Elite Influence on Public Opinion in an Informed Electorate

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Abstract. An enduring concern about democracies is that citizens conform too readily to the policy views of elites in their own parties, even to the point of ignoring other information about the policies in question. This article presents two experiments that suggest an important condition under which the concern may not hold. People rarely possess even a modicum of information about policies, but when they do, their attitudes seem to be affected at least as much by that information as by cues from party elites. The experiments also include measures of the extent to which people think about policy, and contrary to many accounts, they suggest that party cues do not inhibit such thinking. This is not cause for unbridled optimism about citizens' ability to make good decisions, but it is reason to be more sanguine about their ability to use information about policy when they have it.

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The experiments reported in this article can be replicated with materials that are available at http://bullock.research.yale.edu/elite. An appendix is available at the same URL, as are files that can be used to reproduce the figures and tables in the article.

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Most people are unfit for self-governance: scholars since Thucydides have expressed this fear, and social science has done more to confirm it than to allay it. Two findings seem to especially impeach the public's fitness for democracy. The first is that most people are "awash in ignorance" of politics (Kinder 1998, 785-89). Their ignorance of policy is especially acute (Delli Carpini and Keeter 1996, 79-86; Lewis-Beck et al. 2008, 177-81). The second finding is that most people conform readily to the wishes of authority figures even when those wishes are extreme (Milgram 1974; Browning 1992). This latter finding has a cousin in research showing that party identification powerfully shapes people's views and that its effects are strongest among the best-informed (Zaller 1992, Green, Palmquist, and Schickler 2002, ch. 8). Collectively, these findings have helped to give rise to a common claim about the way democracy really works: even when people know about important attributes of policies, they neglect that knowledge and mechanically adopt the positions of party leaders as their own.

No one believes that this claim holds true for everyone. And some disagree that it holds on average in the American electorate (e.g., Key 1966; Nie, Verba, and Petrocik 1976). But the modern student of public opinion cannot escape the claim that cue-based processing of messages about policy "predominates" evaluation of their content (Iyengar and Valentino 2000, 109). Citizens "neglect policy information in reaching evaluations" even when they are exposed to it; instead, they "use the [party] label rather than policy attributes in drawing inferences" (Rahn 1993, 492). And even when "citizens are well informed, they react mechanically to political ideas on the basis of external cues about their partisan implications" and "typically fail to reason for themselves about the persuasive communications they encounter" unless those communications are extremely clear (Zaller 1992, 45). Cohen (2003) summarizes this view in the title of his article on political decision-making: "Party Over Policy: The Dominating Impact of Group Influence on Political Beliefs."

From a normative standpoint, this claim is dour. Facts about policy are the "currency of democratic citizenship" (Delli Carpini and Keeter 1996, 8-11), and traditionally, the greatest concern about elite influence on public opinion has been that it causes people to hold positions that they would not hold if they knew more facts (e.g., Kuklinski and Hurley 1994). But if people ignore facts about policy even when exposed to such facts, there is little reason to expect that facts will help them to make better decisions or protect them from manipulation by elites.

In spite of numerous claims about the relative influence of policy attributes and position-taking by party elites, direct evidence is slight because few studies directly compare the effects of these variables. Those that do make such comparisons use policy descriptions that are short and vague—for example, "decrease services a medium amount." This article presents two studies that permit comparison of party-cue effects to the effects of more substantial policy descriptions. Of course, people often express their views without prior exposure to relevant policy details. But much interest hinges on how party cues and policy details would influence people *if* they were exposed to more than a few of the latter. Examining that counterfactual condition is the point of this article.

The results suggest that position-taking by party elites affects even those who are exposed to a wealth of policy detail. But—contrary to some previous claims—the effects of such position-taking are generally smaller than the effects of policy details. The experiments also include extensive measures of the attention that subjects pay to policy, and they suggest that when people are exposed to both party cues and policy details, the cues do not reduce their attention to the details. If anything, they enhance it. To the extent that party cues have large effects in nonexperimental settings, it may be because citizens often know nothing else about the policies and candidates that they are asked to judge.

I begin by reviewing theory and evidence about the effects of policy attributes and party cues. The next two sections introduce experiments that permit direct comparison of these effects. The following section revisits previous studies in light of the findings from these experiments. Both previous studies and those reported here are rooted in American politics, and the next section considers what we can learn from relevant research in other countries. The final section concludes with suggestions for future research.

Theory and Prior Evidence

A *cue* is a message that people may use to infer other information and, by extension, to make decisions. *Party cues* come in two forms. They may reveal a party affiliation: "Obama is a Democrat." Or they may link a party to a stand on an issue: "The Republicans voted for tax cuts." *Policy attributes* are the provisions and immediate consequences of policies: "this legislation will loosen Medicaid eligibility standards" or "that bill will increase co-payments for Medicaid recipients." People often use party cues to make inferences about policy attributes, but party cues are not themselves policy attributes in the sense intended here.[1]

Many studies have considered the effects of cues and information on voters' views. For example, one line of research asks how general knowledge of politics moderates the connection between values and vote choice (e.g., Zaller 1992; Althaus 2003, pt. 2). Another asks whether cues lead voters astray or help them to act as though they were informed (e.g., Lupia and McCubbins 1998; Lau and Redlawsk 2006; Cutler 2002; Kuklinski and Quirk 2000). But research on the specific effects of *party* cues is relatively rare; I return to this point below. And general political knowledge, while correlated with exposure to descriptions of policy, is a different variable. Most importantly, little of this research speaks directly to the question at hand, which is about the relative effects of party cues and policy descriptions among people who are exposed to both.

That said, there is a prominent generalization about people who are exposed to both types of information: they will be far more affected by party cues. Thus, Rahn (1993, 492) writes that people "use the [party] label rather than policy attributes" even when exposed to such attributes. Cohen (2003, 808) contends that even when one knows about important attributes of a policy, one's attitude toward the policy depends "almost exclusively upon the stated position of one's

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[1] A few political scientists define "party cues" or "partisan cues" more broadly than I do. For example, Squire and Smith (1988) examine an experiment in which California residents were asked whether they would vote to recall certain judges. Some residents were randomly assigned to hear the name of the governor who appointed the judges. The governor's name may be important, but it is not a party cue by the definition given here.

own political party.'' McGuire (1969, 198) writes that a citizen is a "lazy organism'' who relies heavily on source cues but "tries to master the message contents only when it is absolutely necessary.'' And Popkin writes that "the Michigan approach emphasized that no information could be used, *even if obtained,* when voters identified with a party'' (Popkin 1994, 55, emphasis in original). These claims are only weakly qualified: their scope is typically not limited to particular issues or to particular kinds of people.[2]

But there have always been countervailing claims. Key (1966) is adamant that voters are "responsible,'' by which he largely means responsive to policy considerations. Erikson, MacKuen, and Stimson (2002) and Ansolabehere, Rodden, and Snyder (2008) also mount general arguments about voter responsiveness to policy, while Aldrich, Sullivan, and Borgida (1989) make the case for responsiveness to policy in foreign affairs. And Butler and Stokes (1974, esp. ch. 14) make a qualified argument that party ID itself is influenced by policy preferences. These views imply a public whose policy views are more than adjuncts to partisan feeling. They are hard to reconcile with the claim that people's policy attitudes depend "almost exclusively'' on messages from party elites.

Claims about the relative power of party cues and policy messages are often grounded in dual-process theories of attitude change. These theories hold that persuasion can occur through "systematic'' or "heuristic'' information processing (Eagly and Chaiken 1993; see also Petty and Cacioppo 1986). Systematic processing is effortful; it entails checking messages for internal consistency and against one's existing stock of knowledge. Heuristic processing is passive; it occurs through the use of simple decision rules rather than through evaluation of policy content. Dual-process theories imply that heuristic processing is more likely when people lack motivation or ability to scrutinize the messages that they receive. This suggests that party cues will have greater effects on policy attitudes: cues are widely thought to be processed heuristically (e.g., Rahn 1993; Kam 2005), but few people are motivated to scrutinize messages about policies, and

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[2] Popkin's characterization of the Michigan school may be too strong. Compare it to the treatment of voting in Campbell et al. (1960, ch. 8).

fewer still possess the knowledge that is typically required to evaluate arguments about policies (Delli Carpini and Keeter 1996; Converse 2000).

A further claim is that party cues reduce attention to descriptions of policy even among people who have been exposed to such descriptions. This claim is consistent with research on cues as “information shortcuts,” but most of that research focuses on whether cues make people less likely to seek information about policy, not on whether cues make people less likely to use information that they already have (e.g., Downs 1957, chs. 11-12; Popkin 1994, chs. 2-3). Cues might reduce attention to policy—even when people have descriptions of policy in hand—because they permit people to be confident of their views with less effort (Petty and Cacioppo 1986) or because they are clearer guides than policy content to ingroup-consistent views (e.g., Kruglanski and Webster 1996, 264-65; Mackie, Gastardo-Conaco, and Skelly 1992, 145-46, 150). The implications are the same in either case: party cues will lead people to be less affected by policy content, and perhaps to be affected in the wrong ways by superficial understandings of policies.

While dual-process models suggest that cue effects may outweigh policy effects, they also suggest that the weight of these influences on any particular person depends on personal characteristics. Notably, the dual-process emphasis on motivation suggests a moderator: “need for cognition,” the extent to which people enjoy thinking. Because need for cognition is a stable disposition, it is a poor measure of cognitive effort in any particular situation. People low in need for cognition sometimes scrutinize messages, and people high in need for cognition often give them little thought. Still, people do vary in their general tendency to think systematically, and need for cognition captures this variation (Cacioppo et al. 1996). The straightforward prediction is that people who are high in need for cognition should be more affected by descriptions of policy, which require a modicum of thinking to evaluate. A second hypothesis, somewhat less straightforward, is that people who are high in need for cognition should be less affected by party cues. Below, I consider the evidence for these claims.

In spite of dual-process-based reasons to expect that party-cue effects generally outweigh policy effects, the evidence is equivocal. Exposure to party cues is difficult to measure in nonexperimental studies. And comparing the effects of party cues to policy when people are exposed to both requires research designs that expose people to both types of stimuli. Only six published studies (discussed below) fit this description, and they vary on several important dimensions. The most significant variation may lie in their findings: across the six studies, party cues have average effects on attitudes of between 3% and 43% of the range of the attitude scales. Policy-description manipulations have average effects of between 1% and 28%. Variation this great makes generalization difficult.

That said, there are two important respects in which these studies vary little. One is the amount of policy content provided to subjects. Of the six studies in which both policy and party cues are manipulated, five provide no more than three-sentence descriptions of policies, and the fifth offers one to two short paragraphs. The most typical policy descriptions in these studies are brief and vague: for example, "increase the economic status of women" (Riggle et al. 1992, 76) or "decrease services a medium amount" (Tomz and van Houweling 2009, 88). Variation on this dimension is relevant because systematic processing is thought to be more likely when people are exposed to messages that are detailed and unambiguous (Chaiken and Maheswaran 1994; Petty et al. 1993). The relative influence of cues from party elites may therefore depend on variation along this dimension.

A second respect in which prior studies vary little is their reliance on highly indirect measures of depth of processing. For example, subjects in Mackie, Gastardo-Conaco, and Skelly (1992) read a message containing a "strong" argument about an issue. If they later agree with the argument, they are assumed to have processed the message systematically. If they disagree, they are assumed to have processed it heuristically. The possibility that subjects might think intently about the argument and yet disagree with it is ruled out by assumption. Similarly indirect inferences about depth of processing are common in political research (Rahn 1993; Kam 2005).[3] But without more direct measures, it is hard to be confident that cognitive effort is affected by exposure to cues.

Measures of stable traits—for example, political sophistication and need for cognition—are more common (e.g., Mondak 1993; Kam 2005). But because they are stable, they cannot be used to test hypotheses about short-term variation in depth of processing that might be induced by party cues. Moreover, the record of need for cognition—the best-established measure of the tendency to think systematically—is puzzling. In the only previous test of the connection between need for cognition and party-cue influence, Kam (2005) finds no moderating effects. This result is compatible with Bizer et al. (2002) and Holbrook (2006), whose analyses of American National Election Studies data suggest that need for cognition does not moderate the effects of policy information. But it is difficult to reconcile any of these results with psychological studies suggesting that need for cognition moderates the influence of source cues and other kinds of messages (e.g., Cacioppo et al. 1996).

This article presents two experiments that isolate the effects of both policy descriptions and position-taking by party elites. In each experiment, subjects read about a debate modeled on the heated 2005 debate in Missouri over health care for the poor. Each experiment exposes subjects to substantial policy information and contains direct measures of processing depth. Together, the experiments permit direct evaluation of the claim that party cues outweigh the effects of policy information among people who are exposed to sizable amounts of the latter. They also permit evaluation of the extent to which party cues reduce attention to policy information.

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[3] Cohen (2003) is an exception. He uses analysis of subjects' open-ended comments to argue that cues do not decrease and may *increase* depth of processing. See also Rahn (1990).

Experiment 1

Subjects, all partisans, received a detailed newspaper article about health care for the poor in Wisconsin. It contrasted the existing health-care regime with changes that had just been passed by the state House of Representatives. It also offered arguments from supporters and opponents of the changes. The appendix includes a summary of the arguments and the text of each version of the article.

The experiment included a manipulation of policy: subjects were randomly assigned to read that the proposed changes would expand or curtail health-care benefits. It also included a manipulation of party cues: some subjects received no party cues, while others were told that Democratic legislators either supported or opposed the policy changes. In these last two cue conditions, Republican legislators opposed their Democratic counterparts. Table 1 summarizes the experimental design.

Participants, Design, and Procedure

A nonprobability sample of 2,473 subjects who had previously identified as Democrats or Republicans were recruited by Survey Sampling International to participate in a study of reactions to "news media in different states." 50% identified with the Democratic Party and 50% with the Republican Party. The study was fielded from December 16, 2008 through December 26, 2008.

The SSI sample appears to resemble the population of U.S. partisans in most respects, including age, gender, and region of residence. (See Figure A1 of the appendix.) The outlier, as with most Internet samples, is the proportion of people who report having no post-high-school education: 19% of the sample age 25 or older fit this description, against 41% of American partisans age 25 or older. But the appendix shows that subjects' median level of education is identical to the median of all U.S. partisans, and it suggests that low-education subjects are *more* affected by policy descriptions when exposed to them. (See Figure A2, which also suggests that party-cue effects are approximately equal for low- and high-education subjects.) In short, the

	Expand benefits	Reduce benefits
No cues	some legislators support changes; others oppose them	some legislators support changes; others oppose them
“Democrats support” cues	Democratic legislators support changes; Republican legislators oppose them	Democratic legislators support changes; Republican legislators oppose them
“Democrats oppose” cues	Democratic legislators oppose changes; Republican legislators support them	Democratic legislators oppose changes; Republican legislators support them

Table 1: Design of Experiment 1. Experiment 1 had a 3×2 factorial design. Each subject read about legislation that would expand or reduce state-provided health-care benefits. In the “Democrats support” condition, Democratic legislators supported the changes while Republican legislators opposed them. In the “Democrats oppose” condition, Democratic legislators opposed the changes while Republican legislators supported them. In the “no cues” condition, subjects read about support for and opposition to the proposed changes, but the positions were not linked to political parties.

sample's nonrepresentativeness on education is not likely to sharply affect the analyses. And to the extent that it does affect them, it probably causes them to understate the power of policy descriptions.

All subjects were presented with a newspaper article and asked to read it carefully, "as most of the questions that follow will be about your reactions to it."[4] The article was closely modeled on an Associated Press article about Medicaid cuts that were passed by Missouri's legislature (Lieb 2005). It contained between 627 and 647 words, depending on the condition to which the subject was assigned. This length makes it longer than the average article in low-circulation newspapers but shorter than the average article in high-circulation newspapers (Project for Excellence in Journalism 2004).[5] As with the Associated Press article, much of the article that subjects read was devoted to the policy provisions of the bill that the legislature was considering.

Policy treatment. The status quo health-care policy was held constant across all versions of the article. It provided coverage for single parents of two if they earned less than $1,334 per month. Under it, Medicaid costs had tripled in the past twelve years, and they accounted for nearly one third of Wisconsin's budget at the time of the article's publication. The status quo was contrasted with changes that would either restrict or expand health care for the poor. In one condition, changes would reduce coverage for 100,000 of the state's one million Medicaid recipients by tightening eligibility standards. In another, changes would increase coverage for the same number of recipients by loosening eligibility standards. For brevity, these conditions are here labeled the "conservative" and "liberal" policy conditions; they were not so labeled in the articles that subjects read. The article included many more details about the status quo and the proposed alternatives, e.g., details about co-payments and disability coverage. Table A2 of the appendix provides an extensive summary.

[4] The prompt does not seem to have induced high levels of attention: most subjects did not correctly answer three basic factual questions about the policy described in the article. See pages 20 and 28 in this paper and Figures A3 and A8 in the appendix.

[5] The Project for Excellence in Journalism last analyzed the length of newspaper articles in 2004.

Party-cue treatment. In the first paragraph of every article, the proposed changes were said to have passed the House by an 87-71 vote. In one condition, the parties were not identified. In another, 90% of Democratic legislators supported the proposed policy changes, whether liberal or conservative; 90% of Republican legislators opposed the changes. In the final condition, 90% of Democratic legislators opposed the proposed policy changes, whether liberal or conservative; 90% of Republican legislators supported the changes. For brevity, these last two conditions are here labeled "Democrats legislators support" and "Democrats legislators oppose." They were not so labeled in the articles that subjects read, which gave equal attention to the stands of each party.

Post-treatment measures. After reading the article, subjects reported their attitudes toward the policy changes on a seven-category scale ranging from "disapprove strongly" (coded as 1) to "approve strongly" (coded as 7). They also answered three factual questions about the policy; these questions were designed to test whether subjects had paid attention to the article. Finally, they answered six items designed to measure need for cognition, all derived from similar items that had high factor loadings in the battery developed by Cacioppo et al. (1996). These items formed a reliable battery ($\alpha = .81$) and were summed and rescaled to form an index that ranges from 0 to 1. The text of all items is reported in the appendix.

Randomization checks. By chance, a greater proportion of Democrats than Republicans was assigned to the conservative policy condition (54.9% against 48.1%). Because the effects of party cues and policy are analyzed separately for members of each party, this difference does not affect the results reported below. Success of random assignment to the party-cue condition was gauged by regressing it on assignment to policy condition (liberal or conservative), age, education, gender, and region of residence. Similarly, assignment to the policy condition was regressed on party-cue condition, age, education, gender, and region. The chi-squared statistics from these regressions were small, suggesting that the randomizations worked as intended. (Results from each regression are reported in the appendix.)

Results

Figure 1 presents the main results. As expected, Democrats were more supportive of liberal policy changes when Democratic legislators supported them (mean attitude rating = 5.15) and less supportive when Democratic legislators opposed them ($M = 4.64$); the difference is significant at $p = .004$, one-tailed. (Because there are clear expectations about the directions of cue effects, significance tests for such effects are one-tailed unless otherwise noted.) Similar patterns emerge—in the opposite directions, of course—for Republicans reading about liberal changes. They were less supportive when Republican legislators opposed the changes ($M = 3.42$), more supportive when Republican legislators supported the changes ($M = 4.48$). This difference, too, is unlikely to have occurred by chance ($p < .001$).

The patterns held when subjects read about conservative policy changes. Democrats were more supportive of the conservative changes when Democratic legislators supported the changes ($M = 2.45$) than when Democratic legislators opposed the changes ($M = 2.05$). And Republicans were more supportive of the conservative changes when Republican legislators supported those changes ($M = 3.42$), less supportive when Republican legislators opposed the changes ($M = 2.77$). These differences, too, are unlikely to have occurred by chance ($p = .004$ and $p < .001$, respectively). It appears, then, that party cues affect even those who are exposed to ample policy descriptions. But how much?

By conventional standards, not much. The largest effect of party cues is depicted in the upper right-hand corner of Figure 1: Republicans reading about liberal policy changes had a mean attitude of 4.48 when Republican legislators supported those changes, 3.42 when Republican legislators opposed those changes. This is a shift of 18% on the 1-7 attitude scale—sizable but not extraordinary. And as the left-hand panel of Figure 2 shows, the average effects of party cues are smaller. The mean absolute attitude change caused by exposing subjects to “Democratic legislators support” cues rather than “Democratic legislators oppose” cues is .65 points, or 11% of the range of the seven-point attitude scale. This is substantial, but it is swamped by the average absolute effect of exposing subjects to details about a liberal rather than

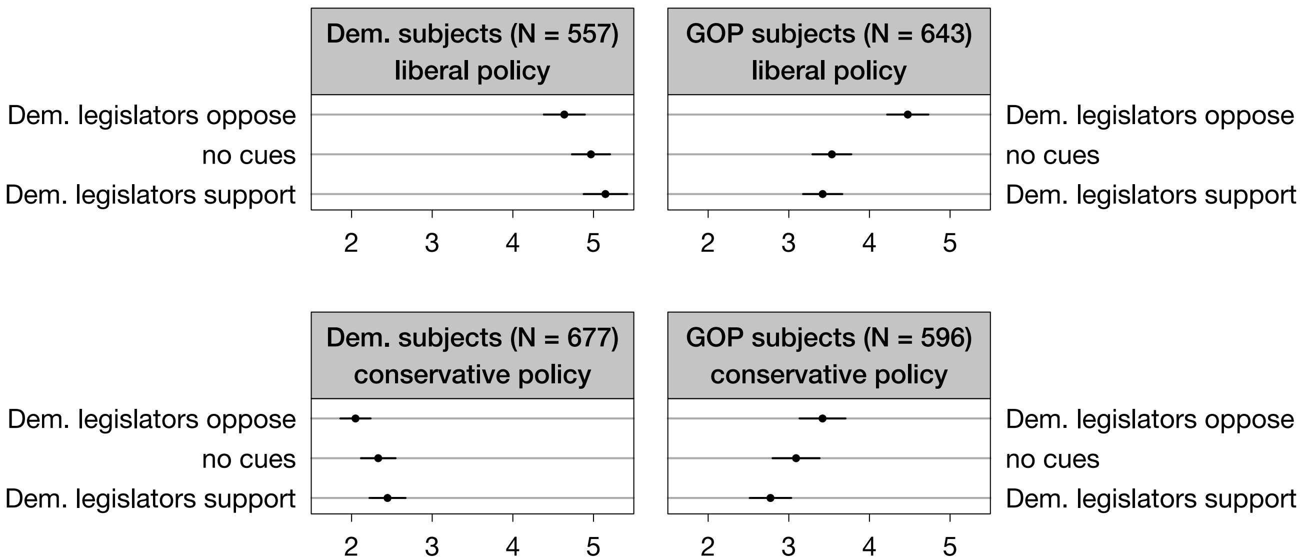

Figure 1: Effects of Cues and Policy Direction. All panels plot mean attitude toward the proposed policy changes. Responses range from 1 ("disapprove strongly") to 7 ("approve strongly"). Black lines are 95% confidence intervals. The results show that both party cues and policy affected attitudes. The effect of policy was greater on average and greater for Democratic than for Republican subjects.

a conservative policy: 1.68 points, covering 28% of the scale. Note that the average difference caused by changes in policy (1.68) exceeds even the greatest difference caused by a change in cues (4.48 – 3.42 = 1.06).[6]

The average effects depicted in the left-hand panel of Figure 2 mask differences between Democratic and Republican subjects. Among Democratic subjects, the average effect of exposure to the "Democratic legislators support" cues instead of the "Democratic legislators oppose" cues was .45 points on the 1-7 scale; among Republican subjects, it was .85 points. The partisan difference in policy effects was starker: 2.64 points for Democratic subjects against .71 for Republican subjects. These differences were unexpected; Experiment 1 was not designed to investigate differences between Republicans and Democrats and cannot shed much more light on them. I revisit this finding in the discussion of Experiment 2.

