

Bargaining with the Banned?

The Impact of Terrorist Designations on Mediation in Civil Wars

Magnus Lundgren, University of Gothenburg

Sophie Haspeslagh, King's College London

Desirée Nilsson, Uppsala University

Isak Svensson, Uppsala University

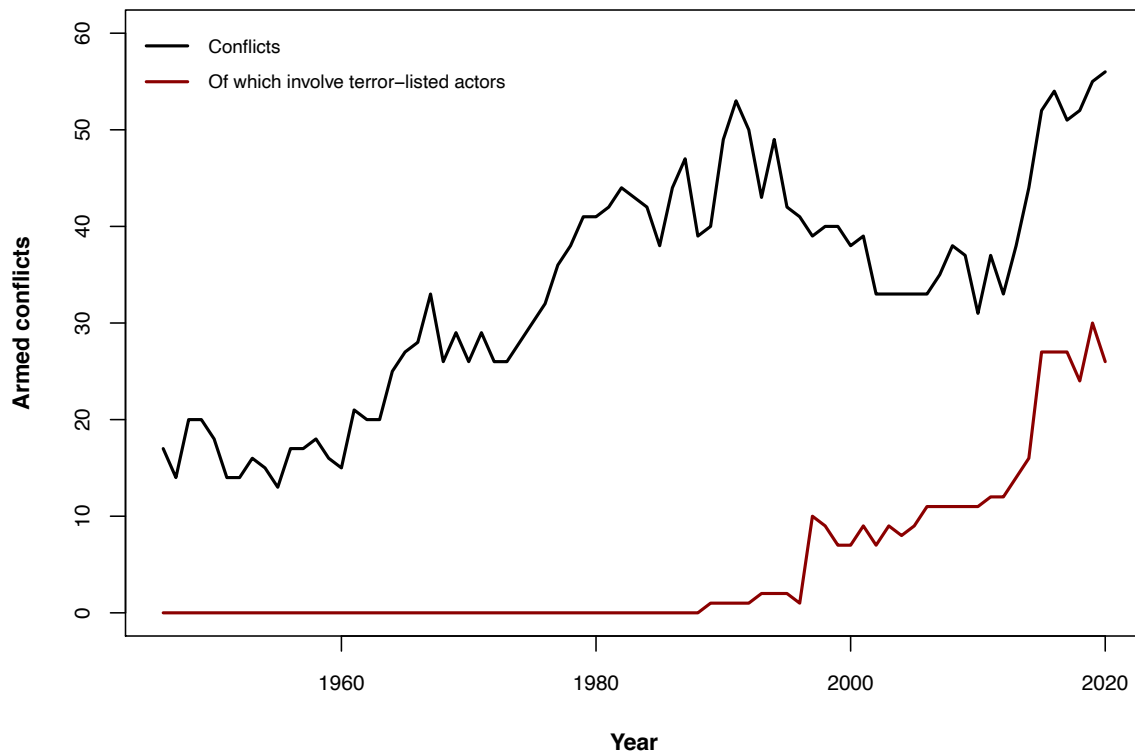
Abstract: This study provides the first global analysis of how terrorist designations influence mediation in civil wars. Terrorist designations – also known as proscription – impose legal, material, and reputational costs on both mediators and belligerents, affecting their willingness to offer or participate in mediation. We argue that the impact of proscription is shaped by the characteristics of the proscription regime, which determine the severity of the imposed costs, and by mediator type, reflecting variation in susceptibility to such costs. Analyzing proscription data worldwide, 1989–2019, coupled with difference-in-difference analysis of event data from the Philippines and Mali, our findings reveal that United Nations terrorist designations significantly reduce the probability of mediation; notably, state-based mediators are more deterred by proscription than their civil society counterparts. By identifying when and for whom proscription constrains peacemaking, this study demonstrates how counterterrorism institutions reshape the occurrence of mediation, with important implications for contemporary conflict resolution.

INTRODUCTION

Over the past decades, two primary developments have characterized global efforts to contain and resolve armed violence. On the one hand, the combined efforts of states, intergovernmental organizations (IGOs), and non-state actors have led to the emergence of an “international regime for treating civil war” aimed at facilitating peace agreements between belligerents (Gowan and Stedman 2018). To that end, mediation has become an important tool for resolving armed conflicts worldwide (Clayton and Dorussen 2022; Gartner and Bercovitch 2006; Wallensteen and Svensson 2014). On the other hand, a parallel policy effort, often spearheaded by the same actors, has led to the creation of international regimes to outlaw and sanction non-state armed groups. Proscription regimes, or “terror-lists,” have been established by states and IGOs, placing an increasing number of individuals and armed groups under international sanctions (Haspeslagh 2025; Lee and Tominaga 2024).

The tension between these two policy efforts is increasingly evident. While mediation presupposes access, interaction, and dialogue, proscription relies on exclusion, condemnation, and penalties. For mediators in the field, engaging with proscribed armed actors is associated with considerable difficulty, involving reputational risks, legal complexities, and even criminalization. As the proportion of armed conflicts involving terror-listed actors has increased from less than five percent in the early 1990s to more than 50 percent in the 2020s (Figure 1), the possible friction between proscription regimes and standard methods of peaceful conflict resolution is a question of systematic concern and broad relevance for both policy and research.

FIGURE 1. Proportion of civil wars involving at least one terror-listed actor, 1945-2020.



Data: UCDP (Pettersson and Öberg 2020) and PAAD (Lundgren, Janson, and Lundqvist 2024).

Scholars and practitioners have explored the tension between proscription and mediation through case studies, often concluding that the former impedes the latter (Boon-Kuo et al. 2015; Haspeslagh 2013, 2021b; Matesan 2022; Nadarajah and Sriskandarajah 2005). These studies have provided important insights, particularly in conflicts such as Sri Lanka, Colombia, and Palestine, but their reliance on a small number of cases limits their generalizability. Moreover, the case-based literature has not systematically assessed variation across sanctions regimes or mediators' susceptibility to proscription, and the different theoretical accounts have not been integrated into a general theoretical explanation of how proscription influences mediation. The absence of a unified theoretical framework and comprehensive and

causal empirical tests leaves us without clear evidence on whether proscription generally reduces opportunities for mediation, whether different regimes exert distinct effects, and whether some mediator types are more constrained than others. Without such tests, we cannot fully assess the broader implications of the dramatic increase in proscriptions over the recent decades.

In this study, we present the first comprehensive investigation of the effect of proscription on the occurrence of mediation in civil wars. We synthesize and draw on prior work to develop a theoretical framework that explains how proscription alters the costs of mediation for both mediators and belligerents, thereby making mediation less likely. Building on this framework, we argue that the effects of proscription are conditional: they vary across proscription regimes, reflecting differences in the costs they impose, and across mediator types, reflecting variation in their susceptibility to those costs.

We evaluate our argument in two complementary studies, employing (i) a yearly panel of intrastate armed conflict dyads from 1989 to 2019, using time-series cross-sectional regression, and (ii) difference-in-difference analysis of event data from the civil wars in the Philippines and Mali. Our analysis demonstrates that the impact of proscription is not a uniform “chilling effect” for mediators, but is instead shaped by two forms of institutional heterogeneity. First, we find that the effect is regime-specific: while proscription in general does not systematically reduce mediation, multilateral (UN) listings create a significant barrier to mediation. In our global sample, conflicts involving rebel actors designated as terrorists by the UN have a substantially lower probability of receiving mediation, dropping from 34 to 9 percent, a reduction of about three quarters. Second, we find that proscription constraints are actor-specific: non-state mediators, including civil society organizations, appear to operate with greater

flexibility in conflict environments with proscribed actors, compared with state-based mediators, likely because they are less affected by the reputational and material effects that flow from proscription.

This study makes several important contributions. Using recent data and rigorous statistical techniques, we demonstrate that the negative association between proscription and mediation, discussed in the case-based literature,¹ only generalizes to a wider population under certain conditions. In doing so, our findings not only challenge prevailing assumptions in this literature but also clarify the conditions under which proscription deters mediation and which mediators are most affected. Importantly, we show that proscription can shape the likelihood of mediation, even when accounting for radical ideologies and terrorist violence, indicating that a key obstacle to mediation lies in the characteristics of the proscription regimes.

More broadly, our results speak to several core debates, which we elaborate in the concluding section. First, in line with scholarship on naming and shaming (Bassan-Nygate 2025; Kelley and Simmons 2021; Rousseau 2026; Zhou, Kiyani, and Crabtree 2023), our results suggest that the deterrent effects of proscription on mediation operate through social and reputational mechanisms, as we find the strongest effect on mediation coming from the body with the largest international legitimacy, the UN. Second, whereas scholarship on economic statecraft often assumes that sanctions can ripen conflicts for resolution (e.g., Radtke and Jo 2018), we demonstrate that proscription-related sanctions can generate externalities that undermine efforts to exploit such openings. Third, our findings suggest that the expansion of proscription regimes is a plausible explanation for the global downturn in mediation (Lundgren and

¹ E.g., Sri Lanka (Helgesen 2007; Nadarajah and Srisankandarajah 2005), Colombia (Haspeslagh 2021b), or Palestine (Boon-Kuo et al. 2015; Matesan 2022).

Svensson 2020), a factor previously overlooked in research on mediation occurrence (e.g., Clayton and Gleditsch 2014; Greig and Regan 2008; Melin and Olander 2019).

Finally, by examining the friction between proscription regimes and conflict resolution regimes, our study also speaks to broader debates about how well-intentioned efforts to penalize illicit actors can make conflicts harder to resolve, a dilemma well known across research fields, including scholarship on the trade-off between peace and justice (e.g., Prorok 2017).

STATE OF THE ART

Mediation uses diplomatic tools to bring belligerents into dialogue and facilitate a shift from violence to more peaceful relations (Bercovitch 1992; Greig and Diehl 2012; Wallensteen and Svensson 2014). A large body of literature shows that mediation increases the likelihood of de-escalation, crisis termination, ceasefires, and peace agreements (Beardsley 2008; Beardsley, Cunningham, and White 2018; Clayton and Dorussen 2022; Lundgren 2016; Ruhe 2020; Svensson 2007). We focus here on the determinants of mediation occurrence, i.e., what shapes whether third parties and belligerents are willing to participate in a mediation process in the first place.

What do we know about the conditions shaping the willingness of *belligerents* to participate in mediation? Quantitative research has highlighted their relationship to the mediator (Savun 2008; Svensson 2007), national interests and historical ties (Greig and Regan 2008), formal alliances (Melin and Olander 2019), and transborder kin-group networks (Feinberg and Greig 2021). Others have pointed to conflict dynamics, such as relative strength (Clayton 2013), conflict duration (Greig and Regan 2008), severity of violence (Clayton and Gleditsch 2014; Melin, Gartner, and

Bercovitch 2013), and battlefield performance (Greig 2015). A central insight across this literature is that parties rarely pursue negotiations until the cost of continued fighting becomes prohibitively high. Mediation is therefore more likely when conflicts are “ripe” for resolution, typically when both sides perceive a mutually hurting stalemate and view negotiation as a viable exit from an otherwise unsustainable situation (Greig 2001; Kleibor and Hart 1995; Regan and Stam 2000; Zartman 2003).

