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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? To answer these questions, we estimate 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 aid especially for military financing and education increase the likelihood of anti-American terrorism in recipient countries. Examining potential transmission channels, we show that more U.S. military aid undermines military capacity and increases corruption and exclusionary policies in recipient countries. Our findings are consistent with the argument that military aid aggravates local grievances, creating anti-American resentment and leading to anti-American terrorism. Indeed, we also provide tentative evidence that military aid lowers public opinion about the United States in recipient countries.

Keywords: Anti-American Terrorism, Corruption, Instrumental Variable Estimation, State Capacity, U.S. Military Aid

JEL: D74, F35

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1. Introduction

There is substantial evidence that U.S. interventions can produce unintended consequences. Dell and Querubin (2018) study the effect of the U.S. air campaign in the Vietnam War, finding that aerial bombing increased both the military and political activities of insurgents that opposed the U.S. and weakened local governance. Similarly, Kocher et al. (2011) find that U.S. aerial bombardment during the Vietnam War shifted local control in favor of those insurgents. Considering more recent evidence, Mahmood and Jetter (2023) investigate the effects of U.S. drone strikes in Pakistan and show that such strikes lead to an increase in both terrorism and anti-American protests. In addition to military interventions, the provision of U.S. aid to prop up local governments that sympathize with the United States can likewise have inadvertent effects. For example, Nunn and Qian (2014) show that U.S. food aid increases the incidence and duration of civil conflict in recipient countries. Sexton (2016) finds that U.S. counter-insurgency aid leads to a significant increase in insurgent violence when allocated to contested Afghani districts. Ahmed (2016) demonstrates that U.S. economic assistance leads to more human rights abuses in aid-receiving countries. Finally, Dube and Naidu (2015) show that U.S. military aid to Colombia increases attacks by paramilitaries, undermining domestic political institutions.[1]

In this paper, we advance the literature on the unintended consequences of U.S. interventions by asking whether *U.S. military aid* makes the United States safer or rather contributes to insecurity.[2] We focus on the relationship between U.S. military aid and transnational *anti-American terrorism*.[3] In our study of the military aid-terrorism nexus,

[1]This is not to say that U.S. aid will always have unfavorable effects. 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 reduced insurgent violence. Dell and Querubin (2018) highlight that U.S. efforts to win local hearts and minds in Vietnam led to better outcomes compared to more aggressive military approaches. See also the more general discussion of potential outcomes of military and non-military interventions in Rohner (2023).

[2]As defined by the *United States Agency for International Development* (USAID, 2019), military aid refers to assistance that subsidizes or substantially enhances the military capability of the recipient country.

[3]Transnational terrorism is defined 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

we show that U.S. military aid increases insecurity by leading to more anti-American terrorism.

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). Given this threat, the United States has provided military aid in the hope of reducing anti-American terrorism by strengthening state capacity in recipient countries. We call this way of thinking the *policy-maker argument*, as it corresponds to the thoughts of American policy-makers about the efficacy of military aid (e.g., Obama, 2013; The White House, 2013).[4] The rational-economic model of terrorism 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 political goals (e.g., Landes, 1978; Sandler et al., 1983; Enders and Sandler, 1993; Schneider et al., 2015; Gaibulloev and Sandler, 2019). Within this theoretical framework, the policy-maker argument implies that by supplementing local state capacity, U.S. military aid raises the material costs of carrying out terrorism, lowering—ceteris paribus—terrorist activity. For instance, it is more costly for terrorist organizations to operate when opposing governments are likely to retaliate (Lai, 2007). Indeed, there is evidence suggesting that terrorism is less prevalent in strong states (e.g., Lai, 2007; Hendrix and Young, 2014; George, 2018).

In contrast to the policy-maker argument, existing empirical evidence shows that there is an unfavorable correlation between measures of U.S. foreign and military policy (e.g., U.S. arms exports, U.S. support for Israel) and anti-American terrorism (Neumayer and Plümper, 2011; Gries et al., 2015; Krieger and Meierrieks, 2015; Saiya et al., 2017; Meierrieks and Gries, 2020). These studies make the *strategic logic argument*, which posits that U.S. military aid leads to more anti-American terrorism. Consider a three-way interaction between a government, a terrorist group that wants to extract concessions from this government and a foreign actor (the U.S.) that provides military aid to the government. The provision of military aid shifts the local balance of power in favor of the government (e.g., Neumayer and Plümper, 2011; Savage and Caverley, 2017; Biddle et al., 2018). This creates

transcend national boundaries. (Mickolus et al., 2019, p.1).

[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).

an incentive for the terrorist group to attack the United States in the hope of forcing the U.S. to withdraw its support from the local government (e.g., due to political pressure from the American public). In other words, by successfully bolstering local state capacity, the United States perversely incentivizes anti-American terrorism to negate this very success.[5]

Yet, the aforementioned empirical studies on the relationship between U.S. foreign policy and anti-U.S. terrorism have not appreciated endogeneity concerns. For instance, such concerns may be rooted in the notion that military aid can also *respond* to terrorist activity (e.g., Balla and Reinhardt, 2008; Bapat, 2011; Boutton and Carter, 2014; Bezerra and Braithwaite, 2016; Lis, 2018). We study the relationship between U.S. military aid and anti-American terrorism for 174 countries between 1968 and 2018. We provide *causal estimates* of the effect of U.S. military aid on anti-U.S. terrorism, using an instrumental variable (IV) approach to address endogeneity concerns due to reverse causality or joint determination. Contrary to the policy-maker argument, we show that higher levels of military aid, especially for military financing and training, result in *more* anti-U.S. terrorism in recipient countries. In our preferred IV-specification doubling aid for military financing and training increases the risk of anti-American terrorism by 7.9 percentage points at the sample-mean, which is approximately 50.9% of the mean-incidence of anti-U.S. terrorism.

We instrument local military aid by the region-specific provision of U.S. military aid. The local provision of military aid is guided by U.S. interests. That is, the amount of aid a specific country receives is determined by the economic, political or geo-strategic interests of the United States related to that country. Indeed, there is considerable evidence that the allocation of U.S. foreign assistance is driven by U.S. interests (e.g., Poe and Meernik, 1995; Irwin, 2000; Kuziemko and Werker, 2006; Dreher et al., 2008; Boutton and Carter, 2014). For instance, the U.S. may provide aid to foreign countries to buy political favors at the United Nations (e.g., Dreher et al., 2008). Ideally, we would instrument military aid by (plausibly exogenous) country-specific U.S. interests. However, these interests are not observed. Yet, there is also a region-specific dimension to U.S. interests. Various region-specific factors (e.g., cultural proximity to the U.S.; resource wealth; common political history; geographical proximity to the U.S. or its geo-strategic adversaries) can explain why different world regions receive specific levels of U.S. military aid. For instance, the ge-

[5]Terrorism may produce additional incidental benefits, e.g. media attention and peer acknowledgment (e.g., Rohner and Frey, 2007; Neumayer and Plümper, 2011; Jetter, 2017). These benefits are expected to be disproportionately large when the U.S. is targeted, further incentivizing anti-U.S. terrorism.

ographical and historical proximity (as, e.g., exemplified by the Monroe Doctrine) between the U.S. and Latin America helps us understand why this part of the world enjoyed—on average—elevated levels of military assistance (e.g., Schoultz, 1987; Taffet, 2007). In addition, there is evidence that countries that are proximate to U.S. geo-strategic enemies (e.g., European countries close to the USSR/Russia) will receive more U.S. aid (e.g., Poe and Meernik, 1995). Given this region-specific dimension of U.S. military aid, we can draw conclusions about U.S. interests and their development over time and space by approximating these interests via variations in region-specific levels of military aid.[6]

If there is a region-specific dimension to U.S. aid-giving, this instrument ought to be sufficiently relevant, with regional aid positively predicting local aid. This is because variation in regional aid that reflects changes in U.S. regional interests ought to affect countries located in the same world region in similar ways. Importantly, U.S. interests are causally prior to U.S. military aid (in that changes in U.S. interests induce changes in the provision of military aid). This causal ordering implies that simultaneity in the first-stage equation cannot emerge, which would otherwise threaten our IV-approach (e.g., Betz et al., 2018).

At the same time, region-specific aid ought to be exogenous to the nexus between local military aid and anti-American terrorism. Decisions concerning the region-specific distribution, composition and level of aid are made in the United States (e.g., Irwin, 2000; Newhouse, 2009; Albouy, 2013; Boutton, 2021). For instance, these decisions may be affected by budgetary considerations in Congress, bureaucratic choices within responsible departments and offices (e.g., the *Department of Defense* or the *Bureau of the Political-Military Affairs*) as well as other deliberations by the U.S. executive and Congress (e.g., as a consequence of the relative political power of hawkish/dovish or isolationist/internationalist policy-makers). Such bureaucratic, legislative and executive considerations, in turn, are influenced by the U.S. electorate and other U.S. stakeholders (e.g., the defense industry lobby and policy think tanks). This suggests that the influence of foreign countries on regional aid, which we use to approximate regional U.S. interests and instrument local aid, is

[6]This argument mimics Acemoglu et al. (2019) who instrument local democratic institutions via regional democratization to estimate the causal effect of local democracy on economic growth. They argue that regional democratization reflects "the demand for democracy [...] across countries within a region, which tend to have similar histories, political cultures, practical problems, and close informational ties" (Acemoglu et al. 2019, p.80), where this demand for democracy itself is not observed. Analogous to this, we argue that U.S. military aid is similar in countries located in the same world region due to (unobserved) region-specific U.S. interests, where we can approximate this "hidden" variable via regional military aid levels.

negligible. Therefore, in comparison to the influence of various U.S. stakeholders, regional aid can be considered exogenous to local government decisions, policies and further local conditions. 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).[7]

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, the exclusion restriction may be violated, i.e., regional aid could influence local anti-American terrorism through channels other than local aid. There may be economic, political or demographic shocks that are regionally correlated (and thus affect the construction of our IV) and which might simultaneously influence the provision of military aid to and the production of anti-American terrorism in the country of interest. As part of our robustness checks, we run additional specifications where we include time-variant covariates accounting for such shocks. To additionally probe the validity of our IV-approach, we also employ a number of placebo instruments, e.g., creating "artificial" world regions that should not be sound instruments as they do not share regional characteristics that would affect regional U.S. interests. Moreover, we use the plausibly exogenous framework of Conley et al. (2012) that allows us to relax the assumption of perfect instrument exogeneity and investigate how violations of the exclusion restriction matter to our IV-estimates. Finally, we consider it highly unlikely—given the intricacies of the U.S. political and bureaucratic system—that recipient countries can accumulate enough lobbying power to not only affect aid decisions concerning their respective home country (which are *not* considered in our IV) but also the flow of U.S. aid to proximate countries. Still, as part of our extensive sensitivity analysis, we identify countries (e.g., Afghanistan, Iraq, Pakistan, Israel, Greece or Turkey) that could exercise such lobby power (e.g., because they are of preeminent importance for U.S. foreign policy) and assess whether our results are sensitive to their inclusion/exclusion in our dataset. Reassuringly, our main empirical finding—U.S. military aid leads to more anti-American terrorism—survives all aforementioned robustness checks.

[7]The complexities of the U.S. budgeting process add to the difficulties for foreign governments to influence the regional allocation of aid. Because of the low popularity of foreign policy issues among constituents, foreign aid appropriations are typically folded into omnibus spending bills, implying that members of Congress will have to balance various issues of—mainly—domestic policy simultaneously (Obey and Lancaster, 1988; Irwin, 2000; Morgenstern, 2019; Lawson and Morgenstern, 2020).

In addition to examining the effect of U.S. military aid in anti-U.S. terrorism, we also explore potential *transmission channels* from U.S. military aid to anti-American terrorism. We show that U.S. aid for military financing and training reduces military capacity (but not administrative capacity). This finding, however, runs counter to the strategic logic argument (which argues that anti-American terrorism is a consequence of increased state capacity). Rather, we also find that military aid amplifies corruption, clientelism and exclusionary policies in recipient countries. These findings speak to a hitherto unexplored pathway from more military aid to more anti-American terrorism that we introduce and empirically evaluate in this contribution, the *grievances channel*.[8]

The provision of aid yields benefits to the United States, e.g., market access or favorable voting behavior of recipient countries at international organizations. For making such concessions, recipient countries obtain non-financial and financial rewards. Especially monetary benefits, in turn, enable *rent-seeking behavior*. Local politicians, bureaucrats and other interest groups who have some leeway with respect to its distribution can appropriate aid, letting some social groups share in the benefits of aid, while excluding other groups from it (for the case of economic aid, see, e.g., Svensson, 2000; Hodler, 2007; Keefer and Knack, 2007; Djankov et al., 2008). Furthermore, a recipient country may use U.S. aid to supplement its own military spending, using associated savings to finance other projects that disproportionately benefit its supporters.[9] For the government, this redistribution of aid is beneficial because the winners of this distribution process ought to help the government stay in power (e.g., Bueno de Mesquita and Smith, 2009). However, those excluded from aid rents may experience economic deprivation. According to the rational-economic model of terrorism, the opportunity costs of terrorism for these individuals are expected to decrease as non-violent socio-economic participation becomes more constrained. This, in turn, ought to make terrorism —as an alternative way to achieve such participation and overcome economic deprivation—comparatively more attractive (Blomberg et al., 2004; Kurrild-Klitgaard et al., 2006; Schneider et al., 2015; Gaibulloev and Sandler, 2019).

