

Bearing the burden of peace: Intergroup attribution bias and public support for peace provisions

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Abstract

What drives public support for peace provisions? Based on intergroup attribution theory, we argue that public support for peace provisions depends on “who bears the burden of peace,” with people wanting to protect their ingroup while holding the outgroup accountable. To examine this claim, we conducted a series of question-wording experiments with more than 1650 Azerbaijani participants shortly after a deadly resurgence of the Nagorno-Karabakh war. Our initial findings confirm that support for war crime punishments and monetary reparations decreases when the ingroup stands trial or must pay. Conversely, support increases when these same burdens fall on the outgroup. A follow-up study shows that these patterns persist for at least 6 months but also reveals a more nuanced perspective. Public support for peace provisions does not automatically decrease when the ingroup bears the cost or increase when the outgroup bears it; rather, it depends on the group-based implications of the provision under consideration. Taken together, our results underscore the importance of crafting and communicating peace provisions in a tailored manner and the potential of emphasizing burden sharing as an effective strategy for addressing intergroup hostilities and fostering sustainable peace.

KEYWORDS

attitudes, Azerbaijan, experiments, intergroup bias, peace provisions

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How do we establish peace agreements that durably end conflict and build sustainable peace? Scholars have traced the success of peace agreements back to various factors, including the presence or absence of specific peace provisions (e.g., Druckman & Wagner, 2019; Matanock, 2017), women's involvement in peace negotiations (e.g., Krause et al., 2018), and the timing, sequence, and extent of the implementation (Joshi et al., 2017; Joshi & Quinn, 2017; Langer & Brown, 2016). Recently, scholars have also turned to public opinion, and particularly the perceived legitimacy of peace policies, as a critical determinant of peace settlement success (e.g., Garbiras-Díaz et al., 2021; Haas & Khadka, 2020; for a recent special issue on civilians and peace processes, see Haass et al., 2022; Loizides, 2014; Tellez, 2019a, 2019b). Most obviously, citizens influence peace processes when settlements are put to a referendum. However, emerging work and practice demonstrate how civil society and citizens can also weigh in on peace negotiations (Kew & Wanis-St. John, 2008) and how their cooperation determines success during the implementation of peace policies (Nilsson, 2012; UNSSC, 2020).

In this article, we contribute to the nascent work on public attitudes toward peace agreements by elucidating how intergroup attribution bias shapes public support for specific peace provisions. Peace provisions are designed to punish the parties who engaged in violence and rebuild nations after war. This, inevitably, involves certain costs.¹ Yet, how disclosing information about the party responsible for these costs affects support for peace provisions remains to be determined. In this article, we argue that public backing for peace provisions diminishes when the responsibility rests on people's ingroup and increases when the outgroup shoulders the burden. We also explore how an emphasis on burden sharing or contributions of the international community shapes public support.

To test our hypotheses and answer our research questions, we examine public support for different peace provisions in Azerbaijan in the aftermath of the Second Nagorno-Karabakh War. More specifically, we conducted question-wording experiments with over 1650 Azerbaijani respondents. At the time of our study, a cease-fire agreement was recently signed, and talks for a full peace treaty were underway. In an extensive battery on support for peace provisions, respondents were randomly assigned to a question on the desirability of monetary compensation for war victims and war crime punishments with no information on the cost bearer (i.e., control group) or with the cost bearer of those provisions being Azerbaijan (i.e., ingroup condition), Armenia (i.e., outgroup condition), or both groups (i.e., burden sharing condition). The international community was added as another cost bearer for the monetary compensation experiment.²

Overall, Azerbaijanis strongly support various peace provisions, including compensation for war victims and war crime punishments. However, this support drops decidedly when Azerbaijan is primed as the cost bearer. The magnitude of this priming effect is remarkable. On a 5-point scale, respondents in the ingroup condition are about 2 points less likely to support monetary compensation for war victims or war-crime punishments than respondents in the control condition. By contrast, support for peace provisions increases

¹We adopt a broad definition of “punish” and “costs.” We use the verb “punish” to denote imposing a variety of punishments or consequences in response to an action that is perceived as wrong or harmful. As a result, our use of the verb “punish” includes all peace provisions that place a burden on or hold accountable the one(s) deemed responsible for the wrongdoing, including paying monetary reparations and standing trial. Similarly, we use the word “costs” to denote not only monetary expenditures but all commitments and obligations aimed at restoring peace by addressing matters of guilt, harm, and accountability.

²While discussed in Azerbaijan, these two peace provisions were not included in the 2020 ceasefire. Nevertheless, they are included in many other peace agreements, as noted by Bell and Badanjak (2019). We also adopt their definition of peace agreements as “formal, publicly available documents produced after discussion with protagonists and mutually agreed to by some or all of them, addressing conflict with a view to ending it” (Bell & Badanjak, 2019, p. 453). This definition encompasses a wide range of agreements, including ceasefires, such as the one in the 2020 Nagorno-Karabakh War, as well as prenegotiation, substantive, and implementation agreements.

when Armenia is primed as the cost bearer, although to a smaller—predominantly because of ceiling effects—but still a significant extent. Furthermore, the impact of burden sharing on public support is inconsistent, while contributions from the international community do not significantly influence support. The findings from a follow-up panel study, replicating the original experiments and including seven new provisions, suggest that the observed effects endure over an extended period and extend to a broader set of peace provisions. However, how intergroup attribution bias determines support for peace provisions is not always straightforward; it depends on the specific provision under consideration, with the discernible patterns generally reflecting deeply entrenched beliefs about guilt and a desire for outgroup retribution.

This study offers valuable contributions to both science and society. First, it not only adds to the growing literature on public attitudes toward peace provisions but also deepens our understanding of legitimate justice and reconciliation at the individual level. We elucidate how conflict sharpens the boundaries between rival groups and how this, in turn, influences citizens' attitudes toward a diverse array of peace provisions. While previous work has studied the role of intergroup bias in shaping support for combatants (Lyall et al., 2013) or in the context of leadership endorsements (Garbiras-Díaz et al., 2021; Haas & Khadka, 2020), less is known about how sharing information about the cost payer shapes public support. Building on intergroup attribution theory, we clarify how this information relates to fundamental beliefs about responsibility and harm, subsequently impacting support. We also depart from previous work by focusing on *nine different peace provisions*, an *interstate conflict*, and both the *direct and intermediate aftermath* of conflict rather than peace agreements in their entirety, intrastate conflicts, or long-term tendencies. Second, we make several methodological contributions. Our question-wording experiments were embedded within a larger battery on attitudes toward peace provisions to elicit truthful answers to potentially sensitive questions about postwar retribution. Our easy-to-implement, unobtrusive experimental design and sampling strategy may interest other scholars studying the causal effects of informational cues regarding peace provisions in other contexts. Furthermore, without integrating multiple experiments into our online survey, we would not have been able to uncover the complex and intricate consequences of intergroup attribution bias. Finally, by replicating some of our experiments after 6 months, we also evaluate the durability of the treatments—a dimension often overlooked in most (survey) experiments. Third, this study provides timely policy implications for Azerbaijan—a geopolitically important country at the crossroads between Europe and Asia—and beyond. Our results present elites in post-conflict countries with a fundamental paradox: Although emphasizing outgroup contributions can increase support for (certain) peace provisions in the short term, tightening group boundaries may not be the best option for building sustainable peace in the long term. Instead, our results suggest emphasizing burden sharing as a more constructive alternative. Baseline support for all peace provisions was found to be high, and an emphasis on burden sharing could provide a more nuanced understanding of the fault lines in the long run, while not substantially reducing support for peace provisions in the short run.