Previous research has also examined what motivates a *third party* to offer mediation (e.g., Melin and Olander 2019). Although conflict resolution is often a genuine motive for third parties, it is commonly complemented by other considerations, including reputational concerns, a wish to decrease conflict externalities—such as refugee flows, economic shocks, and regional turmoil—or attempts to advance the mediator’s own agenda (Böhmelt 2016; Touval and Zartman 2001). Some of these motivations are linked to the prospects of a settlement (“settlement rewards”), others to the mediation process itself (“process rewards”) (Mitchell 1988), and yet others to strategic interests beyond the process. Research has also highlighted that mediation requires resources, including personnel and money, and may put crucial relationships or reputation at stake, especially if efforts fail (Crocker, Hampson, and Aall 2003). Taken together, this literature portrays mediation as a strategic choice, involving the weighing of potential benefits against material and reputational costs.

PROSCRIPTION AND MEDIATION

Proscription, often referred to as terror-listing, is the formal designation of an armed group as a terrorist organization (Jarvis and Legrand 2018). By imposing sanctions on groups and criminalizing membership or support, proscription serves as a counter-terrorism measure aimed at reducing the targeted group’s capabilities. As the US

Department of State notes, proscription is a means of “curtailing support for terrorist activities and pressuring groups to get out of the terrorism business” (US Department of State 2025). Proscription typically involves the banning of the group, asset freezes, travel bans, and prohibitions on material support. Beyond such material measures, proscription also serves as a signal of public condemnation that can tarnish a group’s reputation.

Terrorist lists and proscription processes have attracted growing scholarly attention (de Goede 2018). Quantitative research has looked at the constitutive aspects of proscription, such as what determines terrorist designation (Lee and Tominaga 2024), often viewing it as a process of diffusion mainly driven by the US (El Masri and Phillips 2024) and motivated by security threats and diplomacy between allies (Lee and Tominaga 2024; Shuaibi and Almutairi 2024). Others have examined the formation of national proscription regimes, with studies of the UK (Jarvis and Legrand 2017, 2018, 2020; Kirkpatrick 2019), China (Zhang 2021), and Spain (Bourne 2018). One particular focus of this constitutive strand of literature has been on the type of group that tend to be proscribed, concluding that a group’s Islamic ideology is an important motivator for proscription (Beck and Miner 2013; El Masri and Phillips 2024).

The effectiveness of proscriptions remains debated. Some argue that proscription impedes targeted groups (Schwartz 2014) by cutting off resources and support (Abrahms 2008), leading them to carry out fewer attacks over time, particularly those designated as Foreign Terrorist Organizations in US-aligned countries (Phillips 2019). Others contend that the war on terrorist financing has failed (Neumann 2017) or that proscription can backfire, as certain groups, particularly Islamist ones, take it on as a badge of honor and use it to build legitimacy and commitment among supporters (Haspeslagh 2025). The effects appear to depend on the type of groups

being targeted. Proscriptions are more likely to constrain the violence of younger, secular, less entrenched, and less adaptable groups as opposed to older religious groups or those with state sponsors which tend to become more violent post-proscription (Best and Lahiri 2021; Jo, Phillips, and Alley 2026).

Going beyond whether proscription constrains the behavior of its targets, research has also focused on its broader consequences. Scholars have looked at its impact on humanitarian actors, human rights and civil society organizations (Cortright et al. 2011; Howell 2006; Pantuliano et al. 2011; Sullivan and Hayes 2010), as well as self-determination movements and diasporas (Sentas 2018; Sentas 2014), concluding that proscription can criminalize these groups and undermine principles of neutrality and impartiality. Similarly, when it comes to the effects of proscription on peacebuilding actors, the scholarship highlights negative effects (Boon-Kuo et al. 2015), leading to legal uncertainties and self-censorship (Boon-Kuo et al. 2015; Gross 2011). Its effect on negotiations is somewhat contested, as some find that it prolongs the pre-negotiation phase as third parties and conflict actors need to find ingenious ways of overcoming the barriers it creates (Haspeslagh 2021b), while others argue that proscription heightens the probability of peace negotiations, particularly for stronger, ethnically based groups (Avdan et al. 2025). Taken as a whole, this literature on proscription shows that it increases costs and constraints on actors, but that more needs to be understood about how the effects may vary based on the type of listing regime and the actors involved.

Regarding the specific effect of proscription on mediation, practitioner accounts (Dudouet 2010, 2011; Haspeslagh and Dudouet 2011; Helgesen 2007) and case studies suggest that terrorist designations limit options for dialogue (Renner and Spencer 2012), forestall non-violent strategies (Toros 2012), and lead to exclusion

from negotiations (Lanz 2011). For example, case studies show that the terror-listing of the Liberation Tigers of Tamil Eelam (“Tamil Tigers”, LTTE), the Sri Lankan separatist group, became an impediment to a negotiated settlement (Nadarajah and Sriskandarajah 2005) and prevented contact with mediators from the US and EU (Birkeland 2014).

Overall, the qualitative literature has accumulated important and novel insights regarding the effect of proscription on mediation (Boon-Kuo et al. 2015; Haspeslagh 2013, 2021b; Haspeslagh and Dudouet 2015; Matesan 2022; Palmiano Federer 2019). Yet these theoretical insights remain fragmented and have not been integrated into a general explanation, and no study offers a comprehensive, global assessment of the relationship between proscription and mediation. Therefore, we cannot determine whether the observed effects reflect a general pattern or are specific to the examined cases. We also lack knowledge on whether effects are constant across proscription regimes and mediator types. Advancing this research agenda requires developing theoretical propositions about when and how proscription affects mediation, conducting systematic tests using large-N data, and further probing causal relationships.

ARGUMENT: WHEN AND WHY TERRORIST DESIGNATIONS MAKE MEDIATION LESS LIKELY

We synthesize and build on prior research to provide a theoretical account of how proscription may shape mediation. Our argument is that proscription reduces, or even

eliminates, the conditions under which actors favor mediation, with variation across proscription regimes and mediator types.

For mediation to occur, a third party must be willing and able to serve as a mediator, while belligerents must accept, and be able to receive, outside assistance. In other words, a government, the opposing rebel group, and a mediator must each simultaneously prefer mediation to non-mediation. Importantly, because a mediation process requires the participation of all three parties, a change in any single actor's preferences can be sufficient to shift the outcome away from mediation. We consequently discuss the interests of each party in turn: rebels, government, and prospective mediators. For each, we distinguish between legal-material mechanisms, emphasizing relative military capabilities and access to resources, and social-reputational mechanisms, emphasizing legitimacy and political costs. We begin by presenting a baseline expectation (H1), before outlining our arguments on how variation in proscription regimes (H2) and mediator types (H3) shape these calculations.

By subjecting a *rebel group* to material sanctions such as asset freezes and travel bans, proscription can reduce its material capabilities (Walker 2017), thereby weakening it relative to its governmental opponent. This may increase the listed group's incentives to seek mediation: deteriorating battlefield prospects can make a negotiated process preferable to military defeat. However, proscription can also motivate continued reliance on military strategies. For instance, a group facing diminished capabilities as a result of proscription may double down on a military strategy to signal resolve. Proscription also pushes militant groups underground, hindering their ability to access and process information accurately, which increases the risk of information asymmetries and miscalculation, typically understood to

underpin violent strategies (Fearon 1995). For instance, during the civil war in Sri Lanka, the EU's proscription of the LTTE may have contributed to misjudgment of relative military power, prompting a return to the battlefield from 2006 onward (Sørbø et al. 2011).

Beyond its material impact, proscription also entails symbolic stigmatization, resembling what de Goede (2012) describes as a "modern day exile". This stigmatization can shape rebel groups' willingness to participate in mediation. In some cases, proscription may generate shame, encouraging groups towards dialogue as a way of shedding stigma (Haspeslagh 2025). In others, it may instead harden resolve, leading them to privilege continued armed struggle over mediation, particularly where external stigmatization strengthens hardliners who reinterpret proscription as a badge of honor (Haspeslagh 2025).

Whereas proscription can thus have varying effects on rebel groups, it is likely to more consistently diminish *governments'* incentives to accept mediation. By weakening rebel military capabilities, proscription can bolster the government, legitimating a militarized strategy and increasing expectations of victory without negotiation. Additionally, governments may perceive proscription as signaling improved prospects of external support against listed rebels. If proscription diminishes the capabilities of rebels and strengthens those of governments, it will make them less likely to consider mediation.

Proscription also raises the government's reputational costs of mediation (Matesan 2022). By stigmatizing listed groups, proscription places them beyond the pale of dialogue, creating a (sometimes intended) policy straitjacket for the government (Haspeslagh 2021b). It then becomes politically costly to switch strategies or appear to be 'negotiating with terrorists'. The FARC in Colombia during the two

terms of the Uribe administration illustrates this dynamic: international listing criminalized contact with the group, deepened its political isolation, and reduced incentives for dialogue. The delegitimization of proscribed entities such as the FARC is often so deep that their return to negotiations requires an “unlabelling” (Toros 2012) or a “linguistic ceasefire” (Haspeslagh 2021a, b), which are difficult to attain. As a result, the reputational effects of proscription tend to create disincentives for governments to accept mediation.

While *mediators* typically reap process rewards from mediation, proscription introduces constraints that can reduce these incentives. First, criminalization creates legal and material obstacles for potential mediators seeking to involve listed actors in negotiations (Bialostozky 2011; Gross 2011). In particular, compliance with sanctions regimes complicates the financing of mediation activities, as funding bodies often require assurances that no designated entities benefit from supported initiatives, increasing financial and logistical burdens. Travel bans associated with listings further constrain mediation by restricting the movement of listed individuals and groups. Second, these material impediments are reinforced by reputational and political concerns, as engaging with proscribed actors can carry social and political costs that reduce mediators’ willingness to intervene. Qualitative research discusses a “chilling effect” (Dudouet 2011) and argues that political and reputational risks could deter mediators from engaging with listed entities (Dumasy and Haspeslagh 2016).

In line with our framework, because mediation requires the participation of all relevant actors, a reduction in incentives for any single party is sufficient to make mediation less likely. Since proscription can reduce the incentives for rebel groups, and more consistently does so for governments and mediators, we theorize:

H1: Conflicts involving proscribed actors are less likely to experience mediation than other conflicts.

However, because proscription regimes vary (Lee and Tominaga 2024; Lundgren, Janson, and Lundqvist 2024), and mediators differ in their vulnerability to legal, political, and reputational costs (Wallensteen and Svensson 2014), we expect this effect to be conditional. We therefore develop hypotheses capturing how variation in the costs imposed by proscription (H2) and in actors' susceptibility to those costs (H3) shapes incentives for mediation.