On average, Republicans disapproved of both the liberal and the conservative policies. But they disapproved less of the liberal policy. The right-hand panels of Figure 1 make this clear: averaging over all cue conditions, the mean Republican attitude toward the liberal policy is 3.79; for the conservative policy, it is 3.09. (For the difference, $p < .001$, two-tailed.) This result does not speak directly to the influence of party cues or policy information, but in light of Republican opposition to the national health-care plan that was enacted in March 2010, it is striking. I return to it in the discussion of Experiment 2.

NEED FOR COGNITION

Although the analyses above distinguish between Democratic and Republican subjects, they still conceal much variation in policy attitudes. For example, the median Democratic rating of the

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[6] Figure 1 also suggests that cue and policy effects depend little on whether the cues that are stereotypical (e.g., Democrats support expansion of benefits) or counterstereotypical (e.g., Democrats oppose expansion of benefits). The sole exception lies with Republican subjects who read about a benefit-expanding health-care policy. For these subjects, the effect of counterstereotypical "Democrats oppose" cues (.94) was greater than the effect of stereotypical "Democrats support" cues (.11). The difference between the effects is .83 (95% CI: [.19, 1.18]).

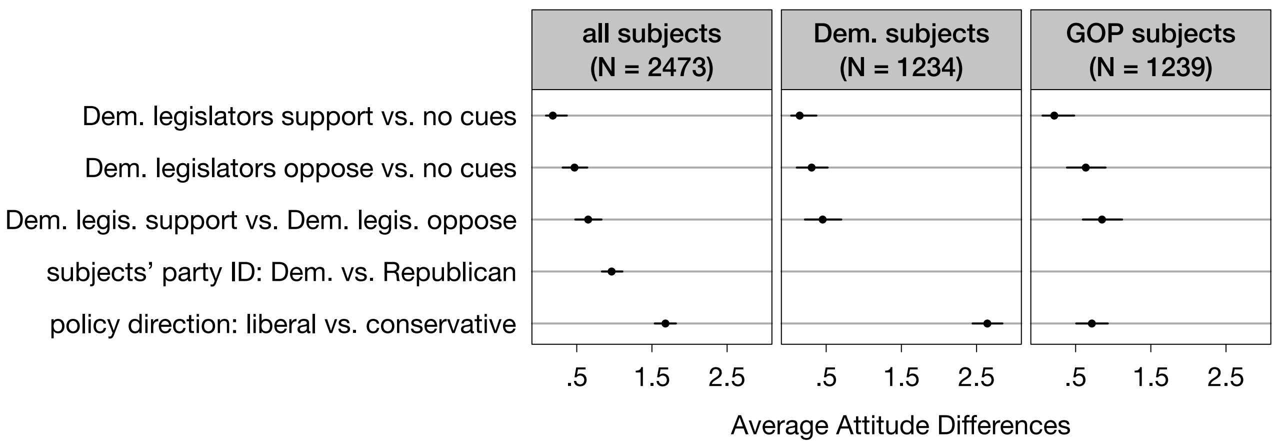

Figure 2: Mean Attitude Differences by Changes in Party Cues, Party ID, and Policy. Each row plots the average of absolute differences between different groups' attitudes toward the proposed policy changes. For example, the middle row of the left-hand panel shows that, on average, exposing subjects to "Democratic legislators support" cues instead of "Democratic legislators oppose" cues changed attitudes by .65 points on the seven-point attitude scale. In each row, black lines are 95% confidence intervals.

The top three rows show that changes in cue condition have slight-to-middling effects on attitudes. The average difference between Republicans and Democrats, displayed in the fourth row of the left-hand panel, is greater. The greatest effect is caused by exposing subjects to liberal rather than conservative policy changes, but this result masks a large difference between Democratic and Republican responsiveness to policy.

"liberal" policy changes was "somewhat approve" when Democratic legislators supported these changes, but fully 21% of Democratic subjects disapproved of the policy. Similarly, the median Republican rating of the policy changes under the same conditions was "slightly disapprove," but 17% of Republican subjects approved "somewhat" or "strongly."

To better understand this diversity of responses, I consider the effects of need for cognition by estimating the model

$$\begin{aligned}\text{policy attitude} = {} & \beta_0 + \beta_1(\text{Democratic legislators support}) + \beta_2(\text{Democratic legislators oppose}) \\ & + \beta_3(\text{liberal policy changes}) + \beta_4(\text{need for cognition}) \\ & + \beta_5(\text{Democratic legislators support} \times \text{need for cognition}) \\ & + \beta_6(\text{Democratic legislators oppose} \times \text{need for cognition}) \\ & + \beta_7(\text{liberal policy changes} \times \text{need for cognition}) + \epsilon. \end{aligned} \tag{1}$$

"Policy attitude" is scored from 1 to 7, where higher values indicate more positive attitudes toward the proposed policy changes. "Democratic legislators support," "Democratic legislators oppose," and "liberal policy changes" are scored 1 for subjects who were assigned to these conditions, 0 for other subjects. Need for cognition is scaled to range from 0 to 1. And $\epsilon \overset{\text{iid}}{\sim} N(0, \sigma^2)$ is a vector of disturbances.

Table 2, which reports OLS estimates of the model, shows that need for cognition moderated party-cue effects only inconsistently (consistent with Kam 2005) but that it heavily moderated policy effects. The estimated coefficient of "liberal policy changes × need for cognition" represents the expected effect of a shift from the low to the high extreme of need for cognition among those who read about liberal policy changes, net of the effect that would have been expected under any circumstances from the increase in need for cognition. This effect is stronger among Republicans than among Democrats, but in both cases it is large

	Democratic subjects		Republican subjects	
Intercept	2.51	.28	1.80	.36
Democratic legislators support	−.74	.38	.14	.45
Democratic legislators oppose	−.30	.38	.74	.46
Liberal policy changes	2.09	.32	2.09	.37
Need for cognition	−.34	.46	1.89	.58
Democratic legislators support × need for cognition	1.43	.61	−.59	.73
Democratic legislators oppose × need for cognition	.00	.62	−.13	.74
Liberal policy changes × need for cognition	.95	.51	−2.27	.60
Standard error of regression	1.63		1.88	
R^2	.41		.09	
Number of observations	1163		1183	

Table 2: Need for Cognition Moderates the Effects of Policy in Experiment 1. Each column reports OLS estimates and standard errors for the coefficients in Equation 1. The dependent variable is attitude toward the proposed policy changes, which is measured on a seven-point scale; higher values indicate a more positive attitude. The party-cues variables ("Democratic legislators support" and "Democratic legislators oppose") and the policy variable ("Liberal policy changes") are scored 0 or 1. Need for cognition ranges from 0 to 1.

The interactions in the last row of estimates suggest that need for cognition strongly moderates the effects of policy. It does not consistently moderate the effects of party cues. These patterns hold under other model specifications: see Table A4 of the appendix.

and of the expected sign: among Democrats, it increases approval of liberal policy changes ($\hat{\beta}_7 = .95$, $p = .03$); among Republicans, it decreases approval ($\hat{\beta}_7 = -2.27$, $p < .001$).[7]

Although need for cognition moderates policy effects for both Democrats and Republicans, it does so in opposite ways. It makes Democrats more responsive to policy: *Ceteris paribus*, the estimated effect of a change from the conservative to the liberal policy is 2.09 points for Democrats lowest in need for cognition, 3.04 points for Democrats highest in need for cognition. This is the result predicted by dual-process theory. But need for cognition makes Republicans *less* responsive to policy: the estimated effect of switching from the conservative to the liberal policy is 2.09 points (again) for Republicans lowest in need for cognition but only −.18 points for those who are highest. Further inspection shows that this result holds across all three cue conditions. (See Table A4 in the appendix.) This result was unexpected. Like the finding that Democrats are more responsive than Republicans to policy in Experiment 1, this is a case in which partisan differences in political cognition merit further study.[8]

Moderation of policy effects by need for cognition is consistent with apolitical findings from social psychology (e.g., Cacioppo et al. 1996), but it is at odds with a raft of studies which suggest that the variable plays no role in thinking about politics. For example, Bizer et al. (2002) argue that need for cognition does not moderate the effect of issue information on candidate preference. Holbrook (2006) argues that it does not affect respondents' ability to explain their support for the candidates whom they prefer. And Napier and Jost (2008) maintain that it does not moderate the effects of liberalism or party ID on happiness (even as they maintain that these effects are substantial). Kam (2005, 175) suggests that need for cognition is too apolitical to play a role in political information processing, a conclusion echoed in part by Holbrook

[7] These results are robust to model specifications that include higher-order interactions among the experimental conditions and need for cognition. Estimates from such models are reported in Table A4 of the appendix. I present a simpler model here for ease of interpretation.

[8] I thank an anonymous reviewer for focusing my attention on this point.

(2006, 349-50). Bizer et al. (2002, 25) infer that "a greater inclination to be thoughtful is not an inspiration for ideal democratic behavior."

The results presented here suggest a different interpretation: need for cognition seems more effective in this experiment because it is measured more reliably here. Bizer et al. (2002), Holbrook (2006), and Napier and Jost (2008) build a need-for-cognition index from only two ANES items, and Cronbach's α for the items is .61 (Bizer et al. 2002, 16). Kam uses the same two items in a study for which α = .48 (Kam 2005, 179). But in Experiment 1, the need-for-cognition battery comprises the two ANES items and four others, and α = .81, suggesting that the larger battery is doing a better job of tapping a single dimension. And when the models from Table 2 are re-estimated with only the standard two-item measure of need for cognition, its estimated effect declines by 20% for Republican subjects, by more than 50%—and into statistical insignificance—for Democratic subjects.

DEPTH OF PROCESSING

The need-for-cognition results described above do not speak to the claim that party cues cause people to think less about policy. Other measures collected in Experiment 1 do speak to the claim. If people pay less attention to policy once they are exposed to cues, they should recall fewer details about policy. And the effects of policy information on attitudes should decline. These patterns do not appear in the data—suggesting that exposure to cues does *not* limit thinking about policy content among people who have been exposed to such content.

Consider first subjects' ability to recall policy details. Subjects were asked whether the policy would expand or reduce Medicaid benefits, to state the maximum amount that single parents of two would be able to earn while remaining eligible for benefits, and to state the number of Medicaid recipients at the time that the bill was being considered. If cues reduce attention to policy, subjects who received cues should answer these questions less well than subjects who did not. But in this experiment, cues had no obvious effect on recall: the average number of facts recalled was 1.63 out of 3 among uncued subjects, 1.61 among cued subjects (p = .37).

If cues reduce attention to policy, we might also expect them to reduce the effects of policy on attitudes. But inspection of Figure 1 shows that cues did not operate in this manner. Among Democrats, the average effect of a switch from reduction to expansion of health-care benefits was 2.64 points on the seven-category attitude scale among those who received cues, 2.64 points again among those who did not. Among Republicans, the average effect of the same switch was .44 points among those who did not receive cues, *rising* to .85 points among those who did receive cues ($p = .07$, two-tailed). Thus, the data again suggest that cues have little effect on processing of policy content. And if they are affecting it, they are at least as likely to be increasing attention to policy as they are to be reducing it.

Experiment 1 thus indicates that subjects exposed to both party cues and policy descriptions were never "predominated" by the cues. Contrary to bold claims about the relative power of party cues and policy considerations (see pages 2 and 4), subjects always responded to the policy content that they received. Indeed, Republican subjects were affected almost equally by policy as by party cues, and Democratic subjects were far more affected by policy. Moreover, Experiment 1 suggests that cues do not reduce attention to policy content when people are exposed to such content. That said, these are findings from only one experiment, and they do not show all that one might like. In particular, the policies described in Experiment 1 were very distinct. Policy considerations may matter less when the contrast between policies is smaller. The depth-of-processing measures were also not as direct as one might wish, leaving open the possibility that better measures would show that cues do reduce attention to policy. Experiment 2 speaks to these concerns.

Experiment 2

Experiment 2 followed the form of Experiment 1, but it included more direct measures of depth of processing, and it varied the extremity of the policies that subjects were asked to consider. The "Democrats support" cue condition was dropped; subjects either received no party cues or "Democrats oppose" party cues. A policy extremity condition was added; subjects were assigned to read about either large or small changes to the health-care status quo. The experiment thus had a $2 \times 2 \times 2$ factorial design: {"Democrats oppose" cues, no cues} × {expand benefits, reduce benefits} × {large changes, small changes}.

Participants, Design, and Procedure

A nonprobability sample of 3,713 subjects who had previously identified as Democrats or Republicans were recruited by Survey Sampling International to participate in a study of reactions to "news media in different states." None of these subjects participated in Experiment 1. To enhance the statistical power of relevant comparisons, more Republicans than Democrats were recruited: 62% of subjects identified with the Republican Party, 38% with the Democratic Party. The study was fielded from May 17, 2010 through May 28, 2010.

The sample resembles the population of U.S. partisans in most observed respects, including gender, region of residence, and need for cognition. The main outliers are education and age. With respect to education, the sample is more representative than the Experiment 1 sample, but the gap is still sizable: 28% of subjects age 25 or older have no more than a high-school education, against 41% of U.S. partisans age 25 or older. The Experiment 2 sample is also older: 48% of subjects are at least 56 years old, against 32% of U.S. partisans. (See Figure A5.) But subjects' median level of education is identical to the median for all U.S. partisans, and Figure A6 suggests that age- and education-based differences between the sample and the population of U.S. partisans are unlikely to sharply affect the analyses.

All subjects received a newspaper article that contrasted a health-care status quo with proposed changes that would expand or reduce benefits. The description of the status quo was unchanged from Experiment 1. Some subjects were assigned to read about large changes to the status quo, and to maximize comparability of results across experiments, these "large-change" policies were the same as the policies described in Experiment 1. But other subjects were assigned to read about small changes to the status quo. For example, the "small-change" policies would directly affect about 10,000 Medicaid recipients rather than the 100,000 affected under the large-change plan, and income cutoffs for Medicaid eligibility would increase 21% under the liberal small-change plan, as opposed to 64% under the liberal large-change plan. The appendix provides an extensive summary of policy differences between conditions.

To conserve statistical power, the “Democrats support” cue condition—the weaker cue condition in Experiment 1—was eliminated. Subjects were assigned to either a no-cue condition or a “Democrats oppose” condition.

Post-treatment measures. Experiment 2 included the post-treatment measures that were used in Experiment 1, and several measures were added to better gauge subjects’ attention to the article that they received. The time that each subject spent on the article was recorded. As in laboratory studies, we cannot know how much time subjects actually spent reading the article; the “time spent” measure reflects the time that subjects’ web browsers spent displaying the article before subjects advanced to the next page of the survey. This measure has been used before to gauge depth of processing, albeit more often in psychology (e.g., Parker and Isbell 2010) than in political science.

After indicating their attitudes toward the policy, subjects were given unlimited time to list their “thoughts about the article and the policy changes that it described.” Two coders, working independently and blind to subjects’ experimental conditions, read the responses to this prompt. Following Cacioppo and Petty (1981), they identified specific thoughts in the responses and coded them as positive, negative, or neutral with respect to the proposed health-care policy. Their ratings were reliable (α = .72, .76, and .91, respectively) and were averaged into a single index for each dimension.

Randomization checks. The success of each random assignment was gauged by regressing it on the other randomizations, age, education, gender, and region of residence. The chi-squared statistics from these regressions were small, suggesting that the randomizations worked as intended. (Results from each regression are reported in the appendix.)

Results

Comparison of the main results to those from Experiment 1 suggests a slight conservative trend. On average, Democrats approved of a large expansion of benefits as much as they had in Experiment 1, but they were also .25 points more approving of a large reduction in benefits ($p = .05$). Republicans were .16 points less approving of a large expansion in benefits, .21 points more approving of a large reduction ($p = .22$ and $p = .15$, respectively).[9] In light of what transpired between the two experiments—the White House changed hands and a massive federal health-care bill was enacted—the absence of stronger attitude change may be more striking than any of the changes that were observed.

Comparing Figure 2 to Figure 3 draws out the consistency of patterns across both experiments. The average absolute effect of switching from a liberal to a conservative policy, taken over both large- and small-change conditions, is 1.24 points, or 21% of the seven-category attitude scale. The average absolute effect of switching from "Democrats oppose" to "Democrats support" party cues cannot be directly calculated because Experiment 2 does not have a "Democrats support" condition. But we can estimate this effect by noting that the average effect of switching from "Democrats support" to "Democrats oppose" in Experiment 1 was 55% greater than the effect of switching from an uncued condition to "Democrats oppose." (See Figure 2.) Multiplying the effect of "Democrats oppose" cues in Experiment 2 by 1.55 yields an estimate of the cue-switching effect: .52 points, or 9% of the attitude scale. This is an important effect, but as in Experiment 1, it is much smaller than the average policy effect.

These overall results again mask large partisan differences. As in Experiment 1, the effect of policy swamps the effects of party cues among Democrats (2.19 points against .31 points). But for Republicans, the effects of policy are clearly weaker than the effects of party cues: .30 points against .73 points, $p = .003$. This finding is not consistent with the claim that party cues

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[9] Because Experiment 2 does not include a "Democrats support" cue condition, these comparisons do not account for the views of Experiment 1 subjects who received "Democrats support" cues.

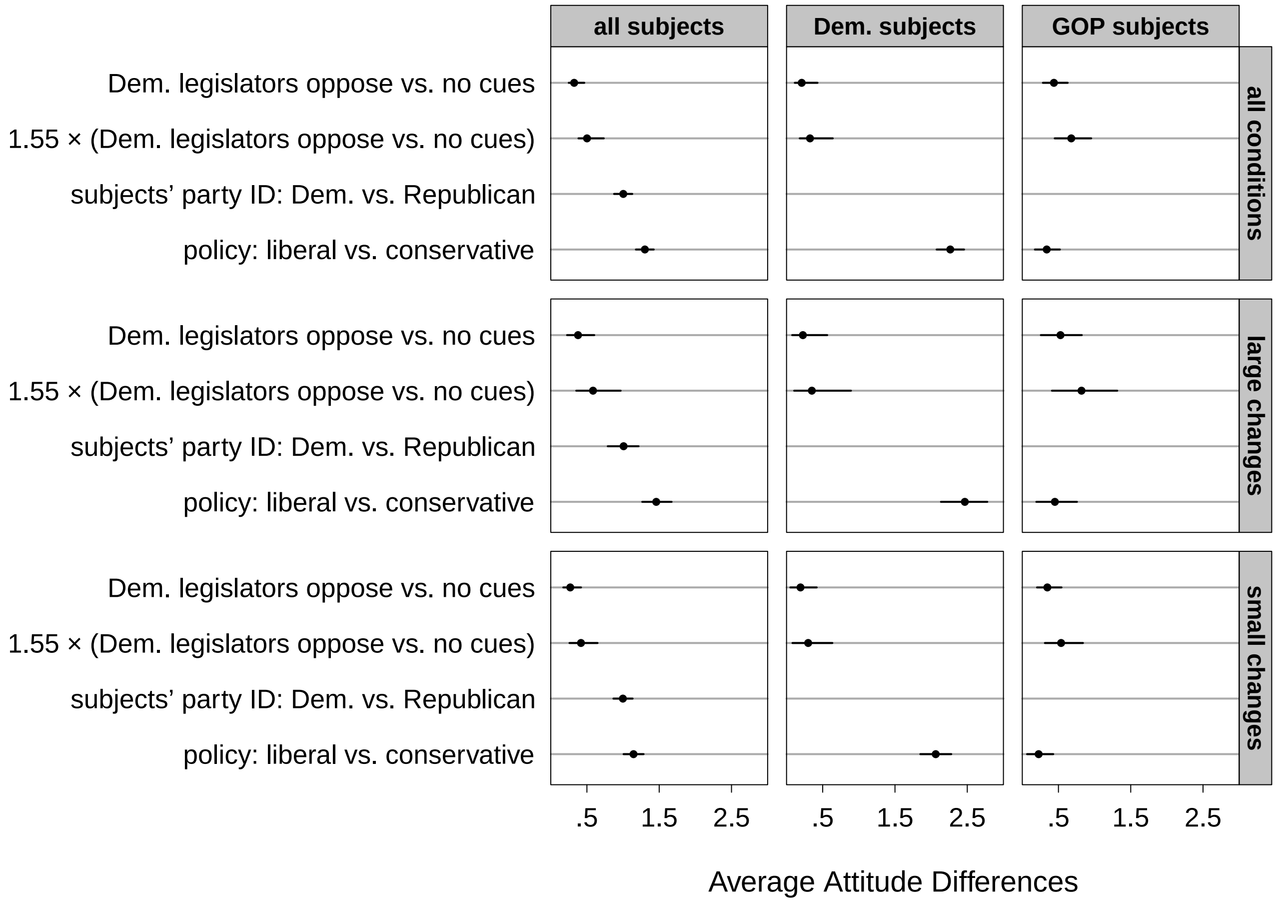

Figure 3: Mean Attitude Differences in Experiment 2 by Changes in Party Cues, Party ID, and Policy. Each row plots the average of absolute differences between different groups' attitudes toward the proposed policy changes. For example, the top row of the upper left-hand panel shows that, on average, exposing subjects to "Democratic legislators oppose" cues instead of no party cues changed attitudes by .35 points on the seven-point attitude scale. Black lines in each row are 95% confidence intervals.

The most important feature of the figure may be the similarity of the panels within each column. This similarity indicates that subjects were little affected by reading about small rather than large policy changes. In particular, the effect of switching from a benefit-expanding to a benefit-reducing policy—given by the bottom row in each panel—did not depend much on whether the expansion or reduction was small or large.

In other respects, the results displayed here mirror the Experiment 1 results displayed in Figure 2. The second row in each panel, "1.55 × (Dem. legislators oppose vs. no cues)," approximates the size of a switch from "Democrats oppose" cues to "Democrats support" cues. Averaging over all subjects, party-cue effects seem much smaller than policy effects. But as in Experiment 1, this result masks a substantial difference between Democratic and Republican responsiveness to policy.

have a “dominating impact” on political beliefs—.73 points is 12% of the range of the attitude scale—but it is the strongest evidence in support of the claim that is to be found in Experiments 1 or 2.

