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On The Meaning of Survey Reports of Roll Call "Votes"[*]

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Abstract: Contemporary efforts to evaluate representation often compare survey measures of how citizens say they would vote on legislation to what elected officials do in office. These comparisons generally suggest poor representation. We argue here that this common design is unlikely to effectively evaluate representation because responses to survey questions differ in important aspects from voting in legislatures. Measurement error and construct validity undermine the comparison. Three survey experiments show that providing partisan and non-partisan information readily available to legislators materially changes respondents' expressed preferences on roll call votes. With information, expressed policy positions are both less centrist and more closely matched to legislator behavior in their preferred party. Respondents also appear aware of their own lack of knowledge in evaluating roll call policy votes. The treatment effect of information decreases in confidence judging policy in that area. We show similar patterns for respondent opinions on Supreme Court decisions.

Word count: 7,914.

Understanding representation is a central and longstanding research topic in political science. How much do elected officials do what citizens want? Among the most common empirical assessments of the quality of representation are studies that calculate the correspondence between an elected official's behavior and the preferences of his or her constituents. Two potential ways to measure correspondence are to ask (1) would the citizen do something different if granted the legislator's vote, or (2) would she punish or reward the legislator on the basis of the legislator's vote if she were aware of it? Recently, both forms of research have turned to comparing the roll call votes cast by members of Congress with survey measures of the preferences of constituents on those same roll calls. To measure the first version of representation, the researcher compares whether survey respondents' expressed preferences comport with how their member voted. To measure the second version of representation, scholars inform respondents how the member voted and see if it changes respondents' evaluations of, or vote intentions for, the member. In this article, we argue that both of these approaches are subject to large survey measurement challenges that call into question inferences drawn about representation.

The key challenge in comparing the choices representatives make to what constituents want is creating equivalent measures between the two contexts (e.g., Achen, 1977). For example, if a survey respondent believes that abortion rights should be restricted "in some situations," how should that preference map onto voting on a particular bill concerning abortion policy? Scholars have turned to survey measures designed to directly place citizens' choices in the same policy space as their legislative counterparts. The most promising approach in this regard is survey roll call measures that ask respondents how they would vote on the exact piece of legislation that has been considered in the legislature. Such measures, used either in isolation or as a means to bridge a larger set of survey items into a common ideological space with legislator behavior, are presumed to provide measures of citizen preferences that are comparable to legislative roll call votes because the survey respondent is making the same binary choice as the legislator. With such measures, researchers have conducted several important studies assessing the quality of representation, with a canonical finding being that legislators appear substantially more extreme than both the electorate as a whole and their partisan counterparts in the general population (Bafumi and Herron, 2010).

However, there are many reasons that a survey response to a roll call item might not accurately capture the same contextual and strategic dynamics associated with voting in the US Congress. For example, respondents may not appreciate the importance of partisan agenda control and building legislative coalitions, bargaining before an electoral audience, long- versus short-term tradeoffs, etc. Although these limitations are acknowledged in some prior work, this paper argues that such challenges are specific instances of more general sources of systematic error in survey roll call measures. By *systematic error*, we mean a combination of instrumentation-induced measurement error (question wording, etc.) and a lack of construct validity (survey respondents do not possess a preference comparable to legislators on roll call votes).

Each of these manifestations of error call into question the assumption that survey roll call votes yield estimates of citizen preferences that are appropriate to compare to legislator behavior in evaluating quality of representation. Respondents and policymakers are likely not in the same policy space when one is answering survey questions and the other is casting roll call votes. Our argument is that the central problem confronting such comparisons is that voters know very little about the items they are asked to "vote on" in a survey context. As a consequence, responses to roll call items likely have many of the same features that the survey design literature argues challenge the validity and reliability of survey measurement more generally (e.g., Groves et al., 2009). These

"top of the head" responses to survey roll call votes are therefore unlikely to accurately reflect how individuals would act either in more consequential settings or with greater reflection and information. A strong take on our argument – advocated by some of our anonymous referees – would conclude that respondents can never approximate the decisions made by legislators. While we are not prepared to go this far, we do advocate that applied scholars think much more carefully about what they are measuring and what they are assuming.

To demonstrate the importance of this argument for measuring citizen preferences, we present results from three survey experiments with more than 9,000 respondents who voted on survey roll call items similar to those used in previous studies. Departing from most prior work, we randomly varied the amount and type of information available to respondents when they answered the questions. We show that three different types of simple information about the bill materially change the level of support survey respondents express for that roll call. We first examine the effect of providing party margins on the vote when it was taken in Congress. The party margin provides rough information about the strategic, policy, and political implications surrounding the roll call vote that was surely available to a member of Congress when voting but not as readily available to most survey respondents. If the treatment systematically changes the rate of support for the roll call vote among survey respondents, it suggests the survey responses absent that information may not accurately describe how the subject would vote in the legislature.

Prior work in public opinion finds that party cues affect measured survey responses in a wide variety of contexts. While the reasons for these effects are contested, in light of this literature we believe it is also useful to test whether party cues generate partisan fealty and whether non-partisan cues also alter survey roll call responses. First, we show below that the effect of providing information about party voting on a specific bill is moderated by how confident respondents feel evaluating policy in that domain. The treatment effect on roll call support of providing the party vote margin is decreasing in the confidence respondents report in judging policy in the issue area of the roll call. This suggests that, rather than blind obedience, citizens use additional information when they know their own judgments are less informed.

Second, we investigate the effect of providing two non-partisan heuristics on the level of support survey respondents express on roll call votes. In two conditions, we informed respondents about the overall margin in the chamber (i.e., without the party breakdown), and we supplied the budgetary implications of a bill as assessed by the non-partisan Congressional Budget Office (CBO). The CBO score gives individuals a piece of information, apart from the domain-specific policy content of a bill, about how a bill is likely to affect overall government revenue and spending, information that few respondents likely have at the time they are surveyed.

Finally, to demonstrate the general implications of this work for efforts to place elite and mass opinion in the same policy space, we present a parallel party cue experiment in which respondents express opinions on important Supreme Court cases.

To briefly summarize our findings, we show that providing either partisan or non-partisan information changes expressed preferences about important roll call votes cast by Congress and partisan information affects votes on important decisions by the Supreme Court. The votes we selected from our study vary from "easy" issues on which individuals likely have considered their own opinions (e.g., abortion) to "hard" issues where many individuals have likely never considered the issue or understand its technical details (e.g., requiring financial advisors to act as fiduciaries). They also span votes that are partisan (e.g., defunding Planned Parenthood) to issues that split the parties internally (e.g., requiring warrants for electronic surveillance). Substantively, we find that

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elected officials appear less extreme relative to their partisan counterparts in the public when the public's preferences are measured in the condition with the party split, suggesting current findings about "leapfrog representation" (Bafumi and Herron, 2010) may be partially an artifact of survey error. More broadly, our results provide experimental evidence that using standard roll call survey items to create estimates of citizen preferences comparable to legislator behavior does not address the fundamental problem of placing the two sets of choices in a common space.

In addition to providing insights for ongoing work about how best to measure citizen preferences and compare them to legislative behavior, this research also has implications for work on representation more generally. Survey data provide a useful metric for understanding citizen preferences if those responses are informative of those preferences. If, as we find, however, measured policy preferences are sensitive to context, it raises important doubts as to how seriously analysts should (and citizens do) treat those responses. We provide some preliminary ideas about ways to move forward in light of this finding in our concluding discussion.

Our results and the core of our design also point toward future research that can decompose the meaning and consequences of informational cues about policies. Exploring the influence of different types of information (e.g., group or leadership endorsements, substantive policy information) across policy areas and types of legislative action (e.g., procedural versus final passage votes) in an experimental context can help us understand when and under what conditions our tools of measurement provide estimates of citizen preferences that are more comparable to the legislators' policy space. That sort of work, in conjunction with the results presented in this paper, also speaks to research that uses statistical models such as item-response theory to scale respondent policy preferences into underlying spaces. While one advantage to these procedures is to aggregate individual measurement error, our findings suggest that measurement error is systematic. We explore item-response models briefly in the Appendix, but our design provides a framework to evaluate the influence of additional policy information on scaling procedures.

Assessing Representation Using Survey Roll Call Measures

Evaluating representation is a rich and diverse area of ongoing research in political science. While there are many different definitions of what constitutes "good" representation or how to measure public preferences, a large body of scholarship in recent decades has focused on evaluating the quality of representation empirically by assessing the correspondence between what elected officials do in office and what the public says they would like those elected official to do. Building on this work, we adopt a simple definition that allows us to make comparisons between a representative's behavior and a citizen's preferences: Does a representative behave in the manner that the voter would if she were granted the legislator's vote? Thus, we follow closely the example of Bafumi and Herron (2010) when "we characterize the extent to which there is congruence between the preferences of voters and the preferences of their corresponding members of Congress (p. 519)."[1]

The quantitative study of representation was invigorated by the rise of public opinion surveys. This technology allowed researchers the possibility of directly soliciting citizen policy preferences and comparing those measures to elite behaviors. Seminal in this line of work was Miller and Stokes (1963), which used survey data to compare district-level estimates of public opinion with

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[1] This definition sets aside issues of coordination, multidimensionality, and cycling, as well as alternative definitions of representation.

corresponding behavior of sitting members of Congress.

In response to criticism about existing survey measures of preferences being subject to a variety of sampling and measurement error (e.g., Achen, 1977, 1978; Clinton, 2006; Wright, Erikson, and McIver, 1985), scholars have subsequently embraced the idea of asking ordinary citizens how they would vote on specific roll call votes, what we call "survey roll call measures." The promise of such measures is transparent: Because survey respondents are now making the same binary choice as legislators, their responses are presumably informative of their preferences over outcomes in the same way as legislators' votes (Lax and Phillips, 2009; Broockman, 2016). To assess the nature of representation, scholars can then compare member votes on specific roll calls to survey respondent preferences elicited about those same roll calls or use a set of roll call votes in joint scaling procedures to provide more precise measures of citizen and legislator preferences in a common space (e.g., Bafumi and Herron, 2010; Clinton, 2006; Gerber and Lewis, 2004; Tausanovitch and Warshaw, 2013). Cumulatively, all of these methods aim to generate measures of citizen preferences comparable to member behavior to assess representation. In general, findings from these different approaches suggest measurable but imperfect congruence between what voters want and how representatives behave.

Approaches that use survey roll call measures to assess representation rely on a built in but often unstated assumption about measurement. If the survey response reflects how the citizen would vote in the legislature, the survey response measures the citizen's preferences in the policy space of the legislator. If this assumption does not hold, however, then the relevance of survey roll call measures for assessing the quality of representation is in question.

There are a number of reasons that the context a member of Congress faces when voting on a particular bill is different from the context faced by a survey respondent. Setting aside normal issues associated with survey respondents being inattentive (which would itself likely generate measurement error), perhaps the most salient difference between ordinary citizens and legislators is the information they have about the choice they are making. For legislators, their job is on the line when making public roll call votes (Mayhew, 1974). As such, they devote substantial attention and resources to understanding the policy choices they face and the larger political context associated with a yea or nay vote, anticipating how the public will react to that choice (Arnold, 1990). By contrast, ordinary survey respondents may not readily know the particular details of a piece of policy legislation (or procedural vote), the nature of status quo policy, or other strategic considerations that are on the line when voting (for example, how this vote affect subsequent votes on amendments or other bills, bargaining with the president, or elections). Particularly in the contemporary US Congress where many of these concerns are tightly tied to party coalitions and party conflict (e.g., Cox and McCubbins, 1993), survey respondents are unlikely to have access to partisan-relevant information that affects how members of Congress vote on those bills. This list of contextual differences is of course not exhaustive, but generally the incentives facing a member of Congress when voting on a bill are likely very different from those facing a survey respondent.

What remains uncertain is whether altering the context of the survey response to more closely resemble that of the legislature affects patterns of survey response and subsequent estimates of citizen and legislator preferences. If survey responses are largely unaffected by additional information, researchers are warranted in treating survey roll call measures as more meaningful measures of citizen policy preferences.[2]

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[2] Of course one cannot rule out the possibility that some additional informational intervention would affect mea-

Previous research finds that survey responses to political questions change when subjects are provided with cues or information that can substitute for their own lack of policy knowledge. These results are in keeping with the broader literature on survey design that shows that responses to survey questions vary considerably in their reliability, stability, and validity (e.g., Groves et al., 2009; Zaller and Feldman, 1992). At one extreme, for example, survey respondents are willing to express opinions on non-existent bills (Bishop, Tuchfarber, and Oldendick, 1986). The survey design literature generally recommends careful investigation of any survey item to evaluate its value for measuring underlying constructs. Because such empirical work has not been applied to survey roll call measures, however, and because the arguments we discuss above imply such measures are likely subject to substantial error, many of the inferences drawn to date from designs using these measures rest on untested assumptions.[3]

Suppose, for example, that survey respondents with less information were less likely to "vote" with their party because they did not understand the issue at stake, reducing the correlation between partisanship and measured preferences. This measurement error would have the consequence of making voters appear more centrist (less partisan) than the better-measured preferences of members of legislatures and create a disconnect in representation, exactly as much of the existing literature finds (Bafumi and Herron, 2010; Hill and Tausanovitch, 2015).

Survey responses in general have been shown to be particularly responsive to partisan cues. While this is sometimes taken as evidence that citizens lack true preferences and are instead beholden to elite persuasion (e.g., Zaller, 1992) – a finding that nonetheless calls into question the idea that preferences measured in the absence of those cues reflect how citizens want their elected representatives to behave – citizens also appear self-aware of their own lack of detailed knowledge. For example, survey respondents distinguish among issues, being less likely to reward or punish elected officials in policy domains where they acknowledge that they do not understand the complexity of mapping policy tools to policy outcomes (Gerber et al., 2011). Furthermore, when citizens have a sound informational basis for their expressed opinions, partisan cues appear to have modest effects on expressed citizen preferences (Bullock, 2011). A study related to ours presents evidence that informational interventions do change survey responses to roll call vote items (Lauderdale, 2013). Our study presents different non-partisan cues, considers confidence in policy knowledge as a moderator, breaks down results by party and roll call vote, and considers Supreme Court cases to provide further empirical evidence in support of our argument.

Research design and data collection

In order to assess the importance of the arguments raised in the previous sections, we fielded three survey experiments in which we varied the amount of information provided to respondents when answering survey roll call measures. These questions allow us to estimate citizen preferences obtained under different informational conditions. None of these conditions fully replicate what a legislator faces. But, if we find that information readily available to the legislator influences survey

sured preferences. Theory should guide what information is likely to affect citizens opinions.

[3] One indication of the challenges for this assumption is revealed in recent work by Jessee (2016) and Lewis and Tausanovitch (N.d.), who examine the performance of the joint scaling procedures that use multiple survey items and roll call votes to place legislators and survey respondents in the same policy space. Both find that the underlying structure generating legislative votes and survey responses is statistically distinct, but come to different conclusions about the continued usefulness of joint scaling. This suggests that the validity of survey roll call measures cannot be determined by statistical procedures alone.

responses, being exposed to the legislator's environment in whole seems likely to do so as well.

The key intervention in each experiment is similar. In our control groups, respondents answered standard survey roll call measures. In our treatment groups, respondents answer the same questions but with one of three kinds of additional information. The "party split" treatment provides the party breakdown of the vote in Congress on that piece of legislation. The intuition for this particular intervention is that it provides a highly salient cue that is almost certainly available to sitting members of Congress when deciding their own actions. Party splits therefore likely reflect both immediate policy differences between the parties, strategic considerations around compromise legislation, amendments, and status quo policy, and the larger political environment in which strategic elites perform before the electoral audience. The party split conveys a range of considerations that are likely relevant for citizens when forming their own opinions about a roll call vote as if they were sitting in Congress. In Appendix Figure A8, we provide evidence that party split information is likely novel to respondents by showing a very weak relationship across roll calls between actual party splits and respondent guesses about the split (in the control [no information] group).

A second treatment is a "chamber split" condition, which provides the breakdown of the vote for the full chamber (House or Senate) in which it was cast. This piece of information is less partisan than the party split treatment, instead providing information about how popular the bill was in the chamber as a whole. While participants may bring their own beliefs about the party breakdown to the intervention and attempt to infer party positions, this is a less precise partisan cue than the party split.

A third treatment provides a synopsis of the Congressional Budget Office (CBO) analysis of the bill voted upon. We simplified and shortened the CBO reports to provide paragraph-length analyses for participants to read. This piece of information should be almost entirely non-partisan, consistent with the mandate for the CBO.

The first study, fielded online with YouGov's Cooperative Congressional Election Study (CCES) surrounding the 2014 midterm election, asked respondents about eight roll call votes. The second study, fielded online with Survey Sampling International (SSI), expanded the project by asking respondents in 2016 for their opinions across 12 recent roll call votes. The first two studies randomized respondents into the party split or control conditions only. The third study, fielded online with Lucid, Inc. in late 2017 and early 2018, asked respondents for their opinions on 11 roll call votes along with the Supreme Court cases used in Malhotra and Jessee (2014). The third study randomized participants into all four conditions for legislative voting and to two conditions for judicial decisions. We detail the design and sample of each in Appendix Sections A, B, and C, and refer to them as Study 1 (CCES), Study 2 (SSI), and Study 3 (Lucid). For each sample, we use post-stratification weights for all analysis to improve representativeness. The Appendix presents an extensive set of robustness tests related to weighting and variable coding, and provides the text of all treatments.

Roll call question wording for each study is the measure fielded on the CCES Common Content, "Congress considered many important bills over the last few years. For each of the following tell us whether you support or oppose the legislation in principle."[4] Each piece of legislation was described using a short bill title followed by a brief description. For example, one item was:

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[4] Note that we did not design this question wording and so it does not line up exactly with our definition of representation. We use the standard wording to help our results speak to existing work using these items.

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> **US-Korea Free Trade**: Implements the United States-Korea Free Trade Agreement. (Emphasis in original)

Respondents could indicate either that they "supported" or "opposed" the bill.

In the party split condition, for each item we added the observed party split in the chamber:

> **US-Korea Free Trade**: Implements the United States-Korea Free Trade Agreement. <u>91%</u> of Republicans voted in favor of the bill, and <u>31%</u> of Democrats voted in favor of the bill. (Emphasis and underlining in original)

The chamber split condition displayed the phrase "Of members in the [House/Senate] voting on the bill, xx% voted in favor of the bill" instead of the breakdown by party. The CBO condition included a synopsis of the text from the CBO report on the bill without any information on the vote outcome.

For each study, we use a between-subjects design to identify the treatment effect of information (party split, chamber split, or CBO score) relative to subjects in the control condition. On the SSI study, we additionally asked respondents at the end of the survey about their confidence to evaluate policy across a set of policy areas that match to the roll call votes asked.

Results: Responses to Survey Roll Call Measures Affected by Party Split Information

In each of the three studies, respondent support for roll call votes cast in Congress is materially affected by the delivery of information about the party split. In particular, we focus on partisan respondents (including partisan "leaners") because it is for these respondents that partisan voting patterns may provide signals about (more informed) preferences on the vote. In nearly all cases, partisans' average responses move toward the observed party position in the legislature.

For all three studies, we present our analysis graphically with parallel statistical analysis in Appendix Tables A6, A7, and A8. In Figure 1, we present the effect of the party split intervention on support for each roll call vote from Study 1. For each bill (horizontal axis) reading left to right, we begin by plotting the proportion of Democrats (circles) and Republicans (squares) supporting the bill in the control condition. Next, connected to these points by solid lines are the proportions of Democrats and Republicans supporting the bill in the party split condition. Finally, the last pair of points is the proportion of Democrats and Republicans voting yea on the bill on the House floor, the information provided to respondents in the party split condition.

[Figure 1 about here]

For example, the leftmost frame shows that Democrats moved from supporting the Bipartisan Budget Bill of 2013 at a rate of 76 percent in the control condition to a rate of 79 percent in the party split condition, with 84 percent of Democratic members of the House voting yea. Republican respondents also move towards the rate of Republican members (73 percent yea), from 69 percent supporting in the control condition to 71 percent in the party split condition.

Two important patterns emerge. First, both Democrats and Republicans expressed support for each bill at rates closer to that observed in the House for their party when informed of the House vote for seven of eight items (88%). For Democrats, the exception is the Simpson-Bowles compromise budget, which was not supported at high rates by either party but was seen, according to some

commentators, as particularly anathema to Republicans because it would have raised taxes.[5] For Republicans, the exception is the bill to end the government shutdown and raise the debt ceiling in 2013, which was endorsed by the Republican leadership and all Democrats, but supported by only about 40% of Republican House members. After learning that the bill was supported at a much higher rate by Democrats than Republicans, Republicans become less supportive.

Second, responses in the party split condition show greater divergence in support between identifiers of the two parties. Apart from the Bipartisan Budget Bill of 2013 where actual partisan divergence in the legislature was low, roll call support diverges more when informed of the party split. Averaging across these eight bills, the average party split in the House is about 56 points. In the control condition, the average party split among survey respondents is 26 points. In the party split condition, this gap grows to 41 points, an increase of more than 60%.

Figure 2 presents parallel results for Study 2. All roll call votes in Study 2 are from the Senate, so the last pair of points shows the proportion of Senate Democrats and Senate Republicans voting yea. Here our analysis also excludes respondents who failed an attention screener.[6]

[Figure 2 about here]

The results shown in Figure 2 are consistent with those in Figure 1. Providing the vote split in the legislature changes expressed support for the bill in the public. Across the 24 party-vote observations in Figure 2, in 17 of them (71%) partisan respondents move toward the observed party vote in the treatment condition.

Additionally, as with Study 1, support in the party split condition diverges in the party split condition (this is true on average, and for 11 of the 12 individual bills). The average party split in the Senate is about 62 points. Among respondents, the average split moves from 28 points (control) to 40 points (party split), an increase of more than 40%.

Finally, Figure 3 presents support for roll calls in Study 3 from the control and party split conditions. Study 3 roll call votes come from both the House and the Senate. The patterns for Study 3 are consistent with the results from Studies 1 and 2.

[Figure 3 about here]

We selected the roll calls in Study 3 to cover easy issues such as abortion and hard issues such as the fiduciary rule, and also with varying magnitude of party split. One might think that on easy issues participants would have more crystalized opinions less likely influenced by party cues (Tesler, 2015). However, our results suggest that ease of issue is not strongly related to the party split treatment effect. We see notable movement on easier issues like gender discrimination, death tax repeal, and firearm background checks, and little movement on the hard issue of Puerto Rican debt. Appendix Table A8 does show that the largest point estimates obtain on bonus depreciation, presumably a hard issue, but we do not observe an obvious pattern. Instability is present even in high salience and easy issue domains, showing the concerns we identify with roll call items are not restricted to cases where votes in Congress are technically difficult or on novel issues.

Overall, the evidence from the three studies shows that survey roll call measures may understate the degree to which partisans support what their legislators are doing in Congress. When told how the parties in the chamber vote on an issue, expressed support for bills before Congress

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[5] See, for example, https://www.atr.org/conservative-movement-united-against-simpson-bowles-a6822 (retrieved July 12, 2016), summarizing the conservative opposition to the House vote.

