

Buying Words & Deeds: A Conjoint Analysis of Aid-for-Policy and Aid-for-Rhetoric Deals

Abstract

Under what conditions could a state secure policy concessions on terrorism from another state in exchange for foreign aid? Foreign aid is utilised by donor states in ways that support and facilitate the realisation of their national interests and foreign policy goals, including those related to terrorism. For instance, aid provision can secure donor states from international terrorism and its second-order effects; their associated risks stand to be ameliorated by improvements to recipients' counterterrorism efficacy which might, in turn, come about through aid's capacity to facilitate improved domestic institutions, material wherewithal, and human capital in recipients. Beyond these instrumental aims, however, foreign aid may serve more expansive political ambitions, i.e., of donor-recipient political alignment, programmatic counterterrorism coherency, or specific changes to recipients' terrorism policies. Political conditionalities are known to be attached to aid offers to motivate policy change in aid recipients. But it is unclear whether conditionalities with respect to terrorism would require too costly a concession to see such aid-for-policy deals made in practice, or how these would be evaluated against other aid offer features. In this paper, I conduct a preregistered conjoint experiment with a foreign policy elite and expert sample to identify how an aid offer's features, including any political conditionalities, affect perceptions of its desirability relative to alternatives. The findings confirm that adding political conditionalities regarding terrorism policy to aid offers adversely affects aid offer desirability. However, political conditionality effects are substantively modest relative to other features and do not vary by the type of demand made, i.e., either changes to domestic legislation or changes to one's public political positions on issues of terrorism. Overall, expert attitudes towards hypothetical aid offers suggest states' terrorism rhetoric and/or domestic legislation could be shaped by foreign aid with political conditionalities.

Introduction

International terrorism's transnational nature necessitates cooperation between states for optimal counterterrorism measures (Sandler, 2005). Yet this functional need for collaboration is not matched by convergent state preferences about how such cooperation should be achieved. International counterterrorism has proven particularly intractable, with persistent disagreements among states about how terrorism should be treated politically and addressed through policy. As Daniel Byman (2006) notes, even allies "have their own distinct interests and approaches to counterinsurgency and counterterrorism" and differ on basic questions: "What is the overall level of risk? Who is the primary enemy? How much should different allies contribute? And what is the best means to fight the insurgents [or terrorists]?" These discrepancies among even closely aligned states require deliberate intervention to spur cooperation that might otherwise emerge organically if preferences were more aligned. One mechanism states have employed, particularly the United States, is the provision of substantial foreign aid to potential counterterrorism partners in order to elicit policy concessions and proactive counterterrorism measures (Bandyopadhyay, Sandler, and Younas, 2011).

These aid-for-policy deals appear, in at least some limited respects, to be effective: the empirical results from the second substantive chapter of this thesis suggest that the provision of conflict and peace aid is associated with the recipient's adoption of rhetoric on terrorism's "root causes" that is more similar to, and ostensibly more compatible with, that of their donors (Carter, 2025). Debates about terrorism's root causes fundamentally bear on sensitive, contested questions of terrorism's (il)legitimacy, especially in cases of wars whose participants invoke self-determination or national independence as justification, meaning that changing one's rhetoric or stance on this issue is nontrivial.

However, it's not immediately obvious that this apparent association between foreign aid and changes to recipients' positions on terrorism can be attributed to the coercive and/or incentivisory effects of foreign aid that might be exerted via political conditionalities. After all, the relative popularity of Chinese development finance and South-to-South cooperation schemes has been explained with reference to their *lack* of political conditionality (Woods, 2008; Watkins, 2022). This is juxtaposed against Western foreign aid, which frequently requires (explicitly or otherwise) that the recipient commit to honouring human rights, follow standards of appropriate state conduct, increase trade liberalisation, etc. Notably, political conditionality with respect to terrorism policies is among its most severe and sensitive forms. Rather than requiring compliance with abstract behavioural standards or democratic norms, it asks recipient countries to concede to specific donor demands on issues of domestic and international security, an area in which governments zealously guard their sovereign autonomy (Sandler, 2005, p. 80).

Why, then, do we observe acquiescence to conditions which appear quite severe and constraining with respect to the recipient's sovereignty: offers that expect significant concessions to the United States or its allies, outward-facing commitments to the vision underpinning the Global War on Terror, military cooperation and/or basing rights to the donor, or changes to one's domestic legislative counterterrorism framework? Conventional wisdom expects significant aversion on the part of recipients towards aid offers with onerous political conditions. But recent scholarship finds via a conjoint survey experiment that elites are, at most, "indifferent" to good governance political conditionalities when considering aid offers (Blair, Custer, and Roessler, 2024). It's unclear whether a similar indifference would be observed with respect to terrorism policy conditionalities or how these conditionalities might be weighed against other aid offer features. After all, there are multiple channels through which these conditionalities could impact outcomes that governments are sensitive to; they might affect government support through public opinion, the incidence of violence by domestic opposition groups, or the strength of their relations with other states. And conditionalities impose constraints on recipient decision-making autonomy that could render a conditionality-laden aid offer less attractive independently of these other channels.

Three interconnected questions follow. What is the effective "price" of purchasing terrorism policy concessions with foreign aid? That is, from the perspective of the aid recipient, how much less desirable is an aid offer with conditions relating to terrorism policies relative to one that is unconditional but otherwise identical? Second, does this price vary depending on whether conditions require substantive changes to domestic legislation or merely changes to the recipient's rhetoric on terrorism? And, finally, does the price of purchasing these concessions vary systematically across recipient attributes?

To answer these questions, I conducted a preregistered conjoint survey experiment from September 2024 to March 2025 with 411 foreign policy elites and experts. Broadly, these respondents were identified and contacted according to a sampling strategy which sought to draw from a varied population of intra- and extra-governmental elites that are positioned to advise decision-makers on matters of foreign policy, especially those relating to foreign aid, development, and security. The conjoint profiles respondents were shown were designed to simulate the policies that might be available to the parties to aid policy bargaining processes. In practice, aid policy bargaining takes the form of negotiations between donor agencies and recipient governments in order to arrive at an "aid policy compromise", or a negotiated agreement "in which the donor agency agrees to provide a certain amount of aid in exchange for specific actions by the recipient government" (Swedlund, 2017, p. 3). To elicit the logics that operate during aid policy bargaining, respondents are asked to choose between pairs of foreign aid offers to recommend in an advisory capacity to a hypothetical advisee government, with aid features randomized

across offers. I derive each the average marginal causal effect of each feature's levels, effectively showing how adding or removing features to the aid offer being considered by bargaining parties affects its acceptability to the recipient country, relative to a baseline.

The results show that political conditionality regarding terrorism imposes a negative and substantively significant penalty on aid offer desirability, though this penalty is comparable to those incurred by other major features such as domestic public opinion, aid value, risk of domestic violence, allied attitudes, and donor-recipient relationships. This is consistent with a world in which terrorism policy conditionalities might readily be exchanged for foreign aid. Importantly, the penalty for demanding rhetorical changes is statistically indistinguishable from conditions requiring domestic legislative changes, challenging naive readings based on common IR assumptions about "cheap talk" and suggesting we must consider the broader reputational and political costs recipients face when making such policy concessions. However, some exploratory, non-preregistered analysis suggests a caveat: disaggregating the sample by expert background in a non-preregistered analysis suggests a heterogeneous effect of rhetoric conditions by recipient location, i.e., an aid donor country or an aid recipient country. This finding of potentially heterogeneous treatment effects points toward greater resistance to arrangements that may appear neo-colonial to advisers in the Global South, particularly where rhetorical concessions on terrorism could compromise solidarity with marginalized groups or liberation movements.

I proceed by, first, defining and operationalising political conditionality. I then review prior cases and theory on donors employing political conditionalities in the security and conflict domain, with attention to terrorism cooperation and the distinction between policy- versus rhetoric-targeting conditionalities. Next, I describe the survey's design, sampling strategy, and estimation approach; present the findings and key robustness checks; and conclude with implications, limitations, and avenues for future research.

Literature Review

Foreign Aid, Political Conditionality, and Influence

In Olav Stokke's influential formulation, political conditionality refers to cases where "foreign aid is used as a lever to promote objectives set by the donor which the recipient government would not otherwise have agreed to" (Stokke, 1995, p. 12). The emblematic, key mechanism around which political conditionality is typically defined is *denial*: "the use of pressure by the donor in terms of threatening to terminate aid, or actually terminating or reducing it, if conditions are not met by the recipient" (ibid.).

This denial-based definition is analytically useful and provides a minimal operationalisation of political conditionality. Some scholars of political conditionality have cautioned

against attempts to broaden the concept beyond this essential core (Killick, 1998, p. 11) given the risks of conceptual stretching. But failing to do so omits substantively important cases that nonetheless accord with Stokke's more abstract conception of leveraging foreign aid to motivate recipient policy change. While cognizant of the risk of conceptual stretching, this paper adopts a wider definition of political conditionality. The intension of, and rationale for, this wider definition are detailed below for clarity and methodological transparency. In brief, the wider definition encompasses empirical cases and mechanisms that operate in a significant portion of instances where aid is used as leverage over recipient policy. Moreover, evolving practices in aid delivery mean that the narrower definition's emphasis on denial becomes less representative over time, as donors have increasingly and explicitly eschewed outright aid suspensions and withdrawals in favour of more subtle mechanisms. Political conditionality is broadened here in three primary dimensions, consistent with prior research, that are handled in turn: its modality, temporality, and transparency.

First, political conditionality is traditionally conceived as effecting policy change by imposing costs, i.e., a punitive modality. Alternatively, political conditionality's characteristic leverage over recipient policies might be gained through the promise of future rewards, i.e., an incentivisory modality. These, respectively, are known as "negative" and "positive" conditionalities (Stokke, 1995). In practice, positive conditionality might involve increasing aid allocations or deepening engagement when recipients demonstrate progress toward intended outcomes, or using graduated rewards to encourage further cooperation when aid relationships are tied to broader political realignment between donor and recipient.

Second, the minimal, denial-based definition of political conditionality assumes that the aid relationship is extant from each case's instantiation. Conditionality is enforced via threatened or actual termination of this relationship and/or the reduction of the amount of aid being given. This temporal order of events might not reflect the features of all relevant cases. For instance, it excludes cases where political conditionality is instrumentally enforced by establishing prerequisites prospective recipient states must meet before any aid relationship is formed. Accordingly, the definition is broadened to encompass *ex ante* conditionality, whose conditions must be fulfilled before states enter an aid relationship and *ex post* conditionality, wherein donor conditions and/or expectations are stipulated and relayed to the recipient in advance of entering the aid relationship and their fulfillment is assessed and acted upon at a later date (ibid., p. 12).

Third, political conditionality might vary in its formality, or the extent to which it makes foreign aid explicitly contingent on recipient policy decisions. Aid conditionalities might be overt, formal, publicly stated, and explicit. Threats to revoke aid absent specific some policy change belong to this type. Equally, pressure and leverage might

be imposed through more subtle means without sacrificing diplomatic heft. A donor might communicate its expectations for the aid relationship throughout the course of aid policy bargaining, informal channels and hallway chats, or signalling via public statements without mention of any hard ultimatums. If the recipient state can reasonably infer that the donor's aid allocation decisions are at least partially contingent on the recipient's performance in some policy domain against donor expectations, then pressure conceivably exists to satisfy these expectations.

