Updating amidst Disagreement: New Experimental Evidence on Partisan Cues

Anthony Fowler[1]* (iD), William G. Howell[2]

[1]Professor, Harris School of Public Policy, University of Chicago, Chicago, IL, US
[2]Sydney Stein Professor in American Politics, Harris School of Public Policy and Chair of the Department of Political Science, University of Chicago, Chicago, IL, US

Abstract In this era of hyper-polarization and partisan animosity, do people incorporate the viewpoints of their political opponents? Perhaps not. An important body of research, in fact, finds that the provision of information about opponents' policy views leads survey respondents to reflexively adopt the *opposite* position. In this paper, we demonstrate that such findings arise from incomplete experimental designs and a particular measurement strategy. In a series of experiments that vary information about both parties' positions simultaneously and that solicit continuous, rather than discrete, policy positions, we find that partisans update their beliefs in accordance with the positions of Republican and Democratic leaders alike. Partisans are not perennially determined to disagree. Rather, they are often willing to incorporate opposing viewpoints about a wide range of policy issues.

Rank partisanship has become so acute, many argue, that previously civil disagreements now are infused with malice and enmity (Iyengar and Westwood 2015; Mason 2018), just as psychological attachments to social identities displace political values and policy preferences (Achen and Bartels 2016; c.f. Fowler 2020). This depiction of contemporary politics is buttressed by a startling empirical finding: partisans purportedly update their policy positions negatively in response to the positions of the other party (e.g., Nicholson 2012). Nicole Satherley and her colleagues (2018) summarize the empirical regularity succinctly: "If they say 'yes,' we say 'no.'"

This paper presents new evidence that supports alternative conclusions. Democratic and Republican individuals, we find, do not reflexively oppose the positions of the other party. Rather, information about the positions of leaders from both parties pushes respondents' views in the same direction.

*Corresponding author: Anthony Fowler, Harris School of Public Policy, University of Chicago, 1307 East 60th St, #2031, Chicago, IL 60637, US. Email: anthony.fowler@uchicago.edu

As we would expect if partisans selected their party on the basis of shared values and principles, information about one's own party has a larger effect. Still, even strong and committed partisans update their views in accordance with the positions of the opposition.

Two methodological issues explain why previous studies do not find evidence of positive updating—that is, policy adjustments that align with elite cues. The first issue concerns the satisfaction of all-else-equal requirements—or what Dafoe, Zhang, and Caughey (2018) call "information equivalence"—when manipulating survey respondents' beliefs. As others have shown in the context of conjoint experiments (e.g., Hainmueller, Hopkins, and Yamamoto 2014), the random alteration of one fact about a politician's identity may shift subjects' beliefs about other facts; and consequently, the application of one treatment can unwittingly induce other hidden treatments. Similar dynamics may be at work in studies of partisan cues. When informed about the position of an opposing party, a survey respondent may also update her beliefs about the position of her own party. For all that has been written on partisan cues (see Bullock 2020 for a review), however, none of the studies that document negative updating estimate the effect of information about one party's positions while holding constant information about the other party's position.[1]

The second issue concerns the measurement of respondents' policy preferences. Nearly exclusively, research on partisan cues either presents discrete policy options of the experimenter's choosing or measures support for a single policy proposal. And there are good reasons for doing so. Important domains of politics, after all, are organized around well-defined and usually competing policy choices. As Sniderman and Bullock point out, a basic responsibility of parties and candidates is to "reduce the number of [policy] alternatives open to choice to only a few—indeed, frequently to only two" (2004, p. 346).

When trying to estimate the effects of partisan cues, however, the presentation of continuous policy options may be preferred. Discrete policy questions, after all, may mask considerable variation in subjects' actual policy preferences. Moreover, assessments of both individual-level updating and group differences may hinge upon the selection of which particular policy options are presented. Whether a respondent is asked about her support of a minimum wage set at $12/hour or $19/hour will obviously affect our estimates of Americans' responsiveness to elite cues and their resulting levels of partisan disagreement. Additionally, continuous measures may do a better job of recovering subjects' *considered* policy preferences (Ansolabehere, Meredith, and Snowberg 2013). Rather than hurriedly selecting from a

1. Berinsky (2009, pp. 119–21) independently manipulates cues from both parties and finds no evidence of negative updating.

limited number of preselected options or gauging their support for any single one, respondents may reflect more on the merits of policy options that are presented continuously; and in so doing, they may respond in less partisan ways. And finally, there are plenty of public domains in which policy is discussed and debated in continuous terms, whether it involves the optimal level of military aid to be provided to Ukrainian soldiers, the size of the minimum wage, or the amount of money to be allocated for a new program.

