PARTISAN BIAS IN FACTUAL BELIEFS ABOUT POLITICS

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ABSTRACT

Partisanship seems to affect factual beliefs about politics. For example, Republicans are more likely than Democrats to say that the deficit rose during the Clinton administration; Democrats are more likely to say that inflation rose under Reagan. What remains unclear is whether such patterns reflect differing beliefs among partisans or instead reflect a desire to praise one party or criticize another. To shed light on this question, we present a model of survey response in the presence of partisan cheerleading and payments for correct and "don't know" responses. We design two experiments based on the model's implications. The experiments show that small payments for correct and "don't know" answers sharply diminish the gap between Democrats and Republicans in responses to "partisan" factual questions. Our conclusion is that the apparent gulf in factual beliefs between members of different parties may be more illusory than real. The experiments also bolster and extend a major finding about political knowledge in America: like others, we show that Americans have limited knowledge about political facts, but we also show that many Americans recognize their own lack of knowledge.

A persistent pattern in American public opinion is the presence of large differences between Democrats and Republicans in statements of factual beliefs. Partisan divisions are expected for questions about political tastes, but they extend even to evaluations of economic trends during a president's tenure (Bartels 2002, 133-38). What do these differences mean? One view is that Democrats and Republicans see "separate realities" (Kull et al. 2004), with differences arising because of partisanship's effect as a "perceptual screen" in information acquisition and processing (e.g., Gerber, Huber, and Washington 2010; Campbell et al. 1960, esp. ch. 8). By this account, scholars and commentators are correct to take survey respondents' statements at face value (e.g., Bartels 2002; Shapiro and Bloch-Elkon 2008; Jerit and Barabas 2012), because those statements reveal respondents' beliefs. Partisan differences in responses to questions about important facts therefore raise concerns about polarization in the mass electorate. Such differences also threaten defenses of democracy that are based on retrospective voting (Fiorina 1981): voters may be unable to hold elected officials accountable for their performance in office if even their views of economic performance are colored by their partisanship (see also Healy and Malhotra 2009).

An alternative view is that survey responses are not entirely sincere. Instead, they may reflect the expressive value of offering survey responses that portray one's party in a favorable light (Brennan and Lomasky 1997; Hamlin and Jennings 2011; see also Schuessler 2000). Partisan divergence in surveys may therefore measure the joy of partisan "cheerleading" rather than sincere differences in beliefs about the truth. Furthermore, divergence in expressed survey responses may occur under two conditions: when partisans are aware that their responses are inaccurate, or when they understand that they simply don't know the truth. In either case, partisan differences in factual assessments would be of less concern than is suggested by prior work, because survey responses would not reveal actual beliefs about factual matters. Despite this possibility, almost no research has attempted to determine the extent to which partisan divergence in responses to factual questions reflects sincere differences in beliefs.

This paper reports results from two novel experiments designed to distinguish sincere from expressive partisan differences in responses to factual survey questions. We motivate our experiments with a model in which respondents value both partisan responding and incentives for correct and "don't know" responses. The

model shows that incentives can reduce partisan divergence when expressive responding would otherwise mask shared (i.e., bipartisan) beliefs about factual matters. In both experiments, all subjects were asked factual questions, but some were given financial incentives to answer correctly. We find that even small incentives reduce partisan divergence substantially—on average, by about 55% and 60% across the questions for which partisan gaps appear when subjects are not incentivized.

Our model also reveals that incentives for correct responses may not deter cheerleading among those who recognize that they don't know the correct response. Even when paid to answer correctly, those who are unsure expect to earn less for offering the response that they think is most likely to be correct (relative to those who are sure of the correct response), and so they are more likely to continue offering an expressive partisan response. In our second experiment, we therefore offer to pay some participants both for correct responses and a smaller amount for admitting that they did not know the correct response. We find that large proportions of respondents choose "don't know" under these conditions. Furthermore, partisan gaps are even smaller in this condition—about 80% smaller than for unincentivized responses. This finding shows that partisan divergence in responses to these questions is driven by expressive behavior *and* by respondents understanding that they do not actually know the correct answers to important factual questions. To the best of our knowledge, this is the first analysis which demonstrates that people are aware of their own ignorance of political facts.[1]

These results speak to questions about the meaning of public opinion and the mechanisms through which partisanship affects important outcomes. Most importantly, they call into question the claim that partisan divergence in expressed beliefs about factual matters is cause for concern about voters' abilities to judge incumbent performance. To the extent that factual beliefs are determined by partisanship, paying partisans to answer correctly should not affect their responses to factual questions. But it does. We find that even modest payments substantially reduce the observed gaps between Democrats and Republicans, which suggests that Democrats and Republicans do not hold starkly different beliefs about many important facts. It

[1] In this regard, the most closely related work is Bishop, Oldendick, and Tuchfarber (1984) and Luskin and Bullock (2011).

also suggests that, when using survey data to understand *why* people make the political choices that they do, analysts should be cautious in interpreting correlations between factual assessments and those choices. Survey responses to factual questions may not accurately reflect beliefs, and the correlation between vote choice and factual assessments (of candidates or political conditions) observed in surveys may be in part artifactual.[2] Thus, even if partisanship is a crucial influence on votes and other political outcomes (Gerber, Huber, and Washington 2010), it may operate more through its effects on tastes than through its effects on perceptions of reality.

These results also affect our interpretation of partisan polarization in the mass electorate. Republicans and Democrats do hold different factual beliefs, but their differences are likely not as large as naïve analysis of survey data suggests. Just as people enjoy rooting for their favorite sports teams and arguing that their teams' players are superior, even when they are not, surveys give citizens an opportunity to cheer for their partisan teams (Green, Palmquist, and Schickler 2002). Deep down, however, many individuals understand the true merits of different teams and players—or, at minimum, they understand that they don't know enough to support their expressive responding as correct. And while our experimental approach cannot be used to discern whether partisan divergence in attitudes is sincere, an implication of our work is that if respondents misstate their factual beliefs in surveys because of their partisan leanings, they may misstate their attitudes in surveys for the same reason. We return to this point in the discussion section.

Our work is also of significance for survey methodology. In particular, how should one interpret experiments which show that partisan cues increase partisan divisions in survey response? Such results are commonly taken to show that partisanship affects attitudes (e.g., Cohen 2003). Our results raise the possibility, however, that partisan cues merely remind participants about the expressive utility that they gain from offering partisan-friendly survey responses. One implication is that studies in which partisan cues bring about partisan variation in survey response may not be showing that partisanship alters actual attitudes or

[2] Our results confirm concerns in the literature on economic voting (e.g., Ansolabehere, Meredith, and Snowberg 2013) that survey reports of economic conditions may be contaminated by expressive partisan responding.

beliefs. A key task for researchers is thus to understand when survey responses reflect real attitudes and when they reflect more expressive tendencies.

On the whole, then, understanding whether polarization in survey responses is real or artificial speaks to core concerns of subfields throughout political science, for scholars who rely on survey methods to study attitudes and behavior and for those who are interested in polarization in mass attitudes. Finally, it speaks to political psychologists because it bears directly on the connection between partisan identity and perceptions of political reality.

THEORY AND PRIOR EVIDENCE

Prior research documents partisan differences in expressed factual beliefs (e.g., Jerit and Barabas 2012; Gaines et al. 2007; Jacobson 2006), and some of it focuses on differences in evaluations of retrospective economic conditions (e.g., Conover, Feldman, and Knight 1986, 1987; Bartels 2002, 133-38).[3] Many of these differences arise because members of one party issue economic assessments that deviate starkly from objective conditions. For example, despite the large improvement in unemployment and inflation during Reagan's presidency, Bartels (2002) shows that, in 1988, Democrats were especially likely to report that unemployment and inflation had increased since 1980. This pattern was reversed in 2000, when Republicans were more likely to offer negative retrospective evaluations.[4]

How should we interpret these partisan gaps? Bartels presents one common view when he argues that partisans likely believe their divergent assessments: "Absent some complicated just-so story involving stark differences in the meaning of 'unemployment' and inflation…these large differences can only be interpreted as evidence of partisan biases in perceptions" (Bartels 2002, 136-37). An alternative view is that differences

[3] A related but distinct literature concerns partisan differences in responses to non-factual questions (see Berinsky 2012).

[4] Additional work examines conditions that can exacerbate apparent partisan gaps. Asking political questions prior to economic ones increases the correlation between partisanship and subjective economic evaluations (Lau, Sears, and Jessor 1990; Palmer and Duch 2001; Sears and Lau 1983; Wilcox and Wlezien 1993), and partisan gaps are larger when elections are more salient (Lavine, Johnston, and Steenbergen 2012, ch. 5; see also Stroud 2008). As we note above, what is unclear is how to interpret these patterns. Do circumstances that make partisanship more salient call relevant information to mind, or do they simply increase the expressive value of partisan responses?

in survey responses are the result of a combination of motivations. Individuals may offer responses that are consistent with their partisanship not solely because they believe those responses, but also because doing so gives them the opportunity to support their "team" (e.g., Gerber and Huber 2010; Green, Palmquist, and Schickler 2002).

Many social scientists have wrestled with the problem of insincere survey responses (e.g., Berinsky 2005). But they typically focus on responses to sensitive topics like race rather than on problems that may be caused by "expressive benefits" in survey response.[5] And the methods used to overcome problems associated with responses to sensitive topics—for example, "list experiments" (Kuklinski, Cobb, and Gilens 1997)—may not apply to the problem of eliciting sincere responses when people derive expressive benefits from answering insincerely.

Instead, scholars have long used incentives to elicit honest or rational responses. In a review of experiments involving incentives, Morton and Williams (2010, 358-61) argue that incentives often reduce the size and frequency of decision-making errors. But almost all of the studies that they review are apolitical and do not involve tests of factual knowledge. Prior and Lupia (2008) do study the effect of financial incentives on responses to factual questions about politics, and they find that the effects are real but weak.[6] However, they do not examine the effects of incentives on partisan patterns in responding.

To date, only Prior (2007) and Prior, Sood, and Khanna (2015) have examined the effects of incentives on partisan response patterns to factual questions about politics. Prior (2007) asked subjects 14 questions about politics; some were assigned at random to receive $1 for each correct answer. The results were mixed, but they suggest that $1 incentives can reduce party differences in responses to such questions.[7] Prior, Sood, and Khanna (2015) present two experiments in which they urge people to answer correctly or

[5] An exception to this characterization is the literature on economic voting discussed above.

[6] All subjects in the Prior and Lupia (2008) study were asked 14 factual questions about politics. Subjects in a control condition averaged 4.5 correct answers, while those who were paid $1 for each correct answer averaged 5.0 correct answers (Prior and Lupia 2008, 175).

[7] In Prior (2007), incentives reduced partisan gaps in responses to four items. Results on a fifth item were mixed. Results were null for two other items. There was no partisan gap in the control group for three further items, and results for the remaining four items were not reported.

provide relatively large financial incentives ($1 or $2 for each correct response). Both treatments reduce errors in answers to questions about the performance of the U.S. economy during the George W. Bush administration. Across the two experiments, financial incentives appear to reduce the rate of error by about 40%; simply urging people to answer correctly may be still more effective. An important unanswered question from that work, however, is how respondents who do not know the correct answers should be expected to behave in the presence and absence of incentives for correct responses. It may be, for example, that partisan responses are insincere, but that respondents continue to offer them when given incentives because they do not know which other answer might be correct. If respondents could express their lack of knowledge about the truth, would partisan gaps be even smaller?

To address these questions, we present a model of survey response which incorporates the possibility that individuals (a) receive utility from offering partisan-tinged responses and (b) differ in their underlying knowledge of the truth. We use this model to understand the effect of incentives on a respondent's tendency to answer questions in a manner that reflects either her partisan affinity or her beliefs about the truth. We also show that our model can be used to understand the extent to which partisan differences arise because individuals are uncertain about the truth.

A THEORY OF EXPRESSIVE SURVEY RESPONSE

To explore the role that insincere "cheerleading" plays in the partisan polarization of survey responses, and to motivate our experimental design, we present in the appendix a formal model of responses to factual questions in the presence and absence of financial incentives. As in our experiments, incentives take two forms: respondents may be paid for offering the correct answer or for admitting that they don't know the correct answer. We present here a summary of results from the model.

The first results show that incentives for correct responses reduce partisan divergence under three conditions: (1) participants would give inaccurate, partisan-tinged responses in the absence of incentives; (2) the value of the incentive is greater than the value of partisan cheerleading; and (3) the same strong beliefs

about the correct answer are held by members of both parties.[8] The intuition for this result is straightforward: giving a response that one does not believe but that portrays one's party in a favorable light is more costly when it entails giving up the chance to earn a reward for answering correctly. Therefore, under the conditions listed above, a researcher can reduce partisan divergence and elicit responses more informative of people's true beliefs by offering incentives to answer correctly.

The third condition—if incentives for correct responses are to reduce partisan divergence, members of different parties must share the same belief about the truth—requires elaboration. This condition is an implication of our model, not an assumption that underpins it. There are surely cases in which members of different parties hold different beliefs about the truth. In these cases, paying them to answer truthfully will not cause their survey responses to converge. On the other hand, to the extent that payments for correct answers do cause partisans' survey responses to converge, we can infer that partisans' beliefs about the correct answers are more similar than they seem to be under normal survey conditions.

An alternative interpretation of partisan convergence when people are paid for correct answers does not imply that they "know" the correct answers with much confidence. Instead, it suggests that partisan differences arise because of "congenial inference": when trying to answer a question under ordinary conditions, partisans are especially likely to call to mind those considerations that put their own party in a favorable light, and they infer the correct answer to the question at hand from this congenial set of considerations (e.g., Zaller 1992, ch. 5). But payment for correct answers heightens the desire to provide a correct answer. In turn, respondents who are paid for correct answers undertake a more even-handed (and perhaps more effortful) search of their memory for relevant considerations. They make more accurate inferences on the basis of this different set of considerations—even though they were not at all sure of the correct answer before the question was asked. In this paper, we are agnostic about which mechanism better explains the effects of payment for correct answers. We suspect that both mechanisms are at work.

[8] If members of different parties have different underlying beliefs about the truth, there is no strong reason to expect that responses in the presence of incentives will be less divergent than in the absence of those incentives. Additionally, it may be that only members of one party change their responses in the presence of incentives, in which case divergence will be reduced only if members of that party move in the direction of the other party's responses.

In addition to identifying the conditions under which incentives promote partisan convergence, our model highlights a little-appreciated explanation for divergent factual responses: even when partisans are paid for correct responses, their answers may diverge because they are unsure of the correct response and therefore default to an expressive response. To see how uncertainty can increase partisan divergence, note that the expected value of an uncertain respondent's best guess is discounted by her uncertainty. If she is sufficiently uncertain, the expected value of her best guess may be smaller than the expected value of partisan cheerleading. At the extreme, if there are two answers to a question and she is completely uncertain about which response is correct, in expectation she earns the incentive for a correct response half the time for offering either response and therefore has no reason to deviate from her preferred partisan response. This will be true even if the incentives are very large.

In light of this ambiguity, we extend the model by incorporating incentives for admitting one's lack of knowledge. When respondents are paid for both correct and "don't know" answers, our analysis shows that the proportion of respondents choosing "don't know" is increasing in the proportion who (1) place low value on partisan cheerleading relative to the incentive for choosing "don't know," *and* (2) are so unsure of the correct answer that they are better off choosing "don't know" than any other option. This is so because one can earn the incentive for a "don't know" response with certainty (by choosing "don't know"), whereas the incentive for a correct response is earned only if the respondent chooses the response that is correct. Overall, incentives for "don't know" responses allow us to understand the proportion of partisan divergence that arises because respondents default to expressive responding when they are unsure of the correct answer.

Our model implies that an experiment in which subjects receive incentives for correct and "don't know" responses to factual questions can identify the presence of partisan cheerleading. We now describe two experiments that meet these conditions.

EXPERIMENT 1: EFFECTS OF INCENTIVES FOR CORRECT RESPONSES ON PARTISAN DIVERGENCE

Our first experiment was fielded on the Cooperative Congressional Election Study in October 2008. CCES subjects are part of a nationally representative opt-in sample. In our experiment, 626 participants were

randomly assigned to the control group (N = 312) or the treatment group (N = 314). We restrict our analysis to the 419 participants who identified as either Democrats or Republicans.[9]

We told control-group subjects that they would be asked questions about politics, that they would have 20 seconds to answer each question, and that their scores would not be shared with anyone. Treated subjects received the same instructions and were told that answering correctly would increase their chance of winning a prize:

> For each question that you answer correctly, your name will be entered in a drawing for a $200 Amazon.com gift certificate. For example, if you answer 10 questions correctly you will be entered 10 times. The average chance of winning is about 1 in 100, but if you answer many questions correctly, your chance of winning will be much higher.

After receiving their instructions, all subjects were asked the twelve factual questions shown in Table 1.[10] The first ten items had closed (i.e., multiple-choice) response options and were similar to questions for which other research has found partisan differences. No "don't know" option was offered. Each question referred to a potentially salient partisan issue. The last two "placebo" questions were open-ended and required participants to enter numerical responses. We fielded the placebo questions, which were about obscure historical facts, to ascertain whether participants were using their allotted 20 seconds to look up answers using outside references. Using these questions, we find little evidence that participants "cheated": rates of correct responding were below 3% and statistically indistinguishable between the control and payment conditions.

