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ORIGINAL ARTICLE

Political Psychology

An adversarial collaboration on the rigidity-of-the-right, symmetry thesis, or rigidity-of-extremes: The answer depends on the question

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Abstract

In an adversarial collaboration, two preregistered U.S.-based studies (total $N = 6181$) tested three hypotheses regarding the relationship between political ideology and belief rigidity (operationalized as less evidence-based belief updating): rigidity-of-the-right, symmetry, and rigidity-of-extremes. Across both studies, general and social conservatism were weakly associated with rigidity ($|b| \sim .05$), and conservatives were more rigid than liberals (Cohen's $d \sim .05$). Rigidity generally had null associations with economic conservatism, as well as social and economic political attitudes. Moreover, general extremism (but neither social nor economic extremism) predicted rigidity in Study 1, and all three extremism measures predicted rigidity in Study 2 (average $|bs| \sim .07$). Extreme rightists were more rigid than extreme leftists

in 60% of the significant quadratic relationships. Given these very small and semi-consistent effects, broad claims about strong associations between ideology and belief updating are likely unwarranted. Rather, psychologists should turn their focus to examining the contexts where ideology strongly correlates with rigidity.

KEYWORDS
adversarial collaboration, belief updating, bias, political psychology, social cognition

INTRODUCTION

Adorno (1950) hypothesized that ideological beliefs are coherent manifestations of dispositions, prompting decades of scholarship examining the psychological characteristics underlying political ideology (Hibbing et al., 2014; Rokeach, 1960; Tetlock, 1983). An influential framework known as the rigidity-of-the-right hypothesis emerged from this scholarship, contending that conservatism stems from rigid, inflexible thinking and needs for certainty that coalesce to form an authoritarian "syndrome" that exists predominantly among conservatives (Hibbing et al., 2014; Jost, 2017). Although the rigidity-of-the-right hypothesis has been the dominant perspective for many years, symmetry-oriented scholars contend that social motivations promote rigidity in any political group (Clark & Winegard, 2020; Ditto et al., 2019). Finally, rigidity-of-extremes theorists assert that rigidity occurs among ideological extremists on the left and right (Greenberg & Jonas, 2003; Van Prooijen & Krouwel, 2019; Zmigrod et al., 2018). These perspectives continue to fuel scientific debate (Zmigrod, 2020).

Political ideology reflects a set of interconnected values, beliefs, and opinions that bear on political life (Jost et al., 2009). Although there is no "one size fits all" conceptualization (Costello et al., 2023; Hibbing et al., 2014), ideology is commonly operationalized along a left/right continuum. Here, we operationalize ideology along this continuum, while also considering attitudes toward social and economic issues. In an adversarial collaboration, we leverage the insights of scholars with different perspectives to devise and conduct optimal tests of the relationship between ideology and rigidity (here, operationalized as less evidence-based belief updating, which we detail below).

Rigidity-of-the-right

An illustrious literature demonstrates that conservatism correlates positively with myriad self-report measures of cognitive inflexibility, including intolerance of ambiguity (Adorno, 1950), cognitive rigidity (Van Hiel et al., 2016), and low levels of Openness to Experience (Osborne et al., 2024). These ideological asymmetries in (in)flexible thought even emerge, albeit less consistently, in behavioral tasks that avoid content overlap with political attitudes, including working memory tasks (Buechner et al., 2020) and behavioral measures of persistence in unrewarding strategies (Zmigrod et al., 2018). Notably, meta-analyses demonstrate that cognitive rigidity and needs for epistemic certainty correlate positively with conservatism (Jost et al., 2003; Van Hiel et al., 2016). These associations may, however, be confined to social (vs. economic) conservatism, especially in ideologically constrained, Western, and developed societies (Malka et al., 2014). Nevertheless, considerable evidence reveals an ideological asymmetry in rigidity.

Symmetry models

In contrast to the rigidity-of-the-right hypothesis, some contend that status attainment, ostracism avoidance, and in-group cohesion were so essential for human survival that all groups should resist group-discordant information (Clark et al., 2019; Clark & Winegard, 2020). Such symmetry models assert that there is no a priori reason to assume any political group would be immune to rigidity, and the direction of differences varies across domains. In aggregate, differences appear small to nonexistent (Ditto et al., 2019; Guay & Johnston, 2022). For example, right-wing attitudes may predict rigid religious beliefs, whereas left-wing attitudes may predict rigid environmental attitudes (Conway III et al., 2016). Indeed, both left-wing and right-wing attitudes predict opposition to societal change but for different issues (Proch et al., 2018), and they both predict authoritarianism, but toward different authorities (Conway III et al., 2018; Frimer et al., 2014). The potential for domain-specific effects means that researchers must evaluate the properties of rigidity measures to ensure they do not appeal to one ideological group's (in)flexibility. We avoid this pitfall here by adopting a measure of rigidity that is not conflated with one specific political ideology and by conducting a pretest to select balanced items (all of which is detailed later).

Rigidity-of-extremes

A third perspective suggests that rigidity occurs most frequently at both the left and right extremes but acknowledges that extremism effects can be symmetric (i.e., both extremes being similarly more rigid than moderates) or asymmetric (i.e., both extremes being more rigid than moderates, but to a stronger degree at one end of the spectrum). Extreme ideologies increase epistemic clarity by offering straightforward propositions about the world, clearly distinguishing between right and wrong, enabling simple understandings of complex societal issues, and providing meaningful goals (Greenberg & Jonas, 2003; Van Prooijen & Krouwel, 2019). People at the extreme left and right display more judgmental certainty (Van Prooijen et al., 2018) and confidence in the objective correctness of their opinions (Toner et al., 2013), have strong moral convictions (Skitka, 2010), and are less tolerant than moderates of different political opinions (Van Prooijen & Krouwel, 2017). Political extremism predicts decreased insight in the correctness of one's choices, reduced sensitivity to post-decision evidence in non-political perceptual tasks (Rollwage et al., 2018), and reduced flexibility across basic cognitive tests (Zmigrod et al., 2020). Thus, according to this perspective, political extremism, rather than political orientation, predicts rigidity.

Operationalizing rigidity

Rigidity has a long history in personality and social psychology (Costello et al., 2023; Zmigrod, 2020), yet definitional clarity about the construct remains elusive. There is no universally accepted definition or measure of rigidity, as scholars employ heterogeneous definitions of this phenomenon (Costello et al., 2023). For example, in a renowned defense of the rigidity-of-the-right hypothesis, Jost et al. (2003) referred to "rigid" or "rigidity" 23 times but never defined rigidity, and Cherry et al. (2021) noted the rigidity literature contains 25 different conceptualizations assessed by 23 different operationalizations. In addition, many popular measures may be flawed or confounded with ideology itself (Costello et al., 2023). For example, Van Hiel et al. (2010) note that typical dogmatism scales are often treated as ideology-free measurements and yet nonetheless contain many items confounded with ideological content.

