Loss Framing and Risk-Taking in Territorial Disputes*

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Abstract

We explore how framing could affect territorial bargaining. Building on prospect theory, we argue that contemporary international norms give leaders strong incentives to frame territorial claims as losses to be averted rather than gains to be obtained. This framing suggests that domestic audiences would become more supportive of risky escalation and less supportive of compromise settlements. Using survey experiments in Argentina and Chile about disputed territory in Antarctica, we find that loss framing indeed raises risk acceptance. Moreover, we find that, relative to benign or unspecified opponents, hostile opponents raise risk acceptance and somewhat attenuate the framing effect. Contrary to our expectations, historical ownership narratives do not induce framing effects. These findings corroborate the power of framing to shape public preferences in interstate disputes but also highlight limits to the scope conditions within which loss framing operates in such settings.

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Despite growing social and economic interlinkages that have been argued to have diminished the significance of land borders, states continue to go to war over territory more often than any other issue (Vasquez and Henehan, 2011; Toft, 2014). Mutually acceptable bargains appear much more difficult to strike in territorial disputes than rationalist bargaining theories would predict, prompting scholars to theorize about the sources of these bargaining failures and the causes of territorial disputes more generally.

We explore how bargaining failures in territorial disputes may result from risk-acceptant preferences induced by loss framing. Scholars have long observed that post-World War II norms generate strong incentives for leaders to frame disputed territory as having been "lost" or "stolen." Drawing on canonical findings from prospect theory, we argue that such loss frames make the domestic public more willing to gamble on risky escalation and less willing to accept peaceful settlements. To test our hypotheses, we field two preregistered experiments in Argentina and Chile that ask respondents to choose between a certain compromise and a risky bet over a real territorial dispute while varying the framing of the choices as gains or losses.[1] We find strong evidence that loss framing raises support for the risky bet in accordance with the predictions of prospect theory. This effect is robust to how respondents interpret the deliberately vague wording of our risky policy option. Notably, increased risk acceptance raises support for military action only for respondents who understand the risky option to mean military action.

Our experiments also situate the loss-framing mechanism among other factors that may be salient in territorial dispute settings. First, we test how framing effects and risk attitudes behave in the presence of an opponent, particularly a hostile one. We hypothesize that hostile opponents tend to amplify framing effects by making losses feel more acute, and raise overall risk acceptance by eliciting emboldening emotions like anger and by focusing respondents more on what is lost to the opponent even when outcomes are framed in terms of gains. Our results do not support the first expectation but partially support the second: relative to a benign or unspecified opponent, a hostile opponent increases risk acceptance only when the decision itself is framed in terms of gains. The framing effect is thus attenuated in the presence of a hostile opponent, perhaps due to ceiling effects on risk acceptance in the loss condition, but remains robust.

Second, we test whether framing effects result from the use of historical ownership narratives to justify territorial claims (Murphy, 1990). Although these narratives do not explicitly characterize decision outcomes as gains or losses, scholars have nevertheless speculated that historical claims set "golden age" reference points from a real or imagined past that implicitly frame the present-day status quo as a loss

[1] Our pre-analysis plans can be found at REDACTED (Argentina) and REDACTED (Chile).

(Jervis, 2004). Against these predictions, we do not find that historical ownership narratives produce framing effects, raising important questions about the scope conditions within which loss framing may operate in the context of territorial disputes. We do, however, recover suggestive evidence that historical ownership narratives boost perceptions of current ownership—an indication that reference points do not necessarily coincide with perceived ownership.

We contribute to multiple literatures in international relations, including on territorial disputes (Abramson and Carter, 2016; Hensel, 2001; Hensel et al., 2008; Toft, 2014, 2006; Fang and Li, 2020), the importance of enduring hostile relationships (Rasler and Thompson, 2006; Dreyer, 2012; Colaresi et al., 2008; Hensel, 2001; Diehl and Goertz, 2001; Hensel et al., 2008), and the relevance of prospect theory for international disputes (Gal and Rucker, 2018a; Shavit, 2018; Higgins and Liberman, 2018). While previous studies have examined prospect theoretic preferences in international politics (McDermott, 2004; Levy, 1997), to our knowledge we offer the first experimental test of prospect theory in the context of territorial disputes.

Loss-framing in territorial disputes

At its core, prospect theory hinges on the insight that humans derive utility not simply from the sum total of what they have, but also from changes in their assets relative to a baseline called a *reference point*. Ending up with $50, for example, feels very different if one begins with $100 than if one begins with nothing. Humans experience gains and losses differently in two major respects. First, they feel the pain of a loss more strongly than the joy of a gain of the same magnitude. Avoiding a loss is thus worth more than gaining an equivalent sum, a phenomenon known as *loss aversion*. Second, humans are willing to take greater risks to avert losses than to reap gains. Given a choice between a certain gain (e.g. $50) and a lottery with the same expected payoff (e.g. flipping a coin with a $100 gain for heads and $0 gain for tails), people are more likely to choose the certain gain. When choosing between a certain loss and a lottery with the same expected negative payoff, however, people are more likely to choose the lottery. Individuals are risk-averse when striving for gains, but more willing to take on risk when avoiding or recovering losses.

Risk preferences for the same outcomes can be changed by inducing people to compare those outcomes against different reference points, which violates the standard rational choice assumption of preference consistency. Taking the same coin flip example, imagine that person A is asked to choose between accepting $50 for sure or flipping a coin for $100, and person B is handed $100, and then asked to choose between

giving back $50 or flipping a coin to keep or forfeit all of it. In both scenarios, the outcome is the same: the person is choosing between receiving $50 with certainty or $100 with 50 percent probability. Person A, however, is much more likely to accept the $50 "sure thing," while person B is much more likely to take the coin flip. The framing of the situation determines whether the outcomes feel like gains or losses: person A's reference point remains at $0, so a certain outcome of $50 feels like a gain, whereas person B's reference point was set to $100 as soon as she was handed the money at the beginning of the game, so a certain outcome of $50 feels like a loss.[2] As Tversky and Kahneman (1981) write, "[v]ariations of the reference point can therefore determine whether a given outcome is evaluated as a gain or as a loss."

Observing someone's reference point, however, is no mean task, for it does not necessarily correspond to a strictly observable "status quo" (Shavit, 2018). The reference point can differ from the status quo in at least three ways. First, it can be an expectation or aspiration for the future (Mercer, 2005). A smaller-than-expected paycheck is a gain with respect to how much money you currently have, but a loss relative to how much money you thought you were going to make. Similarly, an electric bill that is smaller than you expected will feel like a gain, even though you still "lose" money by paying it. Second, a reference point might arise from social comparison (McDermott et al., 2008). If your neighbors all buy brand-new SUVs, the old hatchback in your driveway feels like a loss. Third, the reference point may be some prior status quo that has yet to update. Tversky and Kahneman (1981) give the example of a gambler who has lost most of her money over the course of an afternoon at the race track. Whether she cuts her losses and goes home or places a long-shot bet to recoup her losses depends on her reference point. If the reference point reflects her status quo asset endowment, then she is more likely to go home than risk losing more. If, on the other hand, her reference point remains where it was at the start of the afternoon, then a Hail Mary bet to undo her losses looks much more attractive.

In contemporary international politics, leaders have strong incentives to frame territorial claims in terms of losses. Post-World War II norms that sacralize territorial integrity and self-defense make it impossible for states to justify territorial claims to international audiences as anything other than "lost" or "occupied" territories that they are legally entitled to reclaim (Murphy, 1990). This constraint forces leaders to be more selective in the claims they make: territorial ambitions that cannot be legitimated as "losses" are not pursued. At the same time, when leaders do deploy these narratives, they set the claim line as the reference point against which outcomes are to be assessed, leading domestic audiences to view any outcome that falls

[2] This is a paraphrasing of an experiment first presented in Kahneman and Tversky (1979, 273).

short of securing control over the whole claim as a "loss." As a result, domestic audiences have higher levels of risk acceptance with respect to the dispute than if the leader had framed the territorial claim as a "gain."

Indeed, when we look at how leaders around the world talk about territorial claims, loss framing is endemic. For example, Vladimir Putin has done his utmost to frame the February 2022 Ukraine invasion as a reversal of loss. In his July 2021 essay entitled "On the Historical Unity of Russians and Ukrainians," Putin laments the estrangement of "hundreds of thousands or even millions" of ethnic Russians from the Russian state resulting from "a harsh and artificial division of Russians and Ukrainians." Not only was Russia "robbed" as a result of changes in the administrative borders of the Soviet Union's constitutive republics, but when the Soviet Union dissolved, "all those territories [of the republics], and, which is more important, people, found themselves abroad overnight, taken away, this time indeed, from their historical motherland."[3] While announcing Russia's annexation of four eastern Ukrainian regions in September 2022, Putin stated that "people in Donetsk and Lugansk, Kherson and Zaporozhye declared their support for *restoring our historical unity*" [emphasis added].[4]

For leaders, a risk-acceptant audience is a double-edged sword. On the one hand, risk-acceptant audiences are more likely to support the use of force, making leaders' threats to use force more credible in international bargaining. On the other hand, risk-acceptant audiences can narrow or even eliminate the range of compromises acceptable to both domestic audiences and the opponent, locking leaders into intransigent bargaining positions. Leaders cannot re-frame their audiences with impunity: gain-framing a claim may jeopardize the international legitimacy of that claim, since speaking of gains may appear to signal aggressive intentions. Experimental evidence also suggests that loss frames may be stickier than gain frames (Arkes et al., 2008, 2010). Once the domestic audience has internalized a loss-framed understanding of a territorial dispute, leaders may find it difficult to re-frame the dispute to muster political support for compromise. Lost territories become "phantom limbs" whose legacies can last multiple lifetimes, tying disparate generations into a shared narrative of collective national trauma (Billé, 2014).

Furthermore, because both challenger and target claimants in a dispute face the same international incentive to frame their claims as losses, we can expect audiences on both sides to hold risk-acceptant preferences. A state need not have controlled the disputed territory in living memory to claim it as a loss to be recovered; as Kahneman and Tversky (1982) write, "in many cases...the reference point is determined by events that are

[3]Putin's essay can be found at http://en.kremlin.ru/events/president/news/66181.
[4]See http://en.kremlin.ru/events/president/news/69465.

imagined." In one prominent instance, shortly after the Argentine invasion of the Malvinas/Falkland Islands in April 1982, Argentine leader Leopoldo Galtieri announced to jubilant crowds that "[w]e have regained the southern islands that legitimately form part of our national patrimony" (Mine, 1982). Naturally, the United Kingdom did not share Galtieri's reference point. Two days after Galtieri's speech, British Prime Minister Margaret Thatcher declared, "[o]ur objective is to recover the Falkland Islands."[5] The implications of such a scenario have not been lost on international relations scholars: Levy (1996, 190) suggests that "[s]tates might identify different reference points to frame their respective decisions and this might lead them all to perceive that they are defending the status quo." Drawing on insights from evolutionary biology, Johnson and Toft (2014, 32) argue that "[w]here both sides perceive themselves to be the territory['s] resident, the problem looms especially large because each side may expect to win and expect the other side to back down, somewhat regardless of size and strength." Christensen (2015) similarly argues that every claimant in the South China Sea "believes that they are defending sovereign territory and the status quo" and appeals to prospect theory in drawing out the implications for possible conflict. Where we depart from these scholars is that we do not consider the scenario where both sides view their claim as the "status quo" to be a special case. Rather, it is an inevitable outcome of the ubiquitous loss-framing of territorial claims.

If all sides of a dispute approach the bargaining table in the domain of losses, their risk-acceptant preferences may produce a certain form of indivisibility. "Indivisibility" has been used in two ways in the literature. The first is the "Solomon's baby" idea: an indivisible good is one "whose value is destroyed if [it] is divided" (Hassner, 2003, 12). This is the approach that Hassner (2003) builds on to show how sacredness constructs indivisibility in places like Jerusalem. The second describes indivisibility as arising when two bargaining parties have incompatible demands over an issue (Goddard, 2006; Toft, 2006). For Goddard (2009), Jerusalem is indivisible not because its division would destroy the essence of what makes it Jerusalem, but because the disputants cannot agree on a division compatible with how each side has legitimated its claim. Two people who each demand more than half of a pizza have not constructed the pizza to be indivisible in the former sense, but nevertheless may find the pizza effectively indivisible in the latter sense. Prospect theoretic preferences can produce indivisibility in precisely this latter sense: when two actors bargain over a good they both believe they have lost or are about to lose, their risk-acceptant preferences can

[5] https://www.margaretthatcher.org/document/104913. Support for a British "recovery" of the Falklands/Malvinas was by no means automatic or universal. The U.S. Ambassador to the UN, Jeane J. Kirkpatrick, apparently supported Argentinean claims to the islands. https://www.nytimes.com/2012/12/29/world/europe/falklands-war-caused-rare-friction-for-thatcher-and-reagan.html.

eliminate the bargaining range even in the absence of information asymmetries or commitment problems.[6]

Although the basic findings of prospect theory have been replicated across a variety of contexts, questions linger about how far these effects travel. From the earliest efforts to integrate prospect theory into international relations, scholars have been justifiably cautious about taking findings from carefully controlled lab experiments and importing them wholesale into the uncontrolled wilderness of real-world politics (Shafir, 1992; Levy, 1997). Since then, accumulated evidence from case studies (McDermott and Kugler, 2001; Elms, 2004; McDermott, 2004), observational data (Perla, 2011), and experiments (Boettcher III, 2004; Kertzer et al., 2022) provide optimism about the explanatory value of prospect theory in international relations. Nevertheless, recent research in psychology and behavioral economics calls for continued scrutiny of prospect theory's scope conditions. Gal and Rucker (2018b, 498) "[suggest] the need for a more contextualized perspective whereby losses sometimes loom larger than gains, sometimes losses and gains have similar psychological impact, and sometimes gains loom larger than losses" (see also Shavit (2018); Higgins and Liberman (2018)). Just as other studies have found that framing effects do not hold when people do not care about what is at stake (Bloomfield, 2006; Bloomfield et al., 2006), we may similarly fail to replicate these effects if territorial disputes affect people too little for them to care. As Yechiam (2018, 1327) notes, "[r]ecent studies have shown that loss aversion does not emerge for small-to-moderate losses." After all, many territorial disputes (including the one we use in our experimental setup) are over territories of negligible material or strategic value. Moreover, even when the disputed territory does hold some value, most who live in the countries involved do not experience the dispute personally (Zellman, 2018, 489-490).

Given these uncertainties about the context-dependence of prospect theory, our first empirical objective in this study is to establish whether or not framing effects hold in the context of territorial disputes. Replicating one of Kahneman and Tversky's original prospect theory prompts (Tversky and Kahneman, 1981), we conceptualize compromise in a territorial dispute as accepting a settlement for part of the disputed territory, and conflict as a risky bet on the entire disputed territory. We expect citizens to be more likely to settle for the certain outcome when the disputed territory is presented as a gain, and more likely to choose the risky bet when the territory is framed as a loss.

> ***H1****: People are more risk-averse when choosing between territorial settlement outcomes that are framed as gains, and more risk-acceptant when they are framed as losses.*

[6] See Butler (2007) for a formal treatment of coercive bargaining under prospect-theoretic preferences and Goemans and Fey (2009) for bargaining under risk acceptance arising from domestic politics.

Loss framing in context: Hostile opponents

As ubiquitous as loss-framing language may be in the way leaders talk about territorial claims, such language rarely appears in isolation. Many contextual factors may complicate the effects of framing language in the real world, making it difficult to map laboratory-produced framing effects onto real-world outcomes. To assess how framing effects operate in a more naturalistic setting, we examine how loss framing fits with two other commonly-cited drivers of territorial conflict: hostile opponents and narratives of historical ownership.

Scholars have often attributed territorial conflict to conflictual relations between the disputants (Goertz and Diehl, 1992; Hensel, 2001; Goertz et al., 2005; Hensel et al., 2008). An intractable dispute may be more a reflection than a cause of antagonism between disputants. Scholars have argued, for example, that the Dokdo/Takeshima dispute persists less because of any intrinsic value the islands hold than due to other unresolved sources of hostility between South Korea and Japan (Bukh, 2015; Wiegand and Choi, 2017). The implications of such dyadic interactions for prospect theory remain unclear because prospect theory is a decision-theoretic framework rather than a theory of strategic interaction. This has been a persistent challenge in applying prospect theory to IR: on its own, prospect theory has little to say about how an actor's decisionmaking is affected by interactions with others, especially in comparison to alternative frameworks like expected utility theory (Levy, 1997; de Mesquita et al., 2001; McDermott, 2004). While the full articulation of a "strategic" prospect theory is beyond the scope of this study, we can nevertheless test for some prospect-theoretic implications of introducing different opponents into a territorial dispute scenario.