This experiment allows us to understand whether some partisan divergence in responses to factual questions arises because of the expressive benefit of providing partisan responses. Specifically, we can learn

[9] In our analysis, Democrats are those who responded "Democrat" to the first question in the standard two-question measure of party identification. Republicans are those who responded "Republican." We discuss the behavior of partisan "leaners" below, and we present question wording for both experiments, along with further information about the construction of the sample, in the online appendix.

[10] We note that in the presence of ambiguity about which responses is correct, incentives should have weaker effects. For our purposes, what matters is not which answer is correct, but simply that partisans of different stripes have common beliefs about which answer is most likely to be correct.

about the role of expressive benefits by comparing partisan divergence in the treatment and control conditions. If divergence is lower in the treatment group, it suggests that, for some respondents, our incentives are of greater value than partisan cheerleading. Given the modest size of the incentives offered, we view the estimates that we obtain from treatment-control comparisons as lower bounds on the extent of expressive partisan responding in this experiment.

To measure partisan divergence, we create scale scores by coding responses to each question to range linearly from 0 to 1. These scores are the dependent variables in our analyses. The most Republican response to each question (either the largest or smallest response) is coded 0; the most Democratic response is coded 1. For example, when we ask about the change in unemployment under President Bush, the response "decreased" is coded 0 because it portrays a Republican president most positively, "stayed about the same" is coded .5, and "increased" is coded 1 because it portrays the president most negatively. If partisans are answering in a manner consistent with their partisanship, Democrats should offer "larger" responses than Republicans.

Table 1 shows the average partisan difference in scale score, by question, for those in the control group. Questions are ordered in Table 1 by the size of these control-group partisan gaps. For nine of the ten questions, the gaps are consistent with our expectations about patterns of partisan responding.[11] Eight of the differences are significant at $p < .10$ (one-tailed). The gaps among these eight items vary substantially in size, with the largest gaps appearing for questions about casualties in Iraq and Bush's economic performance. Because our theory of expressive responding is about the effects of incentives on partisan differences, we focus on these eight items, i.e., the items to which partisanship makes a difference under ordinary survey conditions. (In the online appendix, we analyze our data while including responses to all questions, including those for which we do not find partisan gaps.)

What effect do incentives for correct responses have on observed partisan divergence? To measure the effects, we estimate a model in which we predict scale score R for individual *i* and question *j:*

[11] The exception is the question about the change in the deficit under George W. Bush. For both Democrats and Republicans, 92% of respondents correctly reported the deficit had increased.

$$R_{ij} = b_0 + b_1 \text{Democrat}_i + b_2 \text{PayCorrect}_i + b_3(\text{Democrat}_i \times \text{PayCorrect}_i) + \text{Question}_j + e_i,$$

where *Democrat* equals 1 for Democratic participants and 0 for Republicans, *PayCorrect* equals 1 for those assigned to the incentive condition, and *Question* is a vector of question-specific fixed effects. The coefficient b_1 is therefore the average party difference in scale scores in the control condition, while $b_1 + b_3$ is the average party difference in the incentive condition. Prior research suggests $b_1 > 0$, while our theoretical model predicts that b_3 will be negative if partisans offer partisan-tinged responses in the absence of incentives, share common and sufficiently strong beliefs about the truth, and give less weight to partisan responding than to the expected value of the incentive.

OLS estimates, with standard errors clustered at the respondent level, appear in Table 2. Pooling across the eight questions for which we observe statistically significant partisan gaps in the control condition, column (1) provides estimates of the average effect of incentives on responses. The .118 ($p < .001$) coefficient for *Democrat* (b_1) is the average gap between Democrats and Republicans in the control condition. The –.065 ($p < .001$) coefficient for *Democrat* × *PayCorrect* (b_3) means that this gap is reduced to .053 (.118 – .065), or by 55%, when incentives are offered. In column (2), we add demographic controls; the results are nearly unchanged.[12]

In Table A1 of the appendix, we repeat the analysis for each question individually. The estimate for b_3 is negative in all eight cases. While most of these individual-question estimates are not statistically significant—perhaps because the impact of sampling variability is heightened when we examine individual questions—the estimates are large, accounting for between 13% and 100% of the partisan gap between Democrats and Republicans. These estimates are especially noteworthy for the questions about the most salient issues in the 2008 campaign: the Iraq War and Bush's performance on unemployment. On these matters, incentives reduced partisan gaps by between 33% and 74%. Importantly, these questions about war

[12] We have also repeated our analysis excluding the Bush approval item, which is the item for which we find our largest estimate of b_3. In this case, we continue to find a negative and statistically significant coefficient for b_3 in the pooled analysis (–.06, $p < .01$). Our analysis excludes cases in which participants didn't provide a response, which occurs 3% of the time in both treatment and control conditions. Replacing those responses with party averages for that question produces substantively similar results.

and unemployment were not only salient in 2008: they also speak to the issues that political scientists often use when they link objective conditions to election outcomes (e.g., Hibbs 2000).

These results show that even modest incentives can substantially reduce partisan divergence in factual assessments. For example, in this experiment, participants were told that answering correctly would improve their chances of earning a $200 gift certificate, and that the baseline chance of winning was around 1 out of 100. If they estimate that answering all questions correctly would double their chances of winning this prize, the expected value of answering any given question correctly is approximately 17 cents.[13] In turn, the finding that incentives reduced partisan gaps by more than 50% means that more than half of the party gap may be generated by participants for whom partisan responding to any given question is worth less than 17 cents.

Of course, the effects of incentives are unlikely to be equal across all of the people in our dataset. Fortunately, our data permit us to explore the extent to which the effects vary across different sorts of people. We focus on two characteristics across which variation might be expected: political interest and strength of partisanship. So far as interest is concerned, partisans who are most interested in politics may be most likely to engage in partisan cheerleading under ordinary survey conditions. In this case, they may be *more* affected than low-interest respondents by incentives for correct response. Another possibility, however, is that highly interested partisans are most likely to sincerely hold different factual beliefs about politics (e.g., Taber and Lodge 2000; Abramowitz and Saunders 2008). If they do, they may be *less* affected by incentives. The estimates presented in column (3) of Table 2 show that both accounts are informative. In the control group, partisan gaps are larger among high-interest respondents, i.e., those who report being "very much interested" in politics and current events: the average partisan gap is .14 for high-interest respondents, .08 for all others (whom we label "low-interest respondents"). The treatment reduces partisan gaps more for high- than for low-

[13] Suppose that respondents believe that (a) they will answer six of our twelve questions correctly if they simply respond in a partisan manner, and (b) answering six questions correctly will give them a 1-in-100 chance of winning $200. If they also believe that answering all twelve questions correctly will double their chances to two in 100, then the expected value of answering all 12 questions correctly, relative to the "baseline" of answering six correctly, is [($200 × 2/100) – ($200 × 1/100)]/12 questions = $0.167 per question. These calculations are speculative, because we did not verify how subjects interpreted the instructions. In our second experiment, the calculations are more straightforward, because subjects were given specific rewards on a question-by-question basis rather than entries in a lottery.

interest respondents—but only to an insignificant extent (–.08 versus –.06), and high-interest respondents in the treatment group remain more polarized than their low-interest counterparts. (The treatment-group partisan gaps are .06 for high-interest respondents and .03 for low-interest respondents). Thus, highly interested people are initially more polarized, and their slightly greater responsiveness to incentives is not enough to overcome their initially greater polarization. Political interest is associated with polarization, but it does not significantly moderate the effects of incentives.[14]

The analyses that we report above exclude partisan "leaners" who may identify with a party less strongly than other partisans. In the online appendix, we present parallel analyses that include leaners. The results are similar: partisan leaners appear to behave like those who identify more strongly with the major American political parties.

Treatment-effect heterogeneity aside, the main finding of Experiment 1 is that small incentives for correct answers reduce partisan gaps in responses to factual questions by about 55%. Of course, Experiment 1 cannot tell us why 45% of the partisan gap remains. Following our model, the people responsible for this gap may sincerely disagree about which response is correct. Or they may agree about the correct response but value partisan cheerleading more than giving a correct answer. Or they may be so uncertain about which response is correct that incentives for correct responses cannot offset the expressive value of partisan responding. To evaluate these explanations, we turn to our second experiment.

EXPERIMENT 2: EFFECTS OF INCENTIVES FOR CORRECT AND "DON'T KNOW" RESPONSES ON PARTISAN DIVERGENCE

We fielded our second experiment in 2012 using subjects recruited from Amazon.com's Mechanical Turk marketplace (Berinsky, Huber, and Lenz 2012). Subjects were required to pass a two-question attention screener and were then randomly assigned to a control group (N = 156) or to one of three treatment groups,

[14] 65% of our CCES subjects report being "very much interested" in politics and current events. By contrast, the corresponding percentage among partisans in the 2008 ANES is 38%. That said, the overrepresentation of the interested in the 2008 CCES does not seem to affect the results. See the online appendix for a discussion of this point.

two of which we examine here.[15] In the first treatment group, participants were paid for each correct response (N = 534). In the second treatment group, participants were paid for each correct response and each "don't know" response (N = 660). Below, we restrict our analysis to the 795 individuals in these three groups who identified as either Democrats or Republicans.[16]

There are two major differences between this experiment and Experiment 1. First, and of greatest importance theoretically, we introduce a new treatment here, in which we offer subjects a "don't know" response option and incentives for both correct and "don't know" responses. Therefore, unlike Experiment 1, Experiment 2 permits us to assess the extent to which partisan divergence that persists in the face of incentives for correct responses reflects self-aware ignorance, rather than partisan cheerleading or sincere differences in beliefs. Second, in both treatment conditions, we pay subjects for each correct response (instead of entering them into a lottery, as in Experiment 1), and we vary the amount offered for correct responses across participants. In the treatment that includes payment for "don't know" responses, we also vary the amount offered for that response across subjects. These randomizations allow us to assess the degree to which partisan divergence is affected by the size of incentives.[17]

As before, we gave subjects 20 seconds to answer each question to limit opportunities for consultation of outside information sources. In all conditions, participants were initially asked five questions that were selected at random from a larger list that we describe below. All questions had a closed response format without a "don't know" option. Subjects then received instructions that indicated how they would be paid for answers to the subsequent questions. They were then asked seven more questions: two new questions

[15] In the third treatment, we paid participants a flat fee to answer questions post-treatment, just as we did in the control group. However, in this condition, we also allowed respondents to offer "don't know" answers. 14.8% of responses in this condition were "don't know."

[16] We fielded a one-item replication of this experiment on the 2012 CCES. The item was an economic retrospection item similar to those that have been used in the past to document partisan divergence (e.g., Bartels 2002). The results were similar. See the online appendix for a discussion.

[17] As we discuss in the online appendix, one additional difference is that we used a graphical input device (slider/thermometer) to gather responses for this experiment. The advantage of this input device is that it allows subjects to provide responses continuously across the entire range of possible responses instead of requiring them to select one response from a small set of predefined options. The 2012 replication uses a traditional multiple-choice response set.

followed by the same five questions that they had previously been asked. (See the online appendix for details.) This design feature addresses one potential objection to our analysis of Experiment 1, which is that we use the control group in that experiment both to identify questions for which party gaps arise and to compare to the treatment group. In this experiment, by contrast, we use pre-treatment responses from all subjects to identify items for which partisan divergence arises, and we then compare post-assignment responses across treatment and control conditions.[18]

In the control condition, participants were paid a flat $0.50 bonus to answer those seven post-treatment questions. In the pay-for-correct (PC) condition, participants were informed that they would be paid for each correct response. The amount offered for each correct response was randomly assigned to be $0.10 (p = .25), $0.25 (p = .25), $0.50 (p = .25), $0.75 (p = .15), and $1.00 (p = .10). (These amounts varied only across subjects, not within subjects.) Finally, in the pay-for-correct-and-"don't know" (PCDK) condition, participants were again informed they would be paid for each correct response, and the amount offered for each correct response was assigned as in the prior treatment. Participants in this condition were also given "don't know" response options, and if they selected "don't know," they were randomly assigned to receive a fraction of the amount offered for a correct response: 20% of the payment for a correct response (p = 1/3), 25% (p = 1/3), and 33% (p = 1/3).

We list the 12 questions that we fielded in this experiment in Table 3, which also shows the correct response and the range of the response options that we offered. The correct responses varied across the entire range of potential answers: they were not concentrated at either end of the scale or in the middle. The effects of incentives therefore cannot be attributed to a tendency among treated subjects to offer middle-of-the-scale responses. The direction of partisan responding also varied: sometimes, responses at the higher end of the scale favored the Democratic Party; sometimes, they favored the Republican Party. As before, we fielded a

[18] In the online appendix, we also show that if we leverage this pre-post design (a within-person analysis) we find similar results to those focusing only on post-assignment comparisons across conditions.

placebo question to assess whether participants were consulting outside references, and we found little evidence of this behavior.[19] (See the online appendix.)

As with Experiment 1, we recoded all responses to range from 0 to 1, with 0 corresponding to the response that portrayed Republicans most favorably and 1 corresponding to the response that portrayed Democrats most favorably.[20] Table 3 reports, for each non-placebo question, the observed pre-treatment difference in mean scale scores between Democrat and Republican participants. (Recall that each participant was asked five pre-treatment questions.) We find statistically significant ($p < .10$, one-tailed) partisan gaps for ten of the eleven questions, with the largest gaps for questions about unemployment under Bush and Obama, and the smallest gaps for a question about the proportion of the population that is foreign-born. Our subsequent analysis is restricted to these ten questions, i.e., the questions to which partisanship makes a difference under ordinary survey conditions.[21] (Including all items produces similar results; see the appendix.)

The Effect of Incentives for Correct and "Don't Know" Responses

We begin by reporting the effect of the treatments on the frequency of selecting "don't know." Our model suggests that the rate at which participants select "don't know" when offered a payment for doing so indicates the degree to which they understand that they don't know the correct responses. In particular, if participants are sufficiently uncertain about the correct response and preferences for expressive partisan responding are

[19] In this experiment, subjects were explicitly asked after they had completed the entire experiment whether they had consulted any outside resources for an answer. (We told them that their pay would be unaffected by their answers to this question.) In the control condition, 1% of respondents reported consulting an outside reference, compared to 4% who reported doing so when paid $1.00 for a correct response. In the online appendix, we show that excluding all responses from any respondent who reported looking up the answer to any question produces highly similar results.

[20] We coded one end of the (continuous) input range at 0 and the other end at 1. Empirically, subjects use the entire scale range for all 10 questions. Our scaling implies that identical movements on the scale response range (e.g., 1 additional point of unemployment) are equivalent across the entire scale range.

[21] In pooled models, we assume movements across the scale range are on average the same across questions. As the units and endpoints of each question are different, this is a simplification for ease of presentation. While this is not a necessary assumption for our data analysis, we do not have strong *ex ante* theoretical reasons for presuming a different functional relationship for each question. We present a question-by-question analysis, which does not use this approximation, in Table A2 of the appendix.

not too large, then choosing "don't know" when paid to do so will yield greater expected utility than either expressive or sincere responses.

Pooling across the ten questions for which we found pre-treatment partisan gaps, we find that 48% of responses in the PCDK condition are "don't know." That is, nearly half of participants forgo a response that would allow them to support their party or give them a chance to earn the larger payment that we offered for a correct response. Recall that for "don't know" responses, participants were randomly assigned to receive 20%, 25%, or 33% of the payment that they received for correct responses. Across these conditions, "don't know" responses were given 46%, 47%, and 50% of the time, respectively. These percentages are ordered as the theoretical model predicts, but only the difference between the 20% and 33% conditions approaches statistical significance ($p < .07$, one-tailed).[22]

This pattern—frequent "don't know" responses when subjects are paid to give that response, even when they are also offered more for correct responses—implies that many participants are so uncertain about the correct answers that they expect to earn more by selecting "don't know." In this experiment, uniformly distributed blind guesses will be correct about 17% of the time. Subjects who are completely unsure of the correct answers can therefore receive, in expectation, 17% of the payment that we offer for correct answers just by guessing blindly. Yet, when we paid subjects just 20% of the correct-answer payment for "don't know" responses, 46% chose to say "don't know" rather than to guess. We therefore infer that many respondents are highly unsure of which response is correct and give low weight to partisan responding.

As in the previous section, we study the effect of the treatments on party polarization by examining whether post-treatment partisan gaps differ between the control and treatment conditions. Our analysis initially takes the following form:

[22] One concern is that respondents may choose "don't know" simply because it allows them to avoid thinking about the question altogether. In footnote 15, we show that when offered a "don't know" option without payment, only 15% of responses were "don't know," a much lower rate than in this condition. Of note, as our model shows, choosing "don't know" when also offered a payment for a correct response is only optimal if the respondent is uncertain enough about the correct answer that it makes sense to give up the chance to guess and potentially earn a much larger amount.

$$R_{ij} = b_0 + b_1\text{Democrat}_i + b_2\text{PayCorrect}_i + b_3\text{PayCorrectDK}_i + b_4(\text{PayCorrect}_i \times \text{Democrat}_i) + b_5(\text{PayCorrectDK}_i \times \text{Democrat}_i) + \text{Question}_j + e_i,$$

where *Democrat* = 1 for Democratic participants and 0 for Republicans, *PayCorrect* = 1 for those assigned to the PC condition, *PayCorrectDK* = 1 for those assigned to the PCDK condition, and *Question* is a vector of question-specific fixed effects.[23] In this specification, b_1 is the amount of partisan divergence in the control condition, while $b_1 + b_4$ is the gap in the PC condition, and $b_1 + b_5$ is the gap in the PCDK condition.[24]

Our model predicts that $b_1 > 0$, $b_4 < 0$, and $b_5 < 0$. That is, both treatments will reduce partisan divergence relative to the control condition. Additionally, our theoretical model suggests that some partisans who will not respond to incentives for correct responses will nonetheless respond to incentives for "don't know" responses. For this reason, we also predict $b_5 < b_4$ (a larger reduction of partisan differences in the PCDK condition than in the PC condition).