[8]Our subsequent 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 the nexus between aid and anti-American terrorism has so far been neglected.

[9]This refers to *aid fungibility*, i.e., the ability of the recipient country to spend targeted aid on non-targeted programs (e.g., Pack and Pack, 1990, 1993; Feyzioglu et al., 1998). Deger and Sen (1991) and Khilji and Zampelli (1994) find that military aid is as fungible as economic aid. Thus, U.S. military assistance may indeed also be used to benefit government supporters outside of the military sphere.

In addition to having distributional effects, military aid may also lead to weaker institutions. For instance, the availability of aid rents may disincentivize investment in public goods and institutions that would curb rent-seeking (e.g., Svensson, 2000). As another example, U.S. military aid may incentivize corrupt behavior among politicians and bureaucrats, e.g., in the context of arms and procurement deals (Alesina and Weder, 2002; Auer and Meierrieks, 2021, see also Fisman and Golden 2017; Dimant and Tosato 2018; Andersen et al. 2022). Weak institutions, in turn, may contribute to economic deprivation and political disenfranchisement, making terrorism as a means to overcome such issues more likely (e.g., Abadie, 2006; Kurrild-Klitgaard et al., 2006; Gaibulloev and Sandler, 2019; Auer and Meierrieks, 2021).

In sum, military aid may create grievances among those who suffer from poor institutions (e.g., higher levels of corruption, fewer public goods) and exclusionary policies due to the inflow of military aid. Consequently, we expect those population groups that suffer from reduced economic and political participation to direct their dissatisfaction against the U.S. as the perceived linchpin of an unfavorable status quo (e.g., Tokdemir, 2017), ultimately leading to anti-U.S. terrorism. Indeed, our exploration of public opinion data reveals that higher levels of U.S. military aid lead to greater dissatisfaction with the U.S. in recipient countries. This increased dissatisfaction, in turn, correlates with more anti-U.S. terrorism.

Besides adding to the empirical literature on the determinants of anti-American terrorism, our study contributes to three other strands of the literature. First, 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. Grossman, 1992; Djankov et al., 2008) or has 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 speak to a destabilizing effect of aid. Second, 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., Dalgaard and Hansen, 2001; Bourguignon and Sundberg, 2007; Clemens et al., 2012), various studies investigate the detrimental effect of aid on democratic institutions (e.g., Djankov et al., 2008). By showing that U.S. military aid promotes corruption and exclusionary policies, we provide evidence that aid can undermine institutional quality in recipient countries. Third,

we add to the evaluation of counter-terrorism spending and policies (for surveys, see, e.g., Mueller and Stewart, 2014; Schneider et al., 2015; Gaibulloev and Sandler, 2019). Our study highlights the counterproductive nature of U.S. military aid as a counter-terrorism tool, given the backlash effects.

The organization of this paper is as follows. In Section 2, we discuss the operationalization of anti-American terrorism and U.S. military aid. We present OLS estimates of the relationship between terrorism and aid in Section 3. In light of potential endogeneity concerns, we also study this relationship using an IV-approach in Section 4. Potential transmission channels and mechanisms are investigated in Section 5. Section 6 concludes.

2. Data

We use data from 174 countries between 1968 and 2018 to empirically investigate the effects of U.S. military aid on anti-American terrorism. However, due to missing data, we exclude twelve small countries when running models with control variables. A country list that also indicates which countries have to be dropped is provided in Appendix A. The summary statistics are reported in Appendix A, too.

2.1. Measuring Anti-American Terrorism

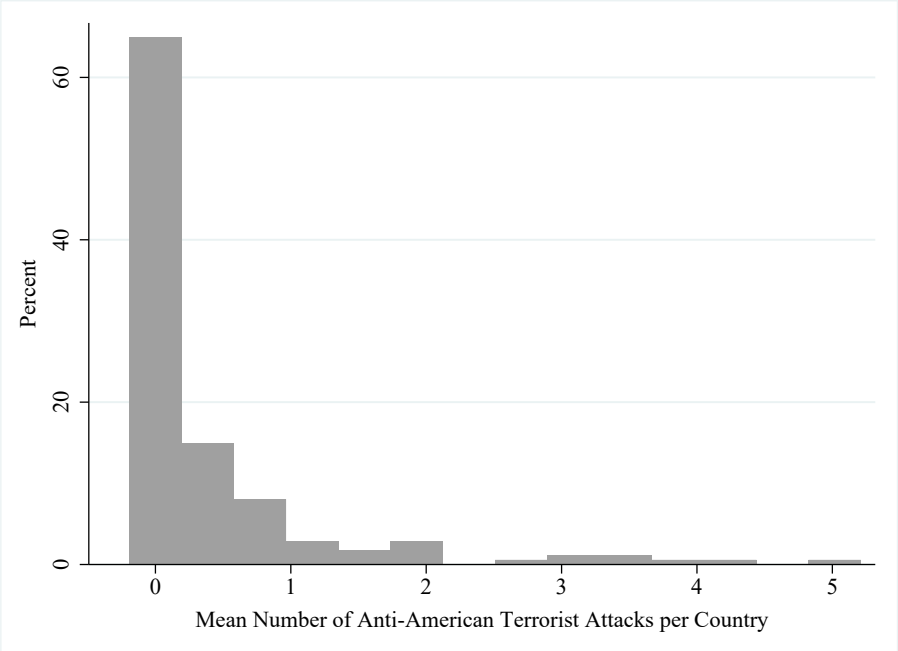
We source data from the *International Terrorism: Attributes of Terrorist Events (ITERATE)* dataset of Mickolus et al. (2019). This is the largest and most consistent dataset that captures transnational terrorist activity (Enders et al., 2011).

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 in a given country-year. As part of our robustness checks, we employ alternative measures of anti-American terrorism such as the count of anti-American terrorist attacks per country-year.

Non-U.S. actors carry out anti-American terrorist attacks targeting U.S. diplomatic, military, commercial and non-official entities, including American diplomats and embassies, U.S. peacekeepers, U.S. businesses and American tourists.[10] As shown in Figure 1, most countries see between zero and one anti-American attack per year. As a share of all

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

Figure 1: Histogram of Anti-American Terrorism



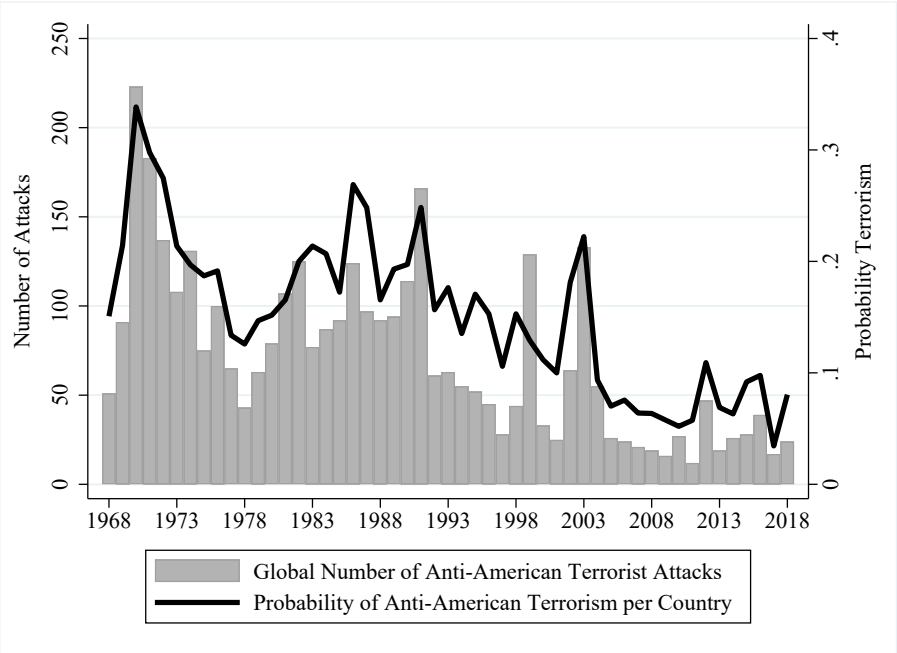
transnational terrorist events reported in *ITERATE*, anti-U.S. terrorism accounts for approximately 31% of all incidents.

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 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. Abrahms and Conrad (2017) report that only one in seven terrorist attacks is actually claimed. In addition, there may be multiple claims related to a single attack, making it difficult to correctly assign an attack (Abrahms and Conrad, 2017). Using the location definition, we therefore avoid undercounting anti-U.S. terrorism. For robustness, we also use an alternative terrorism definition later, where we attribute attacks to the terrorists' country of origin (if known), irrespective of the venue country.

Figure 2 illustrates the patterns of anti-U.S. terrorism. There are noticeable spikes in the early 1970s, early 1990s and the mid-2000s as well as relative lulls in the mid-1970s, mid-

[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.

Figure 2: Anti-American Terrorism, 1968-2018



1990s and after 2005. Between 10% (after 2005) and over 30% (early 1970s) of all countries saw at least one anti-U.S. terrorist attack per year, pointing to a large geographical and temporal variation in anti-American terrorism.

2.2. Measuring U.S. Military Aid

The data on U.S. military aid are drawn from the *United States Agency for International Development* (USAID, 2019). We measure aid in constant (inflation-adjusted) US dollars, in the millions. According to USAID (2019), military assistance is defined as aid primarily benefiting a recipient government's armed forces or that significantly enhances local military capability.[12] Specifically, U.S. military aid can be categorized into six areas:

[12]U.S. military aid does not include payments for U.S. troops stationed in aid-receiving countries. Also, the foreign aid under consideration does not include financial support by U.S. intelligence agencies. Because of their clandestine nature, we cannot consider such aid flows. However, it is reasonable to assume that secret and official foreign aid correlate positively for the majority of recipient countries. Anecdotal evidence of flows of CIA "ghost money" supports this assumption (for the case of Afghanistan, see Rosenberg, 2013). Thus, the aid figures reported here can be seen as conservative estimates of their true amount, also leading us to provide a rather conservative estimate of the impact of aid on anti-American terrorism.

1. *Foreign Military Financing* (*finance*): Provides (non-repayable) grants or direct loans for partner countries to purchase U.S. defense articles and services.

2. *Foreign Military Training/Education* (*train*): Provides education and training to foreign military personnel.

3. *Drug Interdiction and Counter-Drug Activities* (*drug*): Supports foreign governments and militaries to confront international drug trafficking or organized crime networks.

4. *Cooperative Threat Reduction* (*threat*): Aims at securing and dismantling weapons of mass destruction, especially in the former Soviet Union.

5. *Peacekeeping Operations* (*peace*): Focuses on building international peacekeeping capacity.

6. *Additional Special Programs* (*special*): Includes various country-specific programs such as the *Afghanistan Security Forces Fund* and the *Iraq Security Forces Fund*.

In our empirical analysis of the influence of U.S. military aid on anti-U.S. terrorism, we concentrate on aid allocated for foreign military financing and education. This type of aid is by far the most important with respect to volume. Moreover, it clearly serves the purpose of increasing local state capacity. At the same time, this type of aid may also be especially vulnerable to corruption, patronage and misappropriation, e.g., in the context of arms deals (Gupta et al., 2001; Auer and Meierrieks, 2021). Thus, the unfavorable impact of U.S. aid for military financing and education on anti-American terrorism may be especially strong due to the role of this type of aid in aggravating local grievances.

By contrast, the remaining military aid programs are context-specific. For some programs, it is unclear whether they improve state capacity. For instance, the cooperative threat reduction program actually aims at reducing military capacity. Other programs, particularly the counter-narcotics programs (e.g., in Colombia) and security programs for Pakistan, Afghanistan and Iraq, clearly serve a capacity-building purpose—and offer opportunities for corruption—but may be better analyzed in case-study frameworks (e.g., Dube and Naidu, 2015; Berman et al., 2011).