[6] See Appendix Sections B and F for details on the screener and results including all respondents. We include only respondents who pass the screener to weed out survey respondents who are not engaged with the survey instrument and whose responses are therefore likely uninformative of the theoretical arguments we are interested in testing.

polarize by party. In the next section, we show that other, non-partisan information readily available to legislators can also change what survey respondents say about important legislation before Congress.

Results: Responses to Survey Roll Call Measures Affected by Non-Partisan Information

In this section, we present treatment effects from Study 3 for the chamber split and CBO conditions. For consistency with our earlier presentation, in each case we compare partisans' responses, by party, to their responses in the control condition.

We begin in Figure 4 by plotting the difference in partisan support between the control condition and the chamber split condition. While there is often movement towards the overall split, treatment effects are heterogeneous. This is consistent with the chamber split itself having multi-faceted meaning (for example, respondents may perceive bare majorities as indicating a bill that passed with Republican support over unified Democratic opposition, or as a bipartisan vote drawing support from both parties). Of the eleven bills, five have treatment effects with $p < .05$.

[Figure 4 about here]

In Figure 5, we present the treatment effects for the five bills that had a CBO score. The treatment text in each case – excluding the agricultural bill – presented the net deficit impact of the bill, in all cases increasing the deficit. The treatment decreases average support in each case, with an average effect of about eight points. Democrats become less supportive of the Bonus depreciation, Medicare/SCHIP, and Small Business Tax bills in the CBO condition, and Republicans become less supportive of Death Tax Repeal and Medicare/SCHIP. Of the five bills, two have treatment effects with $p < .05$

[Figure 5 about here]

Why does new information induce changes in expressed roll-call preferences?

Our main result is that citizen responses to survey roll call measures are unstable in the face of changes in available information. Citizens respond to information about how the parties voted in the legislature, the overall chamber split, and information from the Congressional Budget Office by changing their reported preferences on those same votes.

How should one interpret these patterns? Building on previous work (Gerber et al., 2011), we examine whether self-assessed citizen confidence to evaluate policy moderates the treatment effect of the party split. If how people respond to the informational interventions is correlated with lack of confidence to evaluate good policy in a particular policy domain, it implies that the response to this information arises partly because citizens look to heuristics in situations where they understand their own lack of knowledge. We note that this analysis looks for heterogeneity in the treatment effect by factors not subject to randomization.

We calculate measures of average confidence for each party for each roll call item from Study 2. These measures come from a battery at the end of the study:

> There are many different problems that government policies are designed to fix. For each of the following policy areas, we'd like to know how confident you are that you could distinguish good from bad policies. That is, how confident are you that you could evaluate policy in each area?

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Respondents could choose Not at all confident (scored 0), A little confident (1), Somewhat confident (2), or Very confident (3).

To avoid treatment affecting measured confidence, we calculate average confidence for each bill and party using responses only among respondents assigned to the control condition. We then assess whether the average effect of the vote split treatment (the absolute difference in support for each item in the party split condition compared to the control condition), is larger for survey roll call measures in policy domains where partisans are less confident. In this aggregate analysis we estimate:

$$\text{Absolute Treatment Effect}_{i,j} = \beta_0 + \beta_1 \times \text{Average Confidence}_{i,j} + \beta_3 \times \text{Republican}_j + \epsilon_{i,j}, \quad (1)$$

using OLS regression where i indexes each bill and j whether the subgroup is Republicans (versus Democrats). We have one observation for each of 22 roll call-cross-party groups.

Parameter estimates appear in Table 1. The dependent variable absolute treatment effect ranges from .004 to .28, with a mean of .09. The average confidence measure ranges from 1.19 to 1.84, with a mean of 1.47. The point estimate in column (1) shows that as confidence increases, the average treatment effect decreases. Subjects respond more to the vote split in those policy domains where self-assessed confidence is low. Partitioning by partisanship in columns (2) and (3) produces similar results, though with some suggestion of more moderation by confidence for Democrats. None of the estimates is individually statistically significant in this small sample.

[Table 1 about here]

Because this is observational analysis, one concern is that confidence might simply be a proxy for importance. For this reason, at the end of our survey we also asked respondents to gauge the *importance* of acting in each of the same policy areas for which we assessed confidence. Respondents could choose Not at all important (scored 0), Of little importance (1), Somewhat important (2), or One of the most important issues (3).[7] As before, we calculate average importance in the control group. Including this measure appear in columns (4) though (6), the coefficient on the confidence measure remains negative, although in the pooled estimate the coefficient shrinks by about 24% (B=-.084, s.e.=.10).

Cumulatively these results are uncertain with large sampling variability. But point estimates suggest that survey respondents are more responsive to the treatment in those domains where confidence to evaluate policy is lower. This result also helps to rule out the possibility that respondents simply abandon their own well-formed and deeply-held opinions to align themselves with their party.

Results: Survey Supreme Court Opinions Affected by Party Split Information

The logic of using equivalent votes by elites and members of the mass public to understand representation is not limited to studies of the US Congress. Recently, work has also sought to understand the correspondence between judicial behavior, specifically voting by US Supreme Court Judges, and citizen preferences by asking citizens to cast their votes on cases previously considered by the Court (Jessee and Malhotra, 2013; Malhotra and Jessee, 2014). In Appendix Section D, we show that expressed opinions by survey respondents on Supreme Court cases are affected by a

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[7] At the aggregate bill x party level, confidence and importance are correlated at $\rho = .41$ (p = .06). See Appendix Figure A7 and discussion.

treatment providing information about the vote split between judges appointed by Republican and Democratic presidents.[8] The opinion of each justice, along with their partisan background is readily available to other members on the court, but may not be available to survey respondents when they consider Supreme Court decisions. (Moreover, they may have no opinion about the relevant Constitutional issues at hand.) Supreme Court cases may be a conservative setting to find effects of information because many of these issues are social policy questions where citizens might hold strong opinions and many cases are well publicized. Nonetheless, we find that in 15 of 18 vote-party observations (83%), expressed support in the treatment condition is closer to the observed split for that party's justices. In sum, the measurement challenge we argue affects survey roll call measures seems to arise in the judicial realm as well.

Roll Call Survey Questions, Representation, and Polarization

In this section, we consider if the more-informed responses in the party split condition lead to different conclusions about the status of representation in the United States. We show that providing party split information lessens the estimated divergence between Congress and their constituents.

It is generally accepted that members of the House and Senate have polarized more than their constituents (e.g., McCarty, Poole, and Rosenthal, 2006; Hill and Tausanovitch, 2015). Almost all comparisons, however, rely on the survey responses of citizens without contextual or policy-specific information. If representatives do represent constituent interests and our argument about information holds merit, then the positions of citizens in the party split condition should more closely align with the positions of their representatives. The positions may not fully align, of course, because we have only delivered one of many pieces of information available to legislators.

We start by considering how often members and citizens vote with the party-plurality position on the 12 roll calls from Study 2.[9] Figure 6 presents distributions of the number of votes cast with the Senate Democratic position, defined as yea if the rate of yea votes among Democratic senators is greater than or equal to the rate of yea votes among Republicans on the bill, nay otherwise. Each line connects the proportion of senators, control respondents, and party split respondents, by party, for each category of votes with the Democratic position.

<div align="center">[Figure 6 about here]</div>

Providing citizens the party split makes rates of voting the party position look more like the rates of senators. More than 60 percent of Senate Republicans voted yea on 2 or 3 of these bills, with 20 percent voting yea on 0 or 1 (dotted density to the left). Most Republicans in the control condition support the Democratic position on 4 to 7 of these roll calls (solid density line), compared to the modal Republican in the party split condition (dash dot line) supporting the Democratic position on 2 to 3 bills. Democratic respondents in the party split condition share the same mode with Democratic Senators, supporting the Democratic position on 10 or 11 bills, while the modal Democrat in the control condition supports 8 or 9 bills. For both parties, the distribution of votes shifts away from the center in the party split condition.

Figure 6 and Appendix Section E confirm the longstanding pattern that polarization among

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[8] Because judges do not use party labels in their day to day deliberations, we provide the partisanship of the president who appointed each justice.

[9] In Appendix Section E, we scale the survey responses in Study 2 using item-response theory models separately by assigned condition (similar to the approach in Lauderdale, 2013). Although our statistical power is limited with only 12 items, we find that providing the party split cue moves the distribution of preferences among citizens towards the distribution of preferences of Senators.

citizens is notably lower than that observed among members of the Senate. However, providing the party split makes divergence between partisans larger and closer to that observed in the Senate. An open question is whether more information or a different context would further lessen the apparent disconnect between citizens and representatives.

Conclusion

We hypothesized that survey roll call measures may be limited in their value for measuring the quality of representation due to important problems of measurement. In particular, we argued that responses to those items may be poor proxies for citizen preferences because survey respondents lack sufficient information to express meaningful "votes."

Our experiments show that each of three informational interventions affects measured citizen preferences, suggesting that citizen survey responses in the absence of such information may not accurately measure what voters want their legislators to do. If that is the case, much of the work concluding failures of representation – including differential representation by income or other characteristics – rests on assumptions about valid measurement of citizen preferences. Moreover, such concerns do not appear limited to legislative behavior – we document similar instability in measures of citizen preferences on Supreme Court decisions.

Our design provides one path forward: if the representational benchmark is described as how the citizen would vote if she were acting as the legislator (or judge), we can develop new instruments that enrich the survey context to make it more similar to that of voting in the legislature. While it is impossible in the current Constitutional system to fully replicate the hypothetical experiment of citizens voting in a legislature, our theoretical approach can be used by scholars to justify comparisons of survey responses to representative behavior.[10] We advocate that future work take more seriously issues of measurement.

As practical advice, if scholars use roll call and similar items to measure citizen preferences, they should embed in their studies tests of how sensitive their measures are to informational interventions such as those we examine here. Stability in measured preferences in the face of different information would help demonstrate the desirable construct validity, stability, and reliability emphasized in the survey design literature. Such work may also be a productive path towards understanding the systematic sources of measurement error in survey responses. One important question for future research is how the single pieces of information we delivered in these experiments compare to combinations of information.

This research also has important implications for those undertaking joint scaling. Joint scaling requires similar item parameters to map policy ideology from one arena to another. Our evidence here challenges this assumption for survey roll call measures and roll call votes in Congress. We argue that a key distinguishing feature of citizens is less information than legislators. It may be that bridging across elite institutions (e.g., bureaucrats to courts or courts to executives) is more appropriate given greater information for those actors. On the other hand, even with similar levels of information, it is possible the votes in the different arenas have sufficiently different meanings or strategic contexts that joint scaling of elites suffers from systematic measurement error similar to that we identify here.

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[10] Voting on ballot propositions may provide similar leverage, although it is notable that many citizens abstain, rely on party or other informational cues to substitute for lack of substantive knowledge, or simply vote no, implying that even those votes are made with self-awareness of lack of policy knowledge. Institutions such as Ireland's 2012 Convention on the Constitution, which randomly selected citizens to serve, may also provide some leverage.

Moving forward, we argue that more empirical work with a cautious eye towards issues of measurement is needed to evaluate how well democratic representation functions. Accepting citizens' responses to survey questions as valid measures of how they prefer their members act relies on important assumptions that appear in question, making current conclusions about representation derived from those measures less certain. Future work might investigate alternative informational interventions, combinations of information, or explore qualitatively or experimentally what voters say or reveal by their behavior that they want from their representatives.

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Table 1: Policy confidence attenuates treatment effect of information, Study 2

VARIABLES	(1) All	(2) Dems	(3) Reps	(4) All	(5) Dems	(6) Reps
Average confidence in control condition by party	-0.11	-0.13	-0.030	-0.084	-0.12	-0.044
	(0.09)	(0.12)	(0.16)	(0.10)	(0.15)	(0.16)
Average importance in control condition by party				-0.038	-0.0061	-0.063
				(0.05)	(0.09)	(0.06)
Republican respondent	-0.017			-0.021		
	(0.03)			(0.03)		
Constant	0.26	0.29	0.14	0.31	0.30	0.29
	(0.14)	(0.19)	(0.24)	(0.16)	(0.22)	(0.27)
Observations	22	11	11	22	11	11
R-squared	0.072	0.106	0.004	0.101	0.107	0.135

Standard errors in parentheses
** p<0.01, * p<0.05

Note: OLS coefficients. Dependent variable is absolute value of treatment effect of providing party split on roll call support by party and bill.

Figure 1: Support for roll call with and without party split information, Study 1
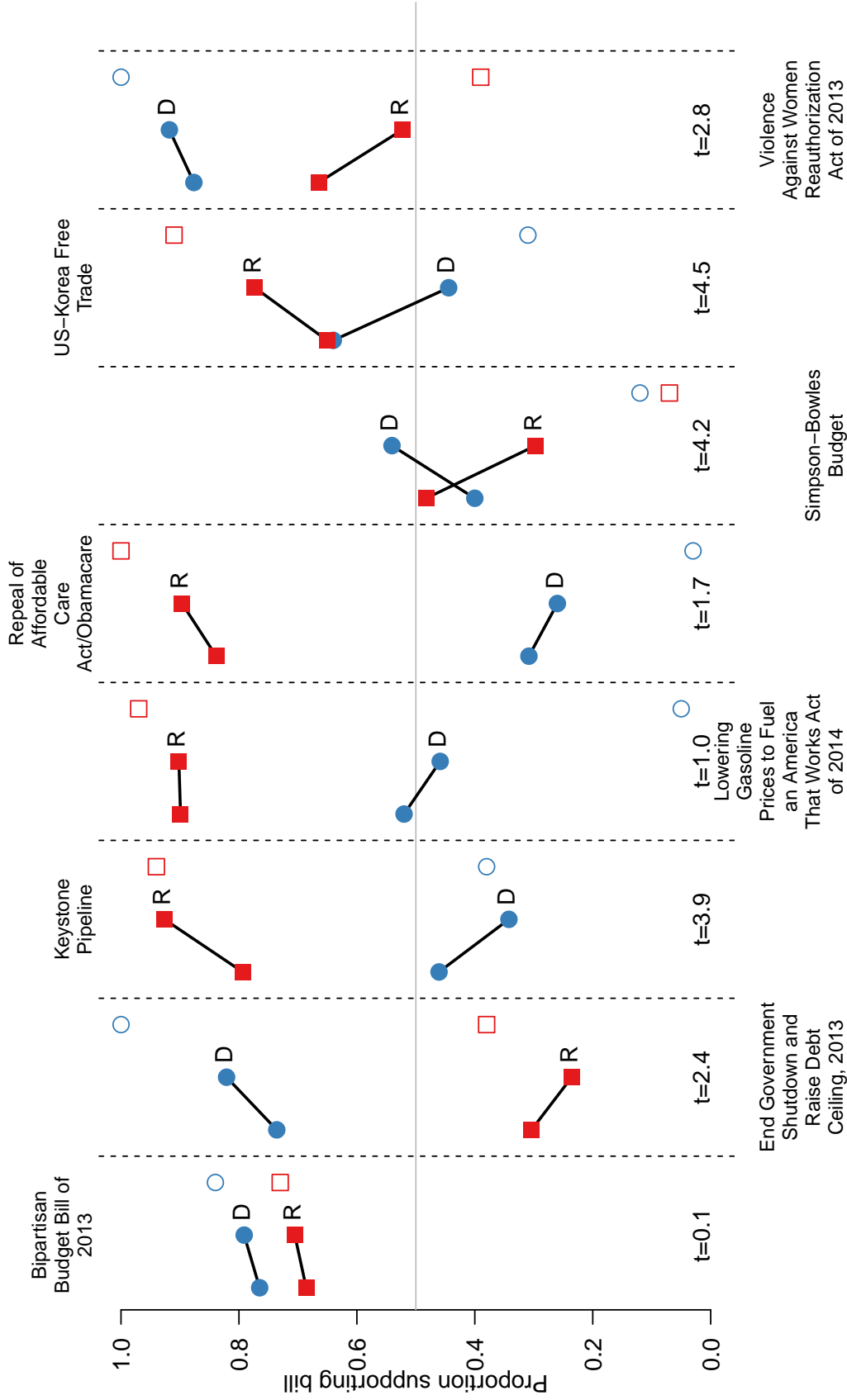
Note: *Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among Democratic (Republican) members of the House. Absolute value of t-ratio from difference-in-difference estimate of party-times-treatment indicated at x-axis.*

Figure 2: Support for roll call with and without party split information, Study 2
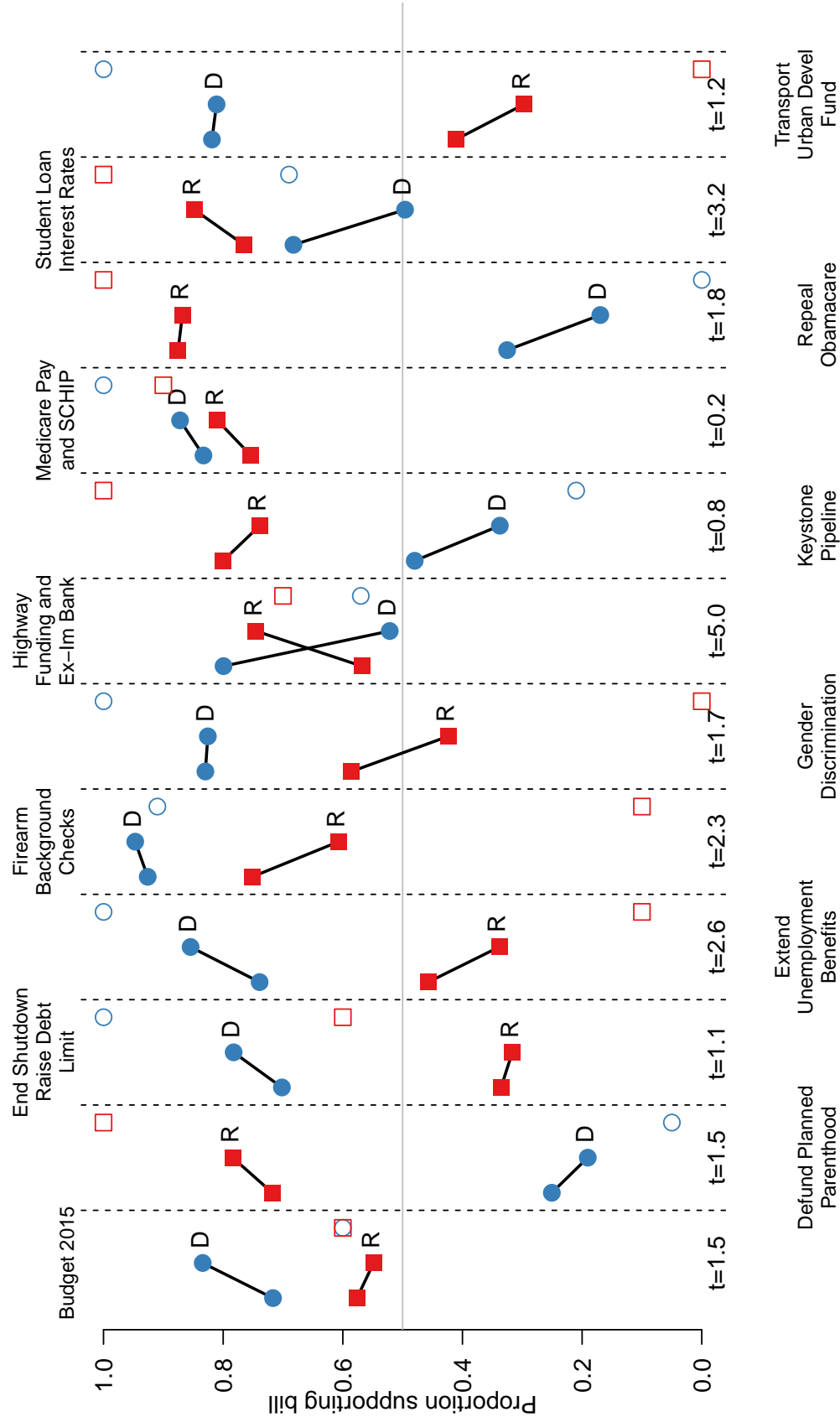
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among Democratic (Republican) members of the Senate. Absolute value of t-ratio from difference-in-difference estimate of party-times-treatment indicated at x-axis.

Figure 3: Support for roll call with and without party split information, Study 3
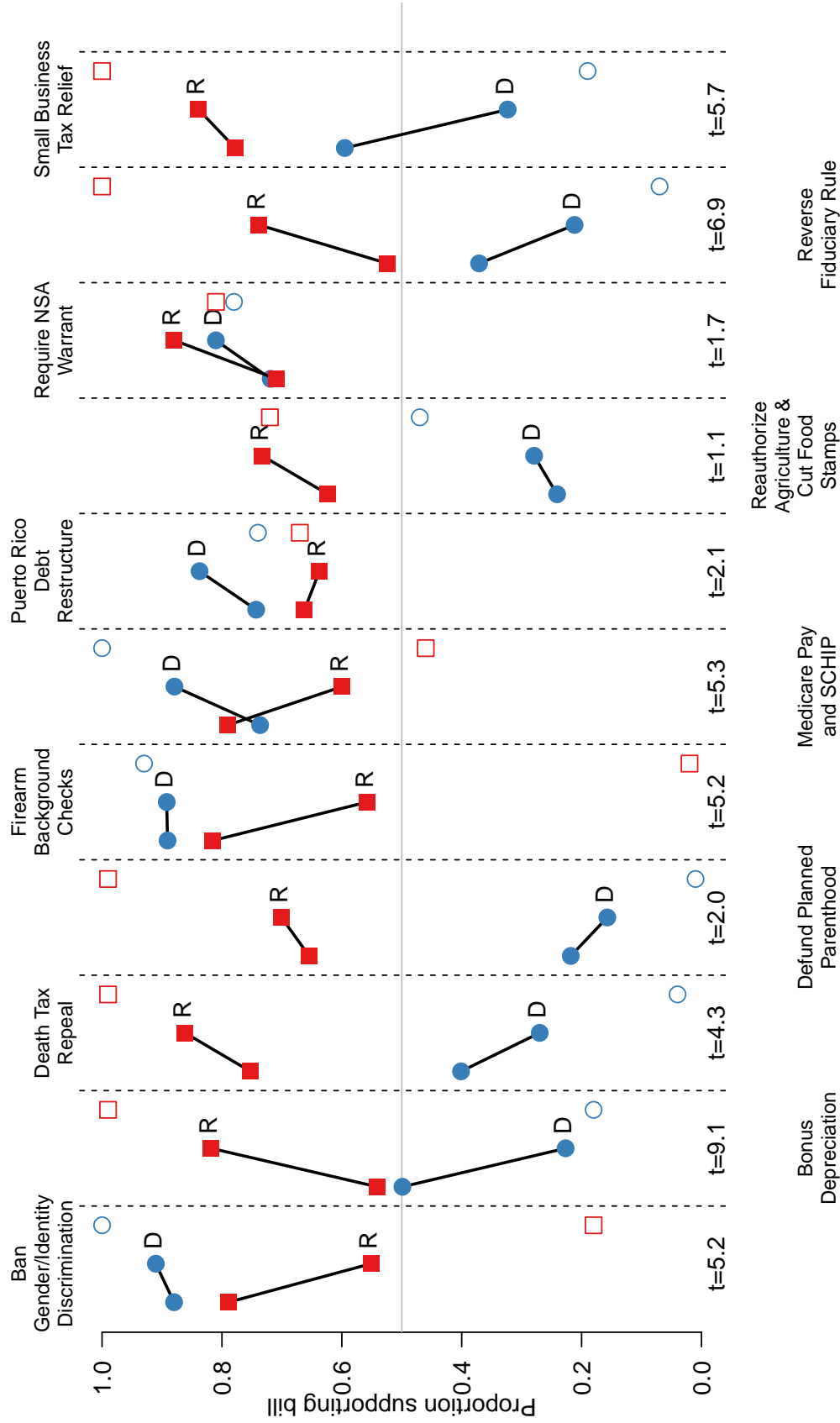
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among Democratic (Republican) members in the chamber. Absolute value of t-ratio from difference-in-difference estimate of party-times-treatment indicated at x-axis.