What of the possibility that Experiment 1 produced large policy effects because the policies in that experiment were so different from each other? Experiment 2 strongly suggests that this explanation is incorrect. The evidence appears in Figure 3: for both Democrats and Republicans, attitudes differed little between the large-change conditions and the corresponding small-change conditions. Sharply reducing the distance between policy alternatives did reduce their effect, but not by much. The average policy effect was 1.46 points in the large-change condition, 1.15 points in the small-change condition ($p = .01$).

This is not to say that small changes always matter nearly as much as large changes. Perhaps even smaller differences in policy would have mattered much less than the ones presented in Experiment 2. Or perhaps the distance between policies matters less when they are same side of the status quo: for example, the distance between two benefit-expanding policies might matter less than the same distance between a benefit-expanding and a benefit-reducing policy. Answers to these questions await future research. What is clear is that a sharp reduction in the scope of the policies described in Experiment 1 did little to reduce the effect of policy descriptions.

NEED FOR COGNITION

As in Experiment 1, need for cognition proved a strong moderator of policy effects but a modest moderator of party-cue effects. Table 3 reports OLS estimates of a model very similar to that reported in Table 2. The sole difference is that the “Democratic legislators support” predictor in the previous model is now replaced by a “large policy change” predictor. The results show that need for cognition moderates policy effects among Democrats and Republicans, making Democrats more sensitive to policy considerations but making Republicans less sensitive. It only

	Democratic subjects		Republican subjects	
Intercept	2.76	.25	2.55	.21
Democratic legislators oppose	−.23	.28	.06	.24
Liberal policy changes	1.56	.27	1.12	.24
Large policy changes	.01	.30	.32	.27
Need for cognition	−.13	.41	1.54	.34
Democratic legislators oppose × need for cognition	.05	.46	.58	.41
Liberal policy changes × need for cognition	1.09	.45	−1.72	.40
Large policy changes × need for cognition	−.37	.49	−.59	.44
Standard error of regression	1.66		1.90	
R^2	.31		.03	
Number of observations	1413		2293	

Table 3: Need for Cognition Moderates the Effects of Policy in Experiment 2. This table mirrors Table 2. Each column reports OLS estimates and standard errors. The dependent variable is attitude toward the proposed policy changes, which is measured on a seven-point scale; higher values indicate a more positive attitude. "Democratic legislators oppose," "liberal policy changes," and "large policy changes" are scored 0 or 1. Need for cognition ranges from 0 to 1.

The estimates in the "Liberal policy changes × need for cognition" row suggest that need for cognition strongly moderates the effects of policy. It seems to moderate the effects of party cues and policy size (small policy changes vs. large policy changes) only modestly and inconsistently. These patterns hold under other model specifications: see Table A8 of the appendix.

weakly moderates party-cue effects, and moderation of those effects is not statistically significant for members of either party.[10]

Because need for cognition again moderates policy effects, the results again suggest that political scientists have been too quick to dismiss its political relevance. And quality of measurement again seems the most likely explanation for the discrepancy between previous findings and those reported here. When the models from Table 3 are re-estimated with only the two ANES need-for-cognition items, the estimated moderating effect of need for cognition on policy content is unchanged for Democrats, but it declines 22% for Republicans.

DEPTH OF PROCESSING

Experiment 1 suggested that party cues do not reduce attention to descriptions of policy when people have such descriptions in hand. But one might expect the results to be different in Experiment 2. Perhaps Experiment 1 was simply anomalous. Even if it was not, a highly partisan debate about health care intervened between the two experiments, and it may have sharpened people's associations of the parties with different sorts of policies. Sharper associations might cause people to infer more about policy from party cues and to spend less effort attending to actual descriptions of policy.

We can test the proposition by using the same analyses that were brought to bear in Experiment 1. As Panel 1 of Figure 4 shows, party cues had approximately no impact on subjects' ability to recall policy-related facts. By the same token, cues did not reduce the effects of policy on attitudes. The average policy effect was actually greater when subjects received cues among both Democrats (2.31 points on the 1-7 scale vs. 2.22 points, $p = .65$, two-tailed) and Republicans (.44 vs. .23, $p = .21$, two-tailed). But neither difference approaches substantive or

[10] These results are generally robust to specifications that include higher-order interactions among the experimental conditions and need for cognition. Estimates from such models are reported in Table A8 of the appendix. I present a simpler here for ease of interpretation.

statistical significance. These results suggest, again, that cues neither inhibited nor promoted attention to policy among the experimental subjects.

We need not stop here. Other data were collected in Experiment 2—data that let us look more closely at the extent to which subjects thought about the article that they received. Consider first the time that they spent reading the articles. So long as there is a positive correlation between time spent reading and total cognitive effort, time spent is at least a rough measure of cognitive effort. And here, too, the evidence suggests that cues did not inhibit thinking about policy. Panel 2 of Figure 4 reports 99%-trimmed means of time spent on the article in various conditions. (The means are trimmed because a few subjects spent between one and 83 hours on the article—presumably because they walked away from their computers and returned to the study much later.) It shows that the mean time spent on the article was 205 seconds for uncued subjects, 210 for cued subjects. These means change trivially if we restrict them to Democrats (205 and 211), Republicans (204 and 209), subjects in small-change conditions (205 and 209) or large-change conditions (203 and 212). None of these differences approach statistical significance.

We can go further still. The open-ended thoughts that subjects provided are an indication of depth of processing: subjects who have fewer thoughts in response to the article are less likely to have thought systematically about it. If cues inhibit systematic thinking, we should observe fewer policy-relevant thoughts among cued subjects. We don't. Panel 3 of Figure 4 provides the evidence: on average, we observe slightly *more* policy-relevant thoughts among cued subjects (3.81 vs. 3.61, p = .13, two-tailed). The difference holds when we restrict our analysis to Democrats, Republicans, subjects assigned to the large-change condition, or subjects assigned to the small-change condition. In all of these cases, $p \geq .24$.

Depth of processing should also cause a greater correlation between positivity of thoughts and positivity of overall attitudes (e.g., Chaiken and Maheswaran 1994, 467-68). And if cues inhibit processing of policy content, we should observe a lower thought-attitude correlation among cued subjects. We don't. Following Chaiken and Maheswaran (1994, 464), I subtracted

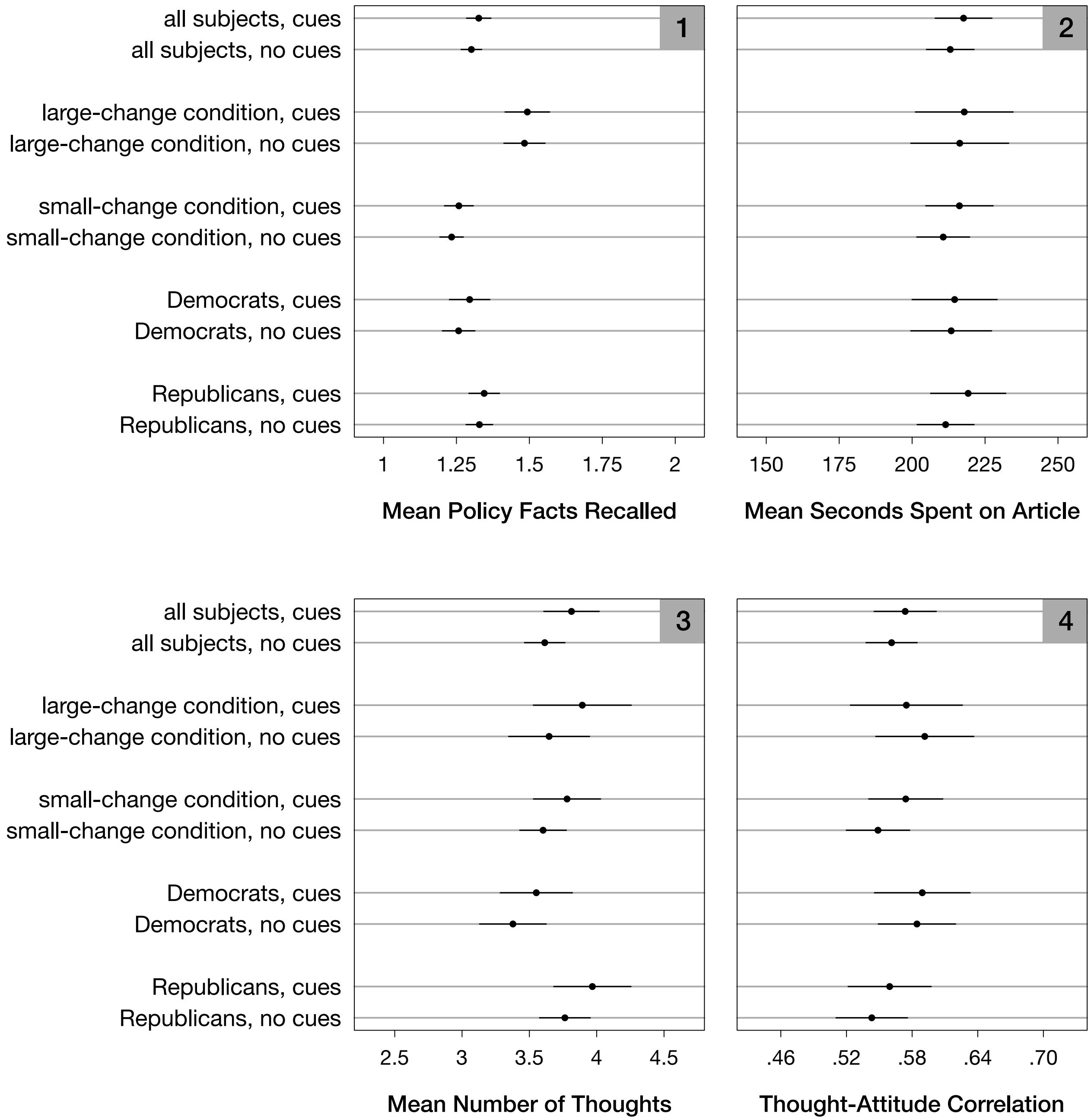

Figure 4: No Effect of Cues on Processing of Policy Content in Experiment 2. Each panel presents a different type of evidence about subjects' processing of policy content. Panel 1 depicts the mean number of policy facts recalled by subjects in various conditions; in all cases, the maximum possible score was 3. Panel 2 depicts the mean number of seconds that subjects spent reading the article that they received. (This panel presents 99%-trimmed means because a few subjects seem to have walked away from their computers for hours at a time. See page 29.) Panel 3 depicts the mean number of thoughts expressed by subjects in open-ended comments that they gave after reading the article. And Panel 4 depicts the correlation between positivity of thought and positivity of attitude in different conditions. Black lines in each panel are 95% confidence intervals.

The evidence is consistent across all four panels: exposure to cues did not inhibit processing of policy content. This result holds in general (see the top two rows of each panel) and for particular subgroups (see the remaining rows in each panel).

each subject's negative policy-relevant thoughts from his positive policy-relevant thoughts to create an index of thought positivity. Among uncued subjects, this index was correlated with attitudes at .54. Among cued subjects, the correlation was .55. As Panel 4 of Figure 4 shows, significantly larger differences did not turn up when the analysis was restricted to subgroups of interest.

The results presented in Figure 4 thus offer no support for the assumption that cues reduce attention to policy content when people have that content in hand (e.g., Kruglanski and Webster 1996, 264-65. This finding does much to explain why policy effects in Experiments 1 and 2 do not decline when people are exposed to cues. It is contrary to most that has been written on the subject, but it is consistent with Mackie, Worth, and Asuncion (1990, 816) and Cohen (2003, 814, 817). Why party cues do not reduce attention to policy content remains uncertain, but two explanations seem likely. One is that party cues have countervailing effects among partisans: they reduce interest in policy (by permitting partisans to hold their views confidently without learning about policy) but also stimulate interest in policy (because the cues clearly indicate party conflict over policy). A second possibility is that cues do reduce attention to policy content when that content is minimal (e.g., Mondak 1993, 171) or difficult to comprehend, which it was not in the studies reported here. I return to this idea in the next section.

REPUBLICAN SUPPORT FOR BENEFIT-EXPANDING POLICIES

As in Experiment 1, Republicans in Experiment 2 disapproved of both the liberal and the conservative policies, but they disapproved less of the liberal policies. Averaging over the cue conditions and the small- and large-change conditions, the mean Republican attitude toward the liberal health-care policy was 3.71; for the conservative policy, it was 3.58. (For the difference, $p = .09$.) Finding the same result in experiments conducted with different samples more than a year apart suggests that it is not a chance occurrence. And in light of Republicans' reputation for opposition to expansion of government-provided benefits—reinforced by their objection to the national health-care proposals that were debated in 2009 and 2010—the result is striking. What can explain it?

Begin by noting that Republican support for cutting benefits is often overstated. Analyses by Ellis and Stimson (2007) suggest that fewer than a fifth of Republicans consistently stake out conservative positions on benefit spending and other social and economic issues.[11] And the 2008 ANES powerfully attests to Republicans' expansionary preferences over "aid to the poor" when it is framed as such. For example, only 17% of ANES Republicans said that federal spending on "aid to the poor" should be cut, while 43% said that it should be increased. 72% favored complete government coverage of prescription drug costs for poor senior citizens. The benefits described in the experiments were explicitly framed as aid to the poor, and the experiments may therefore have evoked Republican aversion to cutting such aid. By contrast, the national debate about health care that occurred in 2009 and 2010 focused less on aiding the poor than on broad expansion of health-care coverage.

Further comparison of the experimental and the national debates is instructive. Opposition to the national plan hinged on the suggestion that it would reduce quality of care for the already-insured, and this suggestion was absent from the experimental articles. Moreover, the experiments described a state-level debate between unknown politicians, while the national debate was conducted by polarizing politicians with national reputations. If the experimental debate had been "nationalized" in these respects, Republican subjects might have approved less of the benefit-expanding policy.

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[11] This datum does not appear in Ellis and Stimson (2007), but we can derive it from their analyses. By Bayes' Theorem, the proportion of "constrained conservatives" in the Republican Party is $P(CC|Rep) = P(Rep|CC)P(CC)/P(Rep)$. According to the 2008 ANES, the proportion of Republicans (including leaners) among adult Americans is $P(Rep) = .38$. According to Ellis and Stimson (2007, 32), the proportion of constrained conservatives who are Republicans is $P(Rep|CC) = .81$. Finally, Ellis and Stimson (2007, 39) suggest that about 28% of self-identified conservatives are constrained conservatives, and self-identified conservatives comprise 32% of the adult U.S. population, implying that $P(CC) = (.32)(.28) = .09$. (By assumption, all constrained conservatives self-identify as conservatives.) It follows that the proportion of Republicans who are constrained conservatives is $(.81)(.09)/.38 = .19$.

A final possibility remains: Republicans may have approved more of the benefit-expanding policies in these experiments than of national health insurance proposals because they were exposed to more information in the experiments than they encountered during the national debate. Even at the height of the national debate, majorities knew little about the legislation being considered. For example, most Republicans believed that some of the provisions of the legislation that they liked most were not in the legislation at all (Kaiser 2010a, 2010b).[12] Still more tellingly, Republicans' support for the legislation more than doubled when they were exposed to information about its key provisions (NBC News 2009).[13] It would go much too far to say that Republican majorities would have liked the legislation if they had learned about all of its major provisions. But the polling data make clear that—as Experiments 1 and 2 suggest—exposure to policy details can substantially affect people's views even when they know where the major parties stand on the policy in question.

In crucial respects, then, Experiment 2 bolsters and extends the findings from Experiment 1. As in Experiment 1, subjects were affected by party cues but more affected—on average—by policy considerations. Partisan differences also reappeared in Experiment 2: policy effects far outweighed party-cue effects among Democrats but not among Republicans. (Indeed, party-cue effects outstripped policy effects among Republicans, albeit to a much lesser extent than policy effects outstripped party-cue effects among Democrats.) Experiment 2 thus shows that the findings of Experiment 1 were not the product of a particular time. It also shows that the strength of policy effects in these experiments cannot be straightforwardly attributed to the degree of difference between the policies under consideration. And it brings a wealth of evidence to bear on the idea that party cues inhibit processing of policy content. The idea does not hold up well.

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[12] Ignorance of health-care legislation was probably even greater than the polls suggested because some unknowledgeable respondents are likely to have guessed the correct answers. See note 3 in the appendix for a way to account for guessing.

[13] The NBC result might have been even stronger if those who were exposed to information about the plan had not expressed their opposition to it only moments before.

Revisiting Previous Findings

In Experiments 1 and 2, elite position-taking effects are rarely larger than policy effects, and they are sometimes much smaller. These findings are consistent with previous studies—not because all of those studies produced similar results, but because the wide variation among them makes them collectively compatible with many different patterns of findings.

Table A1 of the appendix describes the published studies that involve manipulation of party cues and another factor. Six of the studies involve manipulation of party cues and policy: Arceneaux (2008), Berinsky (2009, 118-22), Cohen (2003), Rahn (1993), Riggle et al. (1992), and Tomz and van Houweling (2009). Because these studies involve manipulation of party cues and policy, they are the studies best suited to comparison of the effects of each factor. And their results vary a lot:

- Arceneaux (2008) finds that when a candidate's position on abortion is described, changing his party from Democratic to Republican moves subjects' evaluations of him by 17%, while changing his position moves evaluations by 28%. But when the issue described is environmental regulation instead of abortion, the party-cue effect is 27% and the policy effect is only 4%.[14]

- Berinsky (2009, 118-22) asks subjects whether the United States should intervene militarily in a conflict in South Korea. He varies the positions of party elites, likely casualty rates, and reasons for intervention. Changing the parties from united opposition to united support for intervention has effects that range from 12% to 22%, depending on the other factors. Changing the other two factors has effects that range from 5% to 12%.

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[14] Throughout this section, effect sizes are expressed as percentages of the range of the scale on which preferences or attitudes are measured. For example, a treatment that has an average effect of one point on a 1-5 attitude scale is described as having a $100 \times 1/(5-1) = 25\%$ effect.

- Cohen (2003) finds that changing a welfare policy from “generous’’ to “stringent’’ moves evaluations of it by 15% to 21%, but holding the policy constant and reversing the Democratic and Republican parties’ stands on it moves evaluations by 25% to 43%.

- In Rahn (1993), subjects watch a debate between candidates for a seat in the Minnesota legislature. Mentioning their party affiliations changes attitudes toward the candidates by 7%. Changing the candidates’ positions on five issues moves attitudes by 11% to 14% when subjects do not learn the candidates’ party affiliations, 1% to 6% when they do.

- When subjects in Riggle et al. (1992) read about only one candidate, switching his party from Democratic to Republican moves his approval rating by only 3%, and changing his voting record on six policies moves approval by 23%. But when subjects read about two candidates, the party-cue manipulation has a 10% effect, and the policy manipulation has only a 1%-2% effect.

- Party-cue and policy effects for the sixth study, Tomz and van Houweling (2009), cannot be calculated from the results that the authors report. Their focus is on the effect of ambiguity in candidate position-taking, not on party-cue or policy effects *per se*.

The variation in these findings defeats most attempts to generalize. In particular, the findings do not collectively sustain claims (see pages 2 and 5) about the superior power of party cues among people who are exposed to both party cues and policy content. Of the six studies, only Cohen (2003) consistently finds that party-cue effects outweigh policy effects. This balance of evidence is consistent with other studies that are suggestive although they do not involve policy manipulations. For example, Malhotra and Kuo (2008, 129-31) find that party cues affect the extent to which people blame government officials for mishandling the aftermath of Hurricane Katrina in only five of 14 cases, and they argue that their results point to the “fragility’’ of party cues. Feldman and Conover (1983, 828-31) find “rather minimal’’ effects of party cues on issue attitudes. And Dewan, Humphreys, and Rubenson (2009, 24-25) find that a large effect of voter guides in a Canadian referendum is “entirely due to the arguments used by the campaign, not to the individuals making the case for reform,'' even when those individuals are party leaders. All of these studies run counter to the claim that party cues "predominate'' other message content.

But it remains undeniable that party-cue effects are sometimes enormous. In addition to the effects that Cohen finds, Druckman (2001, 70-72) finds that party cues produce preference reversals of between 40% and 46% in a variation on the Kahneman-Tversky "Asian disease'' experiment. And Meredith and Grissom (2010) and Schaffner, Streb, and Wright (2001) find very large effects of party cues in elections for local and statewide offices. Why are the effects of elite position-taking in these studies so large, and what accounts for the wide variation in findings across studies? Two variables seem especially important: the amount of policy content to which subjects are exposed and the types of issues about which they read.[15]

Message content is thought to be more influential when it is detailed and unambiguous (Chaiken and Maheswaran 1994; Petty et al. 1993; see also Zaller 1992, 47-48). And although shorter policy messages are not always less detailed or less precise than longer messages, they do tend to be. Of the six previous studies in which policy and party cues are manipulated, five provide no more than three-sentence descriptions of policies. The most typical policy content in these studies is a single vague phrase: for example, "increase the economic status of women'' (Riggle et al. 1992, 76) or "decrease services a medium amount'' (Tomz and van Houweling 2009, 88). Cohen (2003) offers more—one to two short paragraphs—but even this is far less than what readers will find every day in articles from the leading American newspapers (Project for Excellence in Journalism 2004). Studies in which subjects receive minimal policy information may mirror the conditions that citizens often face, but they say little about the extent to which citizens would rely on party cues and policy information if they were exposed to substantial amounts of the latter. Experiments 1 and 2 can say more because they expose subjects to more

[15] A third variable, the credibility of the sources giving the party cues, also merits attention. But because source credibility has rarely been integrated into the study of party cues or policy content (see Baum and Groeling 2009 for an exception), it is unlikely to account for variation among the studies discussed in this section.

policy information than any of the six studies described above. Heightened exposure to policy information is also likely to account, in large part, for the balance of party-cue and policy effects in those experiments.

A second set of considerations is about the issues that subjects face. At the heart of these considerations is the idea that party elites will be less influential, and policy attributes more influential, when people have stronger prior beliefs about the issues or are better able to connect their values to positions on those issues. Thus Arceneaux (2008) finds stronger policy effects than party-cue effects when subjects consider abortion, but the opposite pattern when subjects consider whether states or the federal government should regulate the environment. Carmines and Stimson (1989, 11-12) imply that policy effects should be greater when issues are "easy," i.e., "understandable with no supporting context of factual knowledge" (see also Coan et al. 2008). And Levendusky (2010, 120) maintains that it is "all but impossible" to examine cue effects when subjects consider issues about which they have already thought or on which parties have established reputations (see also Gaines, Kuklinski, and Quirk 2007). These arguments suggest that the balance of effects in Experiments 1 and 2 might have been different if subjects had read about a different issue. The policy in these experiments was about health-care benefits that ordinary citizens stand to gain or lose, and such policies are arguably easier for subjects to understand and relate to their values than (say) monetary policy or technical aspects of environmental regulation. That said, this line of reasoning should not be taken too far. Contrary to the claim that it is "all but impossible" to find party-cue effects for familiar issues, Table A1 shows that such effects have been found on multiple occasions when subjects have been asked to consider familiar issues. (See also Slothuus and de Vreese 2010 and Campbell et al. 1960, 135-36.) The choice of issues in experiments is likely to affect the balance of party-cue and policy effects, but it is unlikely—by itself—to account for most of the variation in the effects produced by the studies described here.