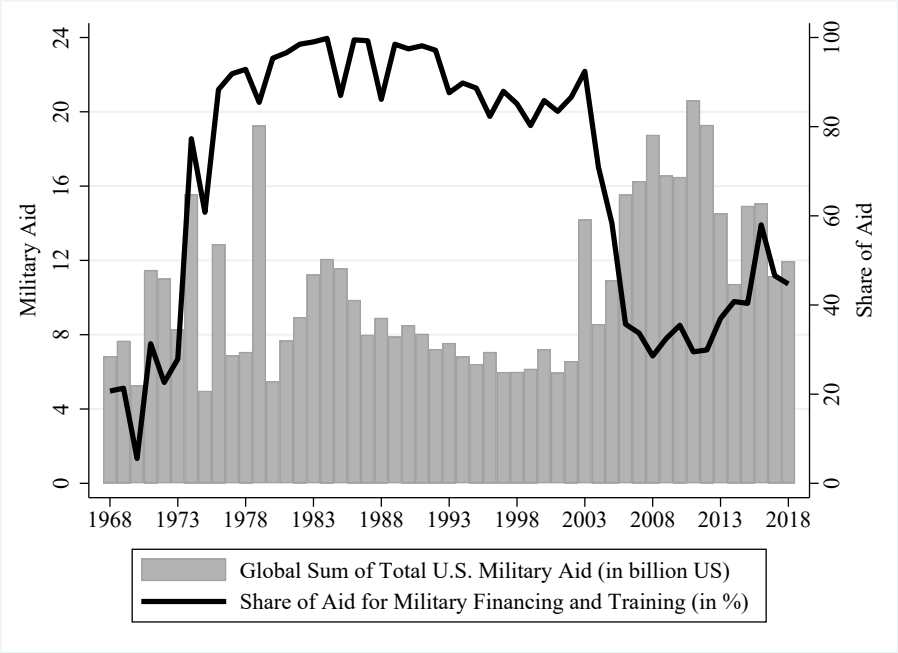
While our primary focus is the link between anti-American terrorism and aid for military financing and training, we also examine the relationship between anti-American terrorism and aid given to other programs (*other military aid*), in addition to the sum of all individual

aid programs, i.e., *total military aid*. This approach primarily allows us to investigate if different military aid programs play varying roles in anti-American terrorism. For a specific country i at year t, our main variable of interest and the other two variables are constructed as follows:

$$\underbrace{totalaid_{it}}_{\text{total military aid}} = \underbrace{finance_{it} + train_{it}}_{\text{military financing and education aid}} \underbrace{+drug_{it} + peace_{it} + threat_{it} + special_{it}}_{\text{other military aid}} \quad (1)$$

Figure 3 depicts the global trends in official U.S. military aid (*totalaid*) throughout our observation period. 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).

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



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. Concerning the composition of U.S. military aid, Figure 3 also shows that especially between 1975 and 2003, almost all military aid was for military financing and training. This decreased especially after 2001.

Over the whole period of observation, approximately 65% of total military aid was used for military financing and training. Of the remaining 35%, approximately 30% were spent on special aid programs especially for South Vietnam (till 1975) as well as Afghanistan, Iraq and Pakistan (after 2001). The remaining 5% were associated with the cooperative threat reduction initiative (2.2%), drug interdiction and counter-drugs programs (1.5%) and peacekeeping (1.3%).

3. OLS Estimates

3.1. Empirical Model

To examine the association between U.S. military aid and 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* represents the binary measure of anti-American terrorism for a given country i in the specific year t. Our independent variable of interest is U.S. aid for military financing and education (*aid*). We apply the inverse hyperbolic sine transformation to this 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).[13] 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 vector X accounts for various confounders. We control for (1) the incidence of *civil conflict* using data from the *Major Episodes of Political Violence Dataset* (Marshall, 2020); (2) *infant mortality* as a measure of economic development, where the data are from the *World Development Indicators* (*WDI*) (World Bank, 2019)[14]; (3) *democracy* drawn from the *Varieties of Democracy Dataset* (*VDEM)* of Coppedge et al. (2021); (4) *population size*

[13] As part of our robustness checks, we also use alternative operationalizations of U.S. military aid, including a log-transformation.

[14] 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, means that we have to forego potentially influential country cases such as Afghanistan.

(WDI data); and (5) an index of *globalization* that reflects a country's economic, social and political integration with the rest of the world, where the data are from Gygli et al. (2019). Both child mortality and population size are inverse hyperbolic sine-transformed to account for skewness.

These confounders are expected to affect both the production of anti-American terrorism and the provision of U.S. military aid. For instance, more populous countries are more likely to generate anti-American terrorism (due to a greater presence of perpetrators, targets and victims) and receive larger amounts of aid due to scale effects. At the same time, we consider these confounders primarily to study the stability of the main effect of interest. Due to the lack of an identification strategy associated with estimating the associations between anti-American terrorism and the confounders, these associations cannot be given causal interpretations.

3.2. Regression Results

Table 1 presents our Ordinary Least Squares (OLS) estimates. These figures indicate a correlation between higher levels of U.S. aid for military financing and training and increased levels of anti-American terrorism, irrespective of whether our model includes confounding variables. Here, the estimated associations for the confounders are as expected. For instance, we find that more populous countries are more likely to see anti-American terrorism. We also report results from a series of panel unit root tests developed by Choi (2001). These test results indicate that the residuals are stationary, reducing concerns that our OLS results are spurious.

Considering economic significance, using the baseline specification (specification (2) of Table 1), we find—ceteris paribus—that a ten-percent increase in U.S. aid for military financing and training increases the likelihood of the aid-receiving country to generate at least one incident of anti-American terrorism by 0.29 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 92 million US$) results in a 2.9 percentage point increase in the incidence of anti-American terrorism, which is approximately 18.7% of the mean incidence of anti-American terrorism.

In Table 1, we also show that there is no statistically significant association between anti-American terrorism and U.S. military aid for purposes other than military financing and training (e.g., counter-narcotics and peacekeeping programs). Furthermore, assessing the

Table 1: OLS Regression Results

	(1)	(2)	(3)	(4)	(5)
Military Aid	0.032***	0.029***	0.028***		
	(0.007)	(0.007)	(0.006)		
Other Military Aid				0.007	
				(0.011)	
Total Military Aid					0.031***
					(0.008)
Civil Conflict		0.113***	0.112***	0.112***	0.113***
		(0.028)	(0.027)	(0.027)	(0.027)
Child Mortality		0.056*	0.057*	0.057*	0.058*
		(0.031)	(0.031)	(0.031)	(0.032)
Democracy		0.013	0.012	0.012	0.010
		(0.011)	(0.011)	(0.011)	(0.010)
Population Size		0.173***	0.166***	0.166***	0.152***
		(0.046)	(0.042)	(0.042)	(0.041)
Globalization		-0.003	-0.003	-0.003	-0.003
		(0.002)	(0.002)	(0.002)	(0.002)
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Control for Remaining Aid	No	No	Yes	Yes	No
Fisher Test p-value	0.00	0.00	0.00	0.00	0.00
Observations	8,001	7,121	7,121	7,121	7,121

Notes: OLS-estimates reported. *FE* = fixed effects. *Control for Remaining Aid* means that we control for other military aid (specification (3)) or military aid for financing and training (specification (4)), respectively. *Fisher Test* = Choi's Fisher-type panel unit root test. Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

correlation between total U.S. military aid and anti-American terrorism, estimated effect sizes for total and financing/education aid are similar, suggesting that our empirical findings are driven by U.S. aid for military financing and education.

3.3. Selection on Observables and Bias from Unobservables

Despite controlling for several fixed effects 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. aid for military financing and education and anti-American terrorism may be due to omitted variables.

To assess the impact of unobservables, we use the method developed by Oster (2019). We evaluate the sensitivity of our OLS estimates to the inclusion of observable control

Table 2: Selection on Observables

Specification	(1) Baseline Effect	(2) Controlled Effect	(3) Bias-adjusted β	(4) Bias-adjusted β	(5) Bias-adjusted β	(6) Bias-adjusted β
Fixed Effects	0.027	0.031	0.031	0.033	0.051	0.088
	$[R^2 = 0.01]$	$[R^2 = 0.08]$	$[R_{max} = 0.10]$	$[R_{max} = 0.15]$	$[R_{max} = 0.25]$	$[R_{max} = 0.50]$
Fixed Effects &	0.030	0.029	0.030	0.032	0.036	0.054
Baseline Controls	$[R^2 = 0.01]$	$[R^2 = 0.08]$	$[R_{max} = 0.10]$	$[R_{max} = 0.15]$	$[R_{max} = 0.25]$	$[R_{max} = 0.50]$

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 fixed effects and baseline controls. Columns (3) through (6) 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). All estimates assume equal selection on observed and unobserved variables (i.e., δ= 1.)

variables, using the associated regression results to derive bounds for the possible bias arising from unobservable variables. We can contrast the shift in the R^2 value between the scenario without control variables and the one with these 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 R_{max}, which is equal to the maximum value of the R^2 if all potential determinants (observables and unobservables) would be included, and 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. Oster (2019) proposes to work with the assumptions that selection on observables and unobservables ought to be equal (i.e. $\delta = 1$) and 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. Using these assumptions, we find that the bias-adjusted regression coefficient associated with military aid is positive and largely unchanged. Moreover, even when we assume an R_{max} that is implausible for a linear probability model (i.e., $R_{max} = 0.5$), we obtain a bias-adjusted coefficient that is larger than zero. In sum, these results indicate that a common source of endogeneity—selection/omitted variable bias—does not matter strongly to our OLS estimates.

4. Instrumental-Variable Estimates

4.1. Empirical Model

In addition to omitted variables, endogeneity concerns associated with estimating Equation (2) may also arise due to measurement error and reverse causality. Given that the military aid data come from official U.S. budgetary data, measurement error in the main explanatory variable seems unlikely. However, endogeneity due to reverse causation is certainly possible: 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 upward-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 aid recipients into intensifying their fight against terrorism. To address these concerns, we estimate the following two-stage instrumental-variable system:

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

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

In Equation (3a), we predict the potentially endogenous variable *aid* using our instrumental variable, *regaid*, the baseline controls 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 *regaid* to account for skewness.

Instrument Construction. Our instrumental variable (IV), *regaid*, is the sum of U.S. military aid to countries proximate to the country of interest, net of local military aid. For our main explanatory variable of interest, U.S. military aid for financing and training, it is constructed as follows:

1. For each country i, we determine in which part of the world it is located. Using the

World Bank's classification, the ten world regions we consider are Eastern Europe and Central Asia; Latin America; the Caribbean; the Pacific region; the Middle East and Northern Africa; sub-Saharan Africa; Western Europe and Northern America (i.e., Canada); East Asia; South-East Asia; and South Asia.

2. For each world region, we calculate the aggregate amount of military aid for foreign military financing and training (regional aggregate).

3. From this regional aggregate, we subtract the respective amount of military aid going to country i itself. The residual regional aid is our instrumental variable *regaid*.[15] For instance, to construct the instrument for Colombia in 1980, we first calculate the aggregate amount of military aid for foreign military financing and training going to countries in Latin America in this year (regional aggregate) before subtracting foreign military financing and training aid going to Colombia in the same year. The difference is our instrument for local foreign military financing and training to Colombia in 1980.

Instrument Relevance and Exclusion Restriction. We instrument local military aid by military aid given to countries located in the same world region. To re-iterate the discussion of our IV-strategy in the introduction, the local provision of military aid is guided by country-specific U.S. interests (i.e., economic, political or geo-strategic concerns of the United States). These interests, however, are not observed. At the same time, there is usually also a region-specific dimension to U.S. interests. That is, region-specific factors (e.g., common political history or geographical proximity to the United States or its geo-strategic adversaries) can explain why different world regions receive specific levels of U.S. military aid. Consequently, we use variation in region-specific military aid to approximate U.S. regional interests and to instrument local aid. For one, if there is a region-specific dimension to U.S. aid-giving, this instrument ought to be sufficiently relevant, with regional aid (net of local military aid) positively predicting local aid. For another, region-specific aid also ought to be exogenous to the local aid-terrorism nexus. Decisions concerning the region-specific distribution, composition and level of aid are made in the United States, e.g., because of budgetary and bureaucratic considerations as well as deliberations by the

[15] We also construct analogous instrumental variables when we instrument local total military aid or local military aid for programs other than military financing and education. That is, for these aid variables, the instruments are equal to regional total military aid (regional aid not used for military financing and education), net of local total military aid (local aid not used for military financing and education).

U.S. executive and Congress, where such considerations, in turn, are influenced by other U.S. stakeholders (mainly, the U.S. electorate). Thus, the impact of foreign countries on regional aid vis-à-vis the influence of various U.S. stakeholders ought to be sufficiently negligible as to make regional military aid exogenous to local conditions.