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Figure 4: Support for roll call from control to Chamber Split condition, Study 3
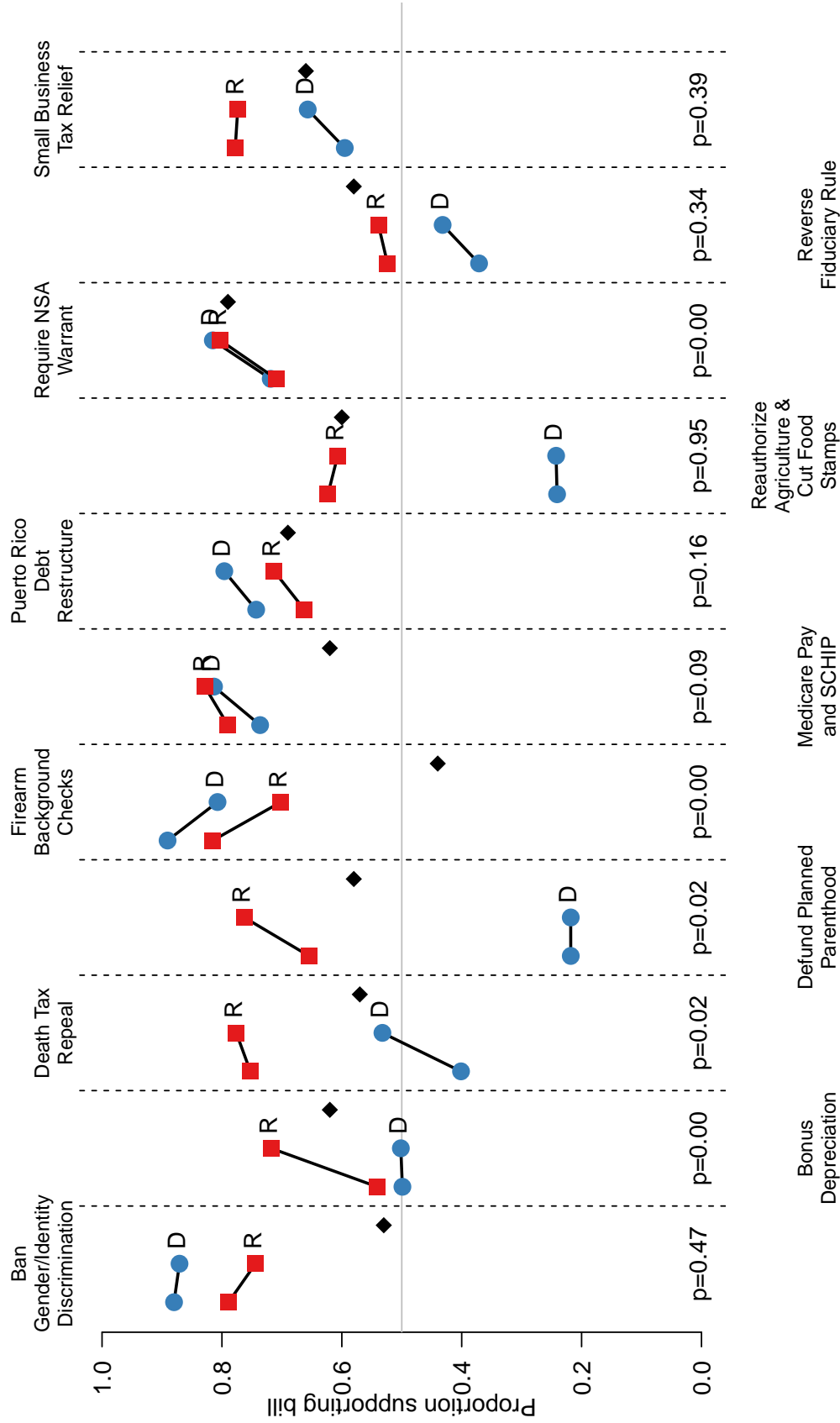
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the chamber split condition (right). Diamonds are the actual rate of support in the chamber. P-value for an F-test on the treatment and treatment times party regression coefficients to evaluate significance of treatment indicated at x-axis

Figure 5: Support for roll call from control to CBO condition, Study 3
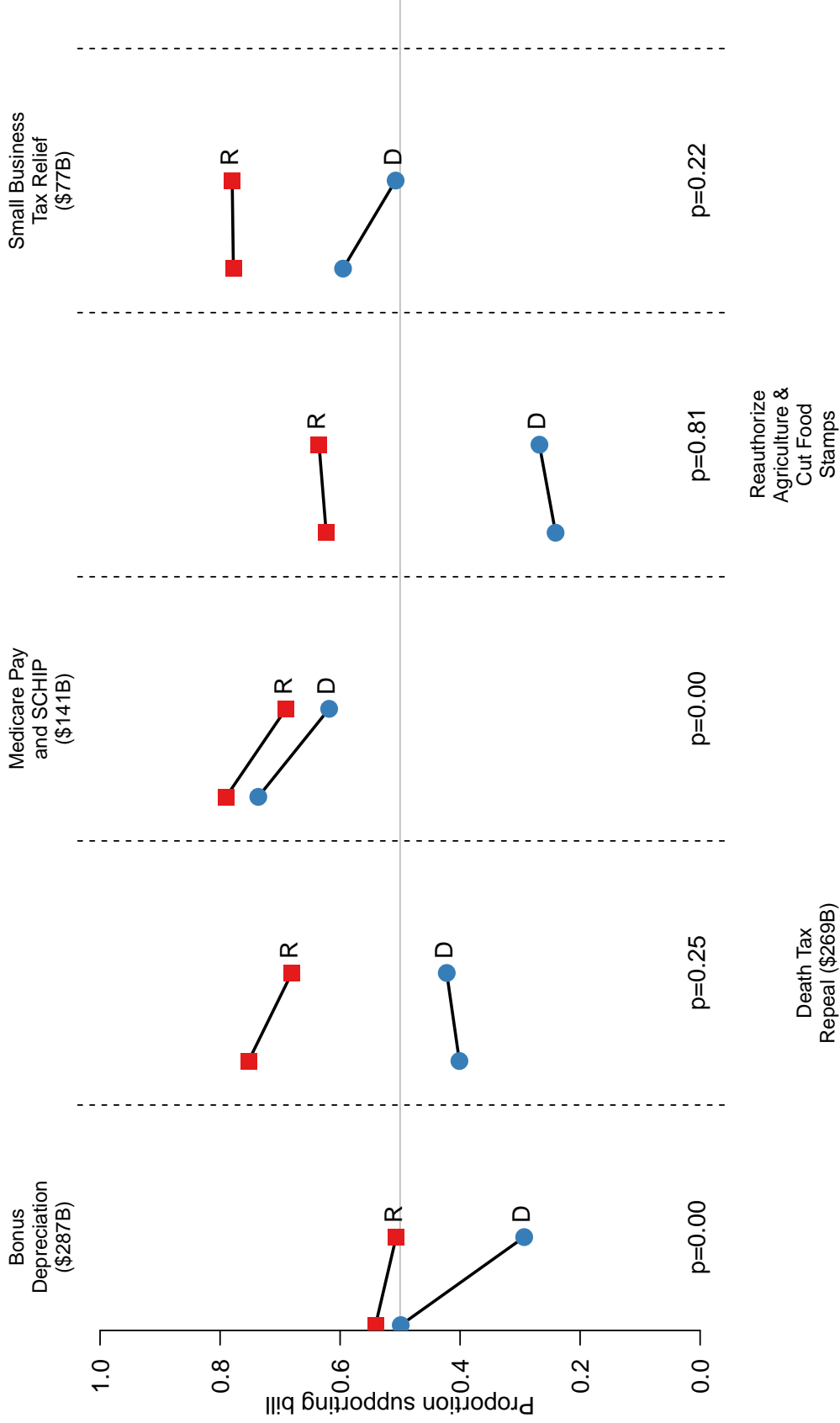
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the CBO condition (right). CBO condition presents respondents with a synopsis of the Congressional Budget Office analysis of the legislation. Number in parenthesis is the deficit impact calculated by the CBO. There was no deficit impact presented in the analysis to the Agricultural authorization. Absolute value of t-ratio from difference-in-difference estimate of party-times-treatment indicated at x-axis.

Figure 6: Votes with Democratic position in Senate by condition and party, Study 2
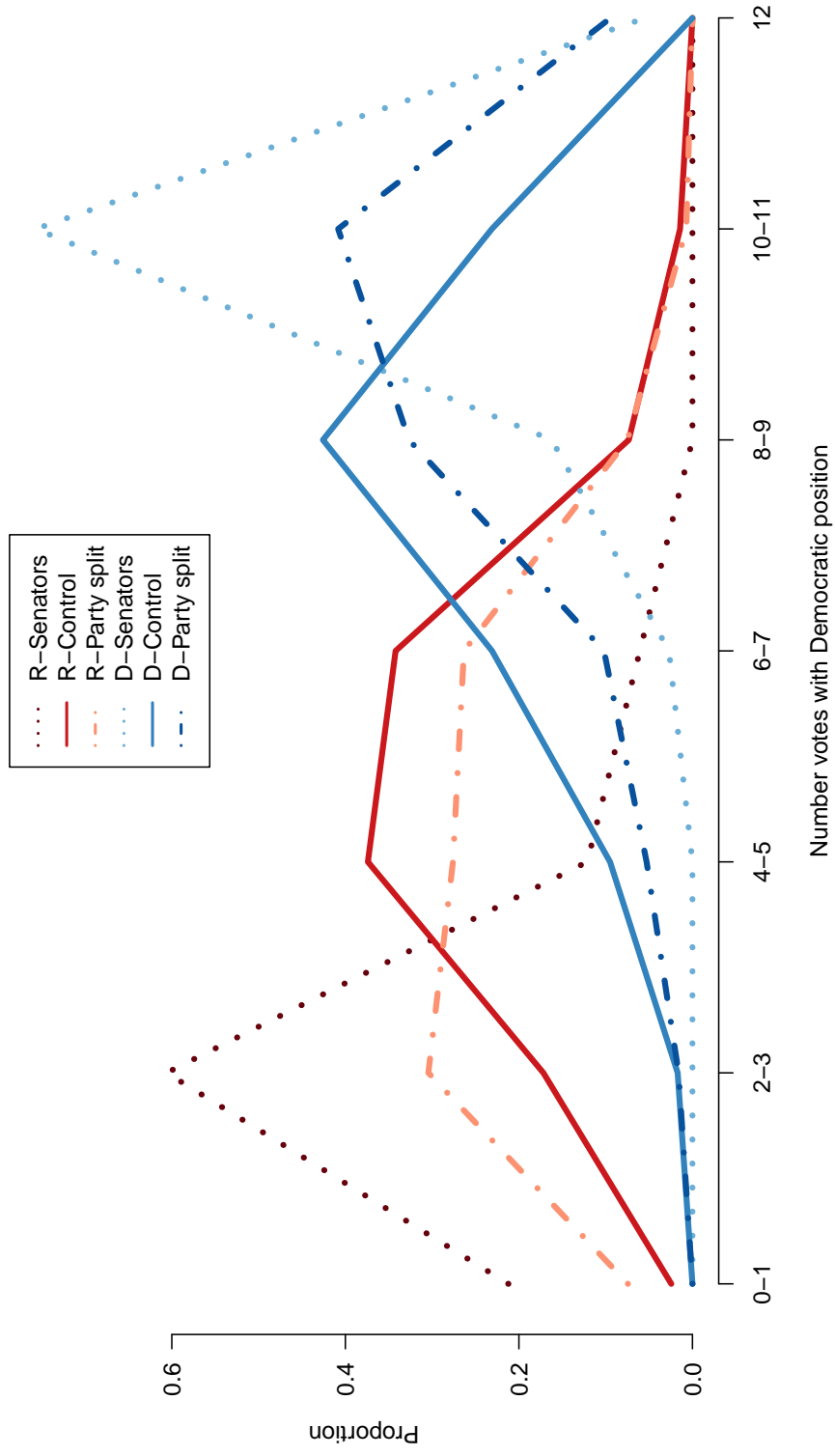
Note: Each line is the distribution across number of votes with the Democrats on the 12 roll call votes in Study 2.

Online Appendix
On The Meaning of Survey Reports of Roll Call "Votes"

American Journal of Political Science

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Contents

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A   Study 1: 2014 Cooperative Congressional Election Study

Our survey experiment for Study 1 was embedded in 3 team modules fielded on the 2014 CCES. Respondents were asked whether they supported or opposed a randomly selected subset of 8 different roll call votes, listed in Table A1. Congress had voted on each in a recent session. We selected these items to follow the set of roll calls survey items from the CCES common content and to vary in both their subject matter and the degree to which voting in the House divided the parties. To assist with summarizing the bill for participants, we sent our bill summaries to six colleagues who are experts in congressional politics. We are grateful to these colleagues for helping us clarify the language of the items.

Respondents were first asked whether or not they supported two of these items, selected at random, using the standard CCES common-content question wording, "Congress considered many important bills over the last few years. For each of the following tell us whether you support or oppose the legislation in principle."[1] Each piece of legislation was described using a short bill title followed by a brief description. For example, one item was:

> **US-Korea Free Trade**: Implements the United States-Korea Free Trade Agreement. (Emphasis in original)

Respondents could indicate either that they "supported" or "opposed" the bill. Respondents were "soft-forced" to choose one of these options.

One-third of respondents, selected at random, were then asked how they would have voted on four additional items, selected at random from those items not chosen for the control items. The question prompt and response options were the same. However, for each item we added a brief summary of the observed pattern of partisan voting in the House. Thus, the US-Korea Free Trade item shown above would have instead appeared as follows:

> **US-Korea Free Trade**: Implements the United States-Korea Free Trade Agreement. 91% of Republicans voted in favor of the bill, and 31% of Democrats voted in favor of the bill. (Emphasis and underlining in original)

Our total sample includes 3,456 respondents. 2,300 individuals were assigned to receive only the two control items, and the additional 1,156 individuals received both the two control measures and four of the party split items. All CCES analysis uses the provided post-stratification weights and is restricted to those respondents who answered all of their assigned roll call items. Of those assigned to two items, 96.1% answered both and 2.4% answered one. Of those assigned to six items, 93% answered all 6, 4.2% answered 5 items, and the remaining 2.9% answered 4 or fewer items. Patterns of non-response do not differ consistently by policy area across the two conditions. Average rates of non-response are 2.6% for the control survey items and 2.2% for the party split survey items.

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[1] Note that we did not design this question wording and so it does not line up exactly with our definition of representation. We use the standard wording to help our results speak to existing work using these items.

Table A1: Eight roll call votes used in Study 1

Bill Title	Long Description	Roll Call #	Democrat margin (Y-N%)	Republican margin (Y-N%)
Repeal of Affordable Care Act / Obamacare	Repeals the Patient Protection and Affordable Care Act and health care-related provisions in the Health Care and Education Reconciliation Act of 2010.	460 (July 11, 2012)	3-97	100-0
US-Korea Free Trade	Implements the United States-Korea Free Trade Agreement.	783 (October 12, 2011)	31-69	91-9
Simpson-Bowles Budget	Adopt budget proposal endorsed by the Simpson-Bowles Commission.	145 (March 28, 2012)	12-88	7-93
Keystone Pipeline	Extends Federal aid for highways and requires the Federal Energy Regulatory Commission to approve the Keystone Pipeline within 30 days.	170 (April 18, 2012)	38-62	94-6
Lowering Gasoline Prices to Fuel an America That Works Act of 2014	Implements a new program to lease space off of America's coasts to drill for new oil and gas resources.	368 (June 26, 2014)	5-95	97-3
Bipartisan Budget Bill of 2013	Implements a budget compromise to reduce some mandatory spending cuts and funds the federal government for fiscal years 2013 and 2014.	640 (December 12, 2013)	84-16	73-27
Violence Against Women Reauthorization Act of 2013	Provides $1.6 billion toward investigation and prosecution of violent crimes against women and establishes the Office on Violence Against Women within the Department of Justice.	47 (February 28, 2013)	100-0	39-61
End Government Shutdown and Raise Debt Ceiling, 2013	Ends the government shutdown that began October 2013 and increases the federal debt limit.	550 (October 16, 2013)	100-0	38-62

3

B Study 2: 2016 Survey Sampling International

Study 2 replicates and extends the Study 1 design. All roll call votes selected for Study 2 were cast in the 113th or 114th Senates and are detailed in Table A2. We selected these roll call votes from all final passage votes to vary on topic as well as party splits in the Senate.[2] Additionally, we included a longer (post-treatment) battery on policy importance and confidence to evaluate policy by issue area.

Study 2 was a simple between-subject design. Subjects were assigned at random either to the party split or control condition, and all 12 of their roll call questions were of that type.[3] We fielded the survey through Survey Sampling International (SSI), a firm that maintains an online panel whose demographics approximate a nationally representative sample. Our sample includes 1,464 respondents who participated in May and June of 2016. Although the sample is approximately representative of the American population, we found some demographic and political variables did not match population targets very well, and so constructed post-stratification weights that make the SSI sample approximate the Pew Research Center 2015 Governance Survey, a random-digit dial telephone sample of about 6,000 fielded in September 2015. All analysis uses these weights. We detail the weighting procedure in Section D.

The question wording for the policy importance item was

> Now, for this same list of policy areas, we'd like to know how important it is to you what government does in that area. Compared to all other policy areas (not just the ones listed below), how important is government policy in this area to you?

We exclude from our analysis 347 respondents who failed an attention screener in the middle of the survey because these subjects appear less engaged and are therefore unlikely to provide meaningful responses, yielding a final sample of 1,117 SSI participants. The 12 roll calls were asked in random order across three screens each with four items. After the first screen of four items but before the second, we screened for attention by asking the respondents which of four roll calls they had just given their opinion about. Only one of the four had actually been asked on the first screen, and we use only those respondents who identified this roll call correctly. This sort of screen likely includes some respondent who simply guessed which of the four was the right answer.

C Study 3: 2017-8 Lucid, Inc.

Study 3 replicates and extends the Study 1 and 2 design. Roll call votes selected for Study 3 come from the 113th or 114th House or Senate and are detailed in Table A3. We selected roll call votes with the goal of variation on easy versus hard (salient versus less salient) topics, partisan versus partisan splits, and availability of CBO analysis of the legislation. We identified the roll calls by considering on these dimensions the key votes identified by the Congressional Quarterly Almanac, the American Conservative Union, the Americans for Democratic Action, and the database of the

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[2]To assist with summarizing the bill for participants, we sent our bill summaries to six colleagues who are experts in congressional politics. We are grateful to these colleagues for helping us clarify the language of the items.

[3] One advantage of this design over Study 1 is that treatment condition is therefore uncorrelated with response order.

Table A2: Twelve roll call votes used in Study 2

Bill Title	Long Description	Roll Call	Democrat margin (Y-N%)	Republican margin (Y-N%)
Expand Existing Background Checks for Firearm Sales	Require federal background checks for gun sales that take place at gun shows or via the internet, the same requirement that exists for sales from regular brick and mortar gun stores.	97 (April 17, 2013)	91-9	9-91
Set Federal Student Loan Interest Rates	Set federal student loan interest rates, raising rates relative to recent rates but decreasing them compared to the rates that were in force because an old law had expired.	185 (July 24, 2013)	69-31	98-2
Allow a Vote on Funding Transportation and Urban Development	Support a motion to end debate and allow a final vote on a bill that would fund at a level of $54 billion for one year Transportation and Housing and Urban Development.	199 (August 01, 2013)	100-0	2-98
End Government Shutdown and Raise Government Debt Limit	End the government shutdown of October 2013 by funding the government for three months and also allowing it to borrow money.	219 (October 16, 2013)	100-0	60-40
Extend Federal Unemployment Benefits	Extend existing federal unemployment benefits for a minimum of an additional 5 months.	392 (April 07, 2014)	100-0	14-86
Allow a Vote on Changing the Standard for Determining Gender Discrimination in the Workplace	Support a motion to end debate and allow a final vote on a bill that would require employers to show that any wage gaps between men and women with similar jobs and qualifications have a business justification.	553 (September 15, 2014)	100-0	0-100
Approve 2015 Budget and Fund Government for 2015	Agree to a measure that would fund almost all federal government agencies for fiscal year 2015.	645 (December 13, 2014)	60-40	57-43
Approve Keystone XL Pipeline	Allow TransCanada to construct the 1,179-mile Keystone XL pipeline that would carry oil from Canada's tar sands to refineries in Texas.	49 (January 29, 2015)	21-79	100-0
Revise Medicare Physician Payment Rates and Reauthorize Child Health Insurance Program	Change the rules used to calculate physician payments so that doctors who see Medicare patients did not experience large drops in the amount the government paid them for providing care and fund for two years the program that provides free or low-cost insurance for low-income children and families.	144 (April 14, 2015)	100-0	85-15
Pass the FAST Act and Extend the Export-Import Bank	Authorize 6 years of federal spending on highways and other transit programs and extend programs to use federal funds to finance and insure foreign purchases of American goods.	260 (July 30, 2015)	57-43	72-28
Allow a Vote on Banning Federal Funding for Planned Parenthood	Support a motion to end debate and allow a final vote on a bill that would prevent any federal money from going to Planned Parenthood.	262 (August 03, 2015)	5-95	96-4
Repeal ObamaCare	Repeal the Affordable Care Act health care program by removing the federal health insurance requirement, eliminating associated taxes, and eliminating federal subsidies for low-income individuals to purchase insurance. Also bans federal funding of Planned Parenthood for one year.	329 (December 03, 2015)	0-100	96-4

Congressional Budget Office, ultimately choosing the 11 roll calls below. We retained the post-treatment battery on perceptions of policy confidence by issue area from Study 2, though dropped policy importance for reasons of space.