To assess the relevance of these two methodological issues for our understanding of partisan cues, we conduct nine new survey experiments as well as additional replications and extensions of previous experiments that estimate the effect of information about party positions on respondents' policy views. In our main experiments, we independently randomize the stated positions of leaders from both parties, and we measure the preferred policies of respondents on the same continuous scale. Administered to both convenience and nationally representative samples, these experiments allow us to estimate the independent effects of cues about the positions of opposing politicians, both with and without partisan identifiers, for a wide range of foreign and domestic policies. Recognizing that citizens are often asked to choose between discrete options selected by elites, we also conduct experiments that present both discrete and continuous policy options, compare and contrast the results, and discuss the implications for public opinion and political discourse.

Across numerous settings and experimental manipulations, we find that partisans respond positively to the positions of their own party's leader, but the effect is far from proportionate. Our respondents do not automatically adopt the stated position of their party, but they do shift their positions in the expected direction. More interesting, and in contrast with the existing literature, partisans also respond positively to the positions of the other party's leaders. Though somewhat smaller in magnitude, the out-party effect is often substantively and statistically significant.

To better understand the discrepancy between our results and those of the previous literature, we revisit the widely regarded experiments presented in Nicholson (2012). First, we confirm that partisans respond negatively to the positions of the other party when they are given no information about their own party and are forced to choose between two policy options. Then, in several extensions, we show that the negative finding attenuates when respondents simultaneously receive independently varying information about their own party's position and when policy positions are stated and elicited in a continuous way.

Overall, our results underscore the value of political dialogue. Amidst all the disagreement and anger that characterize contemporary politics, Americans are not committed contrarians. Rather, they positively update their policy views in response to positions taken by their partisan opponents.

Literature Review

In this era of hyper-partisanship, scholars argue, Americans hold fast to their partisan identities and look upon members of the opposing party with not just skepticism but outright contempt (Iyengar and Westwood 2015; Mason 2018; c.f. Orr and Huber 2020). Partisanship influences the information to which individuals are exposed (e.g., Tyler, Grimmer, and Iyengar 2022), how they perceive that information (e.g., Campbell et al. 1960), and their factual beliefs (e.g., Bartels 2002; c.f. Bullock et al. 2015; Prior, Sood, and Khanna 2015; and Bullock and Lenz 2019). Most revealing, perhaps, partisanship is thought to be so strong that Americans instinctively recoil from the positions of the other party (Arceneaux and Kolodny 2009; Goren, Federico, and Kittilson 2009; Nicholson 2012; Satherley et al. 2018; Merkley and Stecula 2021).

This literature on partisan cues is closely related to research on partisan-motivated reasoning and backlash effects. In various studies, partisans appear to respond to the same information in diametrically opposed ways (Levendusky 2013; Leeper and Slothuus 2014; Druckman, Levendusky, and McLain 2018) and reaffirm, rather than reevaluate, their mistaken views in light of corrective facts (Nyhan and Riefler 2010). The accuracy and generalizability of these results, however, remains a matter of some dispute. Coppock (2021) finds that Democrats and Republicans typically shift their views similarly in response to policy information, although he acknowledges that this finding may not apply to group cues. Guess and Coppock (2020) find no evidence of backlash effects even in theoretically favorable conditions. Porter and Wood (2019, 2022) affirm the general efficacy of fact-checking.

Although a large literature investigates the influence of partisan cues and elite position-taking on public opinion (e.g., Iyengar and Kinder 1987; Jacoby 1988; Cohen 2003; Lenz 2009, 2012), studies typically do not distinguish the separate effects of in- and out-party positions. Numerous experimental studies deliver treatments in which two parties always disagree (Cohen 2003; Kam 2005; Levendusky 2010; Lavine, Johnston, and Steenbergen 2013; Samuels and Zucco 2014), and observational studies investigate settings in which a new issue emerges and the parties take divergent positions (e.g., Lenz 2009). Given this design, however, we can only recover the joint effects of both parties' positions. The individual effect of one party cannot be distinguished from that of the other.[2]

Other experimental studies that deliver partisan cues about only one party at a time recover either null or negative effects of out-party information

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2. Bisgaard and Slothuus (2018) examine an interesting case from Denmark in which the prime minister significantly increased the extent to which he discussed the budget deficit as a problem, while the opposition did not meaningfully change its position or rhetoric.

(Nicholson 2011, 2012; Barber and Pope 2019; Bakker, Lelkes, and Malka 2020; Merkley and Stecula 2021). In a world in which the major political parties typically disagree on the most salient policy questions of the day, however, partisans may reasonably infer that support by one party implies opposition from the other. Consequently, the finding that out-party positions negatively affect voter positions could arise because partisans update their beliefs about their own party's position.