The first column of Table 4 reports OLS estimates of the equation. (Parallel analysis for each individual question appears in Table A2 of the appendix.) The estimate of b_1 is .145 ($p < .01$), which means that, on average, control-group Democrats and Republicans differ by about 15% of the range of the scale. The estimate of b_4 is –.087 ($p < .01$), so the total partisan gap in the PC condition is .058 (.145 – .087). In other words, only 40% of the previously observed party gap remains when participants are paid small amounts for correct responses. Despite the differences between Experiments 1 and 2 in subject pools, questions, and other respects, this effect is similar to the effect that we find in Experiment 1. And, like Experiment 1, this

[23] We have multiple observations from the same respondent, which is why we cluster our standard errors by respondent. To test whether this clustering is sufficient to account for the correlated nature of multiple responses by the same respondent, we have also collapsed the data (to one observation per respondent) and estimated an otherwise identical specification. The results are highly similar, and we present them in the online appendix.

[24] To incorporate "don't know" responses into our analysis of partisan divergence, we must decide where to place those responses on the 0-1 scale that we use to analyze other responses. Because participants who admit that they don't know thereby forgo the opportunity to express support for their party, we treat these responses as being non-polarized. That is, we assign both Democrats and Republicans who choose "don't know" to the same position on the 0-1 scale. In particular, we assign "don't know" responses for a given question to the average pre-treatment response that participants offered to that question. In practice, the specific value makes little difference to our analyses; the important point is that Democrats and Republicans are assigned to the same position on the scale if they say "don't know." If everyone chose "don't know," we would therefore find no differences between the parties.

experiment shows that analyses of ordinary survey responses are likely to overstate the true extent of partisan polarization.[25]

This experiment also allows us to estimate the effect of incentives for "don't know" responses on polarization. The estimate of b_5 is –.117 ($p < .01$), so the total partisan gap in the PCDK condition is .028 (.145 – .117), or 80% smaller than the control-condition gap and about 50% smaller than the PC-condition gap. (These differences are significant at $p < .01$ and $p < .05$, respectively.) In practical terms, whereas the control-group difference between Democrats and Republicans was about 15% of the range of the scale, it shrinks to 3% of the range when we offer incentives for both correct and "don't know" responses.

In column (2) we estimate a Tobit specification because our response scales were bounded and unable to accommodate extreme responses. The estimates are similar to those shown in column (1). Indications of statistical significance do not change.

In column (3) we leverage the variation in incentive size to assess more fully the effect of differences in correct and "don't know" payments on observed divergence. Our specification includes indicators for each level of payment, each interacted with partisanship. The specification is highly flexible because it does not make assumptions about the functional form that relates incentive size to responses (e.g., a linear interaction between incentive size and responses).

Under this specification, the estimated .145 ($p < .05$) coefficient for *Democrat* is the average difference between Democrats and Republicans in the control condition. As expected, all five interactions between the amount paid for a correct response and *Democrat* are negative and statistically significant at $p < .10$, which means that party gaps are smaller when participants are offered incentives for correct responses. With one exception, larger payments are associated with smaller partisan gaps relative to the control group. For example, we estimate that partisan gaps are 56% smaller in the $0.10 payment condition than in the control group and 80% smaller in the $1.00 payment condition. The difference between the two

[25] As in Experiment 1, question-by-question analysis yields less precise estimates and reveals heterogeneity across topics. Incentives have their largest effects on responses to questions about unemployment under Obama and the racial composition of Iraq War casualties. They also have large effects on basic retrospective assessments, reducing average partisan divergence by 41% and 72% in responses to questions about unemployment under Bush and Obama, respectively. (See Table A2.)

coefficients (*Amount correct = $0.10 × Democrat* and *Amount correct = $1.00 × Democrat*) is marginally significant ($p < .10$, one-tailed test).

The third column of Table 4 also reports the effects of variation in the amount paid for "don't know" responses. All of the interactions between the fractional payment amounts and partisanship are in the expected negative direction, meaning that payments for "don't know" responses further reduce partisan gaps. For payments that are 20% or 33% as large as the payments for correct responses, the estimates are statistically significant at $p < .10$ (two-tailed), and the pooled estimate of the effect of "don't know" payments is significant at $p < .05$. To interpret these coefficients, one can fix the payment for a correct response at $0.10, in which case the estimated partisan gap is .063 (.145 – .082, $p < .01$). Adding the "don't know" payment is estimated to reduce this party gap by between .02 (a 25% reduction for a "don't know" payment of $0.025) and .04 (a 65% reduction for a payment of $0.033).

The ordering of the effects for the proportional payments is nonmonotonic. The largest reduction in partisan divergence is associated with the 33% payment for "don't know" responses, the next-largest reduction is associated with the 20% payment, and the smallest reduction is associated with the 25% payment. None of these estimates are statistically distinguishable from one another, perhaps reflecting the relatively small sample sizes in each condition. At the same time, the estimates imply that the combination of a $1.00 payment for a correct response and a $0.33 payment for a "don't know" response will eliminate *the entire* gap between Democrats and Republicans in responses to partisan factual questions.[26]

Taken as a whole, these results have two implications. First, as in Experiment 1, modest incentives for correct responses substantially reduce partisan gaps, which is consistent with these gaps being due partly to expressive responding rather than to sincere differences in beliefs. Second, at least half of the partisan divergence that remains in the presence of incentives only for correct responses appears to arise because people know that they do not know the correct response but continue to engage in expressive responding. On average, payments for correct responses in this experiment reduce partisan gaps by 60%. Adding "don't know" payments reduces partisan gaps by an additional 20 percentage points, leaving only 20% of the

[26] This calculation is .145 – .116 – .041, which is actually slightly smaller than 0.

original gap. This result implies that fully half of the remaining gap arose because participants were unaware of the correct response and understood their lack of knowledge. Indeed, the relatively high rate of "don't know" response (about 48%) reveals that a surprising number of respondents were aware that they lacked clear knowledge of partisanship-relevant facts.

EXPRESSIVE SURVEY RESPONSE AND THE RELATIONSHIP BETWEEN FACTS AND VOTES

Our experiments speak most directly to the role that partisan cheerleading plays in responses to factual questions about politics. But they also speak to the relationship between factual assessments and the political choices that people make. In particular, they suggest that efforts to understand the relationship between facts and votes with survey responses are likely to be biased in the absence of efforts to account for partisan cheerleading. To make this concern clear, we use Experiment 1 to assess the correlation between factual assessments and candidate preference in 2008. By comparing the correlations in the control and treatment conditions, we can understand whether the use of survey measures of economic perceptions to predict vote choice—a common practice in the literature on retrospective economic voting (e.g., Duch and Stevenson 2006)—leads to biased conclusions when those measures are affected by partisan cheerleading.

With the data from Experiment 1, we estimate

$$\text{PresVote}_i = b_0 + b_1\text{FactualAssessments}_i + b_2\text{PayCorrect}_i + b_3(\text{PayCorrect}_i \times \text{FactualAssessments}_i) + e_i,$$

where *PresVote* = 1 indicates an intended vote for Obama and *PresVote* = 0 indicates an intended vote for McCain. (We exclude from the analyses those who aren't registered, prefer other candidates, or report that they won't vote.) *FactualAssessments* is the mean of the eight items that we included in our earlier analysis of the experiment, with each item coded so that 1 is the most Democratic response and 0 is the most Republican response. *PayCorrect* is an indicator for assignment to the pay-for-correct-response condition. Existing research suggests that $b_1 > 0$: statements of factual beliefs that favor the Democratic Party are associated with voting for the Democratic candidate. But if those statements are affected by cheerleading under ordinary survey conditions, then the association should be weaker in the treatment condition, implying $b_3 < 0$.

We present OLS estimates with clustered standard errors in Table 5.[27] Per these estimates, a one-standard-deviation increase (.124) in the factual assessments scale is associated with a 22-percentage-point increase in the probability of voting for Obama ($p < .01$). Among those assigned to the treatment group, however, the negative estimate for b_3 means that this effect is substantially reduced. For those subjects, the same shift in the assessments scale increases the probability of voting for Obama by 13 percentage points, a decrease of more than 40% ($p < .05$) in the association between those assessments and vote choice. This finding suggests that the observed correlation between normal (unincentivized) survey reports of factual assessments and voting is exaggerated by partisan cheerleading.

We are not suggesting that partisanship does not shape vote choices. However, the clear implication of our experiments is that standard survey measures of factual beliefs are affected by expressive responding. Use of those measures to test the claim that partisanship works by shaping factual beliefs is therefore likely to be very difficult. When incentives are used to measure factual assessments more accurately, the apparent role of factual assessments in vote choice is reduced.

DISCUSSION AND CONCLUSION

Differences between Democrats and Republicans in statements about factual matters are a hallmark of American politics. How should those differences be interpreted? One view is that they reveal perceptual biases. That is, Democrats and Republicans answer questions differently because they perceive "separate realities" (e.g., Kull et al. 2004). Another possibility, highlighted in this paper, is that differences in survey responses arise because surveys offer partisans low-cost opportunities to express their partisan affinities.

To explore the distinction between beliefs and expressive statements made in surveys, we have presented a model of survey response that accounts for the possibility of expressive partisan responding. Our model shows that incentives for correct responses can be used to distinguish sincere from insincere partisan responding. It also shows that incentives—no matter how large—may fail to reduce partisan responding if respondents are unsure of the correct answers. However, by providing incentives for both correct and "don't

[27] In this sample, the mean *FactualAssessments* score is .59 and 50% of respondents prefer Obama. Probit results are substantively similar.

know" responses, one can estimate the proportion of partisan responding that arises either because of partisan cheerleading or because of uncertainty about the correct answers.

Guided by the model, we designed and fielded two novel experiments. In the first experiment, some participants were paid for correct answers to factual questions. The payments reduced observed partisan gaps by about 55%. In the second experiment, we also paid some participants for "don't know" responses. Payments for correct responses reduced partisan gaps by 60%. Payments for both correct and "don't know" responses reduced them by an additional 20%, yielding gaps that were 80% smaller than those that we observed in the absence of payments. Taken together, these results from experiments with small incentives provide lower-bound estimates of the extent to which partisan divergence arises because of expressive partisan returns and self-aware ignorance of the truth.

Why do we observe partisan responding in the first place? We have suggested that it follows from a conscious desire to offer a partisanship-consistent message. But it may also arise unconsciously. Survey respondents may not think seriously about correct answers under ordinary survey conditions, but incentives may reduce partisan gaps by causing respondents to think more carefully about correct answers (e.g., Kuklinski et al. 2001, 419-20; Kahan et al. 2012). In either case, the takeaway is the same: conventional survey measures overstate partisan differences.[28]

The article most closely related to ours is Prior, Sood, and Khanna (2015), which also appears in this issue. One basic difference is that Prior, Sood, and Khanna focus on the accuracy of answers to factual questions about politics, while we focus on partisan differences in responses to those questions. That is, Prior, Sood, and Khanna examine the extent to which payments or unpaid appeals for accurate responses reduce respondents' factual errors in surveys. By contrast, we examine the extent to which payments reduce differences in responses between Democrats and Republicans, and we do not focus on whether respondents

[28] We also designed Experiment 1 to test whether merely enhancing accuracy motivations would reduce partisan gaps. Specifically, we fielded an additional treatment, not discussed above, in which some respondents were told that their answers would be scored. This condition is similar to the "accuracy appeal" condition in Study 2 of Prior, Sood, and Khanna (2015). But unlike those authors, we did not find that this treatment made much difference to partisans' responses, perhaps due to imprecision in our estimates.

answer correctly. Despite this difference, the basic results are complementary: ordinary surveys seem to exaggerate both the differences between partisans and the extent to which they are misinformed.

Two other differences between this article and Prior, Sood, and Khanna (2015) merit attention, and we hope that they will guide future research. First, while we asked partisans about a range of issues in our two studies, Prior, Sood, and Khanna focused on economic issues. Both articles show that ordinary surveys have been overstating partisan bias on a set of economic issues. However, our work shows that this pattern also extends to important issues beyond the economy, including evaluations of foreign affairs.

Second, we present a model of survey response which allows for the possibility that respondents know that they do not know the correct answers to factual questions. This model shows that increasing the incentive to be accurate alone will not reduce partisan bias in survey responses if respondents have this sort of knowledge. However, the experimental manipulation that we undertake, in which individuals are paid for "don't know" responses, permits us to gauge how many respondents recognize their own lack of knowledge about basic political matters. We find that a surprisingly large proportion of subjects appear willing to admit their ignorance by choosing "don't know" for a small financial incentive, despite the fact that this means forgoing partisan responding or the chance to earn a larger reward by choosing the correct response. Furthermore, paying respondents to admit their own ignorance further reduces partisan divergence beyond what is achieved by encouraging accuracy. We see this finding and its implications as particularly deserving of further study.

Implications of Our Findings

The main implication of our findings is that partisan differences in responses to factual questions may not imply partisan differences in beliefs. Instead, some portion of partisan polarization in survey responses about facts—perhaps a very large portion—is affective and insincere. Our results thus call into question the common assumption that what people say in surveys reflects their beliefs. Of course, this assumption has often been called into question for sensitive topics. But our results suggest that a broader range of survey responses should be subject to scrutiny.

In light of this concern, efforts to assess the dynamics of public opinion should grapple with the possibility that over-time changes in partisans' expressed attitudes do not reflect changes in real beliefs. Instead changes in survey responses may reflect changes in the social returns to cheerleading (see Iyengar, Sood, and Lelkes 2012) or in the degree to which different responses are understood to convey support for one's party. For example, elections may make more salient the need to support one's party, explaining why party polarization is more pronounced during campaigns (Iyengar, Sood, and Lelkes 2012), just as "sorting" (Levendusky 2009) may arise because holding particular policy positions may come to be associated with public support of one's party.

Our results may also help to resolve the tension between partisans' divergent assessments of objective conditions in surveys and the power of those conditions to explain aggregate election outcomes (e.g., Bartels and Zaller 2001; Hibbs 2000). We show not only that partisans do not fully believe their own survey responses, but also that they appear to understand their own ignorance. Their recognition of their own ignorance may make it easier to inform them of the facts and in turn change their votes. This self-awareness about lack of knowledge helps explain why even some simple informational interventions appear to have relatively large effects on voting (e.g., Ferraz and Finan 2008; Kendall, Nannicini, and Trebbi 2015). And if these interventions can have large effects on vote choice, then partisan patterns in voting may reflect, in large part, a self-aware lack of information rather than some deeper and persistent unwillingness to tie electoral sanctions to performance.

While our experiments are confined to factual questions, our argument applies to a wider range of questions. Our model suggests that, in the absence of a motivation to answer "partisan" questions accurately, partisan divergence should be large. These factors are also likely to apply to non-factual matters. In particular, when survey reports of attitudes have expressive value, they may be inaccurate measures of true attitudes. And survey reports of vote intention may also be systematically biased by expressive responding.

We have focused on factual statements because our experimental design requires objectively verifiable responses. But other approaches that do not rest on payments for objectively correct answers may also create pressures to be objective. For example, forward-looking judgments can be tied to incentives that

are paid based on the realization of future events. And creative studies in psychology show that enhancing accuracy motivations can reduce partisan divergence even for questions that lack objectively correct answers (Waytz, Young, and Ginges 2014; see also Campbell and Kay 2014). In other words, partisans may be exaggerating not only their statements of factual belief but their attitudinal statements as well.

Another area for subsequent research is the potential heterogeneity of treatment effects. We asked questions about many different policy areas, and we found variation across questions, in both the degree of partisan divergence that exists in the absence of incentives and the degree to which incentives reduce that divergence. Further exploration of this variation will be useful. For example, are certain policy topics perceived as more important, leading partisans to feel that they must stay "on message" when answering questions about those topics? In Experiment 2, we find the largest baseline partisan gaps for questions about economic performance under Bush and Obama, key issues in almost all presidential campaigns. (See Table A2.) Did partisans feel that straying from their team's message on these questions would be particularly damning? (Interestingly, despite these large initial gaps, incentives for correct and "don't know" responses reduced partisan divergence for these items by about as much as they did for other items.)

Similarly, which sorts of people are most likely to engage in expressive responding, and how do those people respond to incentives for correct responses? In our discussion of Experiment 1, we find that strength of partisanship does not seem to moderate expressive responding. Political interest does moderate expressive responding—as expected, more interested partisans are more polarized—but neither strength of partisanship nor political interest changes the effects of incentives. That said, these results are tentative, and a comprehensive examination of heterogeneity across subjects awaits future research.

Additionally, the imprecision of our estimates about the effects of increasing incentive size, for example, means that it would be valuable to conduct additional experiments with larger samples. The apparent effect of increasing incentive size also implies that it would be desirable to ascertain whether even larger incentives can further reduce apparent bias. Additionally, it is unclear how incentives change survey response relative to those in the absence of incentives.

Our main contributions are a model of expressive survey response and two experiments that distinguish cheerleading behavior from sincere partisan divergence. We find that small financial inducements for correct responses can substantially reduce partisan divergence, and that these reductions are even larger when inducements are also provided for "don't know" answers. In light of these results, survey responses that indicate partisan polarization with respect to factual matters should not be taken at face value. Analysts of public opinion should consider the possibility that the appearance of polarization in American politics is, to some extent, an artifact of survey measurement rather than evidence of real and deeply held differences in assessments of facts.