While individual donors might be predisposed to either “hard” or “soft” approaches to political conditionality, these are not mutually exclusive strategies and can be employed in tandem. For Jonathan Goodhand, it is “unhelpful” to restrict one's attention to either “hard, ultimatum conditionalities” or softer “post conditionality” approaches to donor influence; donors have pursued hybrid strategies whereby conditionality is not predicated on a “simple yes/no decision on whether to give aid or not” and might be exercised “through social, political and military as well as economic channels” (Goodhand, 2006, p. 28; see also Goodhand and Sedra, 2007).

Political conditionality, then, encompasses both ex ante and ex post mechanisms, denial and incentive structures, and “hard” and “soft” modes of imposition.

	Ex-ante	Ex-post
Positive	Conditions need to be fulfilled as a <i>prerequisite</i> to the granting of <i>benefits</i>	Granting of <i>additional benefits</i> conditioned on performance <i>during the course of a relationship</i>
Negative	<i>Reducing or suspending</i> benefits <i>before</i> entering into beneficial cooperation or agreements	<i>Reducing, suspending or terminating</i> benefits <i>during the course of a relationship</i>

Figure 1: Typology of political conditionality. Source: Koch, S. (2015). “A Typology of Political Conditionality Beyond Aid: Conceptual Horizons Based on Lessons from the European Union.” *World Development*, 75, 97-108. <https://doi.org/10.1016/j.worlddev.2015.01.006>. Licensed under CC BY-NC-ND 4.0.

The importance of broadening the conception of political conditionality in the way I have described is made more pronounced by empirical trends in international development programming. Donors have increasingly disavowed strict policy conditionalities and overtly politicised aid, leading to the pronouncement of an age of “post-conditionality” (Frerks, 2006; Goodhand and Sedra, 2007). However, this does not necessarily signify a fundamental change to the strategic logic of foreign aid provision or political conditionality. Aid donors may simultaneously 1) hope to avoid appearances of being materially coercive and 2) seek to influence the policies of countries to which they direct foreign aid. The wider adoption of positive and/or non-formalised political conditionalities in foreign aid relationships which we observe in “post-conditionality” policy trends is consistent with

these two objectives (Frerks, 2006). The wider definition encompasses these cases while the narrower definition does not. It is important to adopt the wider definition then to not artificially omit cases that nonetheless share a strategic logic with their denial-based counterparts and, presumably, facilitate similar degrees of leverage over recipient policy choices.

Having laid this conceptual groundwork, I now engage more closely with the extant literature on security- and terrorism-related conditionalities to generate theoretical expectations regarding this particular form of political conditionality. Drawing on prior cases and theory, I integrate a distinction between conditions targeting substantive policy versus political rhetoric (H1). I then consider other thematic features of aid offers that I expect to correspond to variation in their acceptability: aid volume (H2), public opinion (H3), domestic security risks (H4), donor-recipient relations (H5), and the perceptions of allied or aligned states (H6).

Political Conditionality and Terrorism Policy: Policy vs. Rhetoric

Recent scholarship pairing formal theory with quantitative empirics has argued that foreign aid is used by donors to reward states which ratify international counterterrorism treaties (Pascoe, 2025). This effect appears to be self-reinforcing: ratification of counterterrorism treaties imposes benchmarks on signatories that can trigger aid withdrawals or suspensions by donors if unmet (*ibid.*). This evidence for systematic aid-for-policy dynamics is significant for two reasons. First, it bolsters claims about aid-for-policy deals that have previously relied primarily on descriptive accounts of individual cases or small-N studies, suggesting that such deals may not be merely idiosyncratic but rather representative of a broader pattern in international aid and policy bargaining. Second, it provides empirical support for formal theoretical and game-theoretic analyses (for instance De Mesquita and Smith, 2007; De Mesquita and Smith, 2009a) that predict such dynamics could readily emerge among states without requiring significant departures from standard assumptions about state behavior, but which are principally deductive exercises with limited large-scale empirical validation.

Some supporting evidence emerges at the mechanistic level as well. Quantitative results from an elite conjoint survey experiment indicate that political conditionalities theorized to drive policy changes are far more acceptable to recipients than prior literature on conditionalities would predict (Blair, Custer, and Roessler, 2024). While this work does not address terrorism policy specifically, the finding that elites were “at worst indifferent to social [...] and good governance conditionalities” suggests a broader attitudinal disposition toward conditionalities that may facilitate aid-for-policy exchanges regarding terrorism.

While these examples are notable, the body of research establishing evidence for aid-for-policy exchanges remains relatively underdeveloped. One possible reason for this is that donors and recipients often arrange aid-for-policy deals privately (Bolton, cited in Vreeland and Dreher, 2014, p. 54). While agreed conditions are sometimes documented, they frequently rest on verbal or informal understandings between officials (Swedlund, 2017, p. 23). Given that the existence and terms of bargains are often opaque to external observers, subjecting them to systematic empirical study is challenging. These constraints limit the availability of definitive “smoking gun” evidence for or against aid-for-policy dynamics with respect to terrorism, and motivate focusing on observable offer features in the experimental design used herein.

Against this backdrop, it is important to consider what, exactly, might be demanded of a recipient and how the particular strategic and political implications of such a demand informs theoretical expectations of state behaviour. Broadly, conditionalities in counterterrorism cooperation can target either substantive, technical policies (e.g., domestic legislation, treaty commitments) or broader political dispositions towards the issue which manifest through a state’s political rhetoric, including how states frame problems, propose solutions, and allocate political attention.

International Relations theory suggests that this distinction is significant. The former are generally costly to implement. Treaties impose new binding obligations. Implementing changes to domestic legislation generally requires gaining the consent of important stakeholders, e.g., the public or political elites. Implementation may fundamentally change relations with violent actors and other states. For instance, while decisions to proscribe armed groups as terrorists appear to be primarily driven by dynamics of policy diffusion between states, not perceptions of a group’s concrete threat (El Masri and Phillips, 2021), proscribing a group can render negotiations more fraught and resolution of civil conflict less likely (Haspeslagh, 2021). Concrete, binding policy changes have clear costs that one must expect to feature heavily in the strategic logic of accepting or refusing aid offers with strings attached.

In contrast, the latter category of conditionality would be more readily recognised by International Relations scholars as relating to “cheap talk”. These are signals which are costless to send; this is opposed to costly signals, like mobilising military forces or investing in arms production, which are informative in crisis bargaining precisely because only a state with sufficient resolve would willingly suffer them. In conventional cases of crisis bargaining and strategic interaction, this costlessness renders cheap talk an incredible signal of the state’s resolve and capabilities. All states are incentivized to project strength and resolve to strengthen their bargaining position, which renders listeners unable to discriminate between states that communicate truthfully and those that do not. Ultimately,

this means cheap talk does not fundamentally alter bargaining dynamics, actor utilities, and expected outcomes.

Although there is some identifiable tendency in the literature to extrapolate from the aforementioned scenario and treat *all* instances of cheap talk as epiphenomenal and unimportant, a strict game-theoretic rendering of cheap talk requires that determinations of its effects or noneffects on a given bargaining process be made contingent upon the particular rules and incentives structuring an interaction (Spaniel, 2023, p. 192). That is, whether cheap talk “matters” for a given interstate interaction or entails consequential costs is dependent on the rules, incentives, and dynamics structuring that very interaction. Nonetheless, cases in which cheap talk signals can be said to alter bargaining dynamics in the absence of its effects on other channels, such as public opinion or reputational arguments, are marginal/rare edge cases in international politics.

A reasonable default position, then, would be to assume that conditionalities targeting the rhetorical positions of aid recipients would be viewed as less costly concessions than “harder” substantive demands, absent strong theoretical arguments or empirical evidence for a bargaining situation being particularly sensitive to cheap talk’s costless signalling.

H1a: Elite respondents prefer unconditional aid offers to otherwise comparable offers that require policy or rhetorical concessions.

H1b: When conditions are present, elite respondents prefer rhetoric-only conditions to requirements for policy change.

Having distinguished conditions on policy from conditions on rhetoric and stated expectations, I now turn to brief empirical vignettes. The aim is not a comprehensive test, but to anchor the hypotheses in concrete cases and to demonstrate how these conditionalities might operate in practice. Cases where aid has been conditioned on concrete counterterrorism policy measures and/or legislative changes have perhaps been the most visible. The United States has been shown using aid with aspects of positive and negative conditionality to spur support for, and active participation in, its “Coalition of the Willing” in the Iraq War and its broader counterterrorism framing (Newnham, 2008). For example, Pacific Island states reportedly feared aid suspensions if they did not meet US foreign policy expectations; after acceding to the US-led coalition, they were rewarded with generous new aid packages (ibid.). France’s anti-war position reportedly motivated it to counter US aid influence attempts over its former colonies Guinea and Cameroon by offering its own positive conditionalities, specifically the promise and ultimate delivery of increased aid to these countries (ibid.).

Even states that publicly disavow political conditionality use aid to advance their counterterrorism agendas. Consider that China's post-2021 engagement with the Taliban appears to have tied generous assistance to curbing support for Uyghur militants affiliated with the East Turkistan Islamic Movement (ETIM), a successionist movements pursuing an independent state in Central Asia and the Chinese province of Xinjiang, including relocating ETIM-linked fighters away from the Xinjiang border into central Afghanistan (Chawla, 2022; Murtazashvili, 2022). Related concessions were made by Tajikistan, which transferred full ownership of a Chinese-operated base in the Pamir Mountains and surrounding territory to China, a region of strategic importance for China's suppression of local dissent and the risk of political violence. Moreover, Tajikistan waived all future rent payments on the base in exchange for an undisclosed amount of debt relief and military assistance (Southerland, Green, and Janik, 2020; Standish, 2021, p. 15). These cases conceivably evidence a linkage between China's aid ties in the region and recipients' counterterrorism policies, despite China's official denials of conditionality. Gulf states, which have been said to eschew conditionalities because of Islamic precepts against conditions being placed on charity (see K. E. Young, 2017, which identifies but critiques this argument), are said to be increasingly turning away from unconditional aid (Fenton-Harvey, 2023) and that this aid has increasingly served geopolitical ends (Yaghi, 2024), including countering regional terrorism (Dandashly, 2025).

Like measures which condition (explicitly or not) aid on changes to a recipient's substantive terrorism policy commitments, political conditionalities have also been observed with respect to the political positions states rhetorically espouse on terrorism. Following the October 7th, 2023 terrorist attack by Hamas on Israeli communities bordering Gaza, the European Commission announced a review of their aid to the Palestinian authorities and NGOs along with an intention to include anti-incitement contractual clauses and monitoring mechanisms. These clauses would ensure the termination of aid to recipients that are found to incite "hatred and violence" (European Commission, 2023).

