

Rethinking the Link Between Cognitive Sophistication and Politically Motivated Reasoning

Ben M. Tappin
Massachusetts Institute of Technology

Gordon Pennycook
University of Regina

David G. Rand
Massachusetts Institute of Technology

Partisan disagreement over policy-relevant facts is a salient feature of contemporary American politics. Perhaps surprisingly, such disagreements are often the greatest among opposing partisans who are the most cognitively sophisticated. A prominent hypothesis for this phenomenon is that cognitive sophistication magnifies politically motivated reasoning—commonly defined as reasoning driven by the motivation to reach conclusions congenial to one’s political group identity. Numerous experimental studies report evidence in favor of this hypothesis. However, in the designs of such studies, political group identity is often confounded with prior factual beliefs about the issue in question; and, crucially, reasoning can be affected by such beliefs in the absence of any political group motivation. This renders much existing evidence for the hypothesis ambiguous. To shed new light on this issue, we conducted three studies in which we statistically controlled for people’s prior factual beliefs—attempting to isolate a direct effect of political group identity—when estimating the association between their cognitive sophistication, political group identity, and reasoning in the paradigmatic study design used in the literature. We observed a robust direct effect of political group identity on reasoning but found no evidence that cognitive sophistication magnified this effect. In contrast, we found fairly consistent evidence that cognitive sophistication magnified a direct effect of prior factual beliefs on reasoning. Our results suggest that there is currently a lack of clear empirical evidence that cognitive sophistication magnifies politically motivated reasoning as commonly understood and emphasize the conceptual and empirical challenges that confront tests of this hypothesis.


Keywords: politically motivated reasoning, cognitive sophistication, prior beliefs, coherence

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Partisan disagreement is a salient feature of contemporary American politics (Pew Research Center, 2019). Disagreements

are observed not only in people’s political preferences and values, but also in their “factual” beliefs—that is, beliefs regarding policy-relevant facts that one might expect to converge on the best publicly available and relevant empirical evidence. Canonical examples include partisan disagreement over the performance of the U.S. economy (Bartels, 2002; Dunn & Oliphant, 2018), the existence of weapons of mass destruction at the time of the 2003 American invasion of Iraq (Bullock, 2009; Nyhan & Reifler, 2010), and the danger to human health posed by global warming, private gun ownership, and fracking (Kahan, 2015). These and other such issues are “factual” in the sense that, although complex, they have answers that are verifiable-in-principle, and that largely turn (or should turn) on empirical evidence—rather than on people’s political preferences or values (Kahan, 2016).

Much evidence suggests that disagreement over policy-relevant facts often tends to be largest among the most cognitively sophisticated opposing partisans. The foremost example of this is a body of U.S. survey data that shows that educational attainment positively correlates with belief in human-caused climate change among individuals who identify on the political left, but less positively correlates—or even negatively correlates—with belief among those who identify on the political right (Bolin & Hamilton,

 Ben M. Tappin, Sloan School of Management, Massachusetts Institute of Technology; Gordon Pennycook, Hill/Levene School of Business, University of Regina; David G. Rand, Sloan School of Management and Department of Brain and Cognitive Sciences, Massachusetts Institute of Technology.

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Correspondence concerning this article should be addressed to Ben M. Tappin, Sloan School of Management, Massachusetts Institute of Technology, 100 Main Street, Cambridge, MA 02142. E-mail: benmtappin@googlemail.com

2018; Drummond & Fischhoff, 2017b; Ehret, Sparks, & Sherman, 2017; Hamilton, 2011, 2016; McCright & Dunlap, 2011; van der Linden, Leiserowitz, & Maibach, 2018). Consequently, the strongest partisan disagreement tends to be observed between the most educated citizens, those who would seem best equipped to parse the relevant empirical evidence and approach bipartisan consensus on the facts of the issue.

Although educational attainment is a crude indicator of cognitive sophistication, the same pattern of widening disagreement has been observed when researchers examine more specific cognitive indicators, such as science literacy and science intelligence, numeracy, domain knowledge, and measures of open-minded and analytic thinking (Bolsen, Druckman, & Cook, 2015; Drummond & Fischhoff, 2017b; Hamilton, Cutler, & Schaefer, 2012; Kahan et al., 2012; Kahan, 2015; Kahan, Landrum, et al., 2017; Kahan, 2017; Kahan & Corbin, 2016; Malka, Krosnick, & Langer, 2009). In addition, the pattern has been observed on issues other than climate change, such as people's belief in evolution by natural selection (Drummond & Fischhoff, 2017b; Hamilton & Saito, 2015; Joslyn & Haider-Markel, 2014), the safety of vaccination (Hamilton, Hartter, & Saito, 2015; Joslyn & Sylvester, 2019; Sarathchandra, Navin, Largent, & McCright, 2018), and the risks associated with fracking (Kahan, 2015; Kahan, Landrum, et al., 2017). In all of these cases, the most cognitively sophisticated opposing partisans tend to be the furthest apart in their beliefs about the relevant facts of the matter.

Cognitive Sophistication and Politically Motivated Reasoning

A prominent explanation for this pattern is that cognitive sophistication magnifies politically motivated reasoning. *Politically motivated reasoning* is typically conceived of as reasoning affected by the motivation to arrive at a conclusion that is *congenial*—normative, preferable, desirable—for the political group (e.g., Republican Party) or ideology (e.g., conservative) with which a person identifies (Bolsen & Palm, 2019; Druckman, 2012; Kahan, 2016; Leeper & Slothuus, 2014; Petersen, Skov, Serritzlew, & Ramsøy, 2013). There are several distinct theories about the latent cause of such a motivation. One influential theory is that people experience psychological and material utility from conforming to the ingroup position, and disutility from deviating (Kahan, 2016; Petersen et al., 2013; Van Bavel & Pereira, 2018). Thus, the motivation is driven by conformity incentives; to “toe the party line” for its own sake, to obtain psychological and material rewards (and avoid costs) imposed by ingroup members and by group identification per se. Although there are other theories for why people may be motivated to reason in ways that are congenial for their political group identity, the important point to note is that political group identity is typically treated as an observed proxy for the *unobserved* latent cause of politically motivated reasoning—however that latent cause may be defined. For simplicity, we refer to this latent cause as *political group motivation*.

The results of numerous recent studies suggest that cognitive sophistication could magnify politically motivated reasoning (Guay & Johnston, 2020; Kahan, 2013; Kahan, Peters, et al., 2017; Kuru, Pasek, & Traugott, 2017; Nurse & Grant, 2019; Sumner, Scofield, Buchanan, Evans, & Shearing, 2018; Taber, Cann, & Kucsova, 2009; Taber & Lodge, 2006). In one study (Kahan,

2013), for example, U.S. partisans were asked to evaluate the validity of the Cognitive Reflection Test (CRT), a behavioral measure of the propensity and ability to think analytically (Frederick, 2005; Pennycook, Cheyne, Koehler, & Fugelsang, 2016). The CRT was described to partisans as a test of “open-minded and reflective” thinking. Before giving their evaluations, partisans first completed the test themselves and were randomly assigned to one of two treatments (or control) in which they were provided information about the test. In Treatment A, partisans were told that people who believe that climate change is happening tend to score higher on the test than people who are skeptical that climate change is happening; implying the former are more open-minded. In Treatment B, partisans were told the reverse: that people who are skeptical that climate change is happening tend to score higher in the test, implying *they* are more open-minded.

On average, partisans who identified on the political left rated the test as more valid in Treatment A than B, and vice versa for partisans who identified on the political right. The key result, however, was that these conditional evaluations tended to be strongest among those partisans who scored the highest on the CRT. In other words, the most cognitively sophisticated opposing partisans tended to disagree most strongly in their evaluations of the validity of the test. A similar pattern has been observed in studies that use the same general design structure, but that used indicators of cognitive sophistication other than CRT performance, such as political knowledge (Guay & Johnston, 2020; Kuru et al., 2017; Taber et al., 2009; Taber & Lodge, 2006) or numeracy (Guay & Johnston, 2020; Kahan, Peters, et al., 2017; Nurse & Grant, 2019); in studies that used measures of political attitudes in lieu of political identity per se (Kuru et al., 2017; Taber et al., 2009; Taber & Lodge, 2006); as well as across several different political issues and outcome variables therein.

As mentioned above, these data are offered as evidence that cognitive sophistication magnifies politically motivated reasoning. The underpinning logic being that cognitively sophisticated partisans are better “equipped” (cognitively) to reason about new information in such a way so as to reach conclusions that are congenial to their political group identities; that they selectively deploy these distinct capacities when reasoning in order to reach such conclusions; and, thus, that they are “particularly susceptible” to politically motivated reasoning (Taber et al., 2009, p.139). In the next section, we critically examine the main evidence underlying this inference—for brevity, referred to as the *magnification inference*—and we suggest that this evidence is less diagnostic than is often implied.

Rethinking Cognitive Sophistication and Politically Motivated Reasoning

The magnification inference rests on a key assumption: in study designs like those described, the correlation between people's political group identity and their patterns of reasoning reveals politically motivated reasoning. To critically examine this assumption and see why it is key, we first describe the general structure of these study designs. The designs are considered “paradigmatic” insofar as they appear repeatedly in the research literature, and are taken to provide some of the clearest evidence of politically motivated reasoning in both psychology and political science (e.g., Flynn, Nyhan, & Reifler, 2017; Kahan, 2016).

The design entails randomly assigning people to receive one of two pieces of information, holding constant the substantive detail of the information across treatments while varying its implication for their political identities between treatments. The outcome variable is typically people's self-reported evaluations or interpretations of the new information. The studies described earlier all use this general design structure, and the basic and robust result from this design is that people's information evaluations differ between treatments, and this difference is correlated with their political group identities. More specifically, on average, people evaluate otherwise-identical information less favorably when it is *uncongenial* for their political identities than when it is congenial for their political identities. The key assumption is that this pattern reveals politically motivated reasoning. Insofar as this assumption holds, it is valid to make the subsequent magnification inference based on data from this type of study design: namely, that cognitive sophistication magnifies politically motivated reasoning.

However, this assumption is undermined by the fact that political group identity is typically correlated with other variables that can cause the same pattern of results, but that do not implicate an effect of political group motivation. In particular, people's political group identity is typically correlated with their prior "factual" beliefs about the specific issue under study. These prior beliefs are "factual" in the same sense as earlier: they refer to propositions about states of the world, upon which empirical evidence may (at

least in principle) be brought to bear—they are not preferences or values. Furthermore, in the same way that political group identity is often treated as an observed proxy for the unobserved latent cause of politically motivated reasoning, prior factual beliefs may be regarded as observed proxies for people's unobserved political information environment—comprising, for example, their exposure to media, discussions with friends, family, coworkers, and so on. In the next section, we explain why the nature of the correlation between political group identity and prior factual beliefs is crucial to understanding whether and how cognitive sophistication could magnify politically motivated reasoning.

Why Prior Beliefs Undermine Inferences of Politically Motivated Reasoning

To aid our explanation, in Figure 1 is a causal diagram (Pearl & Mackenzie, 2018) of politically motivated reasoning in the paradigmatic study design. The nodes represent the key variables, and the edges represent causal influences (the arrows show the direction of influence). The variable "new information" represents random assignment to one or the other piece of information in the paradigmatic study design (as described in the Rethinking Cognitive Sophistication and Politically Motivated Reasoning section). The diagram is an oversimplification and is used primarily to illustrate the challenge faced in testing whether cognitive sophis-

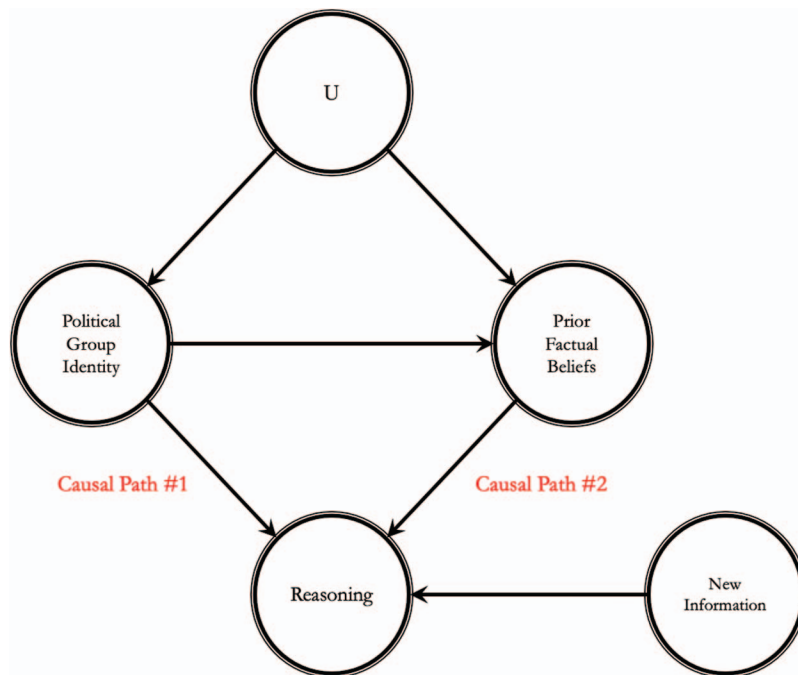


Figure 1. Causal diagram of politically motivated reasoning in the paradigmatic study design. Nodes represent variables and edges represent causal relationships (arrows indicate the direction of influence). U denotes unobserved variables that plausibly influence both political group identity and prior factual beliefs (e.g., one's political information environment, including exposure to media, and discussions with friends, family, coworkers, and so on.). The interaction between new information and either (i) political group identity or (ii) prior factual beliefs (as described in the text) is represented simply by the fact that the two variables point to the same outcome variable (reasoning). This representation communicates that the relevant variables jointly cause the outcome variable, and may do so according to some functional form (such as linear interaction). See the online article for the color version of this figure.

tication magnifies politically motivated reasoning. For example, in the below exposition we refer to “direct” and “indirect” effects and “causal” pathways. For ease of exposition, we temporarily put aside discussion of the important question of whether the causal diagram in Figure 1 is entirely realistic (it’s not); and, thus, whether simple correlations between political group identity, prior factual beliefs, and reasoning precisely identify causal effects (they do not). We revisit these points in our Discussion section. Bearing that in mind, the diagram shows two ways in which politically motivated reasoning could operate in this design.

First, the information could interact with political group identity to affect reasoning through a direct effect of political group motivation (Causal Path 1). As a concrete example of this pathway, consider a U.S. citizen who supports the Democratic Party. Now imagine two counterfactual worlds, representing the information treatments of the study: in one world, she receives information that stricter gun control laws reduce gun crime; whereas, in the other world, she receives information that such laws do *not* reduce gun crime (the information is otherwise identical). Causal Path 1 implies that she would evaluate the former information more favorably, because it supports the explicit goals of her political group (Democratic Party, enacting stricter gun control laws). As described earlier, this could be because she experiences psychological and material incentives to conform to the position associated with her political group identity (Kahan, 2016; Petersen et al., 2013; Van Bavel & Pereira, 2018).

Importantly, this path also implies that her political group identity influences her reasoning regardless of her prior factual beliefs about the specific issue in question (whether stricter gun control laws actually reduce U.S. gun crime). For example, if she supported the Democratic Party but nevertheless believed that stricter gun control laws were unlikely to be effective in reducing gun crime, she would still be expected to evaluate the former information more favorably; because it supports the explicit goals of her political group, despite it being incoherent with her prior factual belief (skepticism about the efficacy of such laws).

The second path through which politically motivated reasoning could operate in the paradigmatic design is Causal Path 2: The new information interacts with a person’s prior factual belief about the issue to affect their reasoning. In this case, across the two counterfactual worlds the new information is either coherent or incoherent with the person’s prior factual belief. Considering again a U.S. citizen who believes that stricter gun control laws are unlikely to reduce U.S. gun crime: She would evaluate the information coherent with this belief (stricter laws do not reduce gun crime) more favorably than the otherwise-identical information that is incoherent with the belief (stricter laws do reduce gun crime). Importantly, an inference of politically motivated reasoning in this case depends entirely on the assumption that the prior factual belief is itself caused by political group motivation (represented by the path: political group identity \rightarrow prior factual belief). That is, the required assumption is that the effect of the prior factual belief on reasoning is (actually) an indirect effect of political group motivation.

This is a strong assumption because there exist numerous variables that are plausible common causes of both political group identity and prior factual beliefs about specific issues (represented by U in Figure 1): such as one’s political information environment, including exposure to media, discussions with friends, family, and

coworkers, and the resulting path-dependent and self-reinforcing perceptions about which sources of information are trustworthy and should thus be listened to (vs. ignored). In other words, that is, political group identity and prior factual beliefs are confounded by common causes.

Concretely, this means that the oft-observed correlation between political group identity and prior factual beliefs about specific issues does not necessarily reveal that the former causes the latter.¹ By extension, observing an effect of prior factual beliefs on reasoning does not necessarily reveal an indirect effect of political group motivation, either. Determining the extent to which such an observation might reveal an indirect effect of political group motivation is a very difficult problem, because the myriad confounding variables in U are challenging to measure and therefore typically remain unobserved. As a result, it is difficult to estimate the extent to which a person’s prior factual beliefs about specific issues were caused by their political group motivations versus the two variables simply share common causes. This estimate is required, however, to infer politically motivated reasoning via Causal Path 2 with reasonable confidence.

This problem of identification is compounded by the fact that there exist plausible alternative explanations and evidence for why prior factual beliefs may affect reasoning (i.e., alternative to an indirect effect of political group motivation). For example, numerous studies show that the coherence between new information and prior factual beliefs commonly affects reasoning in contexts where political group motivation is entirely absent, and thus by definition cannot be attributed to politically motivated reasoning (Evans, Barston, & Pollard, 1983; Klauer, Musch, & Naumer, 2000; Koehler, 1993; Markovits & Nantel, 1989; Trippas, Pennycook, Verde, & Handley, 2015).

As a concrete illustration, consider an experiment in which Koehler (1993) randomly assigned subjects to read two scientific studies in which the results either supported or undermined a fictitious (and apolitical) hypothesis. Before reading about the studies, subjects were randomly assigned separate information about whether the hypothesis was likely to be true or not, causing them to form different prior beliefs about its veracity. Koehler (1993) found that subjects tended to evaluate the quality of the studies less favorably if the results were incoherent (vs. coherent) with their randomly assigned prior beliefs about the veracity of the hypothesis.

This and other such evidence implies that the tendency for reasoning to be affected by the coherence between new information and prior factual beliefs is a feature of human psychology that is independent of political group motivation. And, indeed, this seemingly general human tendency is well-captured by other theoretical frameworks, such as the theory of “epistemic vigilance,” which holds that skepticism of new information that is incoherent (vs. coherent) with prior beliefs forms part of a suite of adaptive cognitive mechanisms that guard against the individual being too

¹ Another explanation for the oft-observed association between political group identity and prior factual beliefs about issues is that the latter causes the former. That is, people may adopt political group identities that cohere with their specific prior beliefs about issues (Fowler, 2020b). For simplicity, we do not consider this possibility in the causal diagram in Figure 1 but note that it represents an additional complication for inferences of politically motivated reasoning via Causal Path 2 in the paradigmatic design.

easily manipulated into holding false or otherwise-costly beliefs by other people (Mercier, 2017, 2020; Sperber et al., 2010).

In sum, the preceding discussion illustrates that Causal Path 1 provides clearer evidence of politically motivated reasoning than does Causal Path 2. To reiterate, this is because political group identities are correlated with (but do not necessarily cause) prior factual beliefs about specific issues, and the latter variable demonstrably affects reasoning in the absence of political group motivation, and for plausible reasons that are unrelated to political group motivation.

