

Is Partisan Bias in Perceptions of Objective Conditions Real?
The Effect of an Accuracy Incentive on the Stated Beliefs of Partisans

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Version 1.0. Preliminary and Incomplete. Comments welcome.

Paper prepared for presentation at the 65th Annual Meeting of the Midwest Political Science Association, Chicago, Illinois, April 12-15, 2007.

This paper uses data collected as part of a joint research project with Arthur Lupia. We thank the Center for Political Studies at the University of Michigan and the University Committee on Research in the Humanities and Social Sciences at Princeton University for funding this research. We thank Rick Li, Mike Dennis, Bill McCready and Vicki Huggins at Knowledge Networks for assistance in programming and implementing the study. For comments on the analysis reported here, we thank Larry Bartels, Marty Gilens, and seminar participants at Princeton University.

A long line of research has argued that people's political evaluations are affected by their partisan predispositions. People tend to accept information that is consistent with their preexisting beliefs. They tend to reject information that contradicts their beliefs. *The American Voter* (Campbell et al. 1960) called this a "perceptual screen." Research has documented the power of "partisan schemas" in candidate evaluations (Lodge and Hamill 1986; Rahn 1993). The literature on "motivated reasoning" is growing (e.g., Lodge and Taber 2000). The persistence of this research paradigm is not entirely surprising: Politics is contestable. There is no such thing as an objective political evaluation.

Recent work has begun to broaden the claim even further. Not only do partisans often disagree about political interpretations, they do not agree on political facts either. Bartels (2002) provides the most stunning evidence to date for partisan differences in beliefs about objective conditions. In 1988, the National Election Study asked respondents if "compared to 1980, the level of unemployment in the country has gotten better, stayed about the same, or gotten worse?" Even though the unemployment rate had declined from 7.1 in 1980 to 5.5 in 1988, only about 30 percent of strong Democrats said that "the level of unemployment... has gotten better." Among strong Republicans, in contrast, over 80 percent gave the correct answer. Among weak or leaning Republicans, around 70 percent did.

The mismatch between responses and reality is similarly severe for other retrospective evaluations of reality. Inflation declined noticeably under Reagan, from 13.5 in 1980 to 4.1 in 1988. Yet almost half of all Democrats say that inflation worsened. When the NES repeated these kinds of questions in 2000 to ask about changes in economic conditions during President Clinton's tenure, Democrats did much better, but Republicans responded incorrectly in large numbers (Bartels 2002). Hence, it would be wrong to single out Democrats as singularly susceptible to partisan bias. Instead, people who do not share the same party affiliation as the president are systematically less likely to acknowledge positive economic change.

Why do partisans differ so dramatically in their reported beliefs about economic realities? This is an important question because without agreement on some key facts, democratic decision-making becomes rather difficult. Hochschild (2001, 321), for example, argues that misperceptions can affect citizens' application of political values to policy questions. People who get relevant facts wrong may easily get their attitudes and preferences "wrong," too. In fact, uninformed people may even get their values wrong.

In this paper, we empirically evaluate two fundamentally different explanations for partisan differences in response to factual questions about economic and political conditions. One possibility is that partisans do not know any better when they report their beliefs about these facts. For a variety of reasons that we describe below, they may never have received or already forgotten the correct information. But a second explanation is also conceivable: Partisans give the wrong answer even though they have more accurate information. They may disregard this information and instead give an answer that is more consistent with their partisan predispositions.

The implications from these two alternative explanations are very different. To the extent that the second explanation is correct—partisans give incorrect responses even though they have information that proves them wrong—the threat of partisan bias for political reasoning is exaggerated because people can base their evaluations on more accurate information than their answers to factual survey questions reveal. In contrast, if partisans' answers reflect information they believe to be accurate, our study would affirm the persistence of partisan bias in political reasoning.

In order to distinguish between these two explanations for observed partisan differences, we take advantage of an experiment that manipulates respondents' incentives to be accurate. If partisan differences persist unabated despite encouragement to respond correctly, these differences are likely to reflect genuine beliefs. If not, we have shown that respondents can in fact provide less biased answers when they are motivated to do so.

We begin by describing our alternative hypotheses more thoroughly. We then describe our empirical test and report the results. The final section of the paper discusses what our findings imply for people's political decision-making and the role of partisan bias in this process.

Theory and Hypotheses

In developing attitudes and beliefs about a wide variety of political objects, individuals have at least two, sometimes competing, goals. They want their attitudes and beliefs to be consistent with each other, and they want them to reflect reality. Brady and Sniderman (1985) used these insights to develop and empirically support a model of attitude attribution. Their objective was to explain how people develop beliefs about something they do not know (the positions of the Republican and the Democratic parties). We will argue that accuracy and consistency goals should operate even when people have already developed a belief about the correct answer and are asked to report it. The Brady-Sniderman model can be adapted to help us understand partisan bias in responses to factual knowledge questions.

Brady and Sniderman start with the observation that many Americans place the position of the Republican party to the right of the Democratic party on many issues despite their general lack of detailed political knowledge. Apparently, many people can infer the relative positions of the two parties with some accuracy by using their feelings towards the parties as an information shortcut. The party you like better will often be closer to your own position on an issue. This "likability heuristic" works for other groups, including liberals and conservatives, as well.

When respondents are asked to place the two parties on a left-right dimension, they have a set of different and often opposing goals. They want to give an answer that is close to what they believe to be the truth (accuracy goal), but they also want their answer to reflect their existing feelings and beliefs (consistency goal). The consistency goal motivates them to place the parties in a way that corresponds to their own issue preferences and their affect towards the parties. The accuracy goal may work in the opposite direction. Someone who favors government aid to the

poor, likes Republicans, and dislikes Democrats will be tempted to report that Republicans support the poor more generously than Democrats even if he has some doubts that this is factually correct. Typically, accuracy and consistency work in the same direction or people's factual information is so thin that accuracy constraints are minimal. Most people who favor aid for the poor like Democrats better than Republicans, and few people know details about the parties' actual record on support for low-income citizens. As a result, relying on their feelings towards the parties often helps people to place the parties correctly.