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Table 1: Experiment 1: Question Wording and Baseline Partisan Differences in Scale Scores

Question	Question Wording	Response Options	Control Group, Mean Democratic Response	Control Group, Mean Republican Response	Control Group Difference in Scale Scores, Democrats – Republicans	P-value of Difference of party means, 1-tailed test	N
Iraq casualties, 2007 vs. 2008	Was the number of U.S. soldiers killed in Iraq in the first half of 2008 lower, about the same, or higher than the number who were killed in the second half of 2007?	*Lower (0),* About the same (.5), Higher (1)	.416	.177	.239	.000	212
Bush inflation change	Compared to January 2001, when President Bush first took office, has the level of inflation in the country increased, stayed the same, or decreased?	*Increased (1),* Stayed about the same (.5), Decreased (0)	.894	.694	.201	.000	207
Bush unemployment change	Compared to January 2001, when President Bush first took office, has the level of unemployment in the country increased, stayed the same, or decreased?	*Increased (1),* Stayed about the same (.5), Decreased (0)	.766	.598	.168	.002	208
Estimated Bush approval	About what percentage of *Americans* approve of the way that George W. Bush is handling his job as President?	*20% (1),* 30% (.75), 40% (.5), 50% (.25), 60% (0)	.909	.817	.092	.000	216
Iraq total casualties	About how many U.S. soldiers have been killed in Iraq since the invasion in March 2003?	*4,000 (0),* 8,000 (.25), 12,000 (.5), 16,000 (.75), 20,000 (1)	.200	.114	.087	.013	210
Estimated Bush approval among Republicans	About what percentage of *Republicans* approve of the way that George W. Bush is handling his job as President?	40% (1), 50% (.75), *60% (.5),* 70% (.25), 80% (0)	.794	.724	.070	.039	211
Obama age	How old is Barack Obama?	37 (0), 42 (.33), *47 (.66),* 52 (1)	.558	.508	.050	.055	213
McCain age	How old is John McCain?	62 (0), 67 (.33), *72 (.66),* 77 (1)	.681	.637	.044	.035	215
Afghanistan casualties, 2007 vs. 2008	Was the number of U.S. soldiers killed in Afghanistan in the first half of 2008 lower, about the same, or higher than the number who were killed in the second half of 2007?	Lower (0), *About the same (.5),* Higher (1)	.608	.598	.010	.430	208
Bush deficit change	Compared to January 2001, when President Bush first took office, has the federal budget deficit in the country increased, stayed the same, or decreased?	*Increased (1),* Stayed about the same (.5), Decreased (0)	.938	.944	-.006	.589	212
Placebo: price of gold in 1980	What was the price of gold, in dollars per ounce, on January 18, 1980?	In dollars, 0=0, 1000=1, Correct is *between $800 and $900*	.791	.680		N/A	
Placebo: Bangladeshi independence	In what year did Bangladesh become independent of Pakistan?	In years, 1800=0, 2000=1, Correct is *1971*	.151	.185			

Note: Source: 2008 CCES. Questions are ordered by size of partisan gap in control-group responses, with placebo questions at the bottom. All responses are scaled from 0 to 1; 1 is the most Democratic response. In the "response options" column, the correct response options are italicized. Placebo questions were open-ended and were recoded to range from 0 to 1.

Table 2: Experiment 1: Effect of Payment for Correct Responses on Partisan Differences in Scale Scores

	(1)	(2)	(3)
Democrat (b_1)	0.118	0.105	0.082
	[0.015]***	[0.016]***	[0.022]***
Political interest × Democrat			0.059
			[0.030]**
Payment for correct response × Democrat (b_3)	-0.065	-0.059	-0.057
	[0.022]***	[0.022]***	[0.037]
Payment for correct response × Political interest × Democrat			-0.023
			[0.046]
Payment for correct response	0.038	0.031	0.045
	[0.016]**	[0.016]*	[0.029]
Payment for correct response × Political interest			-0.005
			[0.035]
Knowledge (0-1)		0.013	
		[0.015]	
White		0.017	
		[0.024]	
Hispanic		0.040	
		[0.028]	
Other race		0.051	
		[0.030]*	
Female		0.016	
		[0.012]	
Age (in years)		0.001	
		[0.002]	
Age²/100		-0.001	
		[0.002]	
Region: Northeast		0.043	
		[0.017]***	
Region: Midwest		0.042	
		[0.016]***	
Region: South		0.014	
		[0.014]	
Income (1 = <$10,000; 14 = >$150,000; 15 = RF/Missing)		0.005	
		[0.002]**	
Income missing		-0.046	
		[0.024]*	
Education (1 = no high school; 6 = graduate degree)		0.000	
		[0.006]	
Education: No high school		0.006	
		[0.024]	
Education: Some college		0.019	
		[0.014]	
Education: 2-year college		0.032	
		[0.026]	
Education: 4-year college		-0.003	
		[0.019]	
Married or in a domestic partnership		-0.007	
		[0.013]	
Religious attendance (1-6)		-0.002	
		[0.004]	
Political interest (0,1)			-0.034
			[0.021]
Constant	0.239	0.160	0.261
	[0.021]***	[0.059]***	[0.024]***
Observations	3321	3299	3305
R^2	0.398	0.407	0.400

Note: Source: 2008 CCES. The dependent variable is the mean scale score for the eight questions on which we observed control-group partisan gaps of $p < .10$. It ranges from 0 to 1. The analysis includes only Democrats and Republicans from the control and pay-for-correct-response conditions. Cell entries are OLS coefficients with robust standard errors, clustered by respondent. Question fixed effects not reported.

Table 3: Experiment 2: Question Wording and Baseline Partisan Differences in Scale Scores

Question	Question Wording	Range of Response Line	Correct Response	Mean Pre-treatment Democratic Response	Mean Pre-treatment Republican Response	Pre-Treatment Difference in Scale Scores, Democrats – Republicans	P-value of Difference of Party means, 1-tailed test	N
Obama unemployment	From January 2009, when President Obama first took office, to February 2012, how had the unemployment rate in the country changed?	–2 (Unemployment decreased) to 4% (Unemployment increased)	Increased by 0.5 %	.552	.378	.174	.000	389
Bush II unemployment	From January 2001, when President Bush first took office, to January 2009, when President Bush left office, how had the unemployment rate in the country changed?	–2 (Unemployment decreased) to 4% (Unemployment increased)	Increased by 3.6 %	.715	.583	.132	.000	383
Defense spending	For every dollar the federal government spent in fiscal year 2011, about how much went to the Department of Defense (US Military)?	3 to 27 cents	19.4 cents	.731	.631	.101	.000	355
Obama vote in 2008	In the 2008 Presidential Election, Barack Obama defeated his Republican challenger John McCain. In the nation as a whole, of all the votes cast for Obama and McCain, what percentage went to Obama?	50 to 62%	53.70%	.544	.444	.100	.001	366
Iraq deaths: percent black	Approximately 12 to 13% of the US population is Black. What percentage of US Soldiers killed in Iraq since the invasion in 2003 are Black?	9 to 21%	9.90%	.430	.344	.085	.006	373
Medicaid spending	Medicaid is a jointly funded, Federal-State health insurance program for low-income and needy people. For every dollar the federal government spent in fiscal year 2011, about how much went to Medicaid?	3 to 27 cents	7.5 cents	.577	.502	.075	.013	343
TARP: percent paid back	The Treasury Department initiated TARP (the first bailout) during the financial crisis of 2008. TARP involved loans to banks, insurance companies, and auto companies. Of the $414 billion spent, what percentage had been repaid, as of March 15, 2012?	1 (Less repaid) to 100 (More repaid)	69.56%	.391	.324	.068	.027	349
Global warming	According to NASA, by how much did annual average global temperatures, in degrees Fahrenheit, differ in 2010 from the average annual global temperature between 1951 and 1980?	–1 (Temperatures cooler) to 2 (Temperatures warmer)	Increased by 1.1 degrees	.685	.640	.045	.013	382
Iraq deaths	About how many U.S. soldiers were killed in Iraq between the invasion in 2003 and the withdrawal of troops in December 2011?	1000 to 7000	4,486	.549	.504	.044	.072	382
Debt service spending	The Treasury Department finances U.S. Government debt by selling bonds and other financial products. For every dollar the federal government spent in fiscal year 2011, about how much went to pay interest on those Treasury securities?	3 to 27 cents	6.2 cents	.501	.458	.043	.095	360
Foreign-born population	According to the Census Bureau, in 2010 what percentage of the total population of the United States was born outside of the United States (foreign-born)?	1 to 100%	12.92%	.785	.772	.013	.239	388
Placebo: Mantle home runs 1961	In 1961, Roger Maris broke Babe Ruth's record for most home runs hit in a major league baseball season. He hit 61 home runs that year. How many home runs did his Yankees teammate Mickey Mantle hit in that year?	36 to 60	54	.339	.319		N/A	

Note: Source: Mechanical Turk, March-April 2012. Questions are ordered by size of partisan gap in pre-treatment responses, with placebo question at the bottom. All responses scaled 0 to 1; 1 is the most Democratic response.

Table 4: Experiment 2: Effect of Payment for Correct Responses on Partisan Differences in Scale Scores

	(1) OLS	(2) Tobit	(3) OLS
Democrat (b_1)	.145	.152	.145
	[.028]***	[.029]***	[.028]***
Payment for correct response × Democrat (b_4)	-.087	-.091	
	[.030]***	[.032]***	
Payment for correct response and DK × Democrat (b_5)	-.117	-.123	
	[.029]***	[.030]***	
Payment for correct response	.018	.018	
	[.025]	[.026]	
Payment for correct response and DK	.049	.052	
	[.024]**	[.025]**	
Amount correct = $0.10 × Democrat			-.082
			[.033]**
Amount correct = $0.25 × Democrat			-.092
			[.033]***
Amount correct = $0.50 × Democrat			-.096
			[.033]***
Amount correct = $0.75 × Democrat			-.061
			[.036]*
Amount correct = $1.00 × Democrat			-.116
			[.036]***
(Proportional payment for DK = .20) × Democrat			-.031
			[.018]*
(Proportional payment for DK = .25) × Democrat			-.016
			[.020]
(Proportional payment for DK = .33) × Democrat			-.041
			[.020]**
Amount correct = $0.10			.010
			[.027]
Amount correct = $0.25			.028
			[.027]
Amount correct = $0.50			.020
			[.027]
Amount correct = $0.75			.005
			[.029]
Amount correct = $1.00			.042
			[.029]
Proportional payment for DK = .20			.023
			[.013]*
Proportional payment for DK = .25			.030
			[.017]*
Proportional payment for DK = .33			.034
			[.016]**
Constant	.614	.617	.614
	[.026]***	[.026]***	[.026]***
Observations	4608	4608	4608
R^2	.179	N/A	.181
F-test, 'Pay Correct × Dem.' > 'Pay Correct and DK × Dem.'	.020	.020	N/A

Source: Mechanical Turk, March-April 2012. The dependent variable is the mean scale score for the ten questions on which we observed pre-treatment partisan gaps of $p < .10$. It ranges from 0 to 1. The analysis includes only Democrats and Republicans. Cell entries are coefficients with robust standard errors, clustered by respondent. Question fixed effects are not reported. * significant at 10%; ** significant at 5%; *** significant at 1% (two-tailed tests).

Table 5: Experiment 1: Association of Factual Assessments with Vote Choice

	Vote for Democratic Presidential Candidate
Average factual assessments scale score (b_1; 0 = most Republican, 1 = most Democratic)	1.770
	[.222]***
Payment for correct response (b_2)	.418
	[.224]*
Payment for correct response × Average factual assessments scale score (b_3)	-.741
	[.367]**
Constant	-.548
	[.135]***
Observations	373
R^2	.130

Note: Source: 2008 CCES. The dependent variable is coded 1 for subjects who expressed an intention to vote for the Democratic candidate (Barack Obama), 0 for those who expressed an intention to vote for the Republican candidate (John McCain). The analysis includes only those Democrats and Republicans who expressed an intention to vote for one of the major-party candidates. "Payment for correct response" is coded 0 or 1. "Average factual assessments scale score" is computed by averaging across the eight non-placebo questions for which we found partisan gaps in the control condition.

APPENDIX: A MODEL OF EXPRESSIVE SURVEY RESPONSE

We begin with a model in which respondents derive utility from their survey responses in three ways: by offering answers that cast their party in a favorable light, by expressing their sincere beliefs, and by earning financial rewards. For now, we set aside the possibility that people can choose to say "don't know." For simplicity, we focus on the case in which there are two survey responses, r_1 and r_2. Individuals, indexed by the subscript *i*, are either Democrats (T = D) or Republicans (T = R). Individuals differ in their taste for partisan cheerleading and their beliefs about the truth.

Turning first to expressive benefits, individual *i*'s taste for partisan cheerleading is denoted by the parameter c_i, for cheerleading, which ranges from 0 (no taste for it) to any positive number. Beliefs about the truth are described by the function $p_i(r_j)$, which is the probability that i believes response r_j, j = 1 or 2, is correct. In this example, we assume that response r_1 portrays Democrats most favorably, that response r_2 portrays Republicans most favorably, and that these assumptions are shared by respondents from both parties. Specifically, the expressive function $e(T, r_j)$ maps an individual's partisanship T to the personal benefit of offering response r_j, and is defined as $e(T = D, r_1) = e(T = R, r_2) = 1$ and $e(T = D, r_2) = e(T = R, r_1) = 0$. That is, Democrats and Republicans receive an expressive partisan utility boost from offering the response that portrays their party in a favorable light, and they receive no partisan utility from offering the response that is inconsistent with their partisan leanings.

The utility associated with providing a sincere response is measured by the "honesty" function $h_i(r_j)$. For simplicity, we assume $h_i(r_j) = p_i(r_j)$, i.e., the honesty value of offering response r_j is the probability that the respondent believes it is true. Finally, some respondents may also receive an incentive, I > 0, which is the additional reward for a correct response. We assume utility is linear in I.

These assumptions allow us to describe a respondent's expected utility for offering response r_j as the sum of three terms. We omit the individual subscript i for clarity:

(1) $EU(r_j|.) = h(r_j) + I \times p(r_j) + c \times e(T,r_j)$.

The first term is simply the honesty value of response r_j. The second term is the additional value of providing response j in the presence of incentive I (realized with the probability that response is correct). The third term is the partisan value of offering response r_j weighted by the respondent's value of expressive partisan responding, c. Using the assumption that h() is equivalent to p(), we rewrite (1) as:

(2) $EU(r_j|.) = (1+I) \times p(r_j) + c \times e(T,r_j)$,

which is the form of the expected utility we focus on here. A respondent will offer the response r_j from (r_1,r_2) that maximizes (2).

To make the exposition as clear as possible, we suppose that the respondent is a Democrat (T = D). The analysis for the Republican partisan mirrors that for the Democratic partisan and is omitted. Recall that r_1 is the partisan Democratic response, and so $e(D, r_1) = 1$ and $e(D, r_2) = 0$.

First, consider how our model predicts that partisans will respond to a survey in the absence of incentives for correct responses. In this case, equation (2) reduces to

(3) $EU(r_j|.) = p(r_j) + c \times e(T,r_j)$.

Using (3), the utility from reporting response r_1 is $p(r_1) + c$, and the utility from reporting r_2 is $p(r_2) = 1 - p(r_1)$. Therefore the Democrat will report r_1 whenever $c \geq c^* = 1 - 2p(r_1)$.

As c is weakly positive, whenever $p(r_1) > .5$ (that is, the Democrat believes response r_1 is at least as likely to be correct as r_2), the Democrat will offer the partisan response r_1 even in the absence of expressive returns (i.e., even if c = 0). By contrast, as $p(r_1)$ grows small (i.e., as the Democrat becomes increasingly likely to believe the pro-Republican response is correct), larger values of c are required to cause her to offer r_1. To produce a response of r_1, the partisan expressive return must be larger to offset the greater cost of providing an answer that is likely to be untrue.

This relationship is displayed graphically in Panel A of Figure 1, which shows that for each value of $p(r_1)$ there is a value of expressive partisan responding such that, for those Democrats with c at least this large, r_1 will be their survey response. Democrats offering r_1 are therefore composed of two groups. The first group consists of those who believe that r_1 is more likely to be correct than r_2; this group is represented by the right-hand side of the panel, for which $p(r_1) > .5$. The second group consists of those who believe that r_2 is

more likely to be correct, but for whom that belief is offset by a larger return from offering an expressive partisan response. This group is represented by the upper segment of the left-hand side of the panel, which is labeled "insincere choice of r_1."

To link expressive returns to polarization of partisan responses, consider Panels B and C. Panel B shows the response pattern for Republicans, which is a mirror image of Panel A. And Panel C displays both partisan response patterns at once. It shows that in the presence of expressive returns, Democrats and Republicans *who share common beliefs about the truth* (are at the same position on the horizontal axis) *can nonetheless offer polarized survey responses if their value of expressive partisan responding is large enough*. When beliefs about the truth are shared, polarization is most prevalent when beliefs are most uncertain, i.e., when $p(r_1) = p(r_2) = .5$. Polarization will also arise, even in the absence of returns to expressive partisan responding (i.e., when $c = 0$), if Democrats and Republicans hold different beliefs about the truth.