To the extent that partisan cues influence public opinion, an ongoing debate persists about whether this is because partisans indiscriminately follow their leaders or, aware of their own information deficits, partisans reasonably update their beliefs after observing those of political leaders. While mimicry is the more common interpretation (e.g., Lenz 2012), partisan cues may facilitate independent reasoning (Bullock 2009, 2011; Hill and Huber 2019). Furthermore, when partisan cues relate to matters of broad public importance, some individuals shift their partisan leanings rather than their issue positions (e.g., Hart and Middleton 2014; Tesler 2015).

Collectively, these studies leave us with a puzzle. For all the accounts of hyper-partisanship, backlash effects, and negative partisan cues, actual public opinion is not nearly as polarized as one might expect. Even as divisions between elected officials have grown (McCarty 2019), the policy positions of most Americans remain ideologically moderate (Ansolabehere, Rodden, and Snyder 2006; Fiorina, Abrams, and Pope 2010; Fowler et al. 2022). How can this be? If Americans either dismiss or respond negatively to the positions of their political opponents, why don't we observe more public polarization? One potential explanation is that positive updating does in fact occur across party lines.

Experimental Design

Our experiments apply two lessons learned in other settings to the study of partisan cues. First, we randomize in- and out-party positions independently and present them both to subjects, which allows us to recover the independent effects of each. And second, whereas our main experiments only present and elicit continuous policy positions, our replications of past experiments also include treatment conditions with discrete policy options.

Our experiments were embedded in three different surveys. The first two were conducted through Amazon Mechanical Turk (MTurk) in September–October 2020 and in November 2020. The third was conducted through YouGov in June 2021. For convenience, we refer to these surveys as MTurk 1, MTurk 2, and YouGov, respectively.

The MTurk surveys were fielded amidst a politically divisive presidential campaign, whereas the YouGov survey was fielded during the contentious

first year of a new presidential administration. Although MTurk relies upon online convenience samples, they have been shown to produce experimental estimates that are similar to those generated by more representative samples (Berinsky, Huber, and Lenz 2012; Mullinix et al. 2015; Thomas and Clifford 2017; Coppock 2019). Furthermore, recent research suggests that the generalizability and reliability of online survey experiments with convenience samples have not changed in light of the COVID-19 pandemic (Peyton, Huber, and Coppock 2021). The YouGov sample is designed to be nationally representative after reweighting, and similar samples have been used extensively in published work across the social sciences. All subsequent analyses of the YouGov data utilize survey weights.[3]

Our samples consist of voting-age Americans who self-identify as either Democrats or Republicans on the standard three-point party identification question, and the YouGov sample also includes independents who report that they lean toward one party or another. For the MTurk samples, we utilized IP tracking to verify that each participant resided in the United States (Kennedy et al. 2020), and we utilized a screener question to remove inattentive respondents prior to the delivery of the treatments (Berinsky, Margolis, and Sances 2014). All participants agreed to join a research study, were paid for their time, and were debriefed afterward. All protocols were reviewed and approved by our university's Institutional Review Board. The YouGov survey and all corresponding analyses were preregistered. In the three respective surveys, we aimed for sample sizes of 1,500, 2,000, and 2,500.[4]

Recognizing that the effects of elite cues likely vary across issue domains, we study a wide range of policies, including the federal minimum wage, US federal aid to South Sudan, refugee admissions, COVID-19 relief spending, and infrastructure spending. When relevant, subjects were informed about the current status quo in the relevant policy domain. Subsequently, subjects were presented with varying positions for leaders of the two major parties. Because real-world cues typically come from individual politicians rather than official party platforms, this choice of treatment delivery should make

3. For more information on the construction of these survey weights, see Rivers (2007). Unfortunately, because our MTurk surveys rely on convenience samples and the YouGov survey relies on panelists, and because we cannot observe everyone who considered taking the survey, we cannot compute response rates. For the YouGov survey, 63.9 percent of the panelists invited to take our survey accepted, and 94.9 percent of those individuals completed the survey.

4. For the MTurk studies, we dropped all cases where a subject reattempted the survey. With MTurk 1, the outcome measures for the minimum wage and foreign aid experiments were not properly recorded for the initial wave of subjects, so we halted the survey, fixed the problem, and relaunched it. This means that in MTurk 1, we have approximately 1,500 subjects for the original experiments but approximately 2,100 for the replication and extension of Nicholson (2012). For the YouGov survey, we did not force subjects to answer all questions. As a result of these complications, the sample sizes for each analysis are slightly smaller than our targets.

our results more generalizable and politically relevant. Although we randomized the positions of the party leaders, we restricted the various treatments to positions that each politician could plausibly take. The order in which leaders' positions were presented was randomized, and the wording was kept as natural as possible while varying nothing other than the positions of the politicians. The precise question wordings and possible treatments for all experiments are shown in the Supplementary Material (pp. A6–A10).