Learning from Observational Research

Few of the findings described above are from observational studies. In part, this is because of the difficulties that we face when we try to learn about party cues from such studies—difficulties that do not always arise when we use such studies to learn about other types of variables.

In typical observational studies about party cues, respondents are asked where they stand on issues and where political parties stand on the same issues (e.g., Feldman and Conover 1983). Those who answer the questions about parties' stances are assumed to have received cues conveying those stances. And if their answers to those questions are correlated with their attitudes, cues are assumed to affect their attitudes. One difficulty with this approach is that many people express views on issues that they have never heard about (e.g., Bishop 2005, ch. 2). Merely answering a question about a party's issue position, then, is no indication that one has received a party cue. A second problem is reciprocal causality: people's own issue stances may influence their perceptions of parties' stances, in which case those perceptions are murky amalgams of party-cue and projection effects (e.g., Page and Brody 1972; Jessee and Rivers 2009). A third problem is that receipt of cues may be confounded with other variables that are responsible for the observed effects. For example, knowledgeable people are more likely to receive cues and to take their parties' positions, but it may be their knowledge of policy, rather than their receipt of cues, that causes them to take those positions. Of course, one can attempt to control for policy-relevant knowledge and to model the relations between it, receipt of cues, and policy attitudes. But even if one perfectly measures relevant knowledge and other variables, the structure of the model that relates these variables and party cues to attitudes will remain unknown. Experiments can overcome these threats to inference about party-cue effects.

It remains true that good observational studies have long argued that party-cue effects are large and pervasive (e.g., Campbell et al. 1960, ch. 6). And in spite of the equally longstanding "optimistic" line of observational research (see page 5), the observational record is less mixed than the experimental record: observational research tilts toward finding large party-cue effects. For example, many have argued that mass polarization in support for U.S. wars is due to differences in the positions staked out by party elites (e.g., Brody 1991; Berinsky 2009, ch. 5). Zaller (1992, ch. 6) and Abramowitz (2010) extend the argument to other issues, including welfare policy and the use of busing to promote racial integration of schools. This divergence of experimental and observational results is instructive: it further highlights the role of policy-specific knowledge, and it casts new light on the importance of the frames in which party cues are almost always couched.

Consider first the role of policy-specific knowledge. Americans know little about policy (Delli Carpini and Keeter 1996) and especially little about the policies that are taken up in initiatives, referenda, and the contests for low-level office that dominate most American ballots. In these cases, we should not be surprised to find that party cues have large effects on voters' choices. Indeed, some of the largest party-cue effects have been found in precisely these settings (Schaffner, Streb, and Wright 2001). And the finding of large effects in observational studies (in which most subjects are ignorant of policy) and smaller effects in Experiments 1 and 2 (in which subjects were exposed to ample policy descriptions) imply that large party-cue effects in observational studies reflect the policy ignorance of the American electorate. Observational studies cannot easily tell us how large these effects would be if people knew more about policy—for that, we should turn to experiments—but they may tell us about the effects of party cues given current levels of policy knowledge.

Differences between observational and experimental results also suggest the importance of frames that parties use to support their policy positions. In political debate, cues and frames almost always appear together: party elites rarely take a position without trying to frame it in a way that will garner support for it (Zaller 1992, 13-14, 95-96). Some experiments, including Experiments 1 and 2, stay true to this aspect of politics while still permitting scholars to identify the effects of party cues independent of the frames that party leaders use. But teasing apart party-cue and framing effects is usually beyond the power of observational studies. The large "party-cue'' effects that observational studies suggest may therefore really be "party-cue-and-frame'' effects.[16]

To see why the large party-cue effects suggested by observational studies may be due partly to the frames in which cues are couched, consider the argument made by Lenz (2009). Lenz uses panel survey data to argue that campaigns cause voters to learn where parties stand on issues, which in turn leads voters to change their own positions on issues. In 1980, for example, he argues that U.S. voters learned where Reagan and Carter stood on defense spending, and that they subsequently brought their own positions on the issue into line with the position of their preferred candidate. Lenz's data do suggest that this happened. But his data (like almost all survey data) make it impossible to distinguish the effect of learning where parties stand on issues from the effect of learning the frames and arguments that party leaders use to support their stands. In 1980, Reagan seized on concerns about America's military stature to argue for a defense-spending buildup, while Carter maintained that such views showed that Reagan was dangerously keen to use military force. The opinion change that Lenz observes may therefore be due partly to voters learning frames and arguments like these, not just to voters learning whether the candidates favored or opposed an increase in defense spending.[17]

Frames and arguments are unlikely to fully account for the difference between observational and experimental findings, but they probably account for some of it. To account for some of the difference, the frames that politicians use need not be thoughtful or even coherent. They need only be appealing. And the large investment that politicians make in "honing their

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[16] Some experiments are like observational studies in the sense that they identify not a party-cue effect but a party-cue-and-frame effect. For example, in the experiments of Cohen (2003), a Republican politician endorsing a policy frames his position in one way, but a Democratic politician who takes the same position frames it in a very different way. It is therefore not clear whether the large differences that Cohen finds between these conditions are due to the cues, the frames, or some combination of the two. This may help to explain why Cohen finds consistently large effects where other experimenters do not: his effects may be due partly to the frames in which the cues are couched.

[17] This possibility does not impeach Lenz's larger argument, which is that apparent priming effects in political campaigns are really the effects of "learning and opinion change.''

messages'' or "staying on message''—that is, in getting the frames right—further suggests that some of the effects that observational studies attribute to party position-taking may instead be due to the frames that party leaders use to justify those positions (Druckman, Jacobs, and Ostermeier 2004; Fenno 1978, ch. 5; Kingdon 1981, 47-54; Jacobs and Shapiro 2000; Vavreck 2009).

Elite Influence on Policy Preferences Outside the United States

Most of the studies described above focus on American politics: they consider conflict between the Democratic and Republican Parties over issues that are prominent in America but often minor in other countries. This focus is not accidental. Although research on various aspects of partisanship in other countries is increasing, the most relevant research—about the relative influence of elite position-taking and policy descriptions on people's policy choices—remains overwhelmingly American. Even so, it is now possible to make two generalizations about cross-national variation in the effects of party-elite position-taking on citizens' policy views. First, the effects are stronger where parties have clearer reputations and where competitive party systems are better established. Second, and related, the effects seem to be stronger in the United States than in other countries.

A burgeoning body of research suggests that the strength of party cues in other countries depends on the extent to which those countries' party systems are well-developed. For example, Brader and Tucker (2009a) conduct party-cue experiments in Great Britain, Poland, and Hungary. They find that party cues change policy attitudes most in Great Britain and least in Poland, with Hungary in between—exactly what we would expect if the strength of party cues depends on the extent to which parties have developed clear reputations. Similarly, Merolla, Stephenson, and Zechmeister (2007) find only modest effects of party cues in Mexico, consistent with the recent development of party competition in that country. And both Merolla, Stephenson, and Zechmeister (2008) and Dewan, Humphreys, and Rubenson (2009) find weak-to-nil effects of party cues in Canada. Canada has a long tradition of competitive parties, but for most of their history, the parties have been part of a "brokerage'' system in which policy and ideology are deeply subordinated to the task of building winning coalitions (Stevenson 1987). Tellingly, the largest exception in the Canadian literature is Merolla, Stephenson, and Zechmeister's (2008, esp. 688) finding that cue effects are most substantial for the New Democratic Party (NDP), which is the Canadian party in their study that has the most consistent set of positions on social and economic issues.

A major concern about the study of party-elite influence in other countries is that theory and findings on these topics, which are mainly rooted in American politics, will not apply to countries where multiple parties crowd the political landscape and party systems themselves are much younger (Sniderman 2000, 83-84). Recent research should temper this concern. Despite large differences in party systems, party-cue effects have been found almost everywhere that they have been sought, and they appear to operate in other countries much as they do in the United States. But recent research also shows that the effects are weaker than those that we often observe in the United States. For example, Brader and Tucker (2009a,b) generally find stronger effects for party cues in Great Britain than in Hungary, Poland, or Russia, but even in Great Britain, the effects are smaller than those that would be typical in the United States.[18] Results from Merolla, Stephenson, and Zechmeister (2007, 2008) are similar in this respect, with the exception of their findings for cues from the NDP. These results are striking because the authors take pains to study issues that are not very salient and might therefore be expected to exhibit larger cue effects. Slothuus and de Vreese (2010) find that party cues can move attitudes about a trade agreement by up to 20% in Denmark, and Sniderman and Hagendoorn (2007, 117) report similar effects in the Netherlands when they confine their attention to "high-conformity" subjects, but findings of this magnitude are rare outside the United States.

This finding—party labels are more influential in the United States than in other countries—is consistent with the United States having one of the oldest systems of party competition and only two major parties, both of which have relatively well-defined policy

[18] Over several studies in Great Britain, Brader and Tucker (2009a) never find that party cues shift attitudes on issues as much as 10% of the range of the attitude scale.

reputations (Brader and Tucker 2009a, 33; Lijphart and Aitkin 1994, 160-62).[19] We might therefore expect policy considerations to be relatively more influential outside the United States—not because people in other countries attend more to policy in an absolute sense, but because they are less influenced by party elites.

Conclusion

The normative case for democracy loses much of its force if citizens arrive at their political views unthinkingly (see Estlund 2007, esp. ch. 9). Many scholars fear that citizens are doing just this—mechanically adopting the positions of their party leaders even when they have other information on which to base their judgments (e.g., Zaller 1992, 45; Rahn 1993, 492; Iyengar and Valentino 2000, 109; Graber 1984, 105; Cohen 2003, 808). But examining this concern entails isolating the effects of both policy descriptions and position-taking by party elites. Few studies have done this, and their implications have not been clear. The experiments presented here do isolate the effects of party cues and policy, and they suggest an important condition under which the concern does not hold. Party cues are influential, but partisans in these experiments are generally affected at least as much—and sometimes much more—by exposure to substantial amounts of policy information.

The experiments reported here also reveal much individual-level variation in the relative influence of policy details and position-taking by party elites. The role of partisanship is most striking: in both experiments, Democrats were far more affected by policy than by party cues, but Republicans were almost equally affected by these factors in Experiment 1 and slightly more affected by party cues in Experiment 2. Need for cognition also plays a clear role in moderating policy effects, but it is less important as a moderator of party cues. These differences help to explain why the effects of elite position-taking and policy considerations differ from person to person. That said, they leave intact the experiments' central findings. On average, when people

[19] Brader and Tucker (2009a, 33) add that the average age of major parties in the United States is far greater than the average age of major parties in any other country.

are exposed to both party cues and ample policy descriptions, they are more affected by the latter. And exposure to party cues does not seem to reduce people's attention to policy descriptions when they have those descriptions in hand.

In light of Republican opposition to the Patient Protection and Affordable Care Act of 2010, it is striking that Republicans in these experiments disapproved less of the benefit-expanding policies than the benefit-reducing ones. This finding does not speak directly to the influence of policy details or elite position-taking. But, consistent with contemporaneous poll results, it does further suggest that exposure to descriptions of policy can have important effects.

Nothing about these results was obvious or foreordained. For example, prior research suggests that exposure to source cues may "short-circuit" the processing of policy descriptions, thereby limiting their effect (Kruglanski and Webster 1996, 265, 270-71). It also suggests that need for cognition has little place in the study of politics (e.g., Holbrook 2006, 349-50). Above all, some well-known prior work suggests that even when partisans know about the attributes of policies, their views will be influenced less by that knowledge than by party cues. Those claims are inconsistent with the results reported here, and they are hard to reconcile with the mixed results of the most relevant previous studies.

To a large extent, the discrepancies may be explained by differences in research design. Most claims about a "short-circuiting" effect of cues are based on apolitical studies that do not involve party cues or measures of depth of processing. The accumulating non-findings about need for cognition may well be driven by measurement error. And two variables may account for much of the between-study variation in party-cue and policy effects: the amount of policy content to which people are exposed and the salience of the issues that they consider.

In addition to sharpening our understanding of the determinants of policy attitudes, Experiments 1 and 2 suggest many avenues for future research. Three stand out:

1. *Exploring cognitive differences between Republicans and Democrats.* Political scientists know much about attitude differences between members of different parties, but partisans' thinking about politics may differ in more basic respects,

and this possibility has received little attention. Experiments 1 and 2 produced two unexpected results in this vein: Republicans were less influenced than Democrats by policy considerations, and while need for cognition made Democrats more responsive to policy, it made Republicans less responsive. More research is required to determine whether these results reflect basic differences between members of different parties. And in general, the possibility of basic partisan differences in political cognition deserves much more attention than it has received. (Some authors have already made a start: e.g., Jost et al. 2003; Druckman 2001, 72n11; and Iyengar et al. 2008, 195.)

2. *Examining the roles of issue salience and the amount of policy content to which people are exposed.* When coupled with the other studies discussed in this article, Experiments 1 and 2 suggest that salience and amount of content are among the most important moderators of elite influence on public opinion. But stronger inferences will require experimental manipulation of these variables in studies that can also identify party-cue and policy effects.

3. *Understanding the sense in which party cues are "cognitive shortcuts."* The most important political psychology idea of recent decades may be that cues are "cognitive shortcuts" which help people to conserve effort when making decisions. There are two senses in which cues may be shortcuts: they may reduce information seeking or information processing. Experiments 1 and 2 suggest that party cues are not "shortcuts" in the second sense because they do not reduce processing of policy content when people have that content in hand. Whether they make people less likely to seek information about policy at all is a separate question. Lau and Redlawsk (2006, 239-40) suggest that they do not, but research on this question has only begun. It will be striking if party cues do not prove to be shortcuts in either

> sense of the term—but that is the direction in which the experimental evidence is tending.

One of the most common concerns about elite influence on mass opinion is that it causes people to neglect what they know about relevant policies. But the studies reported here show that the effects of position-taking by party elites can be more modest than we often imagine, and that the effects of policy considerations can be much greater. The ability of political elites to mislead citizens is correspondingly limited, at least when citizens have other information on which to base their judgments. This is not cause for unbridled optimism about citizens' abilities to make good political decisions, but it is reason to be more sanguine about their ability to use information about policy when they have it.

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Online Appendix to
"Elite Influence on Public Opinion in an Informed Electorate"

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June 28, 2013

Post-Publication Changes to This Appendix

June 28, 2013:

- Fixed a bug that was causing small errors in Figure A9. The revised version of the figure is not substantially different. Thanks to Michael Weaver for spotting the problem.

January 28, 2012:

- Revised Figure A11. It was based on the post-treatment party-ID measure. It is now based on the pre-treatment party ID measure, which makes it consistent with all other analyses in the article and the appendix. The differences between this version of Figure A11 and the previous version are trivial, as one would expect given the extremely high correlation between the pre- and post-treatment party ID measures. (See page A47.)
- Rewrote the first sentences of the captions for Figures A10-11 to improve clarity.

Previous Factorial Experiments Involving Party Cues

Table A1 describes the eleven published articles that report factorial experiments in which both party cues and another factor are manipulated. It is sorted first by the "estimable policy effect" column, then by the first author's name. "Estimable policy effect" indicates whether the experimental design permitted estimation of the effect of a change in policy on subjects' attitudes or preferences.

Effect sizes in the "Findings" column are percentage changes in average attitudes and preferences relative to the scale used. For example, if party cues shift attitudes about a candidate by one category on a five-category scale, they are reported to have a $100 \times 1/(5 - 1) = 25\%$ effect. An asterisk (*) in the "Findings" column indicates that average treatment effects could not be computed from the results in the article and were instead computed from the author's dataset.

The first six published studies listed in Table A1 permit estimation of some manner of party-cue effect and some manner of policy effect. These studies are discussed on pages 34-41.

	Party Cue	Policy Content	Estimable Policy Effect	Main Outcome of Interest	Subjects	Findings
This article	Party positions on health-care policies	10 paragraphs in a news article of 16 paragraphs	yes	Attitudes toward health-care policy; depth of processing of policy content	2473 U.S. adults (Experiment 1); 3713 U.S. adults (Experiment 2)	Switching from expansion to reduction of benefits moves subjects' attitudes by 21%-28%. Reversing party cues shifts them by 9%-11%. Cues do not inhibit processing of policy content.
Arceneaux (2008)	Candidate's party affiliation	2-3 sentences about the candidate's position on an issue in a news article of 6-7 sentences	yes	Desire to see candidate win	1126 U.S. adults	Changing the candidate's stand on an issue moves subjects' desire to see him win by 4% or 28%, depending on the issue. Changing his party moves subjects' desire to see him win by 17% or 27%, depending on the issue.*
Berinsky (2009)	Party positions on military intervention in South Korea	1-3 sentences about the reasons for intervening and likely casualty rates.	yes	Support for intervention	4019 U.S. adults	Changing the parties' stands from united opposition to united support for intervention increased subjects' support for intervention by between 12% and 22%, depending on the other information provided. Changing reasons for intervening and likely casualty rates moved support by between 5% and 12%.*

	Party Cue	Policy Content	Estimable Policy Effect	Main Outcome of Interest	Subjects	Findings
Cohen (2003)	Party positions on welfare policies	1-2 paragraphs in a news article of 4-11 paragraphs, depending on the study	yes	Attitudes toward welfare policy	28 to 79 students, depending on the study. All students were either "extremely Democrat and liberal" or "extremely Republican" and "very conservative" (p. 810).	Changing policy from "generous" to "stringent" shifts policy attitudes by 15%-21%. Reversing party cues shifts them by 25%-43% (pp. 811-12).
Rahn (1993)	Candidates' party affiliations	Each candidate used 1-3 sentences to describe his stands on each of six issues	yes	Attitudes toward candidates	162 students and other adults from university area	Revealing the candidates' party affiliations moves attitudes toward the candidates by 7%. Changing the candidates' stands on issues shifts attitudes by 11%-14% when subjects do not receive party cues, 1%-6% when they do.*
Riggle et al. (1992)	Candidates' party affiliations	1 line for each of six policies (e.g., "allow prayer in public schools")	yes	Attitudes toward candidates	Students in an introductory political science course: 200 in one study, 538 in another	When subjects read about just one candidate, switching his affiliation from Democrat to Republican moved approval by 3%. Changing his voting record moved approval by 23% (p. 78). When subjects read about two competing candidates, switching party cues moved approval by 10%, and changing their voting records moved approval by only 1%-2% (p. 81).

	Party Cue	Policy Content	Estimable Policy Effect	Main Outcome of Interest	Subjects	Findings
Tomz and van Houweling (2009)	Candidates' party affiliations	Short phrases describing candidates' stands (e.g., "decrease services a small amount," "decrease services a medium amount")	yes	Preferences over candidates	1001 U.S. adults	Party cues enhance vote share of candidates who take ambiguous positions by 1.3% relative to candidates who take precise positions. When ambiguous candidate is of subject's own party, effect increases to 5.3% (p. 94).
Baum and Groeling (2009)	Party praise or criticism of U.S. President	3 sentences	no	Approval of President's handling of national security	1610 UCLA undergraduates	Switching a party from criticism to praise increased predicted approval by an average of 8%. Subjects were more affected by their own party's praise or criticism than by the other party's praise or criticism (p. 169).
Druckman (2001)	Party positions on combating disease outbreak	1 sentence on lives saved or lost by each policy	no	Policy preference	464 undergraduates	Party cues produce preference reversals between 40% and 46% in the Kahneman/Tversky "Asian disease" scenario. But they reduce preference reversals due to gain-vs.-loss framing by 25% to 37% (pp. 70-72).
Druckman et al. (2010)	Bipartisan endorsement of a candidate (vs. no endorsement)	none	no	Intended vote choice at end of study and two weeks later	416 students and other adults from university area	No immediate cue effects. Two weeks later, effects of 12-16% (p. 141).

	Party Cue	Policy Content	Estimable Policy Effect	Main Outcome of Interest	Subjects	Findings
Malhotra and Kuo (2008)	Politicians' party affiliations	none	no	Blame of politicians for handling of the aftermath of Hurricane Katrina	397 U.S. adults	Party cues affect blame in only five of 14 cases. When people were exposed to office cues, party-cue effects were weaker still (pp. 129-31).
Slothuus and de Vreese (2010)	Party positions on policies	1 paragraph in a 4-paragraph news article	no	Support for privatization of home care for seniors and Danish membership in a WTO agreement	925 Danish adults	Party cues shift support by 13% for senior care, 20% for trade (p. 638).

Table A1: Previous Factorial Experiments Involving Party-Cue Manipulation. This table describes the published studies in which party cues and another factor were manipulated. It is sorted first by the "policy effect estimable" column, then by the first author's name. See page A3 for more information.

Experiment 1: Summary of the Policy Arguments

Arguments for and against the proposed changes were held constant. Opponents of the liberal changes—whether Democrats or Republicans—framed their position as a matter of equity and fiscal responsibility, arguing that the changes would make other welfare services unsustainable and lead to reduced school funding, a budget deficit, and higher taxes. Proponents emphasized the need to protect the disabled, the elderly, and parents who lacked coverage. The governor, a proponent, argued that the bill's anti-fraud provisions ensured that new spending would be directed to the state's neediest residents.

When the changes were conservative, policy arguments were reversed. Opponents argued that the changes would threaten the disabled, the elderly, and parents who would lose coverage. Proponents emphasized that the cuts would allow a balanced budget while increasing school funding and not raising taxes or cutting other welfare services. The governor, a proponent in this condition as well, again touted the bill's anti-fraud provisions.

Experiment 1: Article Text

In Experiment 1, subjects were assigned to read about either liberal or conservative changes to the health-care status quo. They were also assigned to receive no party cues, party cues indicating that Democratic legislators supported the proposed changes while Republican legislators opposed them, or party cues indicating that Democratic legislators opposed the proposed changes while Republican legislators supported them. There were thus six experimental conditions. Each condition was associated with a different version of a newspaper article that was modeled on Lieb (2005).

Liberal policy changes, no party cues. Gov. David Brady won a key budget battle Thursday as the House sent him a bill authorizing the expansion of Medicaid health coverage for tens of thousands of low-income residents. The House's 87-71 vote came on the same day its Budget Committee was finalizing a roughly $19 billion spending plan that would implement the Medicaid expansion beginning July 1.

Brady said the expansion is needed to protect the disabled, elderly, and parents who currently lack coverage.

But opponents contend the expansion could lead to reduced school funding, a budget deficit, and higher taxes. They also argued that the expansion could threaten the long-term sustainability of the state's other social welfare services.