THEORY AND HYPOTHESES

One of the most well-established ideas in social sciences is that humans have an almost instinctive tendency to divide the world into in- and outgroups and derive a positive social identity from favorable comparisons between the ingroup and relevant outgroups (Brewer, 1981; Tjafel & Turner, 2001). Social identities can be formed based on various social categories, including nationality, race, religion, gender, and partisanship (Huddy, 2001), and they play a pivotal role in forming political preferences (Druckman & Lupia, 2000). Because of the pervasiveness of social identities, ingroup favoritism—often accompanied

by outgroup denigration—constitutes a “remarkably omnipresent feature of intergroup relations” (Tajfel & Turner, 1979, p. 38). While social identities and subsequent intergroup biases are “omnipresent” in everyday life, they are often even more pronounced during and after conflict. For example, Nugent (2020) explains how the shared experience of widespread repression in the wake of the Arab Spring instilled a collective identity among opposition leaders in Tunisia. Similarly, Balcells (2012) finds that victims of the Spanish Civil War and the Franco dictatorship created a political identity opposed to that of their perpetrators. Lupu and Peisakhin (2017) add that not only survivors but also descendants of survivors of the 1944 Soviet deportation of Crimean Tartars identify more strongly with their ethnic group. Together, these studies demonstrate how shared experiences of trauma and violence shape and sharpen social identities (Hewstone et al., 2002; Wood, 2008). Such wartime identity transformation and consolidation have far-reaching social and political consequences (Wood, 2008). Most relevant in the context of our study, wartime cleavages have been shown to influence public preferences regarding transitional justice and punishment in post-ISIS Mosul (Mironova & Whitt, 2022) as well as in post-conflict Burundi (Samii, 2013), Guatemala, Nepal, and Northern Ireland (Dyrstad & Binningsbø, 2019). They also condition the effect of elite endorsements of peace deals (Garbiras-Díaz et al., 2021), with outgroup endorsements being particularly distrusted (Haas & Khadka, 2020).

While we know much about how conflict-related identity cleavages condition post-conflict public opinion, especially in the aftermath of intrastate conflicts, the literature offers no precise predictions or empirical evidence about how information about the actors paying the price for peace provisions will influence support for those provisions. In this article, we build on intergroup attribution theory and argue that social identities condition attitudes toward peace provisions through two mutually reinforcing mechanisms. That is, social identities generate both an *ingroup protection* and *outgroup retribution* mechanism by biasing evaluations of the motives and threats associated with the groups involved in the conflict. A long tradition in social and political psychology has demonstrated how social identities create expectations about responsibility and blame (Pettigrew, 2020). When people share an identity with perpetrators of political violence, they tend to perceive such violence as being influenced by attenuating situational factors or downplay its severity, resulting in a reduced inclination to advocate for strong punitive measures. War crimes allegations directed toward the ingroup, for instance, are frequently dismissed as either fabricated or less harmful (Bilali & Vollhardt, 2019). On the other hand, when members of the outgroup engage in similar violent behaviors, it often reinforces existing prejudices about the inherently violent predisposition of the outgroup. As a result, outgroup members are seen as more threatening (Haas & Khadka, 2020), responsible for their actions (Bilali et al., 2012), and more deserving of harsh punishments (Lyall et al., 2013; Noor et al., 2019). In short, intergroup attribution bias often causes parties involved in intergroup conflicts to believe “that they are the ‘true’ victims of the conflict” while perceiving the outgroup as “the guilty, violent perpetrator” (Bar-Tal, 2000; Bilali et al., 2012; Noor et al., 2012, pp. 352, 356). Assuming that citizens prefer peace provisions that hold actors perceived as perpetrators accountable and reward actors perceived as victims (see also Tellez, 2019a), this leads to the following empirical prediction: Citizens will be more likely to support peace provisions that hold the outgroup accountable (i.e., *Outgroup Hypothesis*) but less likely to support peace provisions that hold the ingroup accountable (i.e., *Ingroup Hypothesis*).³ Put differently, we expect public support for peace provisions to be asymmetric and not reflect a utility-maximizing desire for peace independent of group affiliation.

³The hypotheses were pre-registered before data collection at <https://osf.io/x4trk/>. For more information about deviations and additions to the pre-registration, see Appendix F.

We not only test the hypotheses mentioned above, but we also investigate two additional research questions. First, we examine the impact of emphasizing burden sharing on public support for peace provisions (i.e., *Burden sharing Question*). On the one hand, emphasizing burden sharing could be considered fair as all parties engaged in war. To the extent that people's fairness concerns outweigh their ingroup favoritism in determining peace preferences, public support for peace provisions should increase. On the other hand, burden sharing may do little to alleviate intergroup bias. In that case, there are two options. Citizens may want to hold the outgroup accountable at all costs (even when the ingroup also must chip in), in which case emphasizing burden sharing would increase support.⁴ Alternatively, citizens may want to protect the ingroup at all costs (even when the outgroup also bears some burden), in which case emphasizing burden sharing would decrease support.

Second, we investigate whether and how contributions from the international community influence public support for peace provisions (i.e., *International Community Question*). Again we explore three possible effects. First, hearing that war victims would receive monetary compensation from international organizations (IOs), such as the United Nations, could positively affect public support for peace provisions since the ingroup would not have to bear the costs.⁵ Yet, the international community has received increasingly negative media coverage in Azerbaijan, leading the public to view IOs as biased and incapable of resolving the conflict (de Waal, 2010). Consequently, individuals may see the involvement of the international community as undesirable, which could make them reluctant to support provisions featuring the international community. In this case, we would expect an adverse effect of the international community on support for peace. However, another set of potential explanations—all leading to null results—includes the idea that citizens might harbor ambivalent or indifferent sentiments toward the international community, assume that the international community by default pays for reparations, or a combination of economic self-interest and unfavorable perceptions of international organizations.

THE NEVER-ENDING CONFLICT OVER NAGORNO-KARABAKH

We studied attitudes toward peace provisions in Azerbaijan shortly after a resurgence of the Nagorno-Karabakh war. Located at the crossroads between the East and the West, Azerbaijan has great geopolitical and economic importance. For example, against the backdrop of the Russo-Ukrainian War and energy repercussions for Europe, the so-called Southern Gas Corridor, starting in Azerbaijan, is one of the few rescue plans for energy security in Europe (European Commission, 2020). Illustratively, concerns about a disruption in Russian fuel led Azerbaijan to pledge to nearly double its gas supplies to Europe (Reuters, 2022). Hence, given Azerbaijan's strategic importance, it is surprising that so little academic attention has been paid to the war(s) in Nagorno-Karabakh and pathways to peace and stability in the region.