We anticipate that the presence of an opponent, and especially a hostile one, may have two effects. First, we hypothesize that hostile opponents may *amplify* framing effects relative to benign or unspecified opponents by accentuating the contrast between losses and gains: the losses feel additionally painful when an adversary stands to benefit. Second, we expect hostile opponents to raise people's *overall* risk acceptance. Hostile opponents may trigger "emboldening" emotions that raise risk acceptance such as anger and pride (Pearlman, 2013). Furthermore, even in the domain of gains, hostility may induce people to weigh more heavily what the opponent is getting, and hence what they themselves are *not* getting, in a zero-sum negotiation. In effect, the presence of a hostile opponent may cue or reinforce a loss-framed construal of the scenario independently of outcome framing. People would therefore tend to reject bargains that guarantee the hostile opponent a certain gain—even if the choice is framed as a gain for themselves—in favor of gambles that leave open the possibility of forcing the hostile opponent to accept fewer gains or no gains at all.

We would therefore expect responses to become more risk-acceptant in both gain- and loss-frame conditions in the presence of a hostile opponent, when compared to a more benign opponent or no opponent.[7]

> ***H2****: The framing effect is greater when respondents are primed with a hostile opponent rather than a less-hostile or unspecified opponent.*
>
> ***H3****: Among both gain-frame and loss-frame respondents, those primed with a hostile opponent are more likely to choose the lottery over the certain gain/loss than respondents who are primed with a less-hostile or unspecified opponent.*

Loss framing in context: Historical ownership narratives

Another explanation of territorial conflict concerns the presence of historical ownership narratives. Since World War II, states have almost always legitimated their territorial claims as having some kind of historical basis (Hill, 1945; Pounds, 1954; Strausz-Hupé and Possony, 1954; Burghardt, 1973; Murphy, 2002). As political geographer Alexander Murphy notes: "The contemporary discourse of territorial claim justification reflects the recent ascendancy of the principle that a state is not entitled to seize territory from another unless that territory itself was originally wrongfully seized. . . . What this means is that the justifications now offered in support of territorial claims are almost invariably couched in terms of recovery of territory that historically belonged to the claiming state" (Murphy, 1990, 532).

Recent research has demonstrated that historical ownership narratives lead people to adopt more confrontational preferences on territorial disputes. Fang and Li (2020) and Fang et al. (2022) attribute this effect to historical ownership narratives constructing territories as indivisible. Murphy's argument suggests an alternative mechanism: historical ownership narratives may raise risk acceptance through loss framing. As the examples of territorial loss-framing cited above demonstrate, loss-framing language and historical narratives are deeply entangled, so much so that scholars sometimes characterize historical claims directly as loss frames. Jervis (2004, 174), for example, writes that "[m]ost states . . . adjust to losses of power and influence and come to accept reduced status and territory. But they rarely do so quickly. At first they seek to recoup their losses, and their reference point is the earlier rather than current status quo. This means that at any particular time many states are likely to feel themselves in the realm of losses because they can look back at an earlier 'golden age'." According to this logic, historical ownership narratives convey reference points from a real or imagined past against which potential decision outcomes are subsequently evaluated.

[7] For a related argument, see Lim and Tanaka (2022).

When China claims the Diaoyu/Senkaku islands as an "inherent" part of China, or when Argentina claims the Malvinas/Falkland Islands as "always" belonging to Argentina, these claims portray sovereignty over the territorial claim as the historical and rightful state of affairs. In that case, any outcome short of realizing the full extent of this control may be construed as a loss. Historical claims need not be based in effective control over the claimed territory, or even any history of effective control. Never having controlled the Malvinas/Falkland Islands does not prevent Argentina from justifying its claims as an historical entitlement. Propaganda, maps, school curricula, and works of art and media all perpetuate collective memories (or myths) of old injuries, baking them into national lore. Imagined pasts sustain imagined communities.

Testing the effects of historical ownership narratives within the framework of prospect theory, however, is hardly straightforward. Whereas experimental studies of prospect theory generally manipulate reference points by describing decision outcomes explicitly as "gains" or "losses," historical ownership narratives on their own only imply a reference point and leave it to the audience to work out whether an outcome is a gain or loss relative to that reference point. It is therefore possible for historical ownership narratives to appear alongside gain-framed descriptions of specific decision outcomes. China, for example, may endeavor to "gain" control over historically claimed islands in the South China Sea, in which case audiences would be presented with two competing reference points. Here, we follow other prospect theory scholars in distinguishing between *direct* frames, the explicit characterization of decision outcomes as gains or losses, and *indirect* frames, which do not say anything about particular decision outcomes but nonetheless suggest a domain (of gains or losses) within which decisions are to be made (Boettcher III, 2004). We set out to test whether historical ownership narratives can affect risk preferences as indirect frames, regardless of the direct framing of particular decision outcomes. Our approach bridges a critical gap between the laboratory and the real world: we test whether the direct framing effects that dominate the experimental prospect theory literature can in fact be extrapolated to real-world stimuli like historical ownership narratives that can only operate as indirect frames, if indeed they operate as frames at all. In accordance with the logic of indirect frames, we hypothesize that historical ownership narratives will tend to raise risk acceptance regardless of how the decision outcomes are directly framed.

> ***H4****: People are more risk-acceptant with respect to a territorial dispute when their side's territorial claim is legitimated in terms of historical ownership, and more risk-averse when it is not historically legitimated.*

Research design and case selection

To test our hypotheses, we run pre-registered survey experiments in Argentina and Chile.[8] We manipulate framing language around a territorial dispute and measure how different framing messages influence preferences between a certain option and a risky bet for settling the dispute. We turn to experimental approaches for four reasons: first, we must measure outcomes at the individual level to test a mechanism that operates at the individual level; second, showing whether these effects hold at the individual level is necessary for the theory to operate at higher levels of aggregation; third, survey experiments help circumvent the challenge of observing reference points by allowing us to manipulate them directly; and fourth, the experiments make our results easy to compare with previous work on prospect theory, both within international relations and beyond it, the bulk of which is experimental. We bracket the questions of how these preferences aggregate and how loss frames, antagonism toward opponents, and historical narratives manifest "in the real world," instead focusing on the individual-level mechanism as a first cut toward evaluating our theory empirically.

We design our experimental instrument around Argentina and Chile's disputed territorial claims in Antarctica. We use a real case rather than a hypothetical or fictional one to increase the external validity of our experiment. Case selection proved a formidable challenge, given the competing considerations to be balanced. On the one hand, the dispute could not be *overly* salient. In this case, a null finding would be nearly impossible to interpret: framing effects simply may not apply to attitudes toward territory, or respondents may have already been so deeply framed by prevailing rhetoric/narrative *ex ante* that our treatment could not move them. On the other hand, respondents must also take the scenario seriously: the dispute could not be so obscure that presenting it as a potential political issue would itself appear absurd.

The Antarctic claims balance these multiple concerns remarkably well. Antarctica's remoteness, lack of permanent population, and uneventful political history give the dispute relatively low political salience. Argentina and Chile are both signatories to the 1959 Antarctic Treaty, which bans any military presence or activity in Antarctica and effectively suspends the enforcement of territorial claims on the continent (Peterson, 1980). Indeed, the treaty stipulates that "Antarctica shall continue forever to be used exclusively for peaceful purposes and shall not become the scene or object of international discord." Despite some

[8] We obtained informed and voluntary consent from all survey respondents. Consent was documented via the online survey platforms. Our research adheres to the Principles and Guidance for Human Subjects Research established by APSA Council in 2020, including those regarding power, consent, deception, harm and trauma, confidentiality, and impact. Respondents in both Argentina and Chile were provided modest compensation for completing the survey and were informed of the compensation amount before they consented to participate. The study was approved by the Institutional Review Board of REDACTED.

fears about what global warming and geopolitics may bring, the international consensus that Antarctica be reserved exclusively for scientific research remains robust for now (Nature Editorial Board, 2012).

On the other hand, both countries' claims are common knowledge among their populations (Cardone, 2020). Textbook maps in Argentina and Chile show their respective Antarctic claims. Argentine law even mandates that official maps depict the Antarctic claim as part of the national territory.[9] Argentina's 50 peso note is very careful to show the Antarctic claim on the small national map that adorns its face (Figure 1). Depictions of the Antarctic claim are similarly widespread on Chilean maps (Figure 2). School curricula teach Argentine and Chilean children from a young age that they live in a "bicontinental" and "tricontinental" country, respectively (García, nd; Musalem, nd). The Antarctic claims are also the subject of occasional diplomatic theater. As recently as July 2020, Argentina's legislature passed a resolution "reaffirming" Argentine claims in Antarctica and the South Atlantic islands (including the Falklands/Malvinas). Chile's national legislature responded in kind with a "Chilean Antarctic Statute" of its own, and Chile's government also sent terse notes of protest to both Argentina and the United Nations (Montes, 2020). Presenting the issue to Argentine and Chilean respondents as a political question should therefore be credible.

One additional useful feature of the Antarctic dispute is that it has three claimant states—Argentina, Chile, and the United Kingdom (see Figure 3)—with varying degrees of hostile and friendly relations. Argentina and Chile have enjoyed friendly relations after the settlement of the dispute over the Beagle Channel in 1984, although they still have a minor border dispute in the Southern Patagonia Ice Field. Argentina and the UK continue to be embroiled in a hostile relationship over the Malvinas/Falkland Islands.[10] In contrast, Chile and the UK have not had a single territorial dispute beyond the Antarctic claims (Frederick et al., 2017). By manipulating which opponent we highlight in the prompt, we can begin to disentangle how interstate hostility affects the operation of loss framing in territorial disputes.

Experiment 1: Gain/loss framing and opponent priming

Our survey in Argentina was administered online to a sample of 3,210 adults in January 2020 by PoPa Research, a Brazilian survey research firm. The embedded experiment presents a brief vignette on the Antarctic dispute and then asks respondents to choose between a certain settlement—in which Argentina

[9] Law 26.651, of 10 October 2010. http://servicios.infoleg.gob.ar/infolegInternet/anexos/175000-179999/175020/norma.htm.

[10] For current claims to the Malvinas/Falklands by Argentinean leaders, see https://tinyurl.com/33eaacz7.

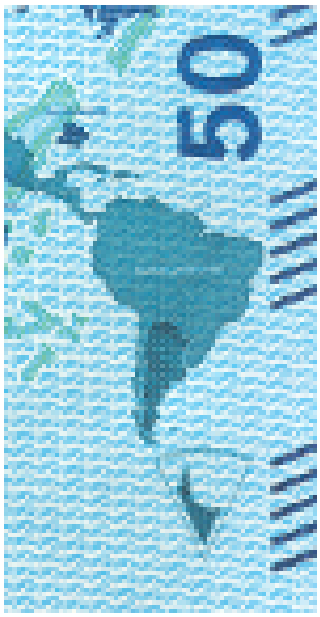


FIGURE 1: Detail of Argentine 50 peso banknote featuring Argentina's Antarctic claim.

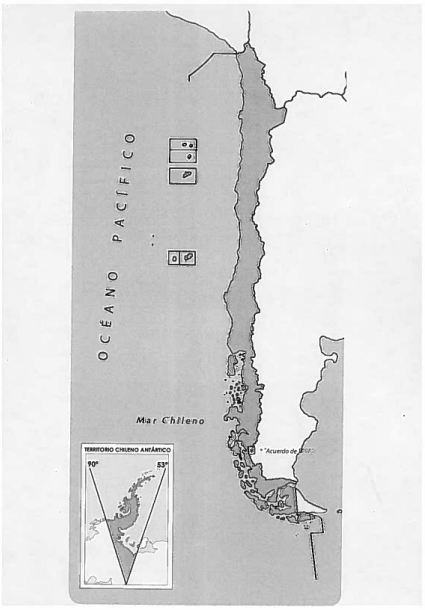


FIGURE 2: Chilean textbook map depicting Chile's Antarctic claim.

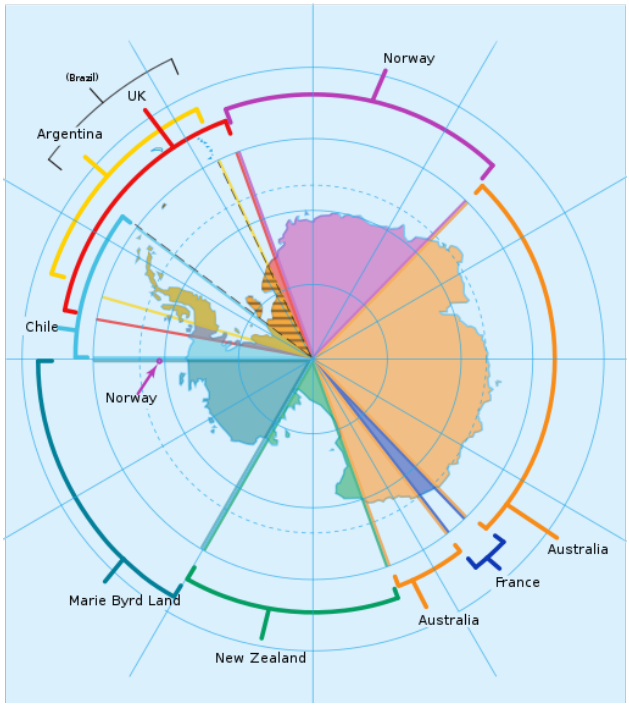


FIGURE 3: Territorial claims in Antarctica. Note the overlapping claims between Argentina, Chile, and the United Kingdom in the upper left. Source: Wikimedia Commons, user "A loose necktie", https://commons.wikimedia.org/wiki/File:Antarctica,_territorial_claims_including_Brazil.svg.

gets part of the disputed territory—and a gamble for the entire territory. We vary the experimental treatment in two dimensions. First, we randomly assign respondents to either a gain condition, in which outcomes are presented as gains, or a loss condition, in which outcomes are framed as losses. Second, we vary the identity of the opponent in the prompt to test if the framing effect is robust to which opponent is highlighted. Respondents are randomly assigned to one of three conditions: 1) no specific opponent mentioned; 2) Chile is mentioned as the opponent; or 3) the United Kingdom is mentioned as the opponent. This 2×3 factorial design produces 6 total treatment conditions, with gain and loss frame conditions on one dimension and the three opponent conditions on the other.

Both framing groups in the no-opponent condition receive the same initial prompt:[11]

> *As you may know, a large piece of territory in Antarctica is disputed between Argentina and several other countries. Imagine that the following two options are being considered to resolve the dispute:*

Respondents in the gain frame condition see the following two options:

A. *Accept an agreement in which Argentina obtains 1/3 of the disputed territory in Antarctica.*

B. *Pursue a policy where Argentina has a 1/3 chance of obtaining the entire disputed territory in Antarctica and a 2/3 chance of obtaining none of this territory.*

Which option do you prefer?

Loss-frame respondents see the following options instead:

A. *Accept an agreement in which Argentina loses 2/3 of the disputed territory in Antarctica.*

B. *Pursue a policy where Argentina has a 1/3 chance of keeping the entire disputed territory in Antarctica and a 2/3 chance of losing all of this territory.*

Which option do you prefer?

The option pairs in our experiment yield the same expected outcomes, but one pair presents the choice in terms of "obtaining" territory, while the other casts the choice in terms of "losing" territory. Under Hypothesis 1, a higher proportion of respondents should choose Option B when the prompt is framed in terms of loss than in terms of gain.

In the second and third opponent conditions, we identify Chile and the United Kingdom, respectively, as the dyadic opponent in the prompt:

> *As you may know, a large piece of territory on Antarctica is disputed between Argentina* ***and Chile/the United Kingdom****. Imagine that the following two options are being considered to resolve the dispute:*

[11] We model the prompt on Tversky and Kahneman (1981)'s famous "disease problem" experiment.

A. *Accept an agreement in which Argentina obtains 1/3 of the territory* ***disputed with Chile/the United Kingdom*** *in Antarctica [loses 2/3 of the territory disputed* ***with Chile/the United Kingdom*** *in Antarctica].*

B. *Pursue a policy where Argentina has a 1/3 chance of obtaining the entire territory* ***disputed with Chile/the United Kingdom*** *in Antarctica and a 2/3 chance of obtaining none of this territory [1/3 chance of keeping the entire territory* ***disputed with Chile/the United Kingdom*** *in Antarctica and a 2/3 chance of losing all of this territory].*

Which option do you prefer?

As articulated in the theory section, we expect that identifying opponents—each with its own history of conflict with Argentina—would amplify the framing effect (Hypothesis 2) and raise overall risk acceptance (Hypothesis 3). In Argentina, we expect the United Kingdom to produce stronger opponent effects than in Chile, due to the UK's victory over Argentina in the 1982 Malvinas/Falklands War, which remains politically salient in Argentine public discourse.

Table A1 in Appendix A summarizes the Argentina sample's demographic characteristics. Highly educated respondents are over-represented in our sample, likely an artifact of lower internet penetration rates among Argentines with lower educational attainment: 55 percent of respondents age 25 to 64 in our sample (53 percent of all respondents) have attained some level of post-secondary education, compared to the OECD's 2018 estimate of 36 percent for the same age group in Argentina's general population (OECD, 2019). Our sample is also somewhat skewed toward right-leaning respondents. To achieve covariate balance across treatment conditions, we block treatment assignment on age, education, gender, and region of residence. Despite these efforts, age and ideology are moderately imbalanced across treatment groups, though controlling for these characteristics does not substantively alter our results. Our regression analysis also controls for race (binary White/non-White), political ideology (10-point left-right scale from LAPOP's 2018 questionnaire[12]), and the following dispositional attitudes from Kertzer and McGraw (2012): international trust, social trust, and national attachment.