The plan would increase health care coverage for nearly 100,000 of Wisconsin's 1 million Medicaid recipients by loosening eligibility standards, and it would add certain services such as dental care for many others. It also would reduce co-payments or premiums for hundreds of thousands of Medicaid enrollees.

Brady praised the Legislature for taking "decisive actions to protect the poorest among us." He said the bill's anti-waste and fraud provisions—such as annual Medicaid eligibility reviews—would "ensure that scarce state resources are going to those in need."

The bill would expand mandatory Medicaid coverage of such things as wheelchairs, artificial limbs and eye care for most adults. It is expected to reduce waiting times for wheelchairs and prostheses. Adult Medicaid recipients would be permitted to receive eye care visits once every year. Recipients are currently permitted one eye care visit every two years.

A late provision added by the House would also expand a program that provides Medicaid coverage to disabled people aged 16 to 64 if they work at least three hours a month. Currently, disabled adults qualify for coverage if they earn less than $1,940 a month. The House bill raises the cutoff to $2,600 a month.

Opponents of the expansion point to the growth of Medicaid. In the past dozen years, the Medicaid rolls doubled while its cost nearly tripled. Yet even without the proposed expansion, Medicaid would cost more than $5.5 billion in state and federal money next fiscal year, consuming nearly 29 percent of Wisconsin's budget.

The expansion is dangerous because "we must ensure the children of our state can be educated, that our most vulnerable are protected, and (that) we do it in such a manner that creates solid footing for the state of Wisconsin,'' said David Toolan, Chair of Residents for Responsible Government, a nonprofit group that has been lobbying against the increases.

But supporters claim the Medicaid expansions would ensure that the most vulnerable receive necessary protections.

Currently, most adult Medicaid recipients are required to make co-payments of between 50 cents and $3, depending on the cost of the service, each time they visit a doctor or hospital. The House bill would eliminate copayments.

The bill also would eliminate monthly premiums of families in the MC+ for Kids program, which provides health care to children whose families earn up to three times the federal poverty level but aren't covered by traditional Medicaid or private insurance. Because some families will join the program if the premiums are eliminated, the Department of Social Services estimates about 23,700 children will gain coverage.

Under the House version, a single parent of two could earn no more than $2,184 a month to qualify for Medicaid. The current cutoff for single parents of two is $1,334 a month.

Representative Connie Zimmer, I-Mellen, said she gets a $493.50 state mileage check for driving to the Capitol each month.

To qualify for Medicaid under current conditions, "we're telling somebody that they should raise a family of three for less money than any three of us get for gas, and that's hypocritical," she said.

The bill is HB 593.

Liberal policy changes, "Democratic legislators support" party cues. Gov. David Brady won a key budget battle Thursday as the House sent him a bill authorizing the expansion of Medicaid health coverage for tens of thousands of low-income residents. The House's 87-71 vote came on the same day its Budget Committee was finalizing a roughly $19 billion spending plan that would implement the Medicaid expansion beginning July 1. 80 of 89 House Democrats voted for the bill, while 62 of 69 House Republicans voted against it.

Brady, a Democrat, and Democratic legislative leaders said the expansion is needed to protect the disabled, elderly, and parents who currently lack coverage.

But Republican opponents contend the expansion could lead to reduced school funding, a budget deficit, and higher taxes. They also argued that the expansion could threaten the long-term sustainability of the state's other social welfare services.

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Conservative policy changes, no party cues. Gov. David Brady won a key budget battle Thursday as the House sent him a bill authorizing the reduction of Medicaid health coverage for tens of thousands of low-income residents. The House's 87-71 vote came on the same day its Budget Committee was finalizing a roughly $19 billion spending plan that would implement the Medicaid cuts beginning July 1.

Brady said the cuts are needed to balance a budget that increases school funding without seeking higher taxes or cutting other social welfare services.

But opponents contend the health care cuts could threaten the health of the disabled, elderly and parents affected.

The plan would reduce health care coverage for nearly 100,000 of Wisconsin's 1 million Medicaid recipients by tightening eligibility standards, and it would end certain services such as dental care for many others. It also would increase co-payments or premiums for hundreds of thousands of Medicaid enrollees.

Brady praised the Legislature for taking "decisive actions to protect the long-term sustainability of our state's social welfare services." He said the bill's anti-waste and fraud provisions—such as annual Medicaid eligibility reviews—would "ensure that scarce state resources are going to those in need."

The bill would repeal mandatory Medicaid coverage of such things as wheelchairs, artificial limbs and eye care for most adults. But the overall budget plan would continue funding wheelchairs and prostheses, and allow eye care visits for adult Medicaid recipients once every three years. Recipients are currently permitted one eye care visit every two years.

A late provision added by the House would also eliminate a program that provides Medicaid coverage to disabled people aged 16 to 64 if they work at least three hours a month. Currently, disabled adults qualify for coverage if they earn less than $1,940 a month.

Supporters of the cuts point to the growth of Medicaid. In the past dozen years, the Medicaid rolls doubled while its cost nearly tripled. Without the proposed cuts, Medicaid would cost more than $5.5 billion in state and federal money next fiscal year, consuming nearly 29 percent of Wisconsin's budget.

The cuts are necessary because "we must ensure the children of our state can be educated, that our most vulnerable are protected, and (that) we do it in such a manner that creates solid footing for the state of Wisconsin,'' said David Toolan, Chair of Residents for Responsible Government, a nonprofit group that has been lobbying against the bill.

But opponents claim the Medicaid cuts would affect the very vulnerable people supporters say they want to protect.

Currently, most adult Medicaid recipients are required to make co-payments of between 50 cents and $3, depending on the cost of the service, each time they visit a doctor or hospital. Under the House bill, co-payments would cost from $4 to $10 per visit.

The bill also would require monthly premiums of more families in the MC+ for Kids program, which provides health care to children whose families earn up to three times the federal poverty level but aren't covered by traditional Medicaid or private insurance. Because some families would drop out rather than pay the premium, the Department of Social Services estimates about 23,700 children would lose coverage.

Under the House version, a single parent of two could earn no more than $484 a month to qualify for Medicaid. The current cutoff for single parents of two is $1,334 a month.

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Experiment 1: Pre- and Post-Treatment Measures

The pre-treatment party identification question was “Which of the following describes your political affiliation?” The response options were “1) Republican,” “2) Democrat,” “3) Independent,” and “4) I prefer not to answer.” Measurements were taken between September 17, 2004 and December 15, 2008. Measurement for the median subject occurred on October 6, 2008; for 95% of subjects, measurement occurred on or after September 22, 2007.

The post-manipulation measure of party ID in Experiment 1, not used elsewhere in this paper, was a branching item asked at the end of the experiment. The first part asked

> Generally speaking, do you think of yourself as a . . . [Response options: Democrat; Republican; Member of another party; Independent or unaffiliated.]

Subjects choosing “Democrat” were then asked

> Would you call yourself . . . [Response options: A strong Democrat; A Democrat.]

Subjects choosing “Republican” received a similar follow-up question. Subjects choosing “Member of another party” or “Independent or unaffiliated” were instead asked

> Do you think of yourself as closer to the Democratic Party or the Republican Party? [Response options: Democratic Party; Republican Party; Equally close to both.]

The match between the pre-and post-treatment measures of party ID was very close: counting as partisans those “leaners” who did not at first identify with either party, 95% of subjects who had previously identified as Democrats did so again at the end of Experiment 1. The corresponding figure for Republicans was 94%.

The post-manipulation measure of policy attitude was

> Taking everything you have read into consideration, do you approve or disapprove of the changes to Medicaid policy that were described in the news article? [Response options: disapprove strongly (coded 1); disapprove somewhat (2); disapprove slightly (3); neither approve nor disapprove (4); approve slightly (5); approve somewhat (6); approve strongly (7).]

Three items tested knowledge of policy details related in the article:

> Would the proposed changes to Wisconsin's Medicaid system *reduce* or *expand* the number of services available through Medicaid—or would they do neither? [Response options: reduce; expand; neither.]
>
> When the article was written, how many Wisconsin residents were Medicaid recipients? [Response options: about 10,000; about 50,000; about 100,000; about 500,000; about 1 million.]
>
> If the changes go into effect, what is the most that a single parent of two could earn while still being eligible for Medicaid? [Response options: about $500 a month; about $1,000 a month; about $1,500 a month; about $2,000 a month; about $2,500 a month.]

The correct answers to these questions depended on the conditions to which subjects were assigned.

Six need-for-cognition items were adapted from the battery developed by Cacioppo and Petty (1982):

> Some people prefer to solve simple problems instead of complex ones. Other people prefer to solve complex problems instead of simple ones. What is your preference? [Response options: greatly prefer simple problems (coded 1); somewhat prefer simple problems (2); slightly prefer simple problems (3); no preference (4); slightly prefer complex problems (5); somewhat prefer complex problems (6); greatly prefer complex problems (7).]
>
> How much pleasure do you get from thinking? [Response options: none (coded 1); a little (2); a moderate amount (3); a lot (4); a great deal (5).]
>
> Some people prefer to think about small, daily projects. Other people prefer to think about big, long-term projects. What is your preference? [Response options: greatly prefer small, daily projects (coded 1); somewhat prefer small, daily projects (2); slightly prefer small, daily projects (3); no preference (4); slightly prefer big, long-term projects (5); somewhat prefer big, long-term projects (6); greatly prefer big, long-term projects (7).]
>
> How much do you like or dislike thinking long and hard for hours? [Response options: dislike a lot (coded 1); dislike somewhat (2); dislike a little (3); neither like nor dislike (4); like a little (5); like somewhat (6); like a lot (7).]
>
> How much do you like or dislike having responsibility for handling situations that require lots of thinking? [Response options: dislike a lot (coded 1); dislike somewhat (2); dislike a little (3); neither like nor dislike (4); like a little (5); like somewhat (6); like a lot (7).]

> After finishing a task that required a lot of mental effort, do you feel more *relieved* than *satisfied*, or more satisfied than relieved? [Response options: much more relieved than satisfied (coded 1), somewhat more relieved than satisfied (2), slightly more relieved than satisfied (3), relief and satisfaction to the same degree (4), slightly more satisfied than relieved (5), somewhat more satisfied than relieved (6), much more satisfied than relieved (7).]

The items formed a reliable battery (α = .81). The second item was rescaled to share the range of the other items; the items were then summed to create a single index of need for cognition. The index was then rescaled to range from 0 to 1.

Experiment 1: Summary of Differences Between the Policy Conditions

Liberal Policy Changes	Status Quo	Conservative Policy Changes
Monthly income cutoff: $2,184 for a single parent of two	Monthly income cutoff: $1,334 for a single parent of two	Monthly income cutoff: $484 for a single parent of two
No copayments for visits to doctor	Copayments for visits to doctor: 50 cents to $3	Copayments for visits to doctor: $4-$10
Coverage for children: eliminate premiums for some families, leading 23,700 children to gain coverage		Coverage for children: require premiums for some families, leading 23,700 children to lose coverage
Expand mandatory coverage of wheelchairs, prostheses, and eye care. Reduce waiting times for wheelchairs and prostheses. Eye-care visits once every year.	Coverage of wheelchairs and prostheses. Eye-care visits once every two years.	Repeal mandatory coverage of wheelchairs, prostheses, and eye care. Budget would continue to fund wheelchairs, prostheses, and eye-care visits once every three years.
Coverage of temporarily disabled people aged 16 to 64 who earn less than $2,600 per month and work at least three hours per month.	Coverage of temporarily disabled people aged 16 to 64 who earn less than $1,940 per month and work at least three hours per month.	Eliminate coverage for the temporarily disabled.
Expand coverage for 100,000 of the state's one million Medicaid recipients.		Reduce coverage for 100,000 of the state's one million Medicaid recipients.

Table A2: Policy Details in Experiment 1. All subjects in Experiment 1 read a newspaper article that contrasted the status quo with liberal or conservative policy changes that had just been passed by the state House of Representatives.

Experiment 1: Randomization Checks

	All Subjects					
	DS		DO		LIB	
Intercept	−.41	.26	.02	.25	−.37	.21
Female	−.07	.10	−.06	.10	.03	.08
Age	.01	.00	.00	.00	−.00	.00
Low education	−.11	.19	−.16	.18	.28	.15
Medium education	.15	.16	−.07	.16	.21	.13
Northeast	.02	.16	−.21	.16	.08	.13
South	−.10	.13	−.04	.13	.19	.10
West	−.04	.14	−.10	.14	.18	.12
Liberal policy	−.02	.10	−.03	.10		
"Democrats support" cues					−.02	.10
"Democrats oppose" cues					−.03	.10
Log likelihood	−1127		−1134		−1688	
Likelihood ratio test	10.3 (p = .33)		4.3 (p = .89)		7.9 (p = .64)	
Cragg and Uhler (1970) R^2	.008		.004		.004	
Number of observations	1634		1639		2442	

	Democratic Subjects					
	DS		DO		LIB	
Intercept	−.70	.37	.11	.34	−.60	.30
Female	.06	.15	−.06	.14	−.08	.12
Age	.01	.00	.00	.00	.00	.00
Low education	−.09	.27	−.33	.25	.36	.21
Medium education	.25	.23	−.10	.21	.08	.18
Northeast	−.05	.21	−.32	.21	.26	.18
South	−.09	.19	−.03	.18	.42	.15
West	−.06	.20	−.19	.20	.42	.17
Liberal policy	.07	.14	.05	.14		
"Democrats support" cues					.07	.14
"Democrats oppose" cues					.05	.14
Log likelihood	−557		−578		−835	
Likelihood ratio test	7.4 (p = .59)		5.7 (p = .77)		13.1 (p = .22)	
Cragg and Uhler (1970) R^2	.012		.009		.014	
Number of observations	810		838		1222	

	Republican Subjects					
	DS		DO		LIB	
Intercept	−.06	.36	−.06	.37	−.10	.30
Female	−.16	.14	−.06	.14	.19	.12
Age	.01	.00	.00	.00	−.00	.00
Low education	−.11	.27	.04	.27	.20	.22
Medium education	.03	.23	−.00	.23	.32	.19
Northeast	.12	.24	−.09	.25	−.01	.20
South	−.14	.18	−.06	.18	−.02	.15
West	−.03	.20	−.02	.21	−.06	.17
Liberal policy	−.11	.14	−.10	.14		
"Democrats support" cues					−.10	.14
"Democrats oppose" cues					−.11	.14
Log likelihood	−568		−554		−841	
Likelihood ratio test	5.8 (p = .76)		1.6 (p = .99)		7.6 (p = .67)	
Cragg and Uhler (1970) R^2	.009		.003		.008	
Number of observations	824		801		1220	

Table A3: Randomization Checks for Experiment 1. Each column reports estimates and standard errors from a logistic regression of a randomized variable on other variables. "DS," "DO," and "LIB," are the randomized variables: "Democrats support" party cues, "Democrats oppose" party cues, and the liberal policy condition. "Northeast," "South," and "West" refer to subjects' region of residence as defined by the U.S. Census Bureau; the index category is "North Central." "Low education" indicates no formal education beyond high school. "Medium education" indicates formal education beyond high school but not beyond college. The index category, "high education," includes subjects who had some post-college education. Entries in the "likelihood ratio test" row are χ^2 statistics from a test against an intercept-only model.

As expected, the χ^2 statistics are insignificant and the pseudo-R^2 values are low, suggesting that the randomizations in Experiment 1 were not systematically associated with the predictors. Three of the 81 estimates are significant at $p < .05$, two-tailed: the estimates for "South," "West," and the intercept in the sixth column. This percentage of significant estimates (3/81 = 3.7%) is close to the 5% of estimates that we would expect to be significant by chance if all of the coefficients were null.

Experiment 1: Sample Characteristics

Figure A1 contrasts characteristics of the Experiment 1 sample with those of partisans in the 2008 American National Election Study. It shows that the sample percentages of subjects who are women (55%), 30 or younger (16%), 56 or older (34%), hold an advanced degree (13%), or hail from the Midwest (27%), the Northeast (16%), the South (36%), or the West (22%) are all within 6% of the corresponding ANES percentages. This is consistent with the small discrepancies that Sanders et al. (2007) and Stephenson and Crête (2011) find between Internet surveys and surveys conducted through other modes (though see Malhotra and Krosnick 2007, who find somewhat larger differences).

The outlier, as in many Internet samples, is the proportion of people who report having no post-high-school education: 19% of the subjects age 25 or older fit this description, against 41% of ANES partisans age 25 or older. *A priori,* we might expect more educated subjects to better comprehend policy descriptions and thus be more influenced by them, thereby causing the analyses of Experiment 1 to overstate the influence of such descriptions. But the percentage of the sample holding advanced degrees (13%) is very close to the corresponding percentage for all U.S. partisans (10%), suggesting that the average subject in Experiment 1, while more educated than the average U.S. partisan, is not much more educated. The median education level in the sample is the same as the median for all American partisans: more than 12 years of schooling but no college degree. The sample is very close to the ANES in need for cognition (variables V085170x and V085171); to the extent that education proxies for cognitive effort, this suggests that the results are not affected by under-representation of people who have no post-high-school education. The percentage of subjects who knew Dick Cheney's job title is also close to the corresponding percentage in the 2004 ANES (V045163).[1] The item about Cheney is the only knowledge item common to the two studies, but to the extent that it indicates general political

[1] Data for the 2008 ANES item about Cheney have not yet been released. This is why I refer to the 2004 ANES responses to the Cheney item.

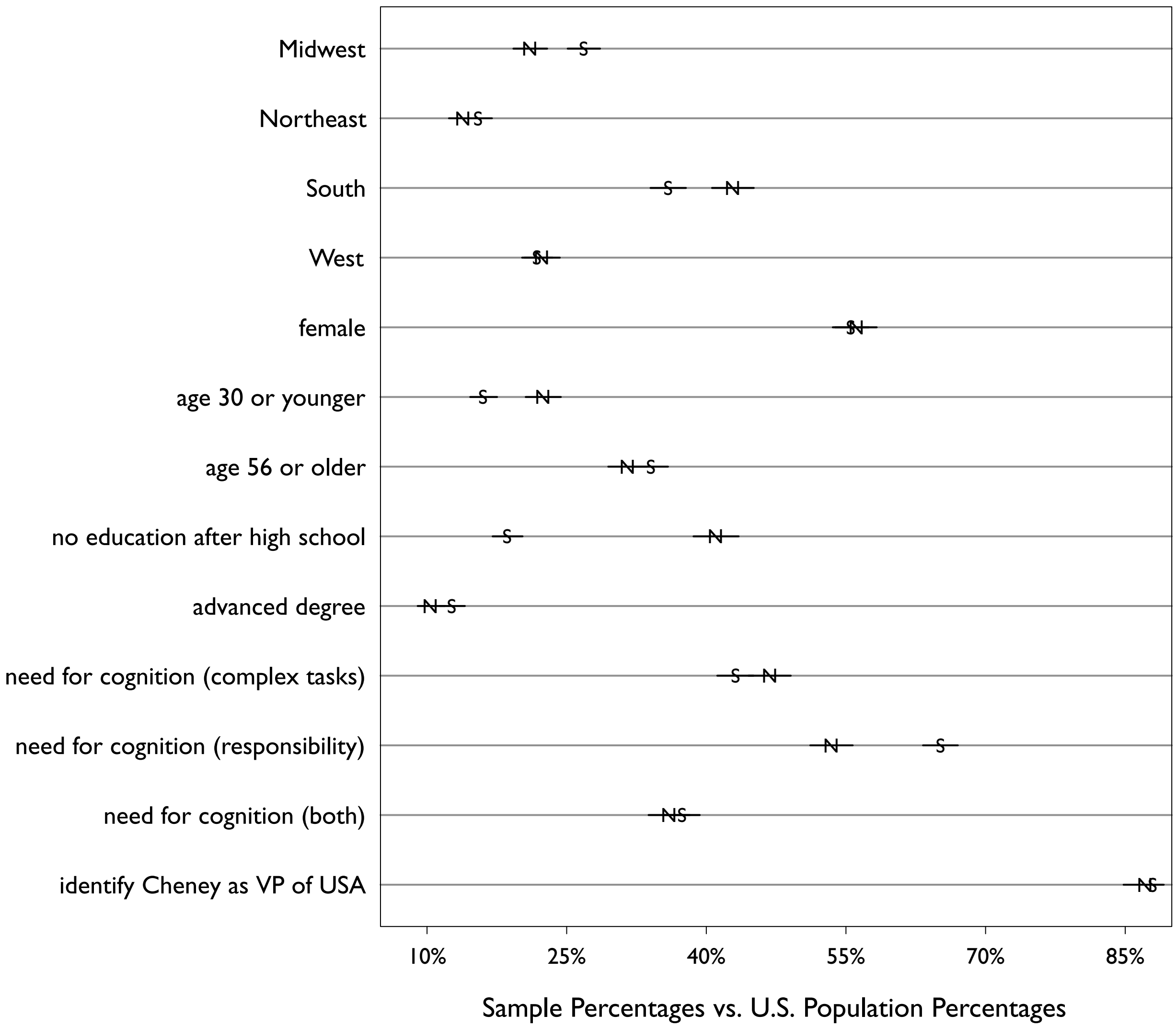

Figure A1: Experiment 1 Sample Representativeness. Each row plots percentages of Experiment 1 subjects ("S") who share a characteristic. The corresponding percentages for partisans in the U.S. population ("N") are drawn from the 2004 and 2008 ANES. Black lines are 95% confidence intervals.

"Midwest," "Northeast," "South," and "West" indicate percentages of subjects residing in each region. "Need for cognition (complex tasks)" plots percentages of subjects indicating that they "prefer complex to simple tasks." "Need for cognition (responsibility)" plots percentages indicating that they like having responsibility for situations that "require lots of thinking." "Need for cognition (both)" plots percentages indicating that they prefer complex tasks and like having responsibility for situations that require lots of thinking. "Identify Cheney as VP of USA" indicates the proportion of subjects who identified Dick Cheney as Vice President of the United States in response to an open-ended question.

knowledge, it also suggests that the results are unlikely to be affected by the under-representation of people who have no post-high-school education.

But the most direct evidence about the consequence of the under-representation of people with no post-high-school education is given in Figure A2, which shows how the average effects of party cues and policy direction differ between subjects who have post-high-school education and subjects who do not. The general pattern of results is the same for both groups: both are substantially more affected by changes in policy than by changes in party cues. But the average effect of policy is greater for subjects who have no post-high-school education: 2.27 points on the seven-point attitude scale, against 1.54 points for subjects who do have some post-high-school education. In short, the sample's nonrepresentativeness on education seems unlikely to sharply affect the analyses. To the extent that it does, Figure A2 suggests that it causes the analyses to be conservative, i.e., to understate the influence of policy considerations.