Brady and Sniderman take their model as an explanation of how respondents actually perceive the parties' positions on political issues. The likability heuristic helps people figure out their actual beliefs about the parties' issue positions. Yet their model can also be seen as a model of the survey response. When asked about a party's issue position, respondents may either report their true belief or give an answer that diverges from their true belief in order to be consistent with their preferences and feelings towards the party. The two goals, accuracy and consistency, may affect not only the development of beliefs, but also how beliefs are reported during a survey. In response to a factual question, respondents may decide to provide the answer they believe to be accurate or the answer they would like to be true (or, more likely, a combination of the two). This weighing of accuracy and consistency goals may happen consciously or unconsciously.

Previous research in social psychology and political science provides a host of mechanisms that can be subsumed as consistency goals. If respondents care about their appearance to the interviewer, they might knowingly give a consistent response for symbolic (or expressive) purposes. Strategic reasons might lead them to give consistent answers in the hope of influencing public opinion. Probably the most prominent explanation of consistency pressures is cognitive dissonance theory (Festinger 1957). Holding inconsistent beliefs causes discomfort that people try to relieve by making their beliefs internally consistent, for example by discounting inconsistent evidence as less credible. When inconsistent information is relevant to a survey

question, respondents' consistency goals might compel them to ignore it in their response. Each one of these explanations of consistency pressures leads us to the same hypothesis:

H1: In response to factual questions with partisan relevance, respondents give false answers although they hold more accurate information in memory.

Our study is not designed to test which of the different plausible explanations of consistency pressures is most accurate. (There is no reason not to expect all of them to be at work.) Neither can we address if consistency pressures operate at a conscious or subconscious level. Our objective is simply to test H1 against the following null hypothesis:

H1_{null}: In response to factual questions with partisan relevance, respondents give the answer they believe to be most accurate.

If H1_{null} is correct, partisans who give biased answer do not know that their beliefs about reality are inaccurate. Either they have no relevant information at all and provide a guess that is influenced by their partisan preferences. Or selective exposure and/or selective perception have led them to store inaccurate information in their memory. According to H1_{null}, only the accuracy goal guides responses to factual survey questions. Consistency goals do not operate during the response process.

We evaluate the two competing hypotheses by experimentally manipulating how much weight respondents give to the accuracy goal. A randomly selected subset of respondents is offered one dollar for each correctly answered knowledge question. If there is indeed a trade-off between accuracy and consistency, the experimental treatment should reduce the impact of the consistency goal. H1 predicts that encouraging respondents' accuracy goal should reduce the partisan difference in beliefs they report about economic or political facts. Respondents in the treatment group should provide answers that are less consistent with their partisan predispositions. H1_{null}, in contrast, predicts that the monetary incentive will have no effect on partisan differences in stated beliefs.

Our survey followed a period of unified government with a Republican president and (narrow) Republican majorities in both houses of Congress. To the extent that respondents deem

the federal government responsible for economic conditions, the direction of partisan distortions should be clear. If consistency pressure matters, Republicans should report economic conditions as more favorable than Democrats. Even in the absence of partisan consistency pressures, respondents may be wrong. A host of other factors may explain why people are misinformed about objective conditions. Media give more coverage to negative news (Groeling and Kernell 1998; Patterson 1993), so regardless of their partisan identification, people may underestimate economic performance. Similarly, and perhaps related, Hochschild (2001) suggests that people are pessimistic about public life (but optimistic about their own private situation.) Cognitive biases, including innumeracy, may also contribute to misinformation (e.g., Kahneman and Tversky 1984; Nadeau, Niemi, and Levine 1993). Hence, H1 does not imply that the beliefs of Democrats and Republicans will converge on the correct answer, only that they will converge on each other. (As we show below, both partisan groups often deviate in the same direction from the correct answer.)

According to H1, consistency goals interfere with accuracy goals only if the factual knowledge question has partisan relevance. As a difference in kind, this is obvious. We would not expect partisan bias on a question about the weather or the number of Supreme Court justices. Our concern is with differences in degree. As many respondents are influenced by cues provided in the question text, question wording could affect the de facto partisan relevance of a question. Even without partisan cues, some, but not all, policy issues may have enough partisan salience to trigger consistency goals. Finally, for very difficult questions non-partisan error variance may simply overwhelm partisan distortions. These considerations lead us to expect only conditional acceptance of H1. Hypothesis 1a states this expectation explicitly:

H1a: The effect of an accuracy incentive on partisan difference in factual beliefs varies as a function of features of the knowledge question, including relevance to partisan politics, emphasis on partisan politics, difficulty of the question.

To the extent that our experimental manipulation succeeds in encouraging respondents to consider information in memory that is inconsistent with their partisan predispositions, it may

affect not only reported beliefs but also subsequent political evaluations. If the experimental treatment changes the relative salience of consistent and inconsistent information in respondents' minds and if respondents derive (or at least update) evaluations when they are asked to report them, then evaluations should be more strongly affected by inconsistent information in the treatment condition.

H2: Encouraging respondents to answer factual knowledge questions accurately reduces the impact of party identification on subsequent political evaluations.

Previous research suggests a strong null hypothesis. According to the online model of candidate evaluation (Lodge, McGraw, and Stroh 1989; Lodge, Steenbergen, and Braun 1995), people update their existing summary evaluations when they encounter *new information* about the candidates. Once the update occurred, they may forget the original information, but can report their summary evaluation in a survey interview or use it to determine their vote choice. Since respondents in our experiment *by design* do not receive any new information, they should not update their political evaluations. Any updating that occurs simply because respondents reconsider information they have already acquired and stored in their memory is inconsistent with the online model because, according to the model, people forget most information quickly. Furthermore, if information already stored in memory can lead to changes in candidate evaluations, the online model essentially becomes a memory-based model—which is exactly its antithesis.

H2_{null}: Encouraging respondents to answer factual knowledge questions accurately does not affect subsequent political evaluations because it does not provide respondents with new information.

The competing hypotheses are evaluated by comparing the relationship between party identification and vote intention for the 2004 presidential election in the two experimental groups. Respondents were asked their party identification in the first part of the study, before any experimental manipulation took place. After completing the knowledge questions in one of the two experimental conditions, all respondents answered the same vote intention question. Any differences in vote intention or its relationship with party identification between the two

experimental groups must therefore be due to the encouragement of accuracy in answering the knowledge questions.