In Chile, we replicate the Argentina study on a smaller sample (N = 847). Data collection took place from June to October 2021 (in tandem with Experiment 2 below). We make two adjustments to the experimental design. First, we replace Chile with Argentina in the relevant opponent treatment. (The UK condition remains the same.) We expect that responses will be more similar across the two opponent conditions in Chile than in Argentina, given that Chile does not share Argentina's fraught history with the UK. Second, we drop the "no opponent" condition to preserve statistical power, resulting in a 2 $\times$ 2 factorialized design.

[12] https://www.vanderbilt.edu/lapop/ab2018/AB2018-V12.0-Eng_190131_W.pdf

Sample characteristics and balance across treatment groups on key covariates in Chile are shown in Table A2 in Appendix A.1. As in Argentina, the sample has a higher proportion of highly educated people than Chile's general population, and a somewhat higher proportion of female respondents. Across treatment groups, age and ideology have the greatest imbalances. Controlling for these characteristics does not substantively alter our main findings. As above, our regression analysis also controls for race (binary White/non-White), political ideology (10-point left-right scale from LAPOP's 2018 questionnaire, same as in Argentina), and the following dispositional attitudes: international trust, social trust, and militant assertiveness (Kertzer and McGraw, 2012), and national identification (Huddy and Khatib, 2007).

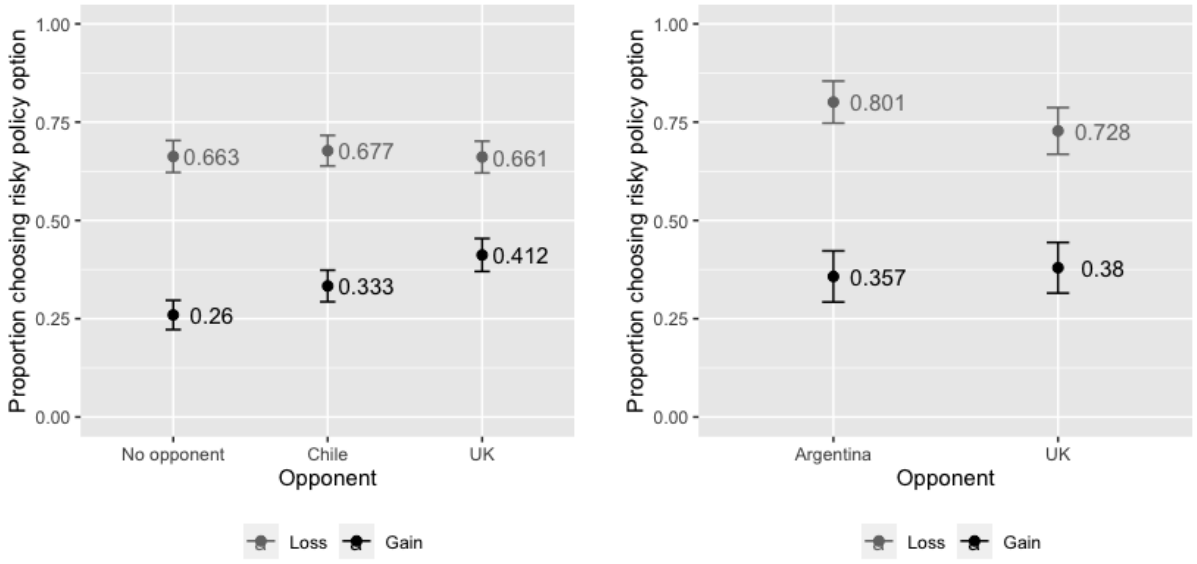
Results

Figure 4 summarizes our results from both samples. We find strong support for Hypothesis 1 in both Argentina and Chile. In Argentina, loss framing raises the probability that a respondent chooses the risky option over the certain option by over 40 percentage points in the baseline condition where no specific opponent was mentioned. Against Hypothesis 2, however, the framing effect *shrinks* when specific opponents are named in the prompt, but remains robust: loss framing induces a 34 percentage point increase in risk acceptance when the opponent is Chile and a 25 percentage point increase when the opponent is the United Kingdom. The reduction of the framing effect is driven entirely by increased risk acceptance in the gain frame but not in the loss frame. In the gain frame, respondents are 7.4 percentage points more likely to choose the risky option when the opponent is Chile relative to the no-opponent baseline ($p = 0.009$), and an additional 7.9 percentage points more likely when the opponent is the UK ($p = 0.009$).[13] In the loss frame, however, risk acceptance remains constant at just under 70 percent—a "ceiling" on risk acceptance that is not unusual in the experimental literature on prospect theory.[14] Hypothesis 3 therefore receives partial support: whereas we expected the hostile opponent to increase risk acceptance relative to benign opponents or no opponent across framing conditions, we find that this effect holds only in the gain frame.

In Chile, we find framing effects of similar magnitude: around 44 percentage points when the opponent is Argentina and 35 percentage points when the opponent is the UK. Notably, the opponent manipulation did not produce any statistically significant changes to the outcome. While the drop in loss-framed risk acceptance induced by replacing Argentina with the UK as the opponent approaches statistical significance

[13] A linear hypothesis test shows that these two differences are not significantly different from each other ($p = 0.922$).

[14] We thank REDACTED and REDACTED for bringing this meta-finding to our attention.



(A) Argentina sample

(B) Chile sample

FIGURE 4: Proportion choosing risky policy option by treatment group with 95% confidence intervals

($p = 0.076$), overall this finding accords with our pre-registered expectation that Chileans would react more similarly to the UK and Argentina in contrast to Argentines' reactions to the UK and Chile.

These results provide strong evidence that framing can affect risk preferences in territorial disputes: gain-framing tends to make people risk-averse, whereas loss-framing makes them risk-acceptant. The effect sizes we recover are comparable to those observed in previous framing studies, including the 50 percentage-point effect found in Tversky and Kahneman's (1981, 453) original "disease problem" experiment, and 28 and 22 percentage points in Reyna and Brainerd's (1991, 255) and Druckman and McDermott's (2008, 309) replications of the same experiment, respectively. The identity of the opponent can also influence risk preferences, but against our expectations we find that hostile opponents tend to *attenuate* framing effects rather than exaggerate them. We observe that the expected increase in risk acceptance due to the presence of a hostile opponent occurs only in gain frame and not in loss frame, perhaps due to ceiling effects in the loss condition. As expected given the relatively recent history of conflict between Argentina and the UK, we find that these effects are stronger in Argentina than in Chile. Nevertheless, our results show that framing effects remain robust no matter whom we name as the opponent in the prompt.[15]

[15] All formal statistical tests of our results are presented in Tables A3 and A4 in the appendix, and Appendix A.3 shows that the results are robust to controlling for demographic and dispositional covariates using OLS and logit regressions.

Experiment 2: Historical ownership narratives

Building on our results from Argentina, we field a second experiment in Chile ($N = 3{,}163$)[16] to test whether historical ownership narratives produce framing effects (Hypothesis 4). We add two elements to the instrument from the first experiment. First, we add and vary historical ownership language in the prompt. Because we are asking respondents about a specific real-life case, we do not manipulate its underlying facts, which would require us to engage in deception.[17] Instead, we restrict ourselves to framing Chile's claim as historical or non-historical by highlighting different facets of its history:

- (**Historical**) *Chile's claim to this territory is based on treaties dating back hundreds of years.*
- (**Not historical**) *This territory was not officially claimed by Chile for most of its history.*

The historical ownership language refers to the Chilean government's longstanding policy of basing the legitimacy of its Antarctic claims on colonial-era treaties dating as far back as the 1494 Treaty of Tordesillas (Peterson, 1980). The non-historical language highlights that Chile did not delimit its territorial claim in Antarctica until a 1940 presidential proclamation, 130 years after independence (Wilson, 1964; Hassner, 2006). We believe this approach is more true-to-life than the manipulation of historical "fact" in a hypothetical scenario because political elites and other "opinion leaders" generally have a limited set of historical facts to draw on when legitimating territorial claims.

The second addition is an assertion about the material value of the disputed territory:

- (**High-value condition**) *There are valuable mineral and energy deposits in this territory.*
- (**Low-value condition**) *The territory has little economic value due to its extreme natural conditions.*

We include this language to control for the possibility that respondents may try to infer something about the value of the territory from the other treatments (e.g. "Chile would not claim this territory for so long if it were not valuable"). The first statement refers to the confirmed presence of hydrocarbons off the Antarctic coast, as well as the likely presence of large mineral deposits on the continent itself. The second statement emphasizes the economic infeasibility of exploiting these resources, even leaving aside the Antarctic Treaty System's legal prohibitions against the commercial exploitation of Antarctica (van der Watt, 2021).

These two experimental manipulations, plus the original gain/loss framing manipulation for the two policy options, are fully cross-randomized in a $2 \times 2 \times 2$ design.[18] Although an ideal test of histori-

[16] This experiment was conducted simultaneously with the replication of Experiment 1 described above.
[17] Previous studies have used hypothetical scenarios (Fang and Li, 2020; Fang et al., 2022).
[18] The full text of this experiment is reproduced in Appendix B.2.

cal ownership narratives as loss frames would examine how such narratives affect choices between two neutrally-framed prospects, our efforts to devise neutral frames brought us to Boettcher's conclusion (2004, 351) that it is "difficult to imagine prospects directly described in a truly neutral manner."[19] Therefore, we cross our historical ownership treatment with direct gain/loss framing in keeping with our conceptualization of historical ownership narratives as indirect frames. We hold Argentina constant as the opponent in all conditions to preserve statistical power. Table B1 in Appendix B summarizes the survey sample's demographic makeup on several key covariates, as well as balance across treatment groups. As in the Experiment 1 sample, highly educated respondents are over-represented. Age and ideology are the most imbalanced covariates across treatment groups, but controlling for them does not substantively change our findings.

Results

Figure 5 shows the sample means for each treatment cell of the historical ownership experiment. The main gain/loss framing effect remains robust across all treatment conditions at similar magnitudes to the previous experiments, again consistent with Hypothesis 1. Historical ownership appears to influence risk preferences slightly, but only in the gain-frame condition: it raises risk acceptance when the territory is portrayed as having low value, but appears to *decrease* risk acceptance when the territory has high value.

To assess the statistical significance of these effects, we turn to regression analysis. We run a logit regression with all three treatment arms fully interacted as well as demographic and dispositional controls, and then plot the marginal effect of the historical ownership treatment across different combinations of the other treatment arms in Figure 6. (We report the full regression results in Appendix B, Table B2.) The historical ownership treatment effect is strongest when the outcomes are framed as gains and the perceived economic value is low, but even here it is only weakly significant ($p = 0.077$). This effect disappears entirely in the high economic value condition, which accords with research showing that efforts to rally support for military action to seize disputed territory are less effective when the territory holds economic value (Lee, 2023). There is little evidence of historical ownership narratives having an effect in loss frame. In fact, a likelihood ratio test is unable to reject the null hypothesis that neither the historical ownership nor economic value treatments add explanatory power to the model ($p = 0.38$). As a result, Hypothesis 4 is not supported.

[19] Druckman (2001) proposes presenting both gain and loss frames simultaneously to estimate an "unadulterated" baseline level of risk acceptance, but validating this approach for our context and issue area is a formidable endeavor worthy of its own study.

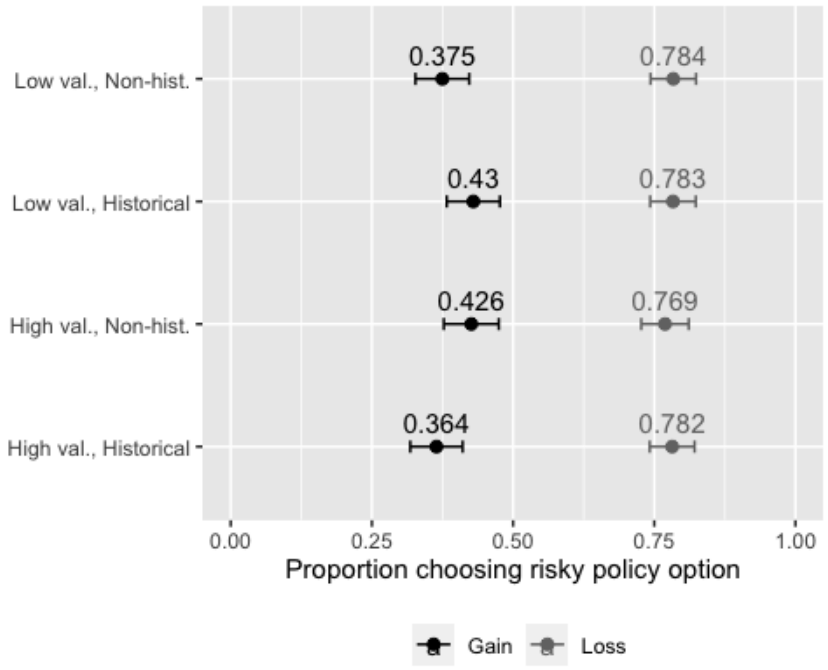


FIGURE 5: Proportion choosing risky policy option by treatment group with 95% confidence intervals

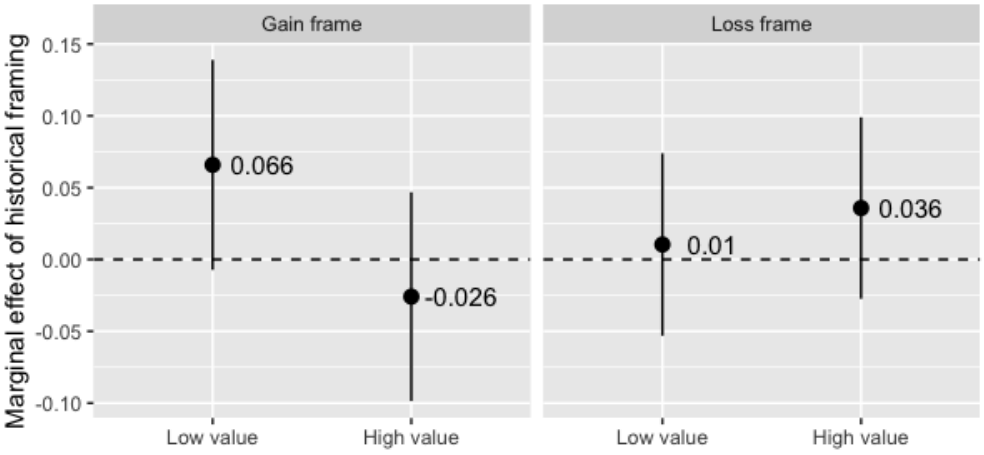


FIGURE 6: Marginal effects of historical ownership on risk acceptance with 95% confidence intervals

Alternative DV: Perceived ownership

Is our (mostly) null finding on historical ownership simply due to a weak treatment? After all, we can hardly argue that Chile's historical claims to Antarctica carry the same potency as, for example, Argentina's claims to the Malvinas/Falklands. Nevertheless, we find that our historical ownership treatment does, in fact, move an alternative outcome measure: perceived ownership of the disputed territory. At the end of our Chile survey, we present the Antarctica prompt again, including the historical ownership and value framing treatments, but instead of having respondents choose between two policies, we ask: "Based on this information, do you consider Chile to be the owner of the disputed territory in Antarctica?" Respondents may answer "yes," "no," or "not sure."[20] We include this question as a simple workaround to a perennial challenge of studying prospect theory: respondents' reference points are generally inferred from their choices *ex-post* rather than directly observed. Our intention was to isolate any effect that the historical ownership and value framing manipulations had on respondents' reference points, independently of the framing of outcomes as gains or losses (which we do not repeat in the prompt for this question). In Figure 7, we see that the historical ownership treatment raises the likelihood that a respondent perceives Chile to already own the disputed Antarctic territory by approximately 6 to 7 percentage points. Regression results in Appendix B.4 show that this effect is robust to controlling for demographic and dispositional covariates.