The Nagorno-Karabakh⁶ conflict, fought between Armenia and Azerbaijan, is one of the world's most disputed, intractable, and widely neglected conflicts. The origin of the first

⁴Unfortunately, the empirical implications of people's fairness concerns and our outgroup retribution mechanism are indistinguishable.

⁵A positive result would not only be in line with economic self-interests but also with the credible commitment literature, in which engaging the international community is often seen as a means of alleviating credible commitment concerns.

⁶The grammatically more correct term is "Nagorniy-Karabakh" (de Waal, 2003), with Nagorniy meaning "mountainous" in Russian and Karabakh "Black Garden" in Azerbaijani.

full-fledged Nagorno-Karabakh War (1992–94) goes back to growing ethno-territorial tensions and self-determination sentiments during the late 1980s when both countries were still a part of a dissolving Soviet Union.⁷ The leading cause of these disputes was the Nagorno-Karabakh (“Karabakh” from now on) region, *de jure* a part of Soviet Azerbaijan but with the majority of its population being ethnic Armenians (de Waal, 2003). In February 1988, Karabakh Armenians demanded the transfer of Karabakh from Soviet Azerbaijan to Soviet Armenia. When this demand was rejected, the situation escalated into a full-fledged war in the early 1990s. After international mediation by several groups, most notably by the Minsk Group,⁸ a Russian-brokered peace agreement ended the fighting in May 1994. By then, Armenia was in complete control of the Karabakh enclave and captured seven additional Azerbaijani territories surrounding the enclave. As a result, Azerbaijan *de facto* lost a significant part of its territory to the self-proclaimed “Republic of Artsakh.” However, this territory remained *de jure* recognized as Azerbaijani (e.g., United Nations Security Council Resolutions 822, 853, 874, and 884 adopted in 1993). The 1992–94 war cost the lives of about 16,000 Azerbaijani and 4000 Armenian civilians and displaced over a million people, mostly Azerbaijanis from Armenia, Nagorno-Karabakh, and the surrounding areas.

Despite the ceasefire, (violent) tensions persisted, escalating into another full-scale war in 2020. This second Karabakh war—now often called the 44-day war—erupted on September 27, 2020, after intensified clashes in July 2020. In terms of casualties, the clashes were the worst since the 1994 ceasefire and caused alarm in the international community. Most of the casualties were soldiers, with more than 5000 dead, although it is estimated that about 150 civilians also lost their lives (Amnesty International 2021). On November 10, Armenia and Azerbaijan agreed to a new Russian-brokered settlement to end the war. The settlement calls for Armenia's army to withdraw from the Nagorno-Karabakh region and to be replaced by Russian peacekeepers. The settlement also stipulates that Azerbaijan can keep the areas of Nagorno-Karabakh it has regained over the 6-week conflict, displacing tens of thousands of ethnic Armenians from Nagorno-Karabakh and sparking angry protests across Armenia (Kramer, 2021). Hence, this time, roles were reversed, with Armenia losing significant territory in and around Nagorno-Karabakh and Azerbaijan regaining control of most of its internationally recognized territory (except for a small part). Table 1 summarizes the conflict's main events.

Azerbaijan and the Nagorno-Karabakh conflict serve as a compelling case study for exploring the impact of intergroup attribution biases on support for peace provisions. First, the case responds to recent calls to expand psychology research beyond Western, educated, industrialized, rich, and democratic (*WEIRD*) countries (Kahalon et al., 2018). Despite Azerbaijan's strategic geopolitical significance, which only increased since the Russian invasion of Ukraine, academic attention on the Nagorno-Karabakh wars and pathways to peace has been surprisingly scarce. Second, the case redirects the literature's focus from democratic to *nondemocratic regimes*. Even in authoritarian settings, public opinion can still influence policy implementation (Bell & Quek, 2018), and leaders may consider public sentiment to prevent unrest or dissatisfaction that could disrupt their rule. Azerbaijan recently started an ambitious postwar resettlement policy, for instance. Yet relocating citizens at such a large scale requires at least a certain level of legitimacy. Third, the recent resurgence of the Nagorno-Karabakh war (and our access to the case) allowed for the examination of support for peace provisions in the *immediate* (and *intermediate*; see below) aftermath of the 2020 peace deal, broadening the temporal

⁷Tensions over the region predate the Soviet Union, however. After the fall of the Russian Empire in 1918, the newly independent Azerbaijani Democratic Republic and the First Republic of Armenia both claimed the Nagorno-Karabakh region, leading to the first violent episode (1918–20).

⁸The Minsk Group was created in 1992 by the Conference on Security and Cooperation in Europe (CSCE), renamed Organization for Security and Co-operation in Europe (OSCE) in 1995, to mediate a peaceful solution to the Nagorno-Karabakh conflict. It is co-chaired by France, the Russian Federation, and the United States. For more information, see <https://www.osce.org/mg>.

TABLE 1 Timeline of main events.

1918–20	Armenian-Azerbaijani War
February 20, 1988	Vote on unification with Armenia; no unification
1988–92	Tensions, violent clashes, and pogroms; dissolution of Soviet Union
1992	Escalation of clashes into the first Nagorno-Karabakh War
May 1994	First Russian-brokered ceasefire
The 2010s	Episodic clashes
September 27, 2020	Escalation of clashes into the second Nagorno-Karabakh War
November 10, 2020	Second Russian-brokered ceasefire, which came into effect at midnight on November 10

The values in bold are the two main wars.

scope of existing research. Gaining insight into public support in the immediate aftermath of ceasefires recognizes the dynamic nature of peacemaking and sheds light on the conditions under which ceasefires are more likely to transform into (comprehensive) peace agreements. Furthermore, the timing of our study provided a real-world context where attitudes toward peace provisions were likely to be highly salient and emotionally charged. Indeed, the conflict and the 2020 peace deal received extensive coverage in local and national media, ensuring public awareness. The peace deal also facilitated the selection of provisions grounded in recent events, enhancing the study's external validity. Finally, the conflict's intergroup dynamic, involving distinct ingroups (Azerbaijanis) and outgroups (Armenians), provides a clear context to examine how biases influence attitudes toward peace provisions. The prolonged and intractable nature of the conflict likely strengthened social identities. In sum, the specificities of the case and the timing of our study allow for a most likely test of our hypotheses. Nevertheless, we believe the findings may contribute valuable insights into how intergroup biases influence support for peace provisions in other post-conflict settings with similar dynamics, such as Russia-Ukraine, Israeli-Palestine, and other conflicts pitting clearly delineated groups against each other and/or involving separatist and ethnic nationalist sentiments. We elaborate on the issue of generalization below.

MATERIALS AND METHODS

Experimental design

We fielded two question-wording experiments 2 months after the November 2020 Armenia-Azerbaijan ceasefire was signed. The experiments were part of a larger survey project that was fielded from January 5 to February 23, 2021, lasted approximately 15 minutes, and was available in Azerbaijani and English. The project obtained ethical clearance from KU Leuven (G-2020-2607) and included, among other things, informed consent, a debriefing, and several measures to ensure participant anonymity. We recruited participants online using Facebook advertisements. After some broadly publicized ads, we targeted our posts and ads to those groups at risk of being underrepresented in the data. We also implemented various measures to enhance the geographical accuracy of our sample (see Appendix A.1 in the online supporting information for details). On the one hand, our sampling strategy provided unique and well-powered experimental data collected at a relatively low cost in a hard-to-reach population (during the pandemic). By embedding experiments in an online

TABLE 2 Comparison of descriptive statistics for the sample and population.