At the end of each question, respondents were asked to share their preferred policy position on the same scale as the positions of the politicians. Answers were required to be numbers within a plausible range. If subjects failed to input an allowed answer, they received an error message informing them of these stated constraints, and they were further informed that if their policy preference exceeded our maximum to simply input the maximum value. The distributions of all outcome variables are shown in Supplementary Material figures A4–A6.

The MTurk 1 survey included experiments on the federal minimum wage, foreign aid to South Sudan, and a replication and extension of the experiments conducted by Nicholson (2012). The MTurk 2 survey replicated the experiments on minimum wage and foreign aid and also included an experiment on COVID-19 relief spending, which allows us to investigate the discrepancy between our results and those of other studies. The YouGov survey replicated the minimum wage and foreign aid experiments, added a similar experiment on refugee admissions, and replicated the design of the COVID-19 relief experiment but focused on infrastructure spending instead. Collectively, these experiments cover a broader range of topics than any previous study of partisan cues.

To further gauge the robustness and generalizability of our findings, the three surveys differed from one another in several other ways. Regarding the specific party leaders utilized, MTurk 1 and YouGov provided positions for Nancy Pelosi and Mitch McConnell, whereas MTurk 2 provided positions for Joe Biden and Donald Trump. To mitigate the possibility of demand effects, both of the MTurk surveys did not explicitly provide party labels for the leaders (although at the end of the surveys, we confirmed that the vast majority of respondents knew the parties of the leaders).[5] The YouGov survey randomly varied whether party labels were provided; and consistent with Broockman and Butler (2017), we find little evidence that this is consequential for our results (see Supplementary Material table A5). Finally, the

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5. In the MTurk 1 survey, 81 and 83 percent of respondents correctly identified the parties of Pelosi and McConnell, respectively. In the MTurk 2 survey, 93 and 95 percent of respondents correctly identified the parties of Biden and Trump, respectively. In the YouGov survey, 94 and 91 percent of the respondents who were not shown party labels correctly identified the parties of Pelosi and McConnell, respectively.

MTurk studies randomized across a limited number of possible policy positions for each party leader, whereas the YouGov study randomized across a large number of possible positions.[6]

Given our design, the average extent to which a partisan's views respond to those of party leaders can be estimated from the following regression:

$$Respondent\ Position = \alpha + \beta * in\text{-}party\ position + \gamma * out\text{-}party\ position \\ + \delta * Democrat + \varepsilon,$$

(1)

where *in-party position* is the stated position of the politician from the respondent's party (Pelosi or Biden for Democrats and McConnell or Trump for Republicans), *out-party position* is the stated position of the other politician, and *Democrat* is an indicator for the partisan identity of the respondent. Because these experiments are explicitly designed to estimate the relative importance of in- and out-party cues, our main analyses appropriately exclude a pure control group with no party cues.[7]

Conditional on the respondent's party, all variation in the positions of the in-party and out-party politicians is random. Consequently, the recovered regression coefficients associated with these variables should yield unbiased estimates of the extent to which the positions of leaders from each party affect respondents' policy positions. Controlling for the respondent's party improves precision by explaining variation in the dependent variable that is unrelated to the treatments, and it also removes bias that could arise from the fact that the distributions of in- and out-party positions differ by party. For transparency, we also present separate results by party.

Because the positions of politicians and respondents are measured on the same scale, the magnitudes of our estimated coefficients are substantively meaningful and readily interpretable. A coefficient of 1 would represent, on average, a one-to-one correspondence between the position of politicians and respondents. If respondents strictly adhere to their own leader's position, we should expect an in-party coefficient of 1 and an out-party coefficient of 0. If respondents are so partisan that they instinctively reject anything the other party proposes, we should expect a negative out-party coefficient. But if respondents draw upon their own values and opinions plus the information they glean from partisan cues to form their positions, we should expect both the in- and out-party coefficients to be positive but less than 1. And to the

6. For example, in the MTurk studies, the Democratic leader (Pelosi or Biden) could have been presented as supporting a federal minimum of $10, $12.50, or $15, whereas in the YouGov study, Pelosi's preferred federal minimum wage was randomly selected from all possible 5-cent increments between $11 and $15.

7. In a subsequent section, however, we present the results of a replication of Nicholson 2012, which includes treatment conditions with two, one, and no partisan cues.

extent that people select their party because of shared values with party leaders, we should expect the in-party coefficient to exceed the out-party coefficient. In the Supplementary Material (pp. A2–A5), we formally characterize these predictions with a model of Bayesian updating.