In its own justification for its own suspension of aid to various Palestinian NGOs that were accused by the Jerusalem-based organisation, NGO Monitor, of rhetorically supporting the Hamas attack, leader of the governing far-right party Sweden Democrats, Jimmie Åkesson, was quoted as saying "it is unreasonable that Swedish tax money risks going to things that may not directly carry out terror, but things that legitimize terror" (Åkesson, quoted in SVT, 2023, machine translation). Based on this statement and those of other party officials, Amnesty International concluded that Sweden's expressed policy would "require Palestinian partners to condemn Hamas" to receive aid in the future (Amnesty International, 2023).

Aid Amount

Barring edge cases, the utility of foreign aid is proportionally to its financial value. Aid offers that promise larger sums of money and/or services and goods of greater value should be more attractive to prospective recipients, all else being equal. This is not a novel or surprising theoretical expectation. Nonetheless, it is formalised here as a hypothesis for completeness. The financial value of aid offers is varied in the survey design to serve two main purposes: to serve as a rationality check (that participants prefer evidently dominant options to inferior ones) and as an effect size benchmark against which other parameters can be compared.

H2: Elite respondents prefer higher-value aid offers to moderate-value offers.

Public Opinion

Previous works have asserted that the relationship between public opinion and “aid with counterterrorism/security strings attached” is “complicated” (Boutton, 2014, p. 69). Public opinion may determine whether a government can accept a conditionality-laden aid offer, or whether it sees doing so as being in its interest (Dasandi, 2022). Importantly, however, public opinion is not fixed or insulated from external influence. Theory holds that recipient state elites might seek to transform public opinion to align with their preferences in relation to an aid offer, from opposition to support, via cueing or framing mechanisms (Boutton, 2014). As to whether the donor states might similarly be able to influence international public opinion to make its desired conditionalities more acceptable, empirical research suggest that US economic aid had essentially no effect on recipient publics’ opinion towards US foreign policy, but that the public’s exposure to the US “message”, or the framing of its foreign policy choices, in domestic media coverage was associated with more positive attitudes towards US foreign policy (Goldsmith, Horiuchi, and Inoguchi, 2005). If one accepts that exposure to this message could be conditional on aid flows or the demands made therein, the complexity of specifying these relations increases further. Together, these arguments suggest that relationships theorised between foreign aid, aid conditionalities, and public opinion may well be characterized by reciprocal or co-constitutive causation rather than unidirectional influence, marked by endogenous feedback loops and high contingency.

The literature identifies a number of channels through which foreign aid might directly influence public opinion in recipient countries, though the mechanisms and conditions under which this occurs remain contested. Dietrich and Winters (2018) find that, while

donor efforts to publicise their aid involvement are not always successful, aid beneficiaries viewed both the donor and their own government more positively when they were made aware of the source of aid. This suggests that both donor and recipient governments may benefit from public opinion improvements when citizens are made aware that their government secured foreign aid and that the donor provided it. However, the same study found no effect of this awareness on foreign policy preferences vis-à-vis the donor, which may be particularly relevant given this paper's focus on terrorism policy conditionalities.

Furthermore, recent experimental evidence indicates that the perceived motivations behind aid provision significantly moderate its public opinion effects. Rhee et al. (2024) demonstrate that while public diplomacy efforts via foreign aid generally improve perceptions of the donor, experimental treatments suggesting that the donor had political or strategic motivations for the aid cancel out positive effects on attitudes towards the donor. Given that political conditionalities regarding terrorism policy inherently signal strategic motivations and come with explicit strings attached, it remains unclear how recipient governments might weigh the potential public opinion benefits of securing aid against the risks that their publics become suspicious about the aid's provenance and purpose. This tension may be particularly acute in the context of counterterrorism aid, where public opinion regarding security cooperation with foreign powers is often contentious.

These findings suggest a complex dynamic whereby the very features that make terrorism-related political conditionalities potentially effective may also simultaneously undermine their public acceptability in recipient countries. This creates a dilemma for recipient governments: while accepting aid with terrorism policy conditionalities might provide material benefits and consolidate state power relative to potential opponents, it also risks generating public backlash if citizens perceive the aid as compromising national sovereignty or aligning the government with unpopular foreign policy positions. On the other hand, foreign aid might function as a "free" or "unearned" resource that encourages leaders to concentrate power or autocratise in the face of mass opposition, ultimately insulating themselves from any backlash effects that the receipt of aid incurred (De Mesquita and Smith, 2009b; Ahmed, 2012).

Beth Whitaker's detailed case study analysis of the compliance of three East African states (Uganda, Tanzania, and Kenya) with the prevailing international counterterrorism regime provides a useful initial empirical referent. Whitaker finds that while compliance with external political conditions is uncertain and differs across states, "the regime caused countries to implement certain counter-terrorism provisions" and that absent US funding and political pressure "it is unlikely that any of the countries would have [implemented these counter-terrorism provisions]" (Whitaker, 2010, p. 661). This description of the cases is consistent with political conditionality given that recipients conceded "objectives set by the donor which the recipient government would not otherwise have agreed to" (Stokke,

1995, p. 12) in the absence of aid. The Kenyan case, in particular, illustrates how the intended outcomes of political conditionality might be spoiled by domestic stakeholders and hostile public opinion, supporting contentions that political conditionality operates as a two-level game (Dasandi, 2022).

Whitaker characterizes Kenya as a “reluctant partner” in the US-led counterterrorism regime, pointing to the failure to pass key legislation and sustained domestic public opposition as evidence of incomplete compliance with US demands (Whitaker, 2008). However, Kenya’s then-government did appear to cater to US pressures, even if they could not see these fully delivered on account of wider societal opposition to the measures sought. Following US diplomats urging for more aggressive domestic counterterrorism measures, the governing Kibaki administration presented the Suppression of Terrorism Bill to the Kenyan Parliament (Prestholt, 2011) without any prior public consultation (Bachmann and Honke, 2010, p. 107) in an apparent concessional gesture to the United States. The US Ambassador to Kenya urged the Kenyan Parliament to pass the bill quickly, and the Bush administration announced its intention to disburse \$100 million USD in aid to six East African countries soon after the bill was presented to Parliament (Prestholt, 2011). As noted in Swedlund (2017), the period between political aid pledges, like this one, and actual aid delivery is characterised by intensive aid bargaining, where details are carefully negotiated and when all parties might seek to exercise any influence they hold.

Due to vigorous public opposition to the bill in the Kenyan Parliament by parliamentarians, Muslim civil society actors, and various public stakeholders, the bill ultimately failed to pass. Nonetheless, the US disbursed large volumes of aid to Kenya immediately thereafter and explicitly lauded the government’s “efforts to pass counterterrorism legislation” despite the bill’s ultimate failure (Wycoff, 2004). Simultaneously, US officials praised related measures which had been delivered more successfully by the Kenyan government, such as anti-corruption legislation and the creation of new antiterrorism bodies (ibid.). Given the dynamics observed here, it’s reasonable to conclude that hostile public opinion makes it more challenging for a government to deliver policies promised in order to satisfy aid conditionalities. But favourable public opinion is not a necessary prior condition for recipient governments to make these promises, formally accede to these aid offer terms, or to seek their implementation domestically through the means that are available to them. Nonetheless, government dependence on external support for their political survival, whether from broad or small domestic constituencies, suggests that this aid offers met public hostility or dislike will necessarily be less attractive relative to offers that register more neutral or positive responses in public opinion.

H3a: Elite respondents prefer aid offers viewed positively by the public to offers viewed neutrally or negatively.

H3b: Elite respondents prefer aid offers viewed neutrally by the public to those viewed negatively.

Domestic Security

Aid can increase the risk of domestic political violence in recipient states (Danzell, Kisangani, and Pickering, 2019; M. Findley et al., 2023; reverse relationship identified in J. K. Young and M. G. Findley, 2011). Armed groups may target aid infrastructure, such as schools, hospitals, and other development projects, to undermine donor efforts and demonstrate their power and control over the state. Or, aid may represent a desirable resource for these groups and motivate its seizure or diversion. If successful, seized resources may materially empower these groups and increase their capacity to conduct attacks. Or, public opposition to accepting funds from unpopular foreign donors may also motivate targeted violence in protest of an aid deal, or encourage rebel activity that seeks to provoke government overreach and ultimately trigger withdrawals of donor support (a possibility considered in Nielsen et al., 2011, p. 230; and conceived with reference to Kuperman, 2008).

Empirical examples provide precedents for these claims. In the post-war reconstruction of Afghanistan prior to the 2021 Taliban takeover, the Taliban targeted “physical and human symbols of development” and various aid-funded projects as part of a clear strategy to counter “government and donor promises of a peace dividend” (Goodhand and Sedra, 2010, p. 594) and bolster its own relative legitimacy and agenda. Neil Narang and Jessica Stanton (2017) specifically argue that the Taliban targeted humanitarian aid workers in regions where the government enjoyed stronger civilian support, and thus where the Taliban stood to lose the most from more favourable perceptions of the government that might result from their perceived delivery of public goods and services through aid organisation proxies. Similar dynamics have been noted in the case of Sri Lanka and its protracted struggle against the Tamil Tigers, or the Liberation Tigers of Tamil Eelam (LTTE), from the 1980s to the 2000s. The LTTE is said to have coopted aid projects in their controlled territories, using their perceived ownership of these projects to bolster their image among local civilians (Flanigan, 2008). Moreover, they were accused of and threatening, and even attacking, NGOs personnel or aid workers that they thought might subvert their control of these projects (Human Rights Watch, 2006). To the extent that aid provision is a source of legitimacy and material capabilities for the recipient state, it is also a source of vulnerability to the recipient state’s enemies and, by upsetting the balance of power in this regard, can alter the (dis)incentives for political violence.

H4: Elite respondents prefer aid offers associated with a low risk of domestic political violence over those associated with a high risk.

Taken together, these possibilities suggest that prospective recipients of aid will consciously weigh the risks of domestic political violence against the benefits of receiving aid. Given this study's emphasis on aid with terrorism policy conditionalities, the link between aid provision and the subsequent risk of domestic political violence is particularly pronounced. Of course, this relationship might be attenuated by countervailing pressures. While the influx of aid can provoke violent reactions or competition for its resources, aid might increase a state's material capabilities relative to its opponents and reduce the risk of conflict (see De Mesquita and Smith, 2009b, on aid increasing state repression) or enable pacification efforts and reduce conflict risk via patronage (Azam, 1995).

Similarly, the provision of terrorism-adjacent aid might create a moral hazard problem: counterintuitively, recipient states may actually desire that some level of domestic violence results from or persists after receiving assistance, as this validates their continued need for aid. This is the central argument advanced by Andrew Boutton (2019, p. 361), who argues that leaders of personalist, non-democratic regimes, in particular, view the maintenance of a tolerable but sustained level of domestic terrorism as “as source of leverage which can ensure continued assistance” that is “critical to their survival”.