Characteristics	Sample (<i>n</i> = 1657)	Population (<i>N</i> = 10,119,100)	Difference
Female (%)	35.7%	50.1%	−13.9 pp
Age (average)	40.5 years	32.3 years	+8.8 years
Living in Baku (%)	62.9%	22.7%	+40.2 pp
Bachelor's or higher (%)	80.9%	13.7%	+67.2 pp

Note: Population statistics are drawn from the Azerbaijani 2020 census (State Statistical Committee of the Republic of Azerbaijan, 2021).

survey conducted within the respondents' day-to-day context, our design increases both internal and ecological validity (Mutz, 2011). On the other hand, we still rely on an unrepresentative convenience sample, limiting our results' external validity (see also below). For example, Table 2 shows how the sample is overrepresented by male respondents, highly educated people, and people living in the capital.⁹ Nevertheless, due to our demographic targeting strategy, our sample is more diverse than other convenience samples (such as student samples) and contains variance in theoretically relevant variables (such as political trust, conflict exposure, outgroup empathy, and threat perceptions; see Appendix Table A.2). After cleaning the data according to the pre-analysis plan, 1657 participants were included in the analyses.

After measuring participants' sociodemographics, political attitudes, and war exposure, two unobtrusive question-wording experiments were embedded within a larger battery measuring attitudes toward peace in general and peace provisions in particular. At the beginning of the battery, all participants read the following information: “Thinking about the conflict in Nagorno-Karabakh and adjacent areas (in particular the Kalbajar-Lachin region), to what extent do you disagree or agree with the following statements?” Respondents rated their level of agreement with each statement on a 5-point Likert scale ranging from 1 (*strongly disagree*) to 5 (*strongly agree*). Statements included general attitudes like outgroup forgiveness, empathy, and political tolerance, as well as specific views on transitional justice mechanisms like truth commissions, monetary compensation for victims, and punishment for war crimes. The order of the items within the battery was randomized. For the items on monetary compensation and punishment for war crimes the parties paying or standing trial were randomized as follows:

- Victims of the war, both Azeris and Armenians, should receive monetary compensation...
 - ... from the Armenian state. [outgroup condition]
 - ... from the Azerbaijan state. [ingroup condition]
 - ... from the Azerbaijan and Armenian states. [burden sharing condition]
 - ... from the international community (like the United Nations). [IC condition]
 - [control condition]
- All war crimes committed...
 - ... by Armenian forces ... [outgroup condition]
 - ... by Azeri forces ... [ingroup condition]
 - ... by both Azeri and Armenian forces ... [burden sharing condition]
 - [control condition]
- ... should be severely punished.

⁹According to the World Bank, internet penetration in Azerbaijan reached 85% by 2020, but, as in many developing countries, this is concentrated in the major cities, particularly the capital, Baku.

Several design decisions are worth noting. First, we included information on the nationality of the victims receiving monetary compensation to keep the recipients consistent and only manipulate the parties responsible for the payment. Second, attitudes in the control conditions are averaged across all possible assumptions about who should pay or stand trial. Unfortunately, discerning the default assumption from our data is not possible. Therefore, it is essential to interpret treatment effects as priming effects, indicating how public opinion changes when the cost payer is *explicitly mentioned*. Third, compensating victims and punishing war crimes are part of so-called transitional justice mechanisms, which are common, though not ubiquitous, after wars (Samii, 2013). While being discussed in Azerbaijan at the time of our study, they were not included in the 2020 ceasefire.¹⁰ Finally, randomization took place for both items separately; thus, respondents could be assigned to different conditions for the two items. Balance tests, reported in Appendix C.1 in the online supporting information, confirm that sociodemographic and political covariates are balanced across both experiments.

Statistical analyses

To identify the effect of intergroup bias on public support for peace provisions, we run Ordinary Least Squares (OLS) regression models. In all models, the dependent variable is the level of support for the peace provisions in question (1 to 5). The categorical treatment variables indicate which actor was named as the cost bearer, with five possible actors in the monetary compensation experiment and four in the punishment of war crimes experiment. The “no actor” condition served as the baseline category. All in all, the estimates of the average treatment effects (ATEs) are thus obtained using the following model¹¹:

$$\text{Support}_i = \beta_0 + \beta_1 \text{Outgroup} + \beta_2 \text{Ingroup} + \beta_3 \text{Burdensharing} + \beta_4 \text{International Community} + e_i$$

RESULTS

Main analyses

In Figure 1, we display mean levels of support for compensation and punishment across experimental conditions (see Appendix B.1 in the online supporting information for all numerical results). Before discussing ATEs, it is worth noting that public support for both provisions is high. Yet across all treatment conditions, and especially in the control condition, punishment enjoys slightly more support than compensation. Next, as predicted, public support drops when the ingroup (i.e., Azerbaijan) is primed as the cost bearer. The magnitude of this effect is remarkable. Compared to the control condition, respondents in the ingroup condition are 1.72 points less likely to endorse monetary compensation for war victims and 2.14 points less likely to endorse punishment for war crimes (both on a 5-point scale; $ps < .0001$). In terms of standardized effect sizes, this amounts to decreases in support of 1.08 standard deviations (95% confidence interval $[-1.21, -.95]$) and 1.43 standard deviations (95% confidence interval $[-1.53, -1.32]$). These decreases exceed most effect sizes within political science (Bauer et al., 2016; Godefroidt, 2023; Kertzer, 2022), as well as the standard “large” effect of $d = .8$ (Cohen, 1988). Substantively, the effect of priming

¹⁰We address this issue in a follow-up study; see below.

¹¹ β_4 is only estimated in the model on monetary compensation for victims.

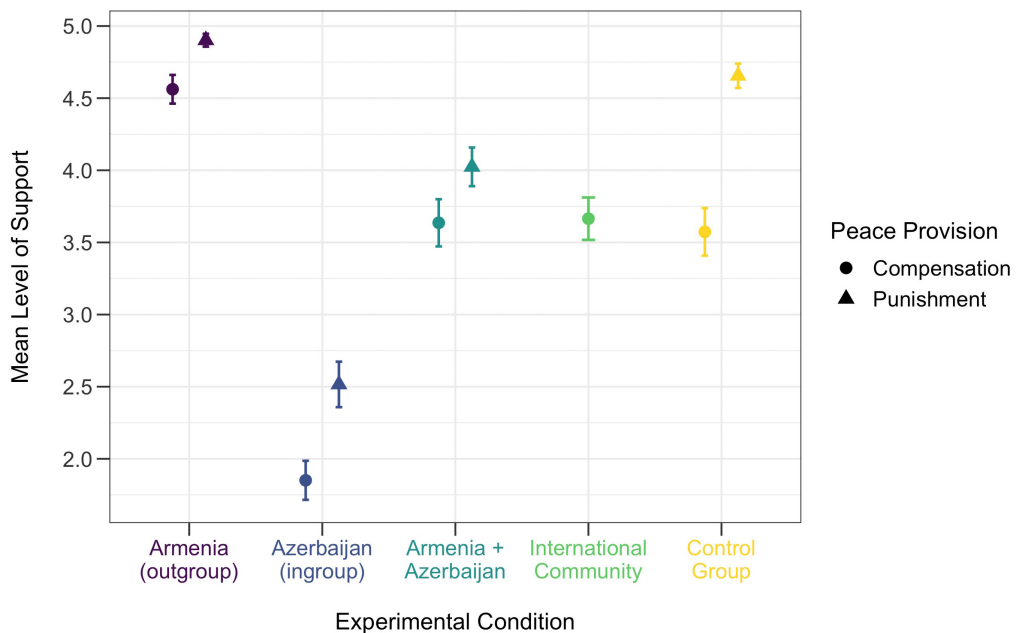


FIGURE 1 Mean level of support by experimental condition and peace provision ($n = 1657$).