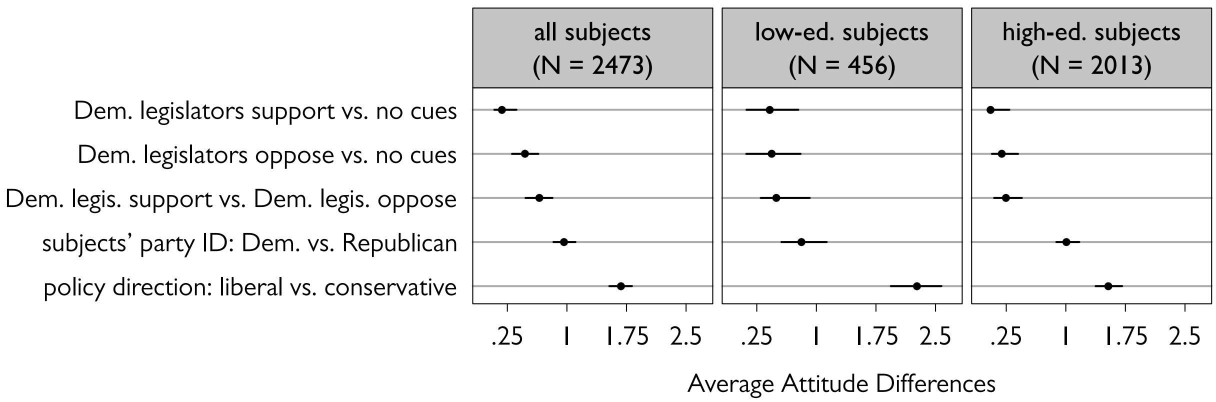

Figure A2: Average Attitude Difference by Changes in Party Cues, Party ID, and Policy Direction (Low- vs. High-Education Subjects). This figure is analogous to Figure 2. The difference is that this figure shows how average effects in Experiment 1 differ across low- and high-education subjects, while Figure 2 does not. "Low-ed." subjects are those who have no formal education beyond high school; "high-ed." subjects do have some formal education beyond high school.

Each row plots the average of absolute differences between different groups' attitudes toward the proposed policy changes. For example, the middle row of the left-hand panel shows that, on average, exposing subjects to "Democratic legislators support" cues instead of "Democratic legislators oppose" cues changed attitudes by .65 points on the seven-point attitude scale. In each row, black lines are 95% confidence intervals.

The top three rows show that changes in cue condition have slight-to-middling effects on attitudes. The average difference between Republicans and Democrats, displayed in the fourth row, is greater. The greatest average effect is caused by exposing subjects to liberal rather than conservative policy changes. This effect is greater for low- than for high-education subjects (2.27 vs. 1.54, $p < .05$), suggesting that policy effects in Experiment 1 would have been still larger if the distribution of education among Experiment 1 subjects more closely matched the distribution of education in the U.S. population.

Experiment 1: Formal Definition of Causal Estimands

Let Y_i be subject i's rating of the policy changes on the seven-point scale. Let $S_i = 1$ if i is exposed to "Democrats support" party cues; otherwise, $S_i = 0$. Let $O_i = 1$ if i is exposed to "Democrats oppose" party cues; otherwise, $O_i = 0$. Note that there is no subject for whom $S_i = O_i = 1$. Thus, following Neyman ([1923] 1990) and Rubin (1974), the effect on i of exposure to "Democrats support" cues is

$$\tau_{S_i} = Y_i(S_i = 1, O_i = 0) - Y_i(S_i = 0, O_i = 0),$$

where $Y_i(S_i = 1, O_i = 0)$ is the rating that the subject would give if assigned to receive "Democrats support" cues and $Y_i(S_i = 0, O_i = 0)$ is the rating that the subject would give if assigned to the no-cue condition.[2] We cannot observe both of these "potential outcomes," because i cannot be assigned to both the "Democrats support" and the no-cue conditions. But given randomization, we can estimate the expected values of these outcomes:

$$E[Y_i(S_i = 1, O_i = 0)] = \frac{\sum_i (Y_i \times S_i)}{\sum_i S_i}, \text{ and}$$

$$E[Y_i(S_i = 0, O_i = 0)] = \frac{\sum_i [Y_i \times (1 - \max(S_i, O_i))]}{\sum_i (1 - \max(S_i, O_i))}.$$

This permits us to estimate the average effect of exposure to "Democrats support" cues:

$$\tau_S = E[Y_i(S_i = 1, O_i = 0)] - E[Y_i(S_i = 0, O_i = 0)].$$

Similarly, the average effect of exposure to "Democrats oppose" cues is

$$\tau_O = E[Y_i(S_i = 0, O_i = 1)] - E[Y_i(S_i = 0, O_i = 0)].$$

---

[2] This definition implies that the "stable unit treatment value assumption" was met, i.e., that the potential outcomes for each person i were unrelated to the potential outcomes for other subjects. Given that the subjects never met and that each subject's treatment status was unknown to the others, this is a very plausible assumption.

Most importantly, the *average effect of switching from "Democrats oppose" cues to "Democrats support" cues* is

$$\tau_S - \tau_O = E[Y_i(S_i = 1, O_i = 0)] - E[Y_i(S_i = 0, O_i = 1)].$$

This is just a difference of average treatment effects.

In the same way, let $L_i = 1$ if subject i is assigned to read about liberal policy changes and $L_i = 0$ if he is not. Let $C_i = 1$ if he is assigned to read about conservative policy changes and $C_i = 0$ if he is not. The average effects for the policy manipulation are then

$$\tau_L = E[Y_i(L_i = 1, C_i = 0)] - E[Y_i(L_i = 0, C_i = 0)],$$

the effect of exposure to descriptions of liberal policy changes, and

$$\tau_C = E[Y_i(L_i = 0, C_i = 1)] - E[Y_i(L_i = 0, C_i = 0)],$$

the effect of exposure to descriptions of conservative policy changes. These effects cannot be estimated, because there are no subjects for whom $L_i = 0$ and $C_i = 0$, i.e., no subjects who received neither conservative nor liberal policy descriptions. But we can estimate the *effect of switching from conservative to liberal policy changes*,

$$\tau_L - \tau_C = E[Y_i(L_i = 1, C_i = 0)] - E[Y_i(L_i = 0, C_i = 1)].$$

Like the effect of switching from "Democrats support" to "Democrats oppose" party cues, this is simply a difference of average treatment effects. The analysis of Experiment 1 focuses on these two "switching" effects because they are estimable and directly comparable: both are differences of average treatment effects. (See Imai, Keele, and Yamamoto 2009, 18-20 for an analogous treatment of the experiment in Nelson, Clawson, and Oxley 1997.)

Experiment 1: Need-for-Cognition Analyses with Higher-Order Interactions

The analyses presented in Table 2 suggest that need for cognition is a strong moderator of policy effects but a weaker and less consistent moderator of party-cue effects. The table below bolsters the finding by reporting estimates from more elaborate models. The first and third columns report estimates from a model that is like the one reported in Table 2 but that permits interactions among the experimental conditions: that is, it includes terms for (Democratic legislators support × liberal policy changes) and (Democratic legislators oppose × liberal policy changes). To this, the second and fourth columns add three-variable interactions: (Democratic legislators support × liberal policy changes × need for cognition) and (Democratic legislators oppose × liberal policy changes × need for cognition).

In all of these models, need for cognition is a consistently powerful moderator of policy-direction effects, a less consistent moderator of party-cue effects. The models reported here offer almost no additional explanatory power: note that the R^2 and standard errors of regression reported here are the same as those reported in Table 2.

	Democratic subjects				Republican subjects			
Intercept	2.50	.28	2.77	.32	1.95	.37	2.05	.44
Democratic legislators support	−.76	.39	−1.22	.49	.01	.47	−.40	.63
Democratic legislators oppose	−.24	.39	−.68	.50	.41	.48	.52	.65
Liberal policy changes	2.12	.34	1.37	.54	1.84	.40	1.62	.63
Need for cognition	−.35	.46	−.82	.53	1.88	.58	1.70	.70
Democratic legislators support × need for cognition	1.43	.61	2.23	.79	−.57	.73	.13	1.02
Democratic legislators oppose × need for cognition	.02	.62	.77	.81	−.09	.74	−.28	1.04
Liberal policy changes × need for cognition	.95	.52	2.24	.87	−2.29	.60	−1.92	1.02
Democratic legislators support × liberal policy changes	.04	.24	1.26	.79	.21	.27	1.03	.90
Democratic legislators oppose × liberal policy changes	−.15	.23	.97	.78	.59	.27	.39	.92
Democratic legislators support × liberal policy changes × need for cognition			−2.06	1.26			−1.40	1.45
Democratic legislators oppose × liberal policy changes × need for cognition			−1.88	1.25			.32	1.48
Standard error of regression	1.63		1.63		1.88		1.88	
Likelihood ratio test vs. Table 2 model	.69	($p = .71$)	4.06	($p = .40$)	4.89	($p = .09$)	6.45	($p = .17$)
R^2	.41		.41		.09		.09	
Number of observations	1163		1163		1183		1183	

Table A4: Need-for-Cognition Analyses with Higher-Order Interactions. Each column reports parameter estimates and standard errors from an ordinary least squares regression. In each regression, the dependent variable is attitude toward the proposed policy changes, which is measured on a seven-point scale; higher values indicate a more positive attitude. The party-cues variables ("Democratic legislators support" and "Democratic legislators oppose") and the policy variable ("Liberal policy changes") are scored 0 or 1. Need for cognition ranges from 0 to 1.

The models reported in Table 2 nest within the models reported here, and the additional terms in the models reported here make little substantive difference. As in Table 2, need for cognition appears to be a strong moderator of policy effects, a weaker and less consistent moderator of party-cue effects. Note that the R^2 and standard errors of regression reported here are identical to those reported in Table 2.

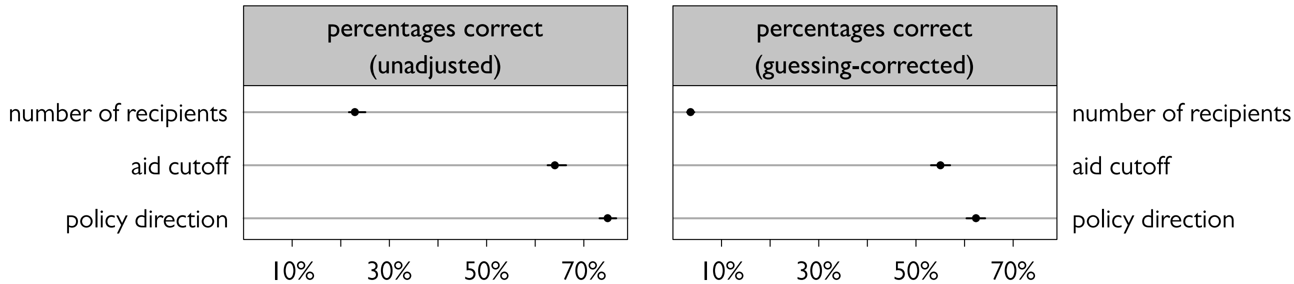

Figure A3: Percentages Holding Correct Beliefs about Policy. Each panel reports summary information about responses to questions about policy. The left panel reports unadjusted percentages of subjects who answered correctly. Following Luskin (2002), the right panel reports the "guessing-corrected" percentages; these are estimates of the percentages of subjects who would have answered correctly in the absence of lucky guessing. In both panels, black lines represent 95% confidence intervals.

Experiment 1: Beliefs about Policy

The left-hand panel of Figure A3 presents the percentages correctly answering the questions about policy. These percentages reflect the responses of both those who knew the answers and those who guessed luckily. To purge these percentages of the influence of lucky guessing, the right-hand panel presents the "guessing-corrected percentages," following the procedure used by Luskin (2002).[3] The guessing-corrected percentages are better indicators of the proportion of subjects in the sample who held correct beliefs. They show that a majority of subjects knew the maximum income that a single parent of two could earn while remaining eligible for Medicaid under the proposed legislation. A majority also knew whether the proposed policy changes would expand or reduce health care benefits. But large minorities did not know the answers to these questions, and almost no one recalled the number of Medicaid recipients in Wisconsin. These

[3] The guessing-corrected percentage correct for any item is % correct – (% incorrect)(# correct response options / # incorrect response options). The implicit assumptions of this method are that all who answer incorrectly are guessing and that guesses are equally distributed across the response options. For example, if 25% of subjects incorrectly answer a question with two response options, another 25% are assumed to have guessed the correct answer. If all subjects answered the question, the guessing-corrected percentage correct is 75% – 25%(1/1) = 50%.

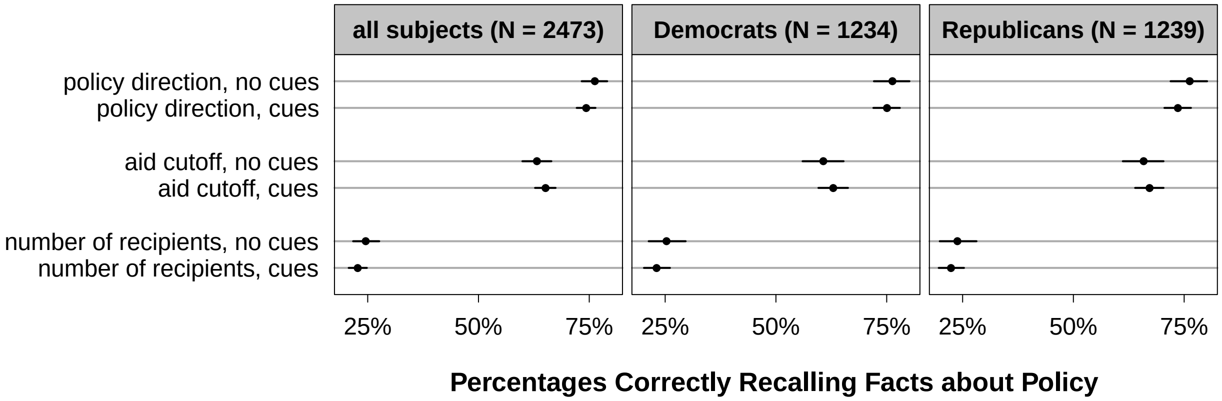

Figure A4: No Effect of Cues on Recall of Policy Facts in Experiment 1. Within each panel, each row plots the percentage of cued or uncued subjects correctly answering a factual question about the proposed policy changes. The "policy direction" rows indicate how many subjects correctly recalled whether the policy would expand or reduce health-care benefits. The "aid cutoff" rows indicate whether subjects correctly recalled the maximum amount that single parents of two could earn while remaining eligible for Medicaid benefits. And the "number of recipients" rows indicate whether subjects correctly recalled the number of people who stood to gain or lose benefits. Black lines in each row are 95% confidence intervals.

Comparing the rows within each tier shows that cues made little difference to subjects' recall of policy facts. This suggests that party cues did not cause subjects to think less about the policy content to which they had been exposed.

percentages are consistent with other research (Schwieder and Quirk 2004), and they suggest that subjects did not come close to using all of the information in the article. If they had, the observed policy effects might have been even larger.[4]

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[4] Subjects assigned to the liberal policy condition received information indicating that the eligibility cutoff for single parents of two would be $2,184 per month under the proposed policy changes. For these subjects, both "about $2,000 a month" and "about $2,500 a month" were counted as correct answers to the question about the eligibility cutoff.

Experiment 2: Factorial Design

	Expand benefits		Reduce benefits	
	Large changes	Small changes	Small changes	Large changes
No cues	some legislators support changes; others oppose them	some legislators support changes; others oppose them	some legislators support changes; others oppose them	some legislators support changes; others oppose them
"Democrats oppose" cues	Democratic legislators oppose changes; Republican legislators support them	Democratic legislators oppose changes; Republican legislators support them	Democratic legislators oppose changes; Republican legislators support them	Democratic legislators oppose changes; Republican legislators support them

Table A5: Design of Experiment 2. Experiment 2 had a $2 \times 2 \times 2$ factorial design. Each subject read about legislation that would expand or reduce state-provided health-care benefits. The departures from the health-care status quo were large or small. Subjects in the large change conditions read about the same changes that were described to subjects in Experiment 1.

In the "Democrats oppose" condition, Democratic legislators opposed the changes while Republican legislators supported them. In the "no cues" condition, subjects read about support for and opposition to the proposed changes, but the positions were not linked to political parties.

Experiment 2: Article Text

In Experiment 2, subjects were assigned to read about either liberal or conservative changes to the health-care status quo. The were further assigned to read about departures from the status quo were large or small, and to receive no party cues or party cues indicating that Democratic legislators opposed the proposed changes while Republican legislators supported them. There were thus eight experimental conditions. Each condition was associated with a different version of a newspaper article that was modeled on Lieb (2005).

Large liberal policy changes, no party cues. The article used in this condition was the same as the article used in the "liberal policy changes, no party cues" condition of Experiment 1. See page A9.

Large liberal policy changes, "Democratic legislators oppose" party cues. The article used in this condition was the same as the article used in the "liberal policy changes, Democratic legislators oppose" condition of Experiment 1. See page A13.

Small liberal policy changes, no party cues. Gov. David Brady won a key budget battle Thursday as the House sent him a bill authorizing the expansion of Medicaid health coverage for tens of thousands of low-income residents. The House's 87-71 vote came on the same day its Budget Committee was finalizing a roughly $19 billion spending plan that would implement the Medicaid expansion beginning July 1.

Brady said the expansion is needed to protect the disabled, elderly, and parents who currently lack coverage.

But opponents contend the expansion could lead to reduced school funding, a budget deficit, and higher taxes. They also argued that the expansion could threaten the long-term sustainability of the state's other social welfare services.

The plan would increase health care coverage for nearly 10,000 of Wisconsin's 1 million Medicaid recipients by loosening eligibility standards, and it would add certain services such as dental care for many others. It also would reduce co-payments or premiums for tens of thousands of Medicaid enrollees.

Brady praised the Legislature for taking "decisive actions to protect the poorest among us." He said the bill's anti-waste and fraud provisions—such as annual Medicaid eligibility reviews—would "ensure that scarce state resources are going to those in need."

The bill would reduce waiting times for such things as wheelchairs and artificial limbs for most adults. Recipients would be permitted one eye care visit every two years, as they are now.

A late provision added by the House would also expand a program that provides Medicaid coverage to disabled people aged 16 to 64 if they work at least three hours a month. Currently, disabled adults qualify for coverage if they earn less than $1,940 a month. The House bill raises the cutoff to $2,160 a month.

Opponents of the expansion point to the growth of Medicaid. In the past dozen years, the Medicaid rolls doubled while its cost nearly tripled. Yet even without the proposed expansion, Medicaid would cost more than $5.5 billion in state and federal money next fiscal year, consuming nearly 29 percent of Wisconsin's budget.

The expansion is dangerous because "we must ensure the children of our state can be educated, that our most vulnerable are protected, and (that) we do it in such a manner that creates solid footing for the state of Wisconsin," said David Toolan, Chair of Residents for Responsible Government, a nonprofit group that has been lobbying against the expansion.

But supporters claim the Medicaid expansions would ensure that the most vulnerable receive necessary protections.

Currently, most adult Medicaid recipients are required to make co-payments of between 50 cents and $3, depending on the cost of the service, each time they visit a doctor or hospital. Under the House bill, co-payments would cost between 50 cents and $2 per visit.

The bill also would eliminate monthly premiums of families in the MC+ for Kids program, which provides health care to children whose families earn up to three times the federal poverty level but aren't covered by traditional Medicaid or private insurance. Because some families will join the program if the premiums are eliminated, the Department of Social Services estimates about 7,900 children will gain coverage.

Under the House version, a single parent of two could earn no more than $1,618 a month to qualify for Medicaid. The current cutoff for single parents of two is $1,334 a month.

Representative Connie Zimmer, I-Mellen, said she gets a $493.50 state mileage check for driving to the Capitol each month.

To qualify for Medicaid under current conditions, "we're telling somebody that they should raise a family of three for less money than any three of us get for gas, and that's hypocritical,'' she said.

The bill is HB 593.

Small liberal policy changes, "Democrats oppose'' party cues. Gov. David Brady won a key budget battle Thursday as the House sent him a bill authorizing the expansion of Medicaid health coverage for tens of thousands of low-income residents. The House's 87-71 vote came on the same day its Budget Committee was finalizing a roughly $19 billion spending plan that would implement the Medicaid expansion beginning July 1. 80 of 89 House Republicans voted for the bill, while 62 of 69 House Democrats voted against it.

Brady, a Republican, and Republican legislative leaders said the expansion is needed to protect the disabled, elderly, and parents who currently lack coverage.

But Democratic opponents contend the expansion could lead to reduced school funding, a budget deficit, and higher taxes. They also argued that the expansion could threaten the long-term sustainability of the state's other social welfare services.

The plan would increase health care coverage for nearly 10,000 of Wisconsin's 1 million Medicaid recipients by loosening eligibility standards, and it would add certain services such as dental care for many others. It also would reduce co-payments or premiums for tens of thousands of Medicaid enrollees.

Brady praised the Legislature for taking "decisive actions to protect the poorest among us." He said the bill's anti-waste and fraud provisions—such as annual Medicaid eligibility reviews—would "ensure that scarce state resources are going to those in need."

The bill would reduce waiting times for such things as wheelchairs and artificial limbs for most adults. Recipients would be permitted one eye care visit every two years, as they are now.

A late provision added by the House would also expand a program that provides Medicaid coverage to disabled people aged 16 to 64 if they work at least three hours a month. Currently, disabled adults qualify for coverage if they earn less than $1,940 a month. The House bill raises the cutoff to $2,160 a month.

Opponents of the expansion point to the growth of Medicaid. In the past dozen years, the Medicaid rolls doubled while its cost nearly tripled. Yet even without the proposed expansion, Medicaid would cost more than $5.5 billion in state and federal money next fiscal year, consuming nearly 29 percent of Wisconsin's budget.

The expansion is dangerous because "we must ensure the children of our state can be educated, that our most vulnerable are protected, and (that) we do it in such a manner that creates solid footing for the state of Wisconsin," said House Budget Committee Member David Toolan, D-Milwaukee.

But supporters claim the Medicaid expansions would ensure that the most vulnerable receive necessary protections.

Currently, most adult Medicaid recipients are required to make co-payments of between 50 cents and $3, depending on the cost of the service, each time they visit a doctor or hospital. Under the House bill, co-payments would cost between 50 cents and $2 per visit.

The bill also would eliminate monthly premiums of families in the MC+ for Kids program, which provides health care to children whose families earn up to three times the federal poverty level but aren't covered by traditional Medicaid or private insurance. Because some families will join the program if the premiums are eliminated, the Department of Social Services estimates about 7,900 children will gain coverage.

Under the House version, a single parent of two could earn no more than $1,618 a month to qualify for Medicaid. The current cutoff for single parents of two is $1,334 a month.

Representative Connie Zimmer, R-Mellen, said she gets a $493.50 state mileage check for driving to the Capitol each month.

To qualify for Medicaid under current regulations, "we're telling somebody that they should raise a family of three for less than any three of us get for gas, and that's hypocritical," she said.

The bill is HB 593.