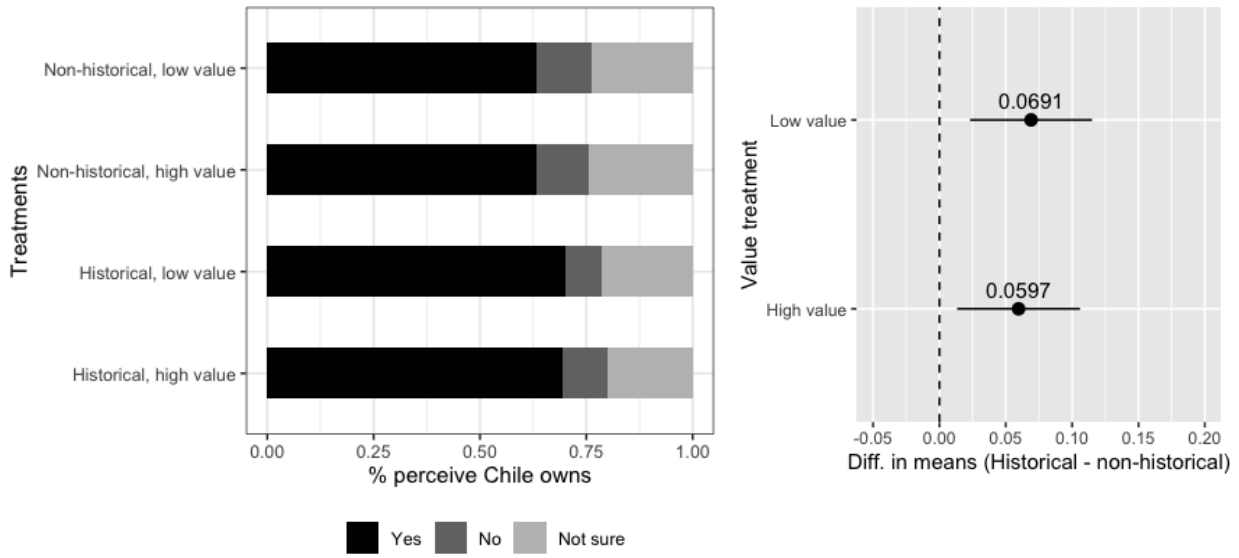


FIGURE 7: Rates of perceived ownership by treatment condition (left) and difference-in-means results for share of "Yes" responses by value framing condition (right).

[20]More specifically, the outcome variable is a binarized version of the perceived ownership measure that takes a value of 1 if the respondent answers "yes" and 0 otherwise.

These results show that our initial assumption about the congruence between perceived ownership and the reference point is incorrect. Historical ownership does not significantly affect risk preferences, whereas loss and gain framing do. At the same time, historical ownership narratives have an effect when respondents are asked directly about ownership. We infer that the historical ownership treatment does not move respondents' reference points in the classical prospect theoretic sense, even though it strengthens perceptions of ownership. Additional analysis in Appendix B.4.2 shows just how complex the relationship between ownership and reference points may be. Superficially, our measures of perceived ownership and risk attitudes are highly correlated: respondents who believe that Chile already owns the disputed Antarctic territory are much more likely to choose the risky gamble. In Table B4, however, we show that *exogenous* variation in perceived ownership induced by our historical ownership manipulation has no effect on risk attitudes. We provisionally conclude that perceived ownership does not necessarily drive risk preferences, and we leave the task of fully disentangling these relationships to future research.

Robustness check: Respondents' interpretation of risky policy option

We do not specify the type of policy that the risky option represents in any of our experiments. We deliberately keep the risky policy option vague to isolate the measurement of risk attitudes and minimize the degree to which responses reflect attitudes about specific policy measures, such as the appropriateness of military action or international arbitration. This approach, however, leaves the specific content of the risky policy open to respondents' interpretation and presents an external validity challenge as leaders hardly ever describe policies so vaguely in real life. Although research suggests that political scientists tend to worry more about abstraction in experimental designs than is warranted (Brutger et al., 2023), we consider two possible ways that policy interpretation might threaten our inferences.

First, our treatments might prime respondents to think of certain policies and not others when considering the risky option, which may in turn drive policy choice rather than capturing changes in risk attitudes. An ideal way to foreclose this possibility would have been to run additional treatment conditions that label the risky policy option specifically as military action, international arbitration, etc. Given limited sample size, however, we take a different approach: after respondents complete the experiment, we ask them what concrete policy they had in mind (if any) for the risky option:

In the last question, as you chose between Option A and Option B, what kind of policy did you

imagine Option B to be? (Select all that apply)

- *Military action*
- *Referral of the dispute to an international court*
- *Renegotiating the agreement*
- *Waiting for the other side to concede*
- *I did not have any particular policy in mind*
- *Other*

This measure is imperfect: we cannot measure how sensitive responses are to the answer options offered, nor the degree to which responses are driven by post-rationalization rather than accurate introspection. Nevertheless, this exercise offers some insight into how respondents construed the risky option, allowing us to measure how our treatments may have influenced those interpretations and how those interpretations, in turn, drove policy choice. In Appendix C.2, we use mediation analysis (Tingley et al., 2014) to show that policy interpretation had a modest role at best in explaining the treatment effects that we observe. While there is some evidence that our treatments influenced how the risky policy was interpreted, these effects do not account for any significant share of the treatment effects we observe.

Second, our treatment effects may be conditional on policy interpretation: do the framing and opponent effects work equally well depending on how respondents interpret the risky policy? This possibility does not threaten the validity of our overall inferences, but it may impose additional scope conditions. In Appendix C.3, we show that strong framing effects generally hold across policy interpretations, though they fluctuate in magnitude. Policy interpretation interacts with the opponent effect more dramatically: in both Argentina and Chile, respondents who view the risky option as military action are less likely to choose it against the UK than against the other opponent, but only when assigned to loss frame.[21] These results should be interpreted with due caution given that we did not experimentally manipulate policy interpretation; indeed, the impact of policy specificity on framing effects may be a fruitful line of future research. We do not find heterogeneity across policy interpretations in the historical ownership treatment effect for Experiment 2.

Risk acceptance and support for military action

Another threat to external validity is the relationship between risk attitudes and support for military action. Scholars have grown accustomed to modeling war as a "costly lottery," and the assumption that war

[21] In Argentina, the framing effect is reduced as a result but remains robust, whereas in Chile, this effect is strong enough to nearly nullify the framing effect (Appendix C.3, Figure C2).

is inherently more risky than non-violent measures underpins all efforts to study the implications of prospect theory for conflict (Kahneman and Renshon, 2007). It is unclear, however, whether the general population has internalized these same assumptions. Having found that loss framing raises risk acceptance, can we infer that loss framing raises support for military action?

To probe this question, we include the following question in our Chile survey after the main experimental prompt: "Would you support the use of military force to resolve this dispute?" to which respondents can answer either "Yes" or "No." (In the following analysis, we pool data from the two experiments in Chile to focus on the impact of loss framing, given that we do not find significant effects on the other treatment arms—opponents, historical ownership, and economic value). In Model 1 of Table 1 we establish that this measure strongly correlates with risk acceptance. To isolate how the exogenous manipulation of risk acceptance can affect support for military action, we instrument risk acceptance with our loss framing treatment in Model 2 and find an attenuated but marginally significant relationship (p = 0.052). Here, however, the respondent's interpretation of the risky option matters greatly. In Model 3, we interact the instrumented risk acceptance measure with whether or not the respondent understood the risky policy to mean military action. We find that risk acceptance has no effect on support for military action among people who do not understand the risky option in the experiment to mean military action, but people who do interpret the risky option this way and who choose the risky option as a result of loss framing are 53 percentage points more likely to support military action (summing the baseline loss frame coefficient with the interaction effect).

Does this mean that, had we labeled the risky option specifically as military action, loss framing would have increased support for military action across the board? We cannot answer this question with certainty since we do not experimentally manipulate the interpretation of the risky option. As an initial cut, however, we show in Model 4 that the result is robust to the addition of an interaction term between the instrumented risk acceptance measure and militant assertiveness, a measure of general hawkishness that strongly predicts interpreting the risky policy as military action (see Tables C3 and C4 in Appendix C). In Appendix D, we obtain the same results controlling for other possible drivers of policy interpretation (Table D1). While far from a perfect substitute for experimental manipulation, these analyses give us greater confidence that labeling the risky option as military action would yield similar results. Indeed, other studies have recovered framing effects while labeling the risky option explicitly as military escalation (Berejikian and Zwald, 2020).

Our results caution against directly inferring foreign policy preferences from risk preferences. The relationship between risk attitudes and support for military action is specific rather than general: risk acceptance

TABLE 1: Risk acceptance on support for military action, OLS and IV results. Models 2 through 4 use loss framing as the instrumental variable for risk acceptance.

	Dependent variable:			
	Support military action			
	OLS	*IV*	*IV*	*IV*
	(1)	(2)	(3)	(4)
Risky option	0.086***	0.070*	0.032	−0.037
	(0.014)	(0.036)	(0.036)	(0.063)
Risky option x Mil. action			0.503***	0.487***
			(0.134)	(0.133)
Risky option x Mil. assert.				0.205
				(0.152)
Mil. action			−0.030	−0.031
			(0.076)	(0.077)
Mil. assertiveness	0.674***	0.677***	0.572***	0.448***
	(0.031)	(0.031)	(0.034)	(0.099)
Age	−0.001*	−0.001*	−0.0005	−0.001
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Female	−0.044***	−0.045***	−0.025*	−0.026*
	(0.014)	(0.015)	(0.014)	(0.015)
White	0.023	0.023	0.029*	0.030**
	(0.015)	(0.015)	(0.015)	(0.015)
Higher ed.	0.006	0.007	0.005	0.005
	(0.015)	(0.015)	(0.015)	(0.015)
Santiago resident	0.018	0.017	0.019	0.020
	(0.014)	(0.014)	(0.014)	(0.014)
Ideology	0.144***	0.145***	0.130***	0.132***
	(0.028)	(0.028)	(0.028)	(0.028)
National identif.	0.200***	0.202***	0.172***	0.176***
	(0.033)	(0.034)	(0.033)	(0.033)
Int'l trust	−0.059***	−0.060***	−0.057***	−0.060***
	(0.016)	(0.016)	(0.016)	(0.016)
Social trust	0.018	0.018	0.025	0.024
	(0.023)	(0.023)	(0.022)	(0.022)
Constant	−0.122***	−0.115***	−0.098***	−0.059
	(0.033)	(0.037)	(0.037)	(0.048)
Observations	3,273	3,273	3,240	3,240
R^2	0.262	0.262	0.304	0.301
Note:				*p<0.1; **p<0.05; ***p<0.01

drives support for military action only when people mentally connect risk-taking with military action. This link is hardly automatic: only 13 percent of respondents in our sample reported thinking of the risky option as military action. For those who *do* make the connection, however, exogenous manipulations of risk preferences can influence support for the use of force, as international relations scholars have come to expect.

Discussion

Our findings are both reassuring and sobering for the power of prospect theory to explain territorial disputes and international relations phenomena more broadly. We show that prospect theory does indeed replicate in the context of territorial disputes in a literal sense: people are more willing to gamble on uncertain territorial dispute outcomes when the outcomes are framed as losses rather than gains. Across our two experiments, we also show that this effect is robust to a number of contextual factors, including the identity of the opponent and the perceived material value of the territory.

Against our expectations, we find that hostile opponents attenuate rather than amplify framing effects. Although it may initially seem counterintuitive that loss framing is less potent when the losses additionally benefit an adversary, we note that this attenuation is driven by increased "baseline" risk acceptance in the domain of gains rather than decreased risk acceptance in losses. Indeed, these results are consistent with previous work on emotions and framing which finds that anger can simultaneously increase risk acceptance and reduce sensitivity to framing (Druckman and McDermott, 2008).[22] An equivalent restatement of our finding is that hostile opponents make gain-framing less effective at *decreasing* risk acceptance. Seen in this light, the implications of our results are both intuitive and concrete: hostile opponents make it more difficult, but not impossible, for gain-framing to raise support for peaceful settlements.

We also find clear limits to how prospect theory can be applied to territorial disputes. Against longstanding scholarly speculation, we do not find evidence that narratives of historical ownership produce prospect-theoretic framing effects on their own. These results highlight the need for scholars to specify clearer scope conditions when applying prospect theory to the study of conflict. While existing work has often invoked prospect theory to explain how historical narratives motivate enduring conflict, our results suggest that these narratives do not amount to "framing" unless they are used to characterize the outcomes of a specific decision. Of course, we have conducted only one test and there is no telling if larger effects may be found in a different setting or on a different issue area. Nevertheless, our finding is consistent with previous research finding weak support for indirect framing effects (Boettcher III, 2004). In retrospect, it may not be surprising that loss framing and historical narratives occupy different parts of human psychology. Although emotion may condition the effects of prospect theory (as outlined in our discussion of hostile opponents), prospect theory itself operates through cognition, not emotion. The core message of prospect theory is that decision-

[22] In Appendix E, we use the Chile survey to test a number of pre-registered secondary hypotheses about the role of emotions in explaining the opponent effect, but our results are inconclusive given the overall weakness of opponent effects in Chile.

making is driven by the cognition of gains and losses, which is in turn highly susceptible to manipulation via framing. Nationalism and historical narratives, on the other hand, are designed to elicit *feelings* of loss and past grievance, which derive their power from their stickiness. Our findings caution against applying prospect theory to stimuli that evoke a global sense of loss without describing specific outcomes as losses.

Several interesting implications follow. First, as in the case of hostile opponents, our results suggest that gain-framing may remain effective as a means of *reducing* audience risk acceptance even when historical ownership narratives over that territory are prevalent. Indeed, different political communicators may advance competing frames: pro-compromise advocates may employ gain frames while pro-escalation voices advance loss frames, and future research could examine how historical narratives might lead audiences to favor one frame over another. Second, historical narratives may yet affect attitudes toward conflict through channels other than risk preferences. (While we do not find that historical narratives affect support for military action in our study, we do not optimize our experimental design to test for this relationship given our focus on risk preferences.) For example, we find that historical narratives *are* effective in strengthening a sense of prior ownership. Our results therefore leave open the possibility that historical narratives elicit endowment effects—that is, greater willingness to pay higher costs to hold onto what one already owns rather than to gain something new—leading to greater tolerance for conflict.[23]

Another scope condition to which our findings call attention is the relationship between risk attitudes and support for military action. For people who do not cognitively link these two concepts, framing-induced risk acceptance may not necessarily serve as a "hawkish bias" (Kahneman and Renshon, 2007). Leaders certainly do their best to avoid characterizing their military endeavors as "risky." Kertzer (2016) has argued that "doves" and "hawks" differ in how their risk preferences inform attitudes toward the use of force: doves consider fighting to be risky, whereas hawks consider it to be the "better safe than sorry" strategy. If the characterization of military action as "risky" is both contingent and contested, then much work remains to be done in identifying the conditions under which risk preferences can explain attitudes toward conflict.

We want to stress that, even within these scope conditions, the fact that framing effects hold in the realm of territorial disputes has potentially far-reaching implications. Given how much the framing effect varies across problem domains, it was far from a foregone conclusion that we would replicate framing effects—let alone of such magnitude—for territorial disputes. And while the relevant scope conditions may be narrower

[23] Alternatively, historical narratives may affect support for the use of force by increasing the perceived appropriateness or legitimacy of using military force. If this were the case, we would expect the historical ownership narrative in Experiment 2 to increase support for the use of force without increasing risk acceptance. We do not, however, see this in our data.

than IR scholars have recognized up to now, it is not difficult to imagine situations in which these conditions are met. For example, framing may well influence how Ukrainians decide whether to cut a deal with Russia or continue to fight for the chance to recover all Russian-occupied land, even while risking the loss of more territory. We motivated this project by arguing that postwar norms around territorial integrity and self-defense incentivize leaders to frame decisions about territorial claims in loss terms. Although we have not tested this proposition here, if it is correct then our findings suggest that postwar norms have made it harder, not easier, to resolve territorial disputes. Most accounts of the territorial integrity norm in IR emphasize its pacifying effects in making territorial disputes harder to start (Zacher, 2001). Our results, however, are more in line with scholarship showing how territorial integrity concerns also make such disputes more difficult to end, especially among postcolonial states (Miller, 2013; Abraham, 2020). These two faces of "territorial integrity" have yet to be reconciled in IR scholarship. Indeed, historical data (Frederick et al., 2017) suggests that territorial disputes are both starting and ending at slower rates, and therefore enduring for ever longer periods of time, since the end of World War II (see Appendix F). While many potential explanations may account for these trends, our experimental findings point toward prospect theory and loss-framing as one possible mechanism linking international norms to the hardening of disputes over time.

Conclusion

We build on well-established findings in prospect theory to explain how framing effects could influence territorial conflict. If citizens and leaders on one or both sides of a dispute perceive themselves to be in the domain of losses and become risk-acceptant, a peaceful bargain may not be possible, as the terms required for a peaceful deal exceed the value of the issue at stake. With survey experiments in Argentina and Chile—involving a territorial dispute in Antarctica—we show that citizens on both sides of an actual territorial dispute are indeed susceptible to manipulation by loss framing. We find at least three other striking results. First, the identity of the opponent matters: more hostile opponents tend to raise risk acceptance when the dispute is framed in terms of gains, but risk acceptance is not raised further in the domain of losses, leading to an attenuated framing effect. Second, despite extensive scholarly speculation about the framing implications of historical narratives, we find that historical ownership narratives have little impact on risk preferences over disputed territory, but do induce a sense of current ownership. Third, risk acceptance increases willingness to use force among respondents who understand the risky option to mean military

action, but not among respondents who interpret the risky option to mean something else. While we have recovered strong evidence that framing can play an influential role in territorial disputes, we have also shown some of the limitations of prospect theory as an explanatory framework for conflict: manipulations that do not directly frame specific decision outcomes as gains or losses are unlikely to produce framing effects, and risk acceptance does not translate automatically into greater appetite for conflict.[24]

This study advances the territorial conflict literature by providing novel evidence on the sources of territorial dispute persistence. Although scholars have long speculated that loss framing contributes to the intractability of territorial disputes, our study is the first to subject this proposition to rigorous empirical testing. Our findings also speak to long-running debates about the general applicability of prospect theory. Research by Gal and Rucker (2018a) (see also Alempaki et al. (2019)) casts doubt on the efficacy of framing effects when the stakes are low; for individual citizens in both Argentina and Chile, the material stakes of the dispute are negligible. That we find framing effects in our experiments highlights the perhaps surprising importance of territorial disputes for ordinary individuals. The low material value of the stakes may be trumped by the high symbolic value of the dispute. At the same time, our results highlight scope conditions for applying prospect theory to the study of conflict, namely the importance of direct over indirect frames and the contingent relationship between risk attitudes and support for military action.