Results

Table 1 presents the results of our primary experiments of interest. The top panel shows the regression results for all nine of our original experiments, pooling across Democratic and Republican respondents. The middle panel shows the same results for only Democratic respondents, and the bottom panel shows results for only Republican respondents. The sample sizes are smaller for the experiments on COVID-19 relief spending and infrastructure spending because those experiments included other experimental conditions (including the provision of discrete policy options) that we discuss and analyze in a subsequent section.

As expected, partisans respond positively to the position of the leader of their own party. The estimated effect of the in-party leader's position on respondents' positions is positive and statistically significant in every case. Notice, however, that most of the estimated coefficients are far below 1, the result we would expect if partisans naively adhere to elite cues. For example, the coefficient of 0.224 in the first minimum wage experiment suggests that for every additional dollar of federal minimum wage advocated by the in-party leader, partisans increase their own preferred minimum wage by approximately 22 cents. Only in the refugee experiment do we observe an in-party effect that is consistent with a naïve adherence to elite cues. In that case, we estimate that for every additional thousand refugees that the in-party leader wants to allow into the United States, partisan respondents increase their own preferred position by 906. In the eight other experiments, however, the in-party effects are notably smaller, and we can statistically reject the possibility that partisans uncritically adopt the position of their in-party leader.

Interestingly, and contrary to much of the existing literature, we also find that partisans respond positively to the positions of the leader of the *opposition* party. The estimated out-party effects are positive in 7 out of 9 cases, they are positive and statistically significant in 6 out of 9 cases, and neither of the 2 negative estimates is statistically significant. In most cases, the estimated out-party effects are substantively meaningful; and in several instances, they are almost as large as the in-party effects.

With only a few exceptions, the results are strikingly similar for respondents of each party. Democrats and Republicans both tend to respond positively to the positions of the leaders from the opposition party, although they

Table 1. Effects of in- and out-party cues.

	MTurk 1		MTurk 2			YouGov			
	Min wage	S. Sudan	Min wage	S. Sudan	COVID	Min wage	S. Sudan	Refugees	Infrastructure
All respondents									
In-party position	0.224	0.233	0.334	0.375	0.652	0.330	0.350	0.906	0.650
	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)	(0.005)	(0.000)	(0.000)	(0.000)
Out-party position	0.153	0.191	0.098	0.094	0.417	−0.072	0.144	−0.322	0.284
	(0.022)	(0.000)	(0.062)	(0.000)	(0.000)	(0.500)	(0.000)	(0.115)	(0.026)
Democrat	1.916	1.421	2.245	1.214	0.374	3.786	2.724	−15.660	0.942
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.459)	(0.001)
Constant	7.003	0.757	6.274	0.731	0.020	7.207	−1.223	6.848	−0.249
	(0.000)	(0.004)	(0.000)	(0.002)	(0.949)	(0.000)	(0.000)	(0.001)	(0.402)
Observations	1,422	1,422	1,845	1,845	749	2,499	2,495	2,493	621
Democratic respondents									
In-party position	0.240	0.322	0.364	0.395	0.677	0.225	0.517	0.923	0.787
	(0.003)	(0.000)	(0.000)	(0.000)	(0.000)	(0.182)	(0.000)	(0.000)	(0.000)
Out-party position	0.195	0.143	0.097	0.074	0.474	−0.085	0.185	−1.224	0.321
	(0.005)	(0.000)	(0.096)	(0.021)	(0.000)	(0.560)	(0.001)	(0.307)	(0.261)
Constant	8.304	1.939	8.147	1.948	0.248	12.472	0.246	56.982	0.348
	(0.000)	(0.000)	(0.000)	(0.000)	(0.469)	(0.000)	(0.501)	(0.011)	(0.425)
Observations	790	790	1,114	1,114	458	1,371	1,370	1,367	341
Republican respondents									
In-party position	0.208	0.124	0.292	0.345	0.623	0.474	0.160	0.112	0.300
	(0.111)	(0.020)	(0.001)	(0.000)	(0.003)	(0.002)	(0.000)	(0.904)	(0.276)
Out-party position	0.095	0.248	0.100	0.125	0.291	−0.060	0.095	−0.276	0.242
	(0.456)	(0.000)	(0.322)	(0.006)	(0.149)	(0.698)	(0.038)	(0.181)	(0.034)
Constant	7.889	1.055	6.661	0.728	0.323	5.735	0.215	68.823	0.197
	(0.000)	(0.012)	(0.000)	(0.045)	(0.567)	(0.022)	(0.551)	(0.002)	(0.583)
Observations	632	632	731	731	291	1,128	1,125	1,126	280

Note: Two-sided *p*-values computed using heteroskedasticity-robust standard errors in parentheses.

understandably respond more to the positions of their own party's leaders. This is all the more remarkable given the timing of our experiments. For example, the MTurk 2 survey took place in the three days leading up to and including Election Day 2020. If we ever would expect to find negative out-party effects, it should be at the peak of a highly contentious presidential election. Nevertheless, even then, Democrats updated their positions positively in response to the positions of Donald Trump, just as Republicans updated positively in response to the positions of Joe Biden.