the ingroup as the cost bearer also exceeds other commonly used predictors of support for peace, such as war exposure.¹²

By contrast, when respondents read that the outgroup (i.e., Armenia) bears the burden of peace, support increases and reaches almost the threshold of the scales. Notwithstanding possible ceiling effects, these increases in support are still substantial ($b = 1.00$ and $\beta = .62$ for compensation; $b = .25$ and $\beta = .16$ for punishment) and significant ($p < .0001$ for compensation; $p = .003$ for punishment). Ceiling effects could stem from a default assumption, in line with intergroup attribution theory, that Armenia is the sole culprit and, therefore, should bear the burden and face repercussions. Illustratively, over 85% of our participants *strongly* agreed that “the Nagorno-Karabakh conflict is caused by Armenian aggression and territorial claims for Azerbaijan” and that “Azerbaijan had no other choice than to use violence to protect its territory” (pretreatment statements).

Finally, neither the burden sharing nor the international community condition significantly influences support for monetary compensation compared to the control condition ($p = .546$ and $p = .376$, respectively). However, respondents in the burden sharing condition are less likely to endorse punishment for war crimes ($b = -.63$; $\beta = -.42$; $p < .0001$). This suggests that the ingroup protection mechanism may outweigh punishing the outgroup when it comes to this retributive transitional-justice mechanism, and that people might not perceive Azerbaijan as a perpetrator of war crimes to begin with. All results are robust to including pretreatment covariates (Appendix Table D.1), using robust confidence intervals (Appendix Table D.2), and excluding respondents who took the survey in English (Appendix Table D.3).

¹²The difference in support between people in the control group ($n = 321$) who were and were not exposed to war-related violence in 2020 equals $b = .23$ or $\beta = .15$ ($p = .19$) for monetary compensation and $b = -.13$ or $\beta = -.09$ ($p = .46$) for punishment for war crimes. Thus, contrary to previous studies (e.g., Grossman et al., 2015; Tellez, 2019b), we find no association between exposure to war and support for peace provisions in our observational data (but see also null findings in Dyrstad et al., 2022). War exposure was measured by asking respondents whether they had been exposed to one or more of 10 war events (=1) or not (=0).

Follow-up panel study

To further probe the robustness of the identified effects, we invited all original participants to complete a follow-up questionnaire half a year after the initial study.¹³ Out of the 1657 participants who completed Wave 1, 1040 (63%) provided an email address to be contacted again. Of these, 394 (38%) completed Wave 2. Thus, a significant proportion did not participate in our follow-up survey, which may have compromised the validity of the panel study. However, three things mitigate this concern. First, attrition rates are very similar across treatment arms (Appendix Table C.3 in the online supporting information). Second, attrition is not systematically related to respondents' attitudes toward peace provisions measured at Wave 1 nor to a series of sociodemographics (Appendix Tables C.4 and C.5). To the extent that the observable pretreatment variables proxy potential outcomes, there is no reason to expect differential attrition patterns across treatment groups related to potential outcomes (Gerber & Green, 2012). Consequently, we report unadjusted regression estimates below (but the results remain the same when using inverse probability weighting; see Appendix D.3). Third, as preregistered, the sample size at Wave 2, while being substantially smaller, is still sufficiently powered, given the large effect sizes found in Wave 1.

The follow-up study served three main purposes. First, we wanted to explore if the adage “time heals wounds” holds by evaluating the endurance of the effect sizes. To do so, we replicated our experiments and assigned respondents to their *original* condition for the monetary compensation and punishment for war crimes provisions.¹⁴

Second, we wanted to determine the generalizability of our findings across different peace provisions. To do so, respondents were assigned *seven new* question-wording experiments in which we manipulated the cost bearer analogous to the original experiments. We carefully selected peace provisions from either the 2020 ceasefire agreement or the PA-X codebook (Bell & Badanjak, 2019). To improve external validity, we ensured that selected peace provisions had garnered media attention. We also ensured that all experimental conditions were plausible, meaning the international community was not always included as a cost bearer. Through this selection strategy (summarized in Table 3), we exposed participants to a set of peace provisions that are not only applicable in Azerbaijan but have also been implemented in other post-conflict contexts. Moreover, three discrepancies from the Wave 1 experiments merit attention. First, randomization at Wave 2 occurred at the respondent (and not item) level to keep the cost-bearer constant. Second, we changed from a 5-point to a 7-point scale, hoping to enhance variation. Lastly, the new experiments were introduced with the statement: “To what extent do you disagree or agree with the following proposals from the 2020 Armenia-Azerbaijan peace deal?” Consequently, in Wave 2, we explicitly mention the ceasefire rather than referring to the conflict in a general sense. After the experiments, we debriefed respondents that only some of the provisions are included in the 2020 ceasefire and that it is not yet clear who will have to pay for them.

Third, we theorized that individuals' support for peace provisions is rooted in intergroup attribution biases. However, the findings observed in Wave 1 may (in part) also be driven by economic calculations rather than intergroup bias. Given that money is a limited resource, individuals may prioritize investments in health care, education, and other domestic policies over reparations. Empirically, this means that citizens will be less likely to support provisions that their government has to pay for. To investigate the role of economic

¹³We followed Ryan and Krupnikov's (2021) routine preregistration template to convey what we have learned from the first wave and document the theoretical focus, questions and predictions, and empirical strategy of the second wave (<https://osf.io/x4trk/registrations>). Our first preregistration already anticipated a follow-up study, but the details of that follow-up were not established until the second preregistration.

¹⁴Note that we cannot just measure the outcome at Wave 2 as, by design, question-wording experiments deliver the experimental treatment and measure the outcome at the same time via a single question.

TABLE 3 Justification of selected peace provisions.