We suggest several avenues for further research. First, the significant effects of historical ownership narratives on perceived ownership suggest the importance of exploring whether historical narratives affect policy preferences through endowment effects. Second, text-as-data research could explore whether and when politicians actually frame disputed territories in the domain of losses. It might be possible to trace such framings in party platforms,[25] or politicians' speeches (Osnabrügge et al., 2021). Such research could reveal whether framing varies over different pieces of territory and over time. If there is significant variation in the prevalence and intensity of loss framing over time, this variation might serve as a useful real-world, real-time indicator of increasing dangers to international peace and stability. Given the prevalence of territorial disputes, scholars and policymakers should pay closer attention to how public attitudes can be manipulated to become more risk-acceptant via framing effects.

[24] In Appendix G, we also explore a number of pre-registered secondary hypotheses regarding heterogeneous treatment effects.
[25] https://manifesto-project.wzb.eu/.

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Competing interests declaration

The authors have no competing interests to declare.

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Loss Framing and Risk-Taking in Territorial Disputes
Online Appendices

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A Experiment 1

A.1 Covariate balance tables

A.1.1 Argentina

Covariate	Pop. mean	Sample mean	Max. cell mean	Min. cell mean	Std. mean diff.
Age	41.840	41.299	42.093	40.414	0.107
Female	0.504	0.524	0.556	0.495	0.060
White	0.540	0.635	0.678	0.623	0.055
University	0.337	0.526	0.572	0.501	0.071
Buenos Aires	0.462	0.464	0.491	0.453	0.038
Right-wing ideology (1-10)	5.204	5.807	5.989	5.709	0.118

TABLE A1: Covariate balance by treatment condition, Argentina. "Buenos Aires" refers to residents of both the Autonomous City of Buenos Aires and the surrounding province of Buenos Aires. Ideology is measured on a 1 to 10 scale, with 1 indicating most left-wing and 10 indicating right-wing. All target proportions are taken from LAPOP's nationally representative 2018 sample except Buenos Aires residency, which comes from the 2010 Argentine Census.

A.1.2 Chile

Covariate	Pop. mean	Sample mean	Max. cell mean	Min. cell mean	Std. mean diff.
Age	42.380	39.945	40.874	37.961	0.192
Female	0.511	0.560	0.601	0.522	0.079
White	0.437	0.368	0.409	0.343	0.067
Superior ed.	0.298	0.705	0.725	0.680	0.045
Santiago	0.406	0.372	0.385	0.348	0.036
Right-wing ideology (1-10)	5.054	5.189	5.462	4.933	0.215

TABLE A2: Covariate balance by treatment condition, Chile replication. "Santiago" refers to residents of the Santiago Metropolitan Region. Ideology is measured on a 1 to 10 scale, with 1 indicating most left-wing and 10 indicating right-wing. Population means of age and ideology are taken from LAPOP's nationally representative 2018 sample; the other population means come from the 2017 Chilean census.

A.2 Formal tests of main results

A. Framing effect by opponent condition

Quantity	None	Chile	UK
% prefer risky option, gain frame	0.260	0.333	0.412
% prefer risky option, loss frame	0.663	0.677	0.661
Diff. in means (framing effect)	0.403	0.344	0.249
t-statistic	14.076	11.788	8.159
p-value	0.000	0.000	0.000

B. Opponent effects within framing conditions

Quantity	Chile vs. None	UK vs. Chile	UK vs. None
Difference in means, gain frame	0.074	0.079	0.153
t-statistic	2.602	2.610	5.259
p-value	0.009	0.009	0.000
Difference in means, loss frame	0.015	-0.016	-0.001
t-statistic	0.502	-0.548	-0.046
p-value	0.616	0.584	0.963

TABLE A3: Main results, Argentina. Panel A shows t-tests for difference in mean risk acceptance across framing conditions, conducted separately for each opponent condition. All difference-in-means estimates (third row) are statistically significant. Panel B shows that differences in risk acceptance across opponent conditions among gain-framed respondents are statistically significant. In loss frame, however, the identity of the opponent had no discernible effect.

A. Framing effect by opponent condition

Quantity	Argentina	UK
% prefer risky option, gain frame	0.357	0.380
% prefer risky option, loss frame	0.801	0.728
Diff. in means (framing effect)	0.443	0.348
t-statistic	10.243	7.725
p-value	0.000	0.000

B. Opponent effects within framing conditions

Quantity	UK - Argentina
Difference in means, gain frame	0.022
t-statistic	0.471
p-value	0.638
Difference in means, loss frame	-0.073
t-statistic	-1.780
p-value	0.076

TABLE A4: Main results, Chile. Panel A shows t-tests for difference in mean risk acceptance across framing conditions, conducted separately for each opponent condition. All difference-in-means estimates (third row) are statistically significant. Panel B shows that none of the differences in risk acceptance across opponent conditions in either gain frame or loss frame are statistically significant.

A.3 Regression results

Model 1 in Table A5 replicates the findings from Table A3 using a linear probability model that regresses the binary outcome variable (taking a value of 1 if the respondent chooses the risky option) on the framing treatment (taking a value of 1 if the respondent receives a loss-framed prompt), plus binary variables indicating the opponent named in the prompt, as well as interactions between the framing and opponent treatments. In the Argentina sample, presenting the opponent as Chile reduces the framing effect by 5.9 percentage points relative to not naming any opponent, but this effect is not statistically significant at conventional levels. On the other hand, the 15.4 percentage point reduction triggered by the UK (relative to the no-opponent baseline) is statistically significant.

Model 2 adds demographic and dispositional controls to the linear probability model. The addition of controls does not qualitatively change the main treatment effects, though the reduction in the framing effect when Chile is the opponent is now statistically significant. Female and white respondents tend to be more risk-averse, as are those who display higher levels of international trust. On the other hand, respondents displaying high levels of national attachment are considerably more risk-acceptant. Model 3 re-runs Model 2 using a logit specification, which addresses concerns about introducing bias when using OLS to regress binary outcomes on continuous covariates. The results are substantively indistinguishable.

Table A6 shows that the framing effect in Chile holds using OLS regression (Model 1) after controlling for demographic and dispositional covariates (Model 2), as well as in a logit specification (Model 3), all of which again supports Hypothesis 1. The results are not significantly affected by naming the opponent as the UK rather than Argentina.

TABLE A5: Argentina framing experiment main results

	Dependent variable:		
	Prefer risky policy option		
	OLS		*Logit*
	(1)	(2)	(3)
Loss frame	0.403***	0.440***	1.963***
	(0.029)	(0.032)	(0.159)
Chile	0.074**	0.116***	0.585***
	(0.029)	(0.032)	(0.157)
UK	0.153***	0.181***	0.869***
	(0.029)	(0.032)	(0.156)
Loss frame × Chile	−0.059	−0.122***	−0.612***
	(0.041)	(0.045)	(0.217)
Loss frame × UK	−0.154***	−0.183***	−0.881***
	(0.042)	(0.046)	(0.217)
Age		0.002**	0.008**
		(0.001)	(0.003)
Female		−0.067***	−0.314***
		(0.019)	(0.090)
White		−0.043**	−0.201**
		(0.020)	(0.095)
Higher ed.		−0.022	−0.106
		(0.019)	(0.089)
Buenos Aires resid.		0.004	0.016
		(0.019)	(0.089)
Right-wing ideology		0.006	0.023
		(0.036)	(0.170)
National attach.		0.225***	1.052***
		(0.053)	(0.252)
International trust		−0.105***	−0.495***
		(0.028)	(0.132)
Social trust		0.028	0.133
		(0.029)	(0.137)
Intercept	0.260***	0.118**	−1.757***
	(0.021)	(0.053)	(0.254)
Observations	3,058	2,484	2,484
R^2	0.119	0.148	
Akaike Inf. Crit.			3,087.986
Note:			*p<0.1; **p<0.05; ***p<0.01

TABLE A6: Chile framing experiment main results

	Dependent variable:		
	Prefer risky policy option		
	OLS		*Logit*
	(1)	(2)	(3)
Loss frame	0.443***	0.441***	2.118***
	(0.044)	(0.048)	(0.263)
UK	0.022	0.021	0.091
	(0.044)	(0.048)	(0.232)
Loss frame × UK	−0.095	−0.106	−0.572
	(0.062)	(0.068)	(0.356)
Age		0.002	0.008
		(0.001)	(0.006)
Female		−0.091***	−0.475***
		(0.035)	(0.181)
White		−0.055	−0.281
		(0.037)	(0.192)
Higher educ.		0.034	0.188
		(0.038)	(0.194)
Santiago resident		0.009	0.040
		(0.035)	(0.182)
Right-wing ideology		0.128*	0.676*
		(0.069)	(0.359)
National identif.		0.117	0.620
		(0.083)	(0.420)
Militant assert.		0.088	0.436
		(0.077)	(0.392)
International trust		−0.139***	−0.706***
		(0.039)	(0.202)
Social trust		0.094*	0.481*
		(0.055)	(0.286)
Intercept	0.357***	0.196**	−1.477***
	(0.032)	(0.084)	(0.432)
Observations	847	686	686
R^2	0.162	0.216	
Akaike Inf. Crit.			806.547
Note:			*p<0.1; **p<0.05; ***p<0.01

B Experiment 2

B.1 Covariate balance table

Covariate	Pop. mean	Sample mean	Max. cell mean	Min. cell mean	Std. mean diff.
Age	42.380	40.702	42.235	39.440	0.180
Female	0.511	0.573	0.606	0.550	0.056
White	0.437	0.373	0.411	0.337	0.074
Superior ed.	0.298	0.692	0.741	0.660	0.081
Santiago	0.406	0.393	0.434	0.373	0.061
Right-wing ideology (1-10)	5.054	5.304	5.520	5.084	0.176

TABLE B1: Covariate balance by treatment condition, historical ownership experiment. "Santiago" refers to residents of the Santiago Metropolitan Region. Ideology is measured on a 1 to 10 scale, with 1 indicating most left-wing and 10 indicating right-wing. Population means of age and ideology are taken from LAPOP's nationally representative 2018 sample; the other population means come from the 2017 Chilean census.

B.2 Full text of experimental prompt

As you may know, a large part of Antarctica is disputed between Chile and Argentina. [Chile's claim to this territory is based on treaties dating back hundreds of years./This territory was not officially claimed by Chile for most of its history.] [There are valuable mineral and energy deposits in this territory./The territory has little economic value due to its extreme natural conditions.] Imagine that the following two options are being considered to resolve the dispute. Which option do you prefer?

- *Sign an agreement in which Chile [obtains 1/3 / loses 2/3] of the territory disputed with Argentina in Antarctica.*
- *Pursue a policy in which Chile has a 1/3 chance of [obtaining / keeping] the entire territory disputed with Argentina in Antarctica and a 2/3 chance of [obtaining none / losing all] of this territory.*

B.3 Regression analysis of main results

TABLE B2: Model 1 uses an OLS estimator, Model 2 adds demographic and dispositional controls, and Model 3 replicates Model 2 but with a logit estimator. For each of the three models, F-tests (likelihood ratio test for Model 3) are unable to reject the null hypothesis that neither the historical nor the value framing variables add explanatory power ($p = 0.27$ for Model 1; $p = 0.31$ for Model 2; $p = 0.38$ for Model 3).

	Dependent variable:		
	Prefer risky policy option		
	OLS		*Logit*
	(1)	(2)	(3)
Loss frame	0.409^{***}	0.397^{***}	1.790^{***}
	(0.032)	(0.035)	(0.180)
Historical claim	0.055^{*}	0.067^{*}	0.285^{*}
	(0.032)	(0.035)	(0.162)
High value	0.051	0.020	0.089
	(0.032)	(0.035)	(0.166)
Loss frame × Hist. claim	−0.055	−0.056	−0.223
	(0.045)	(0.050)	(0.253)
Loss frame × High value	−0.066	−0.031	−0.146
	(0.046)	(0.050)	(0.254)
Hist. claim × High value	-0.117^{***}	-0.091^{*}	-0.391^{*}
	(0.045)	(0.049)	(0.230)
Loss frame × Hist. claim × High value	0.130^{**}	0.116	0.545
	(0.064)	(0.070)	(0.358)
Age		0.0003	0.001
		(0.001)	(0.003)
Female		-0.057^{***}	-0.286^{***}
		(0.018)	(0.092)
White		−0.017	−0.085
		(0.019)	(0.096)
Higher ed.		0.030	0.156
		(0.019)	(0.097)
Santiago resident		−0.015	−0.076
		(0.018)	(0.091)
Right-wing ideology		0.025	0.126
		(0.036)	(0.182)
National identif.		0.089^{**}	0.443^{**}
		(0.042)	(0.212)
Militant assert.		0.167^{***}	0.849^{***}
		(0.039)	(0.197)
International trust		-0.054^{***}	-0.272^{***}
		(0.020)	(0.101)
Social trust		-0.059^{**}	-0.294^{**}
		(0.029)	(0.145)
Intercept	0.375^{***}	0.296^{***}	-0.937^{***}
	(0.023)	(0.048)	(0.236)
Observations	3,184	2,606	2,606
R^2	0.151	0.182	
Akaike Inf. Crit.			3,072.740
Note:			*p<0.1; **p<0.05; ***p<0.01

B.4 Perceived ownership regression results

B.4.1 Regression evidence for historical ownership and perceived ownership

Model 1 regresses perceived ownership on historical ownership and perceived value, replicating findings from 7. The non-significant interaction coefficient in Model 2 suggests that the effect of historical ownership on perceived ownership holds regardless of the perceived value of the territory. Model 3 verifies that the loss framing treatment assignment from the earlier experiment had no effect on perceived ownership. Model 4 replicates Model 2 using a logit specification, with no substantive changes in the results.

TABLE B3: Effect of treatments on perceived ownership (Logit models)

	Dependent variable:			
	Perceived ownership			
	OLS			*Logit*
	(1)	(2)	(3)	(4)
Historical ownership	0.071***	0.065***	0.071***	0.330***
	(0.017)	(0.025)	(0.017)	(0.125)
High value	0.002	−0.004	0.002	−0.023
	(0.017)	(0.025)	(0.017)	(0.124)
Historical × High value		0.012		0.067
		(0.035)		(0.177)
Loss frame			0.012	
			(0.017)	
Age	0.002***	0.002***	0.002***	0.009***
	(0.001)	(0.001)	(0.001)	(0.003)
Female	0.006	0.006	0.006	0.009
	(0.018)	(0.018)	(0.018)	(0.092)
White	0.018	0.018	0.018	0.084
	(0.019)	(0.019)	(0.019)	(0.097)
Higher ed.	0.047**	0.047**	0.048**	0.243**
	(0.019)	(0.019)	(0.019)	(0.097)
Santiago	−0.001	−0.001	−0.001	−0.010
	(0.018)	(0.018)	(0.018)	(0.091)
Right-wing ideology	0.088**	0.088**	0.088**	0.470**
	(0.036)	(0.036)	(0.036)	(0.185)
National identif.	0.521***	0.521***	0.520***	2.546***
	(0.042)	(0.042)	(0.042)	(0.216)
Militant assert.	0.131***	0.131***	0.130***	0.741***
	(0.038)	(0.038)	(0.038)	(0.200)
International trust	−0.042**	−0.042**	−0.043**	−0.192*
	(0.020)	(0.020)	(0.020)	(0.099)
Social trust	−0.075***	−0.075***	−0.074***	−0.385***
	(0.028)	(0.028)	(0.028)	(0.145)
Intercept	0.139***	0.142***	0.134***	−1.879***
	(0.043)	(0.043)	(0.043)	(0.224)
Observations	2,631	2,631	2,631	2,631
R^2	0.115	0.115	0.115	
Akaike Inf. Crit.				3,039.672
Note:				*p<0.1; **p<0.05; ***p<0.01

B.4.2 Perceived ownership and risk attitudes

In Table B4 we regress the choice of the risky policy option in the main experiment on perceived ownership in this alternative setup. Model 1 shows a robust bivariate correlation between perceived ownership and risk acceptance. This relationship holds after controlling for the treatment manipulations and demographic

and dispositional covariates in Model 2. We then seek to isolate the effect of our exogenous manipulation of perceived ownership by instrumenting perceived ownership on our historical ownership treatment in Models 3 (linear) and 4 (probit). In these specifications, the coefficient on perceived ownership is statistically indistinguishable from 0.