Below, we provide various robustness checks to bolster a causal interpretation of our empirical results, especially concerning the validity of the exclusion restriction. For instance, we rely on the plausibly exogenous framework of Conley et al. (2012), control for various regionally correlated shocks (i.e., we consider spillovers that could constitute alternative pathways from the instrument to terrorism), use placebo instruments and assess the influence of potentially important outliers in the aid-terrorism relationship (e.g., Afghanistan, Iraq or Israel). We also employ an alternative IV-approach inspired by Nunn and Qian (2014), where we exploit cross-sectional variation in a country's probability to receive military aid and plausibly exogenous time-series variation in U.S. political fractionalization (which ought to affect U.S. aid policies through legislative logrolling) to estimate the causal effect of military aid on anti-American terrorism.

4.2. 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 aid for military financing and education result in a greater likelihood of anti-U.S. terrorism. The inclusion of additional controls does not meaningfully influence the size or direction of the effect of aid on anti-U.S. terrorism. Moreover, we show that military aid for other purposes does not have a statistically significant effect on anti-American terrorism, while total U.S. military aid does. These findings speak to our OLS estimates in that the relationship between U.S. aid and anti-American terrorism is driven by aid for military financing and training.

To assess the trustworthiness of our IV-estimates concerning the impact of military financing/education aid we can consider a number of diagnostic checks. Here, we find that our instrument is a strong and positive predictor of local U.S. military aid for financing and training. The effective F-statistics surpass the usual threshold of $F = 10$ that would signal instrument weakness. However, this threshold has received some criticism for being potentially anti-conservative, meaning that instruments may be weak even if $F > 10$ (e.g., Lee et al. 2022). 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 endogenous regressor in the structural equation is not equal to zero. For another, the Anderson-Rubin confidence intervals are similar to the standard IV-intervals, firmly indicating statistical significance at conventional levels.

In comparison to the OLS results, the effect of military aid on anti-American terrorism is approximately twice as large. This could imply that the OLS estimates 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 to extract additional policy concessions (i.e., a tougher stance on anti-American terrorism) from the recipient country.

With respect to the economic significance of our findings, using the baseline specification (specification (2) of Table 3), we find—ceteris paribus—that a ten-percent increase in military aid for financing and education increases the likelihood of the aid-receiving country to generate at least one incident of anti-American terrorism by 0.79 percent. At the sample-mean, doubling this type of aid (to approximately 92 million US$) results in a 7.9 percentage point increase in the incidence of anti-American terrorism, which is approximately 50.9% of the mean incidence of anti-American terrorism. This points to an unfavorable and economically substantive effect of military aid on anti-American terrorism.

4.3. Robustness of Instrumental-Variable Approach

In this sub-section, we evaluate the robustness of our IV-approach—thereby assessing the causal assertions related to the effect of aid for military financing and training on anti-U.S. terrorism—through several methods, probing instrument relevance and the exclusion restriction. We discuss these robustness checks briefly below and in more detail in Appendix B.

First, in Appendix B.1 we consider *alternative constructions of our instrument.* For example, we identify countries that are geographically close to our country of interest from different world regions, which we then consider when constructing the instrument. Furthermore, we augment our IV-approach with additional "internal" instruments constructed according to the method of Lewbel (2012). As shown in Table B.1, using these alternative instruments and/or additional instruments yields results that are similar to our baseline ones, where higher levels of U.S. military aid lead to more anti-American terrorism.

Table 3: Instrumental-Variable Regression Results

	(1)	(2)	(3)	(4)	(5)
Military Aid	0.073***	0.079***	0.081***		
	(0.024)	(0.024)	(0.024)		
Other Military Aid				0.034	
				(0.056)	
Total Military Aid					0.052***
					(0.018)
Civil Conflict		0.107***	0.107***	0.111***	0.111***
		(0.028)	(0.029)	(0.027)	(0.026)
Child Mortality		0.023	0.022	0.062*	0.045
		(0.036)	(0.037)	(0.033)	(0.034)
Democracy		0.001	0.001	0.011	0.003
		(0.012)	(0.012)	(0.011)	(0.012)
Population Size		0.170***	0.173***	0.139**	0.136***
		(0.043)	(0.042)	(0.065)	(0.039)
Globalization		-0.005**	-0.003	-0.002	-0.003*
		(0.002)	(0.012)	(0.002)	(0.002)
First-Stage Regression Results					
Instrument	0.163***	0.166***	0.150***	0.051***	0.190***
	(0.030)	(0.031)	(0.030)	(0.019)	(0.032)
Civil Conflict		0.070	0.065	0.020	0.056
		(0.117)	(0.115)	(0.137)	(0.152)
Child Mortality		0.595**	0.604**	-0.172	0.460
		(0.283)	(0.265)	(0.182)	(0.312)
Democracy		0.161**	0.149**	0.020	0.184**
		(0.080)	(0.076)	(0.066)	(0.092)
Population Size		-0.150	-0.301	0.840***	0.406
		(0.262)	(0.245)	(0.286)	(0.329)
Globalization		0.012	0.174***	-0.021	0.001
		(0.013)	(0.032)	(0.142)	(0.019)
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Control for Remaining Aid	No	No	Yes	Yes	No
F-Stat	30.36	29.10	25.87	7.70	35.27
AR Test p-value	0.00	0.00	0.00	0.56	0.01
AR 95% Confidence Interval	[0.027	[0.031	[0.034	[−0.136	[0.013
	0.131]	0.132]	0.139]	0.178]	0.089]
Fisher Test p-value	0.00	0.00	0.00	0.00	0.00
Observations	8,001	7,121	7,121	7,121	7,121

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Fisher Test* = Choi's Fisher-type panel unit root test. Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Second, we employ several *placebo instruments* (see Appendix B.2 for details regarding their construction). For instance, we randomly assign the values of the instrument to another country for a specific year. Due to randomization, such instruments should no longer be helpful in identifying the impact of U.S. military aid on anti-U.S. terrorism. As shown in Table B.2, the placebo instruments are indeed unable to identify the effect of military aid on anti-U.S. terrorism (the associated IV-diagnostics point to instrument weakness).

Third, an important threat to the validity of our IV-approach comes from the potential impact of *regionally correlated shocks* of an economic, political or demographic nature. Such shocks may affect the construction of our instrument, influence the provision of military aid to and/or matter to anti-American terrorism. For instance, the flaring up of anti-Americanism in proximate countries may impact the local level U.S. aid-giving; at the same time, this may also motivate anti-American terrorism in the country of interest, e.g., by means of the cross-border diffusion of terrorist tactics. To account for such factors, we run additional robustness checks where we control for a variety of regionally correlated shocks. Their construction is discussed in Appendix B.3. For instance, we control for regional levels of economic growth, democracy, population or anti-American terrorism. As shown in Table B.3, we do not find that accounting for such shocks changes our main empirical conclusions, which further raises confidence in the validity of our IV-approach.

Finally, we probe the assumption of instrument exogeneity by means of the *plausibly exogenous framework* of Conley et al. (2012). This method relaxes the assumption of perfect instrument exogeneity, instead allowing for violations of the exclusion restriction (e.g., due to regionally correlated economic or political shocks). We discuss the methodology, its application to our case and our empirical findings in more detail in Appendix B.4. In short, employing the plausible exogenous method, we find that even when allowing for considerable violations of the exclusion restriction, there is still robust evidence that the provision of U.S. military aid results in more anti-American terrorism.

4.4. Alternative Instrumental-Variable Approach

Using an alternative IV-approach and getting similar results ought to improve the credibility of our instrumental variable estimates. Thus, we also employ an alternative instrumental variable inspired by Nunn and Qian (2014). They study the effect of U.S. food aid on conflict in aid-receiving countries, instrumenting food aid by the interaction between a country's mean-probability of receiving aid and U.S. wheat production. Analogous to

this, our alternative instrument is the interaction between a country's mean-probability to receive U.S. aid for military financing and education and U.S. political fractionalization. Ahmed (2016) and Dreher et al. (2019) show that political fractionalization in aid-sending countries affects the provision of aid. For instance, fractionalization (i.e., partisan politics) impacts the provision of aid by influencing the budgeting process through, e.g., legislative logrolling. In sum, with our instrument we exploit time variation in U.S. specific political conditions 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. We also discuss our alternative IV-approach in more detail in Appendix B.5.

As reported in Table B.4, our alternative instrument shows that increases in U.S. military aid lead to more anti-American terrorism. Estimated effect sizes are similar between our baseline and alternative IV-approach. Thus, both approaches point to a single interpretation of the data, where higher levels of U.S. assistance for military financing and training lead to more anti-American terrorism.[16]

4.5. Further Robustness Checks

We also probe the robustness of our empirical finding 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.

First, we study how changes to our baseline model affect our estimates. For instance, we use a country's per capita income rather than the level of child mortality as a control for economic development. Here, we also examine whether dropping potentially suspect confounders ("bad controls") and using different standard error choices affect our results. The various changes to our baseline model are discussed in Appendix C.1. Second, we add further controls to our main specification. For instance, we also consider the roles of economic growth and international conflict. More information on the additional controls is given in Appendix C.2. Third, we evaluate the impact of specific foreign policy variables on our results. For instance, such variables include the deployment of U.S. troops, the

[16]Our alternative instrumental variable can be considered a variant of a shift-share-instrument. These instruments have received critical attention in recent years (Christian and Barrett, 2017; Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). Thus, in Appendix B.5 we also provide some related robustness checks to support the validity of our alternative IV-estimates.

provision of U.S. economic aid and the voting behavior of aid-receiving countries at the United Nations. More information on these policy variables is given in Appendix C.3. Finally, we assess how changes to the lag structure affect our empirical findings, amending our model with lags of the dependent variable and deeper lags of the military aid variable. As reported in Table C.1 (changes to baseline model), Table C.2 (additional covariates), Table C.3 (foreign policy variables) and Table C.4 (lag structure), the various alternative empirical choices do not matter to our main empirical finding that more U.S. aid for military financing and education induces more anti-American terrorism.

Furthermore, our results may be driven by the measurement of anti-U.S. terrorism or U.S. military aid. Concerning the former, we use alternative measurements of anti-American terrorism (e.g., measuring terrorism as a continuous count rather than dichotomous variable), 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. Concerning the latter, we employ different operationalizations 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. Further discussion of the alternative measurements of terrorism and aid is given in Appendix C.5 and Appendix C.6, respectively. As shown in Table C.5 (measurement of terrorism) and Table C.6 (operationalization of military aid), a terror-inducing effect of U.S. military aid on anti-American terrorism remains regardless of how we operationalize aid or terrorism.

Finally, it is possible that specific sub-samples drive our main results. Thus, we sequentially drop from our sample sub-sets 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; these countries are especially prone to anti-American terrorism and receiving U.S. aid. We further discuss the various sub-samples in Appendix C.7. We also consider whether potentially influential country-cases (e.g., Israel, Iraq, Afghanistan or Pakistan) affect our estimates by sequentially dropping them from our sample. For instance, these countries may exert an influence on U.S. aid policy that is strong enough to question the validity of our IV-approach. Moreover, there were two important international events that may have affected both U.S. military aid and anti-American terrorism: the Cold War and the War on Terror (e.g. Fleck and Kilby, 2010). We therefore also study whether there are systematic differences in the nexus between U.S. military aid and anti-American terrorism before and after these events. As shown in Tables

C.7 and C.8, we do not find that specific sub-samples of countries, country-cases or time periods drive our results.

4.6. U.S. Military Aid and Other Types of Terrorism

Next, we assess how U.S. military aid affects other types of terrorism. For one, we consider transnational terrorism directed against the U.K. as an important ally of the U.S. For another, we study the effect of U.S. military aid on the USSR/Russia (after 1992) as a geo-political enemy of the United States. Finally, we use data from the *GTD* and Enders et al. (2011) to investigate the role of military aid in total terrorism (i.e., terrorism by domestic, transnational or unknown actors against domestic or transnational targets) and domestic terrorism (i.e., terrorism by domestic actors against domestic targets). All terrorism measures are indicator variables equal to unity when a country-year pair sees at least one anti-British, anti-Russian, total or domestic attack, respectively, and zero otherwise.

Employing our usual IV-approach and controlling for the incidence of anti-American terrorism, we show in Table 4 that U.S. aid for military financing and education also leads to more anti-British terrorism. 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. By contrast, there is no evidence that U.S. military aid affects the patterns of transnational terrorism directed against the USSR/Russia. This is also an important placebo test: Finding that U.S. military aid does not share the same relationship with transnational terrorism against a hostile nation provides further evidence that our identification strategy is 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 terrorist violence.