Research Design and Measures

We offered one randomly selected half of our sample a monetary reward, one dollar, for each correct answer. We chose \$1 per question because we assumed that the amount would be non-trivial for many respondents and because this amount allowed us to stay within our budget while generating a sufficient number of cases per cell for rigorous statistical evaluations (see Bassi, Morton, and Williams 2006, for a recent review of the consequences of incentive payments for respondent effort in political science experiments).

In our Internet-based survey, which respondents completed using a computer or a WebTV unit, the knowledge questions appeared after an initial battery that solicited the respondent's party identification, level of interest in politics, and prior turnout. Next, all respondents saw a common introduction:

In the next part of this study, you will be asked 14 questions about politics, public policy, and economics. Many people don't know the answers to these questions, but it is helpful for us if you answer, even if you're not sure what the correct answer is. We encourage you to take a guess on every question. At the end of this study, you will see a summary of how many questions you answered correctly.

Respondents in the pay conditions then received the following instructions:

We will pay you for answering questions correctly. You will earn 1,000 bonus points (\$1) for every correct answer you give. So, if you answer 3 of the 14 questions correctly, you will earn 3,000 bonus points (\$3). If you answer 7 of the 14 questions correctly, you will earn 7,000 bonus points (\$7). The more questions you answer correctly, the more you will earn.¹

¹ Respondents received credit for correct answers in the form of "bonus points." The firm that conducted our study, Knowledge Networks, sends their panelists checks for \$25 when they reach 25,000 points (which they can also earn in other surveys they take.) For all practical purposes, we consider our incentives direct cash rewards. The instructions in the pay condition mentioned the bonus points as well as their dollar equivalents. Respondents in the pay conditions were reminded on every screen with a knowledge question that a correct answer would earn them a specific monetary reward.

Some of these questions were open-ended, others multiple choice. (For question wording and response options, see appendix table A1.) To facilitate payment for open-ended questions—all of which asked for a number between 0 and 100—in the relevant experimental condition, we specified in advance a range of answers (e.g., “within X percentage points of the true percentage”) that would earn compensation. Respondents were told the number of questions they answered correctly (and the rewards they had earned) at the very end of the interview. This sequence is necessary because we asked some post-treatment questions about the election and wanted to avoid the possibility of performance feedback contaminating responses to these final questions.

We followed recommendations by Mondak and Davis (2001) and Krosnick et al. (2002) to discourage “Don’t Know” responses by not giving respondents explicit “Don’t Know” options. While our respondents could hit the “next question” button without marking any answer, few of them did. Discouraging “Don’t Know” responses reduces distortions because, in the absence of encouragement, respondents may vary in their propensity to guess—and hence offer more correct answers with positive probability—for reasons that are orthogonal to their true levels of knowledge (e.g., relating to personality, social status, and confidence).

The survey was programmed to allow respondents 60 seconds to read and answer each knowledge question. Respondents were informed about this time constraint at the start of the knowledge sequence. (Respondents had the opportunity to pause the interview when they learned that they would be asked political knowledge questions, but they could not stop the relevant timers once they saw the first knowledge question.) Whether or not respondents had selected one of the response options, the next knowledge question appeared automatically after 60 seconds. Respondents could also move to the next question sooner by clicking the “Next Question” button. The software recorded the response selected when respondents moved to the next question.

Our experiment was embedded in a representative survey of U.S. residents conducted by Knowledge Networks between October 19 and November 1, 2004. Knowledge Networks

interviews national probability samples over the Internet by providing a large panel, selected through Random Digit Dialing, with WebTV units and/or free Internet connections in exchange for taking surveys. The participants for this study constitute a randomly selected subset of the KN panel and approximate a random sample of the U.S. adult population. The version of our survey on which we draw in this paper was assigned to 775 panelists of whom 618 (80 percent) completed it.²

Results

We begin the presentation of results with two questions that illustrate the range of questions types. The first question is about the level of public debt. In the question text, it mentions the level of debt in 2001 and the fact that George W. Bush took office that year. Even though the question offers seven different response options (more than our other multiple-choice questions), the reference point suggests the magnitude of the response. Knowing that the debt had increased since 2001 is enough to eliminate three response options. The reference to Bush suggests partisan relevance and provides respondents with an easy opportunity to apply their consistency goal. The mention of past debt levels and the explicit partisan reference make this one of the easiest questions we asked and one on which we expect to see clear signs of partisan consistency.

As Figure 1 shows, results confirm these expectations. The figure shows average responses for Democrats, Republicans, and independents. Partisan leaners are included with their

² We examined whether assignment to the experimental conditions affected completion rates (i.e., whether paying respondents for correct answers would affect the likelihood that they complete the entire interview). If it does, then we must estimate this indirect effect of the experimental manipulations as well as their direct effects. Part of this complication is avoided because the assignment of the experimental condition occurred only when respondents reached the knowledge section of the interview. Respondents who quit the survey before that point could not have been affected by the monetary incentive as we had not yet revealed that aspect of the survey. Only seven respondents quit after reaching that point in the interview, four in the ‘pay’ condition and three in the ‘no pay’ condition. Hence, selection effects are very unlikely. Therefore, we consider experimental differences between respondents who completed the interview as valid estimates of the true treatment effects.

partisan groups, leaving only pure independents in the middle category. This coding decision did not affect our results except in one case which we discuss below. The averages for all three groups of respondents are higher than \$ 5.7 trillion, the 2001 debt level given in the question, and lower than \$7.4 trillion, the actual debt in October 2004. Although all groups underestimate the level of debt, they correctly perceive an increase over 2001 levels.

[Figure 1 about here]

Consistent with prior research and our expectations, respondents in the control group show clear signs of partisan consistency. Republicans report lower beliefs about the level of debt than Democrats, and independents are roughly in the middle. In the treatment group, this partisan difference disappears entirely. Offered a dollar for the correct answer, Republicans put the debt level at \$7.0 trillion, a significant increase from their average of \$6.5 trillion in the control group. Although Democrats lower their collective estimate slightly, neither they nor pure independents are significantly affected by the monetary incentive.

Our primary measure of support for our first hypothesis, the reduction in the difference between Democrats and Republicans, is statistically significant and quite clear: there is no partisan difference left in the treatment group.