To avoid these (and other) pitfalls, the present project brought together experts from different sides of the ideological rigidity debate to choose the best conceptualization and operationalization of rigidity. Specifically, the adversaries developed a list of all known rigidity operationalizations and then eliminated operationalizations that at least one adversary perceived as flawed, confounded, or not a measure of rigidity. Although this enterprise is inherently subjective, it is a novel solution to the subjectivity evident in prior tests (Cherry et al., 2021; Costello et al., 2023). After this process (see Pretest below), only one operationalization was unanimously regarded as a valid and unbiased measure of rigidity: belief updating in response to evidence (Kossowska et al., 2023; Vlasceanu et al., 2021). Less evidence-based belief updating indicates belief rigidity because it reflects an unwillingness to change one's mind in response to new information (Kossowska et al., 2023). Thus, our results are specific to "evidence-based belief updating" and may not apply to other rigidity measures that our adversarial team deemed non-optimal.

Adversarial Collaboration and Hypotheses

We conducted an adversarial collaboration, a methodological procedure in which disagreeing scholars mutually design optimal methods to test competing hypotheses (Clark et al., 2022; Mellers et al., 2001). Adversarial collaborations restrict researchers' degrees of freedom by preventing scholars from selecting methods most likely to support their hypotheses and avoiding alternative (but equally relevant) approaches (Clark et al., 2022). Our authorship team includes adversaries who have published first-authored work consistent with each perspective, and the adversaries were responsible for the study design and generating hypotheses. An additional moderator who has not published first-authored work consistent with any of the three perspectives (and who was not involved in the study design or hypothesis generation) was responsible for all data analyses.

This adversarial collaboration sought to answer an overarching question: "How does political ideology (i.e., from extremely liberal to extremely conservative) relate to the degree of belief updating in the political domain when exposed to evidence?" We tested the hypotheses that conservatism would be negatively related to evidence-based belief updating (rigidity-of-the-right hypothesis), conservatism would be unrelated to evidence-based belief updating (symmetry thesis), and political extremity (in the form of ideological extremism) would be negatively related to evidence-based belief updating (rigidity-of-extremes hypothesis). The first two hypotheses are in direct conflict, whereas the third hypothesis is falsifiable but not mutually exclusive with the first two hypotheses.

Given the arbitrariness of precise statistical cut points, the adversaries did not select a collective smallest effect size of interest. Instead, the adversaries intentionally left open the criteria for evaluation—interpretation of the results was a key part of the adversarial process. The adversaries planned to decide together whether the results were meaningful, in a practical and theoretical sense, after weighing all the evidence. Also, from a more practical perspective, there was not a clear effect size to choose from in the literature. Existing meta-analytic effect sizes, such as those in Jost (2017), do not apply because we specifically focused on evidence-based belief updating in the political domain and intentionally selected rigidity and ideology measures that were not conflated with one another (a frequent criticism of prior work). Furthermore (and as detailed later), we used mixed-effects regressions, which preclude identifying a clear benchmark for effect size interpretation. All researchers agreed that the methodological approaches were excellent tests of the competing hypotheses, but individual researchers were free to state their predictions/criteria in their own terms. Some researchers focused on specific effect sizes whereas others emphasized general patterns.

PRETEST

Methods

Procedure selection

To clarify the best conceptual and operational tests of the hypotheses for ideology and rigidity, participating adversaries first generated a list of 44 operationalizations of "rigidity" used in the literature via discussion and collaboration. These operationalizations were organized into eight broad conceptual categories (e.g., authoritarianism, reasoning biases). Each conceptual category was viewed by researchers as relevant to rigidity and contained between 2 and 13 specific operationalizations (e.g., self-reported motivations and cognitive styles [intellectual humility, need for closure], confirmation bias [selective exposure, policy evaluations]). For each operationalization, we included one to four sample references that used the given operationalization. Table S1 includes the full list of operationalizations and references.

Next, each operationalization was independently coded by each adversary along these dimensions: (1) estimates of the correlation with social, economic, and general conservatism; (2) whether the operationalization was a personality measure, a cognitive process, or a behavior; (3) whether the operationalization measured a motivation or an ability; (4) the quality of the operationalization for the corresponding latent construct; (5) the quality of the operationalization for rigidity; (6) whether the operationalization was confounded with other constructs including ideology; and (7) other problems with the operationalization. The adversaries provided independent ratings and then met as a team to resolve discrepancies and collectively select the optimal measure of rigidity. After considering these ratings and thorough discussion, only one operationalization was unanimously viewed as a high-quality measure of rigidity: evidence-based belief updating. We next conducted a large-scale pretest of beliefs and information sources to eliminate confounds related to ideological differences in preexisting beliefs and trust toward information sources.

Participants

We recruited a demographically representative sample (based on age, gender, ethnicity, and region of the United States) of 2000 participants on Lucid; Lucid overrecruited by 12 ($N = 2012$; $M_{age} = 41.94$, $SD = 16.51$). Sample sizes for all studies were simply as large as possible with the funds available. Most participants leaned slightly to the right ($M_{conservatism} = 57.06$, $SD = 26.62$) on a 0 (*extremely liberal*) to 100 (*extremely conservative*) scale.

Procedure

A primary goal of the pretest was to identify pairs of statements for a belief updating task that showed no aggregate bias toward liberals or conservatives. Adversaries generated pairs of political statements that could be defended via argument or research. Thus, we only included political statements that had at least some empirical support to reduce implausibility confounds. Many political statement pairs included a liberal-friendly argument and a parallel conservative-friendly argument (Table 1). The team purposefully cast a wide net of different sets of parallel statements for the pretest, given that having a diverse range of items is essential for examining our main hypotheses (Baron & Jost, 2019). We sought to test belief updating for political statements because people display more rigidity in response to political (vs. non-political) information (Kossowska et al., 2023). Moreover, belief updating in the political

TABLE 1 Political belief updating statements used in Study 1.