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

Terrorism Type	(1) Anti-UK	(2) Anti-Russia	(3) Total	(4) Domestic
Military Aid	0.049***	0.012	0.051	0.058
	(0.017)	(0.008)	(0.043)	(0.036)
Civil Conflict	0.038**	0.056***	0.206***	0.197***
	(0.019)	(0.018)	(0.031)	(0.034)
Child Mortality	-0.012	0.007	0.037	-0.016
	(0.024)	(0.015)	(0.059)	(0.056)
Democracy	0.000	0.004	0.011	-0.001
	(0.007)	(0.005)	(0.016)	(0.017)
Population Size	0.032	0.020	0.236***	0.199***
	(0.028)	(0.017)	(0.084)	(0.074)
Globalization	-0.002	-0.002**	-0.001	-0.001
	(0.001)	(0.001)	(0.004)	(0.004)
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Control for Anti-American Terrorism	Yes	Yes	Yes	Yes
F-Stat	29.46	29.79	29.51	30.41
AR Test p-value	0.00	0.11	0.25	0.12
Observations	7,072	7,095	7,121	6,795

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Control for Anti-American Terrorism*= dummy variable reflecting occurrence of anti-American terrorism. Data for the United Kingdom and Russia is excluded when we consider anti-UK and anti-Russia terrorism, respectively. Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

5. Exploration of Transmission Channels

5.1. Model and Variables

So far, we provided a broad battery of evidence that more U.S. military aid leads to more anti-U.S. terrorism. Two channels can explain this effect. First, military aid may promote 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 (strategic logic channel). Second, military aid may encourage anti-U.S. terrorism by those who are adversely affected by weaker institutions and exclusionary policies due to aid (grievances channel).

In this section, we explore whether U.S. military aid indeed affects state capacity and/or grievances, estimating the following two-stage equation system:

$$aid_{it} = \delta \times globalaid_{it} + \mu \times X_{it} + \alpha_i + \tau_t + \epsilon_{it} \tag{4a}$$

$$channel_{jit} = \beta \times \widehat{aid_{it}} + \gamma \times X_{it} + \alpha_i + \tau_t + \epsilon_{it} \tag{4b}$$

We employ our usual IV-approach to instrument military aid, controlling for the usual baseline covariates and fixed effects. We use the first-stage regression results from Equation (4a) to estimate in Equation (4b) the causal effect of U.S. military aid on the various transmission variables (*channel*) which we introduce 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 come from the *National Material Capabilities Dataset (NMC)* (updated from Singer, 1988). Furthermore, from the *VDEM Dataset* we extract a variable measuring a *state's control over its territory* as another indicator of military capacity; this control might be challenged by criminals, warlords or other sub-national actors. To indicate administrative capacity, we use two variables that indicate the *rigor and impartiality of public administration* (reflecting the quality of bureaucracy) and the *rule of law*. For both indices, higher values mean better institutions, where the data come from the *VDEM Dataset*.

Grievances Variables. We employ four variables reflecting rent-seeking behavior, institutional quality 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 more equal access. Second, we consider the *political exclusion of the poor*, which reflects how strong access to political power is due to one's socio-economic position, with higher values indicating more exclusion. Third, we measure the extent of *regime corruption*, using an index that accounts for executive, judicial and legislative corruption. Higher values of this index mean more corruption. Finally, we employ an index of *clientelism*, which reflects how prevalent the exchange of resources for political support is in society (higher values imply more clientelism).

5.2. Empirical Results

We report our IV-results concerning the effect of military aid on the transmission variables in Table 5 (Panel A). In Table 5, we also report the correlation between the mediator and the likelihood of anti-U.S. terrorism to provide tentative evidence for mediation (Panel B).

Concerning the state capacity variables, we find evidence that higher levels of military capacity but not administrative capability share a negative association with anti-American terrorism (Panel B). At the same time, however, we find that higher levels of U.S. military aid *reduce* local military (but not administrative) capacity (Panel A). Thus, while military capacity—as expected—deters anti-American terrorism, we find no evidence that U.S. military aid bolsters local state capacity. Rather, aid appears to undermine it. For instance, aid-receiving countries may rely on U.S. military aid to excessively shirk local military spending. In turn, this suggests that the unfavorable relationship between U.S. military aid and anti-American terrorism cannot be due to the strategic logic channel, which posits that anti-American terrorism ought to be the consequence of the 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, political access of the poor, regime corruption and clientelism (Panel A). We also find that exclusion, corruption and clientelism unfavorably correlate with anti-American terrorism (Panel B). This provides evidence in favor of the grievances channel. Military aid induces rent-seeking behavior, exclusionary policies and weaker institutions, encouraging anti-American terrorism due to lower opportunity costs of violence for those adversely

Table 5: Potential Transmission Channels

Panel A: Effect of Military Aid on Mediators								
	(1a)	(2a)	(3a)	(4a)	(5a)	(6a)	(7a)	(8a)
Mediator →	Military Capacity	Territorial Control	Public Admin.	Rule of Law	Resource Distrib.	Exclusion of Poor	Regime Corruption	Clientelism
Military Aid	-0.190**	-1.209**	-0.109	-0.022*	-0.030***	0.022**	0.068***	0.055***
	(0.076)	(0.571)	(0.073)	(0.012)	(0.011)	(0.011)	(0.020)	(0.020)
F-Stat	32.12	29.10	29.10	29.10	29.10	29.10	29.10	29.10
AR Test p-value	0.00	0.02	0.12	0.06	0.01	0.06	0.00	0.00
Panel B: Correlation between Mediators and Anti-American Terrorism								
	(1b)	(2b)	(3b)	(4b)	(5b)	(6b)	(7b)	(8b)
Mediator	-0.066***	-0.008***	-0.015	-0.074	-0.102	0.298***	0.180**	0.137*
	(0.019)	(0.001)	(0.011)	(0.073)	(0.085)	(0.112)	(0.072)	(0.072)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,687	7,119	7,121	7,121	7,121	7,121	7,121	7,121

Notes: IV-estimates (Panel A) and OLS-estimates (Panel B) reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Baseline Controls* for civil conflict, child mortality, democracy, population size and globalization. Cluster-robust standard errors in parentheses. Information on fixed effects, baseline controls and number of observations are the same for Panel A and Panel B. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

affected by these developments. Moreover, the adverse effect of military aid on institutional quality may also partly explain how aid undermines military capacity. For instance, higher levels of corruption and clientelism due to aid may also adversely affect arms procurement and promotion policies in the armed forces.

5.3. U.S. Military Aid and Public Opinion

Following Krueger and Malečková (2009), we also examine the relationship between aid and public opinion. We use data from the *Gallup World Poll* (Gallup, 2018) which provides continual and representative survey data of residents in most countries of the world. We aggregate the individual survey data at the country-level. As the survey question we are interested in was not asked in each year and/or country, we furthermore average the public opinion data over the 2006–2016 period for which we have data, leaving us with one observation per country.

We consider the following survey item: "Do you approve or disapprove of the job performance of the leadership of the United States?" Higher values of this variable mean

more dissatisfaction with the U.S. government and its foreign policy. Unsurprisingly, anti-American resentment is exceptionally high in countries such as Iran and Russia; however, it is also very high in countries that regularly receive large amounts of U.S. military aid such as Greece, Turkey and Pakistan.

Using our usual IV-approach, in Table 6 we show that higher levels of U.S. military aid lead to greater disapproval of the U.S. government's leadership (Panel A). We also show that disapproval of the United States correlates with a greater likelihood of anti-American terrorism (Panel B). This finding is also in line with Krueger and Malečková (2009) who find a higher incidence of international terrorism when people from one country disapprove of the leadership of another country.

Table 6: Military Aid and Public Opinion

Panel A: Effect of Military Aid on Disapproval of U.S. Government		
	(1a)	(2a)
U.S. Military Aid	0.106***	0.077***
	(0.035)	(0.024)
F-Stat	23.05	32.09
AR Test p-value	0.00	0.00
Panel B: Correlation between Public Opinion and Anti-American Terrorism		
	(1b)	(2b)
Disapproval U.S. Government	0.256***	0.352***
	(0.080)	(0.088)
Observations (Both Panels)	157	151
Baseline Controls (Both Panels)	No	Yes

Notes: IV-estimates (Panel A) and OLS-estimates (Panel B) reported. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Baseline Controls* for civil conflict, child mortality, democracy, population size and globalization. Robust standard errors in parentheses. Constant not reported. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

The evidence in this sub-section builds on rather sparse data. However, it provides tentative evidence that U.S. military aid leads to greater dissatisfaction with the United States, potentially because U.S. aid aggravates local socio-economic and political grievances.

6. Conclusion

We study the effect of U.S. military aid on anti-American terrorism in recipient countries for a sample of 174 countries between 1968 and 2018. Our empirical analysis enables us to challenge the frequently argued notion by U.S. policymakers that U.S. military aid enhances U.S. security. Rather, we show that higher levels of aid especially for military financing and training cause higher levels of anti-American terrorist activity in recipient countries. This finding survives a battery of robustness checks.

Exploring potential transmission channels, we find that more U.S. military aid reduces military capacity in recipient countries, while also contributing to greater economic-political exclusion and corruption. These findings speak to the grievances channel we highlighted in the paper: Social groups that are negatively affected by weakened institutions and increased exclusion due to U.S. aid direct their dissatisfaction against the United States as the perceived linchpin of an unfavorable status quo. Indeed, we also provide tentative evidence that more U.S. military aid unfavorably affects public opinion towards the United States in recipient countries. The results from our preferred instrumental-variable specification suggest that at the sample-mean, doubling U.S. military aid for financing and education increases the risk of anti-American terrorism by 7.9 percentage points, which is approximately 50.9% of the sample-mean incidence of anti-American terrorism. This points to substantial and adverse security effects of this type of U.S. military aid, especially given that the U.S. hands out billions of US$ in aid per year.

Our results show that military aid can backfire. This finding calls into question its effectiveness as a counter-terrorism tool. At the same time, 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 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 single acts of anti-U.S. terrorism can have 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 250 million US$. 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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Appendix A. Summary Statistics and Sample

Table A.1: Summary Statistics

Variable	N*T	Mean	Standard Deviation	Minimum	Maximum
Military Aid (Total)	7,121	71.47	489.0	0	11.56
Military Aid (Financing and Education)	7,121	46.60	347.0	0	10.47
Military Aid (Other)	7,121	24.88	332.3	0	11.53
Anti-American Terrorism (Count)	7,121	0.48	2.25	0	88
Anti-American Terrorism (Binary)	7,121	0.15	0.36	0	1
Civil Conflict	7,121	0.19	0.40	0	1
Child Mortality	7,121	71.64	71.30	2.20	375.80
Democracy	7,121	1.36	1.08	0	3
Population Size	7,121	35.14	127.80	0.12	1,393
Globalization	7,121	50.83	17.43	14.15	90.98
Anti-UK Terrorism (Binary)	7,072	0.05	0.21	0	1
Anti-Russian Terrorism (Binary)	7,095	0.02	0.16	0	1
Total Terrorism (Binary)	7,121	0.51	0.50	0	1
Domestic Terrorism (Binary)	6.795	0.37	0.48	0	1
Military Capacity (PCA Variable)	5,687	-0.04	0.91	-0.52	8.80
Territorial Control	7,119	91.08	10.39	31.11	100
Impartial and Rigorous Public Administration	7,121	0.27	1.54	-3.75	4.01
Rule of Law	7,121	0.52	0.31	0.01	0.99
Equal Distribution of Resources	7,121	0.57	0.29	0.02	0.99
Exclusion of the Poor	7.121	0.44	0.28	0.01	0.98
Regime Corruption	7,121	0.50	0.31	0.01	0.98
Clientelism	7,121	0.50	0.27	0.02	0.98

N*T for sample with baseline controls, trends and fixed effects (sample of 162 countries). See the main text for construction and potential transformation of variables.

Table A.2: List of Countries

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

* indicates that country is only included in the parsimonious sample without control variables.

Appendix B. Robustness of Instrumental-Variable Approach

Appendix B.1. Alternative Construction of Instrument

We employ four alternative definitions of our instrument. For the first two instruments, we alter the world region around the country of interest. In detail, rather than differentiating between ten world regions, we consider six or 19 world regions, respectively. For the other two instruments, we define the alternative instrument as being equal to the sum of U.S. military aid to the rest of the world minus military aid to the country of interest or as being equal to the sum of U.S. military aid to the rest of the world minus military aid to the world region in which the country of interest is located, respectively.

Furthermore, we augment our usual instrument (U.S. aid for military financing and education provided to countries located in the same world region as the country of interest) with additional "internal" instruments constructed following Lewbel (2012). Here, identification is achieved by having regressors that are uncorrelated with the product of heteroskedastic errors, which is a feature of many models where error correlations are due to an unobserved common factor (Lewbel, 2012, p.67). In detail, this procedure involves (i) a regression of the military aid variable on the vector of controls (X) and fixed effects, (ii) storing the residual from this regression, (iii) mean-centering all controls in X and (iv) multiplying the residual from (ii) with the respective mean-centered variables from (iii), where this vector of products are the generated internal instruments. While the residual from (ii) has zero covariance with each of the regressors used to construct it, its products with the centered regressors from (iv) will contain sizable elements if there is evidence of heteroskedasticity, thus allowing for identification via heteroskedasticity.