Study 3 was also a between-subject design. Roll calls were separated into two blocks, the five roll calls with a CBO score and the six roll calls without. Subjects were assigned to the same informational intervention in both blocks (control, party split, or chamber split), unless they were assigned to the CBO intervention in the first block, in which case they were assigned at random to one of the other three interventions for the second six roll calls. We fielded the survey through Lucid, a firm that partners with a network of companies that maintain relationships with research participants by engaging them with research opportunities. Lucid technology matches researchers and participants based on the researchers' desired audience, and delivered to us a sample whose demographics approximate a nationally representative sample. Our sample includes 4,524 respondents who participated in December of 2017 and January of 2018. Although the sample is approximately representative of the American population, we found unrepresentativeness on income and education (too low). After dropping 477 respondents who took the survey in less than 8 minutes too quickly to have been paying attention, we constructed post-stratification weights to the American Community Survey raked to margins of 24 categories of household income and 35 categories of age crossed with education. All analysis uses these weights, although they do not change point estimates in any substantive way. We detail the weighting procedure in Appendix Section D.

D   Post-Stratification weight construction

All CCES analysis uses the provided post-stratification weights. We constructed post-stratification weights that make the SSI sample approximate the Pew Research Center 2015 Governance Survey, a random-digit dial telephone survey. For the Lucid sample, we constructed post-stratification weights to the 2016-17 American Community Survey raked to margins of 24 categories of household income and 35 categories of age crossed with education.

Study 2: Weighting to Pew Governance Survey

To construct weights to make the SSI sample look like the sample to the Pew Governance Survey, we asked six questions of the SSI sample equivalent to those asked of the Pew sample. We use these six variables (age, gender, state of residence, level of education, 7-point party identification, and 5-point self-reported ideology) with the `rake` function from the R library `survey` (R Development Core Team, 2015; Lumley, 2011) to construct post-stratification weights. The Pew survey itself has post-stratification weights to Census targets, which we use to construct the target distribution for our weighting. We trim the resulting weights to range from 1/8 to 8 to limit variance. The case with the largest pre-trimmed weight was a 55-64 year old male from New Jersey with a high school degree who reported being a conservative Republican. The case with the smallest pre-trimmed weight was a 55-64 year old female from Vermont with a postgraduate degree and a very liberal Democrat.

Study 3: Weighting to American Community Survey

To construct weights to make the Lucid sample look like the population totals from the U.S. Census American Community Survey, we use the `rake` function from the R library `survey` (R Development Core Team, 2015; Lumley, 2011) to construct post-stratification weights. The ACS provided us national distributions for household income and age crossed with education, which were sim-

Table A3: Eleven roll call votes used in Study 3

Combined title	Chamber	Democrat support (Yea %)	Republican support (Yea%)	CBO?
Require NSA Warrant: Amend the Patriot Act to require the National Security Agency to obtain a warrant in order to gain access to and hold records of domestic phone calls made by Americans.	House	78	81	No
Puerto Rico Debt Restructure: Establish a financial oversight board for Puerto Rico that would have authority to initiate a proceeding for restructuring the islands debts.	Senate	74	67	No
Defund Planned Parenthood: Withhold all federal funding from Planned Parenthood for one year unless it ceases to perform abortions except in the case of rape, incest, or if a mother's life is in danger.	House	1	99	No
Extend Background Checks to Gun Shows: Allow a vote on an amendment to a criminal justice bill that would extend criminal background checks for all gun sales, including sales by gun dealers that they conduct at gun shows.	Senate	93	2	No
Ban Discrimination by Federal Contractors: An amendment to a spending bill that would bar federal contractors from discriminating against employees based on gender or sexual identity.	House	100	18	No
Reverse Rule Requiring Fiduciary Financial Advisors: The Labor Department unveiled a rule in April 2016 requiring broker-dealers to act in the best interests of clients when giving retirement investment advice. This bill would reverse that rule due to concerns the rule would limit consumer choice and increase the cost of financial advice.	Senate	7	100	No
American Small Business Tax Relief Act: The bill makes permanent a variety of tax breaks including a $500,000 allowance for the expensing of depreciable business property and the $2 million threshold after which the amount of such allowance is reduced, and indexes both to increase with inflation.	House	19	100	Yes
Death Tax Repeal Act: Repeals the estate and generation-skipping transfer taxes for estates of decedents dying or for transfers made on or after the enactment date. Revises gift tax rates to lower the top rate to 35% and raises the lifetime exemption for gifts to $5 million, which will be indexed to increase with inflation.	House	4	99	Yes
Reauthorize Agricultural Programs and Cut Food Stamps: Authorizes federal agricultural programs and cuts SNAP (Food Stamp Nutrition Program) program. Also modifies various crop support policies such as direct payments, counter-cyclical payments, and average crop revenue election.	House	47	72	Yes
Bonus Depreciation Amendment: Makes permanent a variety of tax provisions, including bonus depreciation for qualified property. Increases by $8,000 the maximum allowable depreciation deduction for a passenger automobile. Makes permanent the election to increase the alternative minimum tax (AMT) credit limitation in lieu of bonus depreciation.	House	18	99	Yes
Revise Medicare Physician Payment Rates and Reauthorize Child Health Insurance Program: Change the rules used to calculate physician payments so that doctors who see Medicare patients did not experience large drops in the amount the government paid them for providing care and fund for two years the program that provides free or low-cost insurance for low-income children and families.	Senate	100	46	Yes

Table A4: CBO votes and text, Study 3

Bill	CBO text
American Small Business Tax Relief Act	The nonpartisan Congressional Budget Office said about the bill: [E]nacting H.R. 636 would reduce revenues, thus increasing federal deficits, by about $77 billion over the 2015-2025 period.
Death Tax Repeal Act	The nonpartisan Congressional Budget Office said about the bill: [E]nacting H.R. 1105 would reduce revenues, thus increasing federal deficits, by about $269 billion over the 2015-2025 period.
Reauthorize Agricultural Programs and Cut Food Stamps	The nonpartisan Congressional Budget Office said about the bill: CBO estimates that direct spending stemming from the programs authorized by the conference agreement would total $956 billion over the 2014-2023 period, of which $756 billion would be for nutrition programs.
Bonus Depreciation Amendment	The nonpartisan Congressional Budget Office said about the bill: [E]nacting H.R. 4718 would reduce revenues, thus increasing federal budget deficits, by about $287 billion over the 2014-2024 period.
Revise Medicare Physician Payment Rates and Reauthorize Child Health Insurance Program	The nonpartisan Congressional Budget Office said about the bill: Over the 2015-2025 period, CBO estimates, enacting H.R. 2 would increase both direct spending (by about $145 billion) and revenues (by about $4 billion), resulting in a $141 billion increase in federal budget deficits.

Table A5: Supreme Court votes and text, Study 3

Should the government be allowed to restrict corporations' contributions to political campaigns, despite the First Amendment? Yes means allowing restrictions on political contributions by corporations. Among justices appointed by Democratic presidents, the vote was 3 Yes to 0 No. Among justices appointed by Republican presidents, the vote was 1 Yes to 5 No.
Should the federal government be allowed to involuntarily place sex offenders in mental institutions after their prison sentences have ended? Yes means allowing Congress to pass laws that place sex offenders in mental institutions after they have served their prison sentences. Among justices appointed by Democratic presidents, the vote was 3 Yes to 0 No. Among justices appointed by Republican presidents, the vote was 4 Yes to 2 No.
Should state and local governments be allowed to outlaw the possession of handguns, despite the Second Amendment? Yes means allowing states and localities to restrict handgun ownership. Among justices appointed by Democratic presidents, the vote was 3 Yes to 0 No. Among justices appointed by Republican presidents, the vote was 1 Yes to 5 No.
Should the government be allowed to permit private groups to place religious symbols on government-owned land, despite the First Amendment's language about separation of church and state? Yes means allowing the government to approve private groups placing religious symbols on government-owned land. Among justices appointed by Democratic presidents, the vote was 0 Yes to 3 No. Among justices appointed by Republican presidents, the vote was 5 Yes to 1 No.
Should a city be allowed to try to increase racial diversity by denying the promotion of government employees who passed a promotion test because no black employees passed the test, despite the Civil Rights Act of 1964? Yes means allowing denying promotions to those who passed a test if no black employees passed the test. Among justices appointed by Democratic presidents, the vote was 2 Yes to 0 No. Among justices appointed by Republican presidents, the vote was 2 Yes to 5 No.
Should states be allowed to require voters to provide photo identification at the polling place, despite the fact that it might disenfranchise certain individuals without government issued ID? Yes means allowing states to pass voter ID laws. Among justices appointed by Democratic presidents, the vote was 0 Yes to 2 No. Among justices appointed by Republican presidents, the vote was 6 Yes to 1 No.
Should the government be allowed to use lethal injection to execute convicted criminals on death row, despite the Eighth Amendment prohibitions against cruel and unusual punishment? Yes means allowing the use of lethal injection. Among justices appointed by Democratic presidents, the vote was 1 Yes to 1 No. Among justices appointed by Republican presidents, the vote was 6 Yes to 1 No.
Should the government be allowed to ban a specific abortion procedure, 'partial birth abortion,' without an exception to protect a woman's health? Yes means allowing the banning of partial birth abortions. Among justices appointed by Democratic presidents, the vote was 0 Yes to 2 No. Among justices appointed by Republican presidents, the vote was 5 Yes to 2 No.
Should the President, without Congressional approval, have the right to set up military commissions to try enemy combatants without judicial review, despite the Uniform Code of Military Justice and the Geneva Convention? Yes means allowing the president to set up military commissions. Among justices appointed by Democratic presidents, the vote was 0 Yes to 2 No. Among justices appointed by Republican presidents, the vote was 3 Yes to 3 No.

Note: In control condition, final two sentences presenting justice party split was not displayed.

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ilarly measured by Lucid. We rake to those targets and trim the resulting weights to range from 1/8 to 8 to limit variance. The case with the largest pre-trimmed weight was a 70-105 year old with less than high school and income less than $14,999. The case with the smallest pre-trimmed weight were two 50-69 year olds with doctoral degrees and income from $55,000 to $59,999.

Results: Survey Supreme Court Opinions Affected by Party Split Information

The logic of using equivalent votes by elites and members of the mass public to understand representation is not limited to studies of the US Congress. Recently, work has also sought to understand the correspondence between judicial behavior, specifically voting by US Supreme Court Judges, and citizen preferences by asking citizens to cast their votes on cases previously considered by the Court (Jessee and Malhotra, 2013; Malhotra and Jessee, 2014).[4] We examine whether expressed opinions by survey respondents on Supreme Court cases are affected by a treatment providing information about the vote split between judges appointed by Republican and Democratic presidents. The opinion of each justice, along with their partisan background is readily available to other members on the court, but may not be available to survey respondents when they consider Supreme Court decisions. Supreme Court cases may be a conservative test because many of these issues are social policy questions where citizens might hold strong opinions and the cases have also been subject to extensive public coverage after the decisions, raising the possibility that our treatment would provide little novel information.[5]

Because judges do not use party labels in their day to day deliberations, we provide information on the partisanship of the president who appointed each justice. In our treatment condition, we present respondents with how judges appointed by each party voted on the case (e.g., 100% of Republican-appointed justices voted for and 0% of Democratic-appointed justices voted for). Figure A1 follows our earlier presentation and compares support for the nine cases we asked about in the control and party split conditions.[6] As with the congressional items, support for each judicial decision varies materially between the two conditions. In 15 of the 18 vote-party observations in the figure (83%), expressed support in the treatment condition moves toward the observed party split for that party's justices. In five of the six cases where the majority vote differed by justice party (i.e., the justices were polarized by party), the party split is greater in the treatment group than control condition.[7] In sum, Figure A1 shows that the measurement challenge we argue affects the comparison of survey reports of roll call votes to congressional votes cast also arises in the comparison of survey responses about court cases to judicial decisions in those cases.

E IRT estimates of representation

One standard approach to evaluating the quality of representation is to use IRT models to summarize the preferences of individuals and representatives across issues and then to compare those

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[4] "We ask respondents how they would have voted on a set of cases recently decided by the Court, meaning that we can generate a comparable set of ideal points for both masses and elites in a common space (Jessee and Malhotra, 2013, Abstract)."

[5] Nor do we actually address the salient legal issues at play in these cases, including matters of precedent.

[6] These items were included on Study 3 after the survey roll call measures and were independently randomized at the respondent level. Appendix Table A5 lists the case text and the party splits.

[7] In 18 separate party x bill regressions, reported in Appendix Table A9, the effect of the party vote intervention is statistically significant at p < .10 in 8 instances (44%). The average absolute effect is 7 points.

Figure A1: Support for Supreme Court Opinion with and without party split, Study 3
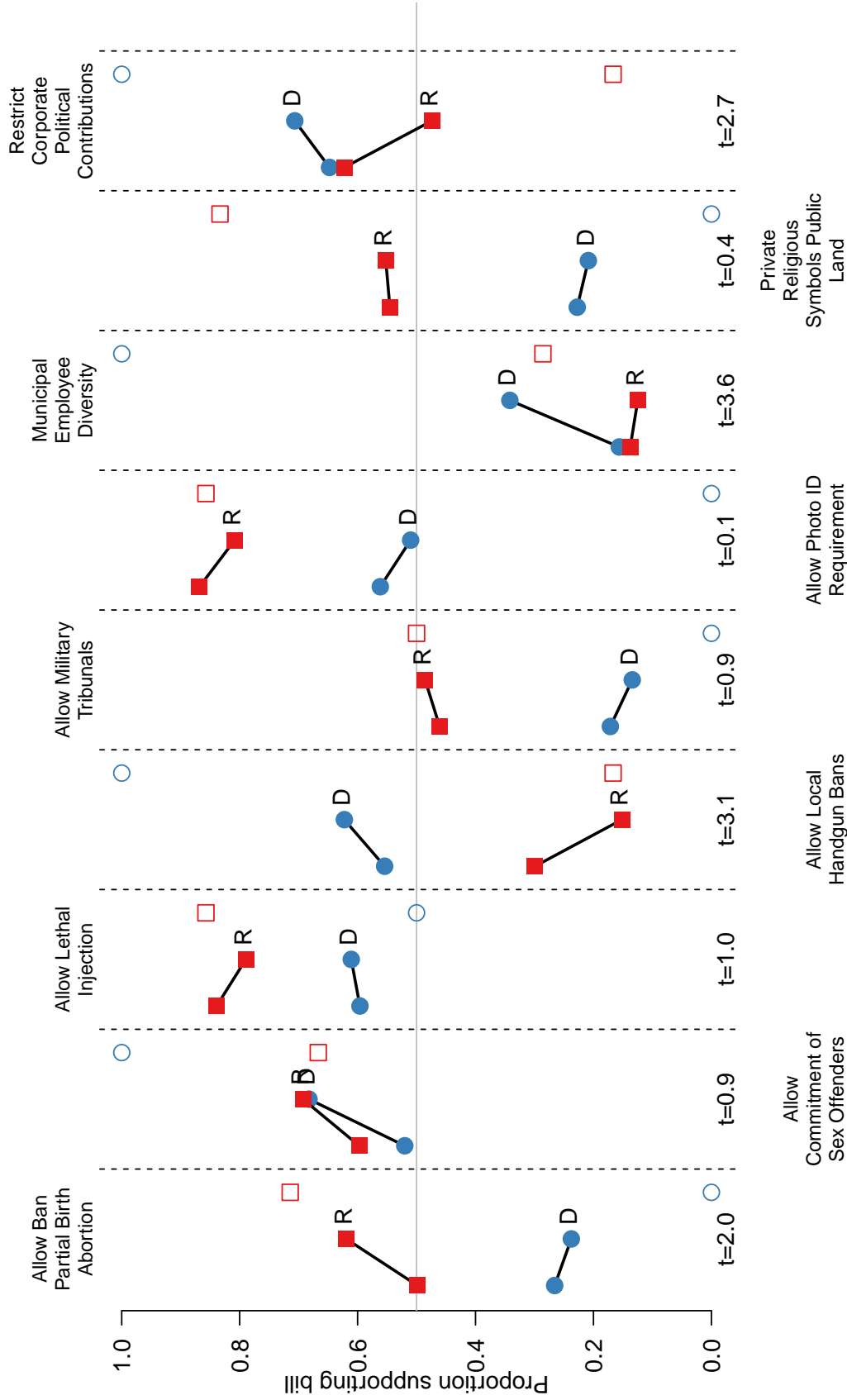
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for opinion from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among justices appointed by Democratic (Republican) presidents. Absolute value of t-ratio on difference-in-difference estimate of party-times-treatment indicated at x-axis.

summaries (e.g. Bafumi and Herron, 2010; Hill and Tausanovitch, 2015; Tausanovitch and Warshaw, 2013). The IRT models help mitigate measurement error in each individual item and have been found to be a fair single summary of member votes across thousands of bills (Poole and Rosenthal, 1997). In this section, we follow this standard practice while examining how evaluations of representation from IRT models vary when respondents are voting on the bills (from Study 2) with and without additional information.

We implement an IRT voting model using the R package pscl (Jackman, 2012). We scale the 12 roll call votes cast by each respondent into the same space as the set of senators who voted on those bills. We summarize the implementation below. To place the respondents in the same space as the Senators, we first scaled the Senators by themselves on the 12 roll calls. We then fixed the item parameters estimated from the Senate-only model and applied them to the joint models of Senators and respondents, yielding respondent ideal points in the Senate-space.

Because we use Markov chain Monte Carlo methods for the IRT model, we are able to summarize our posterior beliefs about multiple statistics of polarization. In particular, the United States Senate is a super-majoritarian legislature. We consider how well the distribution of ideal points in the Senate represents the distribution of ideal points in the public, particularly at percentiles of the Senate distribution that correspond to important veto points in the legislature (i.e., the median and the filibuster pivots, Krehbiel, 1998). We consider whether the estimated distribution of citizen preferences (in percentiles) appears more aligned with that in the Senate when citizens are in the party split condition than the control condition.

In Figure A2, we plot the location of quantiles for citizen and Senator distributions of ideal points separately for citizen distributions in the control (top) and party split (bottom) condition.[8] We characterize features of the posterior distribution of these quantiles for each population. Each point is the posterior median ideal point at that quantile, with lines extending to the posterior 95 percent credible interval. We summarize ideal points for the institutionally-relevant 41st, 50th, and 60th percentiles of each distribution, along with the more extreme 10th, 25th, 75th, and 90th percentiles.

The top frame with respondents in the control condition exhibits the conventional pattern of Senators more polarized than members of the public (e.g., Hill and Tausanovitch, 2015). The ideal point at the 0.1 quantile for citizens has a posterior median of -0.4, while for Senators the 0.1 quantile is one standard deviation more extreme at -1.35. Likewise, the 0.9 Senate quantile is 1.28 compared to 0.29 for citizens. The 0.25 and 0.75 quantiles show similar polarization of legislators relative to their constituents. The slope of ideal point to quantile is notably attenuated for the citizens relative to the Senate, suggesting some breakdown in the representation of preferences.

With respect to the institutional rules of the Senate, invoking cloture requires the votes of 3/5ths of the chamber to proceed to considering most bills. Figure A2 shows that the filibuster generates more status quo bias among the observed set of Senator ideal points than among the set of citizen ideal points in the control condition. Among citizens, the filibuster would have little influence on the set of status quos available to be modified by the legislature. The 0.41, 0.5, and 0.6 quantiles posterior medians are -0.04, 0.01, and 0.06. The Senate filibuster interval, in contrast, ranges from -0.29 to 0.33. Inside this region reside a set of status quo policies that could change in a legislature with the citizens' ideal points but that could not overcome the filibuster with the Senators' ideal points.

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[8] Appendix Figure A6 includes all respondents regardless of screener.

Figure A2: Change in representative divergence at selected quantiles with information, Study 2
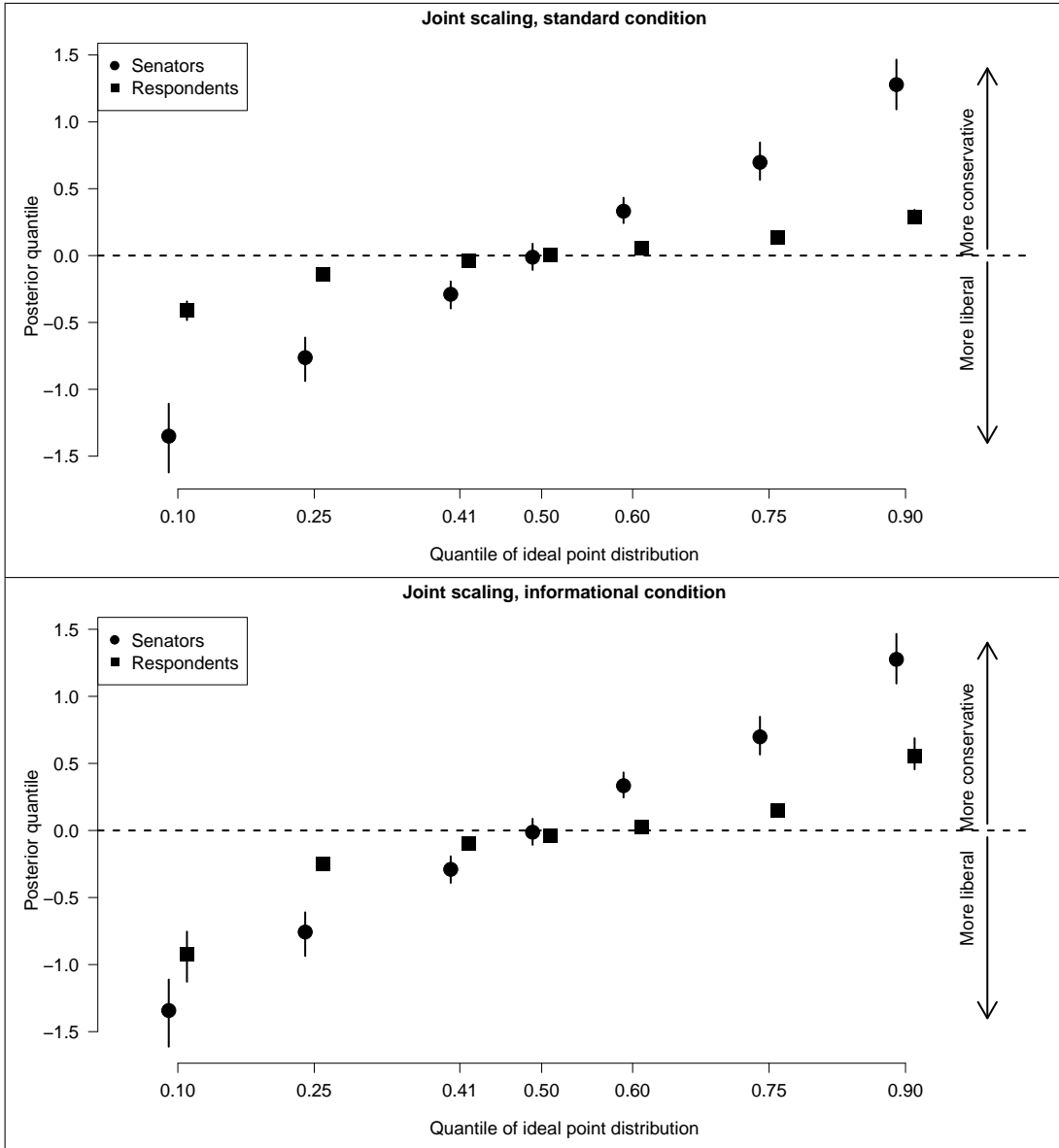
Note: Points represent the estimated ideal point at quantiles of respondent and Senate posterior distributions (posterior median with 95 percent posterior credible intervals). Figure limited to respondents who passed attention screener. Posterior quantiles of the respondent distribution are closer to quantiles of the Senate distribution when respondents are provided information.