Ijaz's (2025) subnational regression discontinuity design analysis finds further support for this dynamic while specifying the precise mechanism; in Pakistan the resumption of US military aid in response to Pakistan's participation in its War on Terror—which had been suspended in 1990 upon Pakistan's reveal of its then-nascent nuclear programme—spurred an overall short-run decrease in political violence accompanied by its spatial displacement to, and greater localised incidence in, areas away from government territorial control. Ijaz interprets this as a consequence of strategic recipient behaviour: increasing state repression to satisfy donor demands while displacing, yet sustaining, political violence elsewhere to maintain the justification for continued foreign aid (*ibid.*). Accordingly, theoretical expectations may need to be revised when considering aid offers to these “terrorism racketeers” (see Boutton, 2019; Berger, 2024), where one might expect that the anticipation of an elevated risk of domestic violence resulting from the acceptance of an aid offer to have null or even positive effects on its probability of acceptance.

Donor–Recipient Relations: Alliances, Rivalries, and Alignment

Political alliances are major determinants of foreign aid allocation (Alesina and Dollar, 2000). Donors tend to give aid to countries with which they are allied and/or share significant strategic, geopolitical interests. But aid can also be directed towards states that are antipathetic to the donor while remaining entirely consistent with logics that prioritise

donor interests. Bermeo's (2017) theoretical framework of "targeted development"—an account that explains donor aid allocation in the post-2001, post Cold War period by supposing that "donors give aid where they most wish to benefit themselves by decreasing underdevelopment"—suggests that aid serves donors by spurring genuine development and, consequently, decreasing the negative spillovers that emanate from underdeveloped states. Within this framework, it's clear that an aid donor might see it in their interest to invest in hostile or unfriendly states precisely because inaction sustains the economic conditions that give rise to negative spillovers: civil conflict, refugee and asylum flows, domestic and transnational terrorism, etc. And unfriendly relations may exacerbate these risks further, i.e., by motivating aggrieved actors to target the hostile state with political violence.

With the logic of extending aid to unfriendly states established, let us assume that a donor has already extended an aid offer to a state with which it has poor relations. With this offer in hand, how is the recipient government likely to evaluate its terms? And how does its relations with the donor affect its desirability?

For one, it's reasonable to assume that the latent risks of political influence engendered by any and all aid offers are magnified in such circumstances. Without the goodwill that would be sustained in a friendlier dyad, and with the higher expected incidence of geopolitical disagreements that would be expected between adversaries or competitors, a recipient state policymaker might rationally expect this aid offer to be more readily subject to donor manipulation and influence. That is, the donor state might more frequently be put in positions where the withdrawal or suspension of aid to effect policy changes in the recipient, or instrumental political conditionality, is attractive. The donor's ability to credibly commit to maintaining this aid is consequently reduced.

The recipient, recognising this, might hesitate before entering into the aid relationship despite its material benefits to avoid this vulnerability. This is especially true if there exists an asymmetry between the benefits accrued from foreign aid rents and the costs suffered in cases of foreign aid suspension or negative shocks to aggregate aid revenue. Hudson and Mosley (2008) show that this is indeed the case: aid's impact is reduced in both cases of upside and downside volatility in aid, but these negative impact is only partially reversible for positive shocks, and is not reversible for negative shocks. Critically, these researchers also demonstrate that the aid volatility is dependent on the level of trust between the donor and recipient states, supporting the contention above about the inability of a donor state to credibly commit to maintaining aid in adversarial or unfriendly dyads (*ibid.*).

Signalling effects must also be taken into account. In their experimental study on the relationship between foreign aid acceptance/rejection and its relationship to a state's

international status, Allison Carnegie and Lindsay R. Dolan (2021) show that states facing a dire emergency, such as a natural disaster or economic shock, will take the extraordinary step of rejecting much-needed offers of foreign aid in order to accrue international status and prestige. By rejecting aid offers that only truly competent, self-reliant states would be expected to decline, these states send a costly signal that misrepresents their actual level of need and governing competence to international counterparts. On the donor-side of this interaction, Cindy Cheng and Shahryar Minhas (2021) show that donors increase their humanitarian and civil society aid to strategic opponents following natural disasters in the recipient. The authors reasonably interpret this as donors seeking to use the context of a natural disaster as an opportunity to develop better relations with strategic opponents. Here, it is also worth considering that foreign aid “transforms material dominance and subordination into gestures of generosity and gratitude” (Hattori, 2001, p. 633); providing aid to a strategic opponent beset by disaster is also a way of performing benevolence, material and moral superiority over them and transforming others’ perceptions of the dyad. Given, first, the status boon presented by the costly signal of aid offer rejection and, secondly, the domestic and international costs of subordinating one’s state to an adversarial donor state, it follows that an aid offer becomes less desirable as the hostility or contentiousness in a dyad increases.

H5a: Elite respondents prefer aid offers where donor-recipient relations are positive over offers where relations are neutral or strained.

H5b: Elite respondents prefer aid offers where donor-recipient relations are neutral over those where relations are strained.

Notably, considerations of donor-recipient relations might extend beyond the immediate dyad in question. There is evidence that other major donors adopted policies that mirrored and/or complemented US policy objectives in their own foreign aid practices. For instance, empirical research on Japan’s Official Development Assistance (ODA) suggests that it was actively aligned with US counterterrorism policy priorities during the US Global War on Terror (Tuman, Strand, and Shirali, 2017). This includes assertions that Japan likely used, or at least was expected by the United States to use, ex-post negative political conditionality by denial in their aid relations to that end, i.e., by reducing or terminating ODA to countries that were believed to sponsor terrorism or that otherwise had terrorism policies that ran counter to the United States’ and Japan’s respective interests (Tuman, Strand, and Emmert, 2009; Tuman, Strand, and Shirali, 2017). Recipients are likely to assess their relation to a prospective donor alongside that donor’s position in a wider strategic network.

Extending this logic, the act of accepting aid is also visible to, and evaluated by, the recipient's own allies and strategic partners. Assistance from a donor that sits at odds with the recipient's alliance network can either reassure or unsettle those partners, with repercussions for diplomatic support and access to club goods. Debates around Chinese Belt and Road Initiative financing underscore this dynamic: deeper aid and investment ties between China and US-aligned states in Europe, Asia, South America, and the Indo-Pacific have been interpreted by the US as steps that could compromise their strategic network and induce policy drift among BRI member states toward a US rival (Li, 2020). Consequently, prospective recipients must weigh not only the bilateral material gains of an offer but also whether acceptance will be welcomed or punished by their own coalition of allies.

H6: Elite respondents prefer aid offers where their allies are supportive of its acceptance rather than critical.

Research Design

Design and Implementation

I field a preregistered online forced-choice conjoint survey experiment to estimate elite preferences over features of foreign aid offers. In each of six choice tasks, respondents assume the role of an advisor to an unspecified government. They view two independently randomized aid packages (i.e., a maximum of 12 profiles total per respondent) and indicate which package they would recommend accepting. Profiles vary across six attributes. Aid conditionality takes three values: unconditional aid; a requirement to add provisions to domestic antiterrorism *legislation*; or a requirement to express *rhetorical* support for the donor's stances on terrorism at the United Nations. Aid amount is moderate or substantial. Public opinion toward the offer is positive, neutral, or negative. Domestic security risk is described as no risk versus some risk of political violence in response to the deal. Bilateral relations with the donor are positive, neutral, or strained. Allied states are generally supportive or critical of the deal. For estimation, I treat the following as baselines: unconditional aid, a moderate amount, positive public opinion, no risk of political violence, positive donor relations, and supportive allies.

Consistent with guidance in (Kertzer and Renshon, 2022), I make explicit the elite population, the dimensions of eliteness, the fit between theory and empirics, and the composition of a bundled sample. The relevant political elites for this study are foreign policy professionals who professionally engage with, advise on, or implement foreign aid and security policy, especially development agency officials and national security

bureaucrats and senior decision-makers who participate in or shape aid bargaining. I conceptualize eliteness along two intertwined dimensions: (i) *occupational eliteness*, or whether one’s professional role confers decision-relevant authority or advisory responsibility in aid/development/foreign policy; and (ii) *cognitive eliteness*, or whether one possesses domain-specific expertise in foreign policy and multilateral diplomacy, international development and the politics of aid, and/or international security and political violence.

The theory centers on accept/reject choices over aid offers made by principals under political and security constraints. The collected sample consists of a geographically dispersed set of government officials, diplomats (junior foreign service officers, senior diplomats, and ambassadors), researchers from foreign policy think tanks, international organization staffers, and development agency officials. This is a close, though not perfect, match to the primary agents implied by theory: respondents are not necessarily actively or previously engaged in aid policy bargaining, and at minimum only have the requisite skills and domain-specific expertise that would be expected to be possessed by those who are. I argue that this is a reasonable approximation given that these shared features comprising eliteness, and not one’s actual experience of or engagement in aid policy bargaining scenarios that perfectly mirror those considered here, are assumed to be the primary determinants of preferences over aid offers. This assumption, of course, is subject to falsification and verification, and the results of this study should be interpreted with this in mind.

To operationalize the target population in practice, I employed complementary recruitment modes with distinct trade-offs. First, I systematically compiled comprehensive contact lists from the top 35 global foreign policy think tanks (from “Top Foreign Policy and International Affairs Think Tanks” in McGann, 2021), attempting to exhaustively catalog all relevant personnel and their email addresses from each organization’s public directories, and sent individualized email invitations; these organizations advise or lobby governments and frequently employ individuals with prior and current government or IO/NGO service, and they maintain public, structured contact directories that enable broad but precise outreach. Second, when an organization contained many likely members of the target population and contact formats were recoverable, I compiled senior professional addresses organization-wide—for example, USAID, the UK FCDO, the Finnish Ministry of Foreign Affairs, and selected UN sub-agencies—and invited them to participate via email. Third, to improve coverage beyond publicly listable contacts at predominantly Western think tanks, I used LinkedIn Sponsored Messaging targeted at senior foreign policy and governmental roles, with campaign parameters vetted by a LinkedIn digital advertising consultant, following emerging practice in recruiting IR elites for surveys (Clark, 2021; Casler, 2025; Heinzl, Reinsberg, and Siauwijaya, 2025). Fourth, I placed an advertisement in the International Intrigue international affairs newsletter to reach

foreign policy professionals without public contact details. Fifth, in a small number of cases where eligibility was known *ex ante*, I used personal-network outreach. Methods three and four carry greater risk of self-misrepresentation than direct email; to enhance transparency and support robustness checks, the survey link appended source-specific URL parameters (e.g., `?source=email, linkedin, newsletter, network`) recorded at intake. Participants were not offered compensation or incentives, consistent with (Heinzel, Weaver, and Briggs, 2025), who find among World Bank elites that incentives do not improve response and monetary incentives reduce response rates by roughly one-third.