Large conservative policy changes, no party cues. The article used in this condition was the same as the article used in the "conservative policy changes, no party cues" condition of Experiment 1. See page A15.

Large conservative policy changes, "Democratic legislators oppose" party cues. The article used in this condition was the same as the article used in the "conservative policy changes, Democratic legislators oppose" condition of Experiment 1. See page A19.

Small conservative policy changes, no party cues. Gov. David Brady won a key budget battle Thursday as the House sent him a bill authorizing the reduction of Medicaid health coverage for tens of thousands of low-income residents. The House's 87-71 vote came on the same day its Budget Committee was finalizing a roughly $19 billion spending plan that would implement the Medicaid cuts beginning July 1.

Brady said the cuts are needed to balance a budget that increases school funding without seeking higher taxes or cutting other social welfare services.

But opponents contend the health care cuts could threaten the health of the disabled, elderly and parents affected.

The plan would reduce health care coverage for nearly 10,000 of Wisconsin's 1 million Medicaid recipients by tightening eligibility standards, and it would end certain services such as dental care for many others. It also would increase co-payments or premiums for tens of thousands of Medicaid enrollees.

Brady praised the Legislature for taking "decisive actions to protect the long-term sustainability of our state's social welfare services.'' He said the bill's anti-waste and fraud provisions—such as annual Medicaid eligibility reviews—would "ensure that scarce state resources are going to those in need.''

The bill would extend waiting times for such things as wheelchairs and artificial limbs for most adults. Recipients would be permitted one eye care visit every two years, as they are now.

A late provision added by the House would also limit Medicaid coverage for disabled people aged 16 to 64 who work at least three hours a month. Currently, disabled adults qualify for coverage if they earn less than $1,940 a month. The House bill lowers the cutoff to $1,300 a month.

Supporters of the cuts point to the growth of Medicaid. In the past dozen years, the Medicaid rolls doubled while its cost nearly tripled. Without the proposed cuts, Medicaid would cost more than $5.5 billion in state and federal money next fiscal year, consuming nearly 29 percent of Wisconsin's budget.

The cuts are necessary because "we must ensure the children of our state can be educated, that our most vulnerable are protected, and (that) we do it in such a manner that creates solid footing for the state of Wisconsin,'' said David Toolan, Chair of Residents for Responsible Government, a nonprofit group that has been lobbying for the bill.

But opponents claim the Medicaid cuts would affect the very vulnerable people supporters say they want to protect.

Currently, most adult Medicaid recipients are required to make co-payments of between 50 cents and $3, depending on the cost of the service, each time they visit a doctor or hospital. Under the House bill, co-payments would cost from $3 to $6 per visit.

The bill also would require monthly premiums of more families in the MC+ for Kids program, which provides health care to children whose families earn up to three times the federal poverty level but aren't covered by traditional Medicaid or private insurance. Because some families would drop out rather than pay the premium, the Department of Social Services estimates about 7,900 children would lose coverage.

Under the House version, a single parent of two could earn no more than $1,050 a month to qualify for Medicaid. The current cutoff for single parents of two is $1,334 a month.

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The bill is HB 593.

Experiment 2: Pre- and Post-Treatment Measures

The pre- and post-treatment measures of party identification were the same as those used for Experiment 1. (See page A21.) Pre-treatment measurements were taken between one and six days before the start of the experiment. 98% of subjects who identified as Democrats before the experiment began also identified as Democrats at the end of the experiment. For Republicans, the corresponding figure was also 98%. (In the post-treatment interview, some subjects initially denied having a major-party affiliation but subsequently said that they were "closer to" one of the major parties than the other. For the purpose of this analysis, these "leaners" are counted as partisans.)

With two exceptions, the other post-treatment items were the same as those used in Experiment 1. The first exception was a question about the maximum income that a single parent of two could earn without becoming ineligible for Medicaid. The question itself remained the same, but the response options were changed to be suitable for subjects in both the small-change and large-change policy conditions:

> If the changes go into effect, what is the most that a single parent of two could earn while still being eligible for Medicaid? [Response options: about $500 a month; about $1,000 a month; about $1,300 a month; about $1,600 a month; about $2,200 a month.]

The second exception was the addition of a prompt that asked subjects to

> Please list your thoughts about the article and the policy changes that it described. The main goal of this study is to better understand what people think about changes like the one that you just read about—so please write as much as you like and take as much time as you need.

Expansion of Benefits			Reduction of Benefits	
Large	Slight	Status Quo	Slight	Large
Monthly income cutoff: $2,184 for a single parent of two.	Monthly income cutoff: $1,618 for a single parent of two.	Monthly income cutoff: $1,334 for a single parent of two.	Monthly income cutoff: $1,050 for a single parent of two.	Monthly income cutoff: $484 for a single parent of two.
No copayments for visits to doctor.	Copayments for visits to doctor: 50 cents to $2.	Copayments for visits to doctor: 50 cents to $3.	Copayments for visits to doctor: $3-$6.	Copayments for visits to doctor: $4-$10.
Eliminate premiums for some families, leading 23,700 children to gain coverage.	Eliminate premiums for some families, leading 7,900 children to gain coverage.		Require premiums for some families, leading 7,900 children to lose coverage.	Require premiums for some families, leading 23,700 children to lose coverage.
Expand mandatory coverage of wheelchairs, prostheses, and eye care. Reduce waiting times for wheelchairs and prostheses. Eye-care visits once every year.	Reduce waiting times for wheelchairs and prostheses. Eye-care visits once every two years.	Coverage of wheelchairs and prostheses. Eye-care visits once every two years.	Extend waiting times for wheelchairs and prostheses. Eye-care visits once every two years.	Repeal mandatory coverage of wheelchairs, prostheses, and eye care. Continue to fund wheelchairs, prostheses, and eye-care visits once every three years.
Coverage of temporarily disabled people aged 16 to 64 who earn less than $2,600 per month and work at least three hours per month.	Coverage of temporarily disabled people aged 16 to 64 who earn less than $2,160 per month and work at least three hours per month.	Coverage of temporarily disabled people aged 16 to 64 who earn less than $1,940 per month and work at least three hours per month.	Coverage of temporarily disabled people aged 16 to 64 who earn less than $1,300 per month and work at least three hours per month.	Eliminate coverage for the temporarily disabled.
Expand coverage for 100,000 of the state's one million Medicaid recipients.	Expand coverage for 10,000 of the state's one million Medicaid recipients.		Reduce coverage for 10,000 of the state's one million Medicaid recipients.	Reduce coverage for 100,000 of the state's one million Medicaid recipients.

Table A6: Policy Details in Experiment 2. All subjects in Experiment 2 read a newspaper article that contrasted the status quo with liberal or conservative policy changes that had just been passed by the state House of Representatives. "Large"-change policies were those that subjects read about in Experiment 1. "Slight"-change policies offered smaller changes from the status quo.

Experiment 2: Randomization Checks

Table A7 reports results from a set of logistic regressions. In each regression, one of the treatments in Experiment 2 is regressed on other treatment and pre-treatment variables. As expected, the χ^2 statistics are insignificant and the pseudo-R^2 values are low for each regression, suggesting that the randomizations in Experiment 2 were not systematically associated with the predictors. The intercepts in the large-policy-change (LARGE) regressions are large and negative, which is consistent with the intentional assignment of more subjects to the large-policy-change condition than to the small-policy-change condition.

The estimates in the last three rows of coefficients are not independent. For example, the coefficient on "large policy changes" in each LIB regression is necessarily the same as the coefficient on "liberal policy" in the corresponding LARGE regression.

Of the 72 independent estimates of terms other than intercepts, three are significant at $p < .05$, two-tailed: the estimate for "female" in the eighth column and the estimates for "liberal policy" in the first and fourth columns (which are necessarily the same as the estimates for "Democrats oppose" cues in the second and fifth columns). This percentage of significant estimates (3/72 = 4.2%) is almost exactly what we would expect by chance if all 72 coefficients were null.

	All Subjects						Democratic Subjects						Republican Subjects					
	CUE		LIB		LARGE		CUE		LIB		LARGE		CUE		LIB		LARGE	
Intercept	−.16	.18	.01	.18	−1.03	.20	.27	.30	.34	.29	−.82	.34	−.40	.23	−.20	.23	−1.08	.26
Female	−.01	.07	.09	.07	.03	.08	−.19	.12	−.15	.12	.01	.13	.09	.09	.23	.09	.02	.10
Age	−.00	.00	−.00	.00	.00	.00	−.00	.00	−.00	.00	−.00	.00	−.00	.00	−.00	.00	−.00	.00
Low education	−.02	.11	−.08	.11	−.13	.12	−.14	.18	−.17	.17	−.37	.19	.03	.14	−.04	.14	.03	.15
Medium education	−.01	.10	−.07	.10	.01	.11	−.09	.16	−.10	.16	−.19	.17	.02	.13	−.06	.12	.14	.14
Northeast	.03	.11	.16	.11	.07	.12	.03	.18	.10	.17	.22	.19	.03	.15	.19	.15	−.06	.16
South	−.02	.09	.04	.09	.05	.10	−.05	.16	.03	.15	.03	.17	−.02	.12	.06	.12	.04	.13
West	.01	.10	.07	.10	−.07	.11	.02	.17	−.14	.16	−.06	.19	−.01	.13	.19	.13	−.08	.15
"Democrats oppose" cues			−.14	.07	.11	.08			−.31	.12	−.03	.13			−.05	.09	.19	.10
Liberal policy	−.14	.07			.01	.08	−.31	.12			−.06	.13	−.05	.09			.04	.10
Large policy changes	.11	.08	.01	.08			−.03	.13	−.06	.13			.19	.10	.04	.10		
Log likelihood	−2245		−2287		−1953		−852		−871		−727		−1388		−1408		−1222	
Likelihood ratio test	7.2	$p = .70$	8.8	$p = .55$	6.0	$p = .82$	11.9	$p = .29$	12.0	$p = .28$	6.5	$p = .77$	5.0	$p = .89$	10.3	$p = .41$	6.3	$p = .79$
Cragg and Uhler (1970) R^2	.003		.004		.003		.013		.013		.007		.003		.007		.004	
Number of observations	3309		3309		3309		1265		1265		1265		2044		2044		2044	

Table A7: Randomization Checks for Experiment 2. Each column reports estimates and standard errors from a logistic regression of a randomized variable on other variables. "CUE," "LIB," and "LARGE," are the randomized variables: party cues, the liberal policy condition, and the large-policy-change condition. "Northeast," "South," and "West" refer to subjects' region of residence as defined by the U.S. Census Bureau; the index category is the Midwest. "Low education" indicates no formal education beyond high school. "Medium education" indicates formal education beyond high school but not beyond college. The index category, "high education," includes subjects who had some post-college education. Entries in the "likelihood ratio test" row are χ^2 statistics from a test against an intercept-only model.

Experiment 2: Sample Characteristics

Figure A5 contrasts characteristics of the Experiment 2 sample with those of partisans in the 2008 American National Election Study. It shows that the sample percentages of subjects who are women (62%), high-scoring on both ANES need-for-cognition items (33%), from the Northeast (16%), the Midwest (24%), the South (37%), or the West (23%) are all within 6% of the corresponding ANES percentages. This is consistent with the small discrepancies that Sanders et al. (2007) and Stephenson and Crête (2011) find between Internet surveys and surveys conducted through other modes (though see Malhotra and Krosnick 2007, who find somewhat larger differences).

The largest outlier—larger than in Experiment 1—is age. Relative to ANES partisans, the Experiment 2 sample has fewer people age 30 or younger (13% vs. 22%) and more who are 56 or older (48% vs. 32%). With respect to education, the Experiment 2 sample is much more representative than the Experiment 1 sample, although it is still more educated than the ANES sample: 28% of Experiment 2 subjects age 25 or older have no more than a high-school education, against 41% of ANES partisans age 25 or older. 18% of Experiment 2 subjects hold advanced degrees, against 10% of ANES subjects. But as in Experiment 1, the median education level in the sample is the same as the median for all American partisans: more than 12 years of schooling but no college degree. The Experiment 2 sample is also close to the ANES in need for cognition (variables V085170x and V085171); to the extent that education proxies for cognitive effort, this suggests that the results are not affected by under-representation of people who have no post-high-school education.

The most direct evidence about the consequences of under-representation of the young and relatively uneducated is given by Figure A6, and it shows that the sample's under-representation of the young and uneducated is unlikely to have large effects on the analyses of Experiment 2. The top row of Figure A6 suggests that underrepresentation of the young may cause the analyses to slightly overstate the effect of policy: the average effect of policy was .28 points lower among subjects 30 or younger than among subjects 56 or older

(p = .12, two-tailed). The bottom row of Figure A6 suggests the opposite: as in Experiment 1, the sample's underrepresentation of low-education subjects may cause the analyses to slightly understate the effect of policy. Policy effects are .14 points greater for low-education subjects than for high-education subjects (p = .35, two-tailed).

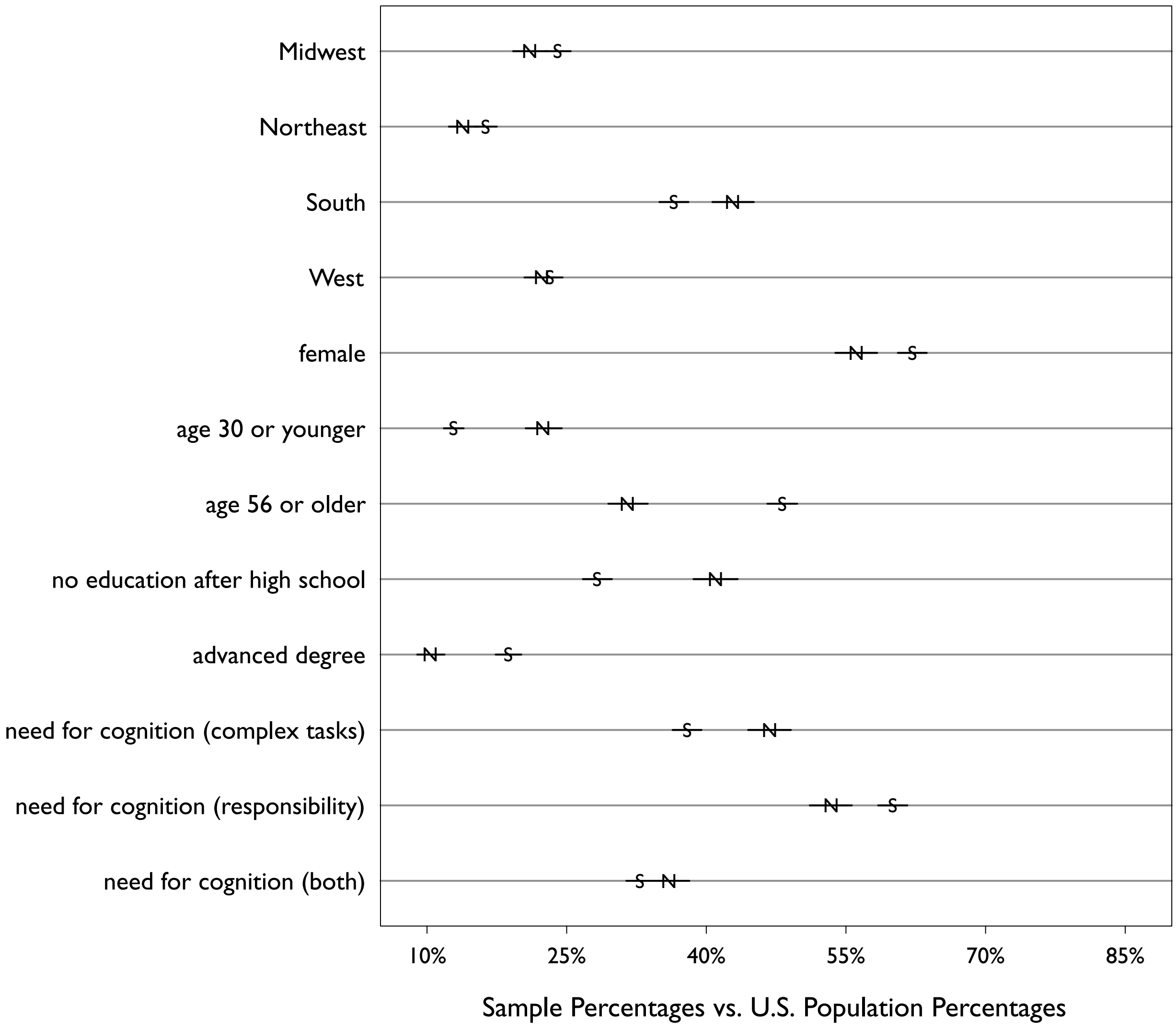

Figure A5: Experiment 2 Sample Representativeness. Each row plots percentages of Experiment 2 subjects ("S") who share a characteristic. The corresponding percentages for partisans in the U.S. population ("N") are drawn from the 2008 ANES. Black lines are 95% confidence intervals.

"Midwest," "Northeast," "South," and "West" indicate percentages of subjects residing in each region. "Need for cognition (complex tasks)" plots percentages of subjects indicating that they "prefer complex to simple tasks." "Need for cognition (responsibility)" plots percentages indicating that they like having responsibility for situations that "require lots of thinking." "Need for cognition (both)" plots percentages indicating that they prefer complex tasks and like having responsibility for situations that require lots of thinking.

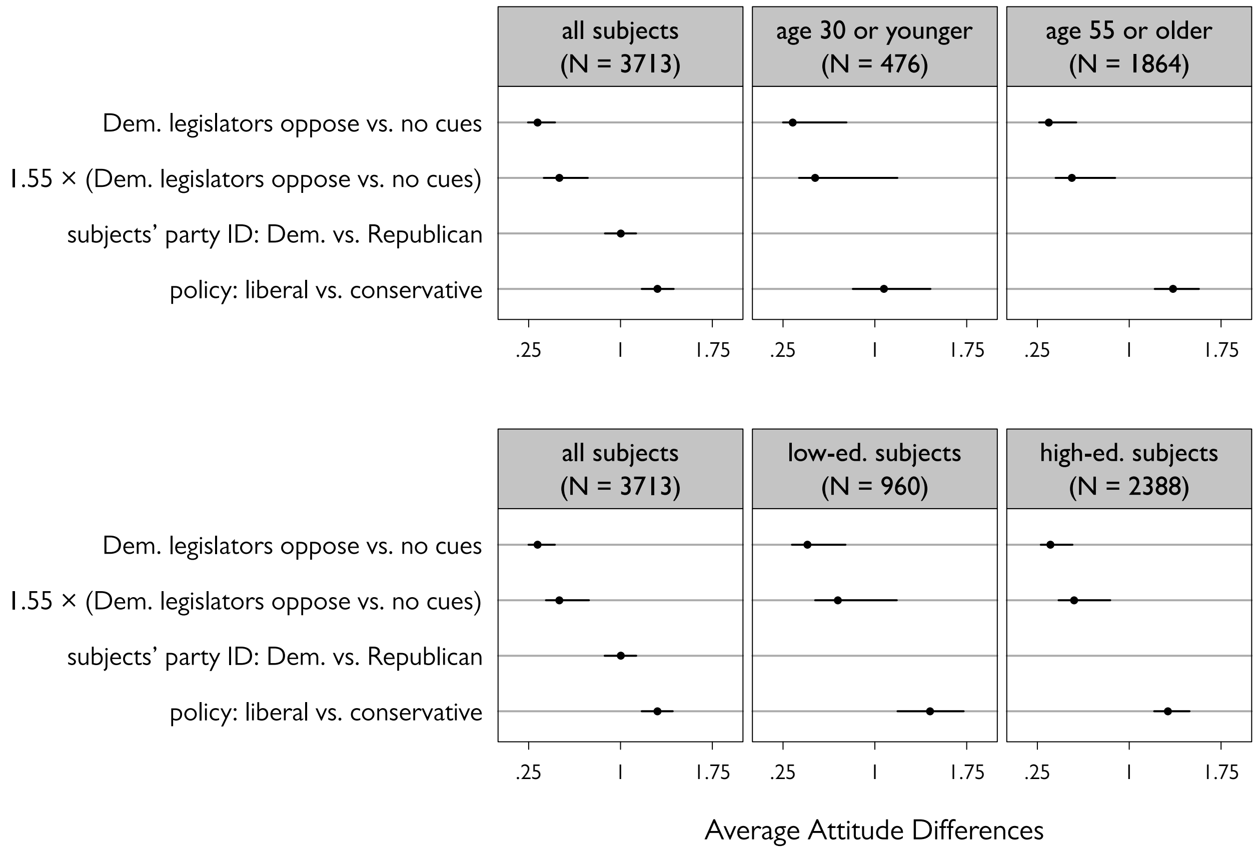

Figure A6: Average Attitude Differences in Experiment 2 by Changes in Party Cues, Party ID, and Policy (Stratification by Age and Education). Each row plots the average of absolute differences between different groups' attitudes toward the proposed policy changes. For example, the bottom row of either left-hand panel shows that, on average, exposing subjects to liberal instead of conservative policy content changed attitudes by 1.24 points on the seven-point attitude scale. Black lines in each row are 95% confidence intervals.

The top two rows of the left-hand panels show that when averaging over all subjects, changes in cue condition have slight effects on attitudes. The average difference between Republicans and Democrats, displayed in the third row, is greater. The greatest average effect is caused by exposing subjects to liberal rather than conservative policy changes. The top panels show that this effect is smaller for subjects 30 years old or younger (1.07 points on the seven-point scale) than for subjects 56 years old or older (1.36 points). The difference is significant at $p = .12$, two-tailed. The bottom panels show that the policy effect is greater for low- than for high-education subjects, but the difference does not approach statistical significance. ("Low-education" subjects are those who never attended college; "high-education" subjects are those who did. Education data are missing for 365 subjects.)