Topic	Direction	Statement
IQ	Pro-left	People who are liberal on social issues score higher on IQ tests than do people who are conservative on social issues
	Pro-right	People who are conservative on fiscal issues score higher on IQ tests than do people who are liberal on fiscal issues
Economy	Pro-left	The U.S. economy performs better under Democratic presidents than under Republican presidents
	Pro-right	State economies perform better under Republican governors than under Democratic governors
Presidents	Pro-left	Compared to past American presidents, Donald Trump is uniquely simple-minded
	Pro-right	Compared to past American presidents, Joe Biden is uniquely simple-minded
Intolerance	Pro-left	Republicans are more intolerant of ethnic groups that differ from their own than are Democrats
	Pro-right	Democrats are more intolerant of political attitudes that differ from their own than are Republicans

realm is ostensibly of more practical importance than belief updating about neutral facts, and our hypotheses centered on evidence-based belief updating in the political realm. Thus, it was important to focus on political belief updating in order to make claims about ecological validity and real-world implications. Neutral items were, however, included in the pretest primarily as filler statements.

Participants received three main blocks of questions. First, participants were randomly assigned to rate the accuracy of 13 political statements from a set of 26 pairs (52 total statements) on a 1 (*extremely inaccurate*) to 9 (*extremely accurate*) scale. The two statements in each pair reflected opposing views on the same topic (e.g., gun control, abortion). Participants only received one item from each pair. Table 1 displays sample items (Table S2 includes the full materials).

Participants then used the same scale to evaluate the accuracy of 12 randomly assigned neutral statements (e.g., "People who like cilantro have more sophisticated palates than people who do not"). Unlike the political items, these items did not necessarily have empirical support. Participants also evaluated the credibility of 12 sources (randomly assigned from 46 total sources) on a 1 (*not at all credible or trustworthy*) to 9 (*very credible and trustworthy*) scale. These included organizations (e.g., Johns Hopkins University) and individuals (e.g., Payton Manning).

Results

We analyzed the correlations between ideology and all items in the pretest (Table S3). We selected pairs of items for Study 1 based on each paired item showing countervailing correlations with conservatism. Items that met this criterion were then reviewed by the adversaries, who selected the pairs unanimously considered to represent the fairest tests. To select information sources, we sought people and organizations for which the correlations between conservatism and trustworthiness were small ($rs < |.20|$ per Gignac & Szodorai, 2016). Sources that met this criterion were reviewed by the adversaries, who selected sources that were unanimously considered both unbiased and also plausible sources of information (e.g., universities without a strong political asymmetry in trust).

STUDY 1

Study 1 tested the associations between evidence-based belief updating and both political ideology and extremism. Evidence-based belief updating in this paradigm reflects how much participants update their beliefs in the direction of the new evidence supporting the statement. Thus, our primary question is the degree to which people shift their views in the evidence-based direction for political statements.

Methods

Participants

Data were collected from participants residing in the U.S. from January to February 2022 on Lucid. We recruited 3000 participants, but Lucid overrecruited by 51 ($N = 3051$). Data from participants who (a) failed the attention check ("please select 0 if you are still reading these questions"; $n = 588$) and/or (b) completed less than 50% of the survey ($n = 8$) were removed from the study ($N_{final} = 2455$, 80% of original sample; $M_{age} = 48.19$, $SD_{age} = 16.68$; 50.7% conservative).

Procedure

Participants completed an online battery of self-report measures of political attitudes. Table S4 displays descriptive statistics and response scales.

Political ideology

Participants rated their general, social, and economic political ideology separately using single items ("How would you rate your political ideology [on social\economic issues]?") on a 0 (*extremely liberal*) to 100 (*extremely conservative*) scale. Participants also completed the *Social and Economic Conservatism Scale* (SECS; Everett, 2013), which includes a series of feeling thermometers on a 0 (*negatively*) to 100 (*positively*) scale regarding 12 political topics (e.g., abortion, patriotism). After reverse coding relevant items, items were summed to generate a total score of feelings toward social ($a = .86$) and economic ($a = .57$) political issues. We also calculated a binary "liberal" and "conservative" score—those who identified as liberal (scores of 0 to 49) were classified as "liberal", and individuals who identified as conservative (scores of 51 to 100) were classified as "conservative".

Political extremism

We scored political extremism in two ways to capture individual differences in ideological extremism. First, we calculated the absolute value of the difference from the midpoint on the general, social, and economic political ideology single-item ratings (Van Prooijen & Kuijper, 2020). We also examined the quadratic relationships between political ideology (single-item ratings and attitudinal measures) and belief updating (Van Prooijen et al., 2018).

Belief updating

We used four pairs of political statements in the present study (Table 1); each pair contained a pro-right-wing statement and a pro-left-wing statement. Participants were randomly assigned to rate either the pro-right-wing statement or the pro-left-wing statement from each of the four pairs at the start of the survey. There were also 12 filler statements (selected at random from the pretest), 9 of which were neutral (e.g., "Left- and right-handed people earn equivalent incomes")

and 3 of which were political (e.g., "Higher rates of gun ownership are associated with safer neighborhoods"). Because the political filler statements were not matched in a pair with an ideological opposite, we retained them separately from the main effects. Upon viewing the statement, participants rated its accuracy on a 0 (*extremely inaccurate*) to 100 (*extremely accurate*) scale.

Participants then viewed evidence in support of each statement, such as the following example: "Research at the University of Alabama has found that people who are liberal on social issues score higher on IQ tests than do people who are conservative on social issues". Sources (selected from the pretest) were not randomized across statements, meaning each statement pair had a unique (and consistent) source (e.g., for the intolerance statement pair, the University of Michigan was the source for both statements). After viewing the evidence, participants rated the same statements again. The difference between the pre- and post-ratings was computed such that a positive difference between the two ratings reflected evidence-based belief updating (difference scores ranged from −100 [less evidence-based belief updating] to 100 [more evidence-based belief updating]). Here, we operationalize rigidity as less evidence-based belief updating.

Data analytic plan

Because Study 1 and Study 2 used the same data analytic plan, we only describe it here. We focus on the matched political statements, as evidence-based belief updating in the political domain was our primary outcome of interest. We preregistered linear mixed-effects regressions with evidence-based belief updating as the dependent variable and political ideology and extremism as fixed effects—these analyses were the focal analyses that addressed our main hypotheses. We included by-participant and by-belief-domain random intercepts. We originally preregistered that we would examine by-item random intercepts but instead examined by-belief domain random intercepts to consider the statement pairs rather than the individual statements. Given that the statements were designed in pairs and that participants were randomized to see only one statement, we were less interested in variation across each individual statement and were more interested in variation across the pairs of statements. By modeling belief domain as a random intercept, our results shed light on potential variation across statement pairs (belief domains).

We ran two sets of mixed-effects regression models for the main analyses. In the first set of models, political ideology and extremism were grand mean-centered so that the intercepts reflect the predicted value of belief updating when political ideology and extremism variables are at their means. In the second set of models, we standardized the fixed-effects independent variables to interpret the effect size. Analyses were conducted using the *lmerTest* (Kuznetsova et al., 2017) and *lme4* (Bates et al., 2015) packages in R Studio.