We next consider what happens when incentives are offered for correct responses, i.e., when $I > 0$. From equation (2), for a given value of I, there is a unique $c^{*\prime} = (1+I)(1 - 2p(r_1))$ such that all Democrats with an expressive responding parameter greater than $c^{*\prime}$ will offer r_1. As before, incentives have no effect on the responses of Democrats who believe that response r_1 is correct (i.e., $p(r_1) > .5$). But for Democrats who believe response r_2 is more likely to be correct, a larger return to cheerleading is now required to offset the earnings that are likely to be lost by offering response r_1. Formally, $c^{*\prime} = c^* + (I \times (1 - 2p(r_1))$. This relationship is shown in Panel A of Figure 2. (For simplicity, we assume throughout Figure 2 that $I = 1$.)

Comparison of Panel A in Figure 1 and Panel A in Figure 2 draws out a basic but important result: incentives for correct responses reduce expressive partisan responding by causing some of those who know that response r_1 is less likely to be true to offer response r_2 instead. In Figure 2, these respondents are represented by the region that is labeled "induced choice of r_2."

Figure 2 draws out a second important result: when a Democrat believes that r_2 is more likely to be correct, the additional value of expressive returns (c) that is required to make her offer response r_1 increases in her belief that r_2 is correct. Formally, $c^{*\prime} - c^*$ is increasing in $p(r_2)$. To see this result graphically, note that the vertical gap between the dashed and solid lines increases as one approaches the left side of the x-axis. This

gap increases because the difference between c*' and c* is a function of $p(r_1)$. In other words, for those who are more uncertain ($p(r_1)$ is closer to .5), incentives have smaller effects. The intuition for this result is that a person who chooses the answer she thinks is most likely to be correct only earns the incentive for a correct response if that answer is in fact correct, which she expects to occur with the probability that they believe that response is correct. If a person believes r_1 is correct with probability .75, she earns the incentive I with probability .75 if she chooses r_1 and .25 if she chooses r_2. At the extreme, an individual who believes that r_1 and r_2 are equally likely to be true—that is, she knows that she doesn't know the truth—continues to offer r_1 regardless of incentives for correct responses because she won't (in expectation) do better by giving up the certain benefit of a partisan response because she earns the incentive I, in expectation, half the time for either response.

To illustrate the effect of incentives on polarization, Panel B of Figure 2 shows the effect of incentives for Republican partisans, and Panel C displays both partisan response patterns at once. Comparison of Panel C in Figure 1 to Panel C in Figure 2 shows that increasing incentives decreases polarization. In particular, incentives reduce the frequency with which Democrats and Republicans who share common beliefs about the truth offer different survey responses, apart from the case in which $p(r_1) = p(r_2) = .5$.

This exposition leads us to two conclusions. First, incentives for correct answers reduce partisan divergence in the presence of shared beliefs about the truth. Second, partisan divergence may persist in the face of incentives. It is clear that if partisan groups have different sincere beliefs about which response is most likely to be true, paying respondents for correct responses will not reduce polarization. However, although it may seem intuitive that persistent partisan divergence in the presence of incentives for correct responses *implies* underlying differences in beliefs about the truth, our analysis suggests partisan divergence may nonetheless persist for two other reasons. First, the taste for expressive partisan cheerleading (c) may be large. Second, even if that taste is small, individuals may be uncertain about the truth. In that case, they will offer partisan responses even in the face of large incentives for correct responding.

We have considered respondents who must provide either a partisan-consistent or a partisan-inconsistent response. But giving respondents the option to decline to provide a response may reduce

observed polarization. To explore this possibility, we consider a model with an additional response option: "don't know."

Incorporating "Don't Know" Responses

To incorporate a "don't know" response option, we must specify the utility that a respondent receives from selecting "don't know." For simplicity, we assume that a "don't know" response (r_{dk}) yields some fixed positive psychological benefit $V_{dk} > 0$ plus whatever financial incentive is offered for giving that response (I_{dk}). (The results here are robust to allowing negative values of V_{dk}.) Specified this way, $U(r_{dk}) = V_{dk} + I_{dk}$. One can think of V_{dk} as the honesty value of choosing "don't know" relative to an incorrect response. As before, the individual is offered an incentive I for providing a correct response.

When will a respondent choose "don't know"? Note that the value of "don't know" is unaffected by c or p(), so a respondent chooses "don't know" when the values of c and p() make both r_1 and r_2 less attractive than "don't know." Critically, one can earn the incentive I_{dk} with certainty by choosing "don't know", unlike the incentive for a correct response which is realized only if the chosen response is revealed after the fact to be correct, which occurs with the belief $p(r_j)$. Ceteris paribus, therefore, increasing uncertainty (r_j=.5) will make the "don't know" option more attractive. Recall from the previous analysis (illustrated in Panel A of Figure 2) that a Democrat's selection of r_1 or r_2 depends on whether c is greater or less than $c^{*\prime} = (1+I)(1 - 2p(r_1))$.

Consider first a Democrat who would otherwise choose the "Republican" response, r_2. Her expected utility for choosing this response is $(1+I) \times (1 - p(r_1))$. This utility is greater than the utility associated with selecting "don't know" when $p(r_1) < p^*(r_1) = 1 - (V_{dk} + I_{dk}) / (1+I)$. This $p^*(r_1)$ is the lowest probability that the Democratic response (r_1) is correct for which the Democrat will select "don't know" rather than the Republican response. When $p(r_1)$ is below this critical value, the Democrat prefers to report the Republican response. Note that this critical value of $p^*(r_1)$ is unaffected by the expressive value of partisan responding c, because the return to r_2 is unaffected by c.

Figure 3 illustrates this logic. For presentation, we assume that I = 1, I_{dk} = .75, and V_{dk} = .5.[1] The value of $p^*(r_1)$ is thus 1 – (.5 + .75) / (1 + 1) = .375. Graphically, this solution is represented in Panel A by the leftmost line that defines the "induced don't know" region. Substantively, the point is that when $p(r_1)$ exceeds the critical value $p^*(r_1)$, all cases in which the Democrat would have offered the Republican response are replaced by "don't know" answers.

We next examine how a Democrat who otherwise would have chosen the "Democratic" response, r_1, behaves in the presence of incentives for "don't know." We have already shown that if c = c*', the Democrat is indifferent between the Democratic and the Republican responses, and that if $p(r_1) = p^*(r_1)$, she is also indifferent between those responses and "don't know." However, as $p(r_1)$ rises above $p^*(r_1)$, the expected return from choosing the "Democratic" response increases. This means that as the Democratic response becomes more likely to be true, smaller returns to expressive responding are required to keep the Democratic response more attractive than "don't know." In Panel A of Figure 3, this condition is illustrated by the downward-sloping line that defines the top of the region labeled "induced don't know." Formally, $c = c^{*\prime\prime} = (V_{dk} + I_{dk}) / (p(r_1)(1+I))$ is the critical value, such that when $c > c^{*\prime\prime}$ (and $c > c^{*\prime}$), the Democrat chooses the Democratic response over "don't know."

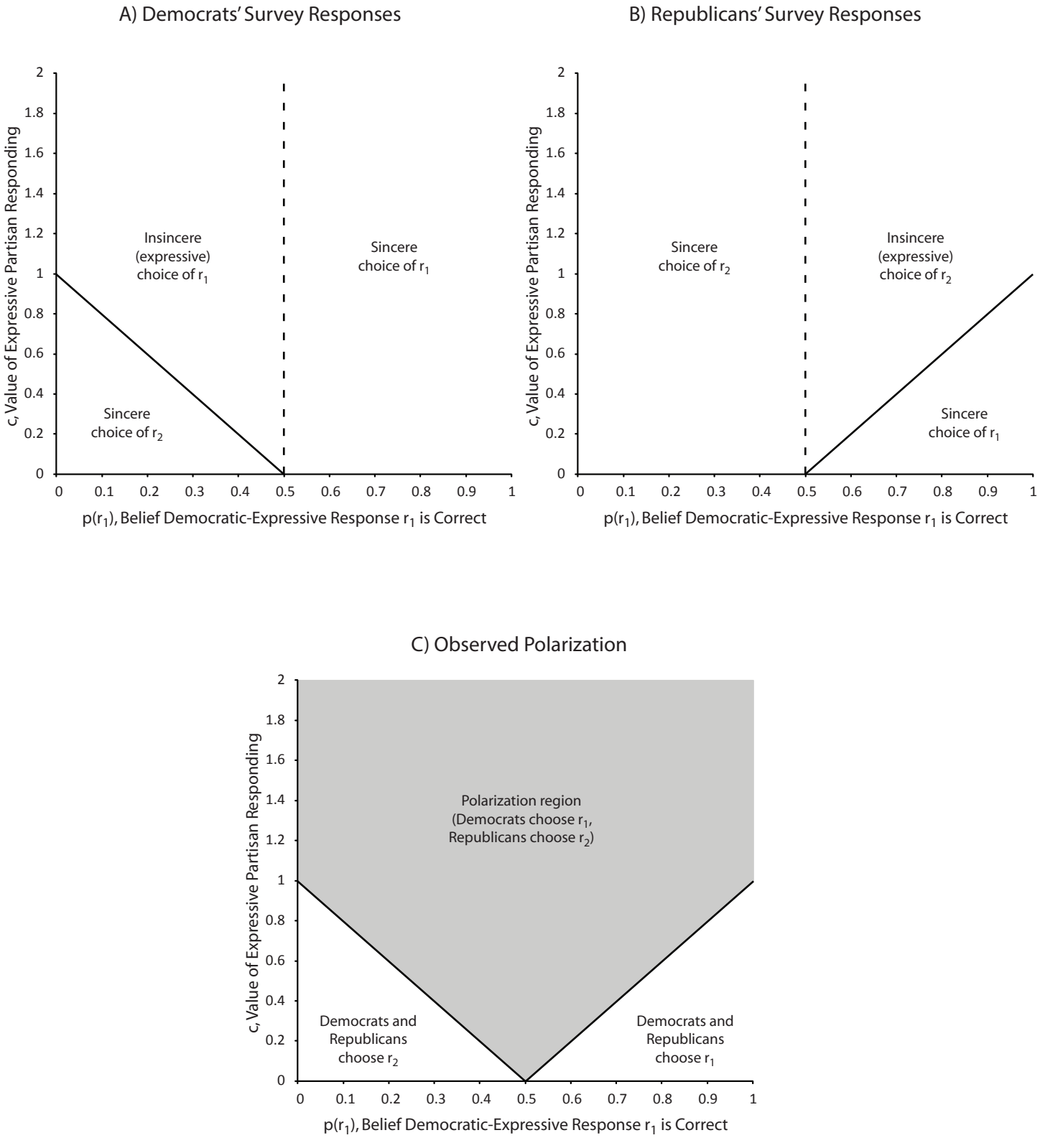
Parallel analysis for Republicans appears in Panel B of Figure 3. For both Democrats and Republicans, the subjects who offer "don't know" responses are drawn from those who are most uncertain about which answer is correct, i.e., from subjects for whom $p(r_1)$ is close to .5. Our analysis above establishes that it is this uncertainty that makes incentives for correct answers least likely to affect survey responses. Accordingly, for these uncertain respondents, the "sure thing" of a "don't know" payment is a more effective inducement than the smaller probability of earning a potentially larger payment for a correct response.

Combining these analyses, as we do in Panel C, and comparing that plot to panel C of Figure 2 allows us to assess the effect on observed polarization of offering incentives for both correct and "don't know"

[1] We choose a relatively high level of I_{dk} because Figure 3 illustrates the logic of our model when there are only two survey responses (in addition to "don't know"). Given only two responses, even complete uncertainty means that one is, in expectation, correct half of the time. In a model with more response options, the value of I_{dk} necessary to sustain don't know responses would be smaller. For example, one could also allow the value of I_{dk} to be negative.

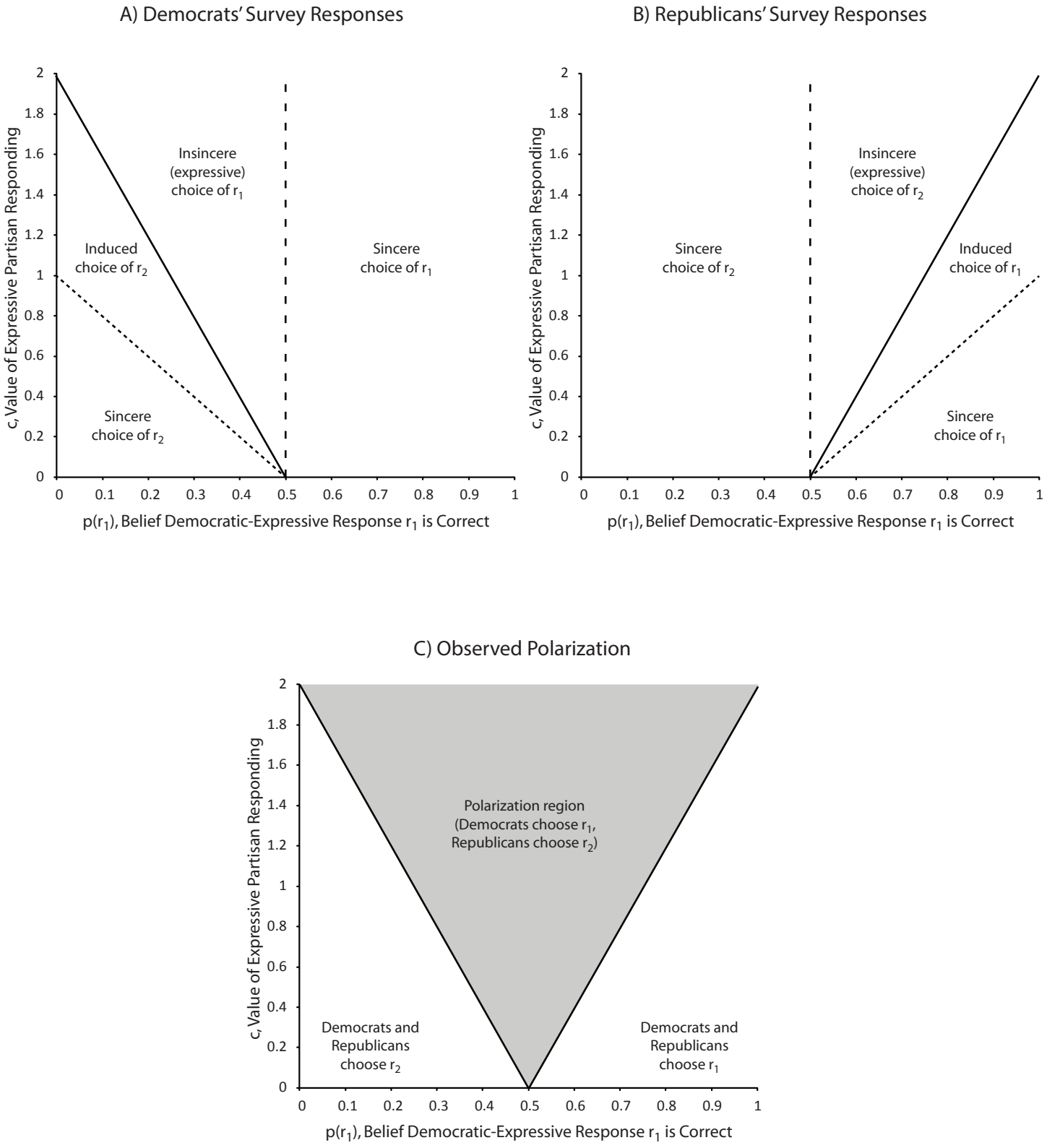
responses. Relative to simply offering incentives for correct responses, adding incentives for "don't know" responses decreases the frequency with which Democrats and Republicans who share common but weak beliefs about the correct response ($p(r_j)$ is not close to 1 for any j) provide divergent (non-"don't know") survey responses.