PROSCRIPTION REGIMES GENERATE VARYING COSTS FOR MEDIATORS

While most proscription regimes entail similar sanctions on listed actors (Lee and Tominaga 2024), they differ in legalization and normative legitimacy, implying variation in the constraints they impose on mediators.

A first dimension of variation concerns legalization, understood as the extent to which proscription regimes impose binding and enforceable legal obligations on third parties (Abbott et al. 2000). Such obligations bear on mediation by shaping a third party's legal exposure: the more binding and far-reaching a proscription regime, the greater the legal risks a prospective mediator, and the institutions supporting it financially, assume in making contact with the sanctioned entity. Sanctions adopted by individual states or regional organizations primarily bind their own legal subjects, whereas sanctions adopted by the UN Security Council are binding on all member states and thus have global reach. Proscription regimes also vary in the extent to which engagement with listed actors is criminalized. Some regimes, including several IO frameworks, leave the legal status of third-party engagement ambiguous, creating

uncertainty but not outright prohibition. Others are more restrictive. The US represents a particularly stringent case: following *Holder v. Humanitarian Law Project* (US Supreme Court 2010), engagement related to peaceful conflict resolution constitutes illegal material support to terrorism, with extraterritorial application, albeit with loopholes through the Office of Foreign Assets Control (OFAC) humanitarian licenses.

A second dimension of regime variation concerns normative legitimacy and the symbolic force of proscription. Legitimacy bears on mediation by shaping a third party's reputational exposure: the more widely a designation is accepted as an authoritative judgment rather than a contestable political act, the greater the reputational cost a prospective mediator incurs by engaging the listed group. Even where third-party engagement is not explicitly criminalized, political and reputational risks may lead mediators to exercise "self-censorship," choosing not to engage with listed entities (Dumasy and Haspeslagh 2016). When proscriptions are imposed multilaterally, they tend to carry a stronger normative message and greater international legitimacy, making them harder to dismiss and amplifying the symbolic stigmatization of targeted groups. This logic aligns with the broader sanctions literature, which finds multilateral sanctions more effective than unilateral measures (Bapat and Morgan 2009; Drezner 2000), and with evidence that UN sanctions are more consequential than those imposed by other actors (Neuenkirch and Neumeier 2015).

Taken together, while some unilateral regimes impose particularly stringent legal constraints, multilateral proscriptions are distinctive in combining broad legal reach with broader international endorsement. By simultaneously widening legal exposure and raising the political and reputational costs for mediators, such regimes

impose higher overall constraints on mediation, and we thus expect that they have a strong deterrent effect.

H2: Conflicts involving actors proscribed by multilateral organizations are less likely to experience mediation than conflicts involving actors proscribed by states.

MEDIATORS ARE VARYINGLY SUSCEPTIBLE TO PROSCRIPTION COSTS

While proscription generates constraints that can reduce mediators' incentives, these do not affect all mediators equally. Differences in legal exposure and reputational vulnerability imply that some mediator types are more susceptible to the costs imposed by proscription than others. Mediators that are highly exposed in both dimensions are more likely to be deterred than mediators less exposed in either or both dimensions.

Mediators such as states and IGOs are likely to be particularly sensitive to these constraints as they tend to operate via formal channels. States often engage in mediation as part of their broader foreign policy, shaped by domestic political considerations, alliance commitments, and international reputation (Greig and Regan 2008). IGO mediators, including the UN, similarly operate under formal mandates and compliance requirements that make them sensitive to both legal obligations and reputational risks.

In contrast, mediators such as civil society actors may display a greater resilience to the legal and social effects of proscription as they can more easily operate through unofficial channels. Civil society mediators often carry less political weight (Palmiano Federer 2019) and face fewer formal obligations, which can make them more agile in adapting to restrictive environments (Whitfield 2024). Their motivations are often shaped by ideological or humanitarian concerns and may be oriented toward

local rather than international audiences, reducing exposure to reputational sanctions. These features can render proscription less effective in dissuading civil society actors' mediation efforts compared with those of states and IGOs. Civil society actors have frequently demonstrated an ability to operate in contexts where official engagement is constrained. During the civil war in Colombia, for instance, the civil society group *Colombianos y Colombianas por la Paz* maintained a letter exchange with the FARC between 2008 and 2012, despite the group's proscribed status. More broadly, the flexibility of civil society mediators often enables them to serve as intermediaries or backchannels for official actors (Donohue and Cristal 2011, 75).

Thus, we anticipate that proscription impacts mediator motivations unevenly, disproportionately discouraging states and IGO mediators while civil society actors are less likely to be deterred:

H3: The negative effect of proscription on mediation occurrence is larger for state and IGO mediators than for civil society mediators.

We test these conjectures via two complementary quantitative analyses. The first uses global data on mediation in armed conflicts, 1989-2019; the second uses longitudinal event data from conflicts in the Philippines and Mali in quasi-experimental designs.

STUDY 1: PROSCRIPTION AND MEDIATION – GLOBAL ANALYSIS

DATA AND MAIN VARIABLES

Our sample of armed conflict dyads stems from the Uppsala Conflict Data Program (UCDP), which defines an armed conflict as a “contested incompatibility that concerns

government and/or territory where the use of armed force between two parties, of which at least one is a state, results in at least 25 battle-related deaths in one calendar year” (Pettersson and Öberg 2020). We include all intrastate armed conflicts, using the government-rebel dyad-year as the unit of analysis.

We source mediation data from the M-IND dataset, which covers mediation episodes globally, 1989–2019 (Kreutz and Cárdenas 2024). They define mediation as “a process whereby a third party assists two or more parties, with their consent, to prevent, manage or resolve a conflict by helping them to develop mutually acceptable agreements” (Kreutz and Cárdenas 2024, 2), a definition sufficiently broad for our purposes and aligned with conventional definitions (e.g., Bercovitch 1992; Svensson 2007). The M-IND data provide a strong combination of temporal coverage and geographic scope, but in our robustness tests, we also examine updated mediation data from Svensson (2007).

Our main dependent variable is *mediation*, a dichotomous measure coded as 1 if the observed conflict dyad received mediation in the observed year and 0 otherwise. We recognize that mediation episodes vary in both duration and depth of involvement, but a binary indicator is suited to gauge how mediation propensity is affected by proscription. To test variation across mediator types, we create three alternative dependent variables: *State mediation*, *IGO mediation*, and *CSO mediation*, each coded as 1 if the mediator is a state, an IGO, or a civil society organization (CSO) (the latter category also includes private individuals), respectively, and 0 otherwise. When multiple mediators are involved, we follow Kreutz and Cárdenas and code these variables as 1 for all types present.

Proscription data come from the Proscription of Armed Actor Dataset (PAAD) (Lundgren, Janson, and Lundqvist 2024). PAAD records proscriptions by 17

governments and IGOs from 1979 to 2022, covering nearly 400 armed groups. To capture proscription, we create a dichotomous independent variable, *any listing*, coded as 1 for dyads involving a rebel actor subject to proscription during the observed year and 0 otherwise. Three complementary variables—*UN listing*, *EU listing*, and *US listing*—are analogously created for proscriptions by the UN, EU, and the US, allowing us to test differences across proscription regimes. Notably, the three listing regimes are nearly perfectly nested in our analysis sample: all UN-listed actors are also listed by the EU and the US, and nearly all EU-listed actors are also US-listed. This reflects the fact that relatively few actors are designated by the UN compared to the EU and the US. In the disaggregated models, each listing coefficient therefore captures the additional effect of that regime over and above proscription by the other regime(s) in the model.

Combining these data yields a panel of 1,116 conflict dyad-years worldwide, 1989–2019, after adjusting for lagging and missingness.

CONTROL VARIABLES

To account for possible confounding, we measure several time-varying variables related to conflict characteristics, rebel behavior, and external involvement.

Conflict intensity may shape both the demand for mediation and the likelihood of proscription. Costlier conflicts are more likely to generate incentives for diplomatic engagement (Beardsley 2011; Clayton and Gleditsch 2014), while also increasing pressure on international actors to sanction armed groups. We therefore include *battle deaths*, measured as logged yearly fatalities in the dyad, using UCDP georeferenced event data (Sundberg and Melander 2013).

Rebel violence against civilians is another potential confounder, as groups engaging in atrocities are more likely to be proscribed and may also be less attractive to involve in mediation. We control for *one-sided violence* by rebels (Eck and Hultman 2007), operationalized as the log of cumulative civilian fatalities prior to the observation year. We also include *terrorism deaths*, measured as the log of cumulative country-level fatalities from terrorist attacks using data from the Global Terrorism Database (START 2021).

Conflicts involving Islamist armed groups, notably Salafi-jihadi factions, may systematically differ in both mediation propensity and exposure to proscription (Lundgren and Svensson 2020). Using RELAC data (Svensson and Nilsson 2018), we code a binary indicator for *Islamist rebels* based on self-articulated claims.

We control for *external support*, as externally backed rebel groups may enjoy political support that makes them less likely to be proscribed and more attractive to mediators. Using data from Meier et al. (2023), we code the binary variable *external support* to capture when rebel groups received assistance from external actors.

We also control for some conflict-dynamic factors that likely affect both the likelihood of proscription and the propensity for mediation (Clayton and Gleditsch 2014). These include conflict *duration*, measured as years since onset; *incompatibility*, separating conflicts over government from those over territory; and *internationalized civil war*, distinguishing conflicts with foreign involvement (e.g., troop support) from those without.

Finally, using data from Di Salvatore et al. (2022), we code the variable *peacekeeping operation* as 1 for dyads in countries where peacekeepers were deployed in the year prior to observation and 0 otherwise. We expect that peacekeeping operations, because they provide information on armed groups and are

targets of terrorism, may affect the propensity for proscription while also making mediation more likely.

Summary statistics are presented in Tables A1 and A2 of the Online Appendix.

ESTIMATION STRATEGIES

To analyze the impact of proscription on mediation, we estimate the following model using generalized least squares regression

$$y_{it} = \alpha + \beta x_{it-1} + \gamma_t + v_{it}$$

where y_{it} is the dependent variable, mediation; α is the global intercept; x_{it-1} is a vector of lagged covariates, including our explanatory variable of proscription and a vector of controls; γ_t represents time effects; and v_{it} is a stochastic term that is a composite of fundamental error ε_{it} and unit-specific error (u_i).

We employ random effects because mediation propensity is shaped by unobserved, time-invariant dyad characteristics, and because proscription varies primarily across dyads rather than within dyads over time. Fixed effects would therefore absorb most of the identifying variation in proscription. We report alternative estimators in robustness tests, and in Study 2, we focus on cases with meaningful temporal variation using difference-in-differences designs.

We estimate linear probability models (LPMs), following methodological advice regarding rare binary outcomes with group-level effects (Timoneda 2021). Our preference for LPMs also stems from their intuitive interpretation: the coefficient corresponds to the change in probability of the dependent variable. For example, a

coefficient of 0.06 indicates that a one-unit change in the observed variable is associated with a 0.06 (or 6 percentage points) increase in probability.