In a third set of exploratory models, we accounted for initial belief strength for the political statements as a fixed-effect independent variable (which was preregistered; Tables S5 and S6). We also conducted exploratory moderation analyses (described in the preregistration). We examined whether evidence-based belief updating was moderated by one's initial level of (dis)agreement with the statement, and we examined whether political extremism was more strongly related to evidence-based belief updating for those who identify as conservatives (vs. liberals; SM 1). In secondary analyses, we ran simultaneous mixed-effects regressions entering all ideology and extremism predictors (Table S7).

Results

Intercorrelations among variables are on the OSF repository. Across the four topics, 18%–25% of participants viewed statements as less accurate after viewing evidence, 11%–24% of participants did not change their views, and 58%–68% of participants viewed statements as more

accurate after viewing evidence. Thus, most participants updated their views in the evidence-based direction (Figures S7 and S8 display the histograms). Moreover, initial agreement with the statements did not significantly vary across statements (based on overlapping standard deviations; Figures S3–S5), suggesting that all the statements were initially viewed as equally plausible and that our pretest for selecting balanced items was successful (Figures S1 and S2).

For all belief domains, we also conducted paired samples *t*-tests and examined two-tailed *p*-values to clarify whether the mean accuracy ratings were significantly higher post-evidence than pre-evidence (see the OSF page). The paired samples *t*-test statistics were significant (*t*s ranged from 13.59 to 22.50, *df*s ranged from 1217 to 1236, *p*s < .001). The post-ratings were always significantly higher than the pre-ratings, indicating that participants updated their beliefs in the evidence-based direction for all belief domains (pre-rating means ranged from 38.43 to 52.87, post-rating means ranged from 61.54 to 71.47). Thus, participants generally perceived the statements as more accurate after viewing evidence.

Main effects

On the descriptive level, conservatives ($N = 1196$; $M = 17.73$, $SD = 24.28$) engaged in slightly less evidence-based belief updating for political statements than liberals ($N = 835$; $M = 20.43$, $SD = 22.86$; $t(2{,}029) = 2.53$, $p = .011$, $d = .11$; Table S16). Table 2 displays the mixed-effects regression results. The full output (including the random-effects and figures) is available on the OSF page. The main effects for the neutral and political filler statements are in Tables S10 and S11 and on the OSF repository.

Approximately 25% of the variance in belief updating was attributable to differences between participants (ICC = .25), whereas approximately 1% of the variance in belief updating was attributable to differences across belief updating domains (ICC = .01). The relationships between general and social conservatism (on the single-item ratings) and evidence-based belief updating were negative and significant, whereas the relationship between economic conservatism and belief updating was negative but not significant. For every standard deviation increase in conservatism, there was a .63 to 1.47 decrease in evidence-based belief updating on a −100 to 100 scale. Thus, general and social conservatism were related to less evidence-based belief updating, with the effects being quite small.

Conversely, endorsement of conservative political attitudes on specific political issues (the SECS) correlated positively with evidence-based belief updating, though the effects were not significant. For every standard deviation increase in conservatism on these political attitudes,

TABLE 2 Mixed-effects regression coefficients in Study 1.

Fixed-effects	*b* (*β*)	*p*	*df*	*SE*	95% CI for *b*
General political ideology	−.05** (−1.47)	.002	2452	.02	−.09, −.02
Social political ideology	−.05** (−1.37)	.004	2452	.02	−.08, −.01
Social political issues	.04 (.86)	.073	2452	.02	−.00, .08
Economic political ideology	−.02 (−.63)	.189	2452	.02	−.06, .01
Economic political issues	.04 (.61)	.204	2452	.03	−.02, .09
General political extremity	−.08** (−1.41)	.003	2452	.03	−.13, −.03
Social political extremity	−.01 (−.25)	.609	2452	.03	−.07, .04
Economic political extremity	−.03 (−.45)	.354	2452	.03	−.08, .03

**$p < .01$;

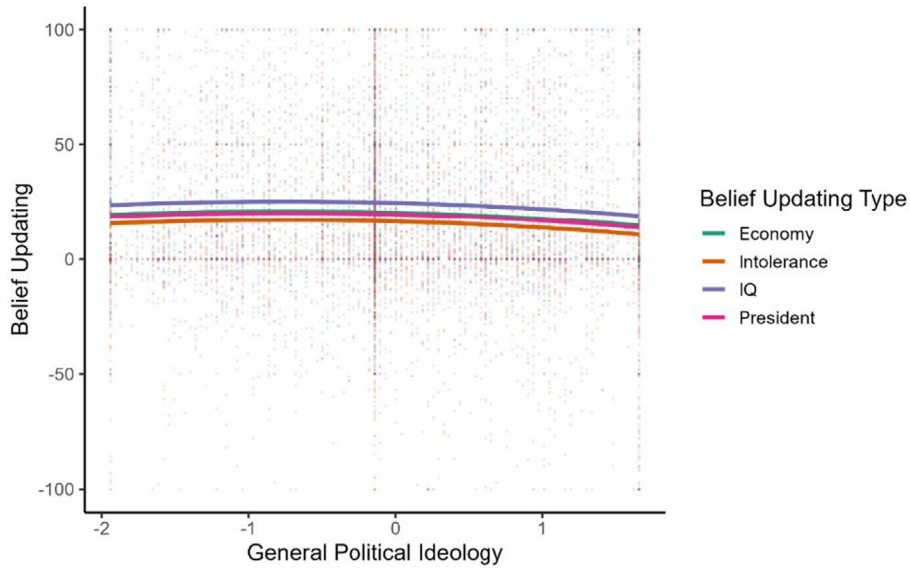

FIGURE 1 Quadratic relationship between general political ideology and evidence-based belief updating in Study 1. The values on the *x*-axis are grand mean-centered. $b = -.00$, $p = .011$.

there was a .61 to .86 increase in evidence-based belief updating. These associations were, however, non-significant, and the effects were weak. Thus, endorsement of conservative political attitudes was not related to evidence-based belief updating.

The relationships between political extremity and belief updating were negative, although the relationship was only significant for general political extremity (and this relationship remained significant when controlling for political ideology; Table S8). For every standard deviation increase in political extremity, there was a .25 to 1.41 decrease in evidence-based belief updating. These results indicate that general political extremity was related to less evidence-based belief updating, with the effects again being very small. Similarly, there was only evidence for a significant, small, and negative quadratic effect of general political ideology (Figure 1; Table S9). Evidence-based belief updating was lowest at the extremes of general political ideology, with effects being lowest at higher ends of conservatism.