Our conservative power analysis (see Lukac and Stefanelli, 2020; Schuessler and Freitag, 2020) assumed a minimum effect size of 0.05, three levels per attribute, and 12 tasks per respondent. The analysis indicated that ~ 4698 effective observations, or 391 respondents with full survey completion, would be sufficient to detect all conjoint treatment effects with 80% power. Upon reaching 95% of our target sample, interim analysis revealed all conjoint treatment effects were statistically significant (all $p < 0.01$), with point estimates ranging in absolute value from 0.05 to 0.25. Given the expense and difficulty of recruitment, and the clear achievement of adequate power across all estimated parameters, active data collection was terminated. In combination, recruitment between September 2024 and March 2025 yielded a heterogeneous but well-covered bundled sample of 411 respondents aligned with the elite definition above, with complementarities that offset the shortcomings of any single approach. If respondents had rated at least one conjoint profile but exited the survey early, their responses were saved, and otherwise discarded. Approximately 14% of respondents rated fewer than the maximum 12 aid profiles, yielding 11.0 profiles per respondent on average. The total sample consists of 4516 effective observations from 411 actual respondents and 376.3 effective respondents.

Attributes are assigned with equal probability and independently for each profile on the server. The profile generator enforces a no-ties constraint by redrawing the second profile until it differs from the first on at least one attribute. Attribute-level order was fixed (not randomized) to reduce cognitive strain; with six attributes visible on a standard desktop view, order effects were deemed unlikely. No exclusion constraints beyond the no-ties rule were imposed. Left/right profile position was not separately randomized; robustness checks indicate no positional bias, and the few always-left/right choosers are consistent with chance. There is no “opt out” option for conjoint tasks. The instrument is a Shiny application that logged task-level metadata, used reCAPTCHA v3 to deter and detect automated responses, and obtained approximate locations via IP geolocation (MaxMind, 2024) solely for aggregate description. Completion time was recorded from web logs; results hold when excluding speeders. Respondents ($N = 411$) were recruited between September 2024 and March 2025 via LinkedIn outreach, direct email, professional networks, and newsletters. Survey sessions were saved and synchronized to secure cloud

storage. Tasks required a choice to proceed; for sensitive demographic/background items, respondents could enter an explicit “NA” or similar. Conjoint choices were parsed to construct profile-level dummies and then collapsed into labeled factors; these profile-level observations were merged with respondent metadata by session identifier. The OECD Development Assistance Committee (DAC) status of the respondent’s country of residence was derived from the country resolved from IP geolocation.

Estimation, Reliability, and Robustness

The outcome is a binary indicator for whether a profile was selected. The principal estimands are Average Marginal Component Effects (AMCEs): the average change in selection probability when setting a level relative to its baseline, averaging over the randomization distribution of other attributes. I estimate AMCEs via a linear probability model with treatment-coded indicators and standard errors clustered by respondent. As a validation, I re-estimate effects using `cregg` and `projoint`; results are substantively identical (Appendix C).

Clayton et al (2025) note that conjoint survey experiments are affected by relatively greater measurement error compared to traditional survey questions. This is on account of their profile’s resemblance to complex real world choices, a highly desirable feature for external validity and efficiency that nonetheless comes with trade-offs; even highly attentive respondents make inconsistent choices when faced with identical or very similar tasks.

Because the measurement error correction methodology of (ibid.) was not known at the time of survey design, I did not include deliberately repeated, identical pairs of profiles, which the authors recommend as a strategy that can be implemented during survey design. This approach allows for straightforward recovery of intra-respondent reliability (IRR) by measuring the rate at which respondents provide consistent choices across these identical tasks. Instead, I implement the authors’ post-hoc extrapolation approach for estimating, and correcting for, intra-respondent reliability and measurement error based on the rate at which respondents provide consistent choices across very similar, but not identical, tasks. This derived an estimate of 0.758 IRR, which is consistent with the observed average IRR of 0.77 (range 0.73-0.81) across the eight conjoint studies replicated in (ibid.). After rescaling AMCEs and standard errors to account for measurement error, the corrected effects are 1.392 greater in magnitude than their uncorrected estimates.

Exploratory analyses probe heterogeneous effects across respondent subgroups. Motivated by theoretical expectations about sovereignty and colonial legacies, I assess whether the penalty for rhetoric-based conditionality differs between respondents based in OECD Development Assistance Committee (DAC) and non-DAC countries. Rhetoric conditions

carry a larger negative effect among non-DAC respondents, whereas legislation-based conditionality does not differ meaningfully by subgroup. These analyses were *not* preregistered and should be interpreted cautiously. Any departures from the preregistration are disclosed in Appendix A.

Ethics and Reproducibility

Participation was voluntary with informed consent obtained prior to any survey content. No deception was used. The instrument minimized collection of personally identifying information; IP-based geolocation was used only for aggregate descriptive mapping and was not retained in a way that permits individual identification in public materials. The study complied with institutional research ethics policies and underwent departmental ethics review. Analyses were conducted in R. A de-identified replication package containing the instrument, analysis code, and derived data necessary to reproduce all figures will be made publicly available upon publication.

Results

This section presents the results of the conjoint experiment examining elite preferences for foreign aid packages with varying conditions and characteristics. The analysis proceeds in four parts: first, descriptive statistics about the survey respondents; second, the main coefficient estimates from the conjoint analysis; third, an assessment of measurement reliability using the Clayton et al. (2025) approach; and fourth, robustness checks examining heterogeneous treatment effects.

Sample Characteristics

The conjoint survey was completed by foreign policy professionals and experts recruited through multiple channels. Figure 2 shows the global distribution of respondents based on IP geolocation, demonstrating substantial geographic diversity across regions including Europe, North America, Africa, Asia, and Oceania.

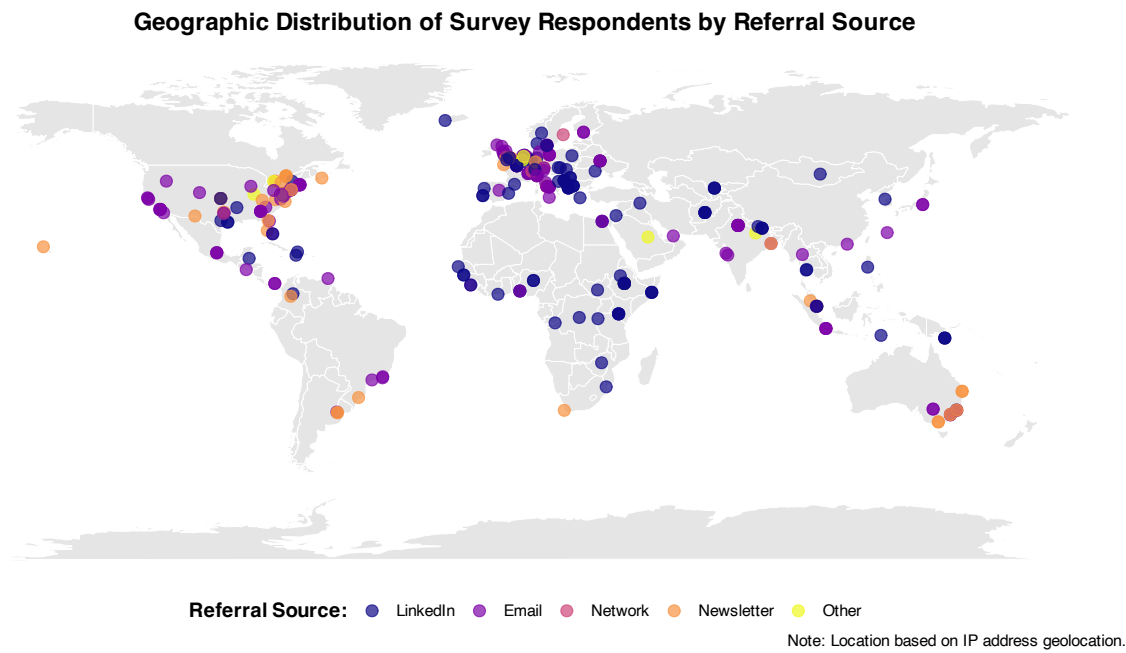


Figure 2: Geographic Distribution of Survey Respondents by Referral Source. Points represent individual respondents colored by how they were recruited to participate in the survey. Location based on IP address geolocation.

The sample includes respondents with varying levels of foreign policy experience and professional backgrounds. Some of these demographic traits are detailed in Table 1.

Table 1: Sample Characteristics

Characteristic	N	%
Sample Structure		
Respondents	411	100.0
Profile evaluations	4516	—
Mean profiles per respondent	11.0	—
Gender		
Male	282	68.6
Female	124	30.2
Other/Prefer not to say	5	1.2
Geographic Location		
OECD DAC Countries	244	59.4
Non-DAC Countries	167	40.6
Foreign Policy Background		
Professional FP work	326	79.3
Prior FP experience	358	87.1
Both professional & experienced	308	74.9
Years of FP Experience		
Less than 2 years	33	8
2-4 years	36	8.8
5-9 years	78	19
10-19 years	110	26.8
20+ years	154	37.5
Current Sector of Employment		
Non-Governmental Advocacy & Research	95	23.1
International Organizations	90	21.9
Foreign Ministry & Diplomacy	73	17.8
Academia	44	10.7
Government & Civil Service	35	8.5
Military & National Defense	30	7.3
Industry / Private Sector	28	6.8
Other	14	3.4
Parliament or National Legislature	2	0.5
Most Common Areas of Expertise		
Diplomacy and International Institutions	200	48.7
Politics and Government	183	44.5
Defense and Security	163	39.7
Foreign Aid and International Development	130	31.6
Human Rights	94	22.9
Terrorism and Political Violence	87	21.2
Economics	84	20.4
Social Issues	78	19.0
Energy and Environment	50	12.2
Health	23	5.6
Referral Source		
Direct Email	181	44
LinkedIn	147	35.8
Newsletter	62	15.1
Other	16	3.9
Professional Network	5	1.2
Survey Completion		
Median completion time	9.1 minutes	
Mean completion time	13.1 minutes	

Note: Sample includes respondents who completed the conjoint survey experiment. Each respondent evaluated multiple aid package profiles in a forced-choice format. Geographic location based on IP geolocation data. Foreign policy (FP) experience categories are not mutually exclusive.

Main Findings

Figure 3 presents the Average Marginal Component Effects (AMCEs) from the conjoint analysis, showing how different attributes of aid packages affect the probability that respondents would recommend accepting the aid offer.