TABLE B4: Effect of perceived ownership on risk preferences

	Dependent variable:			
	Prefer risky policy option			
	OLS		*IV*	*IV Probit*
	(1)	(2)	(3)	(4)
Perceived ownership	0.135***	0.094***	0.306	0.947
	(0.018)	(0.020)	(0.251)	(0.764)
Historical ownership		0.015		
		(0.018)		
Loss frame		0.382***	0.380***	1.062***
		(0.018)	(0.018)	(0.056)
High value		−0.013	−0.013	−0.036
		(0.018)	(0.018)	(0.054)
Age		0.0001	−0.0003	−0.001
		(0.001)	(0.001)	(0.002)
Female		−0.059***	−0.060***	−0.180***
		(0.018)	(0.019)	(0.057)
White		−0.019	−0.023	−0.074
		(0.019)	(0.020)	(0.061)
Higher ed.		0.028	0.018	0.059
		(0.019)	(0.023)	(0.069)
Santiago		−0.018	−0.017	−0.056
		(0.018)	(0.018)	(0.056)
Right-wing ideology		0.016	−0.002	−0.004
		(0.036)	(0.041)	(0.126)
National identif.		0.036	−0.072	−0.232
		(0.043)	(0.135)	(0.411)
Militant assert.		0.157***	0.129**	0.391**
		(0.039)	(0.052)	(0.160)
International trust		−0.050**	−0.040*	−0.119*
		(0.020)	(0.023)	(0.070)
Social trust		−0.055*	−0.040	−0.128
		(0.029)	(0.035)	(0.106)
Intercept	0.497***	0.303***	0.274***	−0.653***
	(0.015)	(0.044)	(0.062)	(0.190)
Observations	3,163	2,591	2,591	2,591
Note:				*p<0.1; **p<0.05; ***p<0.01

C Does it matter what type of risky policy people had in mind?

C.1 Treatments somewhat influence policy interpretation

We first assess whether our treatments had an effect on policy interpretation by running logit regressions to predict substantive policy interpretation (military action, international court, renegotiation, and waiting) using the experimental treatments and a set of demographic and dispositional covariates for each subexperiment (Tables C2, C3, and C4). Since the interacted treatments make individual coefficients difficult to interpret, we estimate the substantive significance of each treatment on policy interpretation by conducting likelihood ratio tests (Table C1) that compare the explanatory value of each model to an alternative specification that excludes the treatment variable and all of its associated interactions. We see that loss framing has some influence on interpreting the policy as military action or renegotiation in Argentina, but has little influence on policy interpretation in either of the Chile experiments. The opponent effects are somewhat more extensive: in Argentina, both opponents affected respondents' likelihood of interpreting the policy as military action, and the UK also affected whether the policy would be seen as international arbitration. In Chile, the UK changed interpretation of the policy as military action relative to Argentina being portrayed as the opponent. In Experiment 2 in Chile, neither the historical nor value framing conditions had any discernible impact on policy interpretation.

A. Argentina

	Military action	Int'l court	Renegotiate	Wait
Loss frame	0.020**	0.210	0.021**	0.525
Chile	0.075*	0.796	0.455	0.936
UK	0.015**	0.003***	0.679	0.116

B. Chile, Experiment 1

	Military action	Int'l court	Renegotiate	Wait
Loss frame	0.330	0.918	0.083*	0.166
UK	0.008***	0.963	0.353	0.898

C. Chile, Experiment 2

	Military action	Int'l court	Renegotiate	Wait
Loss frame	0.209	0.507	0.188	0.783
Historical	0.544	0.711	0.226	0.990
High value	0.167	0.618	0.603	0.739

TABLE C1: Likelihood ratio (χ^2) tests corresponding to Tables C2, C3, and C4. Each figure is the p-value testing whether the row variable explains a significant amount of the variation in the column outcome. *p<0.1; **p<0.05; ***p<0.01

TABLE C2: Treatment effects on perception of risky policy option, Argentina (logit)

	Dependent variable:			
	Military action	Int'l court	Renegotiate	Wait
	(1)	(2)	(3)	(4)
Loss frame	-0.598^{***}	0.242^{*}	0.273^{*}	-0.316
	(0.193)	(0.146)	(0.144)	(0.254)
Chile	-0.380^{**}	0.091	0.136	-0.058
	(0.185)	(0.148)	(0.146)	(0.240)
UK	-0.546^{***}	0.440^{***}	-0.074	0.216
	(0.193)	(0.146)	(0.148)	(0.229)
Loss frame × Chile	0.574^{**}	-0.051	-0.019	0.130
	(0.274)	(0.205)	(0.202)	(0.356)
Loss frame × UK	0.631^{**}	-0.231	-0.029	0.241
	(0.284)	(0.203)	(0.205)	(0.338)
Age	-0.045^{***}	0.010^{***}	0.005^{*}	-0.017^{***}
	(0.004)	(0.003)	(0.003)	(0.005)
Female	-0.369^{***}	-0.579^{***}	-0.008	-0.342^{**}
	(0.117)	(0.085)	(0.085)	(0.143)
White	0.003	0.097	-0.009	-0.123
	(0.119)	(0.091)	(0.090)	(0.147)
Higher ed.	-0.202^{*}	0.315^{***}	0.181^{**}	-0.111
	(0.115)	(0.085)	(0.085)	(0.142)
Buenos Aires	0.074	-0.058	0.002	-0.029
	(0.117)	(0.085)	(0.085)	(0.143)
Right-wing ideology	0.485^{**}	0.055	-0.089	0.146
	(0.221)	(0.161)	(0.161)	(0.269)
National attachment	0.429	0.478^{**}	0.060	0.368
	(0.321)	(0.239)	(0.238)	(0.393)
International trust	-0.109	-0.075	0.103	0.131
	(0.179)	(0.126)	(0.125)	(0.211)
Social trust	-0.636^{***}	0.095	0.247^{*}	-0.452^{**}
	(0.193)	(0.130)	(0.129)	(0.230)
Intercept	0.337	-1.345^{***}	-1.103^{***}	-1.471^{***}
	(0.315)	(0.239)	(0.237)	(0.387)
Observations	2,553	2,553	2,553	2,553
Akaike Inf. Crit.	2,056.064	3,336.417	3,350.534	1,550.035

Note: *p<0.1; **p<0.05; ***p<0.01

TABLE C3: Experiment 1 treatment effects on perception of risky policy option, Chile (logit)

	Dependent variable:			
	Military action	Int'l court	Renegotiate	Wait
	(1)	(2)	(3)	(4)
Loss frame	−0.014	−0.058	0.051	0.551
	(0.280)	(0.231)	(0.239)	(0.390)
UK	−0.414	0.052	−0.187	0.170
	(0.298)	(0.228)	(0.242)	(0.414)
Loss frame × UK	−0.504	−0.016	0.472	−0.099
	(0.451)	(0.324)	(0.337)	(0.536)
Age	−0.015*	0.004	0.009	−0.006
	(0.008)	(0.006)	(0.006)	(0.010)
Female	−0.300	−0.619***	0.117	−0.339
	(0.222)	(0.165)	(0.173)	(0.268)
White	0.215	0.143	−0.027	−0.446
	(0.232)	(0.175)	(0.182)	(0.302)
Higher ed.	−0.118	−0.006	−0.070	0.044
	(0.235)	(0.179)	(0.186)	(0.288)
Santiago	0.157	−0.005	−0.009	0.085
	(0.223)	(0.168)	(0.174)	(0.271)
Right-wing ideology	0.317	−0.543	0.718**	−0.499
	(0.439)	(0.333)	(0.339)	(0.540)
National identification	0.795	0.580	−0.075	0.234
	(0.553)	(0.399)	(0.408)	(0.644)
Militant assert.	1.141**	−0.202	−0.742*	0.156
	(0.481)	(0.362)	(0.381)	(0.592)
International trust	0.132	−0.006	0.103	0.047
	(0.252)	(0.188)	(0.193)	(0.306)
Social trust	0.231	−0.070	0.274	−0.398
	(0.350)	(0.261)	(0.269)	(0.432)
Intercept	−1.908***	−0.414	−1.375***	−1.958***
	(0.530)	(0.398)	(0.417)	(0.657)
Observations	685	685	685	685
Akaike Inf. Crit.	589.563	908.796	862.739	451.684

Note: *p<0.1; **p<0.05; ***p<0.01

TABLE C4: Experiment 2 treatment effects on perception of risky policy option, Chile (logit)

	Dependent variable:			
	Military action	Int'l court	Renegotiate	Wait
	(1)	(2)	(3)	(4)
Loss frame	−0.055	−0.220	0.347**	0.123
	(0.246)	(0.166)	(0.174)	(0.248)
Historical	0.121	−0.184	0.364**	0.055
	(0.237)	(0.165)	(0.173)	(0.249)
High value	0.151	−0.164	0.256	−0.002
	(0.239)	(0.165)	(0.176)	(0.255)
Loss frame × Historical	−0.543	0.188	−0.188	−0.131
	(0.360)	(0.236)	(0.240)	(0.351)
Loss frame × High value	−0.080	0.009	−0.213	−0.265
	(0.342)	(0.239)	(0.244)	(0.363)
Historical × High value	−0.052	0.040	−0.325	0.028
	(0.328)	(0.234)	(0.241)	(0.351)
Loss frame × Historical × High value	0.595	0.022	0.183	−0.004
	(0.485)	(0.336)	(0.337)	(0.507)
Age	−0.014***	0.007**	−0.008***	−0.017***
	(0.004)	(0.003)	(0.003)	(0.005)
Female	−0.509***	−0.475***	0.080	−0.426***
	(0.124)	(0.087)	(0.088)	(0.130)
White	−0.220*	−0.036	0.091	−0.069
	(0.130)	(0.091)	(0.091)	(0.138)
Higher ed.	0.172	0.235**	0.058	−0.094
	(0.134)	(0.094)	(0.093)	(0.137)
Santiago	0.046	0.053	0.041	0.047
	(0.123)	(0.086)	(0.086)	(0.129)
Right-wing ideology	0.351	0.009	0.152	0.069
	(0.244)	(0.173)	(0.173)	(0.260)
National identification	0.698**	0.274	−0.011	0.197
	(0.300)	(0.202)	(0.202)	(0.305)
Militant assert.	2.047***	−0.226	−0.229	0.383
	(0.260)	(0.185)	(0.186)	(0.275)
International trust	−0.279*	−0.007	−0.021	0.038
	(0.144)	(0.096)	(0.096)	(0.143)
Social trust	0.108	0.037	0.003	−0.028
	(0.197)	(0.137)	(0.138)	(0.209)
Intercept	−2.580***	−0.760***	−0.828***	−1.430***
	(0.330)	(0.227)	(0.232)	(0.337)
Observations	2,600	2,600	2,600	2,600
Akaike Inf. Crit.	1,916.599	3,308.396	3,305.586	1,810.603

Note: *p<0.1; **p<0.05; ***p<0.01

C.2 Policy interpretation does not significantly mediate treatment effects

Next, we test two ways that policy interpretation might matter for our findings: as a *mediator* between our experimental treatments and risk acceptance, and as a *moderator* of our treatment effects. The first is more threatening to our inferences as it suggests that our experimental results do not reflect prospectic-theoretic reference dependence, but rather policy-specific risk attitudes that are elicited by our treatments. To test for this possibility, we conduct causal mediation analysis using Tingley et al. (2014)'s `mediation` package in R, treating policy interpretation as a mediator between the manipulated treatments and risk preferences. We test the mediation effect of each variable one at a time, in effect treating each policy interpretation variable as independent of the others. Since viewing the risky option as one type of policy is undoubtedly related to one's propensity to view that option as another type of policy, these mediators are not plausibly independent of one another in reality and therefore the sequential ignorability assumption that makes the causal mediation effect identifiable is not met. However, the spurious correlation we risk introducing by treating the mediators as independent will tend to produce *over*estimates of the true mediation effect of each variable, and so our results should be treated as upper-limit estimates of the mediating effects of policy interpretation in our study.

In Table C5 we show the proportion of the total framing effect attributable to the average causal mediation effect (ACME) of each policy interpretation. For Argentina and Experiment 1 in Chile, each column represents one of the opponent conditions, and the number in each row represents the share of the total framing effect within that opponent condition that could be attributed to the policy interpretation. For Experiment 2 in Chile, we pool across the historical and value conditions because they had little appreciable impact on risk acceptance. Some of the proportions are indeed statistically significant; in particular, interpreting the policy as military action mediates some of the framing effect in the no opponent condition in Argentina, and renegotiation does so across the no opponent and Chile conditions, as well as in Chile for the UK condition. Substantively, however, the mediating effects are modest—military action mediates 3 percent of the overall

framing effect in Argentina when no opponent is named, and renegotiation counts for 2 percent. Summing the shares down the first column, these policy interpretations account for little more than 6 percent of the overall framing effect for the no opponent condition in Argentina. In the UK condition in Chile, estimates similarly come out to between 5 and 6 percent. These effects are negligible in Experiment 2. To summarize, despite some evidence that our treatments influenced policy interpretation, we do not find that these effects mediate our experimental findings to any significant extent.

	Argentina			Chile, Exp. 1		Chile, Exp. 2
Policy	No opponent	Chile	UK	Argentina	UK	Pooled
Military action	0.030***	0.003	-0.009	-0.000	0.004	0.002
	(0.006)	(0.840)	(0.682)	(0.932)	(0.496)	(0.248)
International court	0.011	0.014	0.003	-0.001	-0.002	-0.001
	(0.110)	(0.192)	(0.838)	(0.708)	(0.692)	(0.418)
Renegotiate	0.021*	0.028*	0.030	0.004	0.047**	0.002
	(0.050)	(0.092)	(0.204)	(0.796)	(0.048)	(0.314)
Wait	-0.000	-0.000	0.000	0.009	0.010	-0.001
	(0.974)	(0.972)	(0.990)	(0.274)	(0.332)	(0.712)

TABLE C5: Proportion of framing effect mediated through each substantive interpretation of risky policy option, estimated in R using the `mediate` package with 1,000 simulations (p-values in parentheses). *p<0.1; **p<0.05; ***p<0.01

These mediation effects are somewhat larger for the opponent treatment manipulations, as seen in Table C6. In the first column, the (lower) likelihood of interpreting the policy as military action due to Chile being named as the opponent accounts for 7 percent of the increase in risk acceptance among gain-framed respondents. The UK being named as the opponent (column 2) produces a similar effect, and the interpretation of the risky option as international arbitration contributes an additional 5 percent of the opponent effect. In all, policy interpretation contribute no more than 11 or 12 percent of the effect of the UK treatment in Argentina—again, an upper-limit estimate due to the possibility of spurious correlation. In Chile (columns 3 and 4), none of the mediation effects are statistically significant. The evidence suggests that policy interpretations do not mediate the lion's share of the observed treatment effects.

	Argentina survey		Chile survey	
Policy	Chile: gain	UK: gain	UK: gain	UK: loss
Military action	0.070**	0.065***	0.009	-0.039
	(0.028)	(0.004)	(0.856)	(0.438)
International court	0.015	0.049***	0.003	-0.003
	(0.604)	(0.002)	(0.924)	(0.894)
Renegotiate	0.031	-0.008	-0.002	-0.071
	(0.450)	(0.766)	(0.992)	(0.428)
Wait	-0.000	0.000	0.010	-0.005
	(0.954)	(0.958)	(0.876)	(0.862)

TABLE C6: Proportion of opponent effect (Experiment 1) mediated through each substantive interpretation of risky policy option, estimated in R using the `mediate` package with 1,000 simulations (p-values in parentheses). Only gain-framed responses are examined in Argentina; for the Chile sample, we examine the opponent effect in both gain and loss frame. *p<0.1; **p<0.05; ***p<0.01

C.3 Policy interpretation may moderate treatment effects

Finally, we examine how policy interpretation may have *moderated* our treatment effects. We plot our main results for Experiment 1 (Figures C1 and C2) and Experiment 2 (Figure C4), subsetting by policy interpretation. These results should be interpreted with due caution given that policy interpretation is endogenous to treatment, but they can serve to motivate expectations for future research. The framing effect holds firm across the vast majority of policy interpretations and treatment conditions across both experiments, although its magnitude fluctuates greatly. The opponent effect is less consistent across policy interpretations. Figure C3 plots the marginal effect of making the UK the opponent rather than the neighboring state by policy interpretation, framing, and sampled country (Experiment 1 data only). In both samples, risk acceptance in loss frame drops among respondents who view the policy as military action when the UK is the opponent. In Chile, this effect is large enough to effectively neutralize the framing effect. Among respondents who view the risky option as renegotiation, risk acceptance rises in gain frame when the UK is named as the opponent. Few patterns of note emerge in the same analysis of Experiment 2. While viewing the risky option as military action on its own does not eliminate the framing effect, future research can explore the opponent specificity of framing effects on risk attitudes when the risky option is explicitly labeled as military

escalation.

Figure C3 shows that in both samples, respondents who view the risky option as military action are less likely to choose it when the opponent is the UK, but only in loss frame. In Argentina, the framing effect is reduced as a result but remains robust, whereas in Chile, this effect is strong enough to nearly nullify the framing effect among respondents who view the policy as military action and are presented with the UK as the opponent (Figure C2).[26] On the other hand, respondents in both countries who view the policy as renegotiation are *more* likely to choose it when the UK is the opponent, but only in gain frame (returning to Figure C3).