Discussion

Study 1 effects for ideology and extremism were very small—a one standard deviation increase in conservatism or extremism related to a 1.5 point or less change in belief updating (on a −100 to 100 scale). The only significant effects were for general political ideology, social political ideology, and general political extremity, with relations being negative and indicating that conservatism and extremity correlated weakly with less evidence-based belief updating. Extreme conservatism was the strongest predictor of less evidence-based belief updating, though the effect was still small.

STUDY 2

In Study 2, we sought to replicate our results from Study 1 with two improvements: we oversampled political extremists to ensure we had a sufficient sample size at the ideological extremes,

and we increased the external validity of the scientific information to improve participant engagement with the material. The amount of scientific information and evidence presented was consistent with, or more detailed than, the broader belief updating literature (Sinclair et al., 2020; Vlasceanu et al., 2021). We also focused our analysis on political belief updating, given that there was minimal variability in effect sizes across statement type (the *b*s differed by an average of .05 for political filler statements and neutral filler statements compared with political statements; Tables S10 and S11).

Methods

Participants

Data were collected from U.S. participants via Prolific and CloudResearch from July to August 2022. The two platforms were used because Prolific had higher quality demographically representative samples than CloudResearch (which outsources their representative panels), and CloudResearch, but not Prolific, allowed for selection of political extremity. Via Prolific, our sample was nationally representative based on age, gender, and ethnicity quotas ($n = 2601$). To oversample the extreme ends of the political spectrum, we recruited a second sample from CloudResearch, restricting participation to those who reported being "very conservative" or "very liberal" ($n = 1529$). Data from the two platforms were merged.

We aimed to recruit 4000 participants total, but both platforms slightly overrecruited for a total of 4130. Participants who (a) failed the attention check ("please select 0 if you are still reading these questions"; $n = 345$), (b) provided an invalid/impossible age (e.g., age of 337; $n = 3$), and/or (c) completed less than 50% of the survey ($n = 56$) were removed from the dataset ($N_{final} = 3726$, 90% of original sample; $M_{age} = 42.54$, $SD_{age} = 15.16$; 33.0% conservative).

Procedure

Participants completed an online battery of self-report measures of political attitudes. Descriptive statistics and response scales are in Table S12. We also included a measure of political knowledge, given that political engagement and knowledge can contribute to more politically motivated reasoning (Guay & Johnston, 2022) and to adopting more constrained and consistent ideological views (Malka & Soto, 2015; Osborne et al., 2022). Participants also completed a self-report measure of lexically derived "-isms" that map onto political attitudes (Saucier, 2013). SM 2 and Tables S13 and S14 summarize these measures, their descriptives, and associated effects.

Political ideology

As in Study 1, participants rated their general, social, and economic political ideology on individual items and completed the SECS (*a*s = .93 [social] and .80 [economic]). We again calculated a binary "liberal" and "conservative" score in the same manner as in Study 1.

Political extremism

Political extremism was measured and calculated in the same ways as in Study 1.

Belief updating

There were 3 pairs of political statements that were also included in Study 1. The IQ statements from Study 1 were not included due to a computer error. Because we did not include the IQ

statements in Study 2, we ran sensitivity analyses in which we dropped the IQ statements from Study 1 (results are available on the OSF repository and in Table S15). None of the main effects changed in terms of statistical significance or direction, nor did the magnitude of the effects meaningfully change (change in *b*s ranged from .00 to .04). Thus, excluding the IQ statements does not meaningfully alter the pattern of results.

As with Study 1, upon viewing each statement, participants rated the accuracy of the statement on a 0 (*extremely inaccurate*) to 100 (*extremely accurate*) scale. Participants were randomly assigned to rate 3 of the 6 political attitudes (1 from each pair of statements) at the start of the survey. Participants then viewed evidence in support of each statement. Evidence was designed to appear like a blogpost, and each piece of evidence listed a source and evidence that supported the original statement (Figure 2). The blog titles were fictional to mitigate potential effects of familiarity and partisanship; blog titles were designed to be similar to titles of real journals and blogs (e.g., "Human Behavior Current Blog" was generated based on titles such as *Current Psychology*, *Nature Human Behavior*, and *Current Psychology Letters*). Consistent with Study 1, sources (selected from the pretest) were not randomized across statements. Thus, each statement pair had a unique (and consistent) source (e.g., for the intolerance statement pair, Ohio State University was the source for both statements).

After viewing the evidence, participants rated the same 3 statements again. The difference between the pre- and post-evidence ratings was computed such that a positive score reflected more evidence-based belief updating (scores ranged from −100 to 100). We again operationalize rigidity as less evidence-based belief updating. There were no nonpolitical or political filler statements in Study 2.

Results

Intercorrelations among variables for Study 2 are on the OSF repository. Across the four topics, 11%–17% of participants viewed statements as less accurate after viewing evidence, 22%–35%

FIGURE 2 Example evidence for the belief updating paradigm in Study 2.

of participants did not change their views, and 54%–67% of participants viewed statements as more accurate after viewing evidence. Thus, most participants updated their views in the evidence-based direction (Figures S9 and S10 display histograms). Mirroring Study 1, initial agreement with the statements did not significantly vary across the statements (Figure S6), suggesting that all statements were initially viewed as equally plausible.

For all belief domains, we again conducted paired samples *t*-tests. Consistent with Study 1, results revealed evidence of belief updating for all beliefs (*t*s ranged from 17.00 to 30.44, *df*s ranged from 1822 to 1876, two-sided *p*s < .001). The post-ratings were always significantly higher than the pre-ratings, indicating that participants updated their beliefs in the evidence-based direction (pre-rating means ranged from 42.51 to 64.16, post-rating means ranged from 56.00 to 75.72). Participants generally perceived the statements as more accurate after viewing evidence.

Main effects

On the descriptive level, conservatives ($N = 1114$; $M = 12.83$, $SD = 18.38$) engaged in slightly less evidence-based belief updating for political statements than liberals ($N = 2263$; $M = 14.69$, $SD = 18.98$; $t(3,373) = 2.73$, $p = .006$, $d = .10$; Table S16). Table 3 displays the mixed-effects regression results. The full output (including the random-effects and figures) is available on the OSF repository.

Consistent with Study 1, approximately 24% of the variance in belief updating was attributable to differences between participants (ICC = .24), and approximately 1% of the variance in belief updating was attributable to differences across belief updating domains (ICC = .01). Also consistent with Study 1, general, social, and economic conservatism (on the single-item ratings) had negative relationships with belief updating, but the effects were, again, only significant for general and social conservatism. For every standard deviation increase in conservatism on the single-item ratings, there was a .60 to .79 decrease in evidence-based belief updating on a −100 to 100 scale. Thus, the effects were very small. Altogether, these results reveal that general and social conservatism are weakly related to less evidence-based belief updating.