The quantiles of the citizen distribution in the party split condition are less divergent with those in the Senate. The posterior median 0.1 quantile for citizens is -0.92, half a standard deviation closer to the Senate quantile than in the control condition. The posterior median 0.9 quantile in the party split condition is 0.55, a quarter standard deviation closer to the Senate. The 0.25 quantile moves from -0.14 in the control condition to -0.25 party split, and the 0.75 from 0.14 to 0.15.

The filibuster interval for citizens in the party split condition, however, is as narrow as in the control condition. The posterior medians for the 0.41, 0.5, and 0.6 quantiles are -0.1, -0.04, and 0.02. The party split condition thus appears to change the location of the more extreme quantiles of the citizen distribution, but does not have as large an influence on the location of the center of the distribution. The slope in the party split condition is closer to that in the Senate than the slope in the control condition.

In sum, the IRT models suggest that providing a single piece of information leads to a population distribution of ideal points that moves towards the Senate distribution, in particular with fatter tails more consistent with the bimodal distribution in the Senate, suggesting that more information and/or contexts making the survey environment more similar to that facing legislators would lead to policy positions closer to the votes we observe in the national legislature.

E.1 Details of IRT model

We jointly scaled the respondents with the 117 members of the 113th and 114th Senates who voted on some of these 12 roll call votes using the Bayesian Markov chain Monte Carlo (MCMC) `ideal()` in the `pscl` library in R (Jackman, 2012). To place the respondents in the same space as the Senators, we first scaled the Senators by themselves on the 12 roll calls. We then fix the item parameters estimated from the Senate-only model and apply them to the joint models of Senators and respondents.[9] This creates distributions of ideal points in the space implied by the item parameters from a Senate-only model on the assumption that the item parameters are the same for Senators and respondents. Note that the Senators will have mean zero and unit variance in these joint scalings, but not necessarily the respondents.

F   Additional tables and figures

One concern with the graphical presentation in Figure **??** is that it sorts individuals only on the basis of their partisanship. In fact, some partisans may be "cross-pressured" because their ideological views are inconsistent with their partisan orientation. For this reason, in Appendix Figure A3 we replicate our analysis separately for partisans whose ideology is aligned with their party orientation (i.e., Democrats who are liberal or moderate and Republicans who are conservative or moderate) and those whose ideology is at odds with their party orientation. For the aligned partisans, their behavior closely follows those shown in the pooled Figure **??** analysis. For cross-pressured partisans, the picture is more complicated. There are too few cross-pressured Republicans in our sample for reliable analysis, but for cross-pressured Democrats, they are both generally more conservative and move toward the Republican position on two issues when informed of the House vote. These two bills, on the Keystone Pipeline and the bill described as lower gas taxes, are two notable cases

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[9] The item parameters were fixed by setting the prior mean to the posterior mean from the Senate-only model, the prior variance to 100e-3, and no normalization to the distribution of ideal points. All models were burned in for 150,000 iterations, and then 200,000 samples were taken, thinned by 20 yielding 10,000 posterior values summarizing each parameter. Convergence was evaluated by Geweke statistics, where in each case about 95 percent of Gewekes were inside [-1.96, 1.96].

in which the Republican leadership pushed bills that presented policy options targeting unpopular Democratic policies.

Figure A7 plots, for each policy area, the relationship between self-assessed confidence (the vertical axis) and policy importance (horizontal axis). Each black line is a loess smooth of the individual relationship for that policy area. We indicate the average importance score (vertical grey lines) and average confidence score (horizontal grey lines) for each policy area. We also present the tabulation of each response at each value on the two axes, for example only 6% of responses to the question about policy importance indicated the policy was "not at all important." Several important patterns emerge.

First, on average, respondents think most policy areas are important. The average importance score across all policy areas is 2.14, which is slightly more than somewhat important. Only 21% of evaluations scored the policies as little or not at all important. Second, while respondents think policy in these areas is important, they are on average less confident in their ability to pick policies that give them what they want. The average confidence score is 1.56, which is roughly half way between a little and somewhat competent. While 41% of evaluations indicated the policy area "one of the most important," only 22% of evaluations indicated the individual felt "very confident" that they could distinguish good from bad policies. Further, the loess smooths show that there is only a weak positive relationship between believing a policy area is important and believing one can identify good public policy. Thus, it is not the case that simply thinking something is important means individuals have great confidence that they can pick which policies are best in that area.

Tables A6, A7, and A8 present regression estimates for treatment effects in the three studies.

Figure A3: Support for roll call with and without party split information by party-ideology cross-pressure
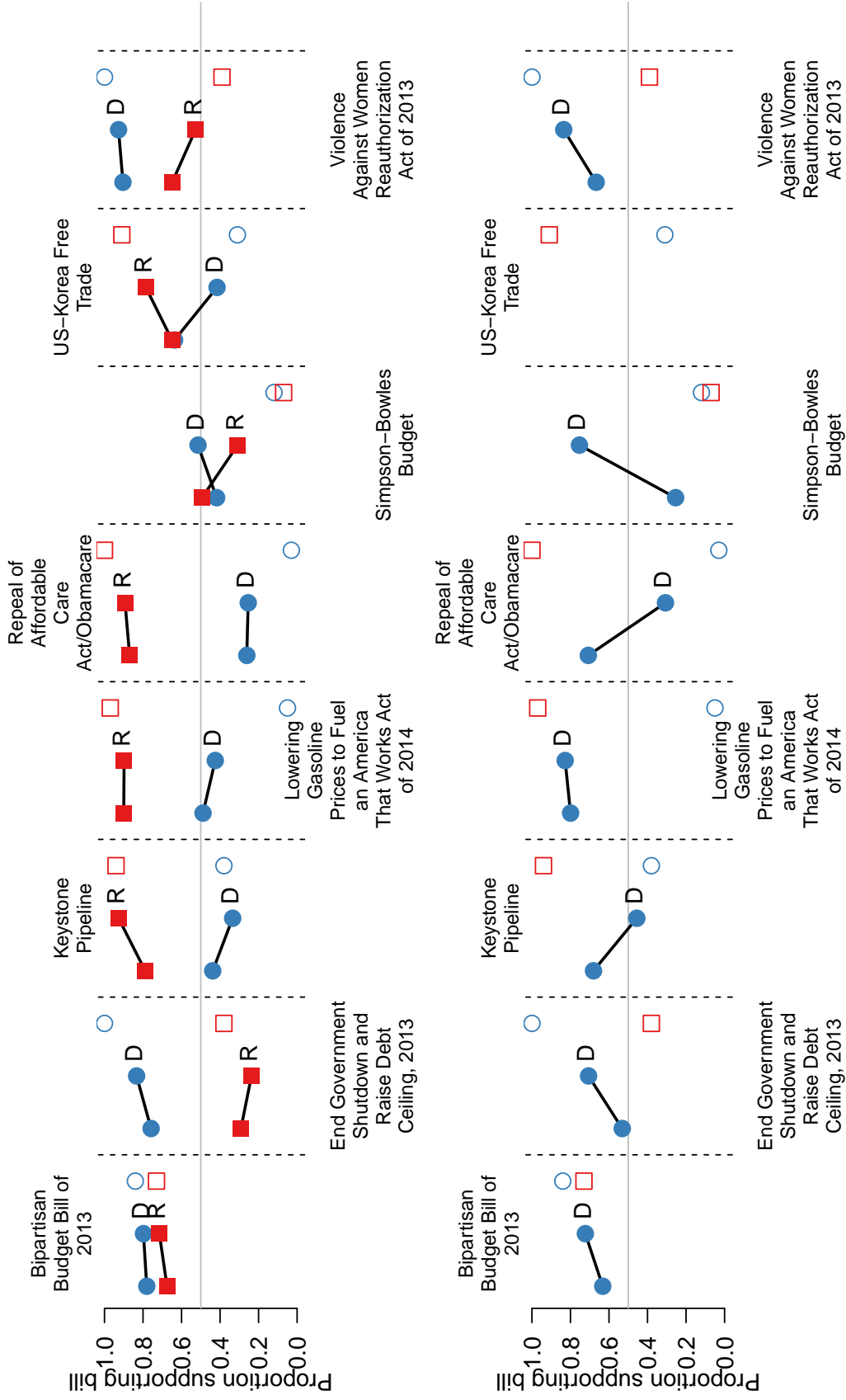
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among Democratic (Republican) members of the House. The top frame presents support for non-cross-pressured partisans, liberal and moderate Democrats and moderate and conservative Republicans. The bottom frame presents cross-pressured conservative Democrats; there are too few liberal Republicans to plot.

Figure A4: Support for roll call with and without party split information, Study 2, All respondents
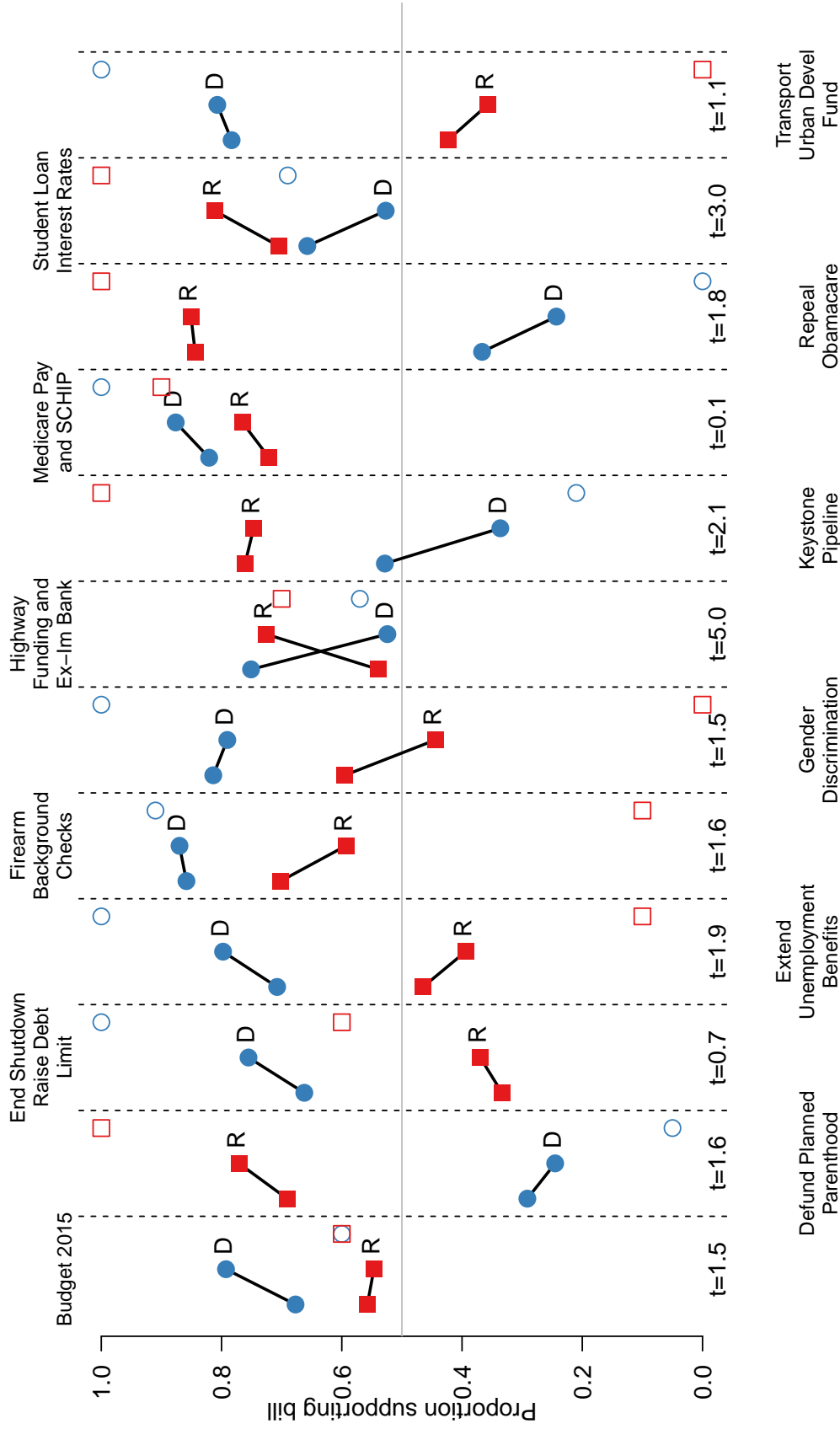
Note: *Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among Democratic (Republican) members of the Senate. These rates were presented to respondents in the party split condition. Absolute value of t-ratio on difference-in-difference estimate of party-times-treatment indicated at x-axis.*

Figure A5: Votes with Democratic side in Senate by condition and party, Study 2, All respondents
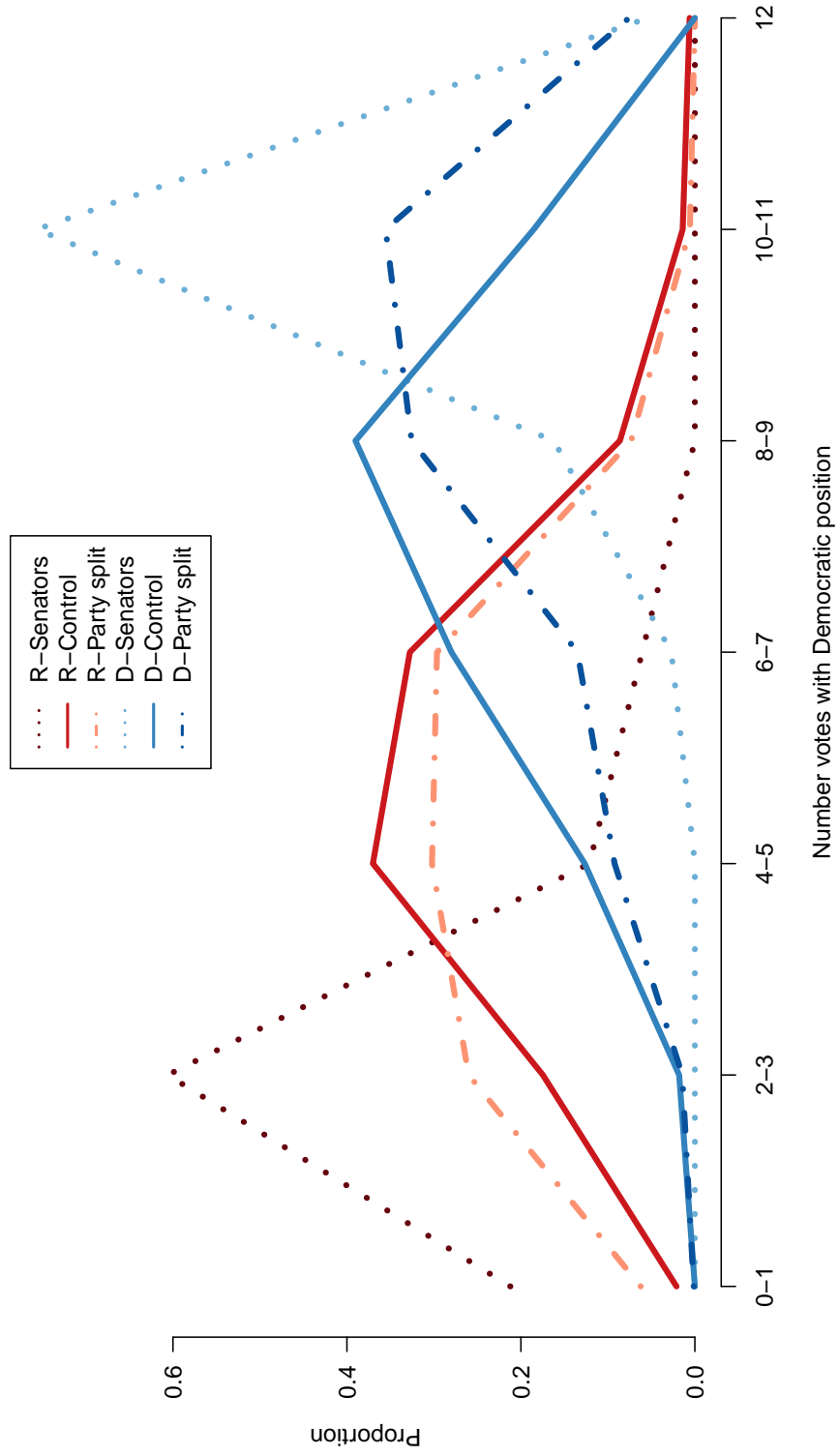
Note: Each line is the distribution across number of votes with the Democrats on the 12 roll call votes in Study 2. Limited to respondents and senators who voted on all 12 roll call votes and to respondents who passed the screener.

Figure A6: Change in relative polarization with information, Study 2, All respondents
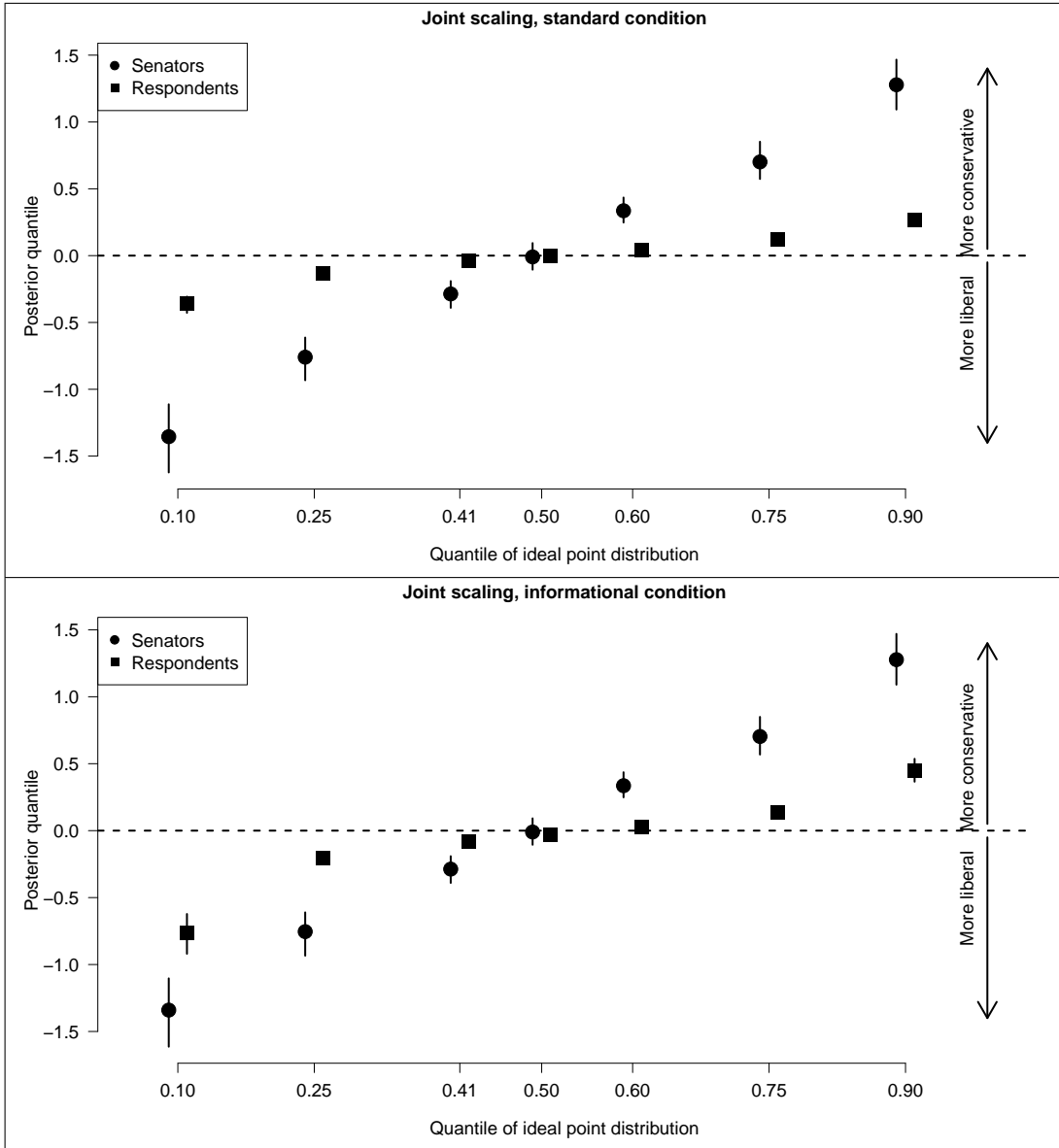
Note: Points represent the estimated ideal point at quantiles of respondent and Senate posterior distributions (posterior median with 95 percent posterior credible intervals). Posterior quantiles of the respondent distribution are closer to quantiles of the Senate distribution when respondents are provided information.