Peace Provision	Included in Ceasefire?	Included in PA-X?	Discussed in the Media?
<i>Wave 1 (repeated in wave 2)</i>			
1 Monetary compensation		✓ (“Material reparations”)	url1 ; url2
2 punishment for war crimes		✓ (“Courts”)	url1 ; url2
<i>Wave 2</i>			
1 Transport route within Azerbaijan	✓ (§9)	✓ (“Socio-economic reconstruction”)	url1 ; url2
2 Transport route between Karabakh and Armenia	✓ (§6)		url1
3 Trust-building workshops		✓ (“Reconciliation”)	url1 ; url2 ; url3
4 Clearing mines of ordnances		✓ (“DDR”)	url1 ; url2
5 Aid to the disabled		✓ (“Victims”)	url1
6 Release prisoners of war	✓ (§8)	✓ (“Prisoner release”)	url1 ; url2 ; url3
7 Return of displaced people	✓ (§7)	✓ (“Refugees/displaced persons”)	url1

Note: Information in brackets indicates relevant paragraphs in the 2020 ceasefire or sections in the Peace Agreements Database and Dataset codebook (version: February 19, 2018).

considerations, we manipulated the costs of the two transportation route provisions to be \$300 million, \$450 million, or \$600 million. These cost variations were deemed significant by the Azerbaijani locals who gave input on our questionnaire and also reflect the estimated cost of the Nakhchivan corridor. If the results in Wave 1 are indeed influenced by economic considerations, we would expect to observe a decrease in support for the transportation routes as the associated costs increase, especially in the ingroup conditions. With a significance level of $\alpha = .05$ and a power of $\beta = .80$, the minimum sample size needed to detect an effect similar to the smallest significant effect size found in the baseline experiments in Wave 1 (i.e., $\beta = .16$) is 303. As a result, the obtained sample size of $N = 394$ ($N = 387$ after listwise deletion) is adequately powered.

Durability of mean scores and effect estimates

Repeating the original experiments 6 months later revealed that intergroup biases persist for at least 6 months. Specifically, our results show that the average level of support for compensation and punishment within each condition (Figure 2; Appendix Table B.3 in the online supporting information) and, consequently, the average treatment effects (Appendix Table B.4) remain stable over time. There is one exception, however: Support for punishment declines over time ($M_{\text{difference}} = -.16 [-.27, -.05]$), especially in the control group ($M_{\text{difference}} = -.27 [-.47, -.07]$).

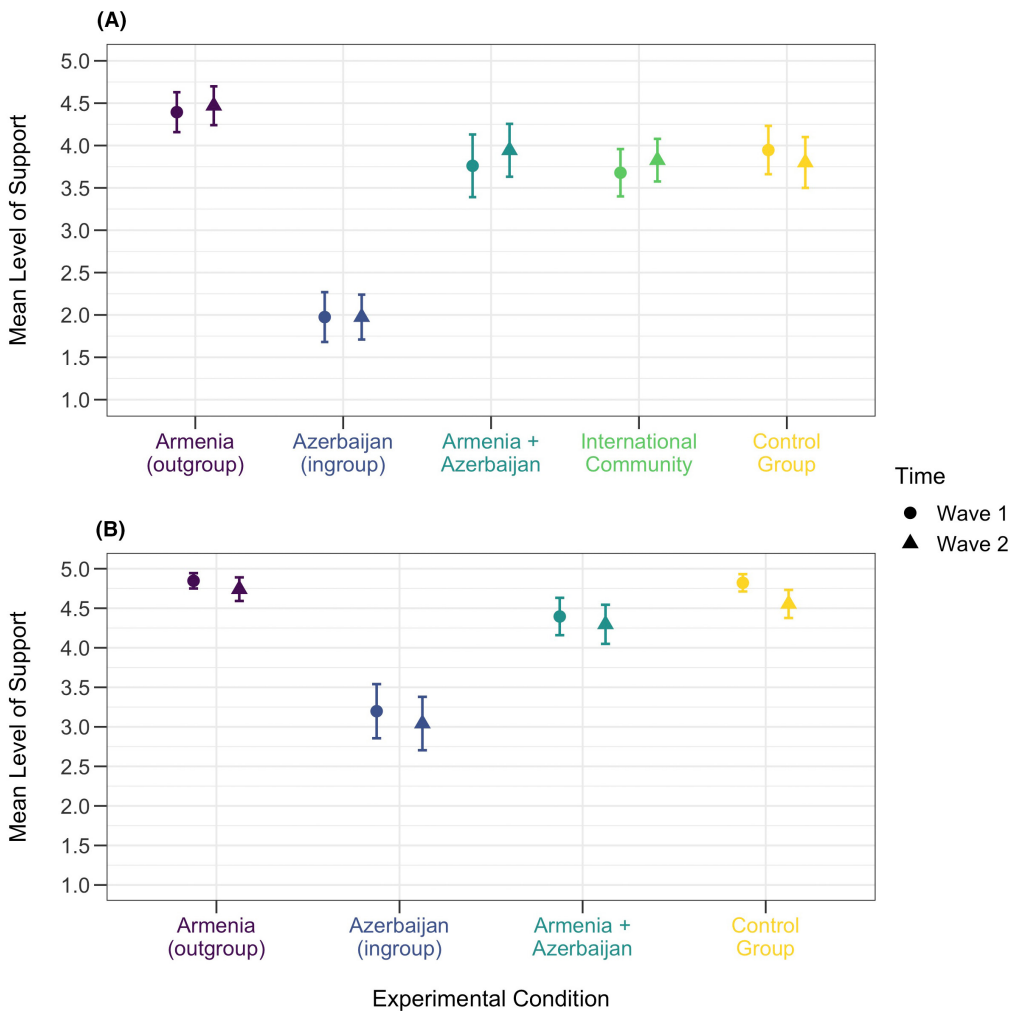


FIGURE 2 Mean level of support over time by experimental condition ($n = 373$). (A) Monetary compensation for victims. (B) punishment for war crimes.

Generalizability across different provisions

Next, our analysis of the seven new peace provisions reveals that support for peace provisions is primarily rooted in intergroup bias. Nonetheless, the specific manifestations of this mechanism are intricate and diverse, as they are contingent upon the specific peace provision being considered. Indeed, Table 4 shows that we could *not* replicate the patterns found in Wave 1 in any of the seven new peace provisions. In other words, public support for peace provisions does not automatically decrease when the ingroup is required to bear the cost or increase when the outgroup bears it; rather, citizens consider the potential on-the-ground implications of each peace provision in a thoughtful manner. Based on an inductive analysis, we identified three patterns (see Appendix Table D.1 in the online supporting information for further details):

TABLE 4 Average treatment effects for new peace provisions at Wave 2.