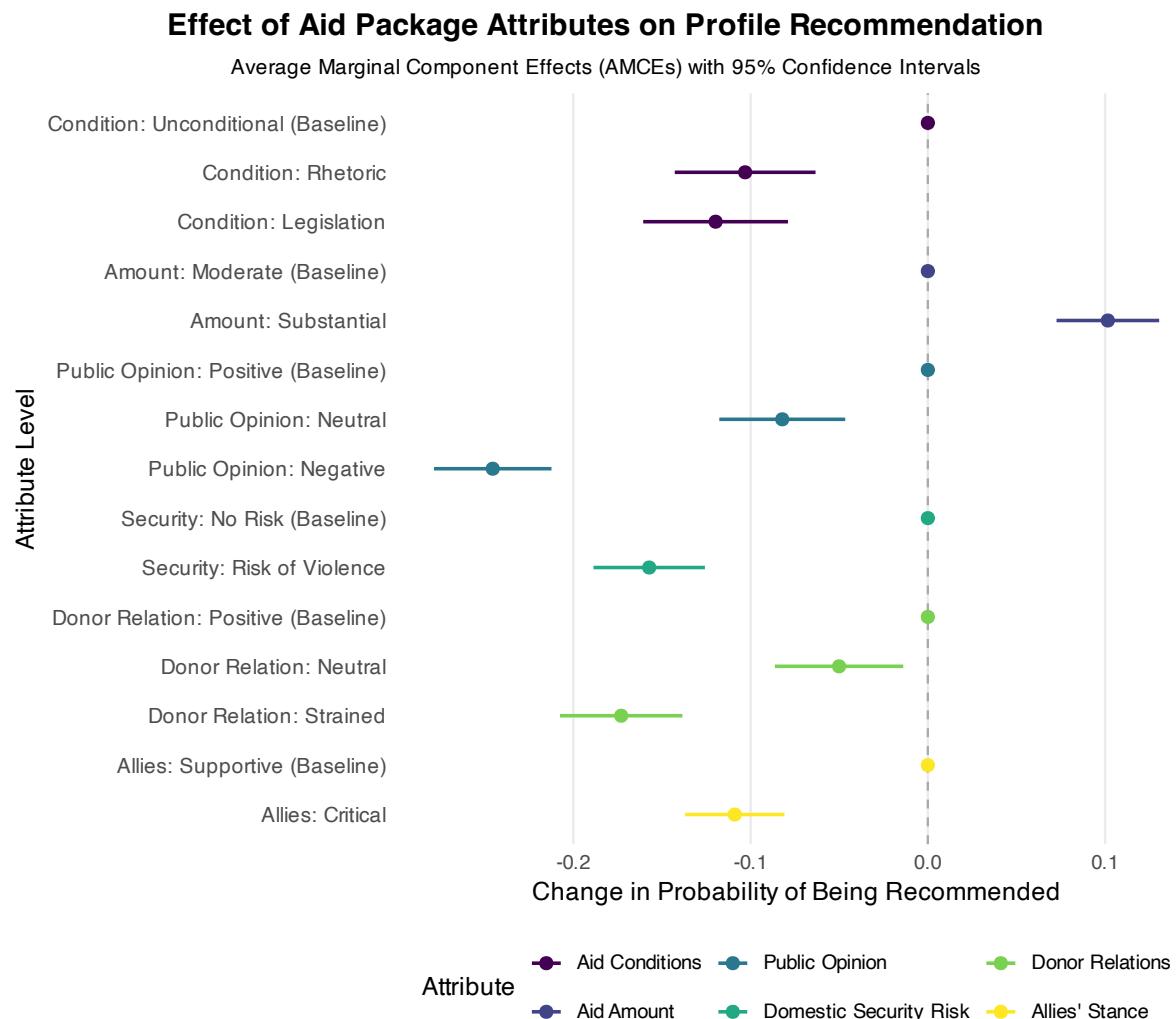


Figure 3: Effect of Aid Package Attributes on Profile Recommendation. The figure shows Average Marginal Component Effects (AMCEs) with 95% confidence intervals. Points represent the change in probability of recommending an aid package relative to the baseline category for each attribute.

Taken together, the estimates indicate that conditionality imposes meaningful political costs while material benefits and favorable contexts raise support. Using the primary, uncorrected AMCEs, both rhetoric-based and legislation-based conditions lower the probability that respondents would recommend accepting an aid package relative to unconditional aid. Rhetoric conditions decrease recommendation probability by 0.103 (SE 0.020), and legislative conditions by 0.120 (SE 0.021). These penalties are sizable and statistically precise, supporting H1a. The difference between rhetoric- and legislation-based penalties is small and not statistically distinguishable, providing no support for H1b.

The material scale of assistance matters. Substantial aid increases the probability of recommending acceptance by 0.102 (SE 0.015) relative to a moderate amount, in line with H2. Domestic political risks also weigh heavily: when aid is expected to provoke a risk of violence, the probability of recommendation falls by 0.157 (SE 0.016), consistent with H4.

Perceptions of public opinion strongly condition elite support. Relative to positive public reception, neutral public opinion reduces recommendation probability by 0.082 (SE 0.018), and negative public opinion reduces it by 0.246 (SE 0.017). The pronounced penalty for negative opinion, coupled with a smaller but still meaningful penalty for neutrality, supports H3a and H3b and highlights how domestic audiences constrain the political feasibility of conditional aid deals.

External political context further shapes preferences. Relative to possessing positive relations with the prospective donor, neutral ties reduce recommendation probability by 0.050 (SE 0.019), and strained ties by 0.173 (SE 0.018), supporting H5a and H5b. Finally, the stance of states which are allied with the recipient country carries independent weight: compared to supportive allies, a critical allied position lowers the probability of recommending acceptance by 0.109 (SE 0.014). This pattern indicates that international diplomatic alignment is a salient consideration for elites in aid policy bargaining scenarios and provides clear support for H6.

Robustness Checks and Heterogeneous Effects

Subgroup Analyses

To assess the generalizability of the main findings, I conducted several robustness checks examining whether treatment effects vary across different types of respondents. Of particular interest is whether preferences differ between respondents from OECD Development Assistance Committee (DAC) countries versus non-DAC countries, given potential differences in perspectives on aid conditionality rooted in colonial history and sovereignty concerns.

The robustness analysis reveals important heterogeneity in responses to rhetoric-based conditions. Figure 4 shows that the negative effect of rhetoric conditions is significantly more pronounced among respondents from non-DAC countries compared to those from DAC countries. This finding suggests that professionals from aid-recipient regions may view rhetoric-based conditions as particularly objectionable, potentially due to perceptions of neocolonialism or undue Western influence over the sovereign political discourse of recipient states.

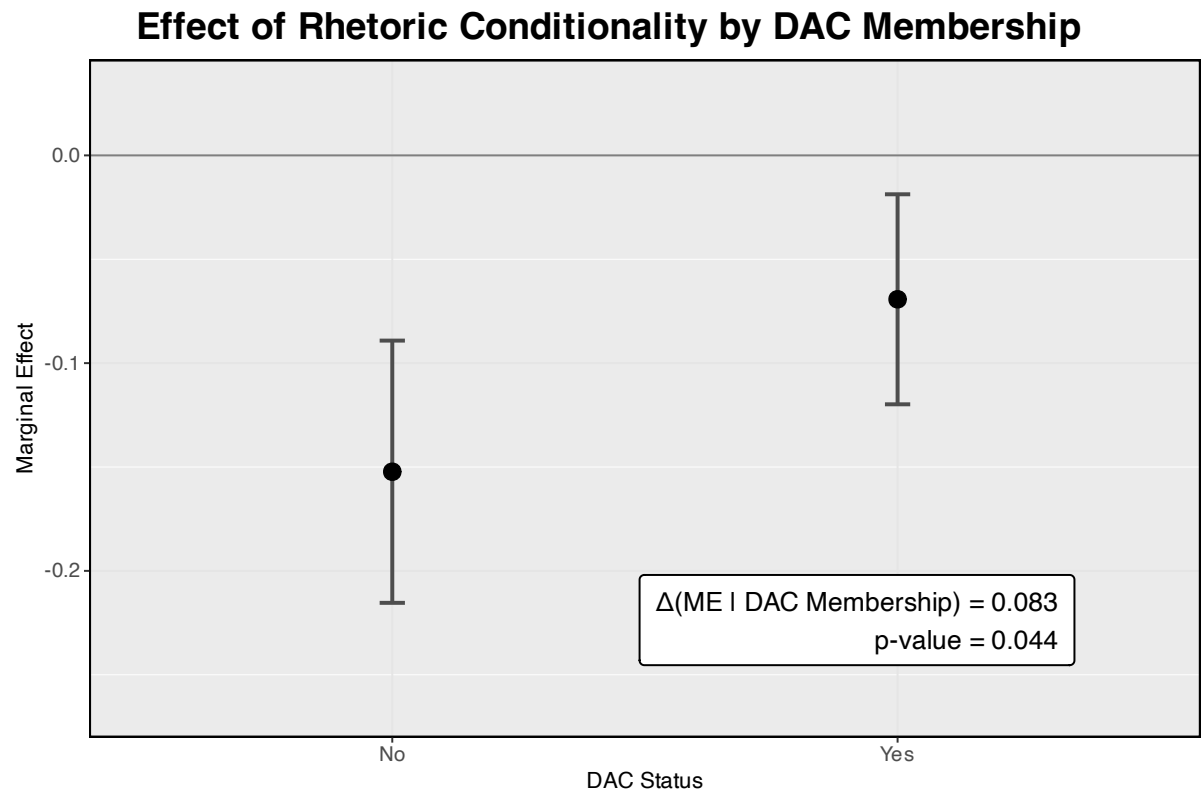


Figure 4: Heterogeneous Treatment Effects of Rhetoric Conditions by Respondent Location. The figure shows how the effect of rhetoric-based aid conditions varies between respondents from OECD DAC and non-DAC countries.

Notably, this pattern does not extend to legislative conditions. The analysis of legislation-based conditionality shows no meaningful or statistically significant difference in effects between DAC and non-DAC respondents, suggesting that policy-based conditions are viewed more uniformly across different regional and institutional contexts.

These heterogeneous effects have important implications for understanding the political feasibility and reception of different types of aid conditionality. While legislative conditions appear to impose similar costs regardless of the recipient’s institutional context, rhetoric-based conditions may face particular resistance in non-Western contexts where they could be perceived as attempts to control sovereign political discourse.

Intra-Respondent Reliability

Following Clayton et al. (2025), I assessed the reliability of respondent choices to correct for potential measurement error bias in conjoint experiments. Conventional analyses presume perfect intra-respondent reliability ($IRR = 1.0$) without empirical verification. By assuming $IRR = 1.0$ when estimating AMCEs, conventional analysis ignores inconsistency in respondents’ choices over profiles that biases effect sizes towards zero. Figure 5 shows the results of their suggested extrapolation approach to estimating intra-respondent reliability (IRR). The derived quantity, an IRR of 0.758 at zero differences between two task pairs

(four profiles total), suggests that respondents would exhibit reliability in their choices that is halfway between perfect reliability ($IRR=1.0$) and random guessing ($IRR=0.5$) if presented with the same pair of profiles twice. The accompanying statistic tau, $\hat{\tau}$, is 0.141, suggesting that about 14% of reported choices do not reflect respondent preferences.