Figure 1: Patterns of Survey Response in the Absence of Incentives
by Value of Expressive Partisan Responding and Beliefs about Correct Responses



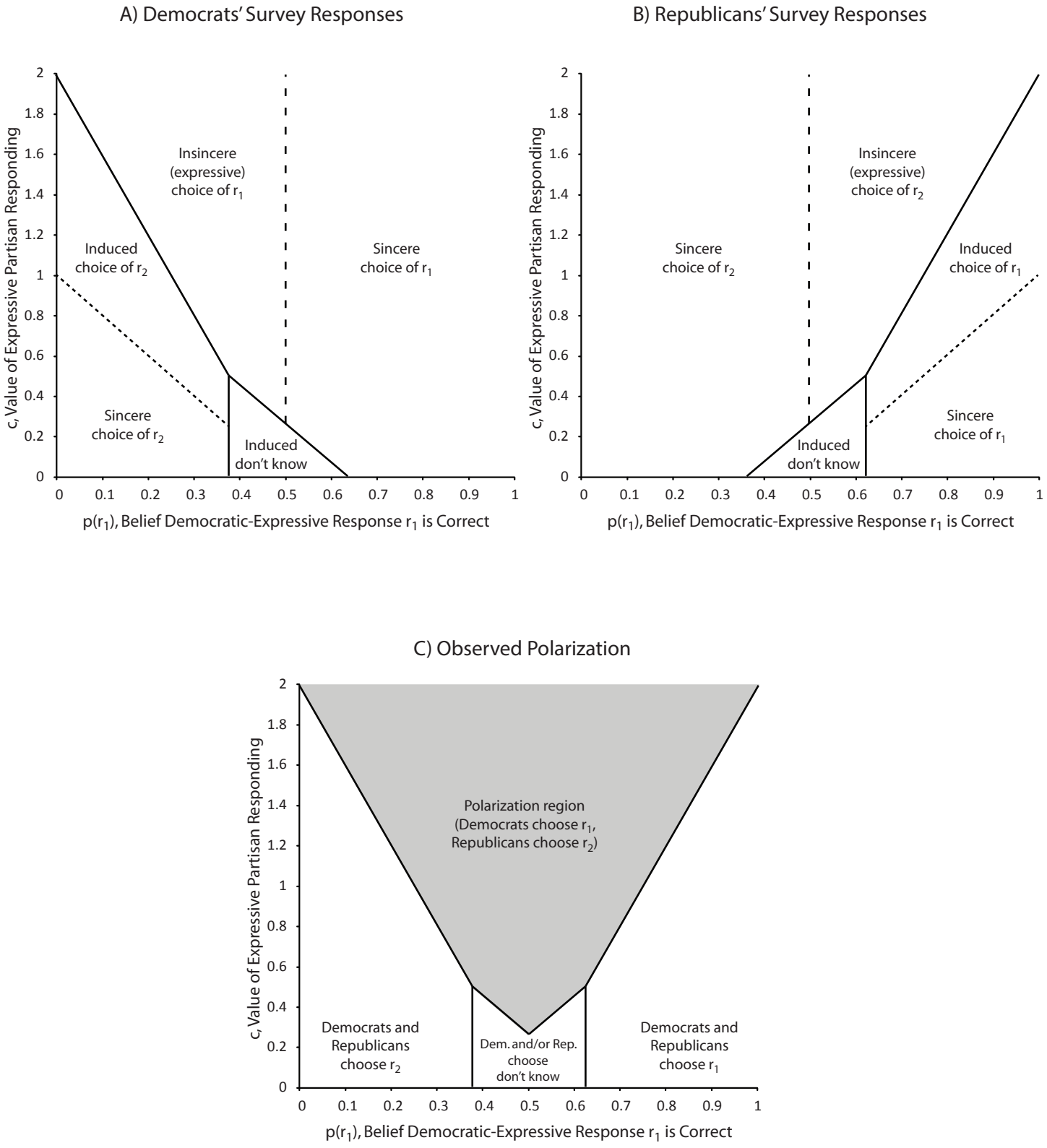
Note: Panel A displays Democrats' survey responses in the absence of incentives for different levels of returns to expressive partisan responding and beliefs about whether response r_1 is correct. Panel B displays responses for the same parameters for Republicans. Finally, the grey area in panel C is the range of parameters for which Democrats and Republicans offer different survey responses despite common beliefs about which response is correct.

Figure 2: Patterns of Survey Response Given Incentives for Correct Responses (I=1) by Value of Expressive Partisan Responding and Beliefs about Correct Responses



Note: Panel A displays Democrats' survey responses given incentives I=1 for correct responses for different levels of returns to expressive partisan responding and beliefs about whether response r_1 is correct. Panel B displays responses for the same parameters for Republicans. Finally, the grey area in panel C is the range of parameters for which Democrats and Republicans offer different survey responses despite common beliefs about which response is correct.

Figure 3: Patterns of Survey Response Given Incentives for Correct (I=1) and Don't Know (I_{dk}=.75) Responses by Value of Expressive Partisan Responding and Beliefs about Correct Responses



Note: Panel A displays Democrats' survey responses given incentives for correct (I=1) and don't know (I_{dk}=.75) responses for different levels of returns to expressive partisan responding and beliefs about whether response r_1 is correct. In all panels, V_{dk}=.5. Panel B displays responses for the same parameters for Republicans. Finally, the grey area in panel C is the range of parameters for which Democrats and Republicans offer different non-don't know survey responses despite common beliefs about which response is correct.

Table A1: Experiment 1: Effect of Payment for Correct Responses on Partisan Divergence in Scale Scores by Question

	(1) Iraq 07 to 08 Change Casualties	(2) Bush Inflation Change	(3) Bush Unemployment Change	(4) Est. Bush Approval	(5) Iraq Total Casualties	(6) Est. Bush Approval Among Reps.	(7) Obama Age	(8) McCain Age
Democrat (1=Yes, 0=Republican)	0.239	0.201	0.168	0.092	0.087	0.070	0.050	0.044
	[0.052]***	[0.044]***	[0.056]***	[0.023]***	[0.038]**	[0.039]*	[0.031]	[0.025]*
Payment for Correct Response * Democrat	-0.078	-0.026	-0.074	-0.100	-0.064	-0.072	-0.048	-0.053
	[0.077]	[0.061]	[0.079]	[0.034]***	[0.054]	[0.055]	[0.045]	[0.033]
Payment for Correct Response	0.043	0.059	0.091	0.018	0.051	0.026	0.005	0.010
	[0.051]	[0.052]	[0.058]	[0.024]	[0.036]	[0.039]	[0.034]	[0.024]
Constant	0.177	0.694	0.598	0.818	0.114	0.724	0.508	0.637
	[0.033]***	[0.036]***	[0.042]***	[0.016]***	[0.024]***	[0.029]***	[0.023]***	[0.019]***
Observations	415	409	407	421	412	416	419	422
R-squared	0.064	0.093	0.032	0.044	0.014	0.008	0.008	0.012
Percentage of Partisan Gap Eliminated by Payment for Correct Response	32.5%	12.9%	44.4%	108.7%	73.3%	103.4%	95.2%	119.1%

Source: 2008 CCES study. Includes only Democrats and Republicans. Cases included are from control and paid for correct response condition. OLS Coefficients with robust standard errors. * significant at 10%; ** significant at 5%; *** significant at 1% (two-tailed tests).

Table A2: Experiment 2: Effect of Payment for Correct and Don't Know Responses on Partisan Divergence in Scale Scores by Question

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Obama Unemployment	Bush II Unemployment	Defense Spending	Obama Vote 08	Iraq deaths % Black	Medicaid Spending	TARP % Paid Back	Global Warming Amount	Iraq deaths	Debt Service Spending
Democrat (1=Yes, 0=Republican)	0.293	0.239	0.118	0.126	0.219	0.136	0.107	0.133	0.051	0.010
	[0.065]***	[0.068]***	[0.085]	[0.086]	[0.081]***	[0.086]	[0.091]	[0.057]**	[0.072]	[0.089]
Payment Correct * Democrat	-0.210	-0.097	-0.021	-0.091	-0.207	-0.088	-0.059	-0.107	-0.043	0.062
	[0.078]***	[0.077]	[0.093]	[0.095]	[0.092]**	[0.096]	[0.100]	[0.063]*	[0.081]	[0.099]
Payment DK and Correct * Democrat	-0.184	-0.202	-0.092	-0.056	-0.209	-0.139	-0.091	-0.093	-0.052	-0.035
	[0.073]**	[0.073]***	[0.088]	[0.092]	[0.086]**	[0.089]	[0.096]	[0.060]	[0.077]	[0.092]
Payment for Correct Response	0.021	-0.049	-0.053	-0.028	0.113	0.081	-0.019	0.058	-0.009	0.042
	[0.057]	[0.072]	[0.080]	[0.080]	[0.069]	[0.079]	[0.073]	[0.055]	[0.064]	[0.082]
Payment for DK and Correct Response	-0.019	0.079	0.059	-0.031	0.158	0.067	0.053	0.038	0.013	0.039
	[0.054]	[0.068]	[0.076]	[0.078]	[0.064]**	[0.073]	[0.071]	[0.053]	[0.062]	[0.076]
Constant	0.401	0.586	0.630	0.467	0.241	0.489	0.346	0.605	0.522	0.490
	[0.048]***	[0.066]***	[0.073]***	[0.073]***	[0.060]***	[0.071]***	[0.066]***	[0.050]***	[0.057]***	[0.074]***
Observations	444	485	446	457	470	442	452	466	479	467
R-squared	0.077	0.099	0.050	0.029	0.023	0.022	0.017	0.028	0.005	0.029
F-test, Pay Correct * Dem. > Pay DK and Correct * Dem.	0.310	0.010	0.060	0.250	0.490	0.150	0.280	0.340	0.420	0.030
Percentage of Partisan Gap Eliminated by Payment for Correct Response	71.6%	40.6%	17.8%	72.1%	94.2%	64.8%	55.3%	80.5%	83.3%	Increases
Percentage of Partisan Gap Eliminated by Payment for DK and Correct Response	62.7%	84.5%	78.2%	44.7%	95.1%	101.9%	84.8%	70.3%	101.0%	335.7%

Source: 2012 MTURK study. Includes only Democrats and Republicans. Comparison of post-treatment responses in control, pay correct, and pay correct and don't know conditions. OLS Coefficients with robust standard errors. * significant at 10%; ** significant at 5%; *** significant at 1% (two-tailed tests).

ONLINE APPENDIX TO
"PARTISAN BIAS IN FACTUAL BELIEFS ABOUT POLITICS"

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Gregory A. Huber

This online appendix has seven parts:

Part I: Details about the Experiment 1 sample

Part II: Details about the Experiment 2 sample and study, including a replication on the 2012 CCES

Part III: Results including partisan leaners

Part IV: Robustness to within, collapsed, and excluding cheaters analysis

Part V: Previous research on partisan divergence in factual assessments

Part VI: References to works cited in the online appendix

Part VII: Complete instructions and screenshots for Experiment 2

PART I: DETAILS ABOUT THE EXPERIMENT 1 SAMPLE

The CCES is an Internet survey of U.S. citizens that was conducted by YouGov/Polimetrix. YouGov/Polimetrix uses sampling and matching techniques to generate a sample that approximates the demographic composition of the adult U.S. population. The full sample for the 2008 CCES is based on the 2005-06 American Community Study, November 2008 Current Population Survey, and the 2007 Pew Religious Life Survey. Thus, this target sample is representative of the general population on a broad range of characteristics including a variety of geographic (state, region and metropolitan statistical area), demographic (age, race, income, education and gender), and other measures (born-again status, employment, interest in news, party identification, ideology and turnout). Polimetrix invited a sample of their opt-in panel of 1.4 million survey respondents to participate in the study. Invitations were stratified based on age, race, gender, education and by simple random sampling within strata. For more detailed information on this type of survey and sampling technique see Vavreck and Rivers (2008). More broadly, see Baker et al. (2010) for a report on the potential strengths and limitations of online panels.

The experiment sample was part of a private module on the 2008 CCES, with a target sample population of 1,800 individuals. These questions were asked of a subset, drawn at random, of 626 of the 1,800 individuals in the full sample. Of the 419 partisans used in our analysis, 81% were white, 7% were black, 8% were Hispanic and 54% were female. Their mean age was 48 years old, their median level of educational attainment was “some college,” and 67% were married or in a domestic partnership.

Respondents in online samples often know more about politics and have more interest in politics than respondents in other surveys. It is not possible to establish whether this pattern holds with respect to knowledge in Experiment 1: the 2008 CCES includes few conventional knowledge questions (and none that have been used in recent ANES studies). But the data show that the pattern does hold with respect to political interest. For example, 65% of partisans in the 2008 CCES report being “very much interested” in politics; the corresponding percentage in the 2008 ANES is 38%. (No question in the 2008 ANES perfectly corresponds to the CCES political interest question. The closest ANES question, which we use here, is V085073a.)

Nonrepresentativeness on baseline characteristics does not necessarily imply that the treatment effects reported in Table 2 are different from those that we would find with a more representative sample (Druckman and Kam 2011). But it is easy to imagine ways in which the over-representation of politically interested people in our sample may cause us to overestimate—or to underestimate—the average effects of incentives. For example, even after conditioning on strength of partisanship, more interested people may be more likely to know the correct answers to our "partisan" questions. They may therefore be more likely to change their answers in response to payments that we offer for correct answers. If so, our estimates of the effects of incentives, while valid for our sample, overstate the effectiveness of such payments among ordinary partisans.

On the other hand, one may imagine that, even after conditioning on party identification, more interested respondents will issue more extreme answers to the "partisan" questions that we ask, or that they will hold to their answers more strongly (regardless of whether they know the correct answers). In either case, our estimates of the effects of payments for correct answers are likely to understate the effects that we would observe in a more representative sample.

We began to examine these possibilities by estimating models in which payments for correct answers are interacted with political interest. The relevant results appear in the third column of Table 2 and are discussed on pages 12-13. We find that the responses of politically interested subjects are more polarized, under ordinary conditions, than the responses of others. But interest does not moderate our estimated treatment effect. (The estimated coefficient on the relevant interaction term, –.23, is half the size of its standard error.) If anything, then, the overrepresentation of the interested makes our results conservative: a less interested population would be less polarized under ordinary survey conditions, and because the effect of incentives would be similar in magnitude, it would bring about a greater proportional reduction of the "distance" between the answers of members of different parties.

We can further consider the issue by considering how the results change when we weight the data to account for sample nonrepresentativeness. Table OA1 reports these results. The analyses are identical to those reported in Table 2, except that those in the "weighted analysis" columns incorporate the sample weights that are provided with the 2008 CCES. The critical coefficient in the table, "Payment for correct response ×

Political interest × Democrat," is again small and approximately half the size of its standard error (–.034, SE .065). This result further suggests that overrepresentation of the interested makes little difference to our results.

Finally, we note that we obtain similar results in our Mechanical Turk sample, which does seem to be representative of the population of U.S. partisans in terms of political interest. See the next part of this online appendix for details.

Coding of Correct Answers

The text of each Experiment 1 question is shown in Table 1, as is the response option that we deem "correct" for each question. We provide information about correctness to satisfy readers' curiosity: our analysis is about partisan divisions in responses to factual questions, not about correctness *per se*. Even so, a few additional words about some of the questions are in order.

One question asks about casualties of U.S. soldiers in Iraq in the second half of 2007 and the first half of 2008. The "surge" of U.S. troops in Iraq occurred during this period, and it corresponded to a widely reported decline in U.S. casualties: there were 37% fewer U.S. casualties in the first half of 2008 than in the second half of 2007 (Iraq Coalition Casualty Count 2014). Accordingly, we have coded "lower" (i.e., casualties fell) as the correct answer to the question. The response options to this question ("lower," "about the same," and "greater") were chosen because they have often been used in ANES retrospection items. See Experiment 2 for items that permit a wider range of responses.

Two of the questions were about the ages of John McCain and Barack Obama. Had McCain won the election, he would have been the oldest first-term president in history. His age was a particular concern to voters in 2008 (e.g., Benen 2008, Alonso-Zaldivar 2008, Pew 2008b), especially among the elderly (Pew 2008a). Obama's age was a lesser issue, although the concern that he was "too young for the no. 1 job" did surface (e.g., Calabresi 2008).

PART II: DETAILS ABOUT THE EXPERIMENT 2 SAMPLE AND STUDY, INCLUDING A REPLICATION ON THE 2012 CCES

We recruited 1,506 participants for the Mechanical Turk study over the web from March 29, 2012 to April 16, 2012. Subjects for the experiment were recruited with an advertisement for "A quick survey to see what you know and how you learn." Because Mechanical Turk samples tend be more Democratic than the general population, we invited equal numbers of Democrats and Republicans who had previously taken our unrelated surveys to participate in this study. We invited 115 each strong Democrats and Republicans, 208 each Democrats and Republicans, and 111 each weak Democrats and Republicans, in an attempt to attract more Republicans than are ordinarily found in Mechanical Turk samples. Of the 795 partisans in our sample, 65% were Democrats, 89 were assigned to the control group, 327 to the pay-for-correct-response group, and 379 to the pay-for-correct-and-"don't know" group. For this group, age ranged from 19 to 75 with a mean of 33, 54 percent were female, and 46 percent had at least a four-year college degree.

We only extended invitations to people who had previously identified themselves as U.S. residents. As a further check on the residence of our subjects, we geocoded the IP addresses that they used to participate in the experiment. Of the 1,506 participants, only 38 (2.5%) had IP addresses that we located outside of the United States, and an additional three participants had IP addresses that we could not geocode. The 38 outside-the-US participants were distributed among 22 different countries. Of course, many of these participants may have been U.S. residents who were connecting to our web site during temporary travels abroad.

For all of the questions asked in this experiment, we used a novel graphical input device to measure participants' attitudes. Part VII of this online e appendix displays examples of the "sliders" that we used to gather answers to each of the questions we asked. After we trained participants to use this interface (complete instructions appear below), we asked them to respond to each question by manipulating the slider. Additionally, in the conditions in which participants were paid for correct responses, subjects were informed that a response would be scored as correct if the slider overlapped the correct answer.

The experiment had three conditions: a control condition, the pay-for-correct condition, and the pay-for-correct-and-"don't know" condition. (It also had a fourth condition that we do not analyze here: see footnote 16.)

Instructions in the control condition: "Once again, your answers will be timed. By answering these questions, you will earn an additional 50 cent bonus."

Instructions in the pay-for-correct condition: "Once again, your answers will be timed. Additionally, we are now going to give you a [X] cent bonus for each question you answer correctly. We'll tell you how many questions you get right at the end of the survey. You'll get credit for answering a question correctly if the thick horizontal bar underneath your arrow covers the correct answer. So, for example, in the picture below, the arrow is at 5. If the correct answer were 5.25, which is under the bar, you would earn the bonus. If the correct answer was 7, however, you would not earn the bonus."