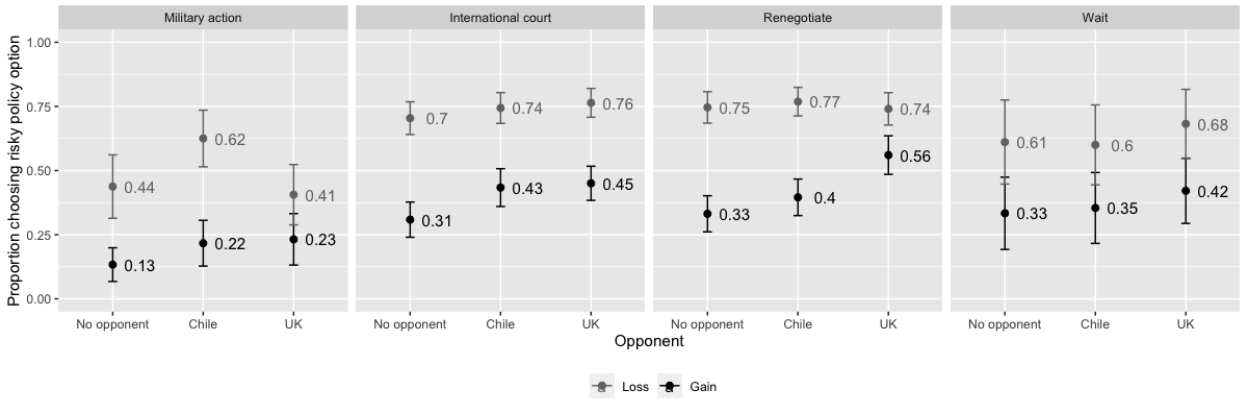

FIGURE C1: Experiment 1 results by risky policy interpretation with 95% confidence intervals, Argentina

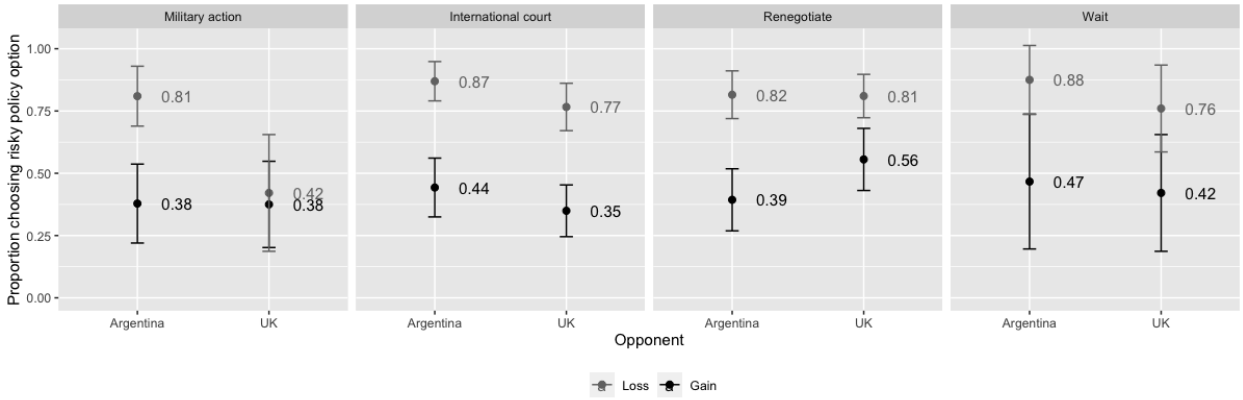

FIGURE C2: Experiment 1 results by risky policy interpretation with 95% confidence intervals, Chile

[26]This is the only combination of sample, policy interpretation, and framing that fails to recover a strong framing effect.

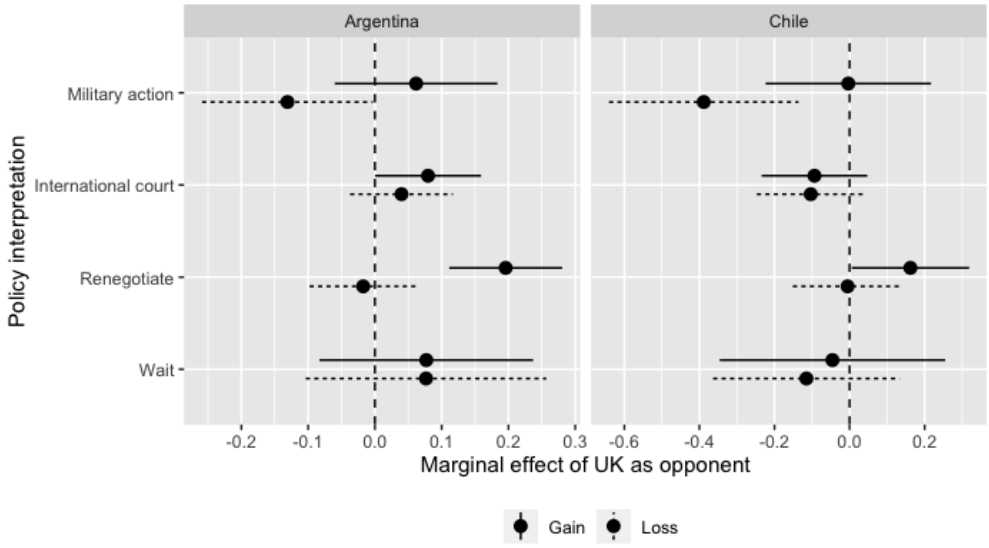

FIGURE C3: Marginal effect of UK being the opponent instead of the neighboring state, Experiment 1

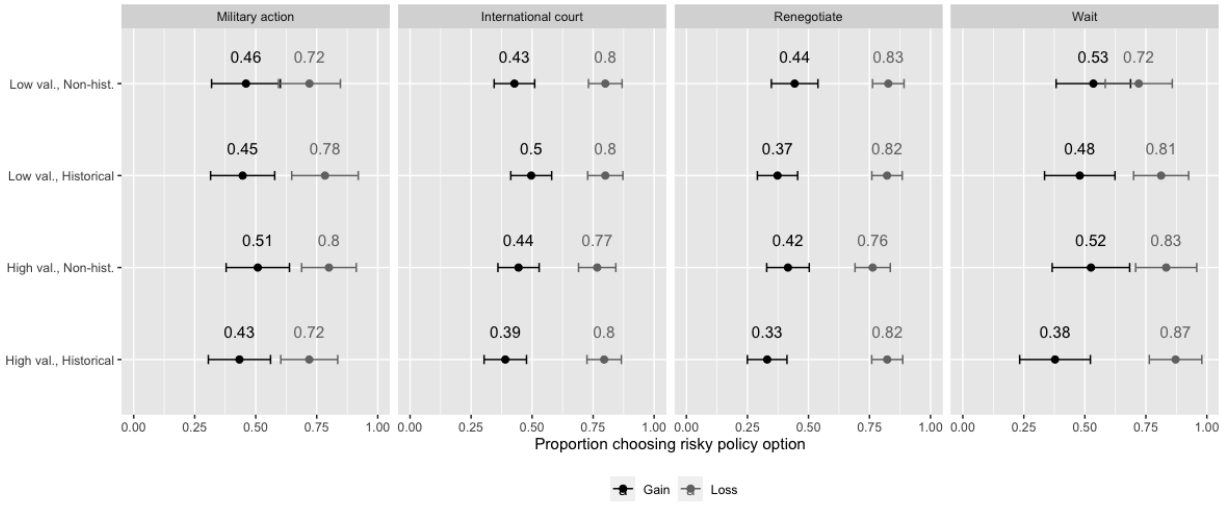

FIGURE C4: Experiment 2 results by risky policy interpretation with 95% confidence intervals

D Support for use of force: robustness check

In Table D1, we present additional robustness checks for the findings shown in Table 1. Model 1 shows that the interaction between the instrumented risk acceptance variable and the interpretation of the risky policy as military action remains robust after controlling for interactions between risk acceptance and a number of demographic and dispositional characteristics that predict interpreting the policy as military action. Model 2 uses a logit model to calculate a propensity score for viewing the risky option as military action, as a function of all available demographic and dispositional covariates. This propensity score is then included in the model and interacted with risk acceptance. The interaction between risk acceptance and policy interpretation as military action remains robust.

TABLE D1: Risk acceptance on use of force, supplemental results

	Dependent variable:	
	Support use of force	
	IV	*IV*
	(1)	(2)
Risky option	−0.099	0.002
	(0.168)	(0.078)
Mil. action	−0.032	−0.028
	(0.078)	(0.074)
Risky option × Mil. action	0.488***	0.496***
	(0.135)	(0.130)
Risky option × Mil. assert.	0.191	
	(0.158)	
Risky option × Nat'l iden.	0.069	
	(0.172)	
Risky option × Age	0.0004	
	(0.002)	
Risky option × Female	0.014	
	(0.077)	
Risky option × Int'l trust	−0.006	
	(0.076)	
Risky option × propensity mil. action		0.198
		(0.479)
Propensity mil. action		−0.201
		(0.503)
Age	−0.001	−0.001
	(0.001)	(0.001)
Female	−0.034	−0.030
	(0.048)	(0.024)
White	0.031**	0.028*
	(0.015)	(0.015)
Higher ed.	0.005	0.006
	(0.015)	(0.016)
Santiago	0.020	0.020
	(0.014)	(0.015)
Right-wing ideology	0.133***	0.134***
	(0.028)	(0.031)
National identif.	0.135	0.180***
	(0.106)	(0.043)
Mil. assertiveness	0.457***	0.591***
	(0.103)	(0.082)
Int'l trust	−0.056	−0.060***
	(0.045)	(0.017)
Social trust	0.023	0.026
	(0.023)	(0.023)
Constant	−0.023	−0.074
	(0.101)	(0.070)
Observations	3,240	3,240
R^2	0.301	0.303
Note:	*p<0.1; **p<0.05; ***p<0.01	

E Emotions and the opponent effect

To gain additional resolution on how attitudes toward the opponent affect risk preferences in territorial disputes, we add pre-treatment measures of how respondents feel about potential opponents in our Chile survey. First, respondents are asked to rate their warmth toward each of four countries, including Argentina and the United Kingdom, on a feeling thermometer of 0 to 100[27]:

> *We'd like to get your feelings toward some different countries in the world on a "feeling thermometer." A rating of zero degrees means you feel as cold and negative as possible. A rating of 100 degrees means you feel as warm and positive as possible. You would rate the country at 50 degrees if you don't feel particularly positive or negative toward the country.*
>
> *How do you feel about:*
>
> 1. *United States*
> 2. *United Kingdom*
> 3. *Peru*
> 4. *Argentina*

Next, we measure fear, anger, and confidence with respect to each country on 5-point Likert scales:

> [Fear] *How much do you fear that each of the following countries poses a threat to the security of Chileans? (1. Not at all; 2. A little; 3. Moderately; 4. Very much; 5. Extremely)*
>
> [Anger] *How much you are angered by the way each of the following countries treats Chile in political affairs? (1. Not at all; 2. A little; 3. Moderately; 4. Very much; 5. Extremely)*
>
> [Confidence] *For each country, please rate whether you believe it is stronger or weaker than Chile. (1. Much stronger; 2. Somewhat stronger; 3. About the same; 4. Somewhat weaker; 5. Much weaker)*

For our analysis, we calculate a *relative* score for each emotion by subtracting the Argentina score from the UK score. In other words, each emotion variable captures how much more a respondent feels that emotion toward the UK relative to Argentina. We also pool our sample across experiments to focus on the framing and opponent treatment manipulations; all Experiment 2 participants are coded as receiving the Argentina opponent condition.

[27] The wording of this question is taken from Pew Research Center (2018).

First, we test how emotions condition the effect of changing the opponent from Argentina to the UK. Using a logit specification, we regress risk acceptance on the opponent treatment interacted with each emotion variable, splitting the sample by gain frame and loss frame:

$$\begin{aligned}\ln\left(\frac{Pr(Gamble=1)}{1-Pr(Gamble=1)}\right) = \alpha + \beta_1 UK + \\ \beta_2 Warmth + \beta_3 Fear + \\ \beta_4 Anger + \beta_5 Confidence + \\ \beta_6 UK \times Warmth + \beta_7 UK \times Fear + \\ \beta_8 UK \times Anger + \beta_9 UK \times Confidence + \\ \lambda \mathbf{X} + \epsilon\end{aligned} \tag{1}$$

UK is a binary variable that equals 1 when the UK is presented as the opponent and 0 when the opponent is Argentina. $\mathbf{X}$ represents a vector of demographic and dispositional controls. We estimate this regression separately for gain frame and loss frame responses. Figure E1 plots the marginal effect of presenting the UK as the opponent, conditional on each relative emotion. As hypothesized, warmth modestly decreases risk acceptance and anger tends to raise it. Contrary to expectations, confidence appears to decrease risk acceptance and fear shows little effect.

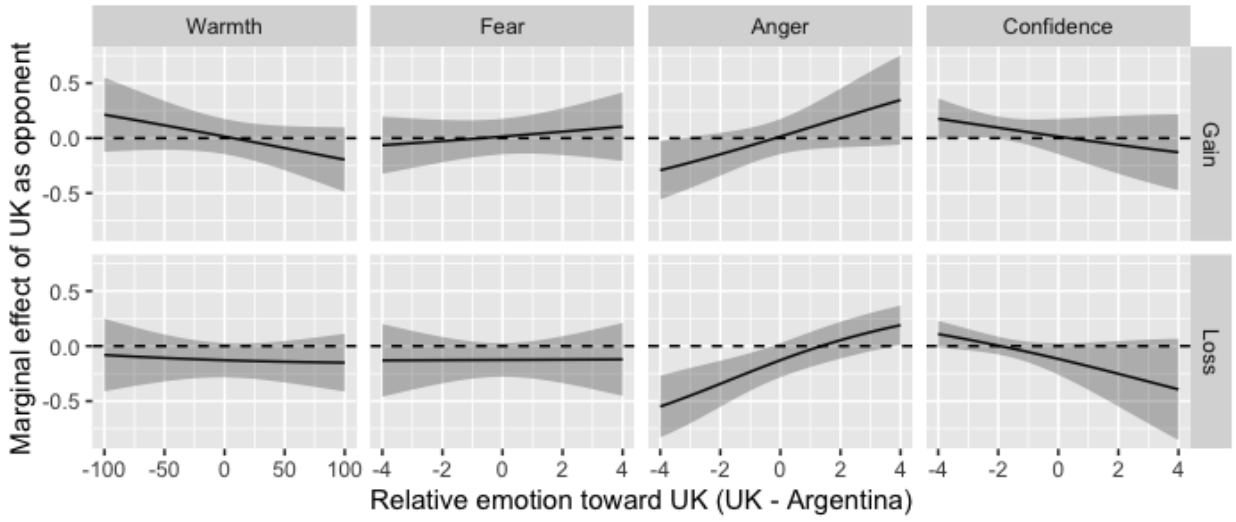

FIGURE E1: Marginal effect of presenting the UK rather than Argentina as the opponent with 95% confidence intervals. Gain frame in top row and loss frame in bottom row.

The figure also does not show drastic differences across gain and loss frames for any of the emotions, against our expectations that warmth and fear would widen the framing effect and anger and confidence would reduce it. To assess more formally how emotions influence framing sensitivity, we again use a logit regression to estimate three-way interactions between loss framing, the opponent treatment, and each emotion in turn:

$$\begin{aligned}\ln\left(\frac{Pr(Gamble=1)}{1-Pr(Gamble=1)}\right) = \alpha + \beta_1 Loss + \beta_2 UK + \beta_3 Loss \times UK + \\ \gamma \mathbf{Emotions} + \delta \mathbf{Emotions} \times Loss + \zeta \mathbf{Emotions} \times UK + \\ \eta \mathbf{Emotions} \times Loss \times UK + \\ \lambda \mathbf{X} + \epsilon\end{aligned} \tag{2}$$

Emotions represents the vector of our four emotion variables. With this approach, we effectively model the change in framing effect induced by switching the opponent from Argentina to the UK as a

function of relative emotion toward the UK (as compared to Argentina). Figure E2 plots the marginal change in loss framing due to the opponent being the UK rather than Argentina, conditional on relative emotion. There is little evidence of any of the emotions moderating the change in the framing effect, though with so few respondents receiving the UK as the opponent, we are underpowered to draw any strong conclusions.