To account for temporal dependencies, we include a cubic polynomial of years since the last mediation event in the dyad (Carter and Signorino 2010). We use heteroskedasticity-robust standard errors clustered on dyads; in the appendix, we also report country-clustered standard errors (Table A12). To account for temporal variation shared by all dyads, such as changes in the general willingness to mediate, we include year fixed effects.

EMPIRICAL FINDINGS

We begin by introducing descriptive patterns before presenting the main results. Table 1 shows, for each listing category, the number of unique rebel groups, scope of dyad-years, and mediation rates.² We identify three key patterns. First, the proscription regimes differ in breadth. The US maintains the most expansive targeting, designating 52 of the rebel actors in our data during the observed time period, covering about twice as many as the EU and three times the UN's number. Second, mediation rates decrease as we move down the table. Dyad-years involving no listed actors received mediation in 34.3 percent of cases, which is higher than any of the listed categories and substantially higher than the mediation rates for conflicts involving actors listed by the EU (14.2 percent) or the UN (8.6 percent). Third, while it is clear that proscription does not preclude mediation, the great majority of mediation interventions nevertheless occur in conflict dyads without listed actors. Of all mediation observed in our data, 1989-2019, only 25 percent of interventions occurred in conflicts with listed actors.

² Figure A1 in the online appendix provides a granular visual presentation of the data.

TABLE 1: Proscription, armed groups, and mediation

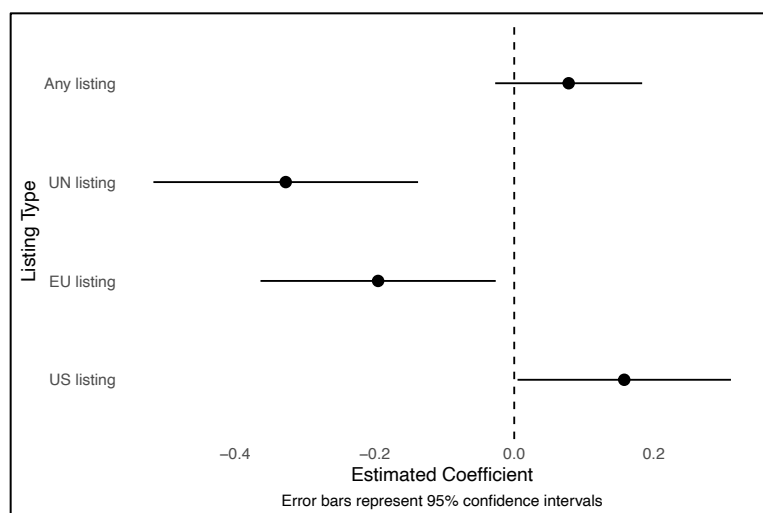
	Rebel actors	Dyad years	Mediated dyad years	Percent mediated
Unlisted	307	1097	376	34.3
Any listing	68	505	130	25.7
US	52	429	107	24.9
EU	27	260	37	14.2
UN	15	175	15	8.6

Note: The mediation rate is the share of dyad-years with at least one recorded mediation event.

We now turn to our regression results, illustrated in Figures 2, 3, and 4. We present results visually, using coefficient plots for key variables, with full regression tables in the online appendix. The results provide no support for the baseline hypothesis that proscription reduces the likelihood of mediation. However, we find support for our second hypothesis about the impact of multilateral proscription regimes, and our third hypothesis that state and IGO mediators are more affected by proscription.

Our results for the first two hypotheses are summarized in Figure 2, listing estimated coefficients of interest from four separate models on the y-axis. We begin by noting that listings in general (*any listing*) exhibit no statistically significant association with mediation. This suggests that proscription, when studied over longer periods and across many cases, has a smaller or less consistent effect on mediation than much of the existing literature suggests.

FIGURE 2. Proscription and mediation, linear probability estimates, global panel, 1989-2019



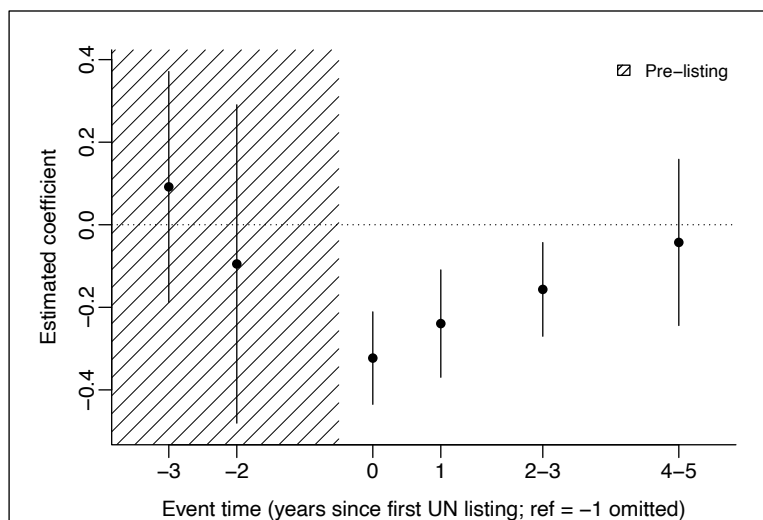
Note: The figure illustrates estimates of proscription on mediation occurrence using linear probability models. Whiskers represent 95 percent confidence intervals using robust standard errors. Listing coefficients from four separate models are shown. Full results in Table A3, Appendix B.

We find support for our second hypothesis, expecting IGO listings to show a more pronounced negative effect on mediation. For UN and EU listings, the coefficient is negative and statistically significant at $p < 0.001$ and $p < 0.05$, respectively, suggesting that their proscriptions are associated with a lower probability of mediation occurrence. Because the listing regimes are nested, the UN coefficient captures the effect of multilateral designation over and above being listed by the EU or US alone. The negative UN and EU effects thus cannot be attributed to proscription in general but reflect the additional constraints imposed by multilateral designation.

Focusing on UN proscription, Figure 3 plots changes in mediation probability over time relative to the year before listing ($t = -1$). Because most UN-listed groups enter the dataset already designated, pre-listing observations are limited, but we find no indication of systematic differences in mediation, suggesting that mediation trends

were similar in listed and non-listed dyads prior to listing and provide no support for a pre-existing downward trend in mediation. Mediation decreases sharply in the years after listing, with the largest drop in year 0 (-0.32 , $p < 0.001$) and a smaller but still significant decline in year 1 (-0.24 , $p < 0.001$) and years 2–3 (-0.16 , $p < 0.01$). This pattern aligns with the theoretical expectation that listings deter mediation immediately after proscription. The negative effect weakens thereafter and becomes statistically insignificant by years 4–5, suggesting that proscription costs may attenuate over time, either because conflicts drag on or because alternative mediators less sensitive to reputational concerns gradually enter the process.

FIGURE 3. Dynamic effects of UN proscription on mediation, global panel

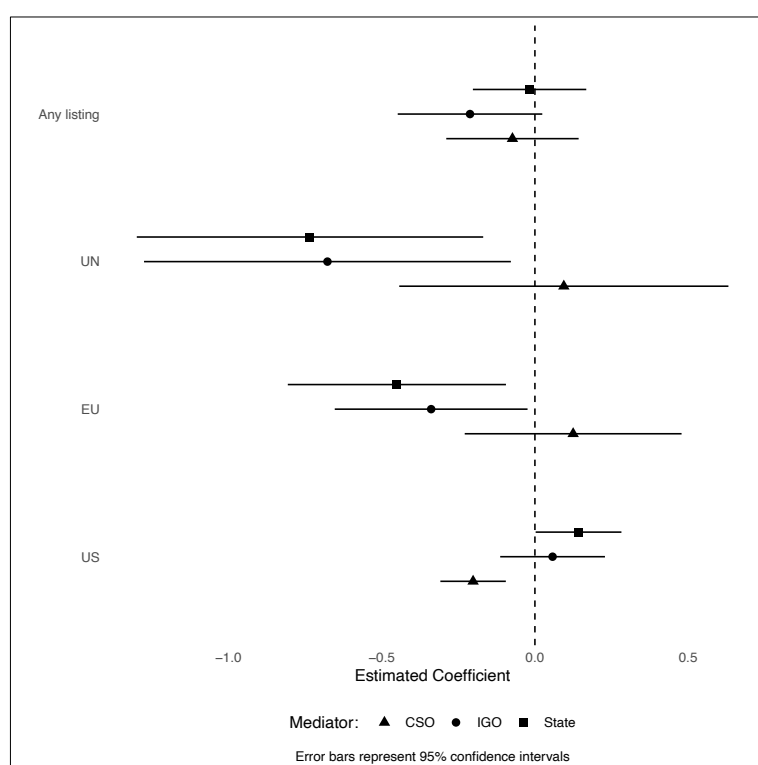


Note: The figure plots estimated differences in mediation probability between listed and non-listed dyads for time periods before and after listing, relative to the year before listing ($t = -1$), with 95 percent confidence intervals. Full results in Table A5, Appendix B.

We also proposed (H3) that state and IGO mediators would be more affected by proscription than CSO mediators. As illustrated in Figure 4, consistent with this conjecture, we find a statistically significant negative association between UN and EU

listings and mediation by states and IGOs, while the coefficients for CSO mediation are non-significant in these cases. These results suggest that multilateral proscriptions have differential effects on prospective mediators, and that some mediator types, notably formal mediators such as states and IGOs, may be more susceptible to the deterrent effects of terrorist designations than CSOs. The exception is US proscription, where this pattern does not hold, again suggesting that the multilateral and US regimes have differential effects.

FIGURE 4. Proscription and mediation, linear probability estimates, global panel, 1989-2019



Note: The figure illustrates estimates of proscription by different actors on actor-specific mediation occurrence using linear probability models with Heckman correction for selection into the mediation sample. Whiskers represent 95 percent confidence intervals using robust standard errors. Listing coefficients from four separate models are shown. Results in Table A6, Appendix B.

To understand whether and how our results are sensitive to alternative data, estimators, and model specifications, we conduct a series of robustness tests, reported in the online appendix.

First, to evaluate whether our findings generalize across different sources and measurement strategies, we rerun our principal models using alternative mediation data. Table A7 presents models estimated on updated data from Svensson (2007) on mediation in civil wars globally, covering 1989-2014, a somewhat shorter period than our main sample. Our key findings on proscription remain unchanged in these data, showing a negative coefficient for the UN ($p < 0.05$).

Second, we use alternative statistical estimators, including linear pooled models (Table A8), generalized linear mixed models (Table A9), and mixed effects models with REML estimation (Table A10). Regardless of the estimator, the results are very similar to our main models, consistently showing a negative coefficient for UN and EU proscriptions.

Third, because governments often face several rebel groups simultaneously, dyads within a country may not be independent. We therefore report models with nested random effects (dyads nested within country) (Table A11) and country-clustered standard errors (Table A12). In both, the UN and EU coefficients remain negative and statistically significant.