We assess significance by regressing responses to the question on a dummy variable for the experimental treatment, separate dummy variables for Democrats and independents, and the interactions between treatment and partisan dummies. The interaction between the treatment and the dummy for Democrats indicates the change in the partisan difference. We estimate these models by OLS even for multiple-choice questions as long as the response options are equally spaced on the underlying scale. A significant and negative value for this interaction term indicates a reduction, and thus support for our hypothesis. We use one-tailed tests because we are testing a directional hypothesis. With just under 200 Republicans and about 270 Democrats in our sample, we will liberally note marginally significant findings as they might reflect our sample size rather than substantively small effects. (The number of respondents per question differs slightly because

not all respondents finished the knowledge section of the interview. We exclude respondents who skipped the questions without providing any substantive answers. Very few did, and skipping does not differ by experimental condition.) For the debt question, the interaction effect is -.6 with a p value of .08.

The second example is a question at the opposite end in terms of difficulty and partisan salience. Respondents are asked for the percentage of Americans without health insurance. The question does not provide past values as a reference point and, as an open-ended question, does not offer response categories that might suggest a range of answers. Even aside from the possibly greater salience of debt in the first Bush term, the health insurance question is thus far more difficult than our first example. The question text does not mention any partisan actor. For consistency goals to operate for this question, respondents would have to perceive health insurance as a partisan issue and associate wide coverage with efforts by the Republican party (or low coverage with a lack of Republican effort.)

Figure 2 shows the average estimates by Republicans, Democrats, and independents in the two experimental groups. The amount of error stands out in this figure. All three groups vastly overestimate the percentage of uninsured. Republicans' estimates are slightly lower, as consistency pressures would lead us to expect, but even they believe that over a third of all Americans do not have health insurance. In reality, less than one sixth were uninsured in 2003.

[Figure 2 about here]

The monetary incentive did not change the reported beliefs about the percentage of uninsured for any of the groups—and thus had no effect on the partisan difference between Democrats and Republicans. The accuracy goal proved illusive on this question, and the consistency goal appears to have been largely irrelevant. This result suggests that we have to consider Hypothesis 1b seriously: properties of the question and the topic seem to condition the role partisan consistency bias plays in reports of factual beliefs.

Table 1 summarizes the results for all factual questions with underlying continuous scales (administered either in open-ended format or as multiple-choice questions with at least five response options). For three of the six questions, the monetary incentive caused a significant reduction in the partisan difference. For a fourth question, about John Kerry’s proposal to repeal some of the Bush tax cut, the experimental effect is marginally significant when partisan leaners are coded as independents. With leaners as partisans, the partisan difference is still 27 percent smaller in the treatment group than in the control group, but this difference is not significant. The Kerry question is the only question for which the coding of leaners affected the results.

[Table 1 about here]

Two of the six questions—the question about uninsured Americans already discussed and a question asking respondents about the percentage of Americans living in poverty—offer no support for H1. At first glance, the experimental effect for the poverty item appears to be an activation of partisan bias. The difference in reported poverty levels between Republicans and Democrats increased from 3.5 percentage points in the control group to 12.7 points in the treatment group, a highly significant change. However, this change is almost entirely caused by a decrease in the level of poverty reported by Republicans. Although this change is in the direction of partisan consistency—Republicans wanting to believe that unified Republican government would ensure low poverty—it is also in the direction of greater accuracy. For Republicans, the beliefs in the control group imply that our experiment cannot distinguish accuracy and consistency. Hence, the partisan difference on this question increased either because the monetary incentive reinforced partisan consistency or because it encouraged accuracy.

For an overall evaluation of the experimental effect on partisan consistency, Table 1 provides the average reduction in partisan difference in the treatment group. Without the poverty item, the partisan difference declines by 57 percent when respondents are offered a monetary

incentive to answer correctly.³ When partisan leaners are coded as independents for the Kerry item, the decline is 72 percent. Excluding one outlier, a small encouragement to respond accurately decreased partisan distortions of reality by more than half. To the extent that we are justified in treating the poverty question as an outlier, this study supports H1.

Examining Variation in Experimental Effects across Questions

The results for the poverty question emphasize the need to consider Hypothesis 1b—that factual knowledge questions vary in the degree to which they trigger consistency goals, so the effect of the monetary incentive to deemphasize consistency should differ, too. We consider three possible reasons for variation: the emphasis on partisan politics in the question, the relevance of partisanship for the question topic, and the difficulty of the question.

Question emphasis on partisan politics. For partisan differences to occur on factual knowledge questions, respondents must perceive the question topic as relevant to partisan politics. The question itself can establish relevance by mentioning a party or a prominent member of a party. Two of the questions in Table 1, the public debt item and the question about John Kerry’s tax proposal, fall in this category. Both of them offer some support for our first hypothesis. We discuss a third supportive example below.

Relevance of partisanship. When the question itself does not provide direct partisan cues, the application of partisan consistency should depend on the perceived partisan relevance of the question topic. For example, if respondents do not see poverty rates as an indicator of partisan performance, they may not attempt to make their estimates consistent with their partisan predispositions. Admitting high poverty may not generate dissonance for a Republican or reduce dissonance for a Democrat.

In his analysis of NES data, Bartels (2002) finds considerable variation in how strongly retrospective assessments by Democrats and Republicans differed. In contrast to the results for

³ If we treat the poverty items as uninformative and include it with a change in partisan difference of zero, the average decline is 48 percent.

inflation and unemployment we described at the beginning of the paper, partisan differences were entirely absent in evaluations of several other objective conditions. Democrats and Republicans agreed that defense spending and the budget deficit had both increased during Reagan's presidency. In 2000, partisans did not differ in their assessment of whether federal spending on assistance to the poor had improved under Clinton. (But assessments of the same development under Reagan was strongly influenced by party identification.)

To test hypothesis 1a, we analyze responses to two neutral questions for which partisan consistency goals should be irrelevant. The first item asked respondents for the length of a term in the U.S. Senate, the second for the number of Republicans serving in the Senate at the time of the interview. Table 2 shows the average estimates by party identification of the respondent. For both items, the difference between average estimates by Democrats and Republicans is tiny and not statistically distinguishable from zero in both the control group and the treatment group. With or without a monetary incentive, supporters of both parties believe the term of a senator to last, on average, between 4.84 and 5.06 years and between 52 and 54 senators to be members of the Republican party.

[Table 2 about here]

If our experimental treatment does indeed encourage accuracy goals at the expense of consistency goals, then it should only lower the difference between the beliefs reported by Republicans and Democrats when the question is susceptible to partisan consistency pressures. The fact that we observe no experimental effects for neutral knowledge questions thus provides additional support for hypotheses 1 and 1a.