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. However, instrumenting local aid with aid to the rest of the world appears to produce a rather weak instrument. This speaks to our idea that the United States provides military aid to specific parts of the world following its own strategic rationale (which we approximate via our usual instrument); this rationale gets blurred when global aid flows are used instead. Finally, using the "internal" instrument proposed by Lewbel (2012), either alone (specification (5)) or in combination with our baseline instrument (specification (6)), we also find that higher levels of U.S. aid for military financing and education lead to more anti-American terrorism. Using these additional

Table B.1: Alternative IV-Construction

	(1)	(2)	(3)	(4)	(5)	(6)
Military Aid	0.076***	0.101***	0.027***	0.099*	0.049***	0.058***
	(0.021)	(0.035)	(0.010)	(0.059)	(0.014)	(0.013)
Civil Conflict	0.107***	0.104***	0.113***	0.105***	0.110***	0.109***
	(0.028)	(0.028)	(0.028)	(0.029)	(0.028)	(0.028)
Child Mortality	0.025	0.009	0.057*	0.010	0.043	0.038
	(0.036)	(0.044)	(0.031)	(0.055)	(0.031)	(0.031)
Democracy	0.002	-0.004	0.013	-0.004	0.008	0.006
	(0.012)	(0.014)	(0.011)	(0.020)	(0.011)	(0.011)
Population Size	0.170***	0.168***	0.173***	0.168***	0.171***	0.171***
	(0.044)	(0.045)	(0.046)	(0.044)	(0.044)	(0.044)
Globalization	-0.004**	-0.005**	-0.003	-0.005**	-0.004*	-0.004*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	24.96	12.74	2.14	5.72	17.90	25.51
AR Test p-value	0.00	0.00	0.28	0.04	0.00	0.00
Hansen Test p-value					0.23	0.23
Observations	7,121	7,121	7,121	7,121	7,121	7,121

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Hansen Test* = Hansen test of overidentifying restrictions. Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

instruments also allows us to run the Hansen test of overidentifying restrictions. Here, we cannot reject the null hypothesis that the overidentifying restrictions are valid, which further bolsters our IV-results.

Appendix B.2. Placebo Instrument

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, U.S. military aid to proximate countries 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 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 from 1970 to the year 1985, its values for the year 2010 to 1992, and so on. Third, we randomly assign a world region to a country before constructing our instrumental variable. For instance, this means to "re-locate" Egypt to the Caribbean, while assigning Haiti to South–East Asia, and so on.

We show our findings using the placebo instruments in Table B.2. 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. At the same time, this finding speaks to the notion that our initial identification strategy is sound.

Table B.2: Placebo Instruments

	(1)	(2)	(3)
Military Aid	0.148	-0.051	0.190
	(0.394)	(0.254)	(0.537)
Civil Conflict	0.099*	0.122***	0.094
	(0.052)	(0.044)	(0.072)
Child Mortality	-0.022	0.109	-0.050
	(0.267)	(0.173)	(0.359)
Democracy	-0.015	0.032	-0.025
	(0.092)	(0.062)	(0.131)
Population Size	0.166***	0.177***	0.163**
	(0.056)	(0.059)	(0.068)
Globalization	-0.007	-0.000	-0.008
	(0.012)	(0.008)	(0.017)
Country FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
F-Stat	0.73	1.16	0.22
AR Test p-value	0.70	0.83	0.68
Observations	7,121	7,121	7,121

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Appendix B.3. Regional Trends

The validity of our instrumental-variable approach rests on the assumption that regional U.S. military aid only matters to local anti-American terrorism by affecting the local provision of U.S. military aid. However, there may exist economic, political or demographic shocks that are regionally correlated. Such shocks may affect the construction of our instrument, influence the provision of military aid to and/or matter to anti-American terrorism.

Below, we control for several time-varying economic and political shocks within a specific world region. In detail, we control for regional shocks in civil conflict, economic and political development, population size, economic growth, political proximity to the United States, U.S. troop presence, and anti-American terrorism. With respect to those variables not already introduced in the main text, data on the per capita rate of economic growth is from the WDI. Political proximity to the United States is operationalized as the difference between a sample country's ideal policy position and that of the United States, where this

Table B.3: Role of Regional Shocks

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Military Aid	0.078***	0.081***	0.083***	0.079***	0.069**	0.091***	0.080***	0.065***
	(0.025)	(0.024)	(0.024)	(0.024)	(0.033)	(0.030)	(0.027)	(0.024)
Regional Civil Conflict	0.018							
	(0.097)							
Regional Child Mortality		0.057**						
		(0.029)						
Regional Democracy			-0.018					
			(0.031)					
Regional Population Size				0.030				
				(0.058)				
Regional Economic Growth					0.001			
					(0.004)			
Regional Affinity to U.S.						-0.018		
						(0.023)		
Regional U.S. Troop Presence							-0.018	
							(0.201)	
Regional Anti-American Terrorism								0.044***
								(0.010)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	27.94	27.69	32.84	29.82	14.81	20.78	24.04	28.55
AR Test p-value	0.00	0.00	0.00	0.00	0.07	0.00	0.00	0.02
Observations	7,116	7,116	7,116	7,116	6,596	6,939	7,116	7,116

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Baseline Controls* are for civil conflict, child mortality, democracy, population size and globalization. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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, data on U.S. troop presence is from an update of Kane (2012). Formally, all variables reflecting regional shocks are defined as the mean levels of civil conflict, child mortality, democracy, population size, economic growth, voting with the U.S. at the United Nations, U.S. troop presence and anti-American terrorism in countries proximate to the country of interest. That is, they are constructed in the same way as our instrumental variable.

Our results are reported in Table B.3. 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 aid-receiving countries. The associated IV-diagnostics and findings for the various controls are also stable and sound across the various specifications.

Appendix B.4. Plausibly Exogenous Framework

The main idea of the plausibly exogenous method of Conley et al. (2012) is to relax the assumption of perfect instrument exogeneity. It involves the following 2SLS model:

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

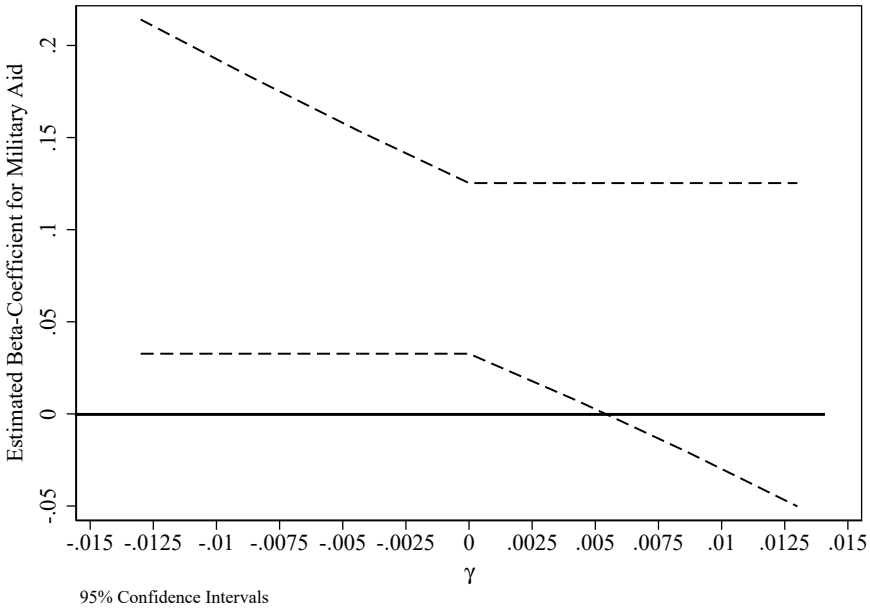
$$terror_{it} = \beta_2 \times \widehat{aid_{it}} + \gamma \times regaid_{it} + \mu_2 \times X_{it} + \alpha_{2,i} + \tau_{2,t} + \epsilon_{2,it} \tag{B.2}$$

In contrast to our main IV-approach, we allow our instrument to enter the second-stage regression with a coefficient γ. That is, regional U.S. military aid for financing and training can now directly affect anti-American terrorism in the country of interest i, meaning that the exclusion restriction is violated; 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). We do so by employing the union of confidence interval approach proposed by Conley et al. (2012). For our analysis, we consider various values of γ, where the upper bound is chosen according to the reduced-form estimates associated with the role of regional aid in local anti-American terrorism. Here, this upper bound is $\gamma = 0.0131$, implying that higher levels of regional military aid correlate with higher levels of terrorism in the country of interest. While we assume per the exclusion restriction that this relationship is only due to the role of region-specific U.S. interests (approximated by regional military aid) in local aid, by means of the plausibly exogenous approach we can assess how robust our findings are to different degrees of instrumental invalidity. As a lower bound, we consider a value of $\gamma = -0.0131$, which would imply that more regional corruption is associated with less terrorism in the country of interest.

Figure B.1 presents the upper and lower bounds for the local military aid coefficient, applying the plausibly exogenous approach for our baseline empirical model. There are two main conclusions. First, if $\gamma < 0$, the upper bound effect of the local military on anti-U.S. terrorism becomes larger. Such a relationship might emerge when regional aid favorably correlates with certain predictors of anti-U.S. terrorism. Second, as long as $\gamma < 0.0053$, the 95% confidence interval does not include zero (for 90% CI, this threshold is $\gamma < 0.0066$). Taken together, the results suggest that γ has to reach a value that is in

Figure B.1: Plausibly Exogenous Framework



excess of 40.5% of the size of the reduced-form estimate for the coverage area to include zero (50.4% of the reduced form effect size for the 90% CI). This, in turn, suggests that our IV-estimates appear to be robust to high degrees of instrumental invalidity.

Appendix B.5. Alternative Shift-Share Instrument

Our alternative instrument is inspired by Nunn and Qian (2014) who examine the effect of U.S. food aid on domestic conflict by instrumenting aid by the interaction between the mean-probability to receive food aid and U.S. wheat production. Similarly, our instrument is the interaction between the mean-probability to receive U.S. military aid and the level of political fractionalization within the United States. For instance, for a country that receives U.S. military aid in 20 out of 50 years for which it is observed, U.S. political fractionalization will always be multiplied by a factor of 2/5. Electoral fractionalization itself is an index that can take values between 0 (minimal fractionalization) and 1 (maximal fractionalization), where the index accounts for the vote share and the number of political parties in the U.S. political system. The data are from Armingeon et al. (2020). In sum, in our alternative IV-approach we exploit plausibly exogenous time variation in U.S. electoral fractionalization 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.

Both Ahmed (2016) and Dreher et al. (2019) use political fractionalization in aid-giving countries to examine the effect of foreign aid on political repression and the out-flow of refugees in aid-receiving countries, respectively. Both studies argue that political conflict (as approximated by the level of fractionalization in the political system) affects government spending, e.g., due to increased legislative logrolling or stronger competition for voters that encourages government spending. Such effects on spending are also expected to influence the provision of foreign aid, meaning that the instrument ought to be sufficiently relevant.

At the same time, the level of fractionalization is anticipated to be shaped by U.S.-specific conditions (e.g., related to the economy or incumbency) rather than foreign policy (e.g., with respect to anti-American terrorism), making U.S. electoral fractionalization plausibly exogenous.

Still, U.S.-specific factors and/or foreign policy variables may matter to the local distribution of U.S. military aid. That is, U.S. economic and political cycles and foreign policy variables may be correlated with U.S. 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 two terms, interacting Democratic incumbency and U.S. economic growth rates with the mean-probability of a country receiving military aid, respectively. Data on U.S. economic growth come from the WDI. Furthermore, to account for foreign policy factors, we interact the year-fixed effects with the average level of U.S. economic aid (inverse hyperbolic sine transformed) and U.S. troops present in the country of interest, respectively. We describe these foreign policy variables in more detail in Appendix C.3. Note that for all interactions, their direct (uninteracted) effects are captured by the country- and year-fixed effects, respectively.

We report our findings in Table B.4. As for our main IV-approach reported in specifications (1) and (2), U.S. aid for military financing and education increases the likelihood of anti-American terrorism. For these specifications, the IV-diagnostics tend to point to instrument weakness, but the Anderson-Rubin diagnostics which are robust to weak instruments are sound. Here, the effect size associated with the effect of military aid on terrorism is approximately twice as large when using the alternative IV-approach compared to our

baseline IV-estimations, while statistical precision is lower. Furthermore, in specification (4) we show that using the interaction between legislative (rather than electoral) fractionalization and the mean-probability to receive aid as an instrument produces similar results. Data on legislative fractionalization are from Armingeon et al. (2020).