Fourth, we re-estimate the models without the cubic polynomial of spell time (Table A13). The UN listing coefficient increases in magnitude (-0.39 vs. -0.33, $p < 0.01$) whereas the EU coefficient remains of a similar size ($p < 0.1$), confirming that the temporal control is not driving our main result.

Fifth, to probe omitted variable bias, we implement Oster bounds analysis (Table A14) (Oster 2019). For the UN, EU, and US listing variables, δ^* values exceed 1, indicating that unobserved confounders would need to be more strongly associated with both treatment and outcome than the full set of observed controls in order to eliminate the estimated effects. The δ^* values are largest for the EU listing (24.7) and remain well above 1 for UN and US listings (9.8 and 8.9, respectively), suggesting that the results are robust to omitted variable bias.

STUDY 2: DIFFERENCE-IN-DIFFERENCE EVIDENCE FROM THE PHILIPPINES AND MALI

By leveraging case-specific longitudinal data, we aim to draw more robust inferences about the causal impact of proscription by the UN – our principal finding above. We use a difference-in-difference design, a quasi-experimental identification strategy that exploits naturally occurring variation in the treatment variable (Cunningham 2021; Morgan and Winship 2015). Specifically, we compare pairs of dyads, one affected by proscription and one not, and examine outcomes before and after proscription. This strategy is similar to the classic difference-in-difference design employed by Card and Krueger (1993) in their study of minimum wage changes on employment. Where Card and Krueger compared two neighboring states within the same country before and after a minimum wage change in one state, we compare two dyads within the same country before and after proscription targeting one dyad but not the other.

To enable this analysis, we require conflict countries that contain two separate dyads, one of which experiences a shift in proscription status (treatment) while the other does not. Additionally, both dyads must offer sufficient and overlapping pre-treatment data, implying that dyads involving a listed actor from conflict onset—fairly

common in our data—are not useful. Applying these criteria yielded eight candidate conflict countries, which were further examined to determine whether they offered sufficient mediation-related interactions for granular longitudinal analysis. Two conflict countries met these data requirements: the Philippines (2000-2004) and Mali (2011-2015).

For each of these conflict countries, we extracted mediation- and negotiation-related events from the Integrated Crisis Early Warning System (ICEWS) dataset (Boschee et al. 2015). The ICEWS data record cooperative and hostile interactions between individuals, groups, and states globally between 1995 and 2023, coding the source, type, and target actor for each event. We subset the data to extract observations pertaining to the relevant dyad and time period, aggregating mediation- and negotiation-related events to the monthly level. For each dyad, we cover 24 months before the listing, the listing month, and the 36 months following the listing. The post-treatment window is informed by Study 1, which indicates that the deterrent effect of UN proscription is concentrated in the first few years after listing.

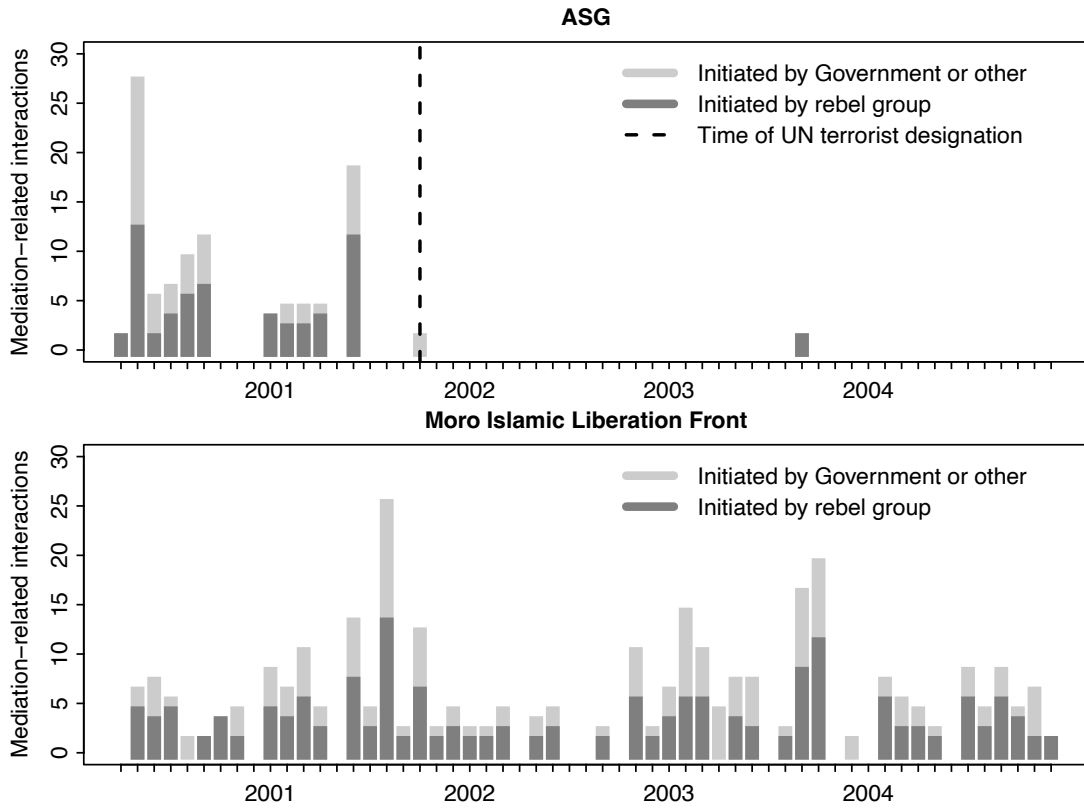
In the Philippines, we compare two civil war dyads, each pitting the government against an armed group with Islamist ideologies, secessionist goals, and operating mainly in Mindanao (Table 2). On October 6, 2001, one of these groups, the Abu Sayyaf Group (ASG), was designated a terrorist organization by the UN, whereas the other, the Moro Islamic Liberation Front (MILF), has never been internationally proscribed.

TABLE 2. Compared civil war dyads, the Philippines.

Rebel group	Conflict time span	Incompatibility	Cumulative battle deaths	UN proscription
Abu Sayyaf Group	1993-	Territory	3,167	October 6, 2001
Moro Islamic Liberation Front	1990-2014	Territory	3,545	-

Figure 5 exhibits the count of monthly mediation- and negotiation-related interactions for each dyad, with the date of the UN proscription of ASG marked by a vertical dashed line. During the pre-treatment period, the monthly count of mediation-related interactions is comparable across dyads, with a mean of 5.1 events involving ASG and 5.5 involving the Moro Islamic Liberation Front. However, following the ASG listing, the mean monthly count of mediation-related interactions involving ASG drops to 0.051, compared with 4.7 for the Moro Islamic Liberation Front. Indeed, the ICEWS data records only a single mediation-related event involving ASG in the three years following the terrorist designation, compared with 92 in the pre-treatment period.

FIGURE 5. Monthly count of mediation- and negotiation-related events involving Abu Sayyaf Group (ASG) and Moro Islamic Liberation Front, March 2000–December 2004. Data: ICEWS.



We estimate the causal impact of proscription on mediation-related events using a difference-in-difference model with the following specification:

$$y_{st} = \gamma_s + \lambda_t + \beta D_{st} + \epsilon_{st}$$

The dependent variable, y_{st} , represents the count of mediation-related interactions involving dyads in month t , γ_s represents dyad fixed effects, λ_t are month fixed effects, D_{st} a treatment indicator, in this case indicating whether the observed dyad involves a proscribed actor at time t , and ϵ_{st} the error term. To account for heteroskedasticity and

autocorrelation, we use robust standard errors clustered on dyad (Cunningham 2021). Table 3 summarizes relevant descriptive statistics and reports the main coefficient of interest. The estimated coefficient, β , is negative and statistically significant, suggesting that, as a result of the terrorist designation of ASG, mediation-related interactions involving this group shifted significantly in the negative direction.

TABLE 3. Difference-in-difference estimates, the Philippines

Rebel group	Mediation-related interactions, monthly		Diff-in-diff	
	m _{pre-treatment}	m _{treatment}	β	Std. error
Abu Sayyaf Group	5.1	0.051	-4.22*	1.78
Moro Islamic Liberation Front	5.5	4.7		

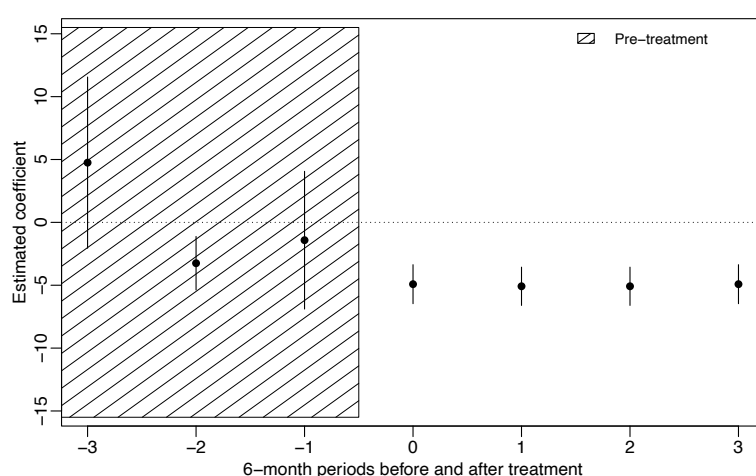
Note: Panel regression with fixed dyad and month effects. Robust standard errors clustered on dyads.

* p<0.05

While the DID design can eliminate unobserved cross-sectional heterogeneity, its causal interpretation hinges on the parallel trends assumption, which implies that the difference between treated and controls is constant on average in the pre-treatment period. In other words, the counterfactual must be plausible: had ASG not been designated terrorists, its involvement in mediation-related interactions would have continued along the same path as before it was listed. A regression model with annual leads and lags (Figure 6) shows that DID coefficients are statistically indistinguishable from zero in two out of three pre-treatment periods but non-zero afterwards. This indicates that both dyads were on a comparable path in their diplomatic interactions

with the government prior to the ASG listing, supporting the parallel trends assumption, but diverged thereafter.

FIGURE 6. Estimates of the effect of proscription on mediation-related events involving Abu Sayyaf Group and Moro Islamic Liberation Front using leads and lags. Data: ICEWS.



Note: Estimated difference in negotiation-related events between groups before and after terrorist designation of ASG (“treatment”). Reference category is t-4.