The presence of experimental effects for questions with obvious and emphasized partisan relevance (unemployment, estate tax, Kerry's tax proposal) and the absence of effects for entirely non-partisan topics (length of a Senate term) does not necessarily help us understand the results for the questions on poverty and health insurance. Rather than less relevant to partisan politics, these questions may simply have been too difficult for consistency pressures to matter much.

Question difficulty. The public debt questions offered respondents some help by stating the debt level in 2001. None of the other questions had this feature. Salience and media coverage of economic indicators should also affect the question difficulty. Kerry's tax proposal and the estate tax were campaign topics more so than the percentage of poor Americans. Whereas unemployment numbers are released each month, the Census bureau publishes the poverty rate and the share of Americans without health insurance only once a year. In 2004, these numbers were released in August, so learning effects through media coverage could have worn off by the time of our survey in October.

Our data do not allow us to determine if partisan differences in the control group were small because poverty and health coverage did not receive a lot of media coverage or because the issues were of less partisan relevance. Either way, the upshot for our analysis is that partisan bias was dwarfed by the extent of misinformation on those questions. For both questions, the partisan difference in the control group is relative small (albeit significant), compared to the deviation from the correct answer. On the poverty item, Republicans and Democrats differ by 3.5 points in the control group, but they deviate, on average, by 19.5 points from the correct answer. On the health insurance question, the partisan difference in the control group was 6.5 points, compared to an average deviation from the correct answer of 24 points.

The poverty and health care questions are open-ended questions. Open-ended knowledge questions are generally more difficult than multiple-choice questions. The four multiple-choice questions in Table 1 support our first hypothesis, but the open-ended ones do not. This contrast suggests that possibility that we failed to observe large partisan differences and any support for H1 because the open-ended administration of the questions—not their lack of partisan relevance or media coverage—made them too difficult. However, several multiple-choice questions about

health coverage asked by other surveys in recent years show little or no partisan differences either. Instead, they confirm our finding that beliefs are dramatically inaccurate.⁴

An explanation of why factual beliefs about poverty and health insurance are primarily inaccurate rather than biased by partisan predispositions has to precede an explanation of why our experimental treatment failed to reduce what little partisan bias we found in the control group. Research on partisan bias in reported beliefs about objective conditions will benefit from a more systematic examination of why it occurs for some topics more than others.

Knowledge Questions without Continuous Underlying Scales

The questions we have examined so far have continuous underlying scales. To elicit respondents' beliefs about the correct value on these scales, we have used either open-ended questions or multiple-choice questions with at least five categories (in order to reduce the chance of guessing correctly). Continuous underlying scales make the interpretation of average reported beliefs in different groups relatively straightforward, even for multiple-choice questions. Our survey also included several questions with discrete response options. The absence of an underlying continuous dimension requires a different analytical strategy (because average beliefs in partisan and experimental subgroups have no inherent meaning).

⁴ We found three surveys in the Roper archive that included factual multiple-choice questions about health insurance and for which the data were available. The most recent question was asked in 2001 in a phone survey by the Harvard School of Public Health (N=1206): “How many Americans do you believe are not covered by any form of health insurance, including Medicare or Medicaid today?” The response distributions (across six categories) for Democrats and Republicans are not statistically different. A phone survey by the Kaiser Family Foundation in 1999 (N=1033, “To the best of your knowledge, about how many Americans have no health insurance...about 15 million, about 30 million, about 40 million, or more than 50 million?”) yields the same insignificant partisan difference. Finally, a CBS News/ New York Times poll in 1991 (N=1430) asked about the percentage of uninsured: “What percentage of Americans would you guess do not have any health insurance coverage at all—neither private insurance, nor Medicare nor Medicaid—less than 10 percent, between 10 and 20 percent, between 20 and 30 percent, between 30 and 50 percent, or more than 50 percent?” This question produced a statistically significant partisan difference (and extremely inaccurate beliefs): 26% of Democrats said that more than 50% (!) of Americans had no insurance, compared to 16% of Republicans. Only 8% of Democrats reported that less than 20% were uninsured, compared to 15% of Republicans.

One knowledge question asked respondents about the status of President Bush's proposed Striving Readers Initiative. As Bush's 2005 budget proposal had included \$100 million to fund this program, the answer to this question is inconsistent with the partisan stereotype that Democrats are the party of education, while Republicans would prefer to abolish the Department of Education (although Bush's education policy and rhetoric may have watered down this stereotype). We offered respondents four different response options ranging from the statement that Bush had proposed to cut funding for the program to the statement that the initiative had already led to literacy improvements. Figure 3 provides the full wording of question and response options.

We would expect Democrats in the control group to follow the partisan stereotype and report that Bush cut funding for this education initiative. For Republicans, their consistency goals should pull them in the direction of a more positive assessment of the initiative. In keeping with these expectations, 42 percent of the Republicans in the control group report that the program was already implemented and producing results. Only 5 percent of them respond that Bush had proposed a cut in funding for this program. Democrats are much more likely to report that Bush had either proposed a funding cut (17 percent) or proposed the program without funding it (43 percent). In short, the responses in the control group are heavily affected by partisan consistency goals (to an extent that few supporters of either party answered the question correctly).

[Figure 3 about here]

Figure 3 shows that the partisan difference declines considerably in the treatment condition. The χ^2 statistics for the treatment group is only 14.4, compared to 32.7 in the control group. The monetary incentive leads Republican identifiers in particular to reduce the role of partisan consistency in their answers. The share of Republicans who report that Bush either did not include funding or proposed a funding cut rises from 25 percent in the control group to 40 percent in the treatment group. (There is no equivalent tendency among Democrats in the

treatment group to acknowledge that Bush has not cut this program.) This question illustrates that a reduced impact of the consistency goal does not necessarily improve the accuracy of responses.

A second political item without an underlying continuous scale provides more evidence about the conditions under which partisan bias distorts responses. We asked respondents the following question:

As you may know, a special government commission—called the “9/11 Commission,” investigated the circumstances surrounding the September 11 attacks and recently issued its final report. Which statement most accurately represents the Commission’s conclusions about the relationship between Iraq and al Qaeda?