Study designs that ignore the correlation between political group identity and prior factual beliefs can therefore only provide ambiguous evidence of politically motivated reasoning, because it is unclear whether they estimate Causal Path 1 or Causal Path 2. In contrast, study designs that can show that people's reasoning correlates with their political group identity *regardless* of whether the information is coherent or incoherent with their prior factual beliefs about the issue—Causal Path 1 specifically—provide stronger evidence of politically motivated reasoning. This broad logic has long been recognized by scholars of motivated reasoning in general (Ditto, 2009; Kunda, 1990; Tappin, van der Leer, & McKay, 2017; Tappin & Gadsby, 2019; Tetlock & Levi, 1982), and politically motivated reasoning in particular (Ditto et al., 2019; Friedman, 2012; Gerber & Green, 1999; Kahan, 2016; MacCoun & Paletz, 2009; Tappin, Pennycook, & Rand, 2020).

For example, MacCoun and Paletz (2009) studied U.S. citizens' reactions to research findings about the policy issues of gun control, medicinal marijuana, the death penalty, and school vouchers. They found that citizens were more skeptical of research findings that were uncongenial (vs. congenial) for their political group identities on these issues, even after accounting for whether the research findings were incoherent or coherent with their specific prior beliefs about the issues. The authors concluded that this result is difficult to explain under a model based on prior beliefs, and thus "appears to be an actual political bias" (p. 55). Results such as this also suggest that political group identities and prior factual beliefs about specific issues are not one and the same but are meaningfully distinct and have empirically distinguishable effects. As another case in point, while the correlation between political group identity and issue-specific beliefs tends (of course) to be positive, it is typically not large, at least in the American population (Baldassarri & Gelman, 2008). This suggests that prior factual beliefs about issues may often be at odds with political group identities.

Implication for the Magnification Inference

As the previous section showed, it is unclear whether the patterns of reasoning commonly observed in the paradigmatic design reveal politically motivated reasoning, because the design typically conflates political group identity with prior factual beliefs about specific issues. This has important implications for the hypothesis that cognitive sophistication magnifies politically motivated reasoning, because the primary evidence supporting that hypothesis comes from studies that use the same type of design. Consequently, it remains unclear from this evidence whether cognitive sophistication magnifies a direct effect of political group motivation on reasoning (i.e., Causal Path 1) or a direct effect of prior factual beliefs on reasoning (Causal Path 2). By extension, it is

unclear how confident we should be in the hypothesis that cognitive sophistication magnifies politically motivated reasoning, because, as shown in the preceding discussion, the former path provides stronger evidence for this hypothesis than the latter path.

Summary of Current Studies

To provide greater clarity, we conduct three studies in which we measure and statistically control for people's prior factual beliefs about the issue in question when estimating the association between their cognitive sophistication, political group identity, and patterns of reasoning in the paradigmatic design; similar to the approach of MacCoun and Paletz (2009).

This approach lets us ask the critical question of interest: whether cognitive sophistication magnifies an effect of political group identity on reasoning—an attempted replication of previous results—while holding constant any corresponding magnification of the effect of prior factual beliefs on reasoning. In other words, we ask whether cognitive sophistication magnifies a *direct* effect of political group identity on reasoning (Causal Path 1). We also ask the reverse question: whether cognitive sophistication magnifies a direct effect of prior factual beliefs on reasoning; that is, holding constant people's political group identity (Causal Path 2). Overall, then, we test for whether cognitive sophistication moderates Causal Path 1, Causal Path 2, or both. Moderation of Causal Path 1 would provide clearer evidence for the hypothesis that cognitive sophistication magnifies politically motivated reasoning; as would evidence of moderation of both causal pathways. Moderation of Causal Path 2 only, however, would offer relatively undiagnostic evidence for the magnification hypothesis, for the reasons outlined in the preceding sections.

To foreshadow our results, we find a lack of evidence to suggest that cognitive sophistication magnifies a direct effect of political group identity on reasoning. Importantly, this was not because our studies failed to detect evidence of a direct effect of political group identity *per se*: on the contrary, in all three studies we observed a robust direct effect of political group identity on reasoning, conceptually replicating MacCoun and Paletz (2009). By contrast, rather than magnifying a direct effect of political group identity, we find fairly consistent evidence that cognitive sophistication magnified a direct effect of prior factual beliefs on reasoning. Our results suggest that previous empirical work supporting the hypothesis that cognitive sophistication magnifies politically motivated reasoning provides evidence of Causal Path 2 only; that is, not particularly compelling evidence for the hypothesis, for the reasons we have outlined at length above. Our results also highlight potential routes that future work might take to more clearly test the hypothesis.

Political Group Identities and Prior Factual Beliefs

Before we describe our studies and results in detail, we first describe the correlation between political group identity and the prior factual belief variables that were measured in our three studies. This is important for two reasons. First, to validate a key assumption of the causal diagram in Figure 1; that political identity and prior factual beliefs are indeed correlated. Second, to establish that these two variables are not so highly correlated as to suggest they are conceptually indistinct, and to cause problems for statistical inference due to multicollinearity.

Studies 1 and 2 are replications of Kahan (2013), and, therefore, the political identity variable is a sum score of standardized and midpoint-centered responses to U.S. party affiliation and liberal-conservative scales. The prior factual belief variable is the extent to which subjects believe that climate change skeptics versus climate change believers are more “open-minded.” In Study 3, the political identity variable is slightly different: a standardized mean of responses on U.S. party affiliation and liberal-conservative scales. The prior factual belief variable is the extent to which subjects believe that stricter gun control laws would reduce U.S. gun crime. Figure 2 displays the raw data and correlation between these variables.

As Figure 2 shows, the political identity and prior factual belief variables are correlated in all three studies, consistent with the assumption of the causal diagram. The coefficients are precisely estimated and are all smaller than .50. Although this correlation is likely to be somewhat attenuated by measurement error, it nevertheless offers provisional evidence that the variables are conceptually distinct, and that multicollinearity is unlikely to be a serious threat to statistical inference. However, alongside the presentation of the relevant results in each study, we also report formal diagnostic tests of multicollinearity for all predictor variables. Across the three studies, these tests imply that multicollinearity is not a serious threat to statistical inference.

Studies 1 and 2

In Studies 1 and 2, we conducted two conceptual replications of the study reported in Kahan (2013), as described in the Cognitive Sophistication and Politically Motivated Reasoning section, but we also measured and modeled a relevant prior factual belief alongside political identity. Because of their close similarity, we present the methods and results of these two studies together.

Method

The design and analysis plans of Studies 1 and 2 were preregistered on the Open Science Framework (OSF), available at <https://osf.io/j7hrb> (Study 1) and <https://osf.io/2byaq> (Study 2). The data and analysis code from both studies are available there also. All analyses were conducted in R (v.3.5.1; R Core Team, 2018), using R *Studio* (v.1.1.423; RStudio Team, 2016). The R packages we used in data analysis were: *broom* (v.0.5.0; Robinson & Hayes, 2018), *papaja* (v.0.1.0.9842; Aust & Barth, 2018), *MASS* (v.7.3–50; Venables & Ripley, 2002), *effects* (v.4.0–3; Fox, 2003), *cowplot* (v.0.9.3; Wilke, 2018), *datatable* (v.1.11.8; Dowle & Srinivasan, 2018), *tidyverse* (v.1.2.1; Wickham, 2017), and *mctest* (v.1.2.5; Ullah & Aslam, 2019). The studies in this article are under the approval of the Committee on the Use of Humans as Experimental Subjects at Massachusetts Institute of Technology (Protocol 1806399553).

Samples.

Study 1. We sought to collect the approximate N -per-group used in Kahan (2013, $N = 583$). Because our design did not use a control group, our target sample size was thus $N = 1,200$ (i.e., N -per-group = 600). Subjects from the United States were recruited online using Amazon’s Mechanical Turk (MTurk) and were reimbursed \$1 for taking part. A total of $N = 1,215$ subjects completed the study. The data were collected in April 2018.

Study 2. It is well documented that MTurk samples skew more liberal and Democrat than the general U.S. population (Chandler & Shapiro, 2016) and have prior experience with the CRT (Chandler, Mueller, & Paolacci, 2014). Although the predictive validity of the CRT appears robust to multiple exposures (Bialek & Pennycook, 2018; Meyer, Zhou, & Frederick, 2018; Stagnaro, Pennycook, & Rand, 2018), the political skew on MTurk could bias against finding evidence in favor of the magnification inference (Kahan, 2016). Therefore, in Study 2 we drew our sample from Lucid, a

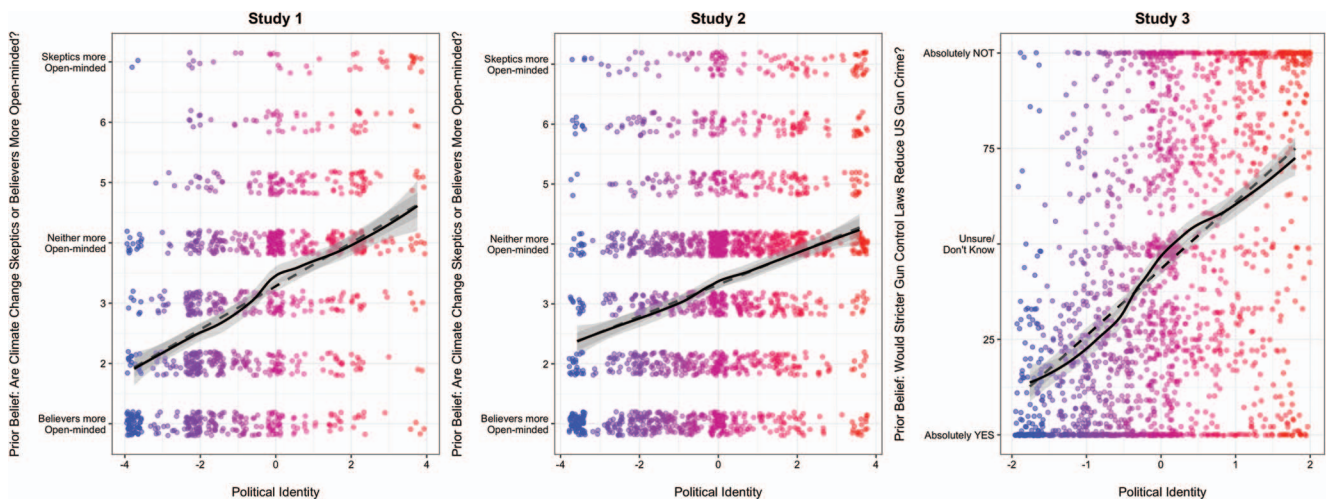


Figure 2. Political identity and specific prior belief variables in Studies 1, 2, and 3. Higher values of political identity indicate more U.S. Republican/conservative. The data have slight horizontal jitter to aid visibility. The dashed line is a linear fit and the solid line is a locally weighted fit. Shaded regions are 95% confidence interval. The Pearson’s correlation coefficients are .42 [.37, .47] (Study 1), .28 [.24, .32] (Study 2), and .47 [.44, .51] (Study 3). See the online article for the color version of this figure.

marketplace for online survey research that uses quota sampling to match respondents to U.S. census demographics. Accordingly, compared to MTurk, samples of U.S. adults recruited via Lucid are much closer to national benchmarks on reported party affiliation and political ideology, as well as on age, gender, education, income, and personality traits (Coppock & McClellan, 2019). We sought to collect $N = 2,000$ in Study 2, increasing the target N -per-condition to 1,000. Subjects were reimbursed \$1 for taking part. A total of $N = 2,060$ subjects completed the study. The data were collected in June 2018.

Design and procedure.

Study 1. To measure and model a relevant prior factual belief alongside political identity, we simplified Kahan's (2013) original design. Recall that, in the original design, subjects were asked to evaluate the validity of the CRT, described to subjects as a test of "open-minded and reflective" thinking. Before giving their evaluations, subjects first completed the test themselves and were randomly assigned to one of two treatments (or a control group) in which they were provided some information about the test. In Treatment A, partisans were told that people who believe that climate change is happening tend to score higher on the test than people who are skeptical that climate change is happening; implying the former are more open-minded. In Treatment B, partisans were told the reverse: that people who are skeptical that climate change is happening tend to score higher in the test, implying they are more open-minded.

In this original design, we identified at least three distinct factual beliefs that could conceivably affect variance in ratings of the CRT's validity as a measure of open-mindedness: subjects' beliefs about (a) the relative open-mindedness of climate-skeptics versus climate-believers, (b) how open-minded they themselves are, and (c) how they just performed on the CRT.

To reduce the number of relevant prior beliefs in our design—and therefore streamline their measurement and modeling—subjects rated the validity of a different but related test (which we labeled the *Open-Mindedness Test*). This was comprised of three self-report questions taken from the Actively Open-Minded Thinking Scale (Baron, 1993; Haran, Ritov, & Mellers, 2013). Importantly, subjects did not complete this test themselves. This modification served to remove the influence of prior belief (c), and reduce the influence of prior belief (b), described above. To measure prior belief (a), that is, subjects' specific belief about the relative open-mindedness of climate-skeptics versus climate-believers, we asked them who they considered to be more open-minded: someone who believes climate change is happening versus someone who is skeptical climate change is happening, with scores ranging from 1 (*believer is definitely more open-minded*) to 7 (*skeptic is definitely more open-minded*). This question was embedded within a list of nine additional questions asking about other targets' open-mindedness, which served as distractor questions.

Before rating the validity of the Open-Mindedness Test, subjects were randomly assigned to one of two treatment groups. In both groups, subjects were told that psychologists were still evaluating the validity of the test, but that a higher score is taken to indicate greater open-mindedness. In one treatment—*believers are open-minded*—subjects were asked whether they agreed that this test supplied good evidence of how open-minded someone is, on the assumption that future research finds that individuals who believe climate change is happening tend to score higher than individuals

who are skeptical climate change is happening. In the other treatment—*skeptics are open-minded*—subjects provided the same judgment, but on the reverse assumption: that future research finds that climate change *skeptics* tend to score higher than those who *believe* climate change is happening. These ratings were provided on a 1 (*strongly disagree*) to 7 (*strongly agree*; as in Kahan, 2013). Verbatim wording is reported in the online supplemental materials.

Finally, subjects also completed a 7-item CRT, comprised of the original three-item test (Frederick, 2005), adapted from Shenhav, Rand, and Greene (2012), and an additional four items that are not as numerically taxing (Thomson & Oppenheimer, 2016). The CRT is a behavioral task assumed to measure the propensity to engage in analytic or "reflective" thinking, and to override intuitive but incorrect responses (Pennycook et al., 2016). Test-retest reliability estimates for the original three-item measure range from $r = .75$ to $r = .81$ (Stagnaro et al., 2018), and CRT performance shares a moderate-strong positive correlation with various other indicators of cognitive ability and "rational thinking" (Blacksmith, Yang, Behrend, & Ruark, 2019; Toplak, West, & Stanovich, 2011). Correct responses to each item were summed to create a 0–7 score for each subject (Study 1 $\alpha = .77$, 95% CI [.75, .79]; Study 2 $\alpha = .68$ [.66, .71]). The distributions of CRT sum scores from Studies 1 and 2 are reported in the online supplemental materials.

To recap, our modified design comprised three key components: (a) ratings of the validity of the "Open-Mindedness Test" (experimental component), (b) ratings of how open-minded climate-skeptics versus climate-believers are (prior factual belief), and (c) completion of the 7-item CRT. The order of (a) and (b) was counterbalanced across subjects, and (c) was always completed in between (a) and (b). At the end of the study, subjects provided simple demographic information, including their political party affiliation—ranging from 1 (*strong Democrat*) to 7 (*strong Republican*)—and political ideology—ranging from 1 (*very liberal*) to 5 (*very conservative*), following Kahan (2013).

Study 2. The design and procedure of Study 2 were identical to Study 1, except for two adjustments. First, we made two changes to the list of distractor questions that asked about other targets' open-mindedness (i.e., targets other than climate skeptics vs. believers). Specifically, we replaced the gun control and abortion targets with supporters/opponents of (a) genetically modified food and (b) driverless cars, respectively. We did this to avoid priming the political identity of respondents in areas unrelated to our focus (climate change).

Second, we slightly adjusted the wording of the treatment delivered to subjects. Specifically, we more closely followed the treatment wording administered by Kahan (2013). Recall that, in Study 1, we asked subjects to rate the validity of the "open-mindedness test," assuming that future research finds that climate change skeptics [believers] tend to score higher in the test. In Study 2, we removed this conditional statement. We asked subjects to rate the validity of the test after simply informing them that "among a group of subjects in one recent study, the researchers found that people who reject [accept] evidence of climate change tend to score higher on the test than people who accept [reject] evidence of climate change." This more closely reflects the wording administered by Kahan (2013). Verbatim wording is reported in the online supplemental materials.

Results

Our analysis plan is as follows and is the same for each study. We fit three OLS regression models. First, we fit a model that estimates the three-way interaction between (a) CRT score, (b) treatment assignment (dummy-coded), and (c) political identity. This functions as a conceptual replication attempt of Kahan (2013). Second, we fit a model that estimates the three-way interaction between (a) CRT score, (b) treatment assignment, and (c) prior factual belief. Third, we fit the critical model that estimates both three-way interactions at the same time, and we ask whether the three-way interaction with political identity or prior belief still predicts reasoning outcomes. In this joint model, the three-way interaction estimate for political identity indicates whether cognitive sophistication magnifies a direct effect of political group identity on reasoning. Conversely, the three-way interaction estimate for prior belief indicates whether cognitive sophistication magnifies a direct effect of prior factual beliefs on reasoning. For brevity, we focus only on the results of the above three-way interaction tests in the main text. However, we report descriptive statistics and full model results in tables in the online supplemental materials.

Data exclusions.

Study 1. For all models, we excluded $N = 14$ (1.15%) subjects who were duplicate respondents (determined by their unique MTurk ID/IP address), retaining the earliest responses only. This exclusion criterion was preregistered. In addition to the preregistered exclusion criterion, $N = 1$ (0.08%) subject did not report their political party affiliation or political ideology and was thus unable to be included in the first and third models (described above). Consequently, Models 1 and 3 were fitted with $N = 1200$, and Model 2 with $N = 1201$.

Study 2. For all models, we excluded $N = 7$ (0.34%) subjects who were duplicate respondents (determined by IP address), retaining the earliest responses only (also preregistered). In addition to the preregistered exclusion criterion, $N = 1$ (0.05%) subject did not report their political party affiliation and was thus unable to be included in the first and third models. Models 1 and 3 were therefore fitted with $N = 2,052$, and Model 2 with $N = 2,053$.

Study 1 results.

Model 1: Identity only. We first fit an OLS regression model with (a) CRT score, (b) treatment assignment, (c) political identity, and all interaction terms. We refer to this as the “identity only” model. The outcome variable is agreement that the test supplies good evidence of how open-minded someone is, scaled to lie between 0 and 1 (higher scores = greater agreement). To construct the political identity variable, we first standardize and midpoint-center the U.S. party affiliation and political ideology variables, and then sum these two variables (following Kahan, 2013).² Thus, negative values indicate a more liberal/Democratic Party identity, and positive values a more conservative/Republican Party identity. The three-way interaction between (a), (b), and (c) is the estimate of interest.

As per the upper-left panel of Figure 3, the three-way interaction estimate for the identity-only model is close to zero and not statistically significant. To illustrate the implication of this null result, Figure 4A shows predicted test validity judgments from the model. They imply that the difference in test judgments between treatments was similar among identity-partisans who scored higher

versus lower on the CRT. Therefore, this result does not replicate the three-way interaction reported in Kahan (2013) and does not support the hypothesis that cognitive sophistication magnifies politically motivated reasoning. We also fitted an exploratory ordered logistic regression model testing the three-way interaction, which produced the same null result (identity-only estimate in the upper-right panel in Figure 3).

Model 2: Priors only. Moving on, we fit an OLS regression with (a) CRT score, (b) treatment assignment, and (c) prior belief in the relative open-mindedness of climate-change believers versus climate-change skeptics, and their interaction terms. This is the “priors-only” model. The prior belief variable is standardized and midpoint-centered original scale anchored at 1 (*believer is definitely more open-minded*) and 7 (*skeptic is definitely more open-minded*). The outcome variable is judgments of test validity, the same as before.