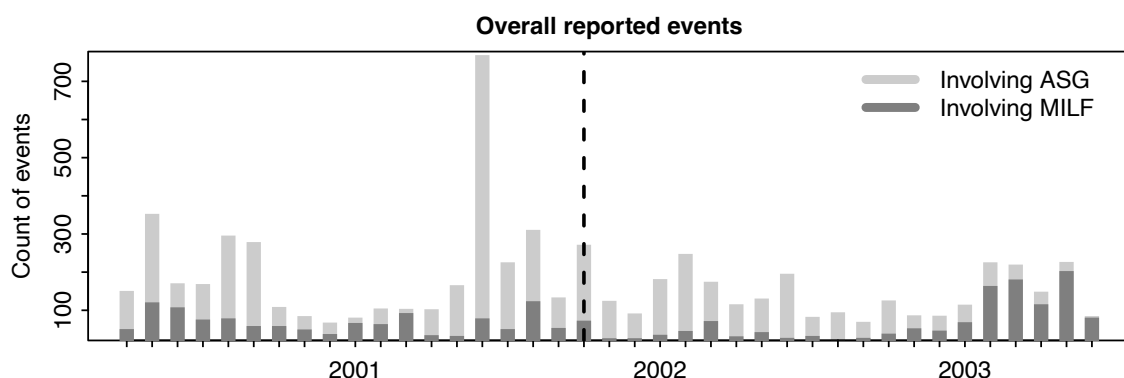
The causal interpretation of the difference-in-difference estimate is further supported by qualitative evidence. The UN proscription of ASG followed the 9/11 terrorist attacks and can reasonably be viewed, in the local context, as an exogenous shock.³ Even though ASG and MILF are Islamist groups seeking self-determination for the *Bangsamoro*, the Moro nation, and were thought to have links with Al-Qaeda and Jemmah Islamiyya to varying degrees (Abuza 2005; Santos Jr 2010), ASG had

³ Terrorist attacks have been used to model exogenous shocks on a wide range of phenomena (Böhmelt, Bove, and Nussio 2020; Cochrane and Piazzesi 2002; Harding and Nwokolo 2023).

previously been perceived more as bandits than terrorists due to their involvement in kidnapping for ransom (Santos Jr and Dinampo 2010).

A basic placebo test rules out the rival explanation that the observed patterns are due to shifts in general ASG behavior. As shown in Figure 7, there are no significant temporal shifts in the monthly count of reported events involving ASG in the data. An analogously specified DID model suggests no temporal shift in ASG reporting. There may be slightly fewer events from around 2003, but this cannot account for the sudden drop in diplomatic interactions in October 2001.

FIGURE 7. Placebo test. Monthly count of all events involving Abu Sayyaf Group (ASG) and Moro Islamic Liberation Front (MILF), March 2000–July 2003. Data: ICEWS.



While the difference-in-difference analysis neutralizes many sources of confounding, there is still the possibility that our results are distorted by time-varying factors coincidental with proscription. For instance, the UN proscription of ASG may have been anticipated internationally before the formal decision. If so, these anticipation effects could have affected diplomatic interactions with ASG between the 9/11 attacks and the October 6, 2001, terrorist designation. Given that our data contain no diplomatic interactions with ASG during September, the evidence does not contradict

that interpretation. However, if such anticipation effects exist, they would not negate the general finding, only imply that proscription operates with a “lead effect”.

The drop in mediation could in principle also reflect the counterterror pressure that accompanied the listing rather than the listing itself. To probe this, we add monthly battle deaths to the model. The estimated effect is virtually unchanged and battle deaths are insignificant, suggesting that the intensity of the fighting does not account for the decline in mediation-related interactions involving ASG.

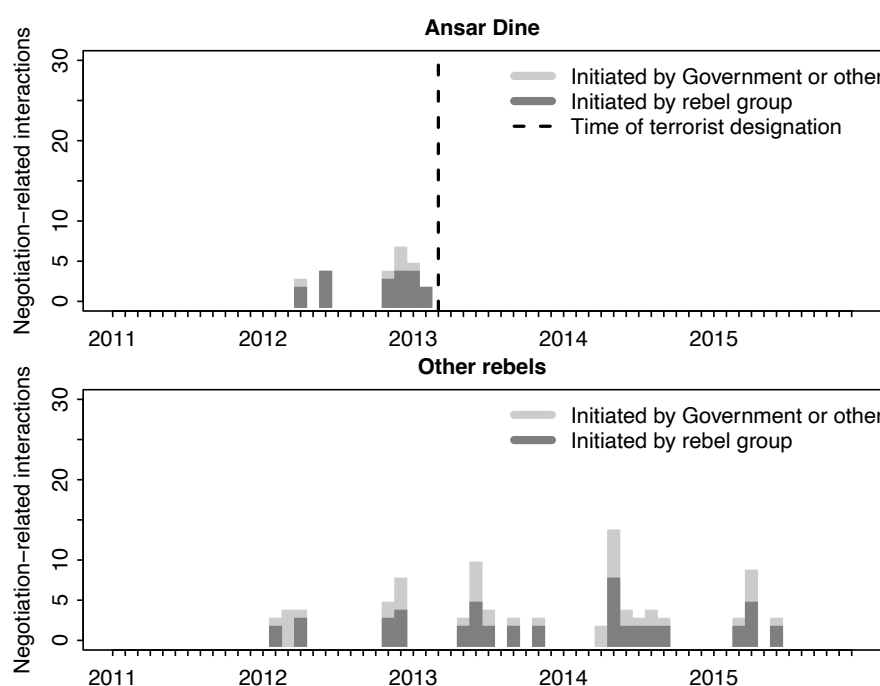
In Mali, we compare mediation- and negotiation-related events involving Ansar al-Dine, an Islamist rebel group proscribed by the UN in 2013, with those involving all other rebel groups (to ensure sufficient data). See Table 4 and Figure 8.

TABLE 4. Difference-in-difference estimates, Mali

Rebel group	Mediation-related interactions, monthly		Diff-in-diff	
	m _{pre-treatment}	m _{treatment}	β	Std. error
Ansar Dine	1.34	0	-2.20**	0.72
Other rebels	0.73	1.59		

Note: Panel regression with fixed dyad and month effects. Robust standard errors clustered on dyads.
 ** p<0.01.

FIGURE 8. Longitudinal data, Mali



The DID coefficient is negative and statistically significant, indicating that the proscription of Ansar al-Dine is associated with a reduction in mediation-related interactions, consistent with our theoretical conjecture. Although the placebo test does not rule out alternative explanations with the same degree of confidence as in the Philippines case, the observed pattern remains informative. Proscription is associated not only with a decline in mediation-related events but also, with some delay, a reduction in overall reported events (Figure A2). This decline is plausibly linked to the intensified militarized response following proscription, including France's Operation Serval (and later Operation Barkhane), which constrained Ansar al-Dine's operational visibility.

Overall, the data from these two cases are largely consistent with the interpretation that UN proscription contributed to the reduction in diplomatic interactions with ASG and Ansar al-Dine. Combined with the findings from Study 1, which revealed a negative correlation between UN proscription and mediation

occurrence in a larger sample, these findings strengthen our belief in the proposed theory that proscription by some actors reduces mediation.

CONCLUSION

This study provides the first comprehensive, quantitative investigation of the effect of proscription on mediation in civil wars. Our findings demonstrate that while proscription in general has no discernible effect on the occurrence of mediation, proscription by the UN is associated with significant reductions in mediation probabilities. We also find that state and IGO mediators are more affected by proscription than CSO mediators, suggesting that the latter are more able to circumvent the material and social constraints that flow from terrorist designations.

Our study refines the conventional wisdom in crucial ways. We show that, on average, proscriptions by powerful actors, such as the US, do not significantly impede the occurrence of mediation, whereas those by IGOs, typically understood as less powerful, present a more binding constraint. This suggests that the deterrent effect of terrorist designations on prospective mediators flows primarily via social and reputational mechanisms, enhanced by the broader membership and legitimacy of IGOs such as the UN, rather than through legal or material restrictions. By contrast, proscriptions by individual states, even criminalization by powerful ones like the US, might be more easily circumvented or selectively ignored.

In broader terms, the study advances our understanding of the interaction between coercive and non-coercive instruments in conflict resolution. First, our findings speak to the literature on naming and shaming (Bassan-Nygate 2025; Kelley and Simmons 2021; Rousseau 2026; Zhou, Kiyani, and Crabtree 2023). This literature has mainly asked whether public condemnation changes the behavior of the actors it

targets. Our results suggest that condemnation can also operate indirectly, by altering the incentives and constraints of governments and prospective third parties and thereby changing how they behave vis-à-vis the targeted group. This suggests that the effects of stigmatizing international labels extend beyond their targets, with consequences for conflict resolution that should be more broadly examined.

Second, in the sanctions literature, considerable attention has been devoted to how coercive measures influence the behavior of targeted actors or provide mediators with leverage to push negotiations forward (Biersteker, Brubaker, and Lanz 2022; Escribà-Folch 2010; Hultman and Peksen 2017; Radtke and Jo 2018). Our findings instead highlight a neglected spillover effect: rather than bolstering third-party mediation, proscription-related sanctions can make it more difficult for mediation to occur. This suggests that scholarship on sanctions should consider a wider range of unintended effects.

Third, the study refines our understanding of the supply and demand for mediation (e.g., Clayton and Dorussen 2022; Clayton and Gleditsch 2014; Greig and Regan 2008), which has not previously considered proscription effects. By doing so, it positions proscription by multilateral actors as a primary candidate for explaining the “decline of mediation” puzzle (Lundgren and Svensson 2020)—the observation that, despite an increasing systemic technical capability to mediate armed conflict in recent years, a decreasing proportion of active conflicts have received mediation since the early 2000s. The co-temporality between the rise of proscription and the decline of mediation—both unfolding in the last two decades—is suggestive in its own right; our evidence here goes a long way further, showing that the association can be identified when controlling for a long range of possible confounders and in more demanding causal tests. Importantly, this effect holds even after accounting for groups’ behavior

and ideologies, indicating that it is the institutional act of proscription itself, rather than changes in the character of conflict actors, that drives the patterns we observe. Judging by the evidence examined here, proscription is an important explanation for why mediation cannot keep pace with the increase in the number of conflicts (Duursma et al. 2025).

Our findings emerge from the most comprehensive study to date, but the question of proscription and mediation remains far from resolved. One area needing further research concerns additional conditional effects: that is, how the effect of proscription varies based on characteristics such as the type of listed group, imposed sanctions, or international norms. We began to examine a few such conditions here, but further work is needed. Another intriguing question is whether the withdrawal of some mediators due to proscription has created opportunities for other types of mediators to step in. Our findings suggest that proscription disproportionately affected mediators with formal mandates, such as states or IGOs, compared to mediators like NGOs. This points to the need for future research to investigate a potential substitution effect, with smaller, more nimble actors increasingly mediating conflicts involving proscribed actors.

The study of mediation has important policy implications. If proscription impedes mediation, the likelihood of ceasefires and peace agreements will also be reduced (Clayton and Dorussen 2022). Our findings highlight inconsistencies in international strategic priorities for handling contemporary armed conflict, specifically the tension between the conflict resolution regime and the broader proscription regime. This tension resembles the “peace-justice dilemma” discussed in, for example, the transitional justice literature (e.g., Prorok 2017), where well-intentioned efforts to hold wrongdoers accountable can make conflicts harder to resolve. By showing when and

how proscription influences mediation, and how this varies across regimes and mediator types, our study offers policymakers insights that can help reduce the tension between conflict resolution efforts and proscription policies.