The extent to which respondents update positively in response to out-party cues does vary somewhat across issues and samples. Foreign aid to South Sudan and COVID-19 relief spending may be less contentious than the other issues we study; and accordingly, the out-party effects are particularly large in those settings. Political debates about refugee admissions, by contrast, are more charged; and there we observe large and statistically significant in-party effects and small, insignificant out-party effects. Minimum wage and infrastructure spending fall somewhere in between, with positive out-party effects that are detectible but notably smaller in magnitude than the estimated in-party effects. We also find somewhat smaller out-party effects in the YouGov surveys than in the MTurk surveys, which may reflect either differences in their sampling procedures or the political conditions under which the surveys were fielded. Across all surveys, however, the out-party effects are generally positive, and in no setting do we observe any clear evidence of negative out-party effects.

The Supplementary Material presents results from a variety of additional analyses. There, we investigate whether the out-party effects are limited to a subset of cases where a politician took a surprising position (e.g., Calvert 1985; Baum and Groeling 2008; Gelpi 2010)—see Supplementary Material table A6—or confirmed a position taken by the in-party (Berinsky 2009)—see Supplementary Material table A8. Similarly, we examine whether the effects of one cue systematically vary according to the contents of the other. When dropping individual treatment conditions and re-estimating our models, the main results carry through—see Supplementary Material table A6. We do not find any clear evidence of nonlinearities in our treatment effects—see Supplementary Material figures A1–A3—nor do we find much evidence of interaction effects between the treatment conditions—see Supplementary Material tables A7–A8. Overall, our main findings appear to be robust across issues and treatments; and for the range of positions studied in these experiments, the effects appear to be approximately linear and additive.

In Supplementary Material table A9, we also test for heterogeneity of these effects across different measures of strength of partisanship or ideological extremism. As expected, in-party effects are greater and out-party effects are weaker for stronger partisans, those with higher levels of affective

polarization, and those with more ideologically extreme views. However, for even the most partisan or ideologically extreme respondents, we still find considerable evidence of positive updating in response to the out-party and hardly any evidence of negative updating.

Investigating the Discrepancy with Previous Results

Why do previous studies report negative out-party effects whereas we consistently find positive ones? Previous studies reporting negative out-party effects, you will recall, either varied the positions of both parties together (making it impossible to separately estimate in- and out-party effects) or provided information about only the out-party's position (raising concerns that these treatments also shifted respondents' beliefs about the in-party's position). In our experiments, by contrast, we inform respondents about the positions of both parties, which are allowed to independently vary. Rather than present and elicit policy positions continuously, as we do in our main experiments, these earlier studies also asked respondents about their support for a single policy proposal.

To investigate the significance of these differences in experimental design, we attempted to replicate and extend the experiments conducted by Nicholson (2012) in our MTurk 1 survey. Nicholson's study presents a bill circulating in Congress that would guarantee up to $300 billion in new loans for at-risk homeowners, and another bill that would offer a path to legal citizenship for undocumented immigrants. Respondents were asked whether they support, oppose, or are not sure about each proposal. Respondents were randomly selected to receive no partisan cue, to learn that John McCain supports the proposal, or to learn that Barack Obama supports the proposal. Because McCain and Obama did not hold elected office at the time our survey was fielded, we replaced them with McConnell and Pelosi, respectively, but we otherwise replicated Nicholson's experiment exactly. We also added additional treatment arms that included information about the positions of both McConnell and Pelosi. Specifically, we independently randomized the positions of each leader so that respondents could learn that a proposal was supported by McConnell but opposed by Pelosi, supported by Pelosi but opposed by McConnell, supported by both, or opposed by both. The precise question wordings are shown in Supplementary Material table A2.

To simplify our analysis and to make it comparable to our previous analyses, we code a trichotomous variable indicating the position of each respondent (support $= 1$, not sure $= 0$, oppose $= -1$), and we code a similar trichotomous variable for the stated positions of the in- and out-party leaders

Table 2. Replication and extension of Nicholson (2012).