- They had no connection at all.
- A few al-Qaeda individuals visited Iraq or had contact with Iraqi officials.
- Iraq gave substantial financial support to al-Qaeda, but was not involved in the September 11th attacks.
- Iraq was directly involved in carrying out the September 11th attacks.

At first glance, one might expect clear partisan differences in response to this question. Consistency pressures would seem to lead Republicans to report a connection between al Qaeda and Iraq, and Democrats to deny such a connection forcefully. And yet, the responses in the control group reveal not even a hint of a partisan difference. Twenty-two percent of both Democrats and Republicans select the first response option (no connection at all). Democrats are slightly (and insignificantly) *more* likely to report that Iraq was directly involved (12 percent, compared to 7 percent among Republicans). Why is there no partisan difference on this politically loaded question?

The problem with this question for our purposes is that it leaves at least two ways to achieve partisan consistency. The first path to consistency is familiar: Supporters of President Bush and the war in Iraq bolster consistency by reporting a direct connection between al Qaeda and Iraq to justify the war; opponents achieve consistency by denying the connection. But there is a second way to achieve consistency: Partisans can also question the objectivity of the 9/11 Commission. Republicans who report that the commission did not find evidence for direct Iraqi involvement can remain consistent by also believing that Democrats on this bipartisan commission watered down the conclusions for purely political reasons. Democrats, in turn,

remain true to their party's stereotype when they acknowledge that the commission's report included some mention of Iraqi involvement, but then interpret these findings as politically motivated speculation by the Republican committee members. In short, partisans can achieve consistency by viewing the source of the ostensibly factual information as politically biased. In light of multiple opportunities to attribute bias, the absence of partisan differences is less surprising.

Table 3 provides a summary of the different questions characteristics and the extent of support for Hypothesis 1. The questions in the table are ordered, first, according to *ex ante* expectations of partisan consistency pressures and, second, according to the ratio of the partisan difference to the average deviation from the correct answer. Seven of the ten questions leave at least some room for partisan consistency pressure in one direction to operate. We do not expect partisan pressure to operate systematically for the last three questions in the table, the two neutral questions and the question about the connection between al Qaeda and Iraq.

The ratio in column three provides an *ex post* measure of the extent to which we might see reductions in partisan difference by the monetary incentive. The ratio is calculated for the control group and describes the relative magnitude of the partisan difference between Republicans and Democrats and the deviation from the correct answer of the average partisans' response (that is, the weighted average of mean response for Republicans and Democrats). Higher ratios indicate that the partisan difference in the control group is large relative to the deviation from the correct answer. The ratio thus provides a rough assessment of the relevance of the partisan consistency goal on a particular question. The next-to-last column in Table 3 shows that the monetary incentive is more likely to reduce the partisan difference on questions with high ratios. (The estate tax item is an exception to this pattern. The ratio for this question is low in part because the correct answer is so small.) Moreover, the monetary incentive reduces consistency effects on all three questions that mention partisan actors. In sum, Table 3 suggests that an

incentive to be accurate reduces partisan distortions of reality most for questions with obvious partisan relevance.

[Table 3 about here]

The last column in Table 3 indicates consistency with our first hypothesis. Questions with partisan relevance are consistent with the hypothesis if the experimental treatment reduced the partisan difference. Neutral questions to which consistency goals do not apply are consistent if the experimental treatment fails to affect the partisan difference. Combining these two criteria, seven of our ten questions support our hypothesis.⁵

Experimental Effects on Subsequent Political Evaluations

Without providing respondents with any new information, the experimental treatment changes the salience of information already stored in memory. The monetary incentive encourages respondents to report information that is inconsistent with their partisan predispositions. It makes Republicans more likely to say that a Republican president has allowed the nation's debt to rise considerably and Democrats more likely to report that unemployment levels are fairly low despite unified Republican government. In this last part of our analysis, we examine if more accurate responses to knowledge questions affected subsequent political evaluations.

⁵ It is not clear that we should expect the experimental effects to depend on respondents' political knowledge. Political sophistication implies more information (some of which accurate), but also more integrated belief systems and thus potentially stronger consistency goals. Previous research does not offer strong expectations about the role of political knowledge in partisan bias. Bartels (2002) finds that knowledge made little difference for the extent of partisan bias in evaluations of candidates. Shani (2006) shows some large knowledge effects on partisan bias in perceptions of economic and social conditions, but the informational effects are smaller for questions with a clear factual basis. To examine if political knowledge conditioned the experimental effects in our study, we used a median split on the combined score on three neutral knowledge items to divide the sample by political knowledge. For all questions, the answers of more knowledgeable respondents were more accurate than the answers of their less knowledgeable counterparts. However, difference between Democrats and Republicans did not vary systematically with the level of knowledge. Most importantly, the reduction in partisan difference caused by the monetary incentive did not differ systematically by knowledge level (although experimental effects were somewhat larger for less informed partisans on several items).

In a typical survey interview without rewards for accuracy, respondents may easily slip into a pattern of partisan consistency. By the time they are asked for a political evaluation, respondents have rehearsed the consistent portion of their belief systems so thoroughly that their evaluations will reflect the consistency pressure. According to our second hypothesis, the incentive to answer accurately in the knowledge section of the interview downplays the rehearsal of consistent beliefs and thereby reduces the impact of partisan predispositions on subsequent political evaluations.

We test H2 by examining the relationship between party identification and vote intention for the 2004 presidential election in the two experimental groups. Respondents were asked their party identification in the first part of the study, before any experimental manipulation took place. After completing the knowledge questions in one of the two experimental conditions, all respondents were asked which candidate they intended to vote for in November. To examine if the monetary incentive affected the relationship between party identification and vote intention, we regressed the latter on the former separately for the two experimental conditions.⁶ Results are shown in Table 4.

[Table 4 about here]

Hypothesis 2 is clearly supported. The first set of columns shows the logit models without weights or control variables. The dependent variable is 1 for respondents who intend to vote for Kerry and 0 for respondents with a vote intention for Bush. The coefficient for the 7-point party identification measure declines from 1.15 in the control group to .84 in the treatment group, a statistically significant 27 percent drop. Weights⁷ or control variables do not change this result (see columns 3 to 6). The repeated monetary incentive during the knowledge sequence of

⁶ The marginals on the vote intention question are not statistically distinguishable in the two experimental conditions, but of course this does not preclude a changing impact of party identification.