Intra-Respondent Reliability and τ Estimates Via Clayton et al. (2025)

$$\widehat{IRR} = 0.758 \quad \hat{\tau} = 0.141$$

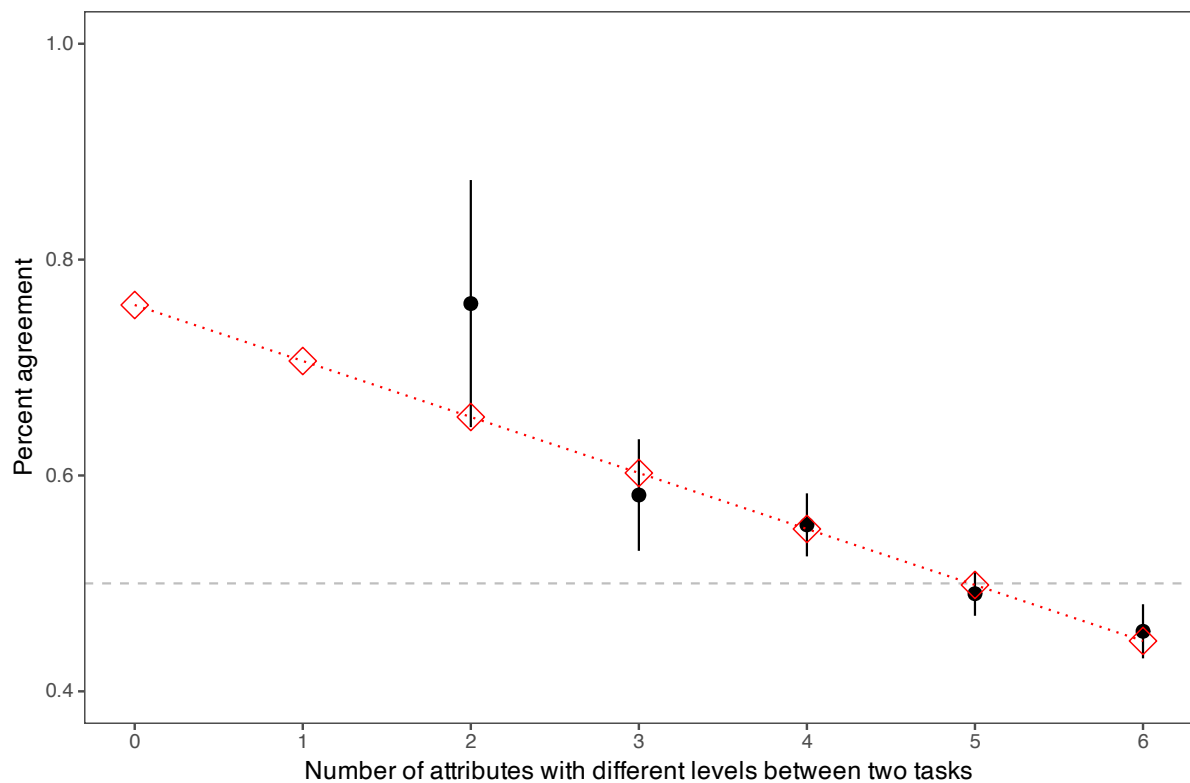


Figure 5: Intra-respondent Reliability Analysis Following Clayton et al. (2025). The figure shows the relationship between the number of attribute differences between task pairs and the probability of consistent responses, with a prediction of reliability at zero differences.

Correcting for this measurement error increases the point estimates and standard errors for the AMCEs by a factor of 1.39. The sign and relative ordering of effects remain unchanged. This naturally modifies their substantive interpretation by indicating larger treatment effects. However, Clayton et al. (2025) note that correcting for global (not by subgroup) measurement error always pushes AMCEs away from zero; effect sizes increase but the broader interpretation of model results should remain unchanged. The correction becomes more consequential with subgroup analyses.

Unlike the constant scaling factor that the correction reduces to when one assumes a constant IRR over respondent attributes, correcting effect estimates within subgroups can scale their respective estimates by different amounts: measurement error can vary between groups, and groups that less reliably translate their true preferences into their reported choice of profile will see *larger* increases in the magnitude of their AMCE's relative to

groups that answer more reliably or consistently. Such corrections can “increase, decrease, or flip the signs [of differences in AMCEs between groups] compared to the uncorrected estimate” (Clayton et al., 2025, p. 10). Accordingly, I derive estimates of IRR for various subgroups in the data and report these in the appendix. Further, corrected estimates were used to reproduce the subgroup analysis by DAC status that follows; the results hold even when accounting for different apparent measurement error between respondents in DAC and non-DAC countries.

Discussion

This article investigates how political conditionality shapes elite support for accepting foreign aid offers. Building on theories of aid policy bargaining and domestic constraints, I conceptualize conditionality as taking the form of requirements to change deeds (legislation) or words (rhetoric) and study how these conditions, alongside material benefits and political context, affect whether elite respondents would recommend accepting or rejecting foreign aid offers. To evaluate these claims, I field a preregistered, forced-choice conjoint experiment among foreign policy professionals, randomizing six attributes of aid packages across repeated choice tasks. This design permits estimation of Average Marginal Component Effects (AMCEs) for each attribute, providing a transparent mapping from theoretical considerations to observable preferences.

Figure 3 summarizes the core findings. First, both rhetoric-based and legislation-based conditions reduce support relative to unconditional aid, indicating that conditionality imposes meaningful political costs. The difference between rhetoric and legislation is small and not statistically distinguishable in the aggregate, suggesting that the two types of conditions are not perceived as differentially costly; “cheap talk” appears to be essentially just as costly to recipients as more concrete, material demands like modifying one’s domestic legislation. Second, larger material benefits increase support for acceptance. Third, domestic political context matters: risks of political violence and less favourable public opinion both depress support, with especially large penalties when public opinion is negative. Fourth, international political context also weighs on preferences: strained bilateral relations with the donor and critical positions from allies decrease support. Heterogeneity analyses further indicate that the penalty for rhetoric-based conditions is more pronounced among respondents from non-DAC countries, whereas effects of legislation-based conditions are comparatively uniform across subgroups (Figure 4).

The survey design guarantees that public opinion and domestic security risks vary independently of political conditionality as these channels are separately randomized. Consequently, the penalties attached to conditionality are direct. Even absent second-order audience costs or reputational spillovers, conditions impose costs that recipients must

bear to comply. Delivering on either deeds or words requires mobilizing administrative resources and cajoling domestic stakeholders. When compliance is not managed, donors retain tools to impose costs (e.g., suspension, delay, or enforcement via other channels). At the same time, the independent effect sizes for conditionality are not so large as to render conditional arrangements infeasible. In equilibrium, one should expect such deals to be struck, especially where material benefits are sizable and the broader political context is favourable.

A caveat is that, as highlighted above, conditionality may generate second-order effects that operate through public opinion and domestic security as both of these have large effects in their own right. Such spillovers could preclude deals if conditions become salient to domestic audiences or provoke backlash. However, because conditionality bargains are often secretive or opaque, recipients may obscure the hardest elements or their full extent, dampening these channels, albeit at nontrivial risk. The salience of allied stances and donor relations underscores that these calculations unfold in a strategic environment where diplomatic cover and alignment shape the perceived costs of acceptance. Further research exploring the extent to which these second-order effects are realised in practice would be especially valuable.

The heterogeneity by the DAC status of the recipient's country, while speculative and not preregistered, is worth noting. The stronger resistance to rhetoric-based conditions among non-DAC respondents is consistent with sovereignty sensitivities over visible, identity-laden signals: rhetoric at the UN is highly public and is taken to express a state's core political commitments. By contrast, legislation-based conditions can be framed as technocratic or as harmonization with international standards, potentially allowing recipient governments to deflect "puppet" accusations even though sovereignty is also implicated. This distinction might help explain why rhetoric-based conditions are particularly costly in non-DAC contexts while the penalty for legislation appears more uniform across groups. Future work could explore, validate, or reject this contention.

For donors designing conditional aid, these results imply that conditionality carries predictable political costs that must be traded off against the intended leverage on recipient policy. Where domestic audience constraints are salient or violence risks are non-trivial, larger material inducements may be insufficient to secure acceptance if conditions are perceived as intrusive or unpopular external impositions. Coordination with supportive allies and offers made in the context of positive bilateral relations appear more feasible. For recipient governments, the findings clarify where bargaining leverage is strongest: public opinion, security risks, and allied diplomatic cover shape the acceptability frontier of conditional deals. Practitioners can use these patterns to anticipate which combinations of conditions, amounts, and contexts are politically tenable.

Several limitations qualify these conclusions and point to future work. First, the study examines elite preferences in a survey setting; external validity to actual bargaining outcomes and mass publics requires caution and complementary designs. Second, the conjoint attribute set captures key dimensions but abstracts from many mechanisms that might be used to alter perceived costs of conditionality in aid policy bargaining scenarios. On this point, it's worth noting that the experimental design effectively restricted the tested mechanism to *ex-ante* conditions without clearly specified positive or negative conditionality properties, even though the theory discussed herein implies a greater set of conditionality mechanisms and types. Although testing the full set of conditionality mechanisms would balloon power requirements and render this study infeasible, future work might benefit from a more comprehensive set of conditions. Third, while measurement-error corrections indicate that baseline estimates are conservative in magnitude, the absence of deliberately repeated tasks means reliability is recovered via extrapolation. Future work could embed planned task repeats to recover intra-respondent reliability directly. Fourth, the subgroup analysis is exploratory, non-preregistered, and suggestive. Expanding coverage and representation across regions and professional roles would help pin down scope conditions for rhetoric- versus legislation-based conditionality.

An additional interpretive caveat concerns the modest independent effects of conditionality. One alternative to the interpretation adopted here that conditionality is perceived as costly but not prohibitive is that recipients may accept conditional offers precisely because they do not expect conditions to be enforced stringently. Ethnographic and policy accounts describe a bargaining equilibrium in which recipients display outward conformity while complying minimally in practice (Goodhand and Sedra, 2007, p. 43). As Kanbur (2000, pp. 5-6) puts it, “when push comes to shove, all of the pressures, mostly from the donor side, are to look the other way when conditionality is violated.” If donors lack appetite or capacity for enforcement, agreeing to conditions may have negligible practical impact relative to an otherwise identical unconditional offer. If this view is correct, the modest effects observed here reflect expectations of weak enforcement rather than inherently low compliance costs. Eliciting respondents' views on conditionality, and understanding which of these best characterise respondents' self-reported expectations vis-a-vis how such conditions might play out in practice, would provide a valuable check on this interpretation.

Conclusion

This article examined how political conditionality shapes elite recommendations to accept or reject foreign aid. In a preregistered forced-choice conjoint with foreign policy professionals, both rhetoric- and legislation-based conditions reduce support relative to

unconditional aid, whereas larger aid amounts increase it. Negative public opinion, risks of political violence, strained donor relations, and critical allied stances impose substantial penalties. These results imply that conditionality reliably carries political costs but is not prohibitive: conditional deals should be observed where material benefits are sizable and domestic and international contexts are supportive. The extent to which conditionalities might produce second-order effects through the public opinion and domestic security channels that are treated independently of conditionality in the experiment could significantly affect the extent to which this is realised empirically. A sharper penalty for rhetoric-based conditions among non-DAC respondents, while exploratory, points to scope conditions tied to signals over issues like terrorism that activate sensitivities over sovereignty and national identity.

The evidence is derived from an elite sample of foreign policy professionals that represent a close proxy of the population of foreign policy professionals that might be engaged in aid policy bargaining. Taken together, the findings clarify when conditional aid is politically tenable and potentially reconfigure theoretical expectations we hold over the political economy of aid conditionality and the behaviour of states with respect to terrorism.