Endorsement of conservative political attitudes was negatively, albeit non-significantly, related to evidence-based belief updating. For every standard deviation increase in conservatism on the political attitudes, there was a .13 to .32 decrease in evidence-based belief updating. These associations were, however, non-significant, which corroborates the results from Study 1 and, again, demonstrates weak effects. In other words, endorsement of conservative political attitudes was not meaningfully related to evidence-based belief updating.

TABLE 3 Mixed-effects regression coefficients for political statements in Study 2.

Fixed-effects	*b* (*β*)	*p*	*df*	SE	95% CI for *b*
General political ideology	−.02* (−.67)	.029	3708	.01	−.04, −.00
Social political ideology	−.02* (−.79)	.011	3701	.01	−.04, −.01
Social political issues	−.00 (−.13)	.674	3703	.01	−.02, .02
Economic political ideology	−.02 (−.60)	.052	3697	.01	−.03, .00
Economic political issues	−.01 (−.32)	.294	3699	.01	−.04, .01
General political extremity	−.07*** (−1.24)	<.001	3705	.02	−.10, −.04
Social political extremity	−.06** (−1.04)	.001	3699	.02	−.10, −.32
Economic political extremity	−.09*** (−1.49)	<.001	3694	.02	−.12, −.05

*$p < .05$; **$p < .01$; ***$p < .001$.

Consistent with Study 1, the relationships between extremity and belief updating were negative, and all effects were significant (and effects were robust to covarying for political ideology; Table S8). For every standard deviation increase in political extremity, there was a 1.04 to 1.49 decrease in evidence-based belief updating. Hence, political extremity was weakly related to less evidence-based belief updating. Similarly, there was evidence for significant and negative quadratic effects of general, social, and economic political ideology (on the single-item ratings) and of economic political attitudes (Figure 3; Table S9). Evidence-based belief updating was lowest at the extremes of political ideology, with effects being stronger at the higher ends of conservatism; all quadratic effects were small, which is consistent with Study 1.

Discussion

Results mirrored Study 1, though often with smaller effects. A one standard deviation increase in conservatism or extremism related to a 1.5-unit or less change in belief updating (on a −100 to 100 scale). General and social conservatism (but not economic or social political attitudes) were weakly negatively related to evidence-based belief updating. All extremity measures were weakly related to less evidence-based belief updating, and those on the extreme right were least likely to update their beliefs (albeit this latter effect is still quite small).

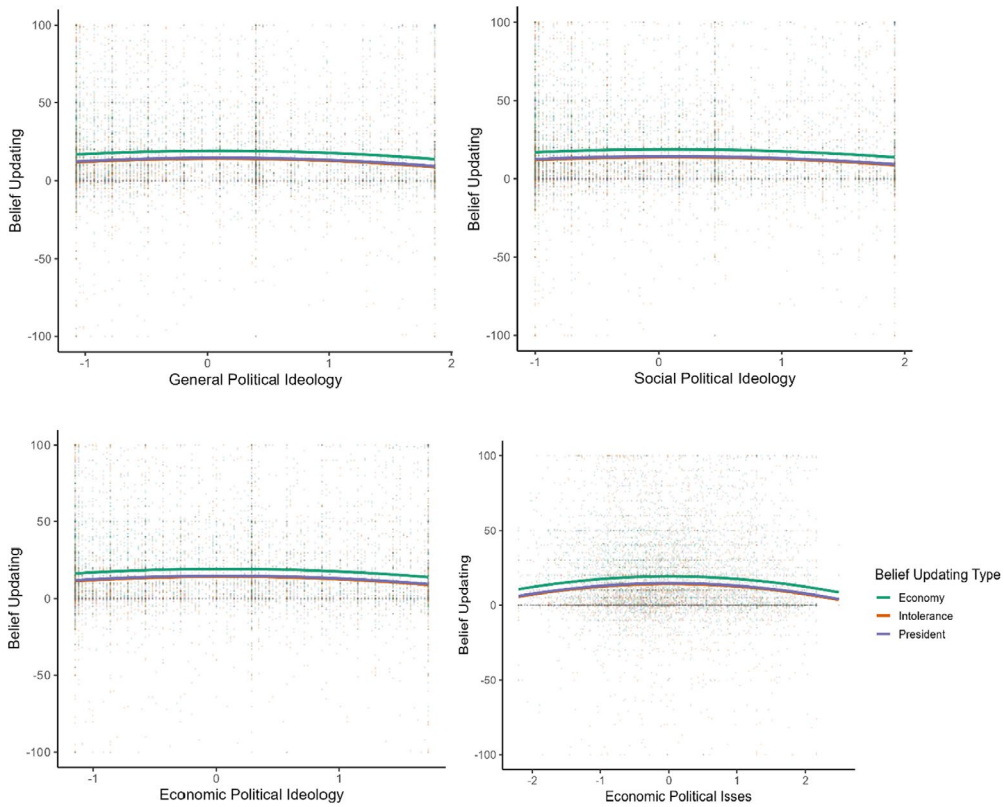

FIGURE 3 Quadratic relationships between political ideology and attitudes and evidence-based belief updating in Study 2. The values on the *x*-axis are grand mean-centered. *b*s were −.00, *p*s were <.001.

COMBINED ANALYSIS

To summarize the results, we combined data from Study 1 and Study 2 to estimate an overall effect of ideology and extremism on evidence-based belief updating ($N = 6181$). We used the same analytic approach described in Study 1 but added study as a random effect (the OSF page contains the full results). Only .6% of the variance in belief updating was attributable to between-study differences, suggesting that it was appropriate to combine the two studies (ICC = .006).

In the combined sample (Table 4), general, social, and economic conservatism (on the single-item ratings), in addition to all indices of political extremism, were significantly related to less evidence-based belief updating. Conversely, economic and social conservatism on the attitudinal measures were not significantly related to belief updating. Every one standard deviation increase in conservatism (self-placement ratings) corresponded to a .63 to 1.04 point decrease in evidence-based belief updating on a −100 to 100 scale. Effects for extremism were relatively larger, as every one standard deviation increase in extremism corresponded to a decrease of 1.03 to 1.53 in evidence-based belief updating. Overall, these effect sizes are very small.

Additionally, the quadratic relationships between all measures of ideology on the self-placement ratings and economic political attitudes and belief updating were significant and negative (Figure 4). Evidence-based belief updating was lowest at the extremes of political ideology (Table S9). When combining Studies 1 and 2, however, there was less consistent evidence that effects were more pronounced at the extremes of conservatism. For example, although extreme conservatives on both general and social ideological measures were the least likely to update their beliefs, the effects of economic attitudes on evidence-based belief updating were more pronounced at the extremes of liberalism. Finally, extremes on the left and right of economic ideological self-placement seemed equally resistant to belief updating. All quadratic effects, however, were quite small.