⁷ The sample was weighted to match the 2004 Current Population Survey on education, age, gender, and race.

the interview affects respondents' subsequent political judgments. Vote intention in the treatment group is decidedly less strongly affected by party identification.

This strong experimental effect is evident for respondents with different levels of political knowledge and involvement. In the more knowledgeable half of the sample (see footnote 5 for measurement), the coefficient for party identification decreases from 1.43 in the control group to 1.02 in the treatment group, a 28 percent decline. For the less knowledgeable respondents, the coefficients are 1.59 and 1.00, respectively, indicating a decline of 37 percent. Among respondents who reported voting in the 2002 congressional elections, the impact of party identification declines by 42 percent (the logit coefficients are 2.03 in the control and 1.18 in the treatment group). Even among knowledgeable respondents and committed voters, an incentive for accuracy lowers the impact of partisanship on reported political evaluations.

For a proper interpretation of the experimental effect on vote intention, it is important to consider the plausibility of an alternative explanation. Perhaps respondents in the treatment condition, offered to make some real money with just a few correct answers, turned to someone else for help or used their web browser to look up information. If this alternative hypothesis were true, we could no longer argue that the experimental treatment did not entail any acquisition of additional information. And the reduced impact of party identification could be attributed to inconsistent new information that respondents only acquired because we encouraged them to do so.

We have strong reason to believe that this alternative hypothesis is not supported. To begin with, respondents had only one minute to answer each knowledge question before they were automatically moved to the next screen. Given the length of our knowledge questions, one minute does not seem to offer sufficient time to look up an answer. Respondents who complete their surveys via a WebTV unit effectively do not have the opportunity to use a web browser. Respondents who access the Knowledge Networks website through a dial-up Internet connection cannot easily take advantage of online resources either. Only respondents with high-speed

connections could thus look up the correct answer in 60 seconds minus the time it takes to read the question. When we exclude respondents with high-speed Internet connections, the experimental effects remain the same. Likewise, our results do not change when we limit the analysis to respondents in single-member households who have much less opportunity to get someone else's help in answering our knowledge questions. In sum, it is highly unlikely that the experimental effects we observe are the result of exposure to new political information.

Conclusion

The results of our experiment indicate that people store political information that is inconsistent with their partisan predispositions. They like to have consistent views and give consistent answers to survey questions, but they can sacrifice some consistency for added accuracy if they are motivated to do so. A small monetary incentive for correct answers succeeded in strengthening the accuracy goal. It reduced partisan differences in perceptions of objective conditions by half or more on many of our knowledge questions.

It follows that typical survey interviews, such as the National Election Studies data analyzed by Bartels (2002) and Shani (2006), overstate partisan bias in perceptions of objective conditions. Either respondents knowingly give incorrect answers or they typically respond by drawing on partisan predispositions subconsciously, but are able to suppress this process when they are so inclined. In both case, respondents' vote decisions may be less strongly influenced by partisanship than their typical survey responses. It may well be the case, after all, that citizens engage in wishful thinking, strategic behavior, or cognitive dissonance reduction during a survey interview, but deliberately confront their full store of information, politically convenient or not, when they make their voting decisions.

We have argued that the experimental effects we observe arise from access to information already in respondents' memory before the beginning of the survey. The survey context made it very difficult to get outside help, and respondents who should have had easier

access to new information (respondents in multi-member households or with high-speed Internet connections) did not respond differently than other respondents. If our results are indeed not the consequence of new information, they raise doubts about several important theories of political reasoning.

Selective exposure cannot explain our experimental effects. If people selectively tuned out inconsistent political information, they would not be able to access it later to earn a few dollars. Our findings add to the voluminous literature that questions the operation of selective exposure in the realm of politics, and of partisan selectivity in particular (e.g., Mutz and Martin 2001; Prior 2007; Sears and Freedman 1967).

The online model of candidate evaluation (Lodge, McGraw, and Stroh 1989; Lodge, Steenbergen, and Braun 1995) cannot account for our results, either. According to the online model, people update their political evaluations, such as their vote intention, only when they encounter new information about the candidates. Respondents in our experiment did not receive any new information, so they should not update their political evaluations. The finding that respondents in the treatment group did change their vote intention as a result of reconsidering information already acquired and stored in memory appears to be more indicative of a memory-based model. Moreover, it is difficult for the online model to explain why the monetary incentive increased the accuracy of responses if people typically forget most information quickly and only store summary evaluations.

The experimental effects on partisan differences—and, in fact, the magnitude of partisan differences in the control group—varied across knowledge questions. Perhaps not surprisingly, partisan bias—and any reduction in it as a result of lower consistency pressures—appears to be lower for questions with less partisan relevance, questions that do not emphasize a partisan connection, and difficult questions. In order to fully understand partisan bias in reports of objective conditions, future research designs need to experimentally vary properties of the question. It would be relatively easy to replicate our results with an added experimental factor

that assigns respondents to either an open-ended or a multiple-choice version of the same knowledge question. Likewise, direct references to the president or other political actors could be experimentally manipulated, as could the presence and nature of a reference point (such as the relevant economic statistic in the past).

Table 1: Experimental Effects on Reported Beliefs about Economic Conditions

	Party ID	N	Mean Response			Reduction in Partisan Diff.	
			No Pay	Pay	Diff.	absolute reduction	as % of diff. in control group
Public debt	Rep.	179	6.48	7.02	.54**	.60 <i>p</i> = .08	39 %
	Indep.	136	6.92	6.88	-.04		
	Dem.	254	7.15	7.03	-.12		
Unemployment rate	Rep.	191	6.55	6.73	.18	.66 <i>p</i> = .04	99 %
	Indep.	143	8.04	7.76	-.28		
	Dem.	269	8.10	7.68	-.42*		
Estate tax	Rep.	192	27.5	20.1	-7.4**	5.6 <i>p</i> = .10	166 %
	Indep.	143	34.6	26.6	-8.0**		
	Dem.	270	24.1	22.3	-1.8		
Kerry tax proposal ^{c)}	Rep.	193	150.0	165.1	15.1**	3.2 n.s.	27 %
	Indep.	142	160.6	147.4	-13.2		
	Dem.	267	162.2	174.0	11.8*	18.3 <i>p</i> = .12	100 %
	Rep. ^{a)}	162	144.8	166.5	21.7**		
	Indep. ^{b)}	227	162.4	160.5	-1.9		
	Dem. ^{a)}	213	163.2	166.5	3.3		
Uninsured	Rep.	188	35.7	34.7	-1.1	[2.9] n.s.	[45 %]
	Indep.	140	42.0	42.0	-.03		
	Dem.	263	42.2	44.1	1.9		
Poverty	Rep.	194	29.8	22.8	-7.0**	[9.2] <i>p</i> < .01	[263 %]
	Indep.	140	34.9	37.6	2.7		
	Dem.	267	33.3	35.5	2.2		
Average						4 %	
Average without poverty item						57 %	
Average without poverty item and excluding leaners on Kerry item						72 %	