Our alternative instrumental variable can be considered a variant of a shift-share-instrument. These instruments have received critical attention in recent years (Christian and Barrett, 2017; Goldsmith-Pinkham et al., 2020; Borusyak et al., 2022). We address some related concerns as part of our robustness checks also reported in Table B.4. First, our IV-strategy may be susceptible to bias arising from spurious trends. Military aid, anti-American terrorism and U.S. political polarization may exhibit longer-run trends, meaning that these variables may be non-stationary, resulting in spurious regressions. We assess this possibility by running panel unit root tests on the regression residuals and the instruments as well as a time-series unit root test (the augmented Dickey Fuller test) on U.S.-specific electoral fractionalization. As shown in Table B.4, we never find that these variables are non-stationary, which ameliorates concerns about spurious regression.

Second, the "share"-component of our instrument (i.e., a country's mean-probability to receive U.S. military aid) may be subject to endogeneity concerns. For instance, as discussed in the main text, the probability to receive aid may be affected by local anti-American terrorism. We thus also present a specification (specification (3)) where we instrument local U.S. military aid by U.S. political fractionalization but do not interact this latter variable with a country's mean-probability to receive aid. This approach yields results that are comparable to those when we use the interacted instrument. However, the nature of this IV also means that we have to drop many potentially important fixed effects.

Finally, we create a placebo instrument that assigns U.S. electoral fractionalization to random years before interacting it with the mean-probability to receive aid. We show that this instrument performs poorly (specification (5)). Furthermore, we create a placebo military aid variable that is instrumented via our usual instrument (i.e., the interaction between U.S. electoral fractionalization and the mean-probability to receive military aid). In detail, this variable is created by randomly assigning U.S. military aid to another country in a given year, where both countries would have also received aid before randomization. We find that we cannot identify the causal effect of military aid on anti-American terrorism with sufficient precision when using the placebo aid variable. In sum, both attempts at randomization suggest that the mechanism associated with our shift-share-like IV-strategy

is indeed in effect and not our alternative IV-approach does not simply capture spurious associations.

Table B.4: Alternative IV-Approach

	(1)	(2)	(3)	(4)	(5)	(6)
U.S.-Specific IV →	Elect. Fract.	Elect. Fract.	Elect. Fract.	Legis. Fract.	Placebo	Elect. Fract.
Military Aid	0.195**	0.182**	0.105**	0.134**	0.029	
	(0.092)	(0.085)	(0.050)	(0.057)	(0.405)	
Placebo Military Aid						0.360
						(0.228)
Civil Conflict		0.097***	0.097***	0.101***	0.111***	0.200***
		(0.032)	(0.029)	(0.030)	(0.042)	(0.072)
Child Mortality		-0.060	0.021	-0.027	0.046	-0.122
		(0.080)	(0.042)	(0.060)	(0.283)	(0.129)
Democracy		-0.029	0.000	-0.016	0.013	-0.045
		(0.028)	(0.015)	(0.021)	(0.111)	(0.046)
Population Size		0.140**	0.131***	0.151***	0.174*	0.111
		(0.060)	(0.036)	(0.049)	(0.101)	(0.072)
Globalization		-0.008**	-0.009***	-0.006**	-0.002	-0.008
		(0.004)	(0.002)	(0.003)	(0.015)	(0.005)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	No	Yes	Yes	Yes
U.S. Controls	Yes	Yes	No	Yes	Yes	Yes
Foreign Policy Controls	Yes	Yes	No	Yes	Yes	Yes
F-Stat	4.56	5.07	17.11	7.83	1.43	3.17
AR Test p-value	0.02	0.01	0.03	0.02	0.98	0.00
AR 95% Confidence Interval	[0.061	[0.081	[0.015	[0.051	[*entire*	[0.101
	1.697]	1.253]	0.227]	0.434]	*grid*]	...]
Fisher Test p-value (Residual)	0.00	0.00	0.00	0.00	0.00	0.00
Fisher Test p-value (Instrument)	0.00	0.00	0.00	0.00	0.00	0.00
ADF Test p-value (U.S. Component)	0.05	0.05	0.05	0.02	0.00	0.05
Observations	7,121	7,121	7,121	7,121	7,121	7,121

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Fisher Test* = Choi's Fisher-type panel unit root test. *ADF test* = Augmented Dickey-Fuller unit root test on U.S. component of interacted instrument. *U.S. Controls* are for Democratic president and U.S. economic growth (both interacted with the probability to receive military aid). *Foreign Policy Controls* are the average amount of U.S. economic aid to and of U.S. troops present in the country of interest (interacted with the year-fixed effects). Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Appendix C. Further Robustness Checks

Appendix C.1. Changes to Baseline Model and Bad Controls

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 Coppedge et al. (2021). 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).

Furthermore, we consider the role of potentially bad controls, i.e., variables that we consider to be confounders even though they may also constitute outcomes associated with the inflow of U.S. aid. For instance, while we argue that civil conflict affects both the inflow of aid and the emergence of anti-American terrorism, one may argue that civil conflict itself is also a consequence of U.S. military aid. Thus, we successively drop from our regression models the potentially bad controls for civil conflict, child mortality, democracy, and globalization.

Finally, we calculate 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, dropping various potentially bad controls does not affect our main empirical conclusion. Rather, our main finding is remarkably stable to various model changes, suggesting that the impact of bad controls is benign. 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)
Military Aid	0.071***	0.076***	0.078***	0.085***	0.080***	0.079***	0.065***	0.079***	0.079***
	(0.024)	(0.023)	(0.024)	(0.025)	(0.023)	(0.023)	(0.021)	(0.021)	(0.022)
Civil Conflict	0.091***	0.108***			0.108***	0.107***	0.112***	0.107***	0.107***
	(0.030)	(0.028)			(0.029)	(0.028)	(0.028)	(0.018)	(0.029)
Child Mortality		0.024	0.028	0.036		0.023	0.058*	0.023	0.023
		(0.037)	(0.037)	(0.041)		(0.037)	(0.031)	(0.019)	(0.033)
Democracy	0.003		0.005	-0.005	0.001		-0.002	0.001	0.001
	(0.013)		(0.012)	(0.012)	(0.012)		(0.012)	(0.011)	(0.010)
Population Size	0.194***	0.169***	0.184***	0.168***	0.173***	0.170***	0.180***	0.170***	0.170***
	(0.051)	(0.043)	(0.045)	(0.046)	(0.043)	(0.044)	(0.045)	(0.023)	(0.051)
Globalization	-0.006***	-0.005**	-0.004**	-0.005**	-0.005***	-0.005**		-0.005***	-0.005*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)		(0.002)	(0.003)
GDP p.c.	0.047*								
	(0.026)								
Polyarchy Index		0.043							
		(0.059)							
State Failure			0.028***						
			(0.005)						
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	21.47	29.27	29.93	29.51	28.11	28.45	35.35	131.6	10.74
AR Test p-value	0.01	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
Observations	6,594	7,121	7,121	7,121	7,121	7,121	7,121	7,121	7,121

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. Cluster-robust standard errors in parentheses. Driscoll-Kraay standard errors and standard errors clustered on country-region pair reported in specifications (8) and (9), respectively. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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 accounting for (1) a country's natural resource wealth, i.e., rents from natural resources as a share of GDP (WDI data); (2) incidences of international warfare (Major Episodes of Political Violence Dataset); (3) the level of urbanization, i.e., the share of the population living in cities (WDI data); (4) the male youth share, i.e., males aged 15-24 as a share of males aged 15-64 (WDI data); (5) government size, i.e., government expenditure as a share of GDP (WDI data); (6) the rate of economic growth (WDI data); (7) a country's level of freedom of expression related to, e.g., freedom of the press as well as the freedom of academic and cultural expression (VDEM data); and (8) the protection of private property rights (VDEM data).

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 being trustworthy. Concerning the additional controls, we find that there is a statistically significant and positive correlation between anti-American terrorism and urbanization as well as freedom of expression. The former result may speak to the notion of terrorism as a form of "urban warfare", while the latter finding may hint at the existence of a reporting bias. In countries in which the press is free, anti-American terrorist events are more likely to be reported (and thus recorded in the ITERATE dataset).

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 the 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 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 U.S. per capita economic aid, using data from USAID (2019). This latter variables is inverse hyperbolic sine-transformed. Third, we control for the presence of U.S. troops using data from an update of Kane (2012). This variable is a dummy variable that is equal to unity when more than 5,000 U.S. troops are stationed in a country. Fourth, we control for the existence of a collective defense

Table C.2: Additional Control Variables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Military Aid	0.087***	0.078***	0.078***	0.079***	0.067**	0.069**	0.073***	0.074***
	(0.029)	(0.024)	(0.023)	(0.024)	(0.027)	(0.027)	(0.023)	(0.026)
Civil Conflict	0.095***	0.106***	0.108***	0.107***	0.086***	0.086***	0.111***	0.109***
	(0.030)	(0.028)	(0.028)	(0.028)	(0.032)	(0.030)	(0.028)	(0.029)
Child Mortality	0.010	0.023	0.031	0.025	0.025	0.036	0.019	0.023
	(0.041)	(0.037)	(0.035)	(0.037)	(0.038)	(0.037)	(0.037)	(0.036)
Democracy	0.001	-0.000	-0.002	0.001	-0.003	0.003	-0.033**	-0.004
	(0.013)	(0.012)	(0.012)	(0.012)	(0.013)	(0.013)	(0.016)	(0.013)
Population Size	0.161***	0.173***	0.143***	0.171***	0.200***	0.165***	0.156***	0.169***
	(0.045)	(0.044)	(0.043)	(0.045)	(0.053)	(0.045)	(0.042)	(0.043)
Globalization	-0.005**	-0.004**	-0.005**	-0.005**	-0.003	-0.004*	-0.005**	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Resource Rents	0.001**							
	(0.001)							
International Conflict		0.019						
		(0.020)						
Urbanization			0.004**					
			(0.002)					
Male Youth Burden				-0.001				
				(0.004)				
Government Size					-0.000			
					(0.001)			
Economic Growth						0.001		
						(0.001)		
Freedom of Expression							0.171***	
							(0.061)	
Property Rights								0.079
								(0.104)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	23.89	29.27	30.49	29.42	20.22	19.55	29.49	25.76
AR Test p-value	0.01	0.00	0.00	0.00	0.04	0.03	0.00	0.00
Observations	6,696	7,121	7,121	7,121	6,097	6,601	7,121	7,121

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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. Finally, 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 are from Bailey et al. (2018).

As shown in Table C.3, controlling for these additional foreign policy variables does not affect our baseline IV-findings concerning the role of military aid in anti-U.S. terrorism. The effect sizes are comparable to our baseline finding and the IV-diagnostics are sound. Concerning the additional controls, there is evidence that a large U.S. troop presence and being an ally of the United States correlates with more anti-American terrorism. For instance, off duty U.S. troops may be attractive targets for terrorist organizations to signal their displeasure with their country's foreign policy alignment with the United States.

Appendix C.4. Lagged Dependent Variable and Lag Structure

Potentially, our empirical results suffer from omitted variable bias due to the non-inclusion of a lagged dependent variable. As a robustness check, we thus consider whether running a dynamic version of our empirical model yields different empirical results. For one, we include as an additional explanatory variable a lag of the dependent variable. For another, we include a three-year moving average of our dependent variable as another way to allow for more complex dynamics. Here, we do not expect the Nickell bias to be influential due to the long period of observation we observe (as this bias is known to become less influential as T increases).

Furthermore, we examine whether using lags of our main explanatory variable, U.S. military aid, matters to our empirical conclusions. For instance, one may argue that the effect of military aid on state capacity and grievances only emerges after a few years, meaning that the adverse effects of aid on anti-U.S. terrorism may similarly only emerge after some time.