As per Figure 3 (upper-left), the three-way interaction estimate for the priors-only model is positive and statistically significant. The predicted test validity judgments from the model are displayed in Figure 4B. They imply that the difference in test judgments between treatments was greater for prior-belief-partisans who scored higher versus lower on the CRT; driven primarily by those who believed that climate-change skeptics are more open-minded (Figure 4B, center panel). The three-way interaction is also evident in an exploratory ordered logistic regression model, as indicated by the priors-only estimate in Figure 3 (upper-right).

Model 3: Joint model. Because the result in Model 1 was null, the joint test in Model 3 is somewhat moot. Nevertheless, we proceed to jointly model the interaction between (a) CRT score, prior factual belief, and treatment assignment, and (b) CRT score, political identity, and treatment assignment. The outcome variable is the same as in Models 1 and 2. The three-way interaction estimate for prior belief remained similarly sized, positive, and statistically significant in this joint model (Figure 3, upper-left). The three-way interaction estimate for political identity also remained similarly sized and statistically nonsignificant. In an exploratory ordered logistic model, the estimates were much the same, though the 95% CI of the interaction estimate for prior belief slightly overlaps zero (Figure 3, upper-right).

Model 3: Multicollinearity. To check for multicollinearity, we computed the variance inflation factor (VIF) and tolerance value of each predictor variable in the joint model, using the R library *mctest* (Ullah & Aslam, 2019). The VIF and tolerance value for the three-way interaction term with prior beliefs slightly exceeded the thresholds of 10 and 0.1, respectively (VIF = 10.45, tolerance = 0.09), suggesting mild multicollinearity due to this term. The VIF and tolerance values for all other predictor variables were below these thresholds. We note that there is no “hard” threshold for VIF and tolerance; however, these thresholds are common heuristics that may often provide a conservative test of multicollinearity—that is, VIF and tolerance can exceed these thresholds despite

² The correlation between party affiliation and ideology variables in Study 1 is $r(1198) = .80$, 95% CI [.78, .82]; in Study 2, $r(2050) = .67$, 95% CI [.64, .69]. In the online supplemental materials, we repeat all analyses in the main text that use the composite political identity variable separately for party affiliation and liberal-conservative ideology variables. The results are substantively identical to the results reported in the main text that use the composite variable.

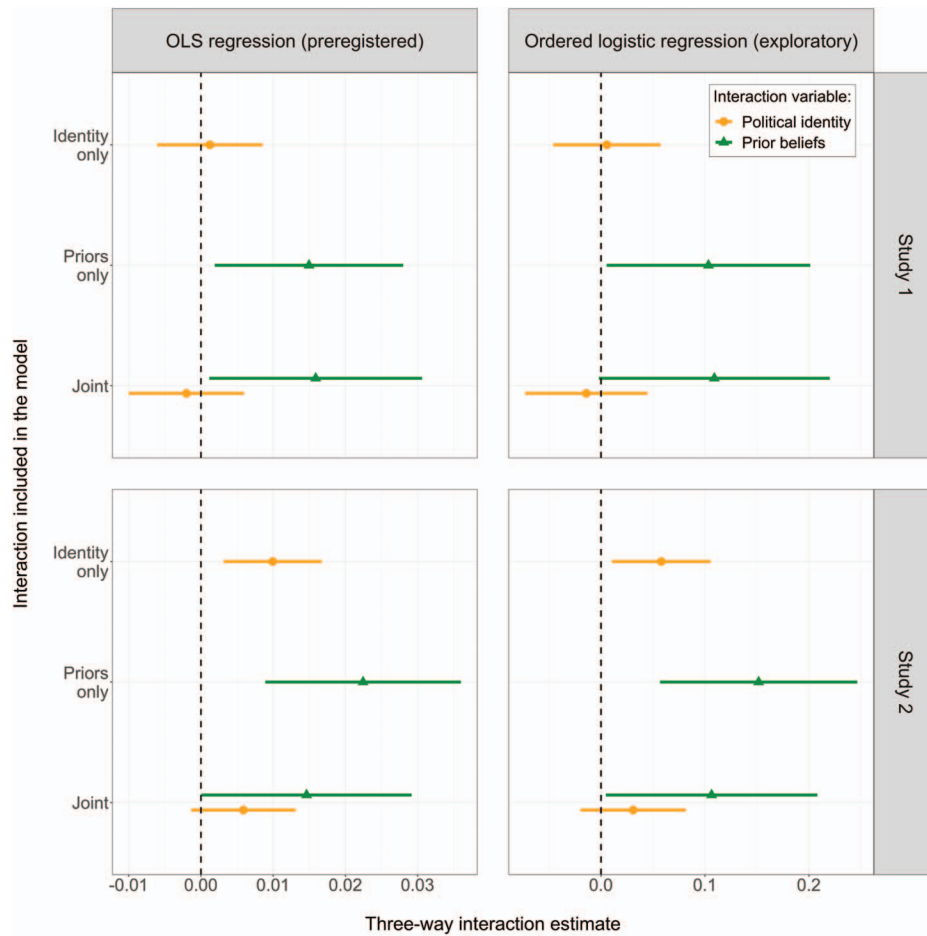


Figure 3. Results of key interaction tests from Studies 1 and 2. Points are three-way interaction estimates from the respective models indexed on the y-axis. Aside from political identity or prior belief, the other two variables in each interaction are treatment assignment (dummy-coded) and Cognitive Reflection Test sum score (0–7). Whiskers are 95% confidence interval. See the online article for the color version of this figure.

multicollinearity remaining within reasonable limits (O’Brien, 2007).

Study 1: Results summary. Surprisingly, we did not find evidence that cognitive sophistication magnified an effect of political identity on reasoning about the validity of new information, even before considering the influence of prior factual beliefs. This is contrary to the result reported in Kahan (2013) and the hypothesis that cognitive sophistication magnifies politically motivated reasoning. Nevertheless, this result could be due to differences in sampling population with the original study, which recruited from a population more representative of the general U.S. population than MTurk.

In contrast, we observed some evidence that cognitive sophistication magnified an effect of prior factual beliefs on reasoning about the validity of the information. However, this result was limited to subjects who believed climate-skeptics to be more open-minded. This asymmetry is curious but is not central to the test of interest (which concerns the interaction per se, not particular simple effects being symmetrical). Furthermore, it is not without precedent: Kahan (2013) also observed an asymmetry such that the magnifying effect of cognitive sophistication on political identity

was more apparent in one treatment condition than in the other. Understanding this asymmetry is an interesting avenue for future work. Finally, we observed the interaction for prior factual beliefs despite there being little evidence of the corresponding interaction for political identity, and after also adding political identity into the same model. This implies that cognitive sophistication magnified a direct effect of prior factual beliefs on reasoning.

Study 2 results. Each of the models in Study 2 was fitted exactly as in Study 1.

Model 1: Identity only. In contrast to the results of Study 1, the three-way interaction estimate for political identity was statistically significant and positive (Figure 3, lower-left panel). The implications of this interaction effect are shown in the predicted values in Figure 5A. They imply that the difference in test judgments between treatments was greater among identity-partisans who scored higher versus lower on the CRT; driven entirely by subjects who identified as liberal/Democrat (Figure 5A, left panel). This result was also evident in the exploratory ordered logistic regression model (see Figure 3). Speaking broadly, this is a conceptual replication of the result reported in Kahan (2013).

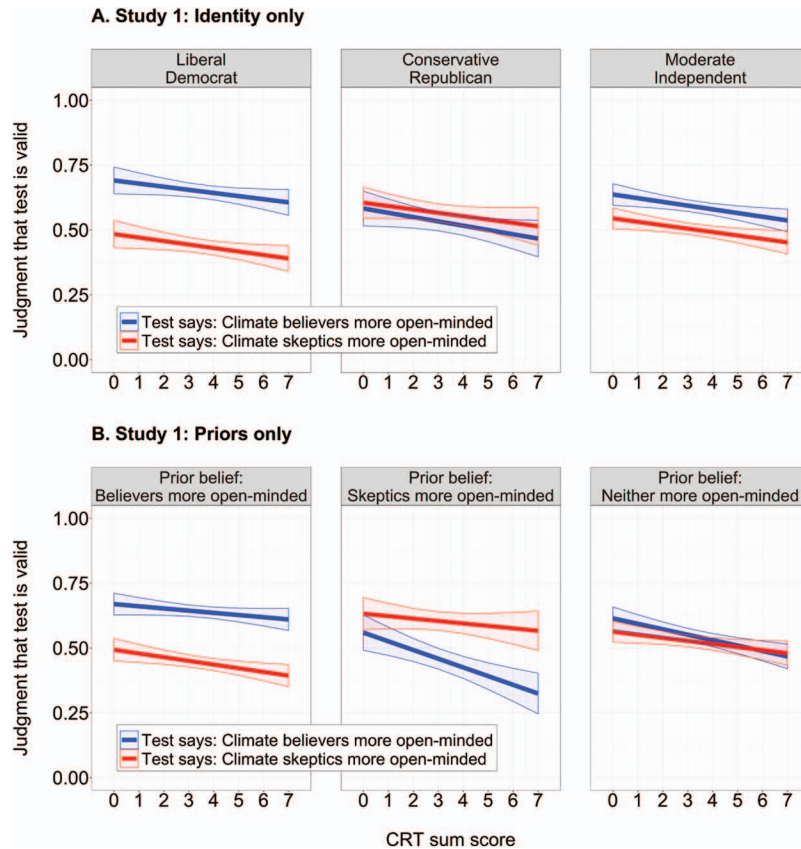


Figure 4. Predicted test validity judgments in Study 1. A: Predicted values for “liberal Democrat,” “conservative Republican,” and “moderate Independent” are computed at values of -2 , $+2$, and 0 on the political identity variable, respectively. B: Predicted values for prior belief: “Believers,” “skeptics,” and “neither more open-minded” are computed at values of -1 , $+1$, and 0 on the midpoint-centered and standardized prior belief variable, respectively. A, B: Cognitive Reflection Test (CRT) sum score is set to range from the 0.05 to 0.95 percentile. Shaded regions are 95% confidence interval. See the online article for the color version of this figure.

Model 2: Priors only. The three-way interaction estimate for prior belief was statistically significant and positive (see Figure 3), as in Study 1. The interaction effect implies a pattern broadly similar to the interaction with political identity, as the model-predicted values in Figure 5B show. They imply that the difference in test judgments between treatments was greater for prior-belief-partisans who scored higher versus lower on the CRT; driven primarily, but not exclusively, by subjects whose prior belief was that climate-change believers are more open-minded (Figure 5B, left panel). The interaction is also evident in an exploratory ordered logistic regression model (see Figure 3).

Model 3: Joint model. We then jointly modeled the association between (a) CRT score, prior belief, and treatment assignment, and (b) CRT score, political identity, and treatment assignment. In this joint model, both three-way interaction estimates diminished in size. Thus, the interaction with political identity was no longer statistically significant (see Figure 3). The three-way interaction with prior beliefs remained below the conventional threshold for statistical significance, albeit only just ($p = .049$: joint-model prior beliefs estimate in lower-left panel of Figure 3). The results were similar in an exploratory ordered logistic regression model (see Figure 3).

Model 3: Multicollinearity. The VIF and tolerance values for all predictor variables were below the thresholds of 10 and 0.1 (Ullah & Aslam, 2019), respectively, suggesting little evidence of multicollinearity.

Study 2: Results summary. In Study 2, we conceptually replicated the interaction between CRT performance, political identity, and treatment assignment previously reported in Kahan (2013). We also conceptually replicated the corresponding interaction with prior factual beliefs from our Study 1. However, similar to Study 1 we did not observe any evidence that cognitive sophistication magnified an effect of political identity on reasoning after statistically controlling for people’s prior belief. In particular, after we included the corresponding three-way interaction with prior belief in the model, the estimate on the interaction between CRT performance, political identity, and treatment assignment shrunk in size and was not statistically different from zero (joint-model result). This implies that cognitive sophistication did not magnify a direct effect of political group identity on reasoning.

Finally, we also again observed evidence of asymmetry in the interaction effect—opposite to the asymmetry pattern observed in Study 1—in that subjects who were (a) left-leaning or (b) held a prior belief that climate believers are more open-minded exhibited

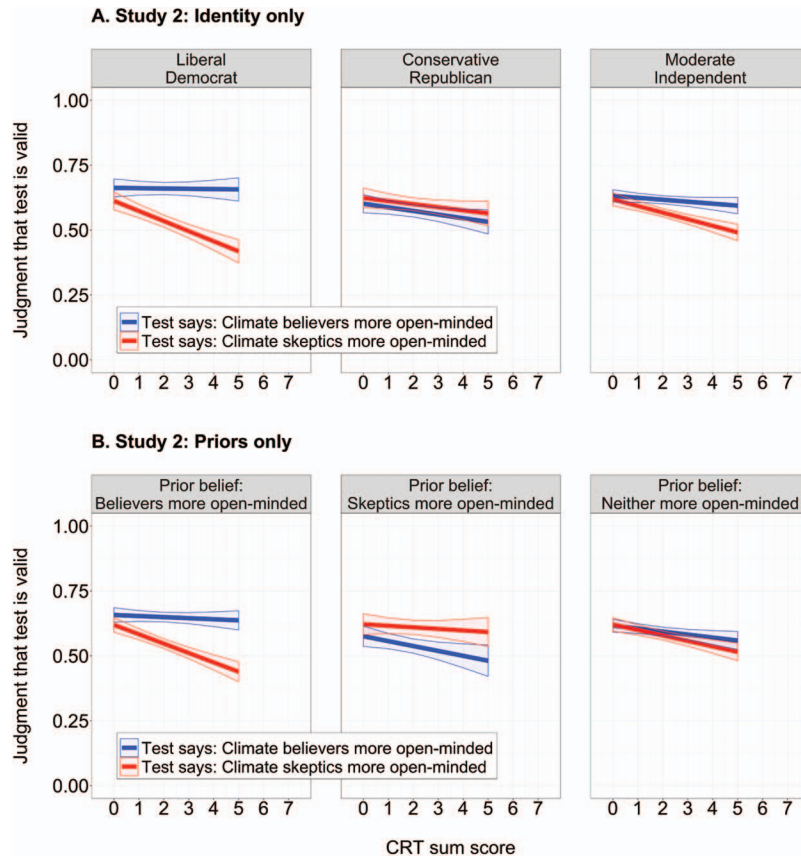


Figure 5. Predicted test validity judgments in Study 2. A: Predicted values for “liberal Democrat,” “conservative Republican,” and “moderate Independent” are computed at values of -2 , $+2$, and 0 on the political identity variable, respectively. B: Predicted values for prior belief: “Believers,” “skeptics,” and “neither more open-minded” are computed at values of -1 , $+1$, and 0 on the midpoint-centered and standardized prior belief variable, respectively. A, B: Cognitive Reflection Test (CRT) sum score is set to range from the 0.05 to 0.95 percentile. Shaded regions are 95% confidence interval. See the online article for the color version of this figure.

a larger interaction between treatment condition and cognitive sophistication (Figure 5A and 5B). This pattern is curious in that it runs somewhat counter to the narrative that right-leaning individuals are more prone to motivated reasoning or dogmatism (Jost, Glaser, Kruglanski, & Sulloway, 2003). We think the difference with Study 1 is likely due to some combination of natural sampling variability and differences in population composition between MTurk and Lucid platforms, but it is an open empirical question which of these two possibilities better explains the difference.

Study 3

In Study 3, we further examine our question of interest using a novel instantiation of the paradigmatic politically motivated reasoning design. Specifically, we provide information to subjects in the form of the results of a real scientific report on U.S. gun control policy (Gius, 2015). This report found that background checks were associated with increased and decreased rates of gun-related murder at the state-level, depending upon the unit of analysis. Because the broad methods and analysis in the report remained identical regardless of the finding (as it was the same report), this provided a natural analogue of the paradigmatic design used to

infer politically motivated reasoning. In Study 3, we also collected a wider range of measures of cognitive sophistication to test the generalizability of our results along this dimension.

Method

The Study 3 design and analysis plan was preregistered at <https://osf.io/w2g9y>. The data and analysis code are also available there. As before, all analyses were conducted in *R* using *R Studio*. The *R* packages we used in data analysis were the same as in Studies 1 and 2.

Sample. We sought to collect $N = 2,000$ (the same as in Study 2). Subjects were from the United States, recruited via Lucid, and were reimbursed \$1 for taking part. A total of $N = 1,980$ subjects completed the study. The data were collected in July 2019.

Design and procedure. Subjects entering the study completed the following procedure. First, they provided simple demographic information, including placement on U.S. party affiliation and liberal-conservative scales (as in Studies 1 and 2). They then provided prior factual belief about gun control in the United States, in the form of self-reported agreement with the statement, “stricter gun control laws would reduce gun crime in the United States.”

Agreement with this statement was indicated on a sliding scale from 0 (*absolutely NOT*) to 100 (*absolutely YES*) in whole integers (the scale integers were not shown). This scale was reverse-coded for analysis, to make the signs of the model estimates consistent with Studies 1 and 2. Subjects then completed a brief four-task cognitive battery (task order was randomized), detailed below. Finally, they were randomly assigned to read one of two brief summaries of a recently published report on U.S. gun control policy (Gius, 2015).³ Our key outcome variables were collected on the same survey page as this summary was presented. Below we describe in greater detail the tasks in the cognitive battery, gun control report treatment information, and outcome variables.

Cognitive battery. Subjects completed all four of the following cognitive tasks in random order. Correct responses to each task are summed, giving four sum scores per subject corresponding to their performance on each of the tasks (Alice Heim 4 Intelligence Test [AH4] $\alpha = .90$, 95% CI [.90, .91]; CRT $\alpha = .64$ [.62, .66]; Political Knowledge Test [PK] $\alpha = .47$ [.44, .51]; Scientific Reasoning Scale [SRS] $\alpha = .52$ [.49, .55]).

AH4. The AH4 is a test of numerical and verbal reasoning ability, completed under timed conditions (Der & Deary, 2018; Heim, 1970). The test consists of 65 items with a maximum time limit of 10 min. To avoid prohibitive survey length, we reduced the time limit to 5 min. An example item is: “Here are three figures: 2 3 4. Divide the biggest figure by the smallest and add the result to the figure printed immediately after the smallest figure.” Subjects could answer the items in any order and were not prevented from proceeding before their 5 min were up. Answer options are a mixture of multiple-choice and open-ended formats.

CRT. The seven-item CRT was administered as in Studies 1 and 2.

PK. We asked subjects five questions testing their knowledge of general political facts. Four questions pertained to U.S. politics, one to British politics. An example question is: “Whose responsibility is it to decide if a law is constitutional or not?” Answer options are multiple-choice format. Subjects were given a maximum of 10 s to answer each question.

SRS. The SRS is an 11-item test that presents subjects with a scientific scenario and asks them a true/false question related to the scenario (Drummond & Fischhoff, 2017a). The SRS is constructed to test understanding of general scientific concepts, such as random assignment, causal inference, ecological validity, and so on. An example scenario/question is “A researcher finds that American states with larger parks have fewer endangered species. True or False? These data show that increasing the size of American state parks will reduce the number of endangered species.” Answer options are multiple-choice format (true/false).

Gun control report treatment. Subjects read about the methods and results of a recently published report on U.S. gun control policy (Gius, 2015). In that report, the author examined the association between background checks and gun-related murder rates at the state-level, using a dataset covering the period 1980–2011. Gius (2015) found that background checks on licensed gun-sellers (i.e., dealers) were associated with fewer gun-related murders, whereas background checks on unlicensed gun-sellers (private sellers) were associated with more gun-related murders. Subjects in our study were randomly assigned to read a summary of this report, focusing on one or the other result. Specifically, in the pro-gun-control treatment, subjects read about the former result; in

the anti-gun-control treatment, subjects read about the latter result. In both treatment groups, subjects were provided the same general information about the report, including its aims, dataset, rules for outlier exclusion in analysis, and so on (wording is reported verbatim in the SI).