GENERAL DISCUSSION

Two studies tested the relationships ideology and extremity had with belief rigidity (operationalized as less evidence-based belief updating). General and social conservatism related to rigidity at roughly $|b| = .05$, and conservatives were slightly more rigid than liberals (Cohen's $d = .05$;

TABLE 4 Mixed-effects regression coefficients for the combined samples.

Fixed-effects	*b* (*β*)	*p*	*df*	SE	95% CI for *b*
General political ideology	−.03*** (−.95)	<.001	6100	.01	−.05, −.01
Social political ideology	−.03*** (−1.04)	<.001	6085	.01	−.05, −.01
Social political issues	.00 (.15)	.606	5668	.01	−.01, .03
Economic political ideology	−.02* (−.63)	.027	6049	.01	−.04, −.00
Economic political issues	−.01 (−.13)	.645	6088	.01	−.03, .02
General political extremity	−.08*** (−1.53)	<.001	5676	.02	−.11, −.05
Social political extremity	−.06*** (−1.03)	<.001	5748	.02	−.09, −.03
Economic political extremity	−.08*** (−1.34)	<.001	6014	.02	−.11, −.04

*$p < .05$;
***$p < .001$.

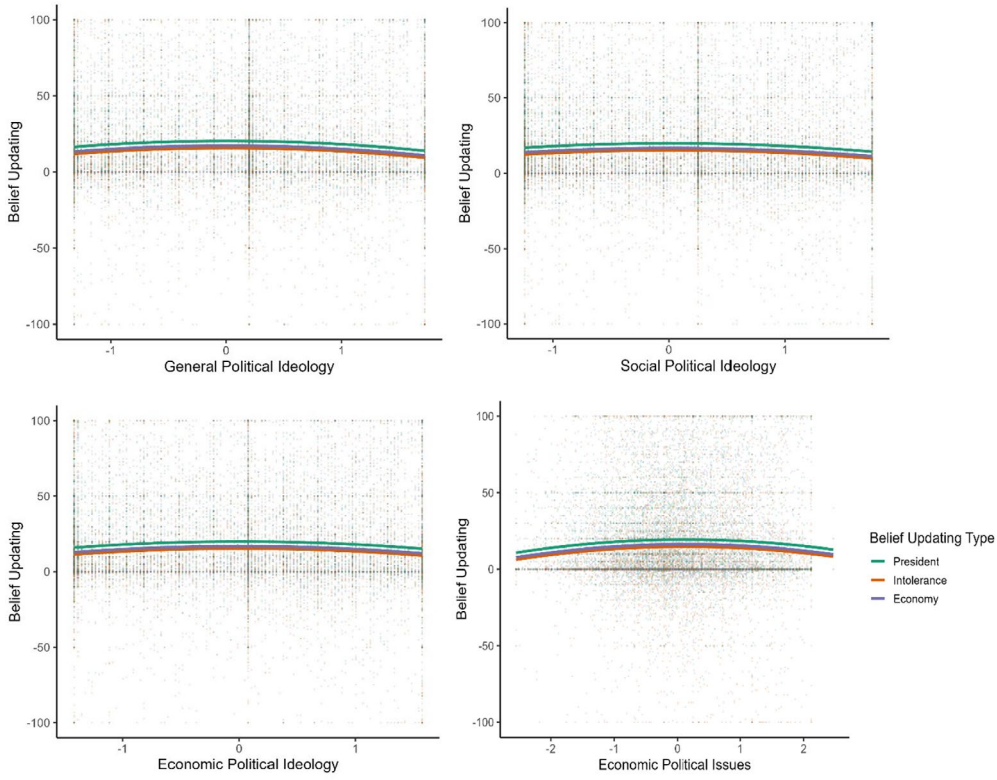

FIGURE 4 Quadratic relationships between political ideology and attitudes in the combined sample. The values on the x-axis are grand mean-centered. *b*s were −.00, *p*s were <.001.

Table S16). Thus, general conservatism, social conservatism, and identifying as conservative correlated with less evidence-based belief updating. Although these effects are smaller than a "small" effect and sometimes displayed equivocal significance, they offer theoretically meaningful support for the rigidity-of-the-right hypothesis because (a) patterns were consistent in both studies and across three operationalizations of conservatism; (b) we selected balanced materials and, thus, subjected this hypothesis to a stringent test; (c) no significant support emerged for the *reverse* conclusion (i.e., that left-leaning ideologies correlated with more rigidity); and (d) extreme rightists were more rigid than extreme leftists in 60% of quadratic relationships. Some scholars may, however, view these results as supporting the symmetry hypothesis because rigidity generally had null associations with economic ideology, social political attitudes, and economic political attitudes, and effect sizes fell within a small confidence band approaching zero.

Our adversarial team agrees on the size and direction of these effects. Thus, the overarching conclusion among our adversarial team is as follows. Centrists/moderates displayed the least rigidity. If any particular ideological group was especially rigid, it was those on the right and especially those on the far right. However, the relationships between rigidity and conservatism did not meet minimum thresholds for small effects and were not consistent across all operationalizations of ideology. Consequently, the practical utility of the relationship between ideology and rigidity in the form of less evidence-based belief updating is questionable. However, more robust relationships between ideology and rigidity likely exist in narrow contexts, such as when the information under consideration regards a particular hot button issue for a particular ideological group. For instance, the degree to which salient issues activate greater internal

value conflicts in one political party than another may moderate the relationship between ideology and belief rigidity—a higher value conflict issue for one party may lead to greater efforts to find integratively complex solutions among those in that party (e.g., Tetlock, 1986). Carefully cataloging these domain-specific differences would allow political psychologists to formulate better explanations and predictions than focusing on broad, domain-general relationships between ideology and rigidity that are very weak and only present in some contexts.

Our adversarial team understands how our results could yield different interpretations regarding the theoretical or practical importance of the rigidity-of-the-right hypothesis or the symmetry thesis. Some scholars, and perhaps especially rigidity-of-the-right proponents, will view our results as providing reasonably strong support for the rigidity-of-the-right hypothesis. Yet other scholars, perhaps especially symmetry theorists, will view our results as providing reasonably strong support for the symmetry hypothesis.