	Foreclosure		Immigration	
	Replication	Extension	Replication	Extension
In-party position	0.019	0.094	0.067	0.078
	(0.726)	(0.000)	(0.316)	(0.001)
Out-party position	−0.148	−0.057	−0.123	−0.002
	(0.015)	(0.009)	(0.072)	(0.924)
Democrat	0.303	0.192	0.614	0.589
	(0.000)	(0.000)	(0.000)	(0.000)
Constant	0.442	0.412	0.015	0.003
	(0.000)	(0.000)	(0.809)	(0.946)
Observations	856	1,417	862	1,428

Note: Two-sided *p*-values computed using heteroskedasticity-robust standard errors in parentheses. The positions of the respondents and the party leaders are measured on the same scale (1 = support, 0 = not sure/no information, −1 = oppose). In the replication columns, subjects in the treatment groups were only informed about the position of a leader from one party. In the extension columns, subjects in the treatment groups were informed about the positions of leaders from both parties.

(support = 1, no information = 0, and oppose = −1).[8] As before, we regress respondents' positions on those of the in-party and the out-party, and we control for respondents' party affiliations.

The results are shown in table 2. The "replication" columns include subjects from a control group receiving either no elite cue or information about the position of only one party leader—effectively replicating Nicholson (2012) but pooling respondents to obtain more precise estimates of the average in- and out-party effects. When we do this, we successfully recover the negative out-party effect reported in the literature. The magnitudes of these negative effects are substantively meaningful in both experiments and statistically significant in the case of the foreclosure bill.

In the "extension" columns, we run the same regression for the subset of respondents in the control condition that received no cues and for those who received information about the positions of both party leaders. Consistent with the supposition that respondents update their beliefs about in-party positions in response to information about only out-party positions, we find that the estimated out-party effects attenuate substantially when respondents receive information about both parties simultaneously. Simultaneously providing information about the in-party increases the estimated out-party effect

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8. In Supplementary Material table A10, we show additional results that do not rely upon this trichotomous coding of the treatment variables.

from -0.148 to -0.057 in the foreclosure experiment, and from -0.123 to -0.002 in the immigration experiment. Respondents' inferences about the in-party positions appear to explain most of the observed negative effect of out-party positions.

To isolate the relevance of different measurement strategies for respondents' updating, we conducted additional experiments that simultaneously varied whether policy positions were presented and elicited in a binary or continuous way and whether respondents were informed about just one or both parties' positions. Given their design, these experiments allow us to evaluate the extent to which each of these key differences contributes to the divergent results between our experiments and those of the previous literature.

For these experiments, we asked about a (then) potential relief package to mitigate the economic consequences of the COVID-19 pandemic in the MTurk 2 survey and about a potential infrastructure bill in the YouGov survey. Some respondents received a binary version of the question in which they were asked whether they support, oppose, or have no opinion on a spending proposal. Within this version of the question, respondents were informed about a Democratic and/or Republican leader's support for or opposition to the proposal. As with our previous experiments, the positions of the candidates were randomly and independently varied. Another randomly selected group of respondents received information about party leaders' preferred policies on a continuous scale, and they were asked to also state on a continuous scale their preferred spending level on either COVID-19 relief or infrastructure. Finally, other randomly assigned groups of respondents were informed about the size of the relief package supported by a Democratic and/or Republican leader; and again, the positions of the two leaders were randomly and independently varied. Supplementary Material tables A3 and A4 present the exact wordings for the binary and continuous versions of the questions.

The results are shown in table 3. The top panel presents results from the COVID-19 experiment in MTurk 2, and the bottom panel shows results from the infrastructure experiment in the YouGov survey. The first column only includes respondents who received the binary version of the question and were only informed about the positions of neither or only one party leader. The second column includes respondents who received the binary version of the question and were informed about the positions of neither or both party leaders. Column 3 shows results for respondents who were only informed about the continuous position of their own party's leader. Column 4 shows those who were only informed about the continuous positions of the other party's leader. And Column 5 repeats what we already saw in table 1: results for respondents shown continuous policy positions for both parties' leaders.

Table 3. Further assessing mechanisms.