Outcome variables. We collected three outcome variables on the same survey page as the treatment information. In particular, we asked subjects the following three questions:

- “Does this study provide weak or strong evidence for the effect of stricter gun control laws on U.S. gun crime?” (extremely weak evidence, unsure/do not know, extremely strong evidence)
- “How appropriate are the study methods for investigating the effect of U.S. gun control policy?” (extremely inappropriate, unsure/do not know, extremely appropriate)
- “How convincing are the study results?” (extremely unconvincing, unsure/do not know, extremely convincing)

Responses to each question were provided on a sliding scale from 0 to 100 in whole integers. Response anchors were located at 0, 50, and 100 and are given in parentheses after each question. For analysis, we use a composite outcome variable computed by averaging responses to these three items, and rescaling to 0–1 ($\alpha = .85$, 95% CI [.84, .86]). Values closer to 1 thus indicate a more positive evaluation of the evidence. For exploratory purposes, we also provided subjects with an open-ended free response box to give reasons for their evaluation. Descriptive statistics of the key variables are reported in the online supplemental materials.

Results

Deviations from preregistered protocol. We made one deviation from our preregistered protocol. In Studies 1 and 2, we operationalized political identity by summing the (separately) standardized U.S. party affiliation and liberal-conservative placement variables. The rationale behind this decision was to reproduce as closely as possible the analysis protocol reported in Kahan (2013). This operationalization remained unchanged in the preregistered protocol for Study 3. However, in the analyses below, we adopt a slightly different operationalization: We first take the mean of the original party affiliation and liberal-conservative variables, and

³ Originally, our aim in Study 3 was to collect data across two waves: Subjects would complete the cognitive battery and prior belief collection in Wave 1 and be re-recruited in Wave 2 to complete the experimental component of the study. We originally preregistered this two-wave design (available at <https://osf.io/pvar5>). However, our attrition at Wave 2 was substantially higher than we allowed for in our sample size assumptions (expected attrition rate was ~10–20%, observed attrition rate was ~60%). Given the substantial reduction in statistical power of the resulting Wave 2 sample, we preregistered a new version of the study; incorporating the cognitive battery, prior belief collection, and experimental component in a single wave. The analyses reported in the main text were conducted on the sample from this new version of the study. In the spirit of transparency, however, we conduct all analyses also on the sample from the original Wave 1/Wave 2 design (note that we did not conduct this analysis until after completing and analyzing Study 3 as reported in the main text). The results of these analyses are reported in the online supplemental materials. The results are qualitatively identical to those reported in the main text.

then standardize this resulting composite variable.⁴ The logic behind this decision is that it allows us to directly compare the effect size of the three-way interaction estimate that includes political identity with that that includes subjects' prior belief (since the latter was also regularly standardized). For transparency, in the online supplemental materials we also report the model estimates for the political identity variable as it was preregistered (i.e., summing the two standardized political variables). The results are substantively identical to those reported below, but—as noted above—comparison of effect sizes is more difficult.

Analysis plan. Our analysis plan is the same as that in Studies 1 and 2. The only difference is that now we analyze four cognitive indicators (as opposed to one). We fit separate models for each of the four cognitive indicators (AH4, CRT, PK, SRS). Thus, we first fit an OLS model that estimates the three-way interaction between (a) cognitive indicator score, (b) treatment assignment, and (c) political identity. Second, we fit a model that estimates the three-way interaction between (a) cognitive indicator score, (b) treatment assignment, and (c) prior factual belief. Third, we fit a model that estimates both three-way interactions at the same time, and we ask whether the three-way interaction with political identity or prior factual belief still predicts reasoning outcomes. Because we examine 4 cognitive indicators, we apply a Bonferroni-corrected p value threshold of .0125 (.05/4). This was preregistered. As in Studies 1 and 2, we focus only on these three-way interaction estimates in the main text for brevity. We report descriptive statistics on our key variables and full model results in the online supplemental materials.

Data exclusions. For all models, we excluded $N = 3$ (0.2%) subjects who were duplicate respondents (determined by their unique IP address), retaining the earliest responses only. This exclusion criterion was preregistered. Consequently, all models were fitted with $N = 1977$.

Model results. The three-way interaction estimates for each of the identity-only, priors-only, and joint models are presented in Figure 6, partitioned by cognitive indicator. The estimates show two clear results.

First, we observe a statistically significant estimate for both political identity and prior belief three-way interactions when they are modeled separately; and this pattern is consistent across all four cognitive indicators (cf. “identity-only” and “priors-only” estimates in all four panels). Note that because the cognitive indicator scores, political identity variable, and prior belief variable are all standardized, the size of the estimates can be directly compared. To visualize what these interaction estimates imply, in Figure 7 we plot the predicted values from the identity-only and priors-only models. In that figure, “treatment information” denotes whether subjects read about the pro- or anti-gun-control result from the report published by Gius (2015); on the y-axis is the model-predicted aggregated judgment about whether that report provided weak-strong evidence, used inappropriate-appropriate methods, and presented unconvincing-convincing results; on the x-axis is the sum score value (unstandardized for interpretation) corresponding to the cognitive indicator labeled in the panel *columns*; and, finally, the panel *rows* show the model predictions for subjects with different political identity (7A) and prior belief (7B) values.

The qualitative pattern is much the same as in Study 2: The difference in test judgments between treatments was greater for

identity-partisans who scored higher versus lower on the cognitive indicators; driven mostly by subjects who identified as Democrat/liberal (Figure 7A, top row). The implied pattern of judgments is almost identical when the political identity variable is replaced with subjects' prior factual belief about gun control: the difference in test judgments between treatments was greater for prior-belief-partisans who scored higher versus lower on the cognitive indicators; driven by those subjects whose prior belief was that stricter gun control laws would reduce gun crime in the U.S. (Figure 7B, top row). Furthermore, as implied by the estimates, the pattern of predicted judgments is quite consistent across all four indicators.

The second clear result is seen in the estimates from the joint models (see Figure 6): accounting for subjects' prior factual belief in the model eliminates the observed three-way interaction with political identity, across all 4 indicators of cognitive sophistication. In all cases, the point estimate for the three-way interaction with political identity shrinks markedly toward zero. By contrast, the corresponding estimate for the interaction with prior beliefs barely moves. In other words, these data are strongly inconsistent with the notion that cognitive sophistication magnifies a direct effect of political group identity on reasoning. This is consistent with the results of Studies 1 and 2. In addition, Study 3 provides more compelling evidence to suggest that cognitive sophistication magnifies a direct effect of prior factual beliefs on reasoning. Finally, the VIF and tolerance values for all predictor variables in each of the four cognitive indicator models were below the thresholds of 10 and 0.1 (Ullah & Aslam, 2019), respectively, suggesting little evidence of multicollinearity.

General Discussion

In this paper, we critically examined the hypothesis that cognitive sophistication magnifies politically motivated reasoning. We first described results from paradigmatic study designs commonly offered as evidence for this hypothesis. Drawing on a range of arguments and evidence, we argued that these designs typically conflate the influence of political group motivation with the influence of prior factual beliefs on reasoning. This conflation obscures empirical assessment of the magnification hypothesis, because prior factual beliefs can affect reasoning in the absence of an effect of political group motivation; and, thus, cognitive sophistication could simply be magnifying the former instead of the latter effect. We conducted three experiments in an effort to address this problem: attempting to estimate the direct effect of political group identity on reasoning by statistically controlling for people's relevant prior factual beliefs. Across the three experiments, we found little evidence to suggest that cognitive sophistication magnifies a direct effect of political group identity on reasoning. In contrast, we found fairly consistent evidence to suggest that cognitive sophistication magnifies a direct effect of prior factual beliefs on reasoning.

⁴ The correlation between party affiliation and ideology variables in Study 3 is $r(1975) = .67$, 95% CI [.65, .69]. As with Studies 1 and 2, in the SI we report all Study 3 analyses that use the composite political identity variable separately for party affiliation and liberal-conservative ideology. The results are substantively identical to the Study 3 results reported in the main text that use the composite.

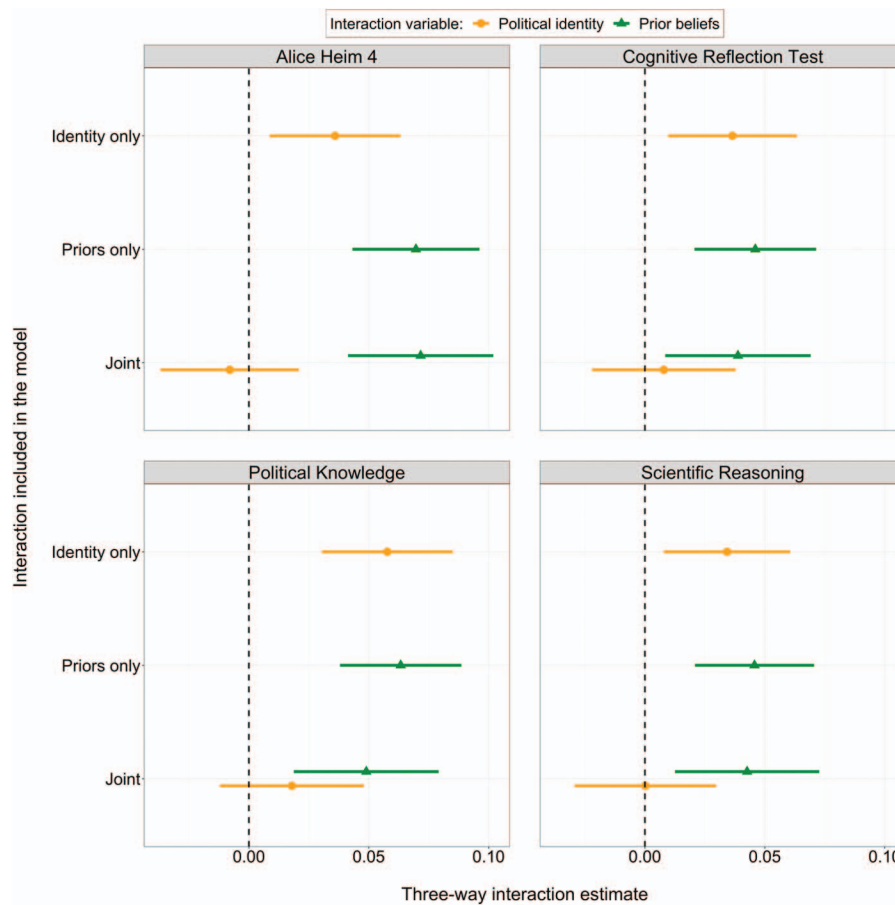


Figure 6. Results of key interaction tests from Study 3. Points are three-way interaction estimates from the respective models indexed on the y-axis. Panels refer to the cognitive indicator included in the model. Aside from political identity or prior belief, and the labeled cognitive indicator, the other variable in each interaction term is treatment assignment (dummy-coded). Whiskers are 98.75% confidence interval, representing a p value threshold of .0125 as preregistered. See the online article for the color version of this figure.

Implications for Theory and Existing Evidence

Our results imply that much existing evidence for the hypothesis that cognitive sophistication magnifies politically motivated reasoning is not particularly diagnostic. Specifically, our argument and results suggest that previous empirical studies supporting the hypothesis that cognitive sophistication magnifies politically motivated reasoning (Guay & Johnston, 2020; Kahan, 2013; Kahan, Peters, et al., 2017; Kuru et al., 2017; Nurse & Grant, 2019; Sumner et al., 2018; Taber et al., 2009; Taber & Lodge, 2006) likely do not demonstrate that cognitive sophistication magnifies a direct effect of political group motivation on reasoning (Causal Path 1, Figure 1); but, rather, that it magnifies a direct effect of prior factual beliefs (Causal Path 2). It is thus difficult to evaluate the extent to which the results therein offer support for the hypothesis that cognitive sophistication magnifies politically motivated reasoning—because, as we have discussed at length, Causal Path 2 offers relatively ambiguous evidence of politically motivated reasoning per se (we refer to the Rethinking Cognitive Sophistication and Politically Motivated Reasoning section for the full discussion).

Taking this ambiguity together with our results, our conclusion is that there is currently a lack of clear empirical evidence for the hypothesis that cognitive sophistication magnifies politically motivated reasoning. To more clearly test it, future work should strive to design studies that can distinguish between the oft-confounded influences of (a) political group motivation and (b) prior factual beliefs on reasoning. We concur with others that “study designs that fail [to do so] impede explanation, prediction, and prescription” (Kahan, 2016, p. 6).

A concrete example of one such design is that which randomly assigns the incentives that people have to arrive at particular political conclusions when reasoning—such as when they are trying to persuade other people of a political position (Schwardmann, Tripodi, & van der Weele, 2019; Schwardmann & van der Weele, 2019)—instead of the features of the information to be reasoned over (as is typical). This design has the joint benefit of both (a) avoiding the confound of relevant prior factual beliefs described in this paper, as well as (b) aligning naturally with the theoretical trend toward understanding politically motivated reasoning as a product of the social incentives

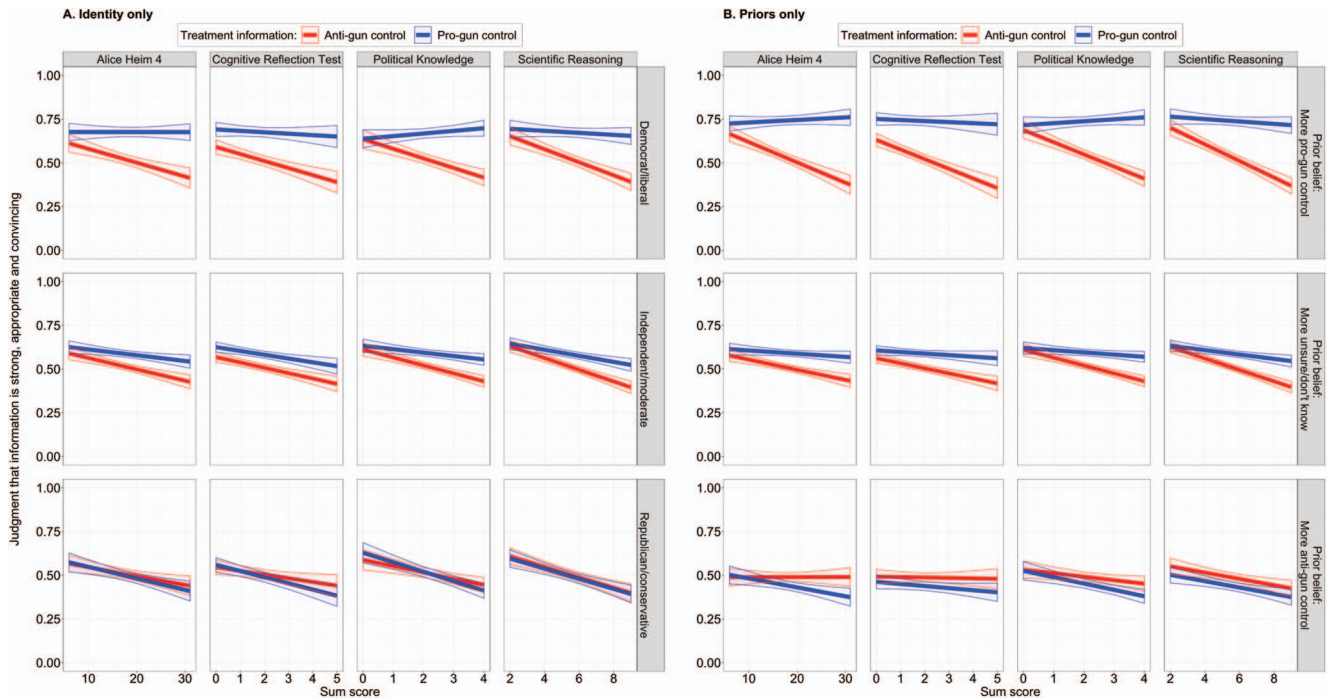


Figure 7. Predicted information judgments in Study 3. A: Predicted values for “Democrat/liberal,” “Independent/moderate,” and “Republican/conservative” are computed at values of -1 , 0 , and $+1$ on the standardized political identity variable, respectively. B: Predicted values for prior belief: More “pro-gun control,” “unsure/don’t know,” and “anti-gun control” are computed at values of -1 , 0 , and $+1$ on the standardized prior belief variable, respectively. A, B: Cognitive indicator sum scores are set to range from the 0.05 to 0.95 percentile. Shaded regions are 98.75% confidence interval. See the online article for the color version of this figure.

people face to hold particular beliefs (Kahan, 2016; Klar, 2014; Williams, 2019).

Consistency With Previous Work

Our results and interpretation are consistent with previous work. First, they are consistent with the results of recent studies using “party cue” designs, another design type whose results are often discussed with respect to politically motivated reasoning (Bullock, 2019; Tappin et al., 2020). Results from numerous such studies suggest that cognitive sophistication magnifies sensitivity to the substance of new information, rather than sensitivity to cues about whether the information is politically congenial per se (Anduiza, Gallego, & Muñoz, 2013; Barber & Pope, 2019; Boudreau & MacKenzie, 2014; Kam, 2005; Mérola & Hitt, 2016).

For example, Anduiza and colleagues (2013) found that people tolerated an act of political corruption more when it was committed by a politician from the same party versus the opposing party. Among people scoring highly on a measure of political knowledge, however, this political bias was not evident—instead, the act was condemned uniformly. Mérola and Hitt (2016) similarly found that highly numerate individuals were persuaded by strong information in favor of prison reform, even when that information was sponsored by the opposing political party. In contrast, less numerate individuals were only persuaded by information their party sponsored—irrespective of its strength.⁵ These patterns are somewhat analogous to our results, which suggest that cognitive sophistica-

tion magnified an effect of information (in)coherence with specific prior beliefs, rather than an effect of political congeniality per se, on reasoning.

A second line of evidence that is consistent with our results and interpretation comes from several studies of “belief bias”. In these studies, subjects are tasked with endorsing (or rejecting) a conclusion that follows from two assumed-to-be-true premises. The premises and conclusion in these studies are typically innocuous, such as whether whales can walk, or whether flowers have petals. The conclusion either does (valid) or does not (invalid) deductively follow from the premises, and either contradicts or aligns with people’s prior beliefs. Results from studies using this paradigm suggest that, while cognitively sophisticated individuals perform better overall—correctly discriminating valid from invalid conclusions—their performance is also more sensitive to whether

⁵ These results are in slight tension with a minority of studies that do find that cognitive sophistication tends to magnify deference to party cues (Bakker, Lelkes, & Malka, 2019; Slothuus & de Vreese, 2010). These differences may be partly attributable to people’s heterogeneous exposure to information in the real world—in particular, to heterogeneity by issue and by sophistication (Slothuus, 2016). This is broadly consistent with our argument and results, which emphasize careful consideration of the role of pretreatment factors—such as confounders of the association between prior beliefs and political group identity—on the patterns of reasoning observed in one-shot experiments.

the conclusion coheres with their prior beliefs (Trippas et al., 2015, 2018).

A study by Trippas and colleagues (2015) provides a concrete illustration. Among subjects who scored lower on the CRT, the difference in endorsement rates for valid versus invalid conclusions was similar irrespective of whether the conclusions contradicted or aligned with their prior beliefs. That is, low CRT scorers endorsed valid over invalid conclusions at roughly the same rate across prior belief manipulations. In contrast, among high CRT scorers, the difference in valid-invalid endorsement rates was larger for conclusions that contradicted their prior beliefs. In other words, whether a given conclusion contradicted or aligned with prior beliefs affected reasoning performance in this way only for those who scored high on the CRT. Given that the content of the stimuli used in these studies was innocuous and entirely unrelated to politics, the implication is that cognitive sophistication magnifies sensitivity to incoherence between specific prior beliefs and new information per se; consistent with the results and our interpretation of the three studies reported here.