Experiment 2: Mean Attitudes in Each Condition

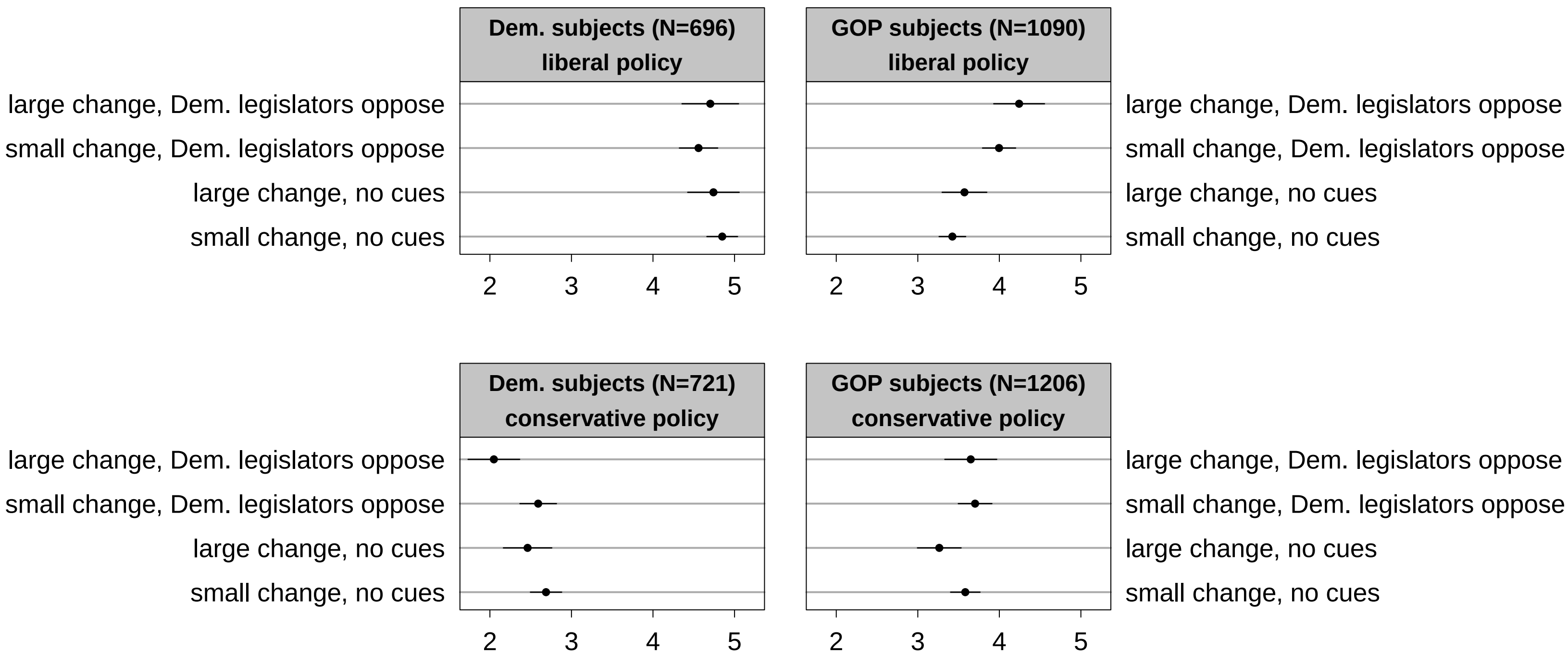

Figure A7: Effects of Cues, Policy Direction, and Policy Extremity in Experiment 2. All panels plot mean attitude toward the proposed policy changes. Responses range from 1 ("disapprove strongly") to 7 ("approve strongly"). Black lines are 95% confidence intervals. The results show that both party cues and policy direction affected attitudes. The effect of policy direction was greater on average and greater for Democratic than for Republican subjects. Policy extremity—exposure to descriptions of large policy changes rather than small policy changes—had little effect on attitudes.

Experiment 2: Need-for-Cognition Analyses with Higher-Order Interactions

The analyses presented in Table 3 suggest that need for cognition is a strong moderator of policy direction but a weaker and less consistent moderator of party cues and policy size. The table below bolsters the finding by reporting estimates from a saturated model in which each predictor is interacted with every other predictor. The substantive results closely mirror those of Table 3. But the models reported here offer little extra explanatory power: note that the R^2 and standard errors of regression reported here are nearly the same as those reported in Table 3.

	Democratic subjects		Republican subjects	
Intercept	2.93	.32	2.40	.25
Democratic legislators oppose	−.49	.46	.08	.39
Liberal policy changes	1.38	.42	1.56	.37
Large policy changes	−.38	.56	.69	.51
Need for cognition	−.43	.52	2.09	.41
Democratic legislators oppose × liberal policy changes	.18	.66	−.30	.58
Democratic legislators oppose × large policy changes	.52	.86	.34	.75
Large policy changes × liberal policy changes	.36	.77	−.97	.71
Democratic legislators oppose × need for cognition	.70	.76	.05	.65
Liberal policy changes × need for cognition	1.42	.71	−3.05	.63
Large policy changes × need for cognition	.28	.93	−1.78	.85
Democratic legislators oppose × liberal policy changes × large policy changes	.02	1.22	−.10	1.07
Democratic legislators oppose × liberal policy changes × need for cognition	−.72	1.10	1.35	.98
Democratic legislators oppose × large policy changes × need for cognition	−1.48	1.43	−.11	1.25
Liberal policy changes × large policy changes × need for cognition	−.51	1.27	2.55	1.19
Democratic legislators oppose × liberal policy changes × large policy changes × need for cognition	1.03	2.02	−.18	1.78
Standard error of regression	1.66		1.90	
Likelihood ratio test vs. Table 3 model	6.48	$p = .60$	24.08	$p = .002$
R^2	.31		.04	
Number of observations	1413		2293	

Table A8: Need-for-Cognition Analyses with Higher-Order Interactions (Experiment 2). This table builds on Table 3. Each column reports OLS estimates and standard errors. In each regression, the dependent variable is attitude toward the proposed policy changes, which is measured on a seven-point scale; higher values indicate a more positive attitude. “Democratic legislators oppose,” “Liberal policy changes,” and “Large policy changes” are scored 0 or 1. Need for cognition ranges from 0 to 1.

The models reported in Table 3 nest within the models reported here, and the additional terms in the models reported here make little substantive difference. As in Table 3, need for cognition appears to be a strong moderator of policy effects, a weaker and less consistent moderator of party-cue effects. Note that the R^2 and standard errors of regression reported here are very close to those reported in Table 3.

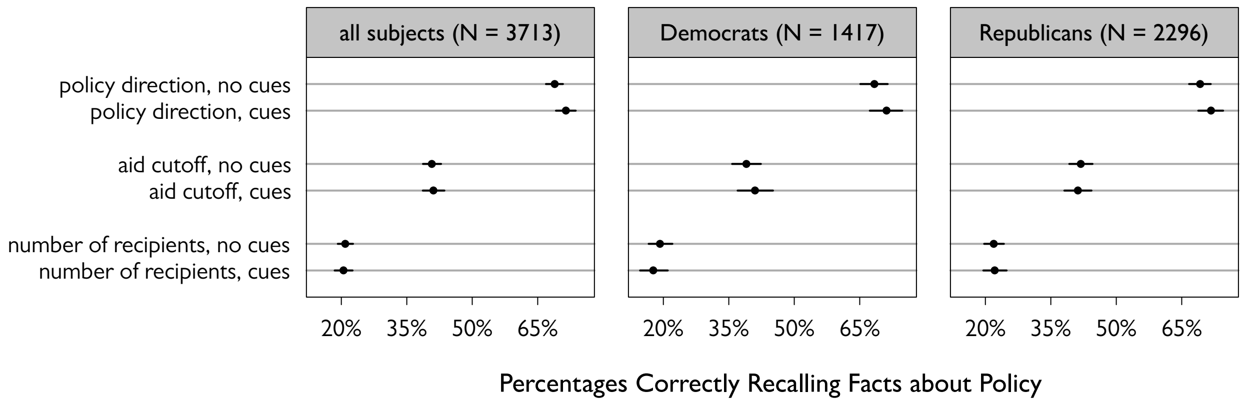

Figure A8: No Effect of Cues on Recall of Policy Facts in Experiment 2.

Experiment 2: Further Analyses of Policy Recall and Time Spent on Article

Figure A8 depicts the percentages of cued and uncued subjects correctly answering each of the policy questions asked in Experiment 2. (Panel 1 of Figure 4 presents related information but averages over results for all three questions.) On average, subjects receiving cues were less likely to recall the number of Medicaid recipients in Wisconsin, *more* likely to recall the proposed cutoff for Medicaid eligibility or whether the policy would reduce or expand benefits. But none of these differences approach substantive or statistical significance. The middle and right-hand panels show that there are no important differences between parties, either.

Figure A9 depicts the average amount of time spent on the article by Democrats and Republicans in each experimental condition. Panel 2 of Figure 4 reports related averages taken over multiple experimental conditions. As with Panel 2 of Figure 4, Figure A9 depicts 99% trimmed means, excluding a few subjects who appear to have walked away from their computers for hours at a time. (See page 29.)

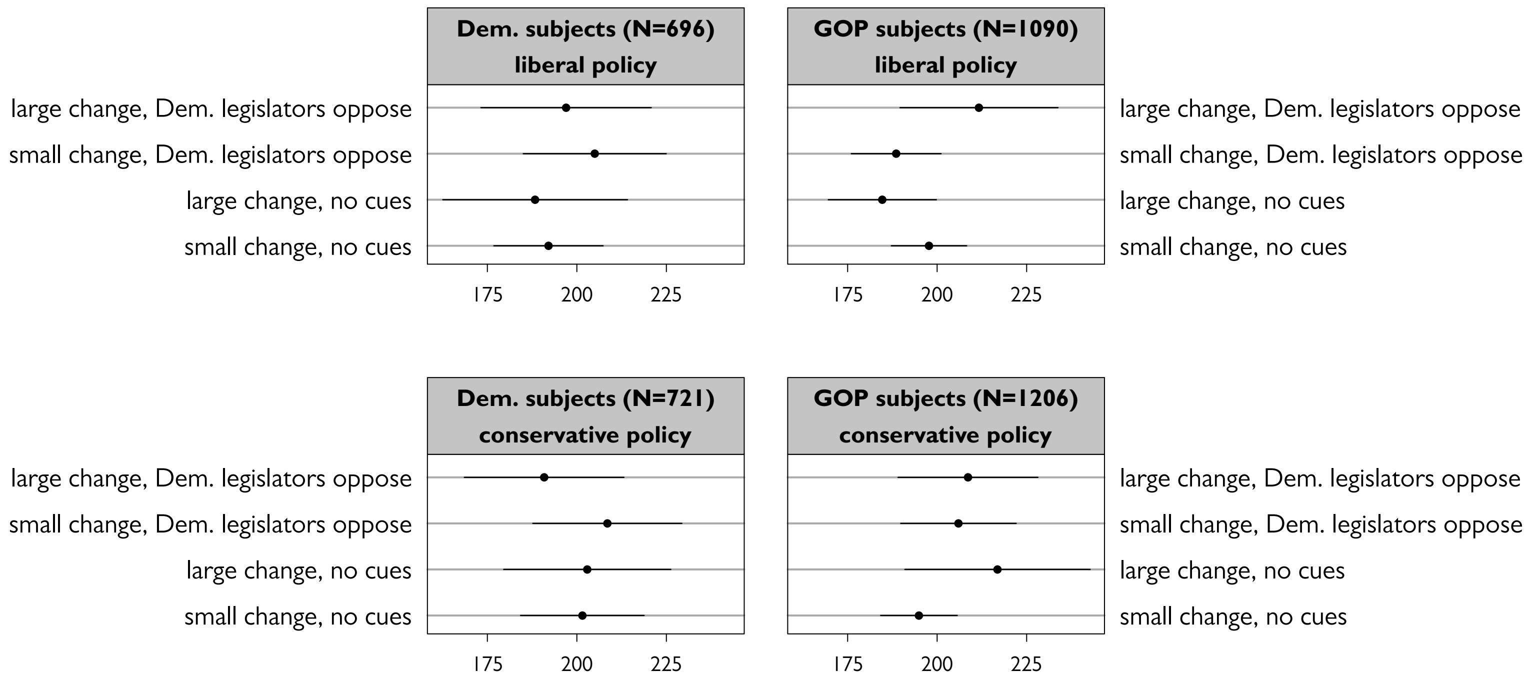

Figure A9: Time Spent Reading Article in Experiment 2. Each row presents 95%-trimmed means of the time that subjects in an experimental condition spent on the article. Panel 2 of Figure 4 presents similar information but averages over many experimental conditions.

Moderators of Republican Support for Benefit-Expanding Policies

In Experiments 1 and 2, Republican subjects dislike benefit-expanding policies less than benefit-reducing policies. The discussion of Experiment 2 (pages 31-33) identifies the most likely reasons for this result. This section considers three possible moderators of the result: need for cognition, depth of processing, and age.

Tables 2 and 3 show that need for cognition is a relevant moderator in both experiments. Republicans with low and middling levels of need for cognition are more approving of the liberal policy, but Republicans highest in need for cognition are not. This result is consistent with the idea that the most thoughtful Republicans are most likely to share the positions of their party's elites.

Need for cognition is theoretically related to depth of processing, but where need for cognition measures stable individual differences in thoughtfulness, depth of processing measures short-term cognitive engagement—in this case, short-term cognitive engagement with the descriptions of the policies that were provided during the experiments. Experiment 2 contained extensive depth-of-processing measures, and Table A9 shows that Republicans who scored high on these measures liked the liberal policy more than the conservative one—unless they were high in need for cognition.

One might expect that age is a third moderator. Older Republicans, more in need of health care, might be more likely to approve a benefit-expanding policy than a benefit-reducing one. This is the pattern that we observe in Experiment 1. Figure A10 shows that Republicans over age 55 liked the liberal policy more than the conservative policy (by .72 points, $p < .001$), while Republicans under 30 were only trivially more approving of the liberal policy (by .07 points, $p = .81$). (For the difference of differences, $p = .07$). But Figure A11 shows that the same result does not hold in Experiment 2. Rather, older Republicans were slightly less approving of the liberal policy (by .11 points, $p = .47$), while younger Republicans were slightly more approving (by .26 points, $p = .22$). The Experiment 2 result is consistent with recent (Brady and Kessler 2010; Newport and Jones 2009) and less recent polling (Gelman, Lee, and Ghitza 2010; Steiber

Intercept	3.44	.08	2.54	.21	2.51	.22
Democratic legislators oppose	.41	.09	.14	.25	.14	.25
Liberal policy changes	.13	.09	1.17	.25	1.18	.25
Large policy changes	−.03	.10	.28	.28	.27	.28
High depth of processing	−.27	.27	−.28	.26	−.01	.49
High depth of processing × Democratic legislators oppose	−.09	.30	−.13	.29	−.11	.30
High depth of processing × liberal policy changes	.67	.30	.69	.30	.68	.30
High depth of processing × large policy changes	−.14	.31	−.15	.31	−.16	.31
Low depth of processing	.00	.20	.07	.20	.10	.38
Low depth of processing × Democratic legislators oppose	−.11	.23	−.08	.23	−.08	.23
Low depth of processing × liberal policy changes	−.22	.23	−.30	.23	−.30	.23
Low depth of processing × large policy changes	.34	.26	.28	.26	.28	.26
Need for cognition			1.58	.35	1.63	.37
Need for cognition × Democratic legislators oppose			.47	.41	.48	.41
Need for cognition × liberal policy changes			−1.82	.41	−1.83	.41
Need for cognition × large policy changes			−.53	.45	−.52	.45
Need for cognition × high depth of processing					−.48	.73
Need for cognition × low depth of processing					−.06	.59
R^2	.02		.03		.03	
Standard error of regression	1.92		1.91		1.91	
Number of observations	2262		2260		2260	

Table A9: Depth of Processing Moderates Republican Preferences (Experiment 2). Each column reports OLS estimates and standard errors. In each regression, the dependent variable is attitude toward the proposed policy changes, which is measured on a seven-point scale; higher values indicate a more positive attitude. "Democratic legislators oppose," "Liberal policy changes," and "Large policy changes" are scored 0 or 1. Need for cognition ranges from 0 to 1. "High depth of processing" is scored 1 for subjects who ranked in the top third of all subjects on the measures of policy facts recalled, time spent reading the article, and number of thoughts reported during the thought-listing procedure. It is scored 0 for all other subjects. Similarly, "low depth of processing" is scored 1 for subjects who scored in the bottom third of all subjects on all three measures, 0 for other subjects.

The "high depth of processing × liberal policy changes" estimate in the first column shows that Republicans who score high in depth of processing approve more of liberal changes to the status quo. The second column shows that this result holds even when we control for need for cognition. The third column shows that the result does not hold among people who are high in both depth of processing and need for cognition: the negative estimate for "need for cognition × high depth of processing" nearly cancels the positive estimate for "high depth of processing × liberal policy changes."

and Ferber 1981), which shows that older citizens are generally more opposed to expansions of government-provided health care.

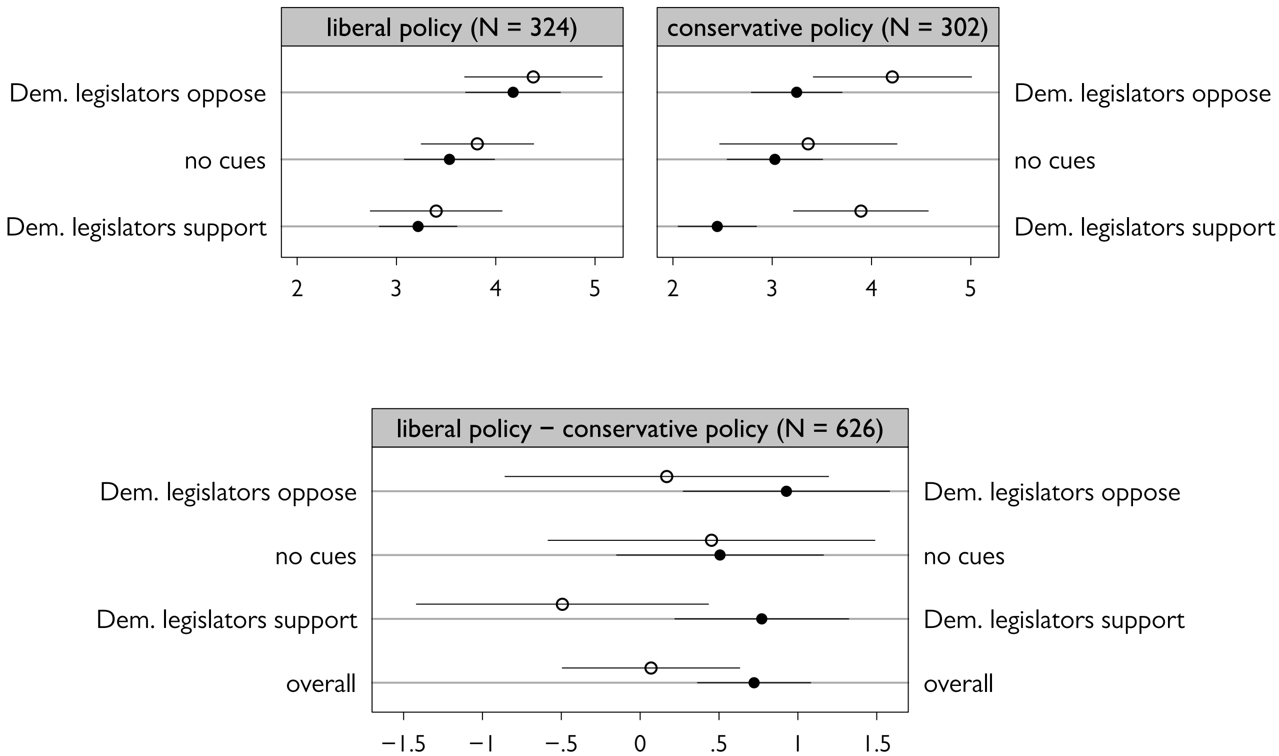

Figure A10: Republican Attitudes by Age in Experiment 1. The top panels plot mean attitudes toward the proposed policy changes among Republicans younger than 31 or older than 55. Attitudes were measured on a 1-7 scale, with higher numbers indicating greater approval. Empty dots represent mean attitudes for Republicans age 30 or younger. Solid dots represent means for Republicans age 56 or older. Black lines are 95% confidence intervals.

The bottom panel plots the differences between mean Republican ratings of the liberal and conservative policies. For example, the bottom row in the bottom panel shows that Republicans age 56 or older were .72 points more approving of the liberal policy than the conservative one ($p < .001$). It also shows that Republicans age 30 or younger were only .07 points more approving ($p = .81$). (For the difference of differences, $p = .07$.)

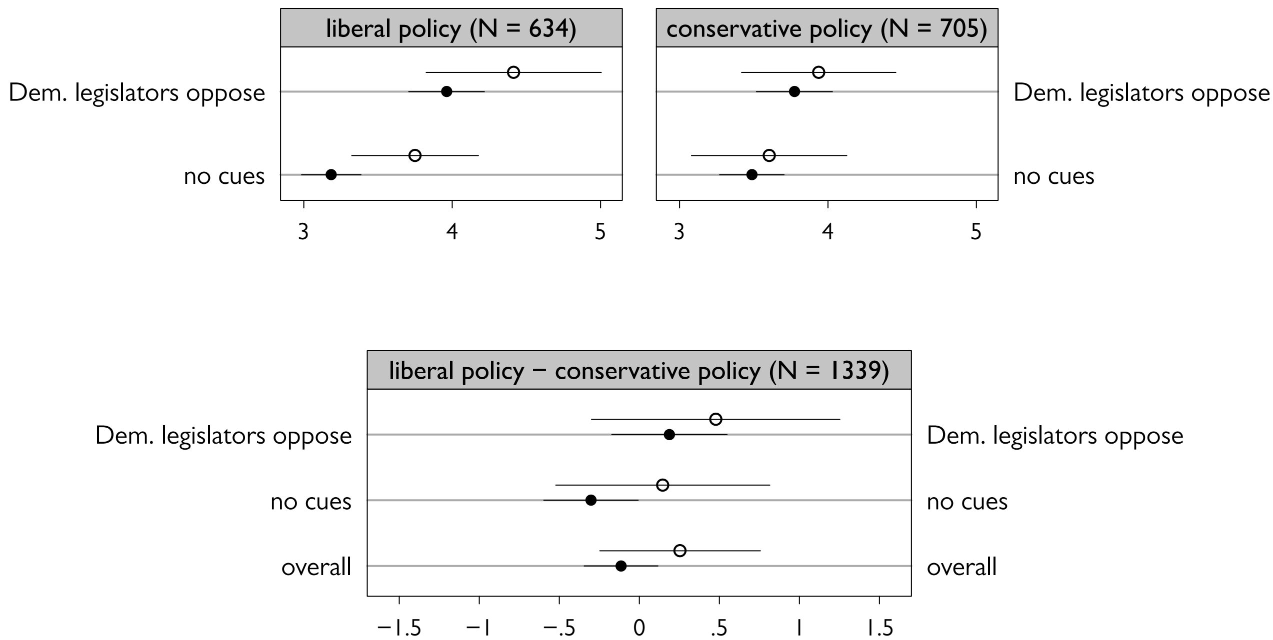

Figure A11: Republican Attitudes by Age in Experiment 2. The top panels plot mean attitudes toward the proposed policy changes (averaging over the "large-change" and "small-change" conditions) among Republicans younger than 31 or older than 55. Attitudes were measured on a 1-7 scale, with higher numbers indicating greater approval. Empty dots represent mean attitudes for Republicans age 30 or younger. Solid dots represent means for Republicans age 56 or older. Black lines are 95% confidence intervals.

The bottom panel plots the differences between mean Republican ratings of the liberal and conservative policies. For example, the bottom row in the bottom panel shows that Republicans age 56 or older were .11 points less approving of the liberal policy than the conservative one (p = .47). It also shows that Republicans age 30 or younger were .26 points more approving (p = .22). (For the difference of differences, p = .18.)

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