REFERENCES

- Abbott, Kenneth W, Robert O Keohane, Andrew Moravcsik, Anne-Marie Slaughter, and Duncan Snidal. 2000. "The concept of legalization." *International organization* 54 (3):401–419.
- Abrahms, Max. 2008. "What Terrorists Really Want: Terrorist Motives and Counterterrorism Strategy." *International Security* 32 (4):78–105.
- Abuza, Zachary. 2005. *Balik-Terrorism: The Return of the Abu Sayyaf*. Strategic Studies Institute, US Army War College.
- Avdan, Nazli, Yasutaka Tominaga, Chia-yi Lee, and Cagil Albayrak. 2025. "Name, Shame, and Blame: Terrorist Designation and Peace Negotiations." *Terrorism and Political Violence*:1–17.
- Bapat, Navin A, and T Clifton Morgan. 2009. "Multilateral versus Unilateral Sanctions Reconsidered: A Test Using New Data." *International Studies Quarterly* 53 (4):1075–1094.
- Bassan-Nygate, Lotem. 2025. "How Does Shaming Human Rights Violators Abroad Shape Attitudes at Home?" *British Journal of Political Science* 55:e4.
- Beardsley, Kyle. 2008. "Agreement without peace? International mediation and time inconsistency problems." *American Journal of Political Science* 52 (4):723–740.
- Beardsley, Kyle. 2011. *The Mediation Dilemma*. Cornell University Press.

- Beardsley, Kyle, David E. Cunningham, and Peter B. White. 2018. "Mediation, Peacekeeping, and the Severity of Civil War." *Journal of Conflict Resolution* 63 (7):1682–1709.
- Beck, Colin J, and Emily Miner. 2013. "Who gets designated a terrorist and why?" *Social forces* 91 (3):837–872.
- Bercovitch, Jacob. 1992. "The Structure and Diversity of Mediation in International Relations." In *Mediation in International Relations: Multiple Approaches to Conflict Management*, edited by Jacob Bercovitch and J. Z. Rubin, 1–29. New York: St Martin's Press.
- Best, Rebecca H, and Simanti Lahiri. 2021. "Hard choices, soft targets: Terror proscription and strategic targeting decisions of FTO." *International Interactions* 47 (6):955–985.
- Bialostozky, Noah. 2011. "Material Support of Peace? The on-the-ground consequences of US and international material support of terrorism laws and the need for greater legal precision." *The Yale Journal of International Law Online* 36:59–73.
- Biersteker, Thomas J, Rebecca Brubaker, and David Lanz. 2022. "Exploring the Relationships Between UN Sanctions and mediation." *Global Governance: A Review of Multilateralism and International Organizations* 28 (2):180–202.
- Birkeland, Øystein. 2014. "The Impact of Terrorist Proscriptions on Peace Processes-Sri Lanka's 5th Peace Process." Master thesis, University of Oslo.
- Böhmelt, Tobias. 2016. "The importance of conflict characteristics for the diffusion of international mediation." *Journal of Peace Research* 53 (3):378–391.

- Böhmelt, Tobias, Vincenzo Bove, and Enzo Nussio. 2020. "Can Terrorism Abroad Influence Migration Attitudes at Home?" *American Journal of Political Science* 64 (3):437–451.
- Boon-Kuo, Louise, Ben Hayes, Vicki Sentas, and Gavin Sullivan. 2015. Building Peace in Permanent War: Terrorist Listing and Conflict Transformation. London: International State Crime Initiative.
- Boschee, Elizabeth, Jennifer Lautenschlager, Sean O'Brien, Steve Shellman, James Starz, and Michael Ward. 2015. ICEWS Coded Event Data. Harvard Dataverse, V37.
- Bourne, Angela K. 2018. "Securitization and the Proscription of Terrorist Organizations in Spain." *Terrorism and Political Violence* 30 (2):318–335.
- Card, David, and Alan B. Krueger. 1993. Minimum Wages and Employment: A Case Study of the Fast Food Industry in New Jersey and Pennsylvania. National Bureau of Economic Research Cambridge, Mass., USA.
- Carter, David B, and Curtis S Signorino. 2010. "Back to the future: Modeling time dependence in binary data." *Political Analysis* 18 (3):271–292.
- Clayton, Govinda. 2013. "Relative rebel strength and the onset and outcome of civil war mediation." *Journal of Peace Research* 50 (5):609–622.
- Clayton, Govinda, and Han Dorussen. 2022. "The effectiveness of mediation and peacekeeping for ending conflict." *Journal of Peace Research* 59 (2):150–165.
- Clayton, Govinda, and Kristian Skrede Gleditsch. 2014. "Will We See Helping Hands? Predicting civil war mediation and likely success." *Conflict Management and Peace Science* 31 (3):265–284.
- Cochrane, John H., and Monika Piazzesi. 2002. "The Fed and Interest Rates—A High-Frequency Identification." *American Economic Review* 92 (2):90–95.

- Cortright, David, Alistair Millar, Linda Gerber-Stellingwerf, George A Lopez, Eliot Fackler, and Joshua Weaver. 2011. *Friend not foe: Opening spaces for civil society engagement to prevent violent extremism*. Notre Dame, IN: Fourth Freedom Forum/Kroc Institute, University of Notre Dame.
- Crocker, Chester A, Fen Osler Hampson, and Pamela R Aall. 2003. "Ready for prime time: The when, who, and why of international mediation." *Negotiation Journal* 19 (2):151–167.
- Cunningham, Scott. 2021. *Causal Inference: The Mixtape*: Yale University Press.
- de Goede, Marieke. 2012. *Speculative Security: The politics of pursuing terrorist monies*. Minneapolis and London: University of Minnesota Press.
- de Goede, Marieke. 2018. "Proscription's Futures." *Terrorism and Political Violence* 30 (2):336–355.
- Di Salvatore, Jessica, Magnus Lundgren, Kseniya Oksamytna, and Hannah M Smidt. 2022. "Introducing the peacekeeping mandates (PEMA) dataset." *Journal of Conflict Resolution* 66 (4-5):924–951.
- Donohue, William, and Moty Cristal. 2011. "Growing Out in Organization." In *Engaging Extremists: Trade-offs, Timing, and Diplomacy*, edited by I. William Zartman and Guy Olivier Faure, 57–80. Washington D.C.: United States Institute for Peace.
- Drezner, Daniel W. 2000. "Bargaining, Enforcement, and Multilateral Sanctions: When is Cooperation Counterproductive?" *International Organization* 54 (1):73–102.
- Dudouet, Véronique. 2010. *Mediating Peace with Proscribed Armed Groups*. In *Special Report 239*. Washington D.C.: United States Institute for Peace.

- Dudouet, Véronique. 2011. Counter-Terrorism Legislations: Impediments for Conflict Transformation. In *Berghof Policy Brief 02*. Berlin: Berghof Conflict Research.
- Dumasy, Teresa, and Sophie Haspeslagh. 2016. Proscribing Peace: The Impact of Terrorist Listing on Peacebuilding Organisations. London: Conciliation Resources.
- Duursma, Allard, Janek Bruker, Carina Diener, Samantha Gamez, Valentin Geier, and Arianna Guidolin. 2025. Introducing the Peace Observatory: Peace Negotiations in Armed Conflicts, 1989-2023. Unpublished manuscript.
- Eck, Kristine, and Lisa Hultman. 2007. "One-Sided Violence Against Civilians in War: Insights from New Fatality Data." *Journal of Peace Research* 44 (2):233–246.
- El Masri, Mirna, and Brian J. Phillips. 2024. "Threat Perception, Policy Diffusion, and the Logic of Terrorist Group Designation." *Studies in Conflict & Terrorism* 47 (8):838–861.
- Escribà-Folch, Abel. 2010. "Economic Sanctions and the Duration of Civil Conflicts." *Journal of Peace Research* 47 (2):129–141.
- Fearon, James D. 1995. "Rationalist Explanations for War." *International Organization* 49 (3):379–414.
- Feinberg, Ayal, and J Michael Greig. 2021. "Does the Distribution of Ethnic Kin-Groups Explain Mediation in Territorial Conflicts?" *International Negotiation* 27 (1):157–189.
- Gartner, Scott Sigmund, and Jacob Bercovitch. 2006. "Overcoming Obstacles to Peace: The Contribution of Mediation to Short-lived Conflict Settlements." *International Studies Quarterly* 50 (4):819–840.
- Gowan, Richard, and Stephen John Stedman. 2018. "The international regime for treating civil war, 1988–2017." *Dædalus* 147 (1):171–184.

- Greig, J Michael. 2001. "Moments of opportunity: Recognizing conditions of ripeness for international mediation between enduring rivals." *Journal of Conflict Resolution* 45 (6):691–718.
- Greig, J Michael. 2015. "Rebels at the Gates: Civil War Battle Locations, Movement, and Openings for Diplomacy." *International Studies Quarterly* 59 (4):680–693.
- Greig, Michael , and Paul F. Diehl. 2012. *International Mediation: Polity*.
- Greig, Michael J., and Patrick M. Regan. 2008. "When Do They Say Yes? An Analysis of the Willingness to Offer and Accept Mediation in Civil Wars." *International Studies Quarterly* 52 (4):759–781.
- Gross, Joshua. 2011. "Proscription Problems: The Practical Implications of Terrorist Lists on Diplomacy and Peacebuilding in Nepal." *Praxis* 26:38–59.
- Harding, Robin, and Arinze Nwokolo. 2023. "Terrorism, Trust, and Identity: Evidence from a Natural Experiment in Nigeria." *American Journal of Political Science*.
- Haspeslagh, Sophie. 2013. "'Listing Terrorists': The Impact of Proscription on Third-Party Efforts to Engage Armed Groups in Peace Processes—a Practitioner's Perspective." *Critical Studies on Terrorism* 6 (1):189–208.
- Haspeslagh, Sophie. 2021a. "The 'linguistic ceasefire': Negotiating in an age of proscription." *Security Dialogue* 52 (4):361–379.
- Haspeslagh, Sophie. 2021b. *Proscribing peace: How listing armed groups as terrorists hurts negotiations*: Manchester University Press.
- Haspeslagh, Sophie. 2025. "What happens to the 'terrorists'?" *European Journal of International Security* 10 (1):27–39.
- Haspeslagh, Sophie, and Véronique Dudouet. 2011. *Mediating Peace with Proscribed Armed Groups: A Policy Workshop Report on the Implications of European Union (EU) Counter-Terrorism Legislation for Mediation and Support*

- for Peace Processes. London and Berlin: Conciliation Resources and Berghof Peace Support.
- Haspeslagh, Sophie, and Véronique Dudouet. 2015. "Conflict Resolution Practice in Conflicts Marked by Terrorist Violence: A Scholar-Practitioner Perspective." In *Researching Terrorism, Peace and Conflict Studies*, 103–123. Routledge.
- Helgesen, Vidar. 2007. How Peace Diplomacy Lost Post 9/11: What implications are there for Norway? In *Oslo Files on Defence and Security 03/2007*. Oslo: Norwegian Institute for Defence Studies.
- Howell, Jude. 2006. "The global war on terror, development and civil society." *Journal of International Development: The Journal of the Development Studies Association* 18 (1):121–135.
- Hultman, Lisa, and Dursun Peksen. 2017. "Successful or Counterproductive Coercion? The Effect of International Sanctions on Conflict Intensity." *Journal of Conflict Resolution* 61 (6):1315–1339.
- Jarvis, Lee, and Tim Legrand. 2017. "Preaching to the Converted: Parliament and the proscription ritual." *Political Studies* 65 (4):947–965.
- Jarvis, Lee, and Tim Legrand. 2018. *The Proscription or Listing of Terrorist Organisations: Understanding, assessment, and international comparisons*. Taylor & Francis.
- Jarvis, Lee, and Tim Legrand. 2020. *Banning them, Securing us? Terrorism, parliament and the ritual of proscription*. Manchester: Manchester University Press.
- Jo, Hyeran, Brian J Phillips, and Joshua Alley. 2026. "When Do Sanctions On Terrorist Groups Work?" *Security Studies*:1–31.