Considering Alternative Interpretations of Our Results

There are several caveats and alternative interpretations of our results that merit discussion. Most of these derive from the fact that politically motivated reasoning is a somewhat loosely defined concept (Leeper & Slothuus, 2014), used in different papers and by different research teams to describe different processes. In this paper, we adopted what we believe to be the most common conceptualization: reasoning affected by the motivation to arrive at a conclusion that is *congenial*—normative, preferable, desirable—for the political group (e.g., Republican Party) or ideology (e.g., conservative) with which one identifies. As noted in the introduction, one prominent theory grounds this motivation in the experience of psychological and material incentives to conform to the position associated with one's political group identity (Kahan, 2016; Petersen et al., 2013; Van Bavel & Pereira, 2018; but there are other theories).

Sticking with this conceptualization, one potential concern is that political group identity and relevant prior factual beliefs are hopelessly intertwined, and so it makes little sense to try and identify the motivational effects of the former while holding constant the effects of the latter. Put another way, we should not expect to observe any motivational effect of political group identity on reasoning after accounting for the effect of prior factual beliefs. Thus, the reason why cognitive sophistication did not magnify an effect of political group identity after accounting for prior factual beliefs in our data is simple: there was no such effect to magnify. This is a reasonable concern, but it is negated by conceptual and empirical analysis.

As illustrated in the Introduction, there is a conceptual distinction between (a) holding a specific belief about, for example, the efficacy of U.S. gun control laws, and (b) identifying with a political group that would prefer a particular answer to this question in order to further a political agenda. Plainly, (a) and (b) need not always align for any given person, and the question that we focused on in this paper was: when the effect of (a) is held constant, does (b) have an effect on reasoning and, in particular, is this effect magnified by cognitive sophistication? Indeed, recognition of the general conceptual distinction between (a) and (b)

undergirds decades of research that emphasizes the importance of ruling out the confounding influence of specific prior beliefs in order to identify motivated reasoning (Ditto, 2009; Ditto et al., 2019; Friedman, 2012; Gerber & Green, 1999; Kahan, 2016; Kunda, 1990; MacCoun & Paletz, 2009; Tappin et al., 2017, 2020; Tappin & Gadsby, 2019; Tetlock & Levi, 1982).

On an empirical level, moreover, this conceptual distinction appears borne out. Recall the study of MacCoun and Paletz (2009) which found that, statistically controlling for American subjects' prior beliefs about several U.S. policy issues, their political group identities (ideology labels) nevertheless explained variation in reasoning outcomes: Subjects tended to evaluate information less favorably when it was uncongenial (vs. congenial) for their political group identity, even after accounting for their specific prior belief about the issue. The same pattern is observed in our own data. In all three of our studies, models in which the treatment is simply interacted with (a) political group identity and (b) prior factual belief (i.e., leaving aside cognitive sophistication) show statistically significant and robust estimates for interaction (a) as well as (b), such that subjects tended to evaluate the otherwise-identical information less favorably when it was uncongenial (vs. congenial) for their political group identity (the estimates are reported in the online supplemental materials). In other words, we observed evidence consistent with a direct effect of political group identity on reasoning, similar to MacCoun and Paletz (2009).

In summary, conceptual and empirical analyses imply that an effect of political group identity is distinguishable from that of specific prior factual beliefs; in general, and in our data in particular. Thus, the reason why cognitive sophistication did not magnify an effect of the former after accounting for the latter in our data does not appear to be because the effect of political group identity and prior factual beliefs are hopelessly intertwined or meaningless-if-separated.

An alternative interpretation of our results is based on the following chain of argument: Political group identity is an imperfect measure of whatever latent variable underpins the motivation to reach politically congenial conclusions—for example, the pressure to conform to the positions of one's ingroup; this latent variable is a common cause of both political group identity and relevant prior factual beliefs, but prior factual beliefs are a less noisy measurement of this latent variable and therefore better reflect the “true” (latent) cause of politically motivated reasoning. According to this chain of argument, evidence that cognitive sophistication magnifies an effect of prior factual beliefs on reasoning is simply evidence that it magnifies politically motivated reasoning. As discussed below, there appears to be at least two additional implications of this chain of argument—neither of which hold up to scrutiny. Consequently, we do not consider it a particularly plausible alternative interpretation of our results.

The first implication is a rejection of any conceptual distinction between reasoning affected by (a) the “true” latent cause of politically motivated reasoning and (b) prior factual beliefs. In other words, prior factual beliefs influence reasoning because (and *only* because) they are correlated with the true latent cause of politically motivated reasoning. But, as we have shown, this notion is roundly refuted: both by our arguments in the preceding paragraphs, and the empirical evidence that demonstrates that specific prior beliefs influence reasoning in domains entirely devoid of the context necessary for politically motivated reasoning (Evans et al., 1983;

Klauer et al., 2000; Koehler, 1993; Markovits & Nantel, 1989; Trippas et al., 2015).

The second implication is that political group identity and prior factual beliefs are confounded by a single common cause: that is, the true latent variable that drives politically motivated reasoning (e.g., pressure to conform to the positions of one's ingroup). However, as illustrated in Figure 1 (variable U) and by our discussion in the Rethinking Cognitive Sophistication and Politically Motivated Reasoning section in the introduction, there are numerous other plausible common causes of political group identity and prior factual beliefs: such as one's political information environment, comprising exposure to media, discussions with friends, family, and coworkers, and the complex (i.e., path-dependent and self-reinforcing) relationships between such variables. Accordingly, the same problem that we have characterized thus far also arises here: how to identify the single "true" latent cause of politically motivated reasoning when it is intermixed with these other, unobserved causes? This point emphasizes the limits of one-shot survey experiments in general (ours, as well as previous experiments on this topic) and further highlights the argument made throughout our paper: that the real-world prestudy context should be borne closely in mind when interpreting the patterns of reasoning observed in experiments.

A final alternative interpretation of our results that merits discussion invokes an alternative conception of politically motivated reasoning altogether, which could be called the "belief-based" conception. This conception defines politically motivated reasoning as the influence of specific prior beliefs on reasoning per se (Taber & Lodge, 2006); the underlying logic being that people desire to hold on to their beliefs because they are akin to closely guarded "possessions." This departs from the conception used in the current paper, which defined it as reasoning influenced by the motivation to form conclusions (and beliefs) congenial to the goals of the political group with which people identify. If one favors the former conception, our results pertaining to prior factual beliefs could be taken as stronger evidence that cognitive sophistication magnifies politically motivated reasoning.

However, this belief-based conception does not appear to be the dominant one in previous work on this topic. Furthermore, what classifies as politically motivated reasoning under this conception is somewhat ambiguous. For example, it appears as though classification depends upon some possibly unattainable normative criterion: Taber and Lodge (2006) lament that determining whether the influence of specific prior beliefs is "rational" skepticism or "irrational" bias is a "critical normative question," "but one that empirical research may not be able to address" (p. 768), because normative questions lie outside the purview of science. Added to this ambiguity, the human tendency to be skeptical of information that is incoherent with prior beliefs is plausibly explained by alternative motivations, such as to avoid being too easily manipulated into holding false and costly beliefs by other people (Mercier, 2017, 2020). Overall, the preceding discussions keenly demonstrate that empirical research on politically motivated reasoning would be greatly served by more rigorous theoretical definition of the concept itself.

A final point that is important to emphasize is the simplicity of the causal diagram depicted in Figure 1. As mentioned in the introduction, the primary function of this diagram was to illustrate the difficulty one faces in identifying clear evidence that cognitive

sophistication magnifies politically motivated reasoning—rather than to illustrate the true or most realistic causal model underlying the system. Thus, one could quite easily propose plausible modifications to this diagram that would render the direct effects of political identity and prior factual beliefs (as conceived here) unidentifiable in the causal sense. For example, drawing an arrow from U to the outcome variable (reasoning) would be one such modification. This arrow could represent the possibility that some people remember an argument made by their coworker regarding the issue under study (e.g., gun control laws) and use this memory to inform their reasoning in the experiment, rather than their own prior factual belief on the issue or their political group identity.

Another related possibility not represented in Figure 1 is that prior factual beliefs may cause political group identities: an arrow from the former to the latter (instead of or as well as the other way around). Indeed, existing work suggests that specific prior beliefs about issues can cause political group identities (Gärtner, Schoen, & Wuttke, 2020; Mummolo, Peterson, & Westwood, 2019), as well as vice versa (Lenz, 2012); and the precise nature of the relationship between these variables is an area of active research and ongoing debate (Fowler, 2020a, 2020b; Rogers, 2020).

Importantly, however, the artificial simplicity of the causal diagram in Figure 1 does not undermine the overriding implication of our results. Which is that existing empirical evidence is largely ambiguous with respect to the question of whether cognitive sophistication magnifies politically motivated reasoning (as commonly understood) versus some other driver of reasoning—such as sensitivity to incoherence between new information and prior factual beliefs. Adding more complexity to the causal diagram like that described above would primarily serve to further undercut the clarity of the existing evidence. Instead, in our view the main implication of the artificial simplicity in Figure 1 is that our model estimates should not be interpreted as de facto causal effects of either political group identity nor prior factual beliefs on reasoning. On the contrary, we would encourage future work to criticize, modify, and empirically test our causal diagram to shed additional light on the complex relationships between political group identities, prior factual beliefs, and reasoning.

Conclusion

Understanding why cognitive sophistication tends to predict increased partisan disagreement over policy-relevant facts is a project of considerable importance. Here we critically evaluated a candidate hypothesis suggested by recent work: namely, that cognitive sophistication magnifies politically motivated reasoning. Based on our argument and results, our conclusion is that existing evidence cannot reasonably distinguish between this hypothesis and the alternative hypothesis of a magnified effect of prior factual beliefs on reasoning. To confidently assess whether cognitive sophistication magnifies politically motivated reasoning demands evidence originating from study designs that obviate the confounding influence of such prior beliefs.

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Studies 1 and 2

Treatment Information: Study 1

The wording of the treatment information in Study 1 is displayed below (blue text indicates wording in the believers-are-open-minded treatment; red text wording in the skeptics-are-open-minded treatment):

In this part of the survey, you will read about an "Open-Mindedness Test" recently developed by psychologists to measure open-minded thinking.

As part of the test, people are asked how much they agree or disagree with several statements. The higher peoples' score on these statements, the more open-minded they are said to be. These statements are written below.

Open-Mindedness Test statements:

1. Allowing oneself to be convinced by an opposing argument is a sign of good character.
2. People should take into consideration evidence that goes against their beliefs.
3. People should revise their beliefs in response to new information or evidence.

Psychologists are still researching the validity of the test.

Suppose that research finds that people who **accept** [**reject**] evidence of climate change tend to score higher on the test than people who **reject** [**accept**] evidence of climate change.

If the test is a valid way to measure open-mindedness, that finding would imply that people who **believe** [**are skeptical**] climate change is happening are more open-minded than people who **are skeptical** [**believe**] that climate change is happening.

Were research to find that result, would you agree or disagree that the Open-Mindedness Test supplies good evidence of how open-minded someone is?

Treatment Information: Study 2

The wording of the treatment information in Study 2 is displayed below (blue text indicates wording in the believers-are-open-minded treatment; red text wording in the skeptics-are-open-minded treatment):

In this part of the survey, you will read about an "Open-Mindedness Test" recently developed by psychologists to measure open-minded thinking.

As part of the test, people are asked how much they agree or disagree with several statements. The higher peoples' score on these statements, the more open-minded they are said to be. These statements are written below.

Open-Mindedness Test statements:

1. Allowing oneself to be convinced by an opposing argument is a sign of good character.
2. People should take into consideration evidence that goes against their beliefs.
3. People should revise their beliefs in response to new information or evidence.

Psychologists believe these questions measure how open-minded someone is.

Among a group of participants in one recent study, the researchers found that people who **accept** **reject** evidence of climate change tend to score higher on the test than people who **reject** **accept** evidence of climate change.

If the test is a valid way to measure open-mindedness, that finding could imply that people who **believe** **are skeptical** climate change is happening are more open-minded than people who **are skeptical** **believe** that climate change is happening.

Would you agree or disagree that the Open-Mindedness Test supplies good evidence of how open-minded someone is?

Descriptive Statistics and Model Results: Studies 1 and 2

Figures S1 and S2 show distributions of key variables in Studies 1 and 2, respectively.

Tables S1-S3 and S4-S6 show the full model results from Studies 1 and 2, respectively.

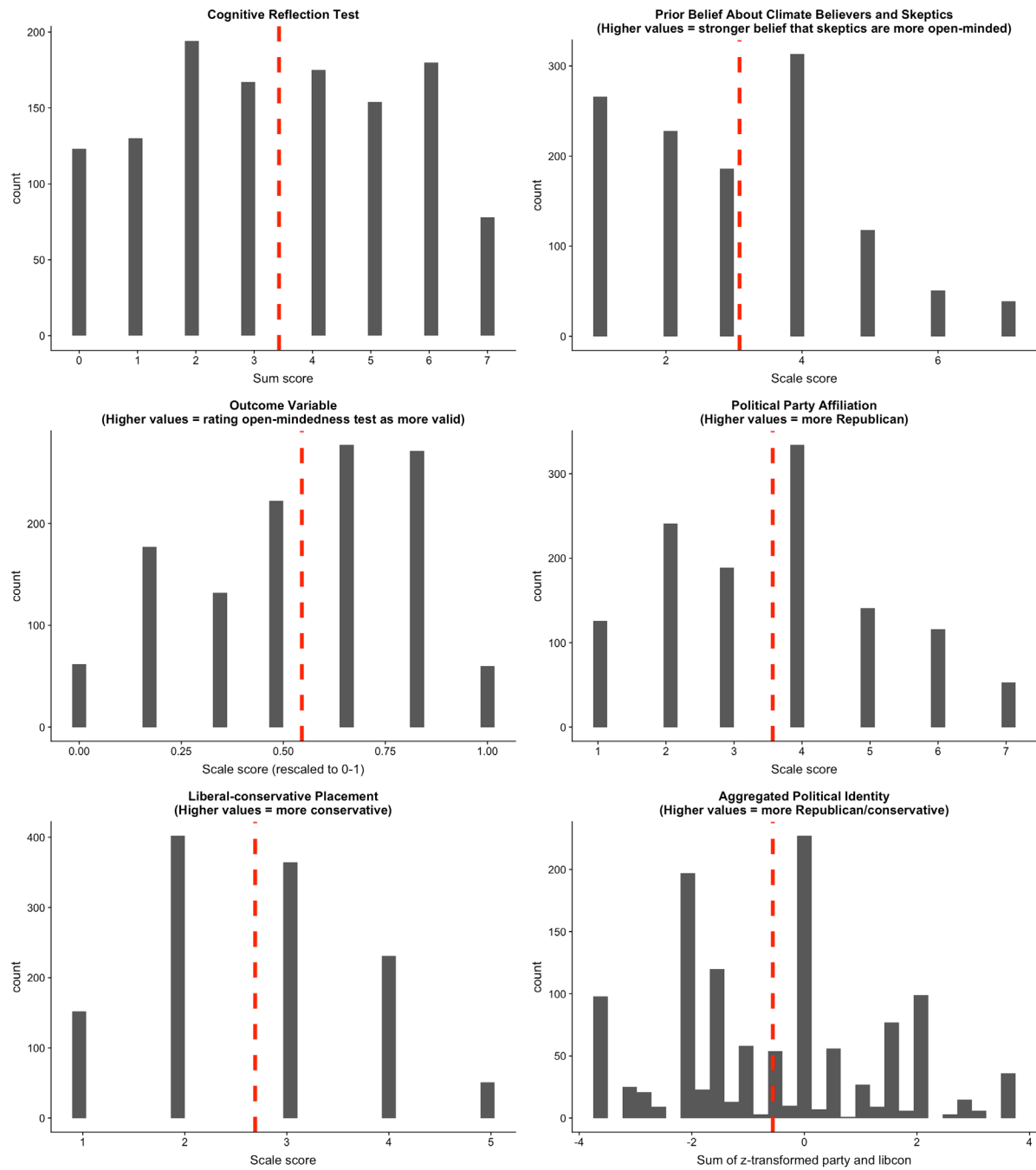


Figure S1. Distributions of key variables in Study 1. The dashed red line indicates the mean.

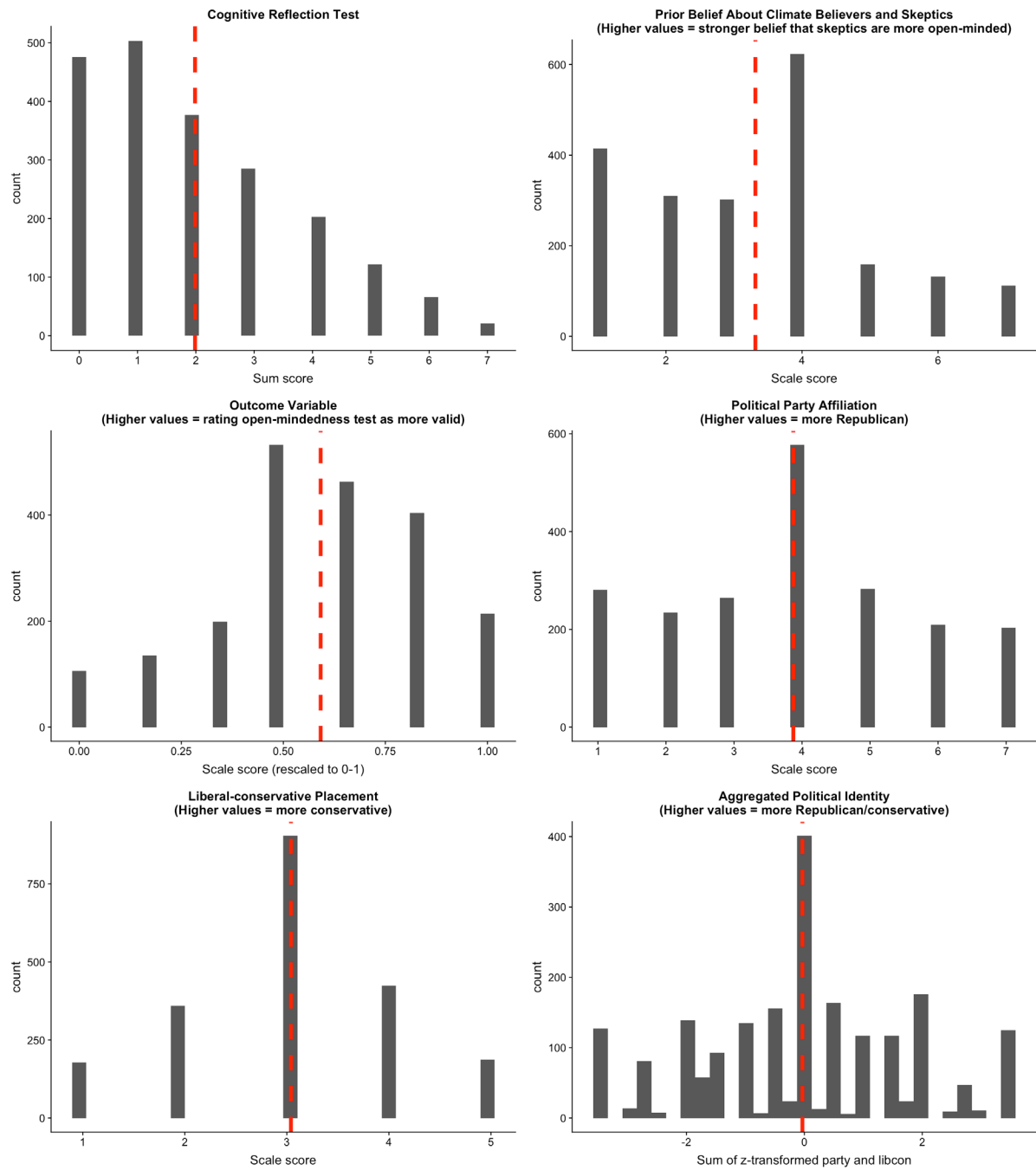


Figure S2. Distributions of key variables in Study 2. The dashed red line indicates the mean.

Table S1. OLS regression results from model with political identity only (Study 1).