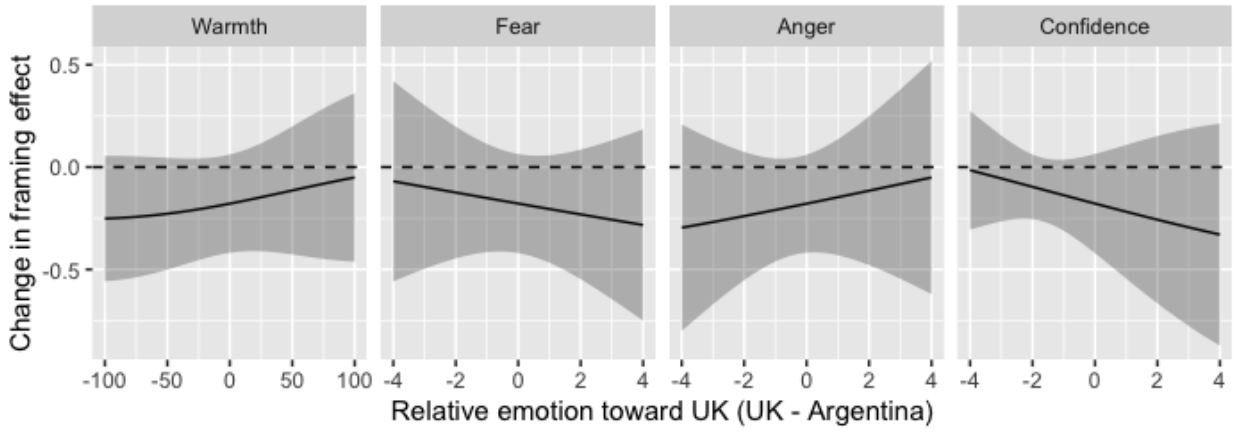

FIGURE E2: Marginal change in loss framing effect due to opponent being the UK rather than Argentina; 95% confidence intervals.

F Historical trends in territorial dispute longevity

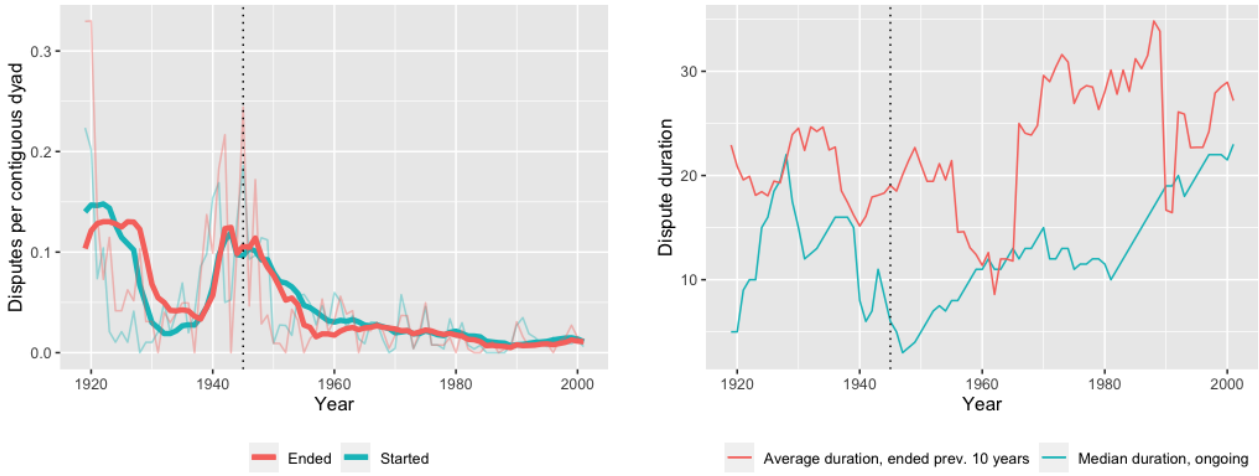

FIGURE F1: Left: Number of disputes beginning and ending each year, normalized by number of contiguous dyads in the international system. Bolded lines are 10-year rolling averages. Right: Median duration of extant disputes in a given year (blue) and 10-year rolling average of median dispute duration (red). The year 1945 is indicated with the dotted vertical line on both plots. Data from the Issue Correlates of War dataset (Frederick et al., 2017).

G Heterogeneous treatment effects

In Argentina, we examine heterogeneous treatment effects on gender, education, authoritarianism, ideology, international trust, national attachment, social dominance orientation, and social trust. We estimate the following logit regression using each of the above variables in turn as the "moderator":

$$\begin{aligned}\ln\left(\frac{Pr(Gamble=1)}{1-Pr(Gamble=1)}\right) &= \beta_0 + \beta_1 Loss + \beta_2 Chile + \beta_3 UK \\ &+ \beta_4 Loss \times Chile + \beta_5 Loss \times UK \\ &+ \beta_6 Moderator + \beta_7 Moderator \times Loss \\ &+ \beta_8 Moderator \times Chile + \beta_9 Moderator \times UK \\ &+ \beta_{10} Moderator \times Loss \times Chile + \\ &+ \beta_{11} Moderator \times Loss \times UK \\ &+ \gamma \mathbf{X} + \epsilon\end{aligned} \tag{3}$$

where $Loss$ is the loss-framing treatment, $Chile$ is a binary variable taking value 1 if the prompt specifies Chile as the opponent and 0 if not, UK does the same for the UK, and X is a vector of demographic controls—age, race, and Buenos Aires residence, as well as gender and education when they are not being used as the moderator.

In Figure G1, we plot the change in predicted probability of choosing the risky option by framing condition, separated by opponent condition. This visualization quickly summarizes a) whether the moderator had an intercept effect on the outcome, as reflected in the slopes of the lines, and b) whether it moderated the framing effect, which would lead to the lines not being parallel. Only two cases show clear evidence of moderating effects: the framing effect widens with right-wing ideology when Chile is the opponent, and with national attachment when no opponent is named.

In Chile, we explore potential moderating effects of gender, education, authoritarianism, ideology, international trust, national identification, and social trust. For Experiment 1, we use a similar specification to Equation 3 above except with only one opponent variable. Figure G2 show predicted probabilities of choosing the risky policy option by moderator, separated by framing condition and opponent condition. There is little evidence that any of the tested covariates moderate the framing effect in any of the treatment conditions. There is some suggestive evidence that right-wing ideology and national identification raise risk acceptance more significantly when the opponent is Argentina rather than Chile.

For Experiment 2, we test only for possible moderators of the framing effect, given the overall null findings on the historical and value framing treatments. The predicted probability curves in Figure G3 show little evidence of heterogeneous treatment effects.

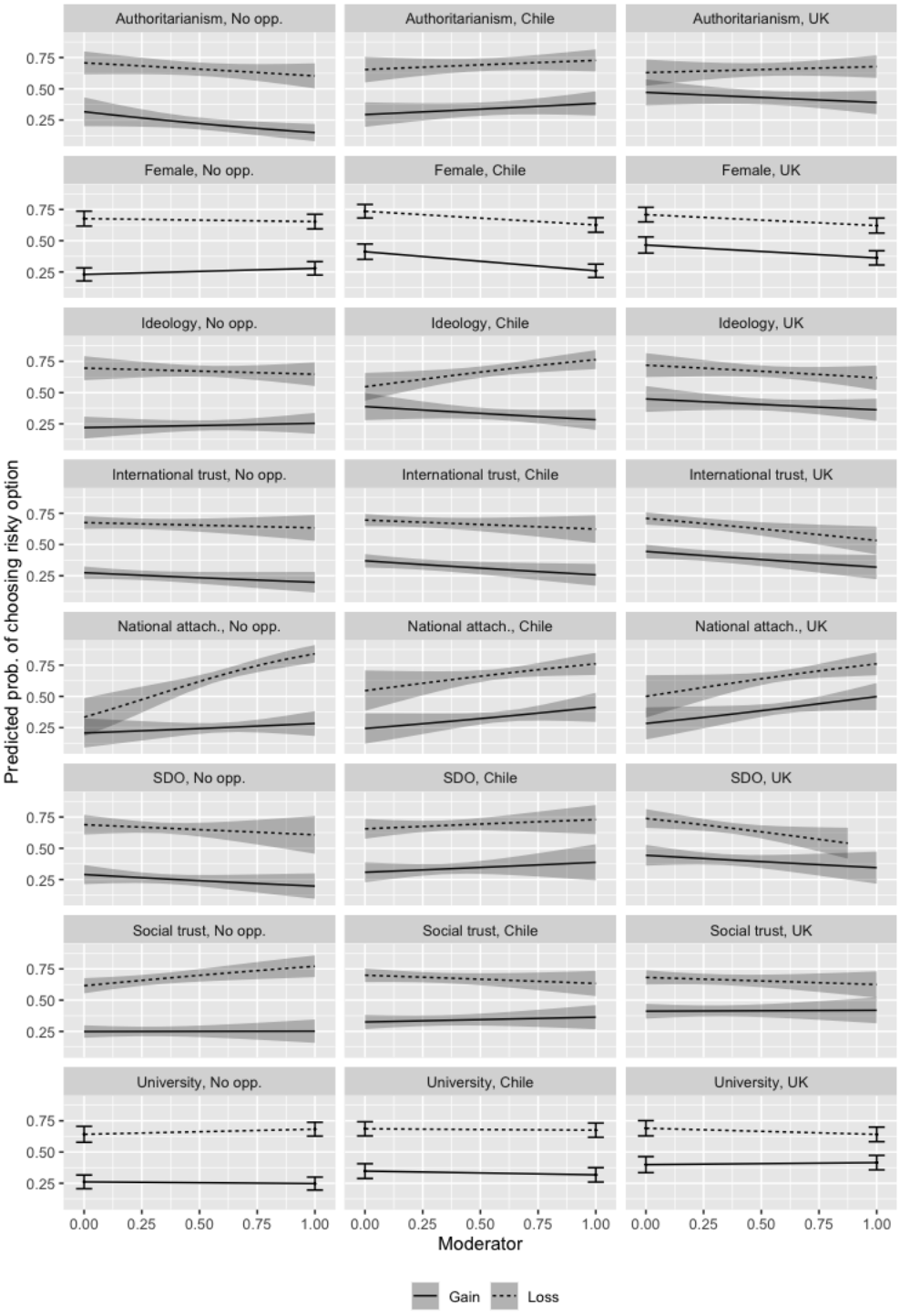

FIGURE G1: Predicted probabilities of choosing the risky policy option by moderator with 95% confidence intervals, Argentina. Solid lines are gain-framed predicted probabilities and dashed lines are loss-framed. Gender and education are binary variables, whereas the rest are continuous.

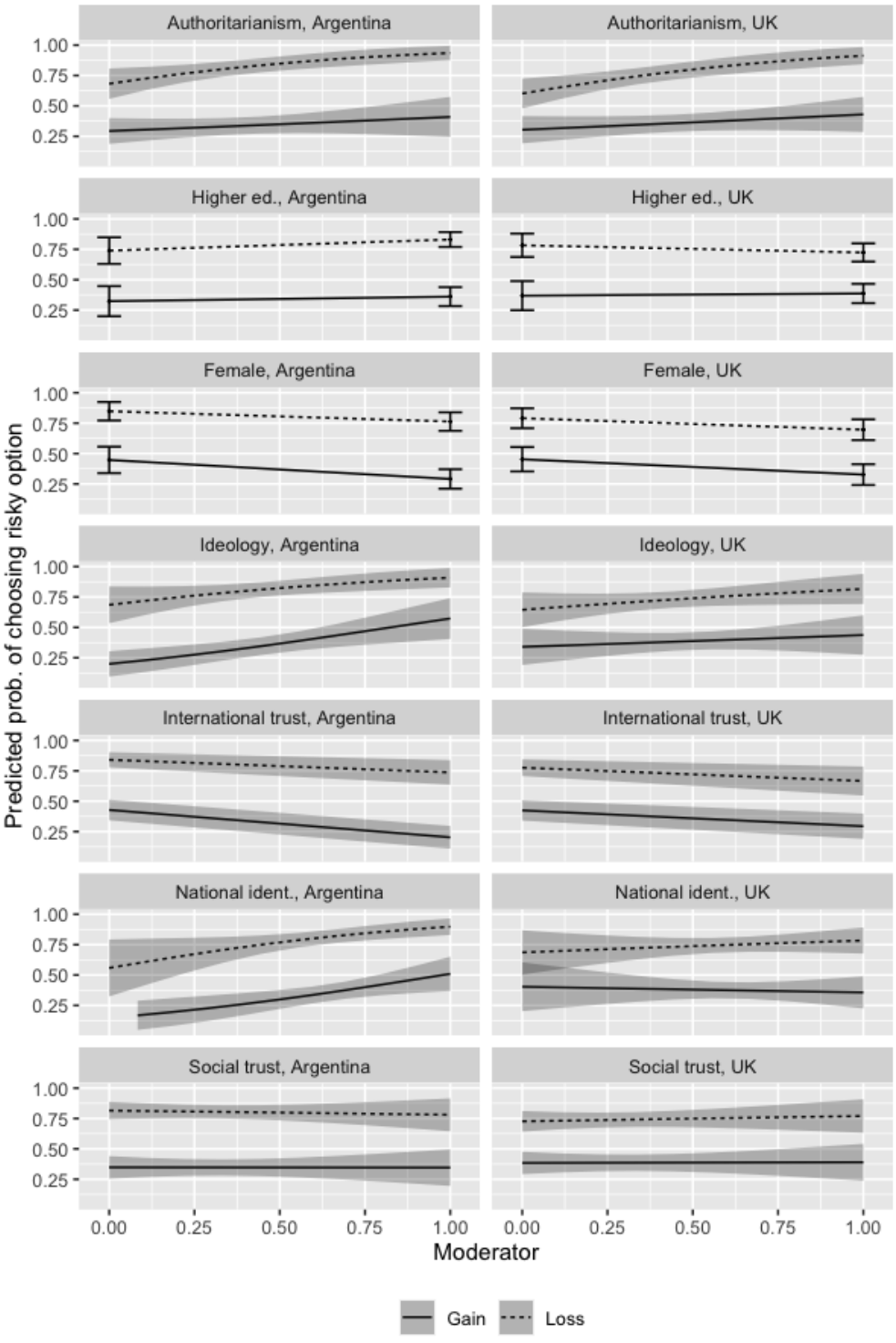

FIGURE G2: Predicted probabilities of choosing the risky policy option by moderator with 95% confidence intervals, Experiment 1 in Chile. Solid lines are gain-framed predicted probabilities and dashed lines are loss-framed. Gender and education are binary variables, whereas the rest are continuous.

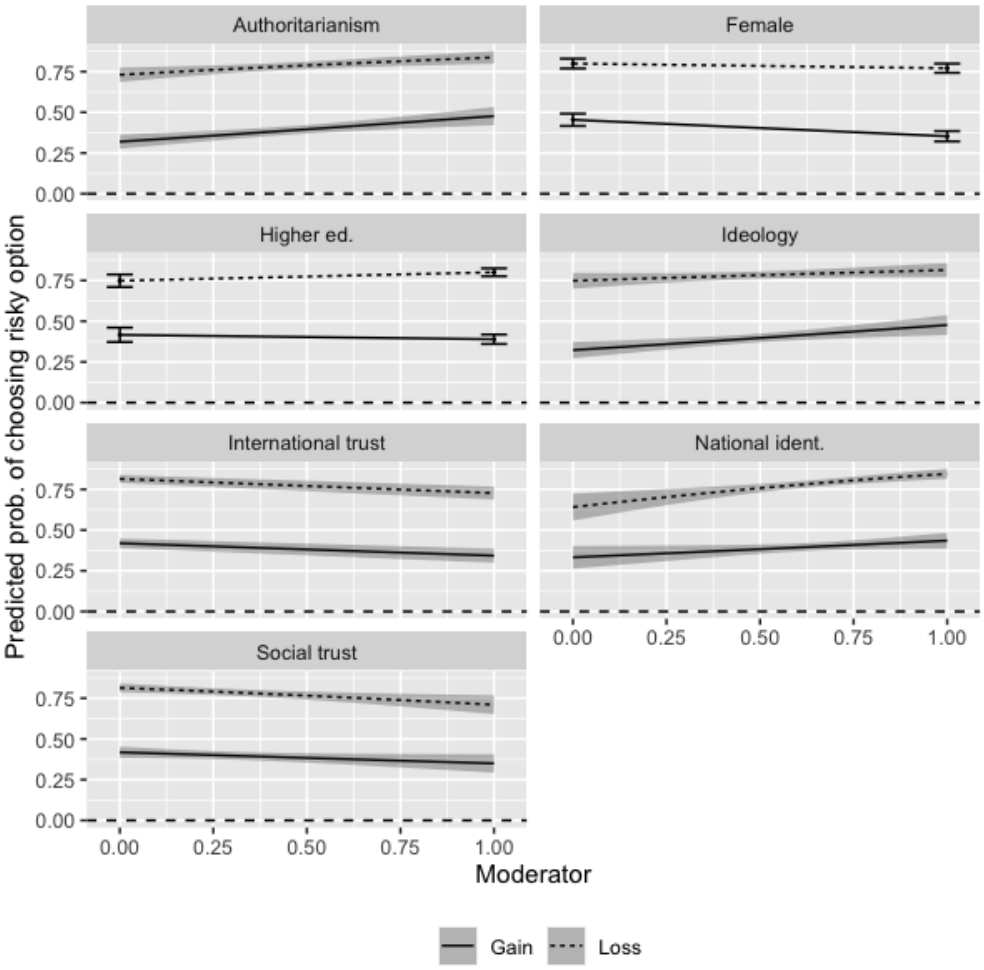

FIGURE G3: Predicted probabilities of choosing the risky policy option by moderator with 95% confidence intervals, Experiment 2 in Chile. Solid lines are gain-framed predicted probabilities and dashed lines are loss-framed. Gender and education are binary variables, whereas the rest are continuous.