	Support for...					Mines	Aid	Release	Return
	Route 1	Route 2	Trust	Mines	Aid				
Armenia	-1.313*** (-1.849, -.776)	-.182 (-.919, .556)	-.529 (-1.203, .144)	-2.160*** (-2.639, -1.682)	-1.326*** (-1.752, -.900)			-1.828*** (-2.357, -1.298)	
Azerbaijan	-.100 (-.627, .427)	-.575 (-1.300, .150)	.363 (-.298, 1.023)	-.050 (-.519, .419)	-.000 (-.419, .419)			-.425 (-1.046, .196)	.075 (-.444, .594)
Both	-.426 (-.964, .112)	1.045** (.308, 1.782)	1.203** (.420, 1.987)	-.392 (-.944, .161)	-.149 (-.646, .348)			.992** (.255, 1.729)	-.102 (-.718, .514)
IC	-.400 (-.930, .131)	.030 (-.700, .760)	.350 (-.315, 1.014)	-.551* (-1.023, -.079)	-.730*** (-1.152, -.309)				
(Intercept: no actor)	5.912*** (5.540, 6.285)	3.675*** (3.162, 4.188)	4.638*** (4.171, 5.104)	6.525*** (6.193, 6.857)	6.512*** (6.216, 6.809)			5.213*** (4.773, 5.652)	6.125*** (5.758, 6.492)
Observations	387	388	356	357	357			278	278
R ²	.069	.050	.054	.226	.135			.050	.193
Adjusted R ²	.059	.040	.043	.217	.125			.040	.184

Note: Cell entries are unstandardized OLS coefficients with confidence intervals in parentheses and significant treatment effects in bold. Cells in gray are provisions included in the 2020 ceasefire. Support for each peace provision is measured on a 7-point scale. The included peace provisions are *Route 1* = New route connecting the Nakhchivan Autonomous Republic and western Azerbaijan, *Route 2* = New route connecting Nagorno-Karabakh and Armenia, *Trust* = Trust- and cohesion-building workshops between Armenian and Azerbaijani citizens, *Mines* = Clearance of towns, villages, roads, and the land around them of mines and unexploded ordnance, *Aid* = Aid to military personnel left disabled by the war and to the families of those killed, *Release* = Release of all prisoners of war, hostages, and other detained persons, *Return* = Return of people displaced during the 1994 and 2020 conflict. Full question wordings can be found at <https://osf.io/x4trk/>.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

1. Our respondents want to punish and hold Armenia accountable *but not* if this entails Armenian interference in Azerbaijani domestic affairs (e.g., investing in a new route on Azerbaijani territory only or clearing mines and unexploded ordnance in Azerbaijan);
2. Emphasizing Azerbaijan's contributions reduces support *but only* for those peace provisions that imply guilt (e.g., compensating victims or being punished for war crimes); and
3. Emphasizing burden sharing is beneficial *but only* for those peace provisions that work toward a common goal and, hence, require high levels of trust (e.g., investing in a new route that connects both countries, initiating trust-building workshops between citizens of both countries, or releasing prisoners of war on both sides).

There may also be design differences that could partly account for the divergent results. For instance, we use provisions included and excluded from the 2020 ceasefire. However, we cannot discern response patterns based on ceasefire inclusion (as evident in the comparison between gray and unmarked cells in Table 4). Furthermore, while Wave 1 manipulated generic provisions within the context of the war in general, Wave 2 focused on more locally relevant provisions explicitly tied to the ceasefire. Notably, all signs in the outgroup condition for more locally relevant provisions are negative (although not consistently significant), and none of the ingroup treatments reaches statistical significance. Again, we see this as evidence for our theoretical argument. The negative signs indicate a desire among Azerbaijanis to exclude Armenia from local Azerbaijani affairs, while the lack of significance in ingroup conditions can be attributed to the fact that such locally relevant matters imply less guilt compared to paying reparations or standing trial. Finally, even when holding the framing of the provisions very consistent, as with the two transportation routes, they still elicit substantially different response patterns based on the on-the-ground implications of the provisions, with citizens acknowledging the necessity of burden sharing in case of a route connecting both countries while excluding Armenia from building a route within their own country. Hence, we cautiously conclude that intergroup biases underlie our results but that the specific manifestation of such biases depends on the implications of the peace provision.

Finally, the findings regarding the role of the international community are not unequivocal but rather negative. In three of the six provisions involving the international community (Wave 1 + Wave 2), the coefficients were positive but not statistically significant. In the remaining three provisions, the coefficients were negative, and two of them were statistically significant. Notably, support for the dismantling of mines and assistance for disabled individuals decreases when the international community is involved, indicating a lack of trust in their role.

Alternative explanations

As mentioned earlier, an important alternative explanation for our results is that individuals may be reluctant about the idea of their government spending tax money on reparations. However, different findings do not support this idea. First, if economic considerations were the driving force behind our results, we would have expected an increase in support in each experimental condition where the ingroup (i.e., Azerbaijan) is not responsible for paying the price of peace. However, we did not find a significant increase in support in any of the six conditions to which the international community contributes. Even more so, if shouldering the economic costs requires granting the outgroup (i.e., Armenia) a say in domestic affairs, support declines. Second, employment status does not significantly moderate the monetary compensation experiment at Wave 1 (Appendix Table B.7 in the online supporting information). This implies that unemployed individuals, who may have greater concerns about their government spending tax money on peace provisions, do *not* exhibit a stronger reaction to the ingroup treatment. Third, as a final way to exclude the alternative explanation that a cost calculus drives our

TABLE 5 Average treatment effects for cost experiments.

	Route 1		Route 2	
	Overall	Ingroup Condition	Overall	Ingroup Condition
(intercept: 300 million)	5.613*** (5.304, 5.922)	5.864*** (5.281, 6.447)	3.548*** (3.132, 3.965)	3.000*** (2.135, 3.865)
450 million	−.106 (−.527, .315)	.208 (−.571, .987)	−.104 (−.673, .465)	−.107 (−1.263, 1.048)
600 million	−.319 (−.760, .123)	−.330 (−1.098, .437)	.729* (.133, 1.325)	.367 (−.772, 1.505)
Observations	387	80	387	80
Adjusted R^2	.0002	.002	.018	−.015

Note: Cell entries are unstandardized OLS coefficients with confidence intervals in parentheses and significant treatment effects in bold. Support for each peace provision is measured on a 7-point scale.

* $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

initial results, we manipulated the costs of the transportation provisions. As shown in Table 5, increasing the cost of peace provisions does not result in reduced support, neither across all conditions nor within the ingroup condition. If anything, higher costs increase support for the route connecting Karabakh with Armenia. Interestingly, this route is already the least favored peace provision (see intercepts in Table 4), and an increase in costs could make the route—a physical symbol of reconciliation—less feasible. The subsequent increase in support thus again points to an outgroup retribution mechanism rather than an economic calculus.

SCOPE CONDITIONS AND GENERALIZABILITY

What are the implications of our findings for understanding public opinion regarding peace provisions, both within the Azerbaijani context and beyond? Regarding the former, our sample is not representative of the general population (Table 2), and the effects we report can, therefore, only be generalized to the Azerbaijani population under certain assumptions. Appendix B.3 in the online supporting information alleviates this concern (at least to some extent) by showing that there is very little evidence of conditional effects based on gender, age, educational level, employment status, and urbanization. This tentatively suggests that if the sample were to include more women, different age cohorts, fewer highly educated people, and fewer residents of Baku, the results might not substantially change. Moreover, we collected respondents through Facebook, which may also bias the sample. However, social media usage is not correlated with support for compensation or punishment (Appendix Table D.1), nor does it moderate the results (Appendix Table B.9). In addition to these reassuring because statistically insignificant conditional analyses,¹⁵ the robustness of our findings is further enhanced by the fact that we analyzed no fewer than nine different peace provisions, which are included in many other peace agreements, and that all results remain essentially the same when we control for sample characteristics (Appendix Table D.1).