Term	Estimate	Std. error	Statistic	<i>p</i> -value	95 LL	95 UL
(Intercept)	0.637	0.021	30.158	< .001	0.595	0.678
Treatment	-0.092	0.029	-3.154	.002	-0.150	-0.035
Political identity	-0.027	0.011	-2.478	.013	-0.048	-0.006
CRT	-0.014	0.005	-2.700	.007	-0.025	-0.004
Treatment x Pol ID	0.057	0.015	3.833	< .001	0.028	0.087
Treatment x CRT	0.001	0.007	0.150	.881	-0.013	0.016
Pol ID x CRT	-0.001	0.003	-0.422	.673	-0.006	0.004
Treatment x Pol ID x CRT	0.001	0.004	0.332	.740	-0.006	0.009

Note. N = 1200. CRT = Cognitive Reflection Test.

Table S2. OLS regression results from model with prior beliefs only (Study 1).

Term	Estimate	Std. error	Statistic	<i>p</i> -value	95 LL	95 UL
(Intercept)	0.614	0.022	27.732	< .001	0.571	0.658
Treatment	-0.052	0.030	-1.704	.089	-0.111	0.008
Prior belief	-0.055	0.018	-3.005	.003	-0.091	-0.019
CRT	-0.021	0.006	-3.757	< .001	-0.032	-0.010
Treatment x Prior	0.125	0.026	4.881	< .001	0.075	0.175
Treatment x CRT	0.009	0.008	1.175	.240	-0.006	0.024
Prior x CRT	-0.013	0.005	-2.630	.009	-0.022	-0.003
Treatment x Prior x CRT	0.015	0.007	2.248	.025	0.002	0.028

Note. N = 1201. CRT = Cognitive Reflection Test.

Table S3. OLS regression results from model with political identity and prior beliefs (Study 1).

Term	Estimate	Std. error	Statistic	<i>p</i> -value	95 LL	95 UL
(Intercept)	0.614	0.022	27.768	< .001	0.571	0.657
Treatment	-0.053	0.030	-1.766	.078	-0.113	0.006
Political identity	-0.017	0.012	-1.414	.158	-0.040	0.006
CRT	-0.021	0.006	-3.810	< .001	-0.032	-0.010
Prior belief	-0.042	0.021	-2.060	.040	-0.083	-0.002
Treatment x Pol ID	0.036	0.016	2.221	.027	0.004	0.067
Treatment x CRT	0.010	0.008	1.344	.179	-0.005	0.026
Pol ID x CRT	0.001	0.003	0.343	.732	-0.005	0.007
Treatment x Prior	0.099	0.028	3.473	.001	0.043	0.155
CRT x Prior	-0.013	0.005	-2.460	.014	-0.024	-0.003
Treatment x Pol ID x CRT	-0.002	0.004	-0.495	.621	-0.010	0.006
Treatment x Prior x CRT	0.016	0.008	2.113	.035	0.001	0.031

Note. N = 1200. CRT = Cognitive Reflection Test.

Table S4. OLS regression results from model with political identity only (Study 2).

Term	Estimate	Std. error	Statistic	<i>p</i> -value	95 LL	95 UL
(Intercept)	0.632	0.012	53.206	< .001	0.609	0.655
Treatment	-0.014	0.017	-0.809	.419	-0.048	0.020
Political identity	-0.015	0.007	-2.315	.021	-0.028	-0.002
CRT	-0.008	0.005	-1.669	.095	-0.016	0.001
Treatment x Pol ID	0.018	0.009	1.924	.054	0.000	0.037
Treatment x CRT	-0.018	0.007	-2.721	.007	-0.031	-0.005
Pol ID x CRT	-0.003	0.002	-1.313	.189	-0.008	0.002
Treatment x Pol ID x CRT	0.010	0.003	2.863	.004	0.003	0.017

Note. N = 2052. CRT = Cognitive Reflection Test.

Table S5. OLS regression results from model with prior beliefs only (Study 2).

Term	Estimate	Std. error	Statistic	<i>p</i> -value	95 LL	95 UL
(Intercept)	0.617	0.013	49.179	< .001	0.592	0.641
Treatment	0.004	0.018	0.221	.825	-0.032	0.040
Prior belief	-0.041	0.012	-3.465	.001	-0.064	-0.018
CRT	-0.012	0.005	-2.318	.021	-0.021	-0.002
Treatment x Prior	0.042	0.017	2.550	.011	0.010	0.075
Treatment x CRT	-0.010	0.007	-1.360	.174	-0.023	0.004
Prior x CRT	-0.007	0.005	-1.472	.141	-0.017	0.002
Treatment x Prior x CRT	0.022	0.007	3.250	.001	0.009	0.036

Note. N = 2053. CRT = Cognitive Reflection Test.

Table S6. OLS regression results from model with political identity and prior beliefs (Study 2).

Term	Estimate	Std. error	Statistic	<i>p</i> -value	95 LL	95 UL
(Intercept)	0.618	0.013	49.389	< .001	0.594	0.643
Treatment	0.002	0.018	0.122	.903	-0.033	0.038
Political identity	-0.011	0.007	-1.673	.095	-0.024	0.002
CRT	-0.010	0.005	-2.083	.037	-0.020	-0.001
Prior belief	-0.039	0.012	-3.227	.001	-0.062	-0.015
Treatment x Pol ID	0.015	0.010	1.524	.128	-0.004	0.033
Treatment x CRT	-0.012	0.007	-1.680	.093	-0.026	0.002
Pol ID x CRT	-0.002	0.003	-0.642	.521	-0.007	0.003
Treatment x Prior	0.041	0.017	2.419	.016	0.008	0.074
CRT x Prior	-0.004	0.005	-0.801	.423	-0.015	0.006
Treatment x Pol ID x CRT	0.006	0.004	1.593	.111	-0.001	0.013
Treatment x Prior x CRT	0.015	0.007	1.971	.049	0.000	0.029

Note. N = 2052. CRT = Cognitive Reflection Test.

Study 3

Treatment Information

The wording of the treatment information is displayed below (blue text indicates wording in the pro-gun-control treatment; red text wording in the anti-gun-control treatment):

Below is the summary of a recent study on gun control policy in the US. This is a real study from the scientific literature. Please read the summary.

Study summary

Gius (2015) studied the relationship between background checks on gun sellers and gun-related murder rates across all 50 US states. He used a dataset covering the period 1980-2011.

The study examined whether the requirement of background checks on **licensed gun sellers (dealers)** [**unlicensed gun sellers (private sellers)**] was associated with rates of gun-related murder at the state-level.

Prior to conducting his analysis, Gius excluded data points that were extreme outliers (i.e., murder rates that were unusually different compared to the majority). In total, this amounted to excluding 18 data points from the full dataset of 1564 observations. Each observation represents a single state's murder rate in a given year.

As part of his analysis strategy, Gius took into account the fact that murder rates tend to differ over time at the state-level. Therefore, these differences could not explain his results.

The results of his analysis showed that states that require such background checks tended to have **lower** [**higher**] rates of gun-related murder.

This result implies that stricter gun control laws may **reduce** [**increase**] gun crime.

The study is published in the *Journal of Applied Economics*.

Descriptive Statistics and Model Results

Figure S3 shows the distributions of key variables in Study 3. Tables S7-S9 show the full model results from Study 3. As can be seen from Figure S3, the prior belief variable has a bimodal distribution. We thus computed a binary indicator for the prior belief variable and refitted all models as a robustness check. This binary indicator was coded 1 if subjects reported

on the original 0-100 scale that their belief was that stricter gun control laws would NOT reduce gun crime in the US (scale score < 50); coded 0 otherwise (scale score \geq 50). The three-way interaction estimates from these models are displayed in Figure S4; the results are qualitatively identical to that of the preregistered analyses.

Original Two-Wave Design Model Results

As described in the main text, our original aim in Study 3 was to collect data across two waves. However, due to the unexpected size of the attrition rate in this two-wave design—and resultant loss of statistical power—we instead designed a one-wave version of the study, as reported in the main text. However, for transparency reasons we repeat all of the Study 3 analyses reported in the main text also on the sample from the original two-wave design. The three-way interaction estimates from the relevant models are displayed in Figure S5. Note that the pattern is qualitatively similar to the Study 3 results reported in the main text, but the uncertainty around the estimates is greater. This is a function of there being only $N = 851$ in the original two-wave design (vs. $N = 1977$ in the updated one-wave design reported in the main text).

Deviations from Preregistered Protocol

As described in the main text, in Study 3 we adopted a slightly different operationalization of the political identity variable than what we preregistered. In particular, our new approach was to average the unstandardized party affiliation and liberal-conservative placement variables, and then standardize the resulting composite variable. We did this to make the various interaction estimates directly comparable. For transparency, we repeat the Study 3 analyses reported in the main text using the preregistered operationalization of political identity; computed by *summing* the standardized party affiliation and liberal-conservative placement variables. The estimates are displayed in Figure S6; the results are substantively identical.

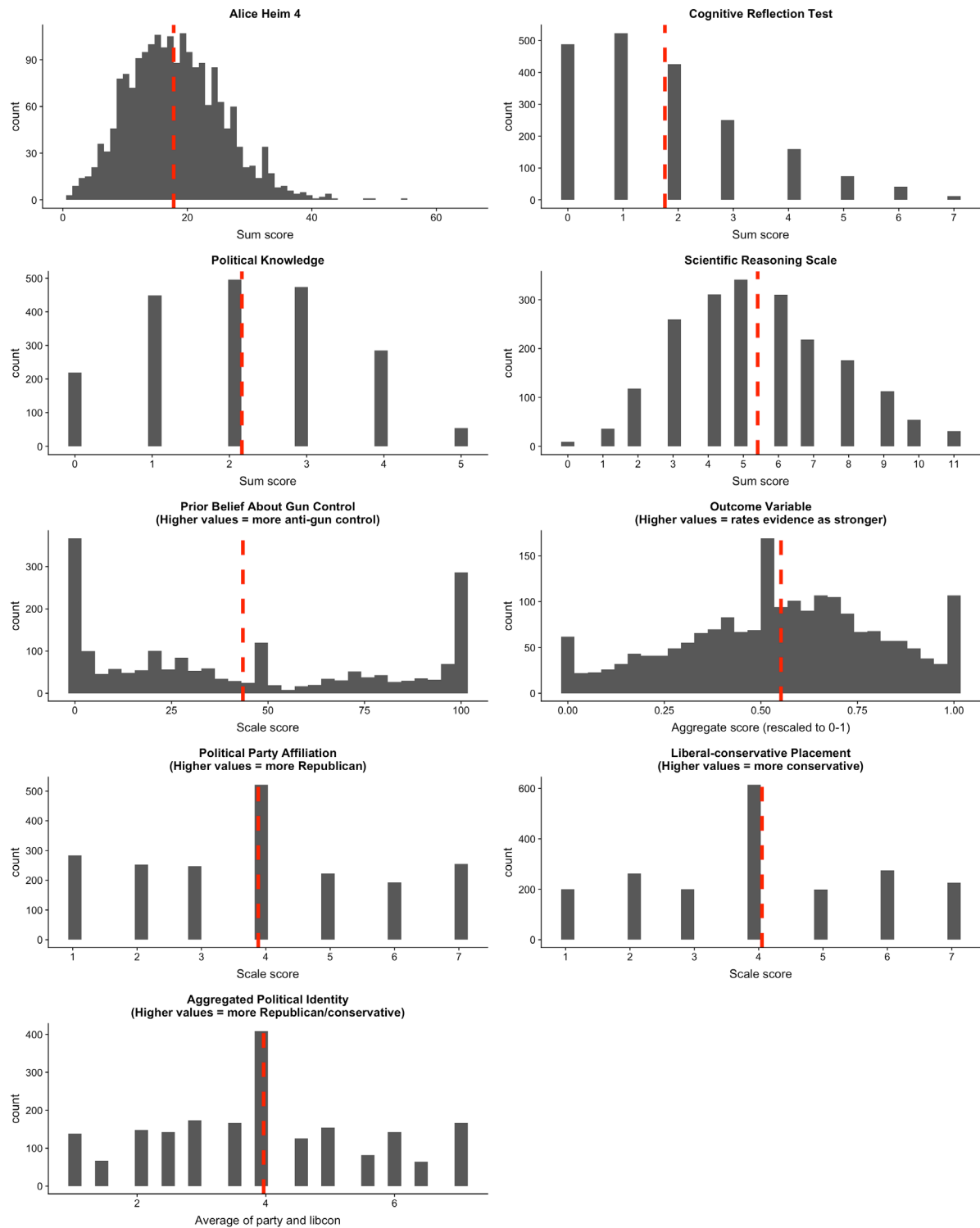


Figure S3. Distributions of key variables in Study 3. The dashed red line indicates the mean.

Table S7. OLS regression results from models with political identity only (Study 3).

Indicator	Term	Est.	SE	Stat.	p-value	98.75 LL	98.75 UL
AH4	(Intercept)	0.586	0.008	75.708	< .001	0.567	0.606
	Political identity	-0.090	0.008	-11.628	< .001	-0.109	-0.071
	Treatment	-0.074	0.011	-6.726	< .001	-0.101	-0.046
	AH4	-0.025	0.008	-3.311	.001	-0.044	-0.006
	Pol ID x Treatment	0.084	0.011	7.638	< .001	0.057	0.112
	Pol ID x AH4	-0.025	0.008	-3.215	.001	-0.044	-0.006
	Treatment x AH4	-0.024	0.011	-2.206	.027	-0.052	0.003
	Pol ID x Treatment x AH4	0.036	0.011	3.289	.001	0.009	0.063
CRT	(Intercept)	0.587	0.008	76.131	< .001	0.568	0.606
	Political identity	-0.090	0.008	-11.693	< .001	-0.109	-0.071
	Treatment	-0.073	0.011	-6.634	< .001	-0.1	-0.045
	CRT	-0.034	0.008	-4.396	< .001	-0.054	-0.015
	Pol ID x Treatment	0.084	0.011	7.689	< .001	0.057	0.111
	Pol ID x CRT	-0.021	0.008	-2.854	.004	-0.04	-0.003
	Treatment x CRT	-0.014	0.011	-1.287	.198	-0.041	0.013
	Pol ID x Treatment x CRT	0.037	0.011	3.397	.001	0.01	0.063
PK	(Intercept)	0.591	0.008	77.033	< .001	0.572	0.610
	Political identity	-0.080	0.008	-10.186	< .001	-0.099	-0.060
	Treatment	-0.077	0.011	-7.119	< .001	-0.105	-0.050
	PK	-0.026	0.008	-3.335	.001	-0.045	-0.006
	Pol ID x Treatment	0.076	0.011	6.849	< .001	0.048	0.104
	Pol ID x PK	-0.045	0.008	-6.019	< .001	-0.064	-0.027
	Treatment x PK	-0.034	0.011	-3.106	.002	-0.061	-0.007
	Pol ID x Treatment x PK	0.058	0.011	5.280	< .001	0.030	0.085
SRS	(Intercept)	0.586	0.008	77.231	< .001	0.567	0.605
	Political identity	-0.089	0.008	-11.734	< .001	-0.108	-0.070
	Treatment	-0.070	0.011	-6.511	< .001	-0.097	-0.043
	SRS	-0.039	0.008	-5.071	< .001	-0.058	-0.020
	Pol ID x Treatment	0.079	0.011	7.362	< .001	0.053	0.106
	Pol ID x SRS	-0.026	0.007	-3.472	.001	-0.045	-0.007
	Treatment x SRS	-0.036	0.011	-3.376	.001	-0.063	-0.009
	Pol ID x Treatment x SRS	0.034	0.011	3.245	.001	0.008	0.061

Note. N = 1977. AH4 = Alice Heim 4; CRT = Cognitive Reflection Test; PK = Political Knowledge; SRS = Scientific Reasoning Scale. All cognitive indicators are standardized sum scores.

Table S8. OLS regression results from models with prior beliefs only (Study 3).

Indicator	Term	Est.	SE	Stat.	p-value	98.75 LL	98.75 UL
AH4	(Intercept)	0.592	0.007	82.583	< .001	0.574	0.610
	Prior belief	-0.150	0.007	-21.203	< .001	-0.168	-0.133
	Treatment	-0.083	0.010	-8.175	< .001	-0.109	-0.058
	AH4	-0.014	0.007	-1.988	.047	-0.032	0.004
	Prior x Treatment	0.131	0.010	12.833	< .001	0.105	0.156
	Prior x AH4	-0.025	0.007	-3.451	.001	-0.043	-0.007
	Treatment x AH4	-0.030	0.010	-2.945	.003	-0.056	-0.005
	Prior x Treatment x AH4	0.070	0.011	6.569	< .001	0.043	0.096
CRT	(Intercept)	0.591	0.007	81.492	< .001	0.573	0.609
	Prior belief	-0.150	0.007	-20.875	< .001	-0.168	-0.132
	Treatment	-0.080	0.010	-7.780	< .001	-0.105	-0.054
	CRT	-0.014	0.007	-1.922	.055	-0.032	0.004
	Prior x Treatment	0.126	0.010	12.317	< .001	0.101	0.152
	Prior x CRT	-0.005	0.007	-0.655	.513	-0.022	0.013
	Treatment x CRT	-0.031	0.010	-3.051	.002	-0.057	-0.006
	Prior x Treatment x CRT	0.046	0.010	4.531	< .001	0.021	0.071
PK	(Intercept)	0.593	0.007	83.022	< .001	0.575	0.611
	Prior belief	-0.147	0.007	-20.584	< .001	-0.164	-0.129
	Treatment	-0.080	0.010	-7.943	< .001	-0.106	-0.055
	PK	-0.016	0.007	-2.296	.022	-0.034	0.001
	Prior x Treatment	0.122	0.010	12.012	< .001	0.097	0.147
	Prior x PK	-0.031	0.007	-4.341	< .001	-0.049	-0.013
	Treatment x PK	-0.042	0.010	-4.167	< .001	-0.067	-0.017
	Prior x Treatment x PK	0.063	0.010	6.251	< .001	0.038	0.089
SRS	(Intercept)	0.591	0.007	83.704	< .001	0.573	0.608
	Prior belief	-0.150	0.007	-21.536	< .001	-0.168	-0.133
	Treatment	-0.076	0.010	-7.624	< .001	-0.101	-0.051
	SRS	-0.028	0.007	-3.937	< .001	-0.046	-0.010
	Prior x Treatment	0.125	0.010	12.542	< .001	0.101	0.150
	Prior x SRS	-0.013	0.007	-1.805	.071	-0.030	0.005
	Treatment x SRS	-0.045	0.010	-4.532	< .001	-0.070	-0.020
	Prior x Treatment x SRS	0.046	0.010	4.598	< .001	0.021	0.071

Note. N = 1977. AH4 = Alice Heim 4; CRT = Cognitive Reflection Test; PK = Political Knowledge; SRS = Scientific Reasoning Scale. All cognitive indicators are standardized sum scores.

Table S9. OLS regression results from models with political identity and prior beliefs (Study 3).