Instructions in the pay-for-correct-and-"don't know" condition: "Once again, your answers will be timed. Additionally, we are now going to give you a X cent bonus for each question you answer correctly. We'll tell you how many questions you get right at the end of the survey. You'll get credit for answering a question correctly if the thick horizontal bar underneath your arrow covers the correct answer. So, for example, in the picture below, the arrow is at 5. If the correct answer were 5.25, which is under the bar, you would earn the bonus. If the correct answer was 7, however, you would not earn the bonus. As an alternative to being paid for a correct answer, you can instead earn a X × Y cent bonus for each question you tell us you don't know the answer to. We'll pay you for saying 'don't know' if you click the check box next to 'don't know,' but when you do so, the location of your arrow, whether correct or incorrect, does not affect your payment. Because the payment for 'don't know' is (Y × 100)% of the payment for getting an answer correct, you will on average earn more by selecting don't know than your best guess if you are less than (Y × 100)% sure that the bar underneath the arrow covers the correct answer."

Analysis of consultation of outside references: After the survey was over, we asked participants if they had looked up the answers to each question that they were asked, noting explicitly that "Your bonus is already determined, and we won't change your bonus in any way on the basis of your answer to these

questions." Of our 795 partisan participants, only 20 (2.5 percent) reported looking up the answer to 41 questions (0.74 percent of all questions asked). The percentages of user-questions by treatment assignment are 0.32 percent (control), 0.96 percent (pay for correct), and 0.64 percent (pay for correct and pay for don't know).

Sample representativeness. As with the Experiment 1 sample, one may be concerned about nonrepresentativeness of the Mechanical Turk sample that we use in Experiment 2. The Experiment 2 sample is far more diverse, and representative of the population of American partisans, than most samples that are used in studies of incentives: the large majority of those studies continue to be composed chiefly of undergraduates, and Mechanical Turk samples tend to be both more diverse and more representative than undergraduate samples (e.g., Berinsky, Huber, and Lenz, 355-65). Even so, one might fear that the sample overrepresents the interested or the knowledgeable, or those who are highly responsive to incentives, in ways that make the results unlike those that would be found in a more representative sample.

Consider first the concern about political interest. The finding that political interest does not moderate the effects of incentives should temper this concern: it suggests that overrepresentation of the interested would make little difference to the results. (See pages 12-13 and the discussion in the previous part of this online appendix.) Perhaps even more to the point, the 2012 Mechanical Turk sample does not seem to overrepresent those who have a great deal of interest in politics. Our Mechanical Turk subjects were asked

> Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say that you follow what's going on in government and public affairs…[most of the time / some of the time / only now and then / hardly at all]?

Only 28% of subjects responded "most of the time." In the 2008 ANES, which used an identical question (V085072), the corresponding percentage was 32%. (The question that we used to measure interest had been used for decades by the ANES, but it was dropped after the 2008 ANES time series study.)

Although highly interested people do not seem to be overrepresented in the Mechanical Turk sample, it remains possible that the sample overrepresents those who know a lot about politics. And

overrepresentation of the knowledgeable might limit the generalizability of our results. For example, if the Mechanical Turk sample contains an unusually large number of knowledgeable subjects, the effects of incentives may be larger in the sample than in ordinary populations: all else equal, knowledgeable partisans will be more able to converge to the same (correct) answer after being offered an incentive to do so.

Our Mechanical Turk sample includes the political knowledge item:

> Do you happen to know how much of a majority is required for the United States Senate and House to override a Presidential veto?

The response options to this question were "a majority (fifty percent plus one vote)," "two-thirds (sixty-seven percent)," "three-fourths (seventy-five percent)," "ninety percent," and "don't know." The question has not been asked in the ANES for decades, but it was asked in a 1999 RDD survey of Tallahassee residents that had an unusually high completion rate (Mondak and Davis 2000, esp. 221). We find that 72% of partisans answer the question correctly—a figure that is very close to the 74% that Mondak and Davis (2000, 213) find, albeit with a question that had slightly different response options.

Amazon.com's Mechanical Turk is an electronic forum in which "workers" offer to complete tasks—typically quite brief tasks—in exchange for money. Mechanical Turk subjects are thus those who are actively seeking small and immediate payments, and one might therefore worry that they are unusually responsive to the financial incentives that we offer for correct and "don't know" responses to knowledge questions. We begin to explore this possibility by nothing that the results that we obtained from Mechanical Turk subjects are similar to those that we obtained from a very different sample of participants—the 2008 CCES sample that we used in Experiment 1. Although CCES subjects are rewarded for their participation (as most survey subjects are), they are not included in the CCES sample on the basis of their willingness to perform small tasks in exchange for immediate payments. Even so, we find similar results across the two samples.

Of course, Experiment 2, which uses the Mechanical Turk sample, includes several innovations that do not appear in Experiment 1, including payments for "don't know" responses. (See pages 14-15 for details.) To more precisely replicate the Experiment 2 results, we included a one-question experiment in the 2012 CCES. The question was

> How did the unemployment rate in the country change between January 2009, when President Obama took office, and September 2012?

We offered seven response options: "decreased 2%" (coded 1), "decreased 1%" (.83), "no change" (.67), "increased 1%" (.5), "increased 2%" (.33), "increased 3%" (.17), "increased 4%" (0). As with our analysis of Experiment 2, respondents who selected "don't know" in the pay correct and don't know condition were assigned the mean (average) response among those in the control condition, regardless of their party. All other variable coding is consistent with Experiment 2. There were 573 subjects in the experiment.

The results are reported in Table OA2, and they are similar to those that we obtained in Experiment 2. Relative to the control condition, payments for correct responses and payments for correct and "don't know" responses both reduced partisan divergence. The effect of paying for both correct and don't know responses was larger than the effect of just paying for correct responses, but the difference was not statistically significant (F-test p-value = .16, one-tailed).[1]

PART III: RESULTS INCLUDING PARTISAN LEANERS

See Tables OA3 and OA4.

PART IV: ROBUSTNESS TO WITHIN, COLLAPSED, AND EXCLUDING CHEATERS ANALYSIS

See Tables OA5, OA6, and OA7.

[1] One may also expect that our estimates are too conservative because of something like panel conditioning: respondents may have taken so many surveys before ours that they have tired of surveys or otherwise become inured to the survey setting. Responses to a question at the end of our survey suggest that this may not be the case. Only 21% of our subjects reported taking at least six Mechanical Turk surveys (counting our own) in the previous month. By contrast, 56% of subject reported taking no more than two Mechanical Turk surveys in the previous month.

PART V: PREVIOUS RESEARCH ON PARTISAN DIVERGENCE IN FACTUAL ASSESSMENTS

A long line of research has noted partisan differences in evaluation of factual matters relating to politics. The questions in our experiments were chosen based on prior research documenting partisan divisions for similar topics. Here, we list the motivating research for our different questions. In Experiment 1, we asked questions about performance during the American invasions of Iraq and Afghanistan (see Jacobson 2010), economic performance during President Bush's tenure (see Bartels 2002; Evans and Andersen 2006; Kinder and Mebane 1983), and Obama and McCain's age during the 2008 campaign (see Pew 2008, documenting partisan divisions over whether McCain was too old to be president). Given stark partisan differences in assessments of president popularity, we also asked examined whether partisans differed in their assessments of Bush's overall and within-party popularity. In Experiment 2, for similar reasons we included questions about economic performance, the Iraq war, and Obama's election performance. The presence of partisan divides on preferences for government spending on health care and defense, the TARP (bailout) program, global warming, and attitudes toward immigrants led us to act factual questions in those areas too.

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PART VII: COMPLETE INSTRUCTIONS AND SCREENSHOTS FOR EXPERIMENT 2

(Begins following appendix tables and figures.)

Table OA1: Experiment 1: Effect of Payment for Correct Responses on
Partisan Differences in Scale Scores (Weighted and Unweighted Analyses)

	Weighted Analysis			Unweighted Analysis		
	(1)	(2)	(3)	(4)	(5)	(6)
Democrat (1=Yes, 0=Republican)	0.128	0.116	0.112	0.118	0.105	0.082
	[0.022]***	[0.022]***	[0.039]***	[0.015]***	[0.016]***	[0.022]***
Political interest × Democrat			0.033			0.059
			[0.044]			[0.030]**
Payment for correct response × Democrat	-0.063	-0.057	-0.045	-0.065	-0.059	-0.057
	[0.030]**	[0.025]**	[0.057]	[0.022]***	[0.022]***	[0.037]
Payment for correct response × Political interest × Democrat			-0.034			-0.023
			[0.065]			[0.046]
Payment for correct response	0.035	0.023	0.031	0.038	0.032	0.045
	[0.020]*	[0.017]	[0.041]	[0.016]**	[0.016]*	[0.029]
Payment for correct response × Political interest			0.005			-0.005
			[0.046]			[0.035]
Political interest (0,1)			0.002			-0.034
			[0.028]			[0.021]
Constant	0.277	0.249	0.276	0.239	0.163	0.261
	[0.033]***	[0.072]***	[0.041]***	[0.021]***	[0.060]***	[0.024]***
Observations	3321	3299	3305	3321	3299	3305
R-squared	0.354	0.369	0.355	0.398	0.407	0.400
Includes additional controls?	No	Yes	No	No	Yes	No

Note: Source: 2008 CCES. Includes only Democrats and Republicans. Robust standard errors, clustered by respondent. Question fixed effects not reported. The "unweighted analysis" results are the same as those that are reported in Table 2. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table OA2: Replication of Experiment 2 on 2012 CCES

	Obama Unemployment Performance (Higher Values Indicate Unemployment Decreased)
Democrat (1=Yes, 0=Republican)	0.366
	[0.050]***
Democrat * Pay Correct	-0.132
	[0.074]*
Democrat * Pay Correct and Don't Know	-0.222
	[0.072]***
Pay Correct	0.130
	[0.053]**
Pay Correct and Don't Know	0.161
	[0.053]***
Constant	0.235
	[0.035]***
Observations	593
R-squared	0.109
F-test, Pay Correct * Dem. > Pay DK and Correct * Dem.	0.110

Note: Source: 2012 CCES. Includes only Democrats and Republicans. Robust standard errors. F-test p-values are one-tailed. * indicates significant at 10%; ** significant at 5%; *** significant at 1%.

Table OA3: Experiment 1 Including Partisan Leaners: Effect of Payment for Correct Responses on Partisan Divergence in Scale Scores

	(1)	(2)	(3)
	Mean Scale Score (0 to 1) (Pooled for 8 questions with partisan gap, p<.10, among control cases)		
Democrat (1=Yes, 0=Republican)	0.115	0.104	0.088
	[0.013]***	[0.015]***	[0.021]***
Interest * Democrat			0.042
			[0.027]
Payment for Correct Response * Democrat	-0.061	-0.056	-0.060
	[0.020]***	[0.019]***	[0.034]*
Pay Correct * Interest * Democrat			-0.007
			[0.042]
Payment for Correct Response	0.032	0.029	0.039
	[0.014]**	[0.014]**	[0.026]
Pay Correct * Interest			-0.007
			[0.032]
Knowledge (0,1)		-0.001	
		[0.011]	
Race: White (1=yes)		-0.030	
		[0.017]*	
Race: Black (1=yes)		-0.045	
		[0.026]*	
Race: Hispanic (1=yes)		-0.020	
		[0.025]	
Female (1=yes)		0.012	
		[0.010]	
Age (Years)		0.002	
		[0.002]	
Age-squared/100		-0.003	
		[0.002]	
Region: Northeast		0.029	
		[0.015]**	
Region: Midwest		0.028	
		[0.014]**	
Region: South		0.002	
		[0.013]	
Income (1=<10k; 14=>150k; 15=RF/Missing)		0.004	
		[0.002]**	
Income Missing		-0.036	
		[0.022]	
Education (1=No HS; 6=Post-grad)		-0.004	
		[0.005]	
Education: No HS		-0.003	
		[0.024]	
Education: Some college		0.022	
		[0.013]*	
Education: 2-year college		0.020	
		[0.020]	
Education: 4-year college		0.008	
		[0.016]	
Married/Domestic Partnership (1=yes)		-0.008	
		[0.011]	
Religious Attendance (1-6)		-0.001	
		[0.003]	
Political Interest (0,1)			-0.026
			[0.019]
Constant	0.231	0.205	0.248
	[0.018]***	[0.053]***	[0.022]***
Observations	4229	4199	4213
R-squared	0.405	0.414	0.407

Note: Source: 2008 CCES. Includes only Democrats and Republicans (with leaners). Robust standard errors, clustered by respondent. Question fixed effects not reported. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table OA4: Experiment 2 Including Partisan Leaners: Effect of Payment for Correct Responses on Partisan Divergence in Scale Scores

	(1)	(2)	(3)
Sample	All 10 non-placebo questions with partisan-gaps (p<.10) pre-treatment		
Specification	OLS	Tobit	OLS
Democrat (1=Yes, 0=Republican)	0.111	0.116	0.111
	[0.024]***	[0.025]***	[0.024]***
Payment Correct * Democrat	-0.056	-0.057	
	[0.026]**	[0.028]**	
Payment DK and Correct * Democrat	-0.076	-0.079	
	[0.025]***	[0.027]***	
Payment for Correct Response	0.013	0.011	
	[0.020]	[0.021]	
Payment for DK and Correct Response	0.039	0.038	
	[0.020]**	[0.020]*	
Amount correct = 0.10 * Democrat			-0.053
			[0.029]*
Amount correct = 0.25 * Democrat			-0.062
			[0.029]**
Amount correct = 0.50 * Democrat			-0.073
			[0.029]**
Amount correct = 0.75 * Democrat			-0.007
			[0.032]
Amount correct = 1.00 * Democrat			-0.083
			[0.035]**
Prop. payment for DK=.20 * Democrat			-0.021
			[0.018]
Prop. payment for DK=.25 * Democrat			-0.020
			[0.020]
Prop. payment for DK=.33 * Democrat			-0.015
			[0.019]
Amount correct = 0.10			0.014
			[0.023]
Amount correct = 0.25			0.019
			[0.022]
Amount correct = 0.50			0.023
			[0.023]
Amount correct = 0.75			-0.024
			[0.025]
Amount correct = 1.00			0.037
			[0.028]
Prop. payment for DK=.20			0.021
			[0.014]
Prop. payment for DK=.25			0.028
			[0.018]
Prop. payment for DK=.33			0.019
			[0.016]
Constant	0.625	0.632	0.626
	[0.020]***	[0.021]***	[0.020]***
Observations	5880	5880	5880
R-squared	0.176		0.178
F-test, Pay Correct * Dem. > Pay DK and Correct * Dem.	0.080	0.080	

Source: Mechanical Turk, March-April 2012. The dependent variable is the mean scale score for the ten questions on which we observed pre-treatment partisan gaps of p < .10. It ranges from 0 to 1. The analysis includes only Democrats and Republicans (with leaners). Cell entries are coefficients with robust standard errors, clustered by respondent. Question fixed effects are not reported. * significant at 10%; ** significant at 5%; *** significant at 1% (two-tailed tests).

Table OA5: Experiment 2 Within person analysis

	(1)	(2)
	Post-treatment cases asked in pre, all questions with partisan gap among pre-treatment cases, p<.10	
Pre (Lagged) directed slider response	0.636	
	[0.015]***	
Democrat (1=Yes, 0=Republican)	0.077	0.160
	[0.014]***	[0.029]***
Payment Correct * Democrat	-0.066	-0.098
	[0.016]***	[0.033]***
Payment DK and Correct * Democrat	-0.090	-0.127
	[0.017]***	[0.031]***
Payment for Correct Response	0.032	0.022
	[0.011]***	[0.027]
Payment for DK and Correct Response	0.057	0.056
	[0.012]***	[0.026]**
Constant	0.198	0.608
	[0.016]***	[0.027]***
Observations	3275	3275
R-squared	0.638	0.190
F-test, Pay Correct * Dem. > Pay DK and Correct * Dem.	0.030	0.060

Source: Mechanical Turk, March-April 2012. The dependent variable is the mean scale score for the ten questions on which we observed pre-treatment partisan gaps of $p < .10$. It ranges from 0 to 1. The analysis includes only Democrats and Republicans answering questions they also answered pre-treatment. Cell entries are coefficients with robust standard errors, clustered by respondent. Question fixed effects are not reported. * significant at 10%; ** significant at 5%; *** significant at 1% (two-tailed tests).

Table OA6: Experiment 2 Excluding cheaters from the analysis

	(1)	(2)
	Post-treatment cases, all questions with partisan gap among pre-treatment cases, p<.10	Excluding people who report any cheating.
Democrat (1=Yes, 0=Republican)	0.145	0.149
	[0.028]***	[0.028]***
Payment Correct * Democrat	-0.087	-0.090
	[0.030]***	[0.030]***
Payment DK and Correct * Democrat	-0.117	-0.123
	[0.029]***	[0.029]***
Payment for Correct Response	0.018	0.018
	[0.025]	[0.025]
Payment for DK and Correct Response	0.049	0.050
	[0.024]**	[0.024]**
Constant	0.614	0.613
	[0.026]***	[0.026]***
Observations	4608	4492
R-squared	0.179	0.179
F-test, Pay Correct * Dem. > Pay DK and Correct * Dem.	0.020	0.010

Source: Mechanical Turk, March-April 2012. The dependent variable is the mean scale score for the ten questions on which we observed pre-treatment partisan gaps of p < .10. It ranges from 0 to 1. The analysis includes only Democrats and Republicans. Cell entries are coefficients with robust standard errors, clustered by respondent. Question fixed effects are not reported. * significant at 10%; ** significant at 5%; *** significant at 1% (two-tailed tests).