The ambiguity in these interpretations produces (at least) two meaningful insights. First, our results help illustrate *why* research on the ideology/rigidity relationship has been so contentious for so long. Given the total possible distribution of relationships between conservatism and rigidity, high-quality studies conducted in earnest will sometimes discover seemingly strong evidence for or against the rigidity-of-the-right hypothesis. Second, given the low practical utility of social and economic conservatism as a predictor of rigidity in this context, scholars' time would likely be better spent examining the contexts that more reliably predict rigidity. For example, here, we confirmed that the conservatism/rigidity relation is more reliable for social than economic conservatism (Costello et al., 2023). This finding provides a promising hypothesis for future research: those moderately high in social liberalism and economic conservatism (i.e., moderate libertarians) might be the least rigid. Although we do not know for sure why social conservatism produces more rigidity than economic conservatism, it likely stems from the larger epistemic and social motives inevitably enmeshed in restrictive social beliefs. Social conservatism—unlike economic conservatism—is often based on a more transcendent (as opposed to pragmatic) epistemological motivational framework. As such, it is likely especially prone to more dogmatic and rigid thinking. Thus, social conservatism maps most clearly onto the original framing of the rigidity-of-the-right theorizing, which focused on entrenched social motives and not on practical economic ones (Jost et al., 2003).

Political extremity at both ends of the ideological spectrum often correlated negatively with evidence-based belief updating (though more so among conservatives), corroborating work showing that political extremity relates to higher belief confidence (Toner et al., 2013; Van Prooijen & Krouwel, 2019). In Study 1, general political extremism (but not social and economic extremism) predicted rigidity, but all three extremism measures were significant in Study 2 (average $|bs| \sim .07$), in which we specifically recruited larger samples of extremists for more reliable estimates. These relationships also fell below "small" effects and sometimes displayed equivocal significance, suggesting there could be important moderators of these effects. Identifying these moderators will be a fruitful area for future research. For example, our results suggest that socially conservative extremists and economically liberal extremists might be the most rigid. Moreover, our results were specific only to comparatively extreme attitudes on a political spectrum among regular citizens. Future research may expand the current findings by examining other, more radical forms of extremism, including violent extremism or membership in radical groups (e.g., Kruglanski et al., 2019).

Limitations and future directions

Although we used validated measures of social and economic conservatism, the economic conservatism scale had low reliability in Study 1. Results were, however, generally consistent with Study 2 when our same measure achieved higher reliability. Although adversaries

considered the belief updating paradigm to be the best operationalization of rigidity, it is not without faults nor able to capture all potential operationalizations of cognitive rigidity and belief rigidity. For example, belief updating for congruent information may encounter ceiling effects, whereas belief updating for incongruent information may elicit demand characteristics (see SM 1). Many experts may also be more interested in individual differences in rigidity rather than behavioral indicators of rigidity. There are also an infinite number of potential statements that could be used in a belief updating paradigm. Although our results were generally consistent across political and non-political topics (Tables S10 and S11), speaking to the potential generality of the results, it may be worthwhile for future research to test these hypotheses on a wider range of topics and statements.

Our samples were also limited to U.S. participants in 2022, capturing a snapshot of a particular political moment. Although this approach was in line with earlier research on the link between political ideology and rigidity (which also tended to rely on U.S. samples), this does raise questions about generalizability. For instance, the meaning of political ideology can vary across national contexts and change over time. In addition, even within the U.S., both integrative complexity and asymmetries in integrative complexity can change depending on which political party is in an opposition role versus a policy-making role (e.g., Tetlock et al., 1984). We suspect that adding these complexities will further undermine broad and generic claims about the link between political ideology and rigidity. For instance, rigidity may be relatively high at the political left in countries with a long history of communism (e.g., De Regt et al., 2011). Future research should therefore examine the generalizability of our results beyond this unique country or zeitgeist. Similarly, our cross-sectional data limit causal inference and cannot indicate whether participants retained their updated beliefs. Longitudinal work should examine potential ideological (a)symmetries in the persistence of updated beliefs over longer stretches of time.

Our results illustrate the difficulty of investigating top-down explanations for complex phenomena such as political ideology. Testing three paradigms required refining definitions of psycho-political phenomena (i.e., rigidity, ideology, extremism). Though useful for conducting rigorous, risky hypothesis tests, such pruning also incurs costs. Given our small effects, there are almost certainly alternative operationalizations of rigidity, ideology, and extremism that would produce different results. Integrative experimental designs (Almaatouq et al., 2022) may offer fertile soil for developing new, precise theories to replace the old, overly broad ones.

The current project underscores the utility of adversarial collaborations for scientific progress. By increasing the likelihood of implementing unbiased methods (Clark et al., 2022), theories are subjected to critical tests of their core underlying tenets. But these results also demonstrate a challenge to widespread implementation: adversarial collaborations will tend to produce small and nuanced effects (as discovered here), whereas science incentivizes big and broad findings (Clark & Tetlock, 2023). If scholars pursue adversarial collaborations at higher rates in the future, metascience research could test whether adversarial collaborations—like replication studies (Camerer et al., 2018)—produce smaller effects than earlier papers by both adversaries. Such results would suggest that adversarial collaborations constrain the cherry-picking of methods that inflate effect sizes and that the scientific community should incentivize high-quality work that produces accurate, not exaggerated, conclusions.

The size and nuance of our results raise questions for future research about why previous scholars have reported much larger and more consistent effects. One idea would be to conduct a thorough audit of alternative operationalizations of rigidity among a theoretically diverse team of experts to assess which characteristics predict larger effect sizes. For example, metrics and operationalizations widely regarded as confounded with ideology itself may explain some especially robust relationships. We hope future work will explore these and other possibilities.

CONCLUSION

The psychological literature includes many strong but discordant claims about the relationship between political ideology and rigidity (conservatism uniquely predisposes one to rigidity; liberalism and conservatism equally predispose one to rigidity; extreme ideological beliefs predispose one to rigidity). These claims may affect not only future hypothesis testing in psychological science but also public perceptions of different political identities. The current adversarial collaboration clarifies inconsistencies in the literature across multiple operationalizations of ideology and belief domains and a mutually agreed upon evidence-based belief updating paradigm to assess rigidity. Our results revealed very small and only semi-consistent support for the rigidity-of-the-right and rigidity-of-extremes hypotheses, calling into question the practical importance of ideological differences in rigidity in this context. Our key takeaway is that researchers (including us) may need to update our beliefs on the relationship between ideology and rigidity and move away from asking *who* is more rigid and toward examining *when* and *where* political ideology and extremism predict rigidity.

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DATA AVAILABILITY STATEMENT

The data that support the findings of this study are openly available in OSF at https://osf.io/ngwpk/, reference number DOI: 10.17605/OSF.IO/NGWPK.

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SUPPORTING INFORMATION

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

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