Indicator	Term	Est.	SE	Stat.	<i>p</i> -value	98.75 LL	98.75 UL
AH4	(Intercept)	0.591	0.007	82.212	< .001	0.573	0.609
	Political identity	-0.024	0.008	-2.924	.003	-0.044	-0.003
	Treatment	-0.083	0.010	-8.112	< .001	-0.108	-0.057
	AH4	-0.016	0.007	-2.183	.029	-0.033	0.002
	Prior belief	-0.139	0.008	-17.248	< .001	-0.159	-0.119
	Pol ID x Treatment	0.025	0.012	2.116	.034	-0.004	0.054
	Pol ID x AH4	-0.003	0.008	-0.308	.758	-0.023	0.018
	Treatment x AH4	-0.028	0.010	-2.699	.007	-0.054	-0.002
	Prior x Treatment	0.120	0.012	10.268	< .001	0.091	0.149
	Prior x AH4	-0.022	0.008	-2.606	.009	-0.042	-0.001
	Pol ID x Treatment x AH4	-0.008	0.012	-0.690	.490	-0.037	0.021
	Prior x Treatment x AH4	0.072	0.012	5.907	< .001	0.041	0.102
CRT	(Intercept)	0.589	0.007	81.087	< .001	0.571	0.607
	Political identity	-0.027	0.008	-3.311	.001	-0.047	-0.007
	Treatment	-0.078	0.010	-7.603	< .001	-0.103	-0.052
	CRT	-0.016	0.007	-2.151	.032	-0.034	0.003
	Prior belief	-0.136	0.008	-16.657	< .001	-0.157	-0.116
	Pol ID x Treatment	0.029	0.012	2.445	.015	-0.001	0.058
	Pol ID x CRT	-0.011	0.009	-1.303	.193	-0.033	0.010
	Treatment x CRT	-0.030	0.010	-2.891	.004	-0.055	-0.004
	Prior x Treatment	0.112	0.012	9.579	< .001	0.083	0.142
	Prior x CRT	0.004	0.009	0.495	.621	-0.017	0.026
	Pol ID x Treatment x CRT	0.008	0.012	0.658	.511	-0.022	0.038
	Prior x Treatment x CRT	0.039	0.012	3.196	.001	0.008	0.069
PK	(Intercept)	0.593	0.007	82.915	< .001	0.575	0.611
	Political identity	-0.018	0.008	-2.221	.026	-0.039	0.002
	Treatment	-0.080	0.010	-7.903	< .001	-0.105	-0.055
	PK	-0.017	0.007	-2.303	.021	-0.035	0.001
	Prior belief	-0.136	0.008	-16.835	< .001	-0.156	-0.115
	Pol ID x Treatment	0.025	0.012	2.137	.033	-0.004	0.054
	Pol ID x PK	-0.019	0.008	-2.286	.022	-0.040	0.002
	Treatment x PK	-0.043	0.010	-4.205	< .001	-0.068	-0.017
	Prior x Treatment	0.108	0.012	9.328	< .001	0.079	0.137
	Prior x PK	-0.017	0.009	-1.975	.048	-0.039	0.005
	Pol ID x Treatment x PK	0.018	0.012	1.491	.136	-0.012	0.048
	Prior x Treatment x PK	0.049	0.012	4.048	< .001	0.019	0.079
SRS	(Intercept)	0.589	0.007	83.482	< .001	0.571	0.607
	Political identity	-0.026	0.008	-3.195	.001	-0.046	-0.006
	Treatment	-0.075	0.010	-7.517	< .001	-0.100	-0.050
	SRS	-0.030	0.007	-4.141	< .001	-0.047	-0.012
	Prior belief	-0.137	0.008	-17.135	< .001	-0.157	-0.117
	Pol ID x Treatment	0.023	0.011	2.009	.045	-0.006	0.052
	Pol ID x SRS	-0.012	0.008	-1.386	.166	-0.032	0.009
	Treatment x SRS	-0.044	0.010	-4.393	< .001	-0.069	-0.019
	Prior x Treatment	0.115	0.011	9.972	< .001	0.086	0.143
	Prior x SRS	-0.003	0.008	-0.345	.730	-0.024	0.018
	Pol ID x Treatment x SRS	0.000	0.012	0.016	.987	-0.029	0.030
	Prior x Treatment x SRS	0.043	0.012	3.541	< .001	0.013	0.073

Note. N = 1977. AH4 = Alice Heim 4; CRT = Cognitive Reflection Test; PK = Political Knowledge; SRS = Scientific Reasoning Scale. All cognitive indicators are standardized sum scores.

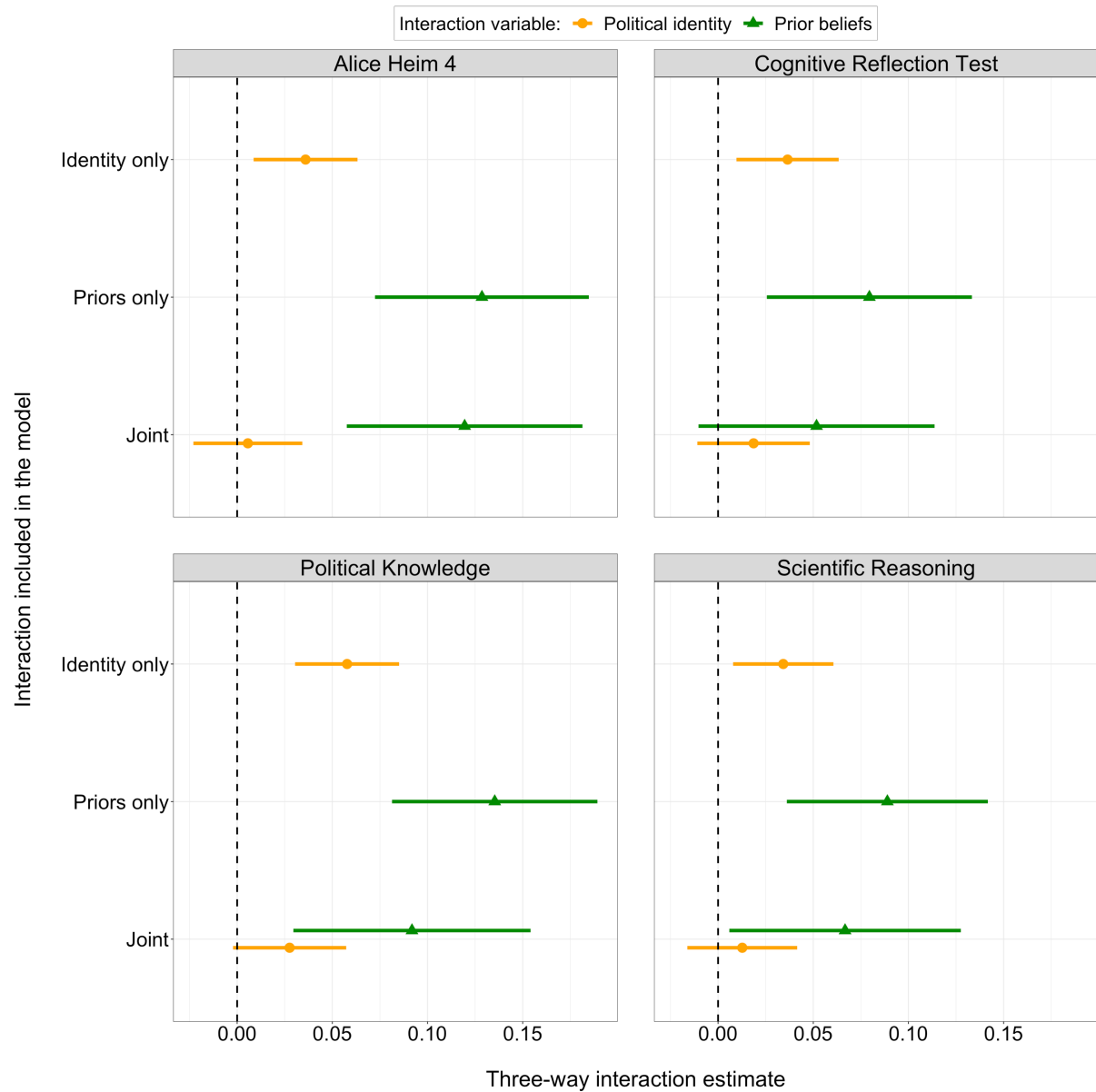


Figure S4. Results of robustness check interaction tests from Study 3 (with prior belief variable binary coded). Points are three-way interaction estimates from the respective models indexed on the y -axis. Panels refer to the cognitive indicator included in the model. Aside from political identity or prior beliefs, and the respective cognitive indicator, the other variable in each interaction term is treatment assignment (dummy-coded). Whiskers are 98.75% CI, representing a p -value threshold of .0125 (as preregistered).

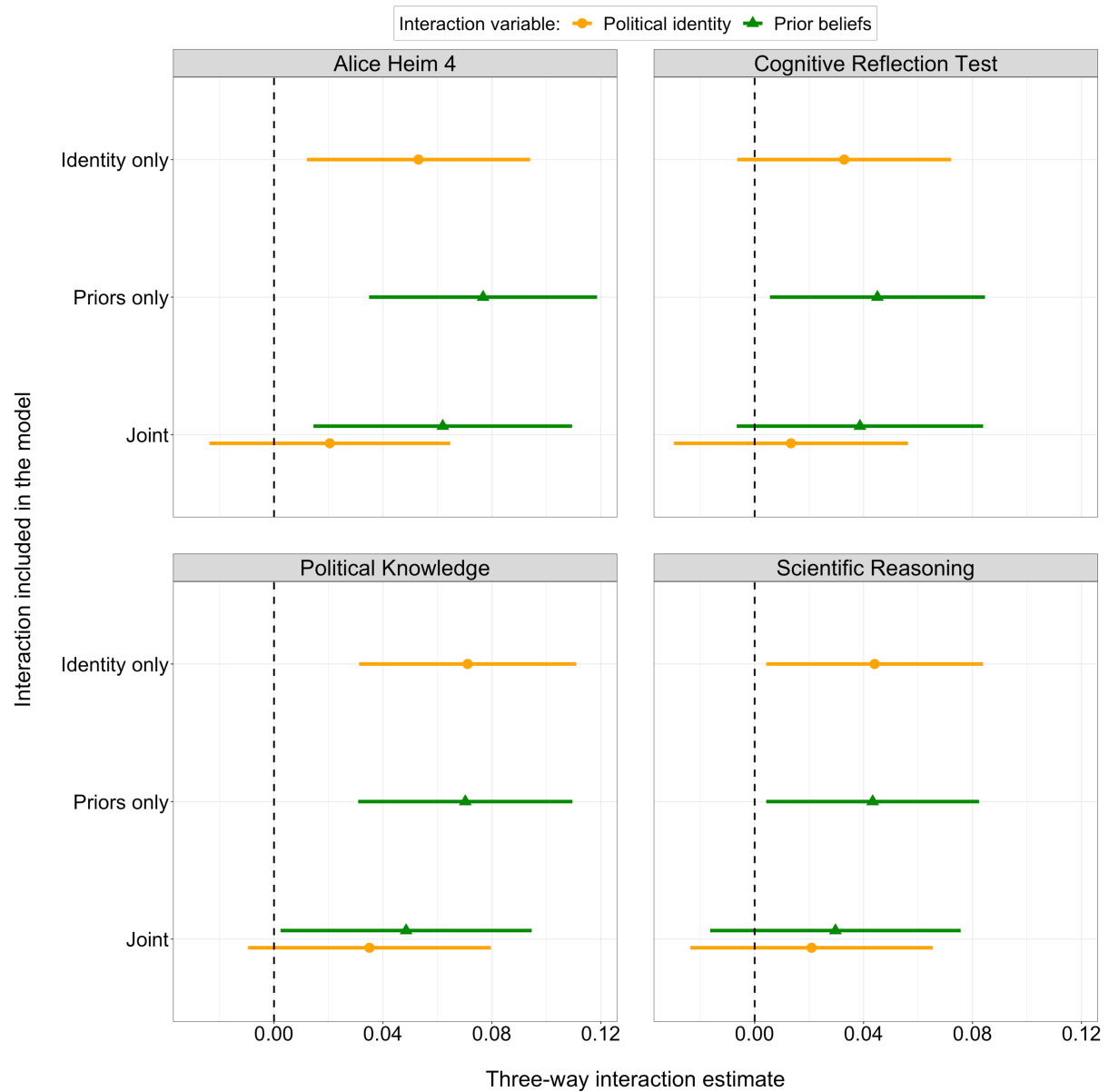


Figure S5. Results of interaction tests from Study 3 original two-wave design. Points are three-way interaction estimates from the respective models indexed on the y -axis. Panels refer to the cognitive indicator included in the model. Aside from political identity or prior beliefs, and the respective cognitive indicator, the other variable in each interaction term is treatment assignment (dummy-coded). Whiskers are 98.75% CI, representing a p -value threshold of .0125 (as preregistered).

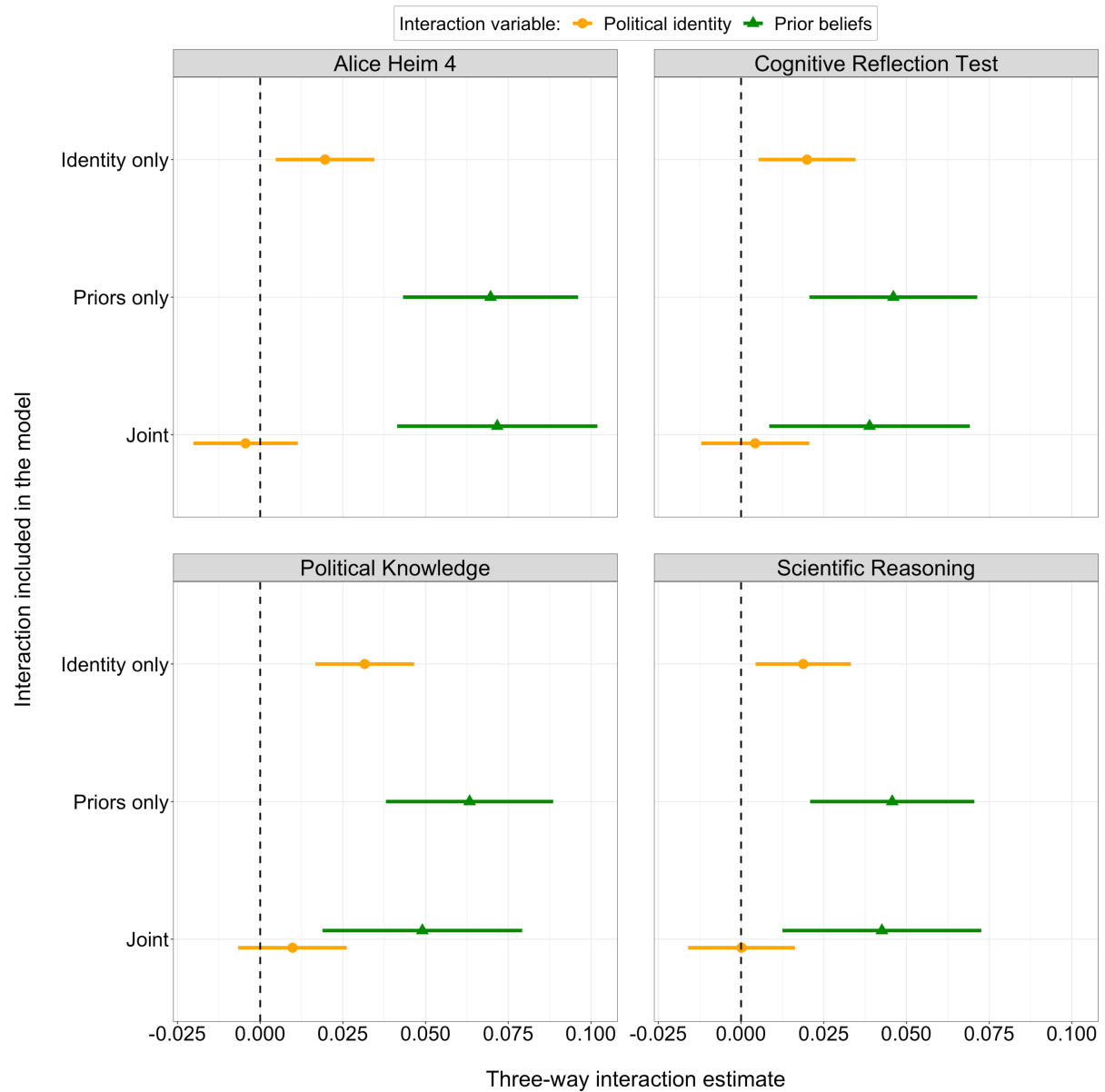


Figure S6. Results of interaction tests from Study 3 using the preregistered operationalization of political identity. Points are three-way interaction estimates from the respective models indexed on the y -axis. Panels refer to the cognitive indicator included in the model. Aside from political identity or prior beliefs, and the respective cognitive indicator, the other variable in each interaction term is treatment assignment (dummy-coded). Whiskers are 98.75% CI, representing a p -value threshold of .0125 (as preregistered).

Further Supplementary Analysis of Studies 1-3: Two-way interactions only

Here we report the results of the analyses that jointly model the two-way interaction between (i) treatment assignment and political identity and (ii) treatment assignment and prior belief i.e., leaving out the cognitive sophistication variables.

Table S10. Two-way interactions model from Study 1.

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.545	0.012	44.209	< .001	0.521	0.569
Treatment	-0.020	0.017	-1.183	.237	-0.053	0.013
Political identity	-0.013	0.006	-2.151	.032	-0.024	-0.001
Prior belief	-0.083	0.012	-7.225	< .001	-0.106	-0.061
Treatment x Pol ID	0.030	0.008	3.590	< .001	0.014	0.047
Treatment x Prior	0.149	0.016	9.325	< .001	0.118	0.181

Table S11. Two-way interactions model from Study 2.

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.599	0.009	69.604	< .001	0.582	0.616
Treatment	-0.024	0.012	-1.934	.053	-0.048	0.000
Political identity	-0.015	0.005	-3.277	.001	-0.024	-0.006
Prior belief	-0.046	0.008	-5.438	< .001	-0.062	-0.029
Treatment x Pol ID	0.028	0.006	4.251	< .001	0.015	0.040
Treatment x Prior	0.067	0.012	5.628	< .001	0.043	0.090

Table S12. Two-way interactions model from Study 3.

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.589	0.007	80.819	< .001	0.571	0.608
Treatment	-0.076	0.010	-7.331	< .001	-0.102	-0.050
Political identity	-0.025	0.008	-3.098	.002	-0.046	-0.005
Prior belief	-0.140	0.008	-17.226	< .001	-0.161	-0.120
Treatment x Pol ID	0.033	0.012	2.839	.005	0.004	0.063
Treatment x Prior	0.111	0.012	9.419	< .001	0.081	0.140

Further Supplementary Analysis of Studies 1-3: Party ID and Ideology ID separately

Here we repeat the main analyses that used the composite political identity variable but we conduct these analyses separately for U.S. party identification and liberal-conservative ideology variables (i.e., not the composite of these). The results are presented in the tables below.

Table S13. OLS regression results from model with ideological identity only (Study 1).

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.639	0.021	30.309	< .001	0.597	0.680
Treatment	-0.092	0.029	-3.161	.002	-0.150	-0.035
Ideological identity	-0.040	0.020	-1.951	.051	-0.080	0.000
CRT	-0.014	0.005	-2.729	.006	-0.025	-0.004
Treatment x Ideo ID	0.101	0.028	3.561	< .001	0.045	0.156
Treatment x CRT	0.001	0.007	0.098	.922	-0.014	0.015
Ideo ID x CRT	-0.004	0.005	-0.729	.466	-0.013	0.006
Treatment x Ideo ID x CRT	0.004	0.007	0.494	.621	-0.010	0.017

Note. Ideological identity refers to the liberal-conservative scale.

Table S14. OLS regression results from model with U.S. party identity only (Study 1).

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.638	0.021	30.283	< .001	0.597	0.680
Treatment	-0.096	0.029	-3.285	.001	-0.153	-0.039
Party identity	-0.053	0.021	-2.567	.010	-0.093	-0.012
CRT	-0.014	0.005	-2.651	.008	-0.024	-0.004
Treatment x Party ID	0.100	0.028	3.544	< .001	0.045	0.155
Treatment x CRT	0.001	0.007	0.100	.920	-0.014	0.015
Party ID x CRT	-0.002	0.005	-0.299	.765	-0.011	0.008
Treatment x Party ID x CRT	0.003	0.007	0.382	.703	-0.011	0.017

Note. Party identity refers to the U.S. party identification scale.

Table S15. OLS regression results from model with ideological identity and prior beliefs (S1).