- Kelley, Judith G, and Beth A Simmons. 2021. "Governance by other means: Rankings as regulatory systems." *International Theory* 13 (1):169–178.
- Kirkpatrick, Daniel. 2019. "Proscribing the past or de-proscribing the future: a genealogy and critical discourse analysis of proscription in the North of Ireland, 1887–2017." *Critical Studies on Terrorism* 12 (2):317–338.
- Kleibor, Marieke, and Paul't Hart. 1995. "Time to talk? Multiple Perspective on Timing of International Mediation." *Cooperation and Conflict* 30 (4):307–348.
- Kreutz, Joakim, and Magda Lorena Cárdenas. 2024. "The Women and Men that Make Peace: Introducing the Mediating Individuals (M-IND) dataset." *Journal of Peace Research*:182–192.
- Lanz, David. 2011. "Who Gets a Seat at the Table? A Framework for Understanding the Dynamics of Inclusion and Exclusion in Peace Negotiations." *International Negotiation* 16 (2):275–295.
- Lee, Chia-yi, and Yasutaka Tominaga. 2024. "The determinants of terrorist listing." *Journal of Conflict Resolution* 68 (1):53–79.
- Lundgren, Magnus. 2016. "Conflict management capabilities of peace-brokering international organizations, 1945–2010: A new dataset." *Conflict Management and Peace Science* 33 (2):198–223.
- Lundgren, Magnus, Emma Janson, and Martin Lundqvist. 2024. "Introducing the Proscription of Armed Actors Dataset (PAAD)." *Journal of Peace Research*.
- Lundgren, Magnus, and Isak Svensson. 2020. "The Surprising Decline of International Mediation in Armed Conflicts." *Research & Politics* 7 (2):<https://doi.org/10.1177/2053168020917243>.
- Matesan, Ioana Emy. 2022. "Ripeness in Negotiating with Proscribed Terrorist Groups." *Ethnopolitics* 21 (2):178–189.

- Meier, Vanessa, Niklas Karlén, Therése Pettersson, and Mihai Croicu. 2023. "External Support in Armed Conflicts: Introducing the UCDP External Support Dataset (ESD), 1975–2017." *Journal of peace research* 60 (3):545–554.
- Melin, Molly M, Scott Sigmund Gartner, and Jacob Bercovitch. 2013. "Fear of rejection: The puzzle of unaccepted mediation offers in international conflict." *Conflict Management and Peace Science* 30 (4):354–368.
- Melin, Molly M, and Paul Olander. 2019. "Supply-side incentives for mediation: which actors mediate international crises and why?" In *Research handbook on mediating international crises*, 94–108. Edward Elgar Publishing.
- Mitchell, Christopher R. 1988. "The motives for mediation." In *New approaches to international mediation*, edited by Christopher R. Mitchell and Keith Webb, 29–51. New York: Greenwood Press.
- Morgan, Stephen L., and Christopher Winship. 2015. *Counterfactuals and Causal Inference*: Cambridge University Press.
- Nadarajah, Suthaharan, and Dhananjayan Sriskandarajah. 2005. "Liberation Struggle or Terrorism? The politics of naming the LTTE." *Third World Quarterly* 26 (1):87–100.
- Neuenkirch, Matthias, and Florian Neumeier. 2015. "The Impact of UN and US Economic Sanctions on GDP Growth." *European Journal of Political Economy* 40:110–125.
- Neumann, Peter R. 2017. "Don't Follow the Money: The problem with the war on terrorist financing." *Foreign Affairs* 96 (4):93–102.
- Oster, Emily 2019. "Unobservable selection and coefficient stability: Theory and evidence." *Journal of Business & Economic Statistics* 37 (2):187–204.

- Palmiano Federer, Julia. 2019. "We Do Negotiate with Terrorists: Navigating Liberal and Illiberal Norms in Peace Mediation." *Critical Studies on Terrorism* 12 (1):19–39.
- Pantuliano, Sara, Kate Mackintosh, Samir Elhawary, and Victoria Metcalfe. 2011. *Counter-Terrorism and Humanitarian Action: Tensions, Impact and Way Forward*. London: Overseas Development Institute.
- Pettersson, Therése, and Magnus Öberg. 2020. "Organized violence, 1989–2019." *Journal of Peace Research* 57 (4):597–613.
- Phillips, Brian J. 2019. "Foreign Terrorist Organization designation, international cooperation, and terrorism." *International Interactions* 45 (2):316–343.
- Prorok, Alyssa K. 2017. "The (in) compatibility of peace and justice? The International Criminal Court and civil conflict termination." *International organization* 71 (2):213–243.
- Radtke, Mitchell, and Hyeran Jo. 2018. "Fighting the Hydra: United Nations Sanctions and Rebel Groups." *Journal of Peace Research* 55 (6):759–773.
- Regan, Patrick M., and Allan C. Stam. 2000. "In the Nick of Time: Conflict Management, Mediation Timing, and the Duration of Interstate Disputes." *International Studies Quarterly* 44.
- Renner, Judith, and Alexander Spencer. 2012. *Reconciliation after terrorism: strategy, possibility or absurdity?*: Routledge.
- Rousseau, Elise. 2026. "Disentangling the "Pathways of Exclusion": Naming, Blaming, Shaming, and Stigmatization in World Politics." *Global Studies Quarterly* 6 (1):ksag028.
- Ruhe, Constantin. 2020. "Impeding fatal violence through third-party diplomacy: The effect of mediation on conflict intensity." *Journal of Peace Research*.

- Santos Jr, Soliman M. 2010. "War and Peace on the Moro Front: Three Standard Bearers, Three Forms of Struggle, Three Tracks (Overview)." *Primed and Purposeful: Armed groups and human security efforts in the Philippines*:58–90.
- Santos Jr, Soliman M., and Octavio A. Dinampo. 2010. "Abu Sayyaf Reloaded: Rebels, Agents, Bandits, Terrorists (Case Study)." *Primed and Purposeful: Armed groups and human security efforts in the Philippines*:115–138.
- Savun, Burcu. 2008. "Information, bias, and mediation success." *International studies quarterly* 52 (1):25–47.
- Schwartz, Aaron. 2014. "National Security and the Protection of Constitutional Liberties: How the Foreign Terrorist Organization List Satisfies Procedural Due Process." *Penn State Journal of Law & International Affairs* 3 (1):292–323.
- Sentas, Vicki. 2018. "Terrorist Organization Proscription as Counterinsurgency in the Kurdish Conflict." *Terrorism and Political Violence* 30 (2):298–317.
- Sentas, Victoria. 2014. *Traces of terror: Counter-terrorism law, policing, and race*: Oxford University Press.
- Shuaibi, Nourah, and Sarah Almutairi. 2024. "The Proscription of The East Turkestan Islamic Movement and Its Impact on The Golden Age Between China and Great Britain." *Political Sciences Journal* (68):1–32.
- Sørnbø, Gunnar, Jonathan Goodhand, Bart Klem, Ada Elisabeth Nissen, and Hilde Selbervik. 2011. *Pawns of Peace: Evaluation of Norwegian peace efforts in Sri Lanka, 1997-2009*. Chr. Michelsen Institute/School of Oriental and African Studies, University of London.
- START, National Consortium for the Study of Terrorism and Responses to Terrorism. 2021. "Global Terrorism Database (GTD)." University of Maryland. <https://www.start.umd.edu/gtd>

- Sullivan, Gavin, and Ben Hayes. 2010. *Blacklisted: Targeted sanctions, preemptive security and fundamental rights*. Berlin: European Center for Constitutional and Human Rights.
- Sundberg, Ralph, and Erik Melander. 2013. "Introducing the UCDP georeferenced event dataset." *Journal of Peace Research* 50 (4):523–532.
- Svensson, Isak. 2007. "Bias, Bargaining, and Peace Brokers: How Rebels Commit to Peace." *Journal of Peace Research* 44 (2):177–194.
- Svensson, Isak, and Desirée Nilsson. 2018. "Disputes over the Divine: Introducing the Religion and Armed Conflict (RELAC) data, 1975-2015." *Journal of Conflict Resolution* 62 (5):1127–1148.
- Timoneda, Joan C. 2021. "Estimating group fixed effects in panel data with a binary dependent variable: How the LPM outperforms logistic regression in rare events data." *Social Science Research* 93:102486.
- Toros, Harmonie. 2012. *Terrorism, Talking and Transformation: A Critical Approach*: Routledge.
- Touval, Saadia, and I. W. Zartman. 2001. "International Mediation in the Post-Cold War Era." In *Turbulent Peace: The Challenges of Managing International Conflict*, edited by Chester Crocker, Fen Osler Hampson and Pamela Aall, 427–443. Washington, D.C.: United States Institute of Peace Press.
- US Department of State. 2025. "Terrorist Designations and State Sponsors of Terrorism." <https://www.state.gov/terrorist-designations-and-state-sponsors-of-terrorism/> (last accessed October 2, 2025).
- US Supreme Court. 2010. *Holder v. Humanitarian Law Project*. 561 U.

- Walker, Justine Suzanne. 2017. "The Foreign Policy Tool of Sanctions, Conflict and Ensuring Continued Access to Finance." *Journal of Financial Crime* 24 (3):480–490.
- Wallensteen, Peter, and Isak Svensson. 2014. "Talking peace: International mediation in armed conflicts." *Journal of Peace Research* 51:315–327.
- Whitfield, Teresa 2024. Still Time to Talk: Adaptation and innovation in peace mediation. Accord.
- Zartman, I. W. 2003. "The Timing of Peace Initiatives: Hurting Stalemates and Ripe Moments." In *Contemporary Peacemaking: Conflict, Violence and Peace Processes*, edited by John Darby and Roger Mac Ginty. Palgrave Macmillan.
- Zhang, Chi. 2021. "The double-track system of terrorism proscription in China." *Terrorism and political violence* 33 (3):505–526.
- Zhou, Yuan, Ghashia Kiyani, and Charles Crabtree. 2023. "New evidence that naming and shaming influences state human rights practices." *Journal of Human Rights* 22 (4):451–468.