	(1)	(2)	(3)	(4)	(5)
	Binary			Continuous	
	One	Both	In	Out	Both
	COVID-19 relief spending (MTurk 2)				
In-party position	0.293	0.250	0.750		0.652
	(0.000)	(0.000)	(0.026)		(0.000)
Out-party position	−0.031	−0.015		0.594	0.417
	(0.620)	(0.581)		(0.049)	(0.000)
Democrat	0.171	0.206	0.298	0.357	0.374
	(0.036)	(0.000)	(0.118)	(0.117)	(0.000)
Constant	0.420	0.429	0.527	0.986	0.020
	(0.000)	(0.000)	(0.428)	(0.118)	(0.949)
Observations	304	608	85	98	749
	Infrastructure spending (YouGov)				
In-party position	0.131	0.168	0.642		0.650
	(0.003)	(0.000)	(0.003)		(0.000)
Out-party position	−0.130	−0.120		−0.327	0.284
	(0.002)	(0.000)		(0.431)	(0.026)
Democrat	0.990	0.872	1.045	1.558	0.942
	(0.000)	(0.000)	(0.002)	(0.004)	(0.001)
Constant	−0.285	−0.215	0.159	2.014	−0.249
	(0.000)	(0.000)	(0.498)	(0.034)	(0.402)
Observations	633	621	327	297	621

Note: Two-sided *p*-values computed using heteroskedasticity-robust standard errors in parentheses.

We nearly replicate the Nicholson (2012) result in the infrastructure experiment but not in the COVID-19 relief experiment. When positions are stated and elicited in a binary way, we detect essentially no out-party effect in the COVID-19 relief experiment and negative out-party effects in the infrastructure experiment. By comparing results from columns 1 and 2, we can assess the implications of informing respondents about one rather than two leaders, as we did in table 2. Interestingly, this choice proves relatively inconsequential in these contexts. By comparing results from columns 3–5 with those of columns 1 and 2, we can assess the implications of eliciting policy preferences in a binary or a continuous way—and this turns out to matter greatly. In the COVID-19 experiment, we detect positive, statistically significant out-party effects so long as positions are provided and measured in a continuous rather than a binary way. In the infrastructure experiment, we detect positive, statistically

significant out-party effects only when positions are provided and measured continuously *and* when positions are provided for both party leaders.

Despite some inconsistencies across experiments, the overall results underscore the importance of both methodological issues highlighted in this paper. Out-party effects are greater (i.e., less negative and more positive) when cues are provided for leaders of both parties rather than just one, and when policy debates do not fix on a single option but rather occur along a continuum.

Discussion and Conclusion

Americans are not committed to disagreement. Rather than uncritically follow their own party and dogmatically reject their opponents, Americans routinely update their policy views in accordance with new information about both parties' policy perspectives. These findings arise in both convenience and nationally representative samples, for both strong and weak partisans, in experiments that include and exclude party labels, and for a diverse set of domestic and foreign policy issues. Amidst disagreement and even anger, partisans are willing to reevaluate their views in light of cues, positions, and presumably arguments from the other side.

These results have a variety of implications for our understanding of opinion leadership and the possibilities for democratic deliberation. To begin, the substantive magnitudes of our estimated in-party effects are notably smaller than what we would expect if partisans were following their party leaders indiscriminately. Furthermore, the fact that partisans respond positively to the leaders of the opposition party suggests that opinion leadership does not reduce to group identities, as the literature on affective polarization argues. Consistent with Bayesian reasoning in light of informational deficiencies (Hill and Huber 2019; Graham 2020), citizens revisit both their own views and the status-quo policies, which are included in the treatment conditions, in response to positions taken by presumably better-informed politicians from both parties.

Our results also help explain how mass moderation can persist in the face of widening levels of elite polarization. The effects of elite polarization, of course, are not entirely innocuous. We find that in-party effects tend to exceed out-party effects, which would imply that elite polarization increases public polarization, consistent with the findings of Druckman, Peterson, and Slothuus (2013). But because partisans respond positively to both parties, the translation of elite to mass polarization does not proceed seamlessly. Importantly, exposing partisans to positions from the opposition mitigates rather than exacerbates mass polarization.

Finally, our analyses characterize particular conditions under which elite discourse can moderate public opinion. Experiments that provide information

and solicit preferences about a single policy proposal reveal null or negative out-party effects, while those that provide information and solicit preferences in a continuous way reveal consistently positive out-party effects. Which type of survey question more closely reflects actual political discourse, of course, varies by time and place. In some real-world political situations like public referenda and voting for political candidates, voters choose between a small number of options that were selected for them. But other times, such as the year-long deliberations over the size of President Biden's Build Back Better Act, political and policy debates encourage Americans to think about their positions in continuous terms. Our results suggest that these latter forms of political exchange are particularly likely to reduce partisan cheerleading and instead foster constructive updating across party lines.

On net, our findings suggest that political discourse is not invariably polarizing. When informed about their own party's views and when evaluating policy in continuous terms, partisans openly consider the positions taken by the other side. Even in this moment of political distrust and division, exposure to the views of opposing parties can serve as a force for political moderation.

Supplementary Material

Supplementary Material may be found in the online version of this article: https://doi.org/10.1093/poq/nfac053.

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Data Availability

Replication data and documentation are available at https://doi.org/10.7910/DVN/QRLO9X.

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