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.614	0.022	27.785	< .001	0.571	0.658
Treatment	-0.053	0.030	-1.741	.082	-0.112	0.007
Ideological identity	-0.017	0.022	-0.764	.445	-0.060	0.026
CRT	-0.021	0.006	-3.822	< .001	-0.032	-0.010
Prior belief	-0.048	0.020	-2.360	.018	-0.089	-0.008
Treatment x Ideo ID	0.058	0.030	1.933	.054	-0.001	0.117
Treatment x CRT	0.010	0.008	1.330	.184	-0.005	0.026
Ideo ID x CRT	-0.001	0.005	-0.117	.907	-0.011	0.010
Treatment x Prior	0.104	0.028	3.687	< .001	0.049	0.159
CRT x Prior	-0.012	0.005	-2.309	.021	-0.023	-0.002
Treatment x Ideo ID x CRT	-0.002	0.008	-0.228	.820	-0.017	0.013
Treatment x Prior x CRT	0.015	0.007	2.021	.044	0.000	0.029

Table S16. OLS regression results from model with U.S. party identity and prior beliefs (S1).

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.614	0.022	27.732	< .001	0.570	0.657
Treatment	-0.053	0.030	-1.763	.078	-0.113	0.006
Party identity	-0.039	0.022	-1.783	.075	-0.081	0.004
CRT	-0.021	0.006	-3.779	< .001	-0.032	-0.010
Prior belief	-0.041	0.020	-2.072	.038	-0.081	-0.002
Treatment x Party ID	0.064	0.030	2.153	.032	0.006	0.123
Treatment x CRT	0.010	0.008	1.307	.191	-0.005	0.026
Party ID x CRT	0.003	0.005	0.619	.536	-0.007	0.014
Treatment x Prior	0.102	0.028	3.644	< .001	0.047	0.157
CRT x Prior	-0.014	0.005	-2.573	.010	-0.024	-0.003
Treatment x Party ID x CRT	-0.004	0.008	-0.570	.569	-0.019	0.011
Treatment x Prior x CRT	0.016	0.007	2.152	.032	0.001	0.031

Table S17. OLS regression results from model with ideological identity only (Study 2).

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.634	0.012	53.383	< .001	0.610	0.657
Treatment	-0.017	0.017	-0.994	.320	-0.051	0.017
Ideological identity	-0.035	0.012	-2.989	.003	-0.058	-0.012
CRT	-0.007	0.005	-1.614	.107	-0.016	0.002
Treatment x Ideo ID	0.038	0.017	2.241	.025	0.005	0.071
Treatment x CRT	-0.018	0.007	-2.730	.006	-0.031	-0.005
Ideo ID x CRT	-0.005	0.004	-1.077	.282	-0.013	0.004
Treatment x Ideo ID x CRT	0.018	0.006	2.832	.005	0.006	0.030

Table S18. OLS regression results from model with U.S. party identity only (Study 2).

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.631	0.012	52.895	< .001	0.607	0.654
Treatment	-0.011	0.017	-0.645	.519	-0.046	0.023
Party identity	-0.011	0.012	-0.965	.335	-0.035	0.012
CRT	-0.008	0.005	-1.708	.088	-0.017	0.001
Treatment x Party ID	0.016	0.017	0.971	.332	-0.017	0.050
Treatment x CRT	-0.018	0.007	-2.731	.006	-0.031	-0.005
Party ID x CRT	-0.008	0.005	-1.788	.074	-0.018	0.001
Treatment x Party ID x CRT	0.020	0.007	3.077	.002	0.007	0.033

Table S19. OLS regression results from model with ideological identity and prior beliefs (S2).

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.620	0.013	49.524	< .001	0.595	0.644
Treatment	0.000	0.018	-0.010	.992	-0.036	0.035
Ideological identity	-0.030	0.012	-2.551	.011	-0.053	-0.007
CRT	-0.010	0.005	-2.025	.043	-0.020	0.000
Prior belief	-0.039	0.012	-3.251	.001	-0.062	-0.015
Treatment x Ideo ID	0.034	0.017	1.999	.046	0.001	0.067
Treatment x CRT	-0.012	0.007	-1.685	.092	-0.026	0.002
Ideo ID x CRT	-0.002	0.005	-0.365	.715	-0.011	0.007
Treatment x Prior	0.042	0.017	2.494	.013	0.009	0.075
CRT x Prior	-0.004	0.005	-0.761	.446	-0.014	0.006
Treatment x Ideo ID x CRT	0.010	0.007	1.511	.131	-0.003	0.023
Treatment x Prior x CRT	0.014	0.007	1.959	.050	0.000	0.029

Table S20. OLS regression results from model with U.S. party identity and prior beliefs (S2).

Term	Estimate	Std. Error	Statistic	p-value	95 LL	95 UL
(Intercept)	0.617	0.013	49.247	< .001	0.592	0.641
Treatment	0.005	0.018	0.268	.789	-0.031	0.041
Party identity	-0.004	0.012	-0.322	.747	-0.027	0.020
CRT	-0.011	0.005	-2.184	.029	-0.021	-0.001
Prior belief	-0.041	0.012	-3.407	.001	-0.065	-0.017
Treatment x Party ID	0.010	0.017	0.593	.553	-0.024	0.044
Treatment x CRT	-0.011	0.007	-1.613	.107	-0.025	0.002
Party ID x CRT	-0.005	0.005	-1.032	.302	-0.015	0.005
Treatment x Prior	0.043	0.017	2.506	.012	0.009	0.076
CRT x Prior	-0.005	0.005	-0.930	.353	-0.015	0.005
Treatment x Party ID x CRT	0.012	0.007	1.753	.080	-0.001	0.026
Treatment x Prior x CRT	0.016	0.007	2.205	.028	0.002	0.031

Table S21. OLS regression results from model with ideological identity only (Study 3).

Indicator	Term	Est.	SE	Stat.	p-value	98.75 LL	98.75 UL
AH4	(Intercept)	0.587	0.008	75.322	< .001	0.568	0.607
	Ideological identity	-0.083	0.008	-10.594	< .001	-0.102	-0.063
	Treatment	-0.075	0.011	-6.752	< .001	-0.102	-0.047
	AH4	-0.026	0.008	-3.426	.001	-0.045	-0.007
	Ideo ID x Treatment	0.071	0.011	6.385	< .001	0.043	0.099
	Ideo ID x AH4	-0.025	0.008	-3.245	.001	-0.044	-0.006
	Treatment x AH4	-0.023	0.011	-2.070	.039	-0.051	0.005
	Ideo ID x Treatment x AH4	0.036	0.011	3.333	.001	0.009	0.063
CRT	(Intercept)	0.588	0.008	75.923	< .001	0.569	0.607
	Ideological identity	-0.082	0.008	-10.612	< .001	-0.102	-0.063
	Treatment	-0.073	0.011	-6.662	< .001	-0.101	-0.046
	CRT	-0.037	0.008	-4.689	< .001	-0.056	-0.017
	Ideo ID x Treatment	0.071	0.011	6.420	< .001	0.043	0.098
	Ideo ID x CRT	-0.026	0.007	-3.497	< .001	-0.045	-0.007
	Treatment x CRT	-0.012	0.011	-1.067	.286	-0.039	0.016
	Ideo ID x Treatment x CRT	0.037	0.011	3.479	.001	0.010	0.064
PK	(Intercept)	0.592	0.008	76.929	< .001	0.573	0.611
	Ideological identity	-0.075	0.008	-9.670	< .001	-0.094	-0.056
	Treatment	-0.079	0.011	-7.192	< .001	-0.106	-0.051
	PK	-0.026	0.008	-3.324	.001	-0.045	-0.006
	Ideo ID x Treatment	0.067	0.011	6.066	< .001	0.039	0.094
	Ideo ID x PK	-0.048	0.007	-6.625	< .001	-0.067	-0.030
	Treatment x PK	-0.033	0.011	-3.063	.002	-0.061	-0.006
	Ideo ID x Treatment x PK	0.059	0.011	5.509	< .001	0.032	0.086
SRS	(Intercept)	0.587	0.008	77.029	< .001	0.568	0.606
	Ideological identity	-0.081	0.008	-10.545	< .001	-0.100	-0.062
	Treatment	-0.071	0.011	-6.598	< .001	-0.098	-0.044
	SRS	-0.040	0.008	-5.200	< .001	-0.059	-0.021
	Ideo ID x Treatment	0.067	0.011	6.152	< .001	0.040	0.094
	Ideo ID x SRS	-0.034	0.008	-4.435	< .001	-0.053	-0.015
	Treatment x SRS	-0.035	0.011	-3.237	.001	-0.062	-0.008
	Ideo ID x Treatment x SRS	0.042	0.011	3.936	< .001	0.015	0.068

Note. AH4 = Alice Heim 4; CRT = Cognitive Reflection Test; PK = Political Knowledge; SRS = Scientific Reasoning Scale. All cognitive indicators are standardized sum scores.

Table S22. OLS regression results from model with U.S. party identity only (Study 3).

Indicator	Term	Est.	SE	Stat.	p-value	98.75 LL	98.75 UL
AH4	(Intercept)	0.586	0.008	75.449	< .001	0.567	0.606
	Party identity	-0.085	0.008	-11.118	< .001	-0.104	-0.066
	Treatment	-0.074	0.011	-6.730	< .001	-0.102	-0.047
	AH4	-0.023	0.008	-3.083	.002	-0.042	-0.004
	Party ID x Treatment	0.087	0.011	7.861	< .001	0.059	0.115
	Party ID x AH4	-0.034	0.008	-4.351	< .001	-0.053	-0.014
	Treatment x AH4	-0.027	0.011	-2.385	.017	-0.054	0.001
	Party ID x Treatment x AH4	0.042	0.011	3.847	< .001	0.015	0.069
CRT	(Intercept)	0.587	0.008	75.701	< .001	0.568	0.606
	Party identity	-0.084	0.008	-10.974	< .001	-0.103	-0.065
	Treatment	-0.073	0.011	-6.660	< .001	-0.101	-0.046
	CRT	-0.033	0.008	-4.191	< .001	-0.052	-0.013
	Party ID x Treatment	0.087	0.011	7.867	< .001	0.059	0.114
	Party ID x CRT	-0.024	0.008	-3.092	.002	-0.044	-0.005
	Treatment x CRT	-0.016	0.011	-1.459	.145	-0.043	0.011
	Party ID x Treatment x CRT	0.042	0.011	3.720	< .001	0.014	0.070
PK	(Intercept)	0.590	0.008	76.723	< .001	0.571	0.609
	Party identity	-0.077	0.008	-10.022	< .001	-0.096	-0.058
	Treatment	-0.077	0.011	-7.035	< .001	-0.104	-0.049
	PK	-0.027	0.008	-3.544	< .001	-0.047	-0.008
	Party ID x Treatment	0.081	0.011	7.334	< .001	0.053	0.108
	Party ID x PK	-0.049	0.008	-6.378	< .001	-0.068	-0.030
	Treatment x PK	-0.033	0.011	-2.999	.003	-0.060	-0.005
	Party ID x Treatment x PK	0.061	0.011	5.497	< .001	0.033	0.088
SRS	(Intercept)	0.586	0.008	76.715	< .001	0.567	0.605
	Party identity	-0.084	0.008	-11.126	< .001	-0.103	-0.065
	Treatment	-0.070	0.011	-6.469	< .001	-0.097	-0.043
	SRS	-0.038	0.008	-4.890	< .001	-0.057	-0.018
	Party ID x Treatment	0.081	0.011	7.480	< .001	0.054	0.108
	Party ID x SRS	-0.025	0.008	-3.318	.001	-0.044	-0.006
	Treatment x SRS	-0.037	0.011	-3.459	.001	-0.065	-0.010
	Party ID x Treatment x SRS	0.032	0.011	2.959	.003	0.005	0.059

Note. AH4 = Alice Heim 4; CRT = Cognitive Reflection Test; PK = Political Knowledge; SRS = Scientific Reasoning Scale. All cognitive indicators are standardized sum scores.

Table S23. OLS regression results from model with ideological identity and prior beliefs (S3).

Indicator	Term	Est.	SE	Stat.	p-value	98.75 LL	98.75 UL
AH4	(Intercept)	0.592	0.007	82.245	< .001	0.574	0.610
	Ideological identity	-0.022	0.008	-2.763	.006	-0.042	-0.002
	Treatment	-0.083	0.010	-8.158	< .001	-0.109	-0.058
	AH4	-0.015	0.007	-2.145	.032	-0.033	0.003
	Prior belief	-0.142	0.008	-18.102	< .001	-0.161	-0.122
	Ideo ID x Treatment	0.013	0.011	1.156	.248	-0.015	0.041
	Ideo ID x AH4	0.002	0.008	0.230	.818	-0.018	0.021
	Treatment x AH4	-0.030	0.010	-2.864	.004	-0.055	-0.004
	Prior x Treatment	0.126	0.011	11.204	< .001	0.098	0.155
	Prior x AH4	-0.024	0.008	-2.938	.003	-0.044	-0.004
	Ideo ID x Treatment x AH4	-0.008	0.011	-0.755	.450	-0.036	0.019
	Prior x Treatment x AH4	0.072	0.012	6.100	< .001	0.042	0.101
CRT	(Intercept)	0.589	0.007	81.037	< .001	0.571	0.607
	Ideological identity	-0.026	0.008	-3.270	.001	-0.046	-0.006
	Treatment	-0.078	0.010	-7.614	< .001	-0.104	-0.052
	CRT	-0.016	0.007	-2.225	.026	-0.035	0.002
	Prior belief	-0.138	0.008	-17.362	< .001	-0.158	-0.118
	Ideo ID x Treatment	0.016	0.011	1.389	.165	-0.013	0.044
	Ideo ID x CRT	-0.013	0.008	-1.515	.130	-0.034	0.008
	Treatment x CRT	-0.030	0.010	-2.898	.004	-0.055	-0.004
	Prior x Treatment	0.120	0.011	10.512	< .001	0.091	0.148
	Prior x CRT	0.005	0.008	0.596	.551	-0.016	0.026
	Ideo ID x Treatment x CRT	0.006	0.012	0.493	.622	-0.023	0.035
	Prior x Treatment x CRT	0.041	0.012	3.431	.001	0.011	0.071
PK	(Intercept)	0.593	0.007	82.963	< .001	0.575	0.611
	Ideological identity	-0.018	0.008	-2.238	.025	-0.038	0.002
	Treatment	-0.080	0.010	-7.909	< .001	-0.105	-0.055
	PK	-0.016	0.007	-2.244	.025	-0.034	0.002
	Prior belief	-0.137	0.008	-17.414	< .001	-0.157	-0.118
	Ideo ID x Treatment	0.014	0.011	1.255	.210	-0.014	0.043
	Ideo ID x PK	-0.015	0.008	-1.910	.056	-0.035	0.005
	Treatment x PK	-0.042	0.010	-4.174	< .001	-0.068	-0.017
	Prior x Treatment	0.114	0.011	10.114	< .001	0.086	0.143
	Prior x PK	-0.020	0.008	-2.454	.014	-0.041	0.000
	Ideo ID x Treatment x PK	0.015	0.011	1.291	.197	-0.014	0.043
	Prior x Treatment x PK	0.053	0.012	4.589	< .001	0.024	0.083
SRS	(Intercept)	0.589	0.007	83.563	< .001	0.572	0.607
	Ideological identity	-0.024	0.008	-3.116	.002	-0.044	-0.005
	Treatment	-0.075	0.010	-7.522	< .001	-0.100	-0.050
	SRS	-0.030	0.007	-4.174	< .001	-0.048	-0.012
	Prior belief	-0.138	0.008	-17.786	< .001	-0.158	-0.119
	Ideo ID x Treatment	0.013	0.011	1.159	.247	-0.015	0.041
	Ideo ID x SRS	-0.013	0.008	-1.603	.109	-0.034	0.007
	Treatment x SRS	-0.044	0.010	-4.431	< .001	-0.069	-0.019
	Prior x Treatment	0.120	0.011	10.731	< .001	0.092	0.148
	Prior x SRS	-0.003	0.008	-0.309	.757	-0.023	0.018
	Ideo ID x Treatment x SRS	0.004	0.012	0.312	.755	-0.025	0.032
	Prior x Treatment x SRS	0.042	0.012	3.598	< .001	0.013	0.071

Table S24. OLS regression results from model with U.S. party identity and prior beliefs (S3).

Indicator	Term	Est.	SE	Stat.	p-value	98.75 LL	98.75 UL
AH4	(Intercept)	0.591	0.007	82.308	< .001	0.573	0.609
	Party identity	-0.020	0.008	-2.545	.011	-0.040	0.000
	Treatment	-0.082	0.010	-8.103	< .001	-0.108	-0.057
	AH4	-0.015	0.007	-2.169	.030	-0.033	0.002
	Prior belief	-0.140	0.008	-17.62	< .001	-0.160	-0.121
	Party ID x Treatment	0.030	0.012	2.626	.009	0.001	0.059
	Party ID x AH4	-0.009	0.008	-1.174	.240	-0.029	0.011
	Treatment x AH4	-0.027	0.010	-2.632	.009	-0.053	-0.001
	Prior x Treatment	0.117	0.012	10.128	< .001	0.088	0.145
	Prior x AH4	-0.019	0.008	-2.373	.018	-0.040	0.001
	Party ID x Treatment x AH4	-0.002	0.011	-0.172	.864	-0.030	0.026
	Prior x Treatment x AH4	0.069	0.012	5.806	< .001	0.039	0.099
CRT	(Intercept)	0.589	0.007	81.236	< .001	0.571	0.607
	Party identity	-0.022	0.008	-2.787	.005	-0.042	-0.002
	Treatment	-0.079	0.010	-7.692	< .001	-0.104	-0.053
	CRT	-0.015	0.007	-2.028	.043	-0.033	0.003
	Prior belief	-0.139	0.008	-17.246	< .001	-0.159	-0.119
	Party ID x Treatment	0.035	0.012	3.064	.002	0.007	0.064
	Party ID x CRT	-0.010	0.009	-1.154	.248	-0.032	0.012
	Treatment x CRT	-0.031	0.010	-2.980	.003	-0.056	-0.005
	Prior x Treatment	0.110	0.012	9.484	< .001	0.081	0.139
	Prior x CRT	0.002	0.008	0.218	.827	-0.019	0.022
	Party ID x Treatment x CRT	0.011	0.012	0.896	.370	-0.019	0.041
	Prior x Treatment x CRT	0.039	0.012	3.303	.001	0.009	0.068
PK	(Intercept)	0.593	0.007	82.975	< .001	0.575	0.611
	Party identity	-0.016	0.008	-2.069	.039	-0.036	0.003
	Treatment	-0.080	0.010	-7.944	< .001	-0.106	-0.055
	PK	-0.017	0.007	-2.369	.018	-0.035	0.001
	Prior belief	-0.137	0.008	-17.255	< .001	-0.157	-0.117
	Party ID x Treatment	0.032	0.011	2.789	.005	0.003	0.061
	Party ID x PK	-0.022	0.009	-2.529	.012	-0.043	0.000
	Treatment x PK	-0.043	0.010	-4.213	< .001	-0.068	-0.017
	Prior x Treatment	0.106	0.011	9.239	< .001	0.077	0.134
	Prior x PK	-0.017	0.009	-2.015	.044	-0.039	0.004
	Party ID x Treatment x PK	0.021	0.012	1.711	.087	-0.010	0.051
	Prior x Treatment x PK	0.048	0.012	4.037	< .001	0.018	0.078
SRS	(Intercept)	0.589	0.007	83.478	< .001	0.572	0.607
	Party identity	-0.021	0.008	-2.709	.007	-0.041	-0.002
	Treatment	-0.076	0.010	-7.568	< .001	-0.101	-0.051
	SRS	-0.029	0.007	-4.056	< .001	-0.047	-0.011
	Prior belief	-0.140	0.008	-17.817	< .001	-0.159	-0.120
	Party ID x Treatment	0.027	0.011	2.416	.016	-0.001	0.056
	Party ID x SRS	-0.009	0.008	-1.158	.247	-0.030	0.011
	Treatment x SRS	-0.044	0.010	-4.383	< .001	-0.069	-0.019
	Prior x Treatment	0.113	0.011	9.971	< .001	0.085	0.141
	Prior x SRS	-0.006	0.008	-0.700	.484	-0.026	0.015
	Party ID x Treatment x SRS	-0.001	0.012	-0.075	.940	-0.030	0.029
	Prior x Treatment x SRS	0.044	0.012	3.738	< .001	0.015	0.073