Table OA7: Experiment 2 Collapsed analysis to one observation per participant

	Post-treatment cases, all questions with partisan gap among pre-treatment cases, p<.10
Democrat (1=Yes, 0=Republican)	0.146
	[0.023]***
Payment Correct * Democrat	-0.091
	[0.026]***
Payment DK and Correct * Democrat	-0.118
	[0.025]***
Payment for Correct Response	0.023
	[0.021]
Payment for DK and Correct Response	0.050
	[0.021]**
Constant	0.546
	[0.030]***
Observations	795
R-squared	0.175
F-test, Pay Correct * Dem. > Pay DK and Correct * Dem. one-tailed	0.050

Source: Mechanical Turk, March-April 2012. The dependent variable is the mean scale score for that respondent across all the questions on which we observed pre-treatment partisan gaps of $p < .10$. It ranges from 0 to 1. The analysis includes only Democrats and Republicans. Cell entries are coefficients with robust standard errors. Question fixed effects are not reported. * significant at 10%; ** significant at 5%; *** significant at 1% (two-tailed tests).

You are being asked to complete an online research survey that will take approximately 7-9 minutes. The survey is conducted by researchers at REDACTED to study how people learn. This page describes your consent.

Findings from this study may be reported in scholarly journals, at academic seminars, and at research association meetings. The data will be stored at a secured location and retained indefinitely. No identifying information about you will be made public and all of your choices will be kept completely confidential. Your participation is voluntary. You are free to stop the survey at any time without penalty.

There are no known risks associated with this study beyond those associated with everyday life. Although this study will not benefit you personally, we hope that our results will add to the knowledge about how people learn. You will receive $0.50 for completing the survey, paid through Amazon Mechanical Turk. You will also have the opportunity to earn a bonus of $0.50 or more, although not everyone will receive a bonus.

To participate in the study, you must be at least 18 years old and a United States resident. JavaScript must be activated on your browser so that the graphics in the survey will work properly. The next page will test your browser.

If you have any questions about the research, you can contact REDACTED. If you have any questions about your rights as a research participant or concerns about the conduct of this study, you may contact the REDACTED Human Subjects Committee, Box REDACTED, REDACTED, REDACTED, REDACTED, REDACTED@REDACTED.edu.

When you are ready to begin, please elect to participate and press the Submit button. You will then be taken to the first page of the survey.

- (•) I agree to participate.
- ( ) I do not agree to participate.

Submit

To confirm that our survey graphics will work with your browser, please follow the instructions in the graphic below. You have 20 seconds to complete this task. After 20 seconds, your browser will automatically proceed to the next page.

Please drag the arrow as far as you can to the right. You can move the arrow by clicking on the arrowhead and dragging.

You have 16 seconds to submit your answer before your current answer is automatically submitted.

Please read carefully and answer the following questions.

Here are two personality traits that may or may not apply to you. Please check the box to indicate the extent to which *you agree or disagree with each statement*. You should rate the extent to which the pair of traits applies to you, even if one characteristic applies more strongly than the other. To demonstrate that you've read this much, just go ahead and select both disagree strongly and agree strongly for both questions below, no matter how you would actually answer each question. In other words, to confirm that you are paying attention, for each question please check both of these two boxes.

I see myself as: Dependable, self-disciplined.

- Agree strongly.
- Agree moderately.
- Agree a little.
- Neither agree nor disagree.
- Disagree a little.
- Disagree moderately.
- Disagree strongly.

I see myself as: Disorganized, careless.

- Agree strongly.
- Agree moderately.
- Agree a little.
- Neither agree nor disagree.
- Disagree a little.
- Disagree moderately.
- Disagree strongly.

Next

Please read carefully and answer the following questions.

What is the highest level of education that you have achieved?

- No high school diploma.
- High school diploma or equivalent.
- Some college.
- Two year degree.
- Four year college graduate.
- Post-graduate.

What is the year of your birth?

What is your gender?

- Female.
- Male.

What is your state of residence?

Generally speaking, do you usually think of yourself as a Democrat, a Republican, an Independent, or what?

- Democrat.
- Republican.
- Independent.
- Other.

Next

Please read carefully and answer the following questions.

Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs...?

- Most of the time.
- Some of the time.
- Only now and then.
- Hardly at all.

We are interested in the kinds of things people do when they use the internet. What kinds of things have you used the internet for in the LAST SEVEN DAYS? (Choose as many as apply to you)

- Get directions.
- Plan vacations.
- Keep in touch with friends.
- Look at sports highlights.
- Find restaurants.
- Pay bills.
- Look up movie times.
- Shopping.
- Read the news.
- Read about politics.

Do you happen to know how much of a majority is required for the United States Senate and House to override a Presidential veto?

- A majority (fifty percent plus one vote).
- Two-thirds (sixty-seven percent).
- Three-fourths (seventy-five percent).
- Ninety percent.
- Don't know.

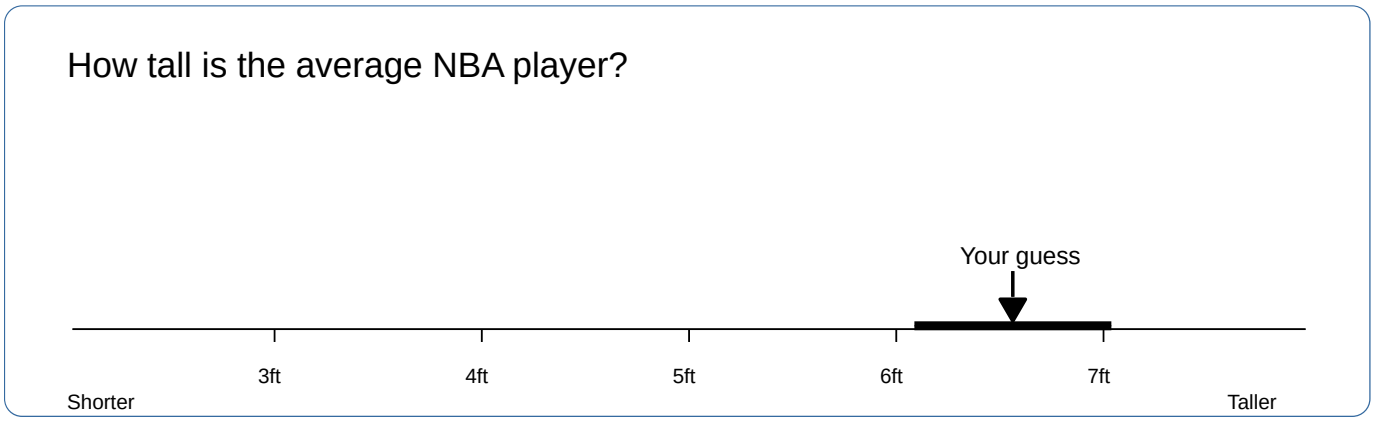
Do you think most professional athletes are good role models for children today?

- Yes.
- No.
- Don't know.

Next

In this study, we'd like you to tell us **what you think the correct answer is** to a number of questions. We don't want you to look up those answers or talk to someone else, so even if you don't know please just give us your best guess. For each question, we'll give you a short period of time -- 30 seconds -- to answer the question before we automatically take you to the next question.

To indicate your answer, we will ask you to slide the arrow on a line like that below to the point that is closest to your answer. You can slide that arrow by clicking your mouse on the arrowhead and dragging it to the left or right.

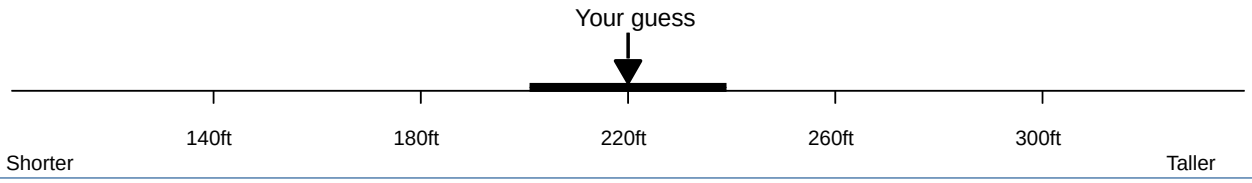


For example, in the above graphic, if you though the correct answer was 6 feet 6 inches, you would slide the arrow to the point midway between the lines marked 6 and 7 ft.

Give it a try! Once you are happy with where the arrow is located, you can click "Next." On the next page, we'll give you a timed example with another question.

Next

How tall is the Statue of Liberty, in feet, from the base of the feet to the top of the torch?



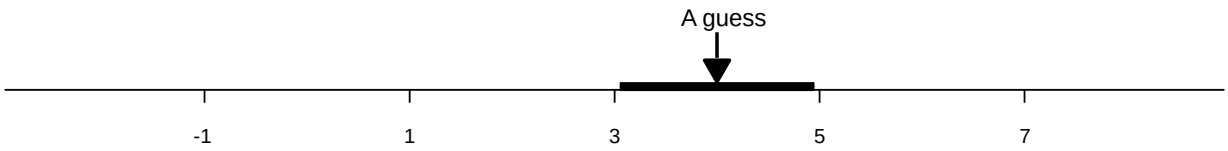
In this example, we are asking you to indicate your best guess as to how tall the Statue of Liberty is. You can also see how the countdown timer works -- you have 45 seconds to indicate your answer (see below). After you've indicated your best guess, click "Next" or just wait to go to the next page. When the timer is up, you will automatically be routed to the next page.

You have 45 seconds to submit your answer before your current answer is automatically submitted.

Next

We're almost ready to begin. Before we proceed, we just want to make sure we've been clear about what we are asking you to do.

Dave has two dozen apples. He eats half of them, and then eight more. How many apples are left?



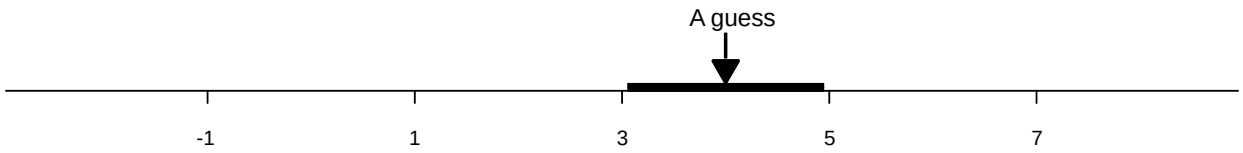
In the graph above, we've placed the arrow at a certain point to indicate somebody's response to the question. Which of the following has that person indicated is their best guess?

Their best guess is...

- 1.
- 2.
- 3.
- 4.
- 5.
- None of the above.

Next

Dave has two dozen apples. He eats half of them, and then eight more. How many apples are left?



The arrow is located midway between 3 and 5, so the person's response is 4.

Next

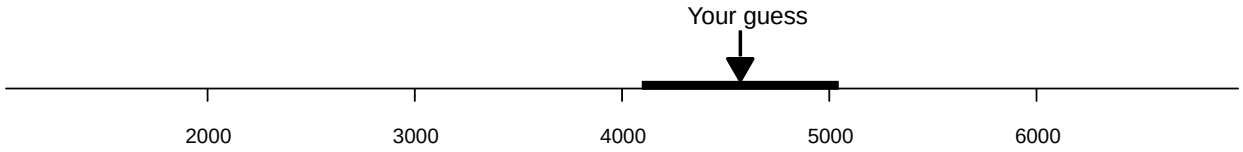
We will now send you to the actual survey. On the next screen, you will be presented with your first question and will only have **a limited amount of time to respond**.

Please **do not use the back button in your browser** during this survey. Any questions your answer a second time by using the back button will not be recorded. When you are ready, please click Next.

Next

Please drag the slider to your best guess to the following

About how many U.S. soldiers were killed in Iraq between the invasion in 2003 and the withdrawal of troops in December 2011?

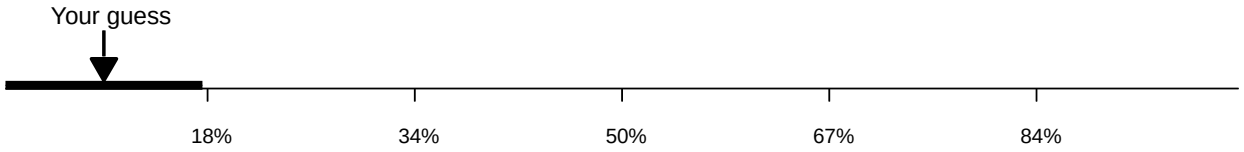


You have 27 seconds to submit your answer before your current answer is automatically submitted.

Next

Please drag the slider to your best guess to the following

According to the Census Bureau, in 2010 what percentage of the total population of the United States was born outside of the United States (foreign-born)?



You have 28 seconds to submit your answer before your current answer is automatically submitted.

Next

Thank you for answering those questions, we'd now like you to answer a few more questions.

Once again, your answers will be timed.

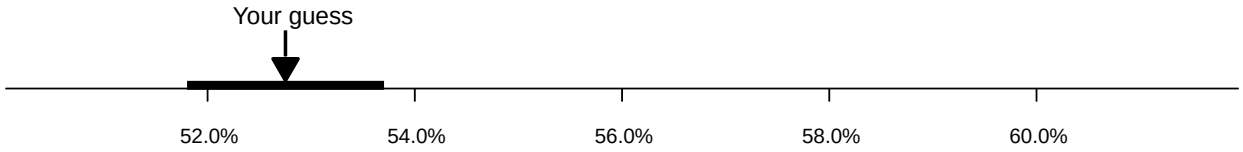
By answering these questions, you will earn an additional 50¢ bonus.

Again, please **do not use the back button in your browser**. Any questions your answer a second time by using the back button will not be recorded. When you are ready to proceed, please click Next.

Next

Please drag the slider to your best guess to the following

In the 2008 Presidential Election, Barack Obama defeated his Republican challenger John McCain. In the nation as a whole, of all the votes cast for Obama and McCain, what percentage went to Obama?

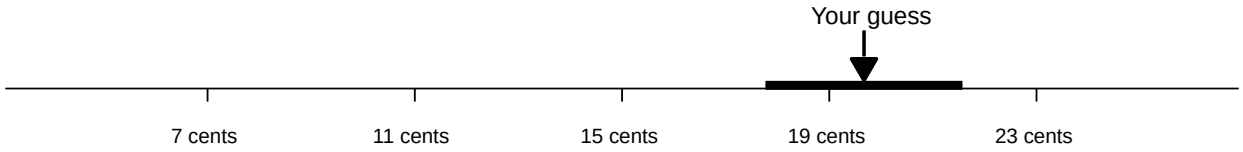


You have 28 seconds to submit your answer before your current answer is automatically submitted.

Next

Please drag the slider to your best guess to the following

For every dollar the federal government spent in fiscal year 2011, about how much went to the Department of Defense (US Military)?



You have 26 seconds to submit your answer before your current answer is automatically submitted.

Next

Thank you for your participation!

Your bonus is already determined, and we won't change your bonus in any way on the basis of your answer to these questions. For research purposes...

Did you look up the answer to this question?

In the 2008 Presidential Election, Barack Obama defeated his Republican challenger John McCain. In the nation as a whole, of all the votes cast for Obama and McCain, what percentage went to Obama?

- No, I did not look up th answer to this question.
- Yes, I did look up the answer to this question.

Did you look up the answer to this question?

For every dollar the federal government spent in fiscal year 2011, about how much went to the Department of Defense (US Military)?

- No, I did not look up th answer to this question.
- Yes, I did look up the answer to this question.

Did you look up the answer to this question?

About how many U.S. soldiers were killed in Iraq between the invasion in 2003 and the withdrawal of troops in December 2011?

- No, I did not look up th answer to this question.
- Yes, I did look up the answer to this question.

Did you look up the answer to this question?

According to the Census Bureau, in 2010 what percentage of the total population of the United States was born outside of the United States (foreign-born)?

- No, I did not look up th answer to this question.
- Yes, I did look up the answer to this question.

Did you look up the answer to this question?

Compared to January 2001, when President Bush first took office, how had the level of unemployment, as measured using the unemployment rate, in the country changed by the time he left office in January 2009?

- No, I did not look up th answer to this question.
- Yes, I did look up the answer to this question.

Did you look up the answer to this question?

The Treasury Department initiated TARP (the first bailout) during the financial crisis of 2008. TARP involved loans to banks, insurance companies, and auto companies. Of the $414 billion spent, what percentage had been repaid, as of March 15, 2012?

- No, I did not look up th answer to this question.
- Yes, I did look up the answer to this question.

Did you look up the answer to this question?

Medicaid is a jointly funded, Federal-State health insurance program for low-income and needy people. For every dollar the federal government spent in fiscal year 2011, about how much went to Medicaid?

- No, I did not look up th answer to this question.
- Yes, I did look up the answer to this question.

Next

Thank you for your participation!

What is the total number of Mechanical Turk surveys you have taken about current events or politics?

What is the total number of Mechanical Turk surveys you have taken about current events or politics **in the last month**?

If you would like to be contacted when we have future studies, please leave your email here. If not, leave blank:

If you would like to leave any comments or feedback, please do so here (up to 500 characters):

Pleast continue to the next page to retrieve your code for payment!

Next

Thank you for your participation!

You have now completed the survey.
If you have any questions, please contact REDACTED@REDACTED.edu. If you have any questions about your rights as a research participant or concerns about the conduct of this study, you may contact the REDACTED Human Subjects Committee at REDACTED@REDACTED.edu.

For the purposes of getting paid on Mechanical Turk, please enter the following code into the box on the survey's Mechanical Turk HIT page. You must enter this code to get your bonus.

vuhtkwysobinecs

If you are curious about the sources we used to score your answers, please contact us through the Mechanical Turk interface and we are happy to provide references to you. Thank you!