Figure A7: Relationship of confidence to importance by policy area, Study 2

Importance of policy

Confidence distinguishing good from bad policies
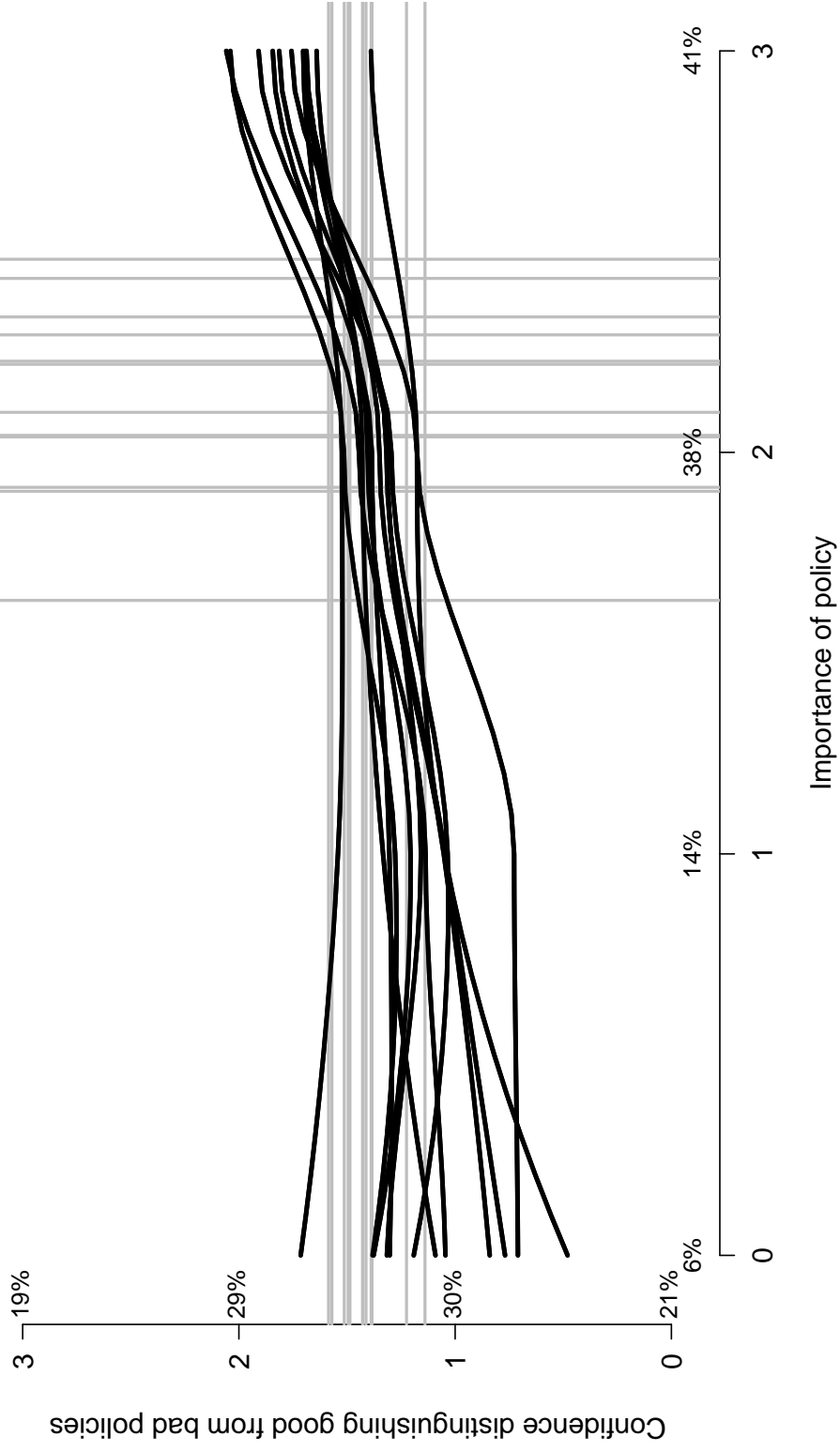
Note: Each line is a loess smooth of respondents' assessment of their own confidence that they are able to evaluate policy in an area to the importance they ascribe to policy in that area. The policy areas correspond roughly to the 12 roll call votes of Study 2. Vertical and horizontal lines indicate the mean confidence and importance for each policy. Percentage of responses in each category across roll calls indicated on each axis.

Table A6: Roll call vote treatment effects, Study 1

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	All Bipartisan Budget Bill of 2013			All End Government Shutdown and Raise Debt Ceiling 2013			All Keystone Pipeline			All Lowering Gasoline Prices to Fuel an America That Works Act of 2014		
		Dems	Reps		Dems	Reps		Dems	Reps		Dems	Reps
Party split condition	0.0084	0.026	0.019	0.052*	0.085*	-0.068	-0.035	-0.12**	0.13**	-0.043	-0.061	0.0028
	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)	(0.02)	(0.04)	(0.03)	(0.02)	(0.04)	(0.03)
Democrat	0.15**			0.25**			-0.17**			-0.26**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	0.071*			-0.23**			0.27**			0.15**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Constant	0.62**	0.76**	0.69**	0.50**	0.74**	0.30**	0.60**	0.46**	0.79**	0.77**	0.52**	0.90**
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)
Observations	1,391	637	478	1,447	665	514	1,464	685	511	1,403	635	512
R-squared	0.018	0.001	0.000	0.193	0.010	0.005	0.160	0.014	0.033	0.160	0.004	0.000

Standard errors in parentheses
** p<0.01, * p<0.05

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	All Repeal of Affordable Care Act/Obamacare			All Simpson-Bowles Budget			All US-Korea Free Trade			All Violence Against Women Reauthorization Act of 2013		
		Dems	Reps		Dems	Reps		Dems	Reps		Dems	Reps
Party split condition	-0.031	-0.048	0.058	0.0032	0.14**	-0.19**	-0.036	-0.20**	0.12**	-0.049*	0.042	-0.14**
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)	(0.05)
Democrat	-0.33**			0.097**			0.017			0.18**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	0.25**			0.037			0.16**			-0.11**		
	(0.03)			(0.04)			(0.04)			(0.03)		
Constant	0.63**	0.31**	0.84**	0.36**	0.40**	0.48**	0.56**	0.64**	0.65**	0.74**	0.88**	0.66**
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Observations	1,463	694	505	1,363	635	453	1,353	627	472	1,393	644	492
R-squared	0.266	0.003	0.007	0.007	0.019	0.035	0.021	0.038	0.017	0.094	0.005	0.020

Standard errors in parentheses
** p<0.01, * p<0.05

Table A7: Roll call vote treatment effects, Study 2

	(1) All Budget 2015	(2)	(3)	(4) All Defund Planned Parenthood	(5)	(6)	(7) All End Shutdown Raise Debt Limit	(8)	(9)	(10) All Extend Unemployment	(11)	(12)
VARIABLES		Dems	Reps		Dems	Reps		Dems	Reps		Dems	Reps
Party split condition	0.066*	0.12**	0.027	-0.016	-0.058	0.15**	0.042	0.097*	-0.027	0.016	0.083*	-0.20**
	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)
Democrat	0.18**			-0.24**			0.31**			0.28**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	-0.050			0.24**			-0.089*			-0.17**		
	(0.03)			(0.03)			(0.04)			(0.03)		
Constant	0.59**	0.74**	0.56**	0.47**	0.25**	0.64**	0.44**	0.72**	0.38**	0.55**	0.78**	0.47**
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Observations	1,117	370	461	1,117	370	461	1,117	370	461	1,117	370	461
R-squared	0.042	0.023	0.001	0.123	0.005	0.028	0.099	0.013	0.001	0.110	0.012	0.042

Standard errors in parentheses
** p<0.01, * p<0.05

	(1) All Firearm Background Checks	(2)	(3)	(4) All Gender Discrimination	(5)	(6)	(7) All Highway Funding Ex-Im Bank	(8)	(9)	(10) All Keystone Pipeline	(11)	(12)
VARIABLES		Dems	Reps		Dems	Reps		Dems	Reps		Dems	Reps
Party split condition	-0.038	-0.018	-0.19**	-0.074**	0.047	-0.20**	-0.048	-0.19**	0.13**	-0.087**	-0.24**	0.031
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.05)	(0.03)	(0.05)	(0.04)	(0.03)	(0.05)	(0.04)
Democrat	0.12**			0.19**			0.015			-0.15**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	-0.15**			-0.20**			0.041			0.19**		
	(0.03)			(0.03)			(0.04)			(0.04)		
Constant	0.83**	0.94**	0.75**	0.70**	0.83**	0.56**	0.66**	0.75**	0.62**	0.61**	0.55**	0.74**
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)	(0.03)	(0.02)	(0.04)	(0.03)
Observations	1,117	370	461	1,117	370	461	1,117	370	461	1,117	370	461
R-squared	0.063	0.001	0.038	0.092	0.004	0.042	0.004	0.040	0.020	0.069	0.061	0.001

Standard errors in parentheses
** p<0.01, * p<0.05

	(1) All Medicare Pay and SCHIP	(2)	(3)	(4) All Repeal Obamacare	(5)	(6)	(7) All Student Loan Interest Rates	(8)	(9)	(10) All Transport Urban Devel Fund	(11)	(12)
VARIABLES		Dems	Reps		Dems	Reps		Dems	Reps		Dems	Reps
Party split condition	0.055*	0.035	0.14**	-0.054*	-0.083	0.035	-0.033	-0.11*	0.11**	-0.038	0.017	-0.17**
	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.04)	(0.03)	(0.04)	(0.05)
Democrat	0.11**			-0.33**			-0.0099			0.29**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	-0.027			0.33**			0.098**			-0.14**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Constant	0.76**	0.88**	0.69**	0.59**	0.27**	0.87**	0.67**	0.71**	0.70**	0.56**	0.82**	0.48**
	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)	(0.03)	(0.03)
Observations	1,117	370	461	1,117	370	461	1,117	370	461	1,117	370	461
R-squared	0.026	0.003	0.025	0.228	0.010	0.003	0.010	0.014	0.017	0.104	0.001	0.029

Standard errors in parentheses
** p<0.01, * p<0.05

Figure A8: Relationship of actual party splits to respondent beliefs, Study 3
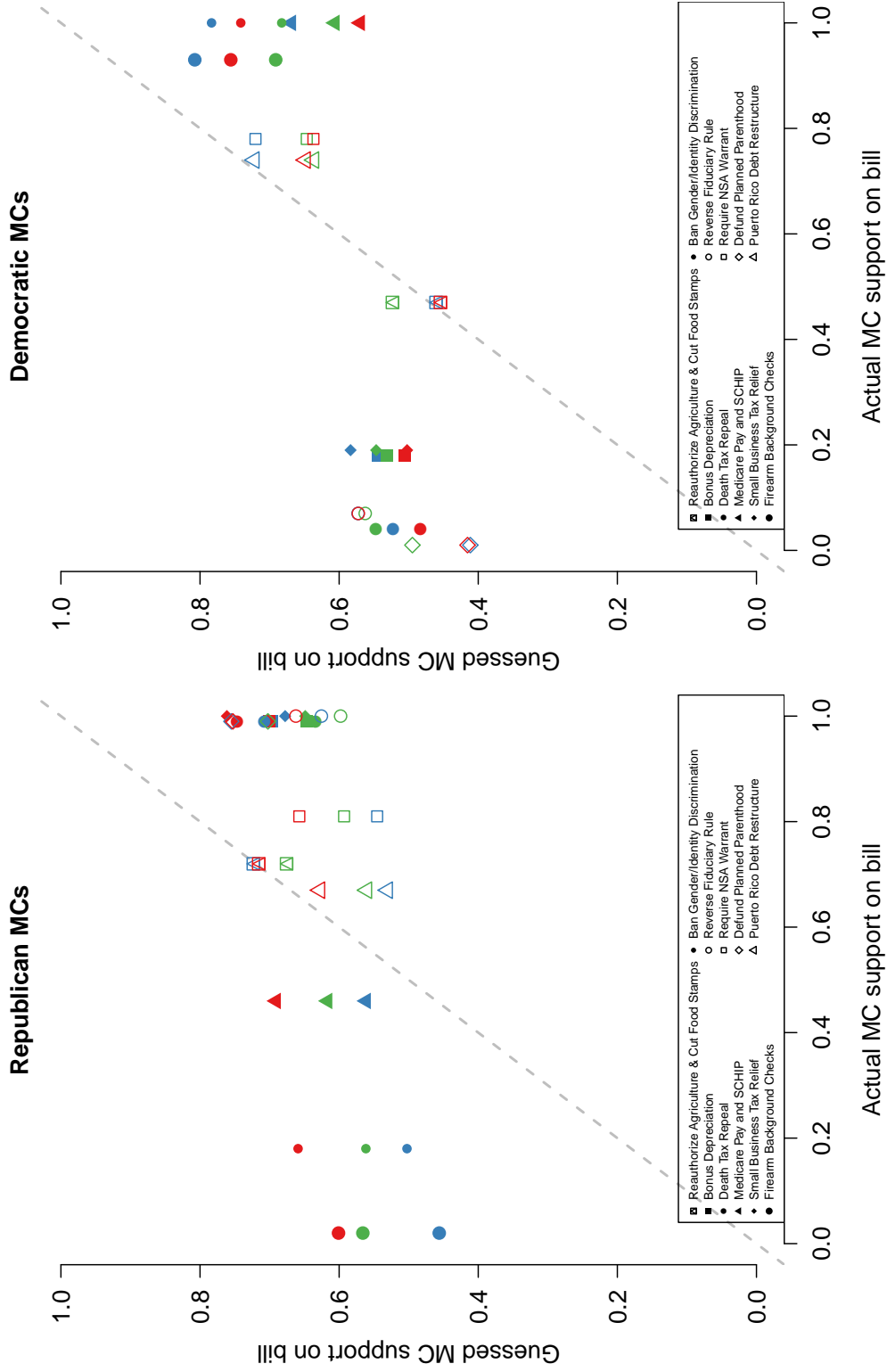
Note: Each frame plots the relationship between actual party split in Congress (x-axis) and Lucid respondent beliefs about the party split (y-axis). The three points above each location on the x-axis correspond to the guess on the party split by Democrats, Republicans, and Independents, respectively. The figure shows respondents were not well informed about the splits on these bills, on average – points fall far from the dashed 45 degree line.

Table A8: Roll call vote treatment effects, Study 3

VARIABLES	(1) All Bonus Depreciation	(2) Dems	(3) Reps	(4) All Death Tax Repeal	(5) Dems	(6) Reps	(7) All Medicare Pay and SCHIP	(8) Dems	(9) Reps	(10) All Reauthorize Agriculture and Cut Food Stamps	(11) Dems	(12) Reps	(13) All Small Business Tax Relief	(14) Dems	(15) Reps
Chamber split condition	0.061**	0.0026	0.18**	0.071**	0.13**	0.023	0.041*	0.077**	0.039	-0.0053	0.0016	-0.016	0.034	0.062*	-0.0042
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
CBO condition	-0.12**	-0.21**	-0.034	-0.017	0.021	-0.072*	-0.11**	-0.12**	-0.100**	0.036	0.027	0.012	-0.072**	-0.088**	0.0020
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Party split condition	-0.020	-0.27**	0.28**	0.013	-0.13**	0.11**	-0.0015	0.14**	-0.19**	0.055**	0.038	0.11**	-0.090**	-0.27**	0.061*
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Democrat	-0.033			-0.070**			0.097**			-0.076**			-0.053**		
	(0.02)			(0.02)			(0.02)			(0.02)			(0.02)		
Republican	0.18**			0.29**			0.083**			0.31**			0.20**		
	(0.02)			(0.02)			(0.02)			(0.02)			(0.02)		
Constant	0.46**	0.50**	0.54**	0.47**	0.40**	0.75**	0.67**	0.74**	0.79**	0.31**	0.24**	0.62**	0.62**	0.60**	0.78**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	4,502	1,994	1,606	4,507	1,997	1,607	4,502	1,998	1,605	4,503	1,996	1,608	4,508	1,996	1,609
R-squared	0.055	0.057	0.063	0.112	0.027	0.018	0.019	0.040	0.037	0.136	0.001	0.009	0.066	0.051	0.004

Standard errors in parentheses

** p<0.01, * p<0.05

VARIABLES	(1) All Ban Gender Identity Discrimination	(2) Dems	(3) Reps	(4) All Defund Planned Parenthood	(5) Dems	(6) Reps	(7) All Firearm Background Checks	(8) Dems	(9) Reps	(10) All Puerto Rico Debt Restructure	(11) Dems	(12) Reps	(13) All Require NSA Warrant	(14) Dems	(15) Reps	(16) All Reverse Fiduciary Rule	(17) Dems	(18) Reps
Chamber split condition	-0.021	-0.0091	-0.045	0.043**	0.000046	0.11**	-0.088**	-0.083**	-0.11**	0.038*	0.053*	0.051	0.084**	0.097**	0.093**	0.046**	0.061*	0.015
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
Party split condition	-0.083**	0.030	-0.24**	0.0046	-0.061**	0.046	-0.096**	0.0014	-0.26**	0.024	0.094**	-0.025	0.10**	0.091**	0.17**	0.022	-0.16**	0.21**
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Democrat	0.14**			-0.16**			0.13**			0.18**			0.11**			-0.051*		
	(0.02)			(0.02)			(0.02)			(0.02)			(0.02)			(0.02)		
Republican	-0.033			0.33**			-0.017			0.073**			0.12**			0.18**		
	(0.02)			(0.02)			(0.02)			(0.02)			(0.02)			(0.02)		
Constant	0.77**	0.88**	0.79**	0.35**	0.22**	0.65**	0.78**	0.89**	0.82**	0.58**	0.74**	0.66**	0.61**	0.72**	0.71**	0.38**	0.37**	0.52**
	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	4,511	2,003	1,608	4,508	2,000	1,610	4,510	2,003	1,610	4,510	2,002	1,608	4,511	2,004	1,609	4,513	2,003	1,610
R-squared	0.047	0.002	0.048	0.206	0.004	0.009	0.044	0.013	0.056	0.026	0.009	0.004	0.022	0.012	0.030	0.048	0.029	0.035

Standard errors in parentheses

** p<0.01, * p<0.05

Table A9: Supreme Court vote treatment effects, Study 3

VARIABLES	(1) All Allow Ban Partial Birth Abortion	(2) All Allow Commitment of Sex Offenders	(3) All Allow Lethal Injection	(4) All Allow Local Handgun Bans	(5) All Allow Military Tribunals	(6) All Allow Photo ID Requirement	(7) All Allow Municipal Employee Diversity	(8) All Private Religious Symbols Public Land	(9) All Restrict Corporate Political Contributions
Party split condition	0.017	0.11**	-0.011	-0.037	-0.020	-0.062**	0.077**	-0.0066	-0.036
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Democrat	-0.19**	-0.013	-0.075*	0.24**	-0.043	-0.080**	0.029	-0.058	0.12**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Republican	0.12**	0.027	0.13**	-0.13**	0.28**	0.22**	-0.097**	0.27**	-0.013
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	0.43**	0.56**	0.68**	0.37**	0.21**	0.65**	0.18**	0.28**	0.58**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Observations	1,823	1,807	1,864	1,796	1,842	1,864	1,832	1,798	1,802
R-squared	0.086	0.014	0.042	0.121	0.114	0.089	0.029	0.104	0.019

Standard errors in parentheses
** $p<0.01$, * $p<0.05$

Democrats

VARIABLES	(1) Dems	(2) Dems	(3) Dems	(4) Dems	(5) Dems	(6) Dems	(7) Dems	(8) Dems	(9) Dems
Party split condition	-0.028	0.16**	0.015	0.068*	-0.037	-0.051	0.19**	-0.019	0.059
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	0.27**	0.52**	0.60**	0.55**	0.17**	0.56**	0.16**	0.23**	0.65**
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	826	823	857	828	821	843	841	816	820
R-squared	0.001	0.027	0.000	0.005	0.003	0.003	0.045	0.001	0.004

Standard errors in parentheses
** $p<0.01$, * $p<0.05$

Republicans

VARIABLES	(1) Reps	(2) Reps	(3) Reps	(4) Reps	(5) Reps	(6) Reps	(7) Reps	(8) Reps	(9) Reps
Party split condition	0.12**	0.095*	-0.050	-0.15**	0.025	-0.060*	-0.013	0.0069	-0.15**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
Constant	0.50**	0.60**	0.84**	0.30**	0.46**	0.87**	0.14**	0.55**	0.62**
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
Observations	636	654	666	634	677	651	634	630	646
R-squared	0.015	0.010	0.004	0.032	0.001	0.006	0.000	0.000	0.022

Standard errors in parentheses
** $p<0.01$, * $p<0.05$

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G   Unweighted tables and figures

In Appendix Table A10 we present balance tests for treatment assignment by whether or not we use the stratification weights. For Study 1, Study 2, and the Supreme Court cases of Study 3, there is one treatment and one control condition. We run a logit model predicting treatment assignment as a function of covariates. Using the Stata `svy` command for weighted logit estimation, we test for imbalance with a joint F-test on the covariates. In each case, we cannot reject the null hypothesis that the coefficients on covariates are zero. For the two blocks of roll calls in Study 3 with four conditions, we run a multinomial logit via Stata `svy`. Again, the F-tests in each case do not reject the null hypothesis of balance across conditions, with or without stratification weights.

Appendix Figures A9 to A15 and Tables A11 to A15 present results from main text and appendix without use of post-stratification weights.