As shown in Table C.4, we find that accounting for past anti-American terrorism does not matter to our finding that more U.S. military aid leads to more anti-American terrorism in the present. While the regression coefficient associated with the aid variable is—as expected—smaller than in the non-dynamic setting, estimated effect sizes between the

Table C.3: Role of Other Foreign Policy Variables

	(1)	(2)	(3)	(4)	(5)
Military Aid	0.079***	0.074***	0.065***	0.073***	0.087***
	(0.024)	(0.025)	(0.023)	(0.024)	(0.026)
Civil Conflict	0.107***	0.107***	0.110***	0.107***	0.106***
	(0.028)	(0.028)	(0.027)	(0.028)	(0.029)
Child Mortality	0.024	0.015	0.039	0.026	0.035
	(0.038)	(0.036)	(0.037)	(0.037)	(0.035)
Democracy	0.001	0.000	0.006	0.000	-0.003
	(0.012)	(0.012)	(0.013)	(0.012)	(0.013)
Population Size	0.170***	0.160***	0.152***	0.183***	0.171***
	(0.043)	(0.044)	(0.040)	(0.044)	(0.045)
Globalization	-0.005**	-0.005**	-0.003	-0.005**	-0.005**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Development Aid	-0.000				
	(0.001)				
U.S. Economic Aid		0.009			
		(0.009)			
U.S. Troop Presence			0.207***		
			(0.077)		
U.S. Ally				0.067***	
				(0.022)	
Affinity to the U.S.					-0.008
					(0.015)
Country FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
F-Stat	29.25	27.88	27.94	28.36	23.38
AR Test p-value	0.00	0.00	0.00	0.00	0.00
Observations	7,121	7,121	7,121	7,121	6,944

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. Cluster-robust standard errors in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table C.4: Lag Structure

	(1)	(2)	(3)	(4)	(5)	(6)
Military Aid	0.063***	0.050**				
	(0.021)	(0.020)				
Anti-U.S. Terrorism t-1	0.148***					
	(0.025)					
Past Anti-U.S. Terrorism		0.283***				
		(0.038)				
Military Aid t-1			0.069***			
			(0.023)			
Military Aid t-2				0.063***		
				(0.021)		
Military Aid t-3					0.071***	
					(0.021)	
Military Aid t-4						0.060***
						(0.019)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	27.49	23.85	30.77	32.13	33.38	37.26
AR Test p-value	0.00	0.02	0.00	0.00	0.00	0.00
Observations	7,084	6,900	7,084	7,045	7,004	6,919

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Baseline Controls* are for are for civil conflict, child mortality, democracy, population size and globalization. Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

dynamic and (baseline) non-dynamic versions of our empirical model are remarkably close to each other. In addition, we find that allowing for a lagged impact of U.S. military aid on anti-American terrorism does not alter our main empirical conclusion, suggesting that such impacts are rather persistent.

Appendix C.5. Measurement of Anti-American Terrorism

To assess whether our results are robust to the operationalization of anti-American terrorism, we proceed as follows. First, employing the location definition of transnational terrorism (as in the main text), we use the *count of anti-American terrorist attacks* per country-year pair. We apply the inverse hyperbolic sine transformation to this variable. Second, we use 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 two additional dependent variables, namely (1) a binary indicator equal to unity when there is any

anti-American terrorism according to the country of origin definition and (2) the (inverse hyperbolic sine transformed) number of terrorist attacks. A major shortcoming of the original 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 2018, for over 5,000 incidents a country of origin could not be determined. 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, e.g., low levels of press freedom and weak communication systems.

Third, we create two additional dependent variables (a binary indicator equal to unity when there is any anti-American terrorism and the inverse hyperbolic sine transformed the number of anti-American terrorist attacks) 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 for 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), GTD data for 1993 is incomplete (ITERATE data for 1993 is complete) and coding conventions changed over time, while ITERATE uses a consistent coding scheme (Enders et al., 2011). Furthermore, compared to the ITERATE dataset, the GTD tends to undercount terrorist activity in the 1970s and overcount it in the 1990s (Enders et al., 2011).

Finally, from ITERATE we construct two additional binary variables that are equal to unity when a country-year pair sees at least one *U.S. victim* (i.e., a wounded or killed U.S. citizen) due to a terrorist attack using the location or origin definition, respectively. On the one hand, this variable may help us understand how military aid affects the ferocity in addition to frequency of anti-American terrorism. On the other hand, in addition to the aforementioned data issues, the victim data is plagued by two additional problems. First, the victim data only starts in the late 1970s. Second, it is possible that U.S. citizens are victimized by an attack that is not directed against U.S. interests (collateral damage).

We report our empirical findings in Table C.5. Regardless of which alternative dependent

Table C.5: Measurement of Anti-American Terrorism

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Measurement →	No. of Attacks (Location)	Binary Attacks (Origin)	No. of Attacks (Origin)	Binary Attacks (GTD)	No. of Attacks (GTD)	Binary Victims (Location)	Binary Victims (Origin)
Military Aid	0.140***	0.052**	0.079**	0.067***	0.119**	0.061***	0.029
	(0.047)	(0.021)	(0.037)	(0.024)	(0.051)	(0.023)	(0.021)
Civil Conflict	0.183***	0.105***	0.163***	0.099***	0.156**	0.114***	0.112***
	(0.068)	(0.027)	(0.052)	(0.028)	(0.068)	(0.029)	(0.028)
Child Mortality	0.099	0.011	0.054	0.023	0.069	0.022	0.026
	(0.078)	(0.032)	(0.057)	(0.036)	(0.070)	(0.042)	(0.040)
Democracy	0.009	0.001	0.014	0.007	0.034	-0.007	0.005
	(0.027)	(0.011)	(0.022)	(0.012)	(0.027)	(0.014)	(0.014)
Population Size	0.302***	0.139***	0.226***	0.158***	0.261***	0.150***	0.105**
	(0.101)	(0.038)	(0.073)	(0.044)	(0.088)	(0.050)	(0.044)
Globalization	-0.007	-0.003	-0.004	-0.004	-0.009*	-0.004*	-0.002
	(0.005)	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)	(0.002)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	29.10	29.10	29.10	29.10	29.10	21.45	21.45
AR Test p-value	0.00	0.02	0.05	0.01	0.03	0.01	0.19
Observations	7,121	7,121	7,121	7,121	7,121	6,195	6,195

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

variable or terrorism dataset we employ, we find that more U.S. military aid leads to a higher likelihood of the aid-receiving country producing anti-American terrorism. What is more, the associated regression diagnostics always indicate that the IV-results are sound. The results for the controls are also consistent for different dependent variables and terrorism datasets. Here, the only exception is the terrorism victim variable (country of origin definition), where the effect of U.S. military aid is too imprecisely estimated. This may have to do with the various data issues highlighted above that especially pertain to this alternative dependent variable.

Appendix C.6. 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 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., one million US$) increase in U.S. mil-

itary 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, as the inverse hyperbolic sine transformation is not scale-invariant, we also apply it to the absolute amount of aid (in US$) and the amount of U.S. military aid in billion US$ to see whether different scales matter. Fourth, we weigh military aid by local population size to account for potential scaling effects, where we use the inverse hyperbolic sine-transformed variant of military aid per capita. Fifth, we 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 with 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.

The results are shown in Table C.6. Irrespective of how we operationalize military aid, we find that more U.S. military aid results in more anti-American terrorism. In the case of the non-transformed aid variable, the first-stage F-statistics points to a potentially weak instrument. However, the Anderson-Rubin test results that are robust to weak instruments reassuringly still point to instrument soundness in this case.

Appendix C.7. 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 Northern Africa) have tended to receive especially high levels of U.S. military assistance. Third, we drop from our sample either (1) all U.S. allies (as defined in Appendix C.3) and (2) all countries where a large number of U.S. troops (more than 5,000 servicemen on average) were stationed (e.g., in Germany, Iraq and Afghanistan). Through the latter approach, we expect to reduce the influence of direct U.S. military interventions and occupations. Finally, we run two specifications that differentiate between the effects of military aid on anti-American terrorism between the Cold War (1968–1992) and post–Cold War era (after 1992) and between the pre–9/11 (1968–2001) and post–9/11 era (after 2001), respectively. Fleck and Kilby (2010) argue that these two historical turning points may have led to changes in the U.S. provision of aid, which, in turn, may also matter to our empirical findings.

As shown in Table C.7, there is no evidence that dropping data from specific world regions or from a sub-set of countries affects our findings. That is, more U.S. military aid continues to lead to more anti-American terrorism for all reduced samples we consider. Moreover, we find that U.S. military aid leads to more anti-American terrorism both during and after the Cold War as well as pre- and post-9/11. Thus, there seems to be no specific sub-sample or time period driving our main empirical finding.

Table C.6: Alternative Operationalization of U.S. Military Aid

Aid Variable →	(1) Not Transformed	(2) (Log+1) Transformed	(3) Aid Absolute (IHS)	(4) Aid in Billions (IHS)	(5) Aid p.c.	(6) Aid Dummy (1=Any Aid)	(7) Aid Dummy (1=Aid over 5,000,000)	(8) Winsorized	(9) Trimmed
Military Aid	0.004*	0.095***	0.019***	3.461**	0.192***	0.351***	0.374***	0.086***	0.091***
	(0.002)	(0.028)	(0.006)	(1.622)	(0.061)	(0.126)	(0.118)	(0.026)	(0.034)
Civil Conflict	0.092**	0.106***	0.137***	0.089**	0.104***	0.149***	0.098***	0.109***	0.109***
	(0.038)	(0.029)	(0.030)	(0.037)	(0.029)	(0.033)	(0.029)	(0.028)	(0.028)
Child Mortality	-0.056	0.021	-0.000	-0.007	0.025	-0.008	0.038	0.028	0.036
	(0.108)	(0.038)	(0.041)	(0.082)	(0.040)	(0.045)	(0.034)	(0.035)	(0.030)
Democracy	-0.027	0.001	-0.013	-0.004	0.006	-0.018	0.003	0.001	0.002
	(0.026)	(0.012)	(0.015)	(0.017)	(0.012)	(0.017)	(0.012)	(0.012)	(0.012)
Population Size	0.078	0.166***	0.144***	0.066	0.195***	0.132***	0.200***	0.182***	0.192***
	(0.074)	(0.043)	(0.039)	(0.064)	(0.050)	(0.041)	(0.051)	(0.045)	(0.049)
Globalization	-0.003	-0.004**	-0.007**	-0.002	-0.003*	-0.007**	-0.004*	-0.005**	-0.006**
	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	2.975	26.52	30.49	4.165	17.47	20.91	25.53	36.45	43.59
AR Test p-value	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01
Observations	7,121	7,121	7,121	7,121	7,121	7,121	7,121	7,121	6,732

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Finally, we drop from our dataset twelve countries that are potentially influential because they received substantial amounts of U.S. aid and/or produced considerable anti-U.S. terrorism over our period of observations. Including these countries in our sample may invalidate our IV-strategy because they may be influential enough to also affect regional aid to proximate countries. As shown in Table C.8, 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, in turn, that these cases do not drive our empirical results.

Table C.7: Sub-Sample Analysis

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dropped Region or Countries →	OECD	Middle East	Latin America	Sub-Saharan Africa	U.S. Ally	More than 5,000 U.S. Troops		
Military Aid	0.065**	0.065***	0.087***	0.069***	0.071**	0.045**		
	(0.028)	(0.025)	(0.027)	(0.023)	(0.030)	(0.020)		
Military Aid (Cold War)							0.120*	
							(0.063)	
Military Aid (Post-Cold War)							0.069***	
							(0.026)	
Military Aid (Pre 9/11)								0.087**
								(0.039)
Military Aid (Post-9/11)								0.074***
								(0.025)
Equal Coefficients [p-value]							[0.44]	[0.74]
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Baseline Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat	38.52	30.12	23.83	34.29	23.27	23.39		
AR Test p-value	0.03	0.02	0.00	0.01	0.04	0.05	0.01	0.01
Observations	6,047	6,228	6,141	5,033	5,602	6,540	7,121	7,121

Notes: IV-estimates reported. *FE* = fixed effects. *F-Stat* = first-stage F-statistic. *AR Test* = Anderson-Rubin test. *Baseline Controls* are for are for civil conflict, child mortality, democracy, population size and globalization. Cluster-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table C.8: Role of Country Outliers

Dropped Country	β-Coefficient for Aid (Standard Error)	IV-Diagnostics
(Full Sample)	0.079***	F=29.10
	(0.024)	p=0.00
Afghanistan	0.067***	F=27.66
	(0.021)	p=0.00
Iraq	0.075***	F=28.27
	(0.024)	p=0.00
Israel	0.080***	F=28.76
	(0.024)	p=0.00
Egypt	0.079***	F=28.86
	(0.024)	p=0.00
Colombia	0.080***	F=28.56
	(0.024)	p=0.00
Saudi Arabia	0.078***	F=28.94
	(0.024)	p=0.00
Pakistan	0.075***	F=28.01
	(0.024)	p=0.00
Jordan	0.079***	F=29.80
	(0.023)	p=0.00
South Korea	0.080***	F=29.26
	(0.024)	p=0.00
Turkey	0.078***	F=30.39
	(0.023)	p=0.00
Greece	0.082***	F=31.58
	(0.026)	p=0.00
Spain	0.075***	F=28.33
	(0.026)	p=0.00

Notes: IV-estimates reported. Cluster-robust standard errors in parentheses. Country- and year-fixed effects and baseline controls for civil conflict, child mortality, democracy, population size and globalization included in all estimations. *IV-Diagnostics* refer to the first-stage F-statistic and the p-value associated with the Anderson-Rubin test. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

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