(0.00)***
Egypt	0.133***	F=29.13
	(0.048)	AR=(0.00)***
Colombia	0.130***	F=30.24
	(0.044)	AR=(0.00)***
Saudi Arabia	0.129***	F=30.69
	(0.044)	AR=(0.00)***
Pakistan	0.138***	F=28.52
	(0.046)	AR=(0.00)***
Jordan	0.128***	F=31.06
	(0.044)	AR=(0.00)***
Russia	0.129***	F=29.92
	(0.045)	AR=(0.00)***
South Korea	0.151***	F=36.33
	(0.047)	AR=(0.00)***

Notes: IV-estimates reported. All models include the baseline controls, trends and fixed effects; see Table 1, model (4). IV-diagnostics are the first-stage F-statistics and the p-values associated with the Anderson-Rubin (AR) test. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01

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Appendix C.7. Non-Linear Models

The use of a linear probability model when studying a binary outcome (as in our case) can be criticized on grounds that (1) the predicted probability may fall outside the range between zero and one and that (2) statistical tests for linear analyses are inappropriate with a binary dependent variable (e.g. Hellevik, 2009). Thus, it may be beneficial to assess whether our empirical results change when we employ non-linear models. Below, we report (1) probit and instrumental-variable simultaneous-equations probit model estimates (Rivers and Vuong, 1988) as well as (2) logit and (instrumental-variable) control-function logit estimates (Wooldridge, 2010) to investigate the relationship between U.S. military aid and anti-American terrorism in non-linear settings that can also accommodate endogeneity concerns using our usual instruments for military aid. Using non-linear approaches means that we have to drop all country-observations for which the various fixed effects perfectly predict success or failure in the dependent variable. Dropping the perfectly predicted observations has no effect on the likelihood or estimates of the remaining coefficients and increases the numerical stability of the optimization process. To make the non-linear results comparable to our baseline results, we also report the OLS and IV-results for a reduced sample that does not consider those observations dropped from the non-linear models.

We report our results in Table C.8. We find that higher levels of U.S. military aid lead to more anti-American terrorism for both non-linear models and their respective instrumental-variable counterparts. We also continue to find that U.S. military aid promotes anti-American terrorism in the linear probability model and its IV-counterpart that use a reduced country-sample. In sum, these results suggest that there is little difference between non-linear and linear approaches. In practice, we employ a linear probability modelling approach because it readily gives us access to tools (e.g., the selection on observables) that can strengthen the causal interpretation of our empirical findings.

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Table C.8: **Non-Linear Model Estimates**

	(1)	(2)	(3)	(4)	(5)	(6)
Estimator →	Probit	IV-Probit	Logit	Control Function	OLS	IV
U.S. Military Aid	0.324***	0.492**	0.523***	0.478***	0.111***	0.125***
	(0.092)	(0.197)	(0.156)	(0.170)	(0.034)	(0.045)
Civil Conflict	0.614***	0.604***	1.084***	1.061***	0.138***	0.137***
	(0.118)	(0.116)	(0.214)	(0.214)	(0.029)	(0.029)
Child Mortality	-0.005***	-0.005***	-0.008**	-0.008**	-0.001**	-0.001**
	(0.002)	(0.002)	(0.003)	(0.003)	(0.000)	(0.000)
Democracy	0.451*	0.437*	0.900**	0.881*	0.081	0.079
	(0.256)	(0.258)	(0.453)	(0.454)	(0.062)	(0.062)
Population Size	0.923***	0.883***	1.963***	1.898***	0.128***	0.124***
	(0.209)	(0.208)	(0.420)	(0.427)	(0.036)	(0.038)
Observations	5,895	5,895	5,895	5,895	5,895	5,895

Notes: All models include the baseline controls, trends and fixed effects; see Table 1, model (4). First-stage F-statistic for model (6): F=28.89. AR-test p-value for model (6): p=0.00. Standard errors clustered at the country-level in parentheses. *p<0.1, **p<0.05, ***p<0.01.

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