Table A10: Balance tables with and without weights

Study 1

VARIABLES	(1) Unweighted	(2) se	(3) Weighted	(4) se
Party split condition		(.)		(.)
Age category	-0.039	(0.03)	0.0045	(0.05)
male	0.057	(0.11)	-0.056	(0.18)
What is the highest level of education you have completed?	0.0018	(0.03)	0.026	(0.06)
region==Northeast	-0.37*	(0.17)	-0.26	(0.27)
region==South	-0.15	(0.15)	-0.20	(0.23)
region==West	-0.10	(0.16)	-0.0038	(0.25)
Total family income last year	0.0015	(0.04)	-0.0071	(0.06)
Are you currently married, living with a partner, divorced, separated, widowed,	0.0081	(0.03)	0.077	(0.05)
Has donated to political candidate last two years	-0.17	(0.13)	-0.19	(0.21)
Definitely or not sure if registered to vote	0.38	(0.20)	0.39	(0.31)
Constant	-0.017	(0.32)	-0.52	(0.48)
Observations	1,454		1,454	
F-test	1.105		0.723	
F p-value	0.354		0.703	

Standard errors in parentheses
** p<0.01, * p<0.05

Study 2

VARIABLES	(1) Unweighted	(2) se	(3) Weighted	(4) se
Party split condition		(.)		(.)
Age in years	0.013	(0.01)	0.017	(0.01)
Female (1=yes)	0.11*	(0.05)	0.045	(0.06)
Age squared / 100	-0.014	(0.01)	-0.018	(0.01)
Race=Black (1=yes)	-0.079	(0.08)	0.0031	(0.11)
Race=Hispanic (1=yes)	-0.11	(0.10)	-0.18	(0.17)
Race=Other (1=yes)	-0.016	(0.10)	0.0044	(0.13)
Church attendance scale (0=Never, 4=¿1 per week	0.033	(0.02)	0.034	(0.02)
Income scale (1-12, DK=6)	-0.00031	(0.01)	0.011	(0.01)
Income refused/dk (1=yes)	-0.098	(0.08)	0.10	(0.11)
Constant	-2.29**	(0.22)	-2.43**	(0.30)
Observations	37,163		37,163	
F-test	1.390		0.917	
F p-value	0.187		0.509	

Standard errors in parentheses
** p<0.01, * p<0.05

Study 3

VARIABLES	(1) Block 1 Unweighted	(2) Block 1 Unweighted	(3) Block 1 Unweighted	(4) Block 1 Unweighted	(5) Block 1 Weighted	(6) Block 1 Weighted	(7) Block 1 Weighted	(8) Block 1 Weighted	(9) Block 2 Unweighted	(10) Block 2 Unweighted	(11) Block 2 Unweighted	(12) Block 2 Weighted	(13) Block 2 Weighted	(14) Block 2 Weighted	(15) SCOTUS Unweighted	(16) SCOTUS Weighted
Education (1=HS or less, 8=PhD+)	0.014	-0.025		-0.0064	0.013	-0.052		0.061	0.0056		0.014	-0.013		0.066	-0.019	-0.0034
	(0.03)	(0.03)		(0.02)	(0.04)	(0.04)		(0.04)	(0.02)		(0.02)	(0.03)		(0.03)	(0.02)	(0.03)
Race=Black (1=yes, 0=no)	0.23	-0.046		0.095	0.19	0.20		-0.015	0.19		0.025	0.20		0.0093	-0.055	-0.33*
	(0.13)	(0.14)		(0.13)	(0.20)	(0.23)		(0.20)	(0.11)		(0.12)	(0.18)		(0.19)	(0.10)	(0.16)
Race=Other (1=yes, 0=no [White or Black])	-0.0083	-0.21		-0.099	-0.050	-0.28		-0.19	0.015		-0.055	-0.014		-0.15	-0.058	-0.13
	(0.12)	(0.13)		(0.13)	(0.18)	(0.21)		(0.19)	(0.11)		(0.11)	(0.17)		(0.17)	(0.09)	(0.14)
Income (Scale, 1-24, refused=25)	0.00037	-0.0025		-0.0016	-0.0040	0.0087		-0.0027	0.0032		-0.0034	0.0014		-0.0023	0.0051	0.0076
	(0.01)	(0.01)		(0.01)	(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)		(0.01)	(0.01)	(0.01)
Income Refused (1=yes)	-0.0012	0.21		0.030	0.064	0.13		-0.30	-0.015		0.037	0.059		-0.29	-0.068	-0.16
	(0.22)	(0.21)		(0.22)	(0.35)	(0.33)		(0.32)	(0.19)		(0.20)	(0.30)		(0.29)	(0.16)	(0.25)
Hispanic (1=yes)	-0.23	0.15		-0.30	-0.19	0.34		-0.096	-0.31*		-0.23	-0.32		-0.057	0.24*	0.20
	(0.15)	(0.14)		(0.16)	(0.22)	(0.22)		(0.25)	(0.13)		(0.13)	(0.21)		(0.20)	(0.11)	(0.17)
Region=Northeast	0.040	0.056		0.093	0.12	0.23		0.23	0.065		0.029	0.12		0.16	0.061	0.0071
	(0.13)	(0.13)		(0.13)	(0.20)	(0.20)		(0.20)	(0.12)		(0.12)	(0.18)		(0.18)	(0.09)	(0.15)
Region=South	0.078	0.12		0.079	0.12	0.20		0.14	0.12		0.096	0.090		0.093	0.0081	-0.014
	(0.11)	(0.11)		(0.11)	(0.17)	(0.17)		(0.17)	(0.10)		(0.10)	(0.15)		(0.15)	(0.08)	(0.12)
Region=West	0.038	0.014		0.0032	-0.030	0.16		-0.061	0.065		0.0023	0.088		0.019	0.040	0.15
	(0.13)	(0.13)		(0.13)	(0.19)	(0.20)		(0.20)	(0.11)		(0.11)	(0.17)		(0.17)	(0.09)	(0.14)
Registered to Vote (1=Yes for sure)	-0.0066	0.16		0.065	-0.050	0.11		-0.16	-0.024		0.034	-0.046		-0.14	0.038	0.12
	(0.10)	(0.10)		(0.11)	(0.17)	(0.16)		(0.16)	(0.09)		(0.09)	(0.14)		(0.14)	(0.08)	(0.12)
Age in years	-0.0036	-0.0027		0.00066	0.00095	-0.000029		0.0026	-0.0033		0.00033	0.0024		0.0025	0.0017	-0.000090
	(0.00)	(0.00)		(0.00)	(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)		(0.00)	(0.00)	(0.00)
Constant	-0.63**	-0.62**		-0.76**	-0.72**	-0.90**		-0.98**	-0.52**		-0.68**	-0.61**		-0.89**	-0.11	-0.034
	(0.17)	(0.18)		(0.17)	(0.23)	(0.26)		(0.24)	(0.16)		(0.15)	(0.21)		(0.21)	(0.13)	(0.18)
Observations	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487	4,487
F-test	0.896	0.896	0.896	0.896	0.999	0.999	0.999	0.999	0.815	0.815	0.815	0.821	0.821	0.821	0.760	1.018
F p-value	0.638	0.638	0.638	0.638	0.470	0.470	0.470	0.470	0.710	0.710	0.710	0.702	0.702	0.702	0.680	0.427

Standard errors in parentheses
** p<0.01, * p<0.05

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Figure A9: Support for roll call with and without party split information, Study 1 Unweighted
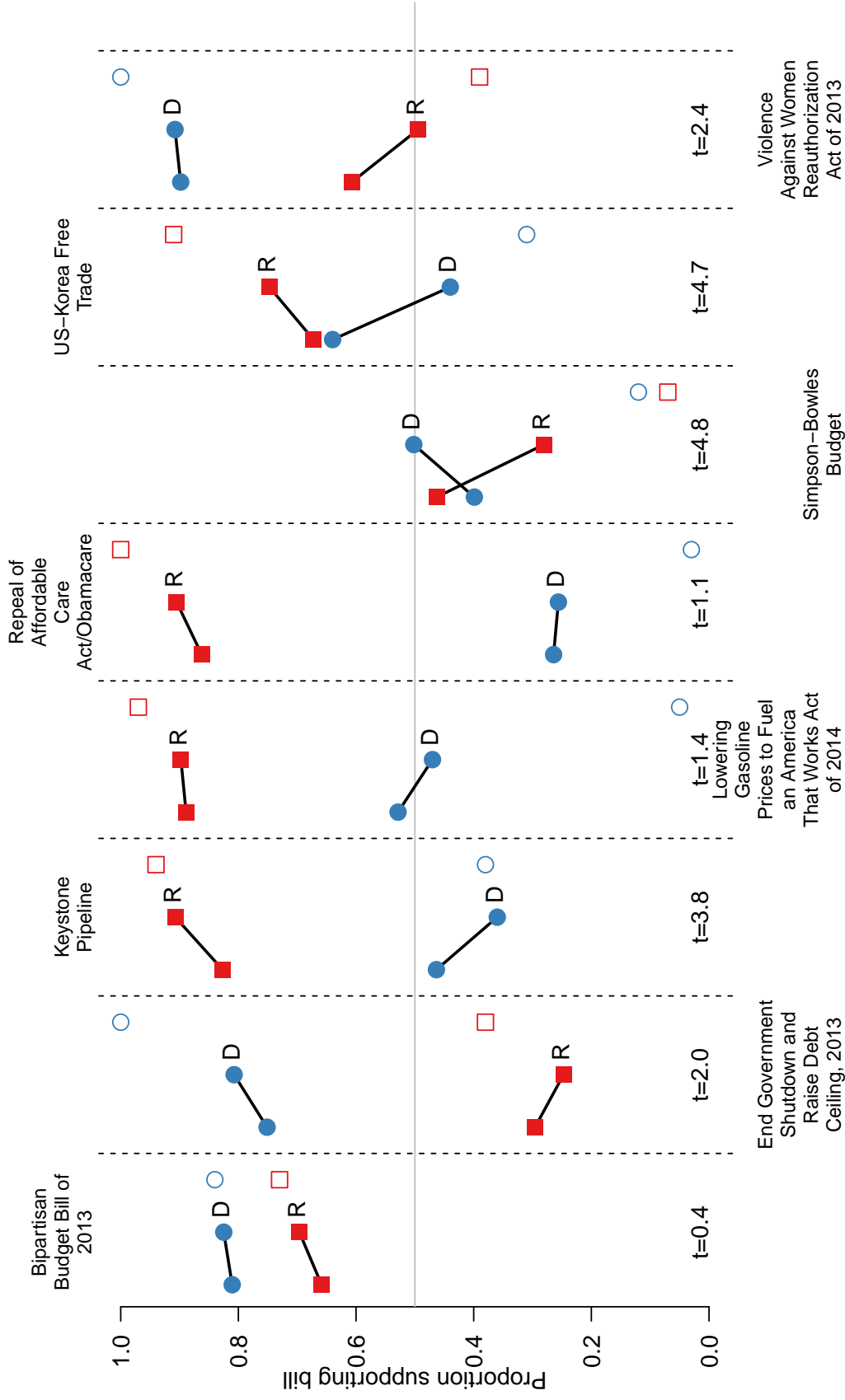
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among Democratic (Republican) members of the House. Absolute value of t-ratio on difference-in-difference estimate of party-times-treatment indicated at x-axis.

Figure A10: Support for roll call with and without party split information, Study 2 Unweighted
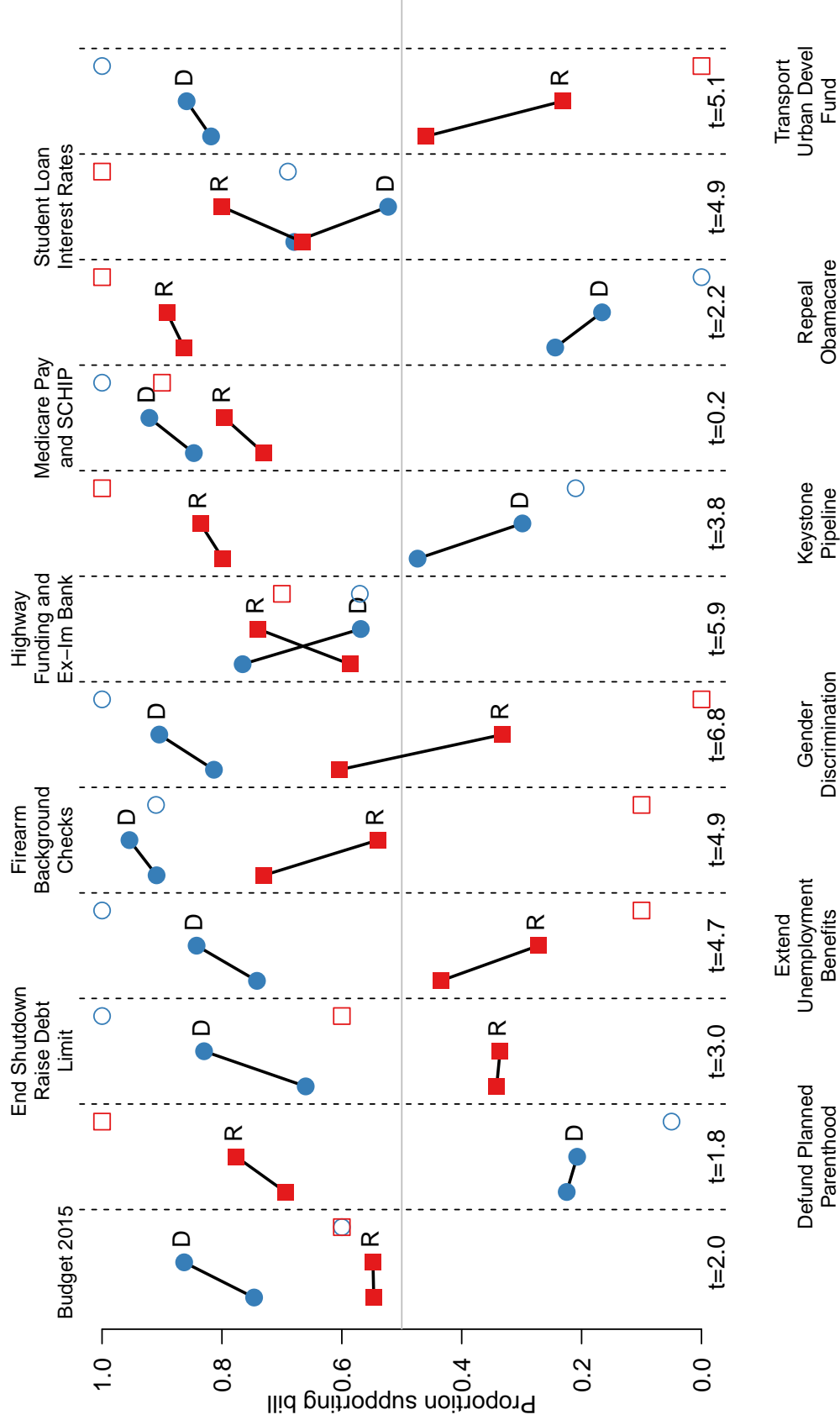
Note: *Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among Democratic (Republican) members of the Senate. Absolute value of t-ratio on difference-in-difference estimate of party-times-treatment indicated at x-axis.*

Figure A11: Votes with Democratic position in Senate by condition and party, Study 2 Unweighted

Note: Each line is the distribution across number of votes with the Democrats on the 12 roll call votes in Study 2.

Figure A12: Support for roll call with and without party split information, Study 3 Unweighted

Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the party split condition (right). Open circles (squares) are the actual rate of support among Democratic (Republican) members in the chamber. Absolute value of t-ratio on difference-in-difference estimate of party-times-treatment indicated at x-axis.

Figure A13: Support for roll call from control to Chamber Split condition, Study 3 Unweighted
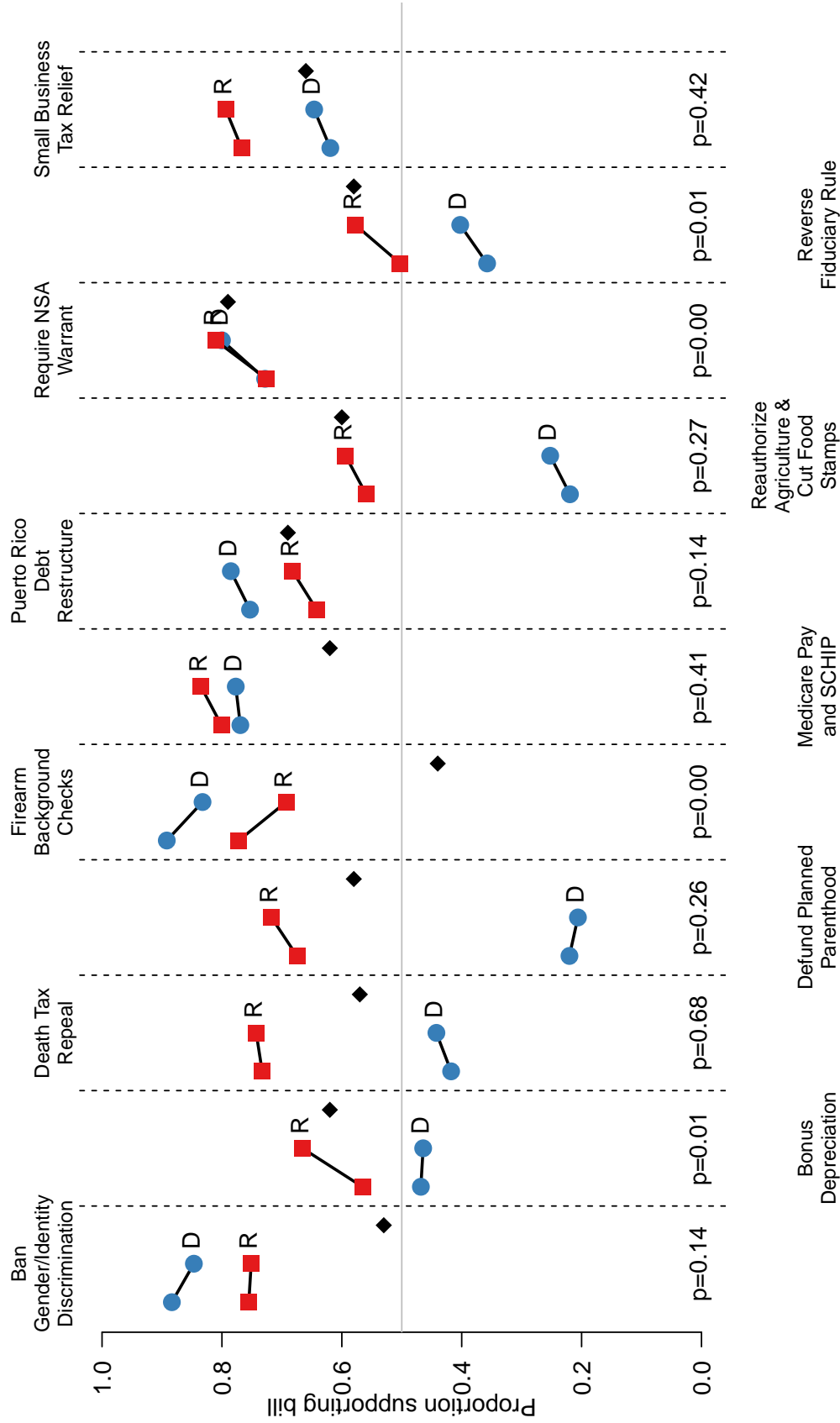
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the chamber split condition (right). Diamonds are the actual rate of support in the chamber. P-value for an F-test on the treatment and treatment times party regression coefficients to evaluate significance of treatment indicated at x-axis

Figure A14: Support for roll call from control to CBO condition, Study 3 Unweighted
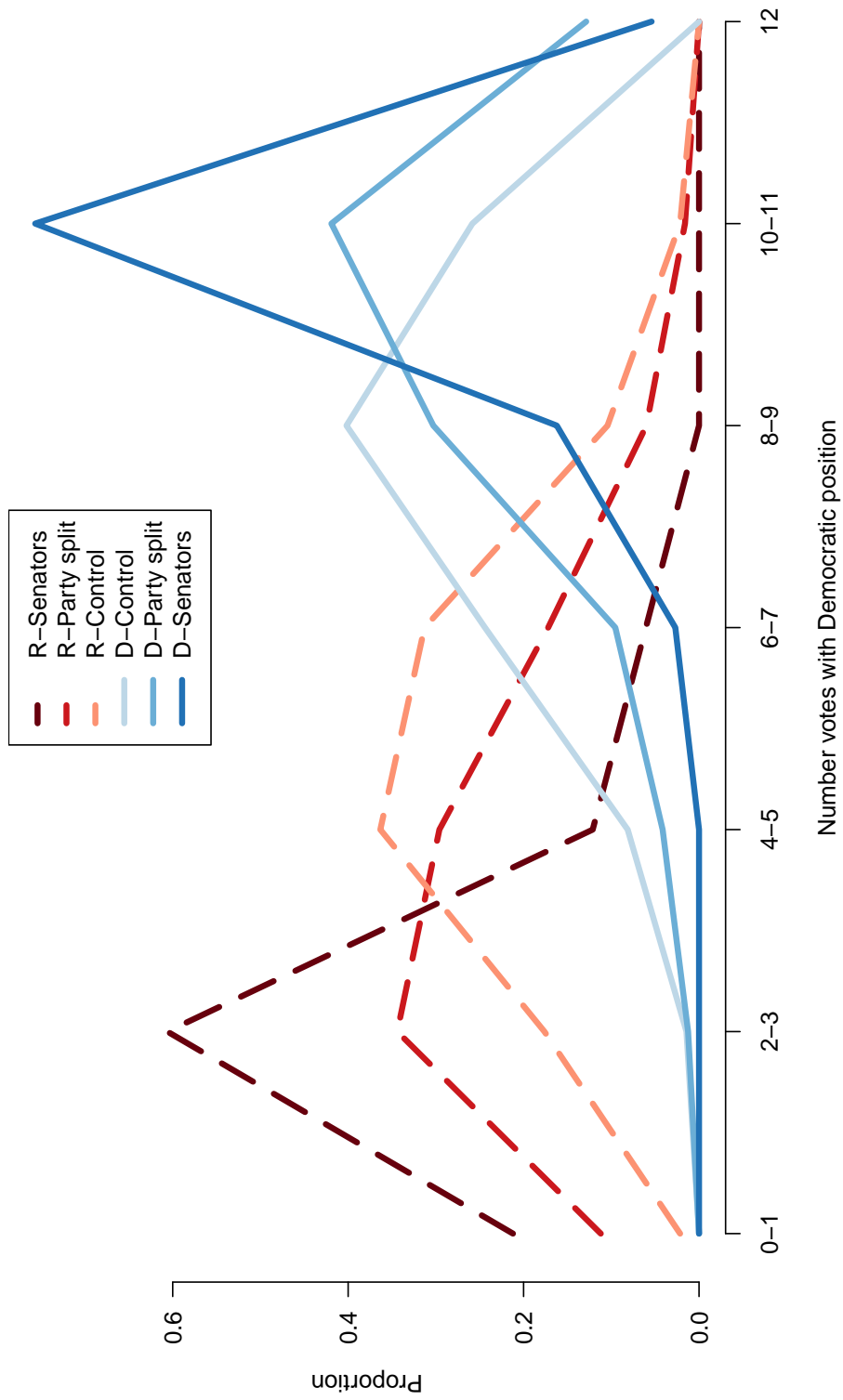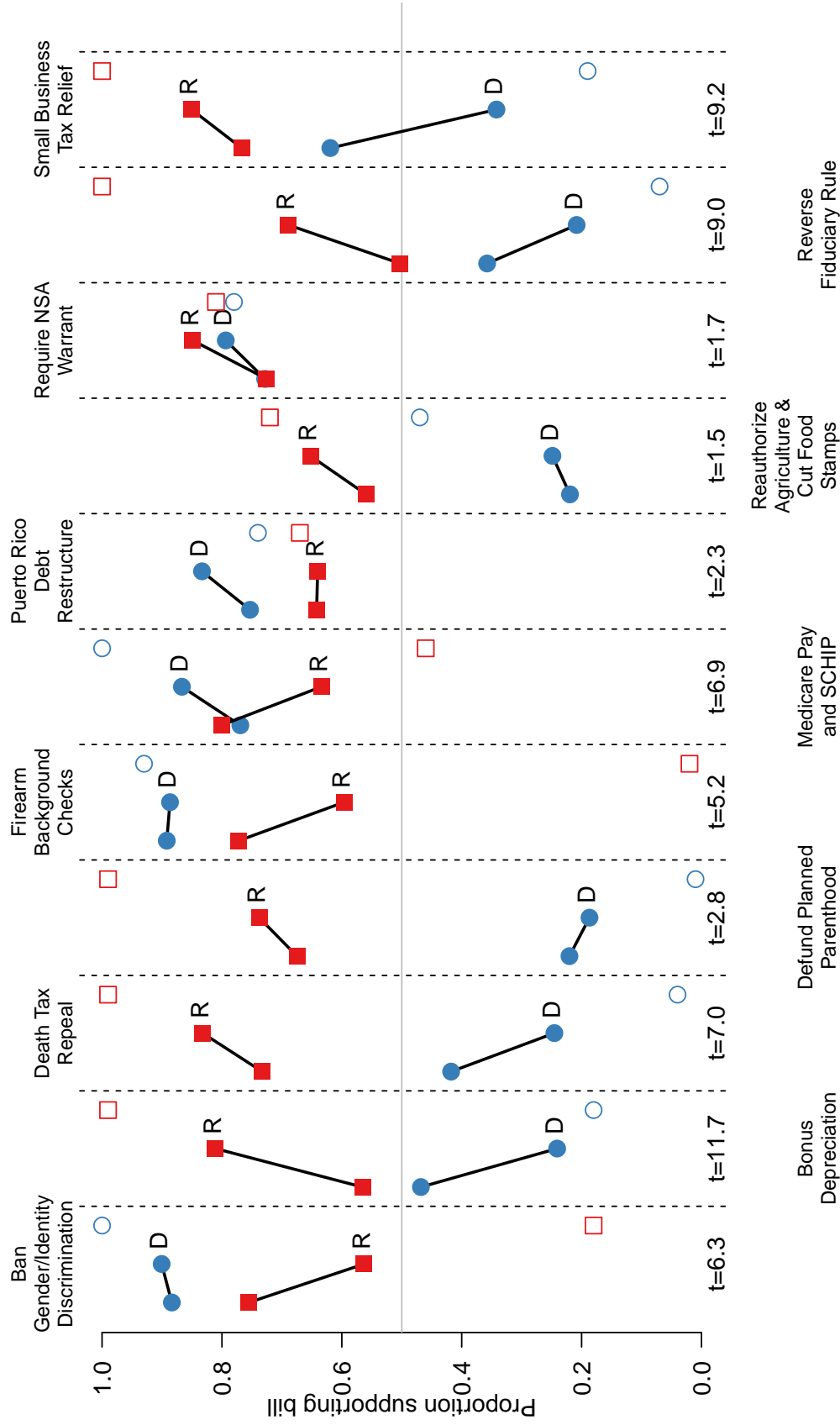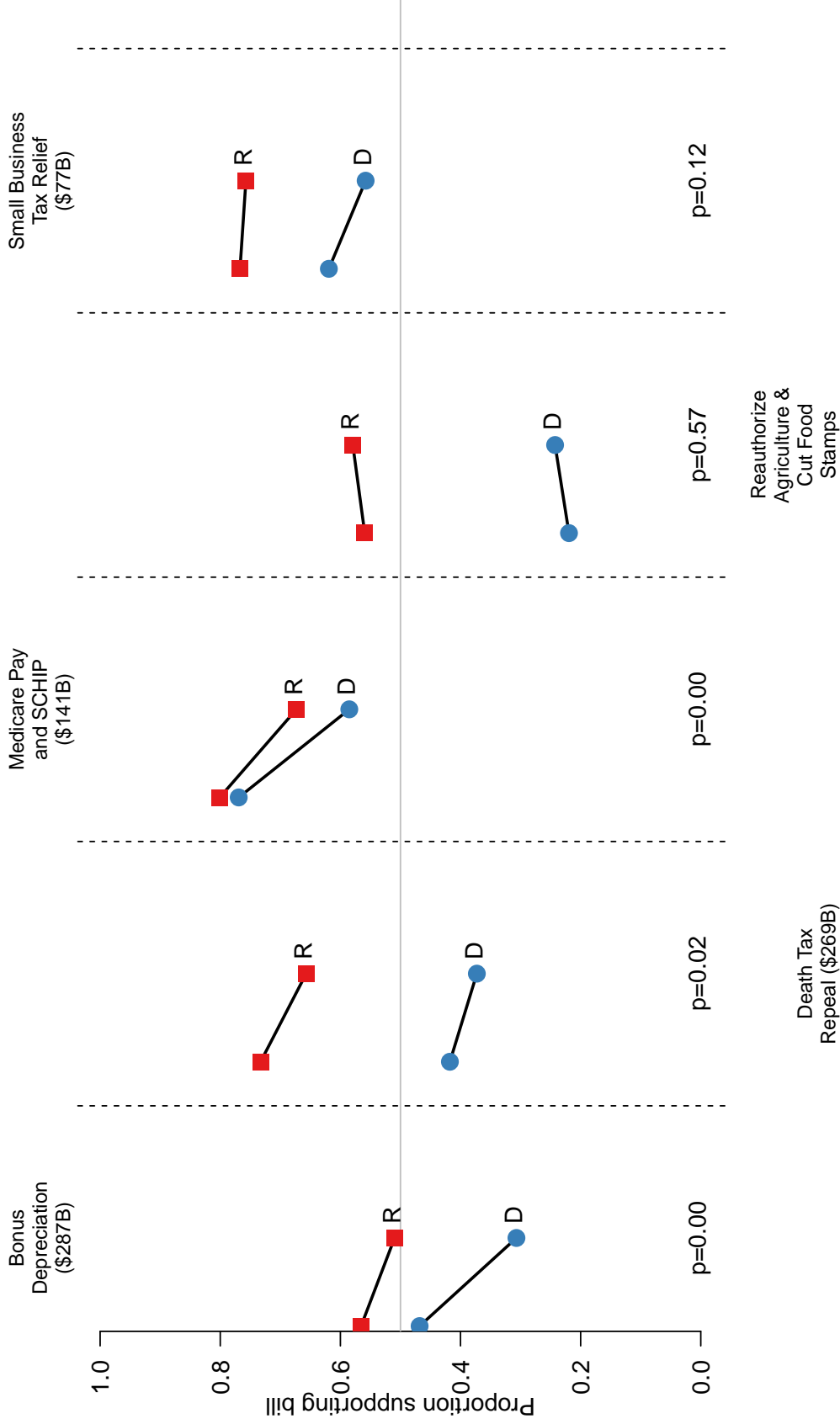
Note: Closed circles (squares) connect support among Democratic (Republican) respondents for bill from those in control condition (left) to those in the CBO condition (right). CBO condition presents respondents with a synopsis of the Congressional Budget Office analysis of the legislation. Number in parenthesis is the deficit impact calculated by the CBO. There was no deficit impact presented in the analysis to the Agricultural authorization. P-value for an F-test on the treatment and treatment times party regression coefficients to evaluate significance of treatment indicated at x-axis