Regarding generalizations beyond Azerbaijan, several features of the Nagorno-Karabakh war render it unique. Since this is an interstate war, group borders coincide with national

¹⁵In the absence of extreme treatment heterogeneity, average treatment effects from convenience samples tend to generalize to the population of interest (Coppock et al., 2018).

borders. It is also an intractable war, marked by decades of state-controlled narratives (e.g., media narratives, teaching materials, etc.; de Waal, 2003). As a result, all generations in Azerbaijan have experienced the war in one way or another and have learned about it in similar ways. Such war experiences might have reinforced a sense of national identity among Azerbaijanis (following more than three decades of efforts by Soviet leaders to suppress local national identities). It is plausible that the effect of identity primes on support for peace provisions is strengthened by such contextual factors that increase the salience of social identities. Indeed, “the more salient the affiliation, the more biased the individual's beliefs about ingroup and outgroup members” (Iyengar et al., 2012, p. 408). The case is also unique in that Azerbaijan de facto lost territory to ethnic Armenians in the 1990s, which it has now (largely) regained. Narratives of Armenia's unjust appropriation of Azerbaijani land are prominent in Azerbaijani discourse (de Waal, 2003, 2010), and the perception that Azerbaijan has rightfully taken back what was theirs is common. Both a heightened sense of injustice after the first war and a heightened guard to protect territorial gains after the second war may have influenced expressed preferences about who should pay the price of peace. As Samii (2013) explains, “a sense of [political] gain may compensate for debts owed due to past abuses and induce a preference to avoid the pursuit of truth or punishment” (p. 220).

At the same time, other *interstate* wars, such as the war between Ukraine and Russia, are also characterized by the overlap of national borders and social identities, state-controlled narratives and disinformation, and strong group-based feelings of injustice. Since social identities are believed to be particularly salient under these conditions, it is not unlikely that our results would also hold for other interstate conflicts. However, we believe that our theoretical argument about intergroup attribution bias has a more general scope, and we expect it to apply to other contexts in which clearly delineated groups have perpetrated violence (including *intrastate* wars). Attribution biases have been documented in Burundi, Indonesia, Israel, Turkey, and the United States, among other countries (Ariyanto et al., 2009; Bilali et al., 2012; Halevy et al., 2022). While limited studies examine the impact of such biases on attitudes toward specific peace provisions, several studies did document how war-related social identities are correlated with political attitudes more broadly. For example, ethnicity still “trumps all other individual and contextual factors in explaining the respondents' [political] preferences,” years after the conflict in Macedonia was settled (Dyrstad et al., 2011, p. 346). Ethnicity is also still a defining social cleavage in post-Apartheid South Africa (Gibson & Gouws, 1999, 2003), post-conflict Northern Ireland (Hewstone et al., 2008), and post-war Burundi (Samii, 2013).

Taken together, we believe that people in Azerbaijan, as well as those in other post-conflict settings, are likely to assess peace provisions through the lens of conflict-based identities. However, certain distinctive features of our case study and sample may have reinforced these tendencies. As is often the case, obtaining definitive answers regarding generalizability would necessitate replicating our experiments using a more representative sample and across a wider range of post-conflict contexts. It would have been particularly intriguing to observe the outcomes of our experiments in Armenia, as it is now on the losing end. Unfortunately, practical and financial limitations prevented us from conducting experiments in Armenia.

Finally, we would like to acknowledge another limitation of our study design. We did not assess participants' prior beliefs regarding who should bear the burden of the various peace provisions. Therefore, caution is warranted when interpreting our treatment effects, as they may be influenced by participants' preexisting beliefs. For instance, people's prior beliefs about compensating victims may align with the burden sharing and/or international community conditions, potentially explaining the lack of observed effects. Similarly, participants' biases toward attributing blame solely to outgroup members may have caused a ceiling effect in the outgroup condition for war crime punishment. Directly measuring participants' priors could provide further clarity on the observed (null) effects.

CONCLUSION

On October 2, 2016, Colombians rejected a historic peace agreement with FARC rebels in a referendum that surprised the world. On November 9 and 10, 2020, Armenians, outraged by their government's concessions in the war with Azerbaijan, took to the streets, trashed the parliament, and stormed the presidential palace. And at the time of writing this article, local communities in Nigeria were fiercely resisting the reintegration of ex-Boko Haram fighters to the extent that authorities felt compelled to take ex-fighters back to rehabilitation centers. These examples illustrate the crucial role citizens can play in the negotiation and implementation phases of peace agreements. Indeed, citizens are not only important when peace agreements are evaluated through referenda (as in Colombia) or an election involving the negotiating leaders (as in the 2021 elections in Armenia), but their cooperation is also critical to the successful implementation of peace provisions (Nilsson, 2012). But what drives citizens' attitudes toward peace deals?

We demonstrate that public support for peace deals is determined by which actors are punished or rewarded (see also Tellez, 2019a) and whether those actors belong to the ingroup or outgroup. However, the specific way in which intergroup bias conditions support is not straightforward, as support does not consistently decrease (vs. increase) when the ingroup (vs. outgroup) is punished. Instead, individuals engage in a more complex but equally biased psychological process, evaluating the consequences of each peace provision. By broadening our analytical lens and examining a diverse array of peace provisions, this study challenges the simplistic notion that individuals are either in favor or against a peace agreement or that group alliances either increase or decrease support. The findings uncover considerable variation in attitudes toward different peace provisions and indicate that citizens engage in a nuanced evaluation process. They carefully weigh the implications of each provision through the lens of their group identity, leading to the formation of opinions aligned with their group-based perspectives. These results add nuance to the argument that intergroup identities matter in post-conflict societies and advance our understanding of legitimate justice and microlevel reconciliation.

Our findings have significant implications for peacebuilding in Nagorno-Karabakh and beyond, underscoring the critical role of tailored communication when disseminating peace agreements to the general public. The identity of the elites endorsing the agreements not only shapes the public's response to their messages (Haas & Khadka, 2020) but will also determine the selected content, framing, and tone of the messages themselves. Our study presents a dilemma for elites as they navigate this selection process: While capitalizing on ingroup affinity and outgroup animosity may yield short-term support, it is often not the most effective strategy for preventing a resurgence of violence in the long run. From this point of view, and based on our results, it seems more advisable to communicate peace provisions as something mutually beneficial and to which all parties contribute. Emphasizing burden sharing may not only create short-term support for peace agreements but also holds the potential to address the deep-seated hostility between rival groups more generally. Indeed, to achieve long-term sustainable peace, it is vital to build bridges between groups, for example, by enabling positive intergroup contact (Paluck et al., 2021; Tropp et al., 2017) and encouraging empathy with the outgroup (Maoz & McCauley, 2005).¹⁶

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¹⁶The latter was indirectly examined by testing whether outgroup empathy moderates the treatment effects (Appendix Table B.11 in the online supporting information). Findings indicate that individuals with higher levels of outgroup empathy exhibit weaker responses to the outgroup treatment, both regarding monetary compensation and punishment for war crimes. Moreover, greater empathy for the outgroup correlates with greater recognition that the ingroup should be punished for their war crimes.

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SUPPORTING INFORMATION

Additional supporting information can be found online in the Supporting Information section at the end of this article.

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