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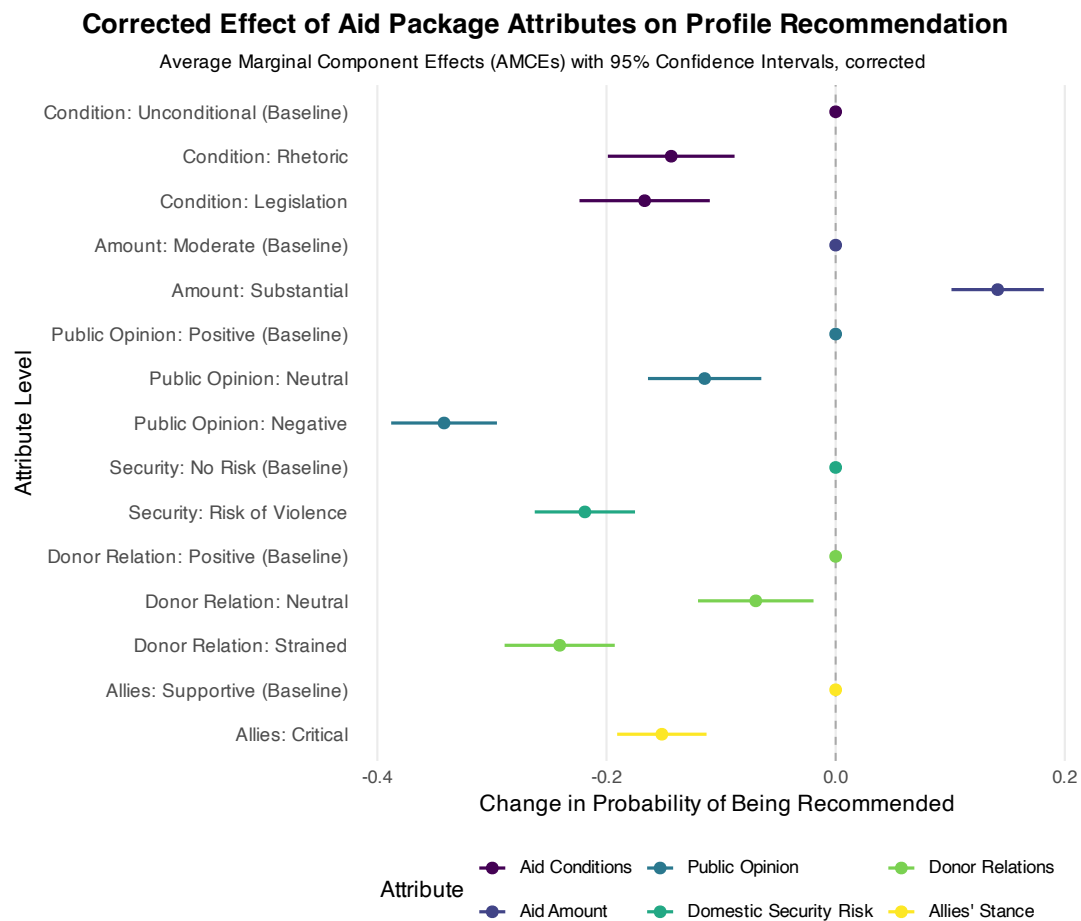
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Appendix

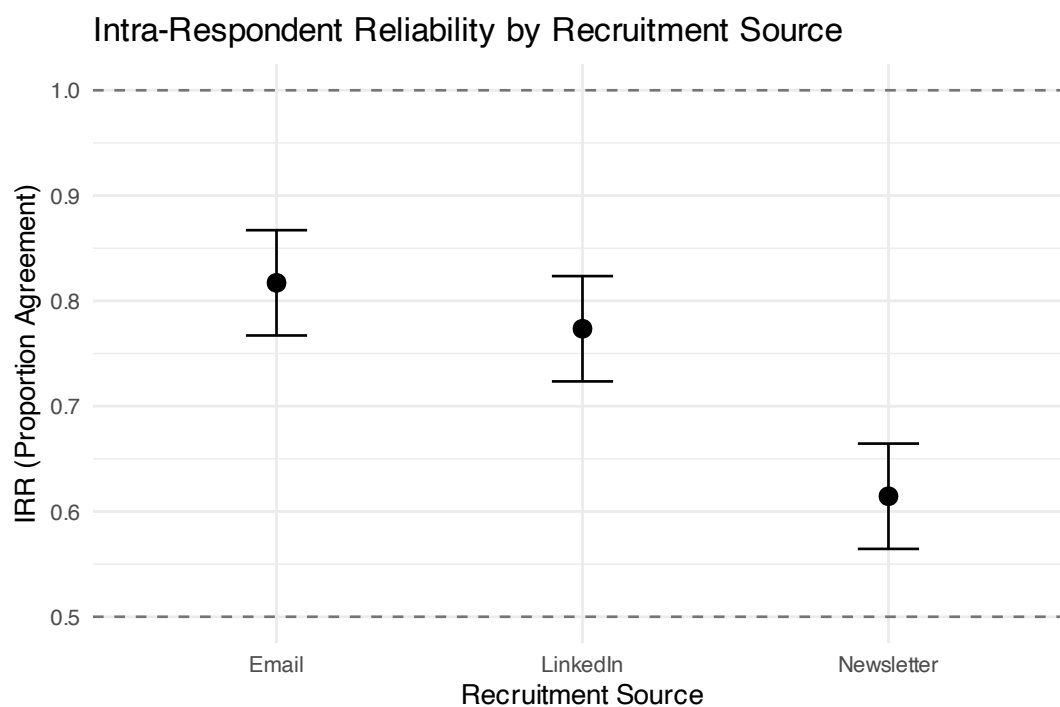
Preregistration

Preregistration link: https://osf.io/mbk5h/?view_only=b46d671705f74665b4f73daababdd123

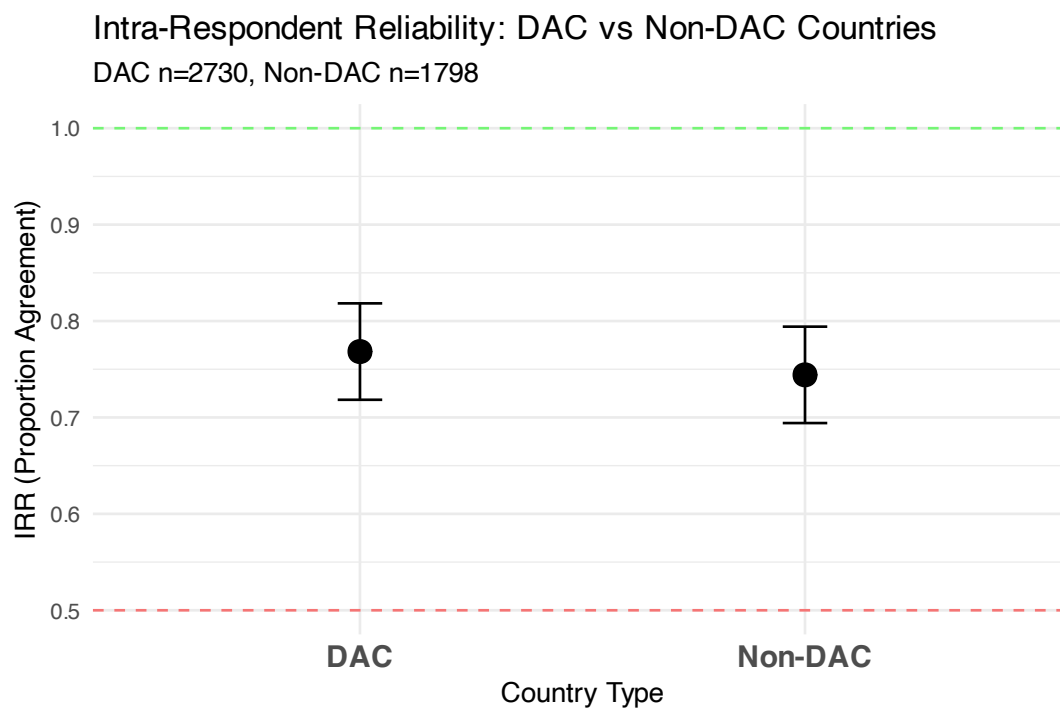
Reliability and Robustness Checks



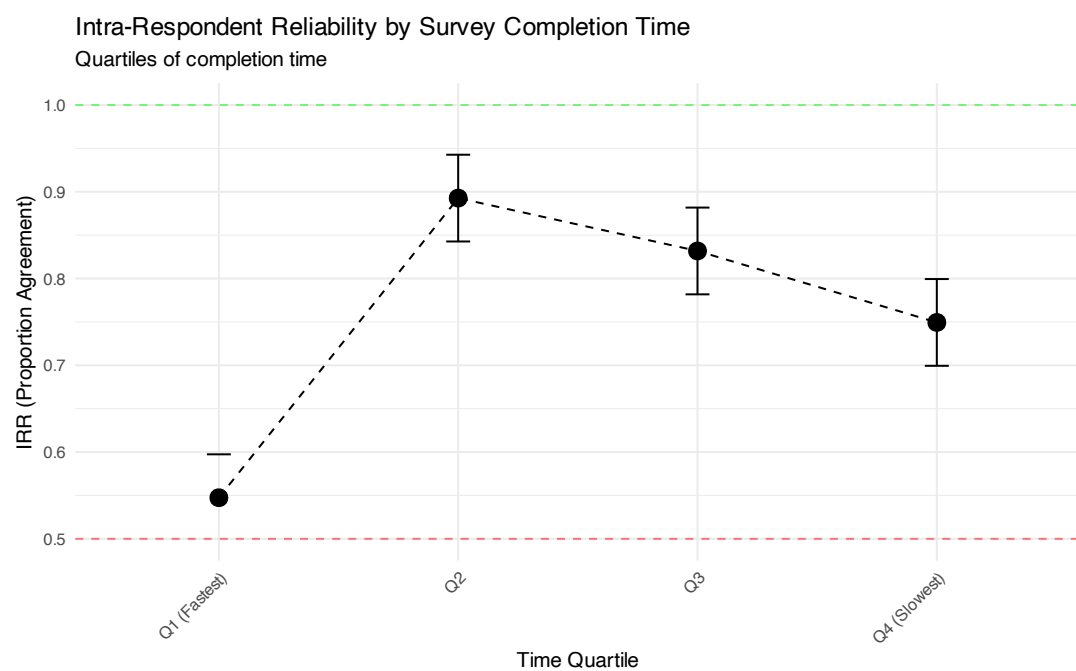
Main coefficients with correction for intra-respondent reliability. Correcting for estimated IRR increases coefficient magnitudes, indicating that uncorrected estimates are attenuated by within-respondent noise: the inability to perfectly translate their preferences into their chosen profiles consistently.



Estimated intra-respondent reliability by respondent source. IRR estimates by recruitment source indicate broadly similar reliability across sources, though generally lower for respondents from newsletter recruitment, supporting the comparability of responses.



Estimated intra-respondent reliability by DAC versus non-DAC affiliation. Reliability is comparable between DAC and non-DAC respondents.



Estimated intra-respondent reliability by interview time quartiles. IRR is notably lower for the fastest quartile of respondents, indicating that speedier completion is associated with less stable responses when presented with similar but non-identical profiles. However, overall results remain robust when these rapid responders are excluded from the analysis.