Figure A15: Relationship of actual party splits to respondent beliefs, Study 3 Unweighted
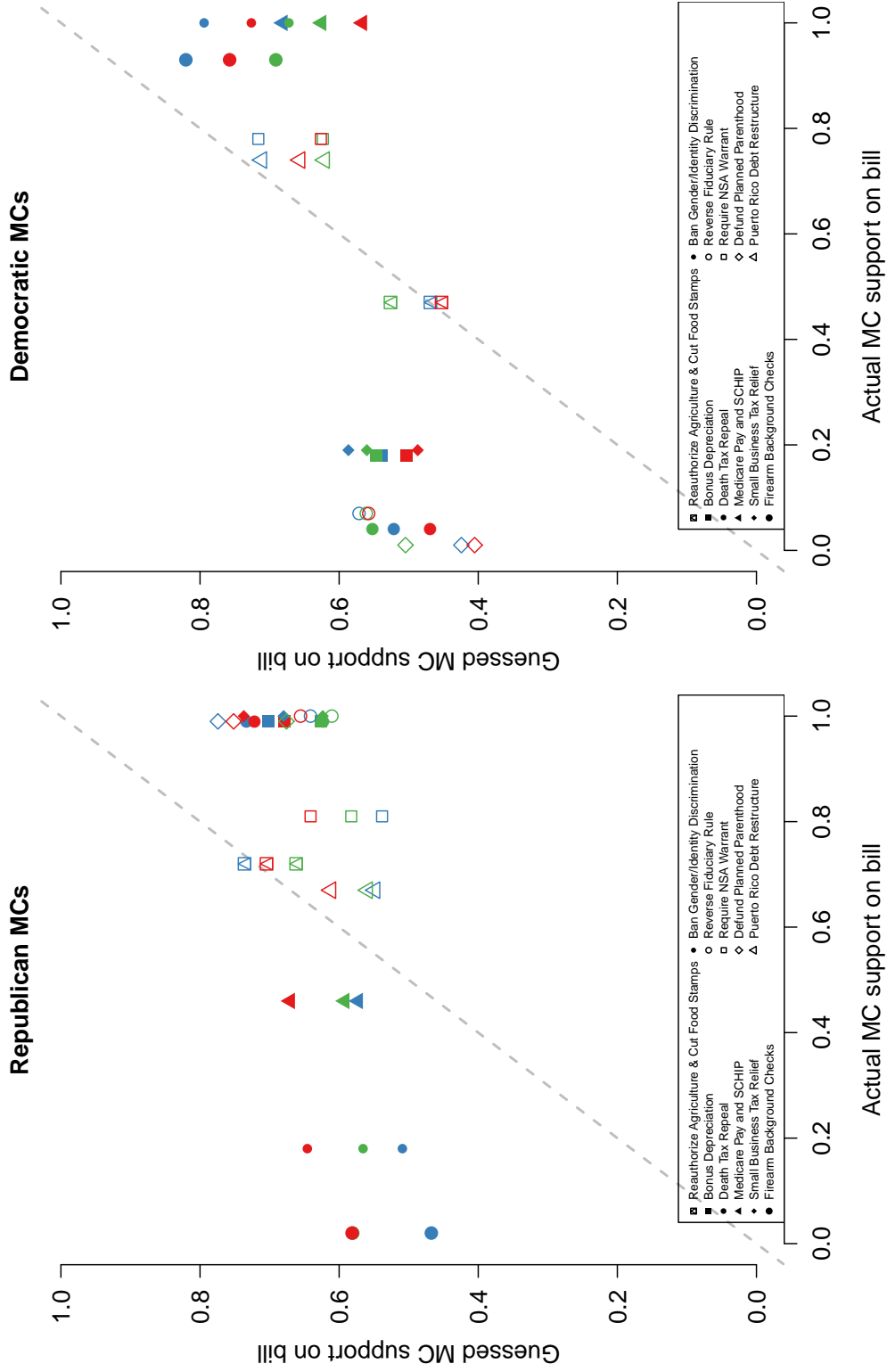
Note: Each frame plots the relationship between actual party split in Congress (x-axis) and Lucid respondent beliefs about the party split (y-axis). The three points above each location on the x-axis correspond to the guess on the party split by Democrats, Republicans, and Independents, respectively. The figure shows respondents were not well informed about the splits on these bills, on average – points fall far from the dashed 45 degree line.

Table A11: Policy confidence attenuates treatment effect of information, Study 2 Unweighted

VARIABLES	(1) All	(2) Dems	(3) Reps	(4) All	(5) Dems	(6) Reps
Average confidence in control condition by party	-0.027	-0.062	0.11	0.013	-0.073	0.076
	(0.10)	(0.08)	(0.26)	(0.10)	(0.10)	(0.24)
Average importance in control condition by party				-0.065	0.014	-0.13
				(0.05)	(0.06)	(0.08)
Republican respondent	0.022			0.014		
	(0.03)			(0.03)		
Constant	0.14	0.19	-0.030	0.23	0.18	0.29
	(0.15)	(0.13)	(0.38)	(0.16)	(0.15)	(0.40)
Observations	22	11	11	22	11	11
R-squared	0.032	0.056	0.018	0.111	0.061	0.247

Standard errors in parentheses
** p<0.01, * p<0.05

Note: OLS coefficients. Dependent variable is absolute value of treatment effect of providing party split on roll call support by party and bill.

Table A12: Roll call vote treatment effects, Study 1 Unweighted

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	All Bipartisan Budget Bill of 2013			All End Government Shutdown and Raise Debt Ceiling 2013			All Keystone Pipeline			All Lowering Gasoline Prices to Fuel an America That Works Act of 2014		
VARIABLES		Dems	Reps		Dems	Reps		Dems	Reps		Dems	Reps
Party split condition	0.0055	0.014	0.038	0.027	0.056	-0.048	-0.044	-0.10**	0.081*	-0.034	-0.059	0.0088
	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)	(0.02)	(0.04)	(0.03)	(0.02)	(0.04)	(0.03)
Democrat	0.17**			0.32**			-0.14**			-0.25**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	0.029			-0.18**			0.29**			0.14**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Constant	0.64**	0.81**	0.66**	0.45**	0.75**	0.30**	0.58**	0.46**	0.83**	0.77**	0.53**	0.89**
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)
Observations	1,391	637	478	1,447	665	514	1,464	685	511	1,403	635	512
R-squared	0.030	0.000	0.002	0.205	0.004	0.003	0.162	0.011	0.013	0.147	0.003	0.000

Standard errors in parentheses
** $p<0.01$, * $p<0.05$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	All Repeal of Affordable Care Act/Obamacare			All Simpson-Bowles Budget			All US-Korea Free Trade			All Violence Against Women Reauthorization Act of 2013		
VARIABLES		Dems	Reps		Dems	Reps		Dems	Reps		Dems	Reps
Party split condition	-0.010	-0.0081	0.043	-0.023	0.10**	-0.18**	-0.068*	-0.20**	0.075	-0.043	0.0096	-0.11*
	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)	(0.05)	(0.03)	(0.04)	(0.04)	(0.02)	(0.02)	(0.05)
Democrat	-0.32**			0.065			0.039			0.15**		
	(0.03)			(0.04)			(0.04)			(0.03)		
Republican	0.30**			0.0072			0.18**			-0.19**		
	(0.03)			(0.04)			(0.04)			(0.03)		
Constant	0.58**	0.26**	0.86**	0.39**	0.40**	0.46**	0.54**	0.64**	0.67**	0.77**	0.90**	0.61**
	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Observations	1,463	694	505	1,363	635	453	1,353	627	472	1,393	644	492
R-squared	0.309	0.000	0.004	0.004	0.010	0.035	0.028	0.040	0.006	0.126	0.000	0.012

Standard errors in parentheses
** $p<0.01$, * $p<0.05$

Table A13: Roll call vote treatment effects, Study 2 Unweighted

VARIABLES	(1) All Budget 2015 Dems	(2) Dems	(3) Reps	(4) All Defund Planned Parenthood Dems	(5) Dems	(6) Reps	(7) All End Shutdown Raise Debt Limit Dems	(8) Dems	(9) Reps	(10) All Extend Unemployment Dems	(11) Dems	(12) Reps
Party split condition	0.066**	0.13**	0.020	0.0037	-0.0053	0.092*	0.091**	0.18**	0.047	-0.031	0.084*	-0.13**
	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)
Democrat	0.086**			-0.11**			0.25**			0.19**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	-0.096**			0.29**			-0.093**			-0.19**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Constant	0.62**	0.67**	0.54**	0.41**	0.31**	0.65**	0.41**	0.62**	0.34**	0.59**	0.72**	0.45**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	1,462	522	586	1,464	523	587	1,464	523	587	1,464	523	587
R-squared	0.033	0.020	0.000	0.125	0.000	0.010	0.103	0.041	0.002	0.110	0.010	0.019

Standard errors in parentheses
** p<0.01, * p<0.05

VARIABLES	(1) All Firearm Background Checks Dems	(2) Dems	(3) Reps	(4) All Gender Discrimination Dems	(5) Dems	(6) Reps	(7) All Highway Funding Ex-Im Bank Dems	(8) Dems	(9) Reps	(10) All Keystone Pipeline Dems	(11) Dems	(12) Reps
Party split condition	-0.067**	0.052	-0.18**	-0.078**	0.13**	-0.24**	0.015	-0.11**	0.15**	-0.043	-0.17**	0.10**
	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.02)	(0.04)	(0.03)
Democrat	0.058			0.13**			-0.0066			-0.097**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	-0.14**			-0.21**			0.013			0.24**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Constant	0.80**	0.80**	0.72**	0.73**	0.75**	0.60**	0.63**	0.68**	0.57**	0.56**	0.53**	0.73**
	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)
Observations	1,464	523	587	1,464	523	587	1,463	522	587	1,464	523	587
R-squared	0.044	0.005	0.036	0.102	0.026	0.057	0.001	0.013	0.024	0.099	0.029	0.016

Standard errors in parentheses
** p<0.01, * p<0.05

VARIABLES	(1) All Medicare Pay and SCHIP Dems	(2) Dems	(3) Reps	(4) All Repeal Obamacare Dems	(5) Dems	(6) Reps	(7) All Student Loan Interest Rates Dems	(8) Dems	(9) Reps	(10) All Transport Urban Devel Fund Dems	(11) Dems	(12) Reps
Party split condition	0.077**	0.088**	0.069	-0.0015	-0.019	0.050	0.029	-0.061	0.13**	-0.069**	0.056	-0.19**
	(0.02)	(0.03)	(0.04)	(0.02)	(0.04)	(0.03)	(0.02)	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)
Democrat	0.071*			-0.26**			-0.020			0.18**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Republican	-0.061*			0.29**			0.072*			-0.22**		
	(0.03)			(0.03)			(0.03)			(0.03)		
Constant	0.74**	0.81**	0.69**	0.55**	0.29**	0.81**	0.62**	0.65**	0.64**	0.64**	0.76**	0.48**
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Observations	1,464	523	587	1,464	523	587	1,463	523	586	1,464	523	587
R-squared	0.029	0.016	0.006	0.233	0.000	0.005	0.009	0.004	0.021	0.128	0.005	0.040

Standard errors in parentheses
** p<0.01, * p<0.05

Table A14: Roll call vote treatment effects, Study 3 Unweighted

VARIABLES	(1) All Bonus Depreciation	(2) Dems	(3) Reps	(4) All Death Tax Repeal	(5) Dems	(6) Reps	(7) All Medicare Pay and SCHIP	(8) Dems	(9) Reps	(10) All Reauthorize Agriculture and Cut Food Stamps	(11) Dems	(12) Reps	(13) All Small Business Tax Relief	(14) Dems	(15) Reps
Chamber split condition	0.038	-0.0038	0.10**	0.018	0.025	0.010	0.013	0.0076	0.034	0.035	0.033	0.034	0.033	0.027	0.027
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
CBO condition	-0.11**	-0.16**	-0.056	-0.055**	-0.045	-0.076*	-0.15**	-0.18**	-0.13**	0.032	0.023	0.020	-0.053**	-0.061*	-0.0098
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Party split condition	-0.015	-0.23**	0.25**	-0.018	-0.17**	0.099**	-0.019	0.098**	-0.17**	0.049**	0.029	0.093**	-0.095**	-0.28**	0.083**
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)
Democrat	-0.052**			-0.12**			0.069**			-0.098**			-0.047*		
	(0.02)			(0.02)			(0.02)			(0.02)			(0.02)		
Republican	0.18**			0.24**			0.065**			0.25**			0.18**		
	(0.02)			(0.02)			(0.02)			(0.02)			(0.02)		
Constant	0.46**	0.47**	0.57**	0.51**	0.42**	0.73**	0.72**	0.77**	0.80**	0.31**	0.22**	0.56**	0.63**	0.62**	0.77**
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	4,502	1,994	1,606	4,507	1,997	1,607	4,502	1,998	1,605	4,503	1,996	1,608	4,508	1,996	1,609
R-squared	0.054	0.039	0.049	0.107	0.021	0.016	0.024	0.045	0.034	0.108	0.001	0.005	0.057	0.049	0.007

Standard errors in parentheses
** p<0.01, * p<0.05

VARIABLES	(1) All Ban Gender Identity Discrimination	(2) Dems	(3) Reps	(4) All Defund Planned Parenthood	(5) Dems	(6) Reps	(7) All Firearm Background Checks	(8) Dems	(9) Reps	(10) All Puerto Rico Debt Restructure	(11) Dems	(12) Reps	(13) All Require NSA Warrant	(14) Dems	(15) Reps	(16) All Reverse Fiduciary Rule	(17) Dems	(18) Reps
Chamber split condition	-0.017	-0.037*	-0.0039	0.0047	-0.014	0.043	-0.060**	-0.060**	-0.079**	0.029	0.032	0.040	0.075**	0.072**	0.085**	0.062**	0.045	0.075*
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)
Party split condition	-0.070**	0.017	-0.19**	0.016	-0.033	0.063*	-0.075**	-0.0054	-0.18**	0.032	0.080**	-0.0017	0.071**	0.066**	0.12**	0.015	-0.15**	0.19**
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)
Democrat	0.13**			-0.16**			0.12**			0.19**			0.072**			-0.075**		
	(0.02)			(0.02)			(0.02)			(0.02)			(0.02)			(0.02)		
Republican	-0.047**			0.33**			-0.054**			0.056**			0.088**			0.17**		
	(0.02)			(0.02)			(0.02)			(0.02)			(0.02)			(0.02)		
Constant	0.77**	0.88**	0.76**	0.36**	0.22**	0.67**	0.79**	0.89**	0.77**	0.58**	0.75**	0.64**	0.65**	0.73**	0.73**	0.39**	0.36**	0.50**
	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Observations	4,511	2,003	1,608	4,508	2,000	1,610	4,510	2,003	1,610	4,510	2,002	1,608	4,511	2,004	1,609	4,513	2,003	1,610
R-squared	0.044	0.004	0.034	0.201	0.001	0.004	0.043	0.006	0.026	0.029	0.006	0.001	0.013	0.007	0.017	0.049	0.025	0.024

Standard errors in parentheses
** p<0.01, * p<0.05

Table A15: Supreme Court vote treatment effects, Study 3 Unweighted

VARIABLES	(1) All Allow Ban Partial Birth Abortion	(2) All Allow Commitment of Sex Offenders	(3) All Allow Lethal Injection	(4) All Allow Local Handgun Bans	(5) All Allow Military Tribunals	(6) All Allow Photo ID Requirement	(7) All Allow Municipal Employee Diversity	(8) All Private Religious Symbols Public Land	(9) All Restrict Corporate Political Contributions
Party split condition	-0.0062	0.081**	0.0047	-0.0091	-0.042*	-0.035	0.077**	0.016	-0.026
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Democrat	-0.15**	-0.021	-0.075**	0.19**	-0.047	-0.12**	0.083**	-0.066*	0.11**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Republican	0.17**	0.041	0.13**	-0.12**	0.29**	0.24**	-0.0095	0.25**	-0.023
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	0.42**	0.53**	0.67**	0.34**	0.22**	0.64**	0.14**	0.29**	0.60**
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.03)
Observations	1,823	1,807	1,864	1,796	1,842	1,864	1,832	1,798	1,802
R-squared	0.086	0.010	0.041	0.083	0.122	0.117	0.022	0.089	0.019

Standard errors in parentheses
** p<0.01, * p<0.05

Democrats

VARIABLES	(1) Dems	(2) Reps	(3) Dems	(4) Reps	(5) Dems	(6) Reps	(7) Dems	(8) Reps	(9) Dems
Party split condition	-0.043	0.11**	0.032	0.063	-0.059*	-0.020	0.15**	-0.0064	0.024
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Constant	0.28**	0.49**	0.58**	0.50**	0.18**	0.51**	0.18**	0.23**	0.68**
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Observations	826	823	857	828	821	843	841	816	820
R-squared	0.002	0.011	0.001	0.004	0.007	0.000	0.030	0.000	0.001

Standard errors in parentheses
** p<0.01, * p<0.05

Republicans

VARIABLES	(1) Reps	(2) Reps	(3) Reps	(4) Reps	(5) Reps	(6) Reps	(7) Reps	(8) Reps	(9) Reps
Party split condition	0.069	0.080*	-0.019	-0.10**	-0.023	-0.037	0.018	0.076	-0.11**
	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)
Constant	0.55**	0.57**	0.81**	0.27**	0.49**	0.88**	0.16**	0.51**	0.61**
	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)
Observations	636	654	666	634	677	651	634	630	646
R-squared	0.005	0.007	0.001	0.016	0.001	0.003	0.001	0.006	0.012

Standard errors in parentheses
** p<0.01, * p<0.05

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