** *p* < .05, * *p* < .10 (one-tailed)

Note: Leaners are included with partisans except where otherwise noted. Values in square brackets are increases in partisan difference. Significance is assessed by OLS regression (described in the text) except where otherwise noted. For question wording, see appendix.

^{a)} excluding leaners

^{b)} including leaners

^{c)} Significance test based on ordered probit regression because regression categories are not equally spaced.

Table 2: Experimental Effects on Reported Beliefs about Neutral Facts

	Party ID	Mean Response			
		<i>N</i>	<i>No Pay</i>	<i>Pay</i>	Diff.
Length of Senate Term	Rep.	189	5.06	4.84	-.22
	Indep.	139	4.74	4.58	-.16
	Dem.	271	5.03	4.96	-.07
Republicans in U.S. Senate	Rep.	189	52.2	52.2	.0
	Indep.	140	54.1	48.2	-5.9**
	Dem.	268	52.5	53.7	1.2

** $p < .05$, * $p < .10$ (one-tailed)

Note: Leaners are included with partisans. Significance is assessed by OLS regression (described in the text). For question wording, see appendix.

Table 3: Summary of Results, by Characteristics of the Question

	Partisan consistency pressures?	Mention of partisan actor?	Ratio of Partisan Diff. to Deviation from Correct ^{a)}	Continuous underlying scale?	Open-ended question?	Reduction in Partisan Diff.?	Consistent with H1?
Public debt	yes	yes	1 : 0.8	yes	no	$p = .08$	yes
Unemployment rate	yes	no	1 : 1.4	yes	no	$p = .04$	yes
Kerry tax proposal	yes	yes	1 : 3.5	yes	no	n.s. / $p = .12$ ^{b)}	(yes)
Uninsured	yes	no	1 : 3.7	yes	yes	n.s.	?
Poverty	yes	no	1 : 5.5	yes	yes	n.s.	?
Estate tax	yes	no	1 : 6.9	yes	no	$p = .10$	yes
Striving Readers	yes	yes	n/a	no	no	yes	yes
Republicans in Senate	no	no	1 : 4.7	yes	yes	n.s.	yes
Senate Term	no	no	1 : 32	yes	yes	n.s.	yes
9/11 Commission	multiple	yes	n/a	no	no	no	?

a) in the control group

b) Second value is for leaners are independents.

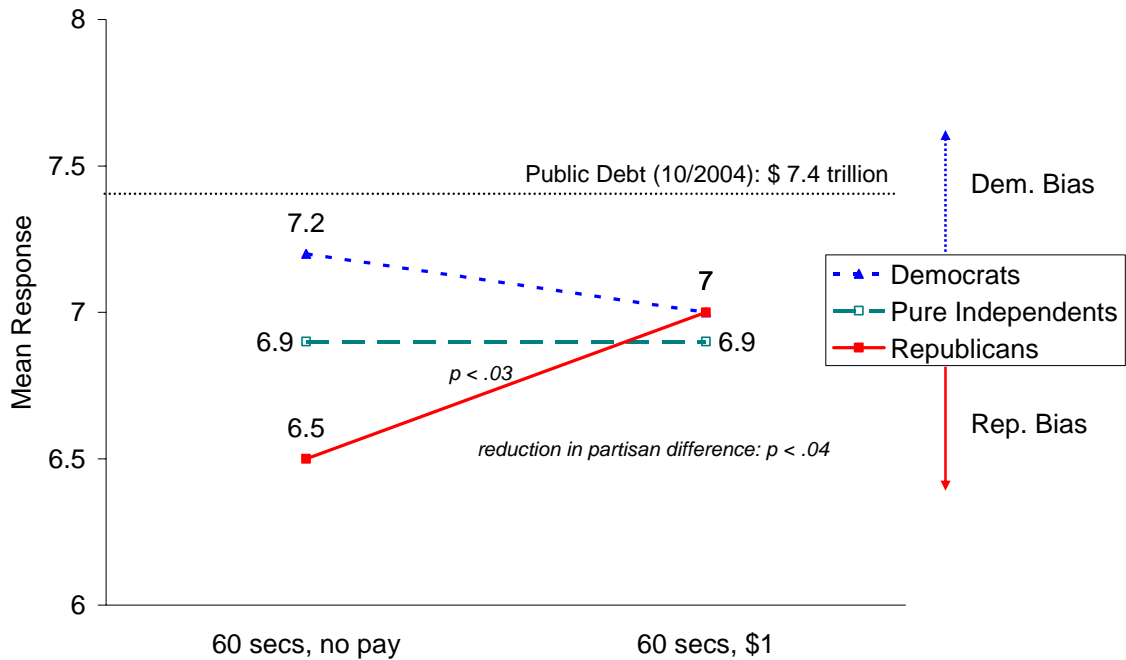
Table 4: Experimental Effects on Impact of Party ID on Vote Intention

<i>Condition:</i>	DV: Vote Intention for Kerry (1) vs. Bush (0)					
	<i>No Pay</i>	<i>Pay</i>	<i>No Pay</i>	<i>Pay</i>	<i>No Pay</i>	<i>Pay</i>
Party ID (7-pt., Dem-hi)	1.15** (.12)	.84** (.09)	1.19** (.13)	.86** (.09)	1.28** (.18)	.90** (.10)
Follows politics "some of the time"					-.51 (.65)	.04 (.52)
Follows politics "most of the time"					.59 (.55)	.33 (.57)
Education					.27 (.23)	.34* (.16)
Female					-.20 (.47)	.04 (.38)
White					-.66 (.53)	-.38 (.47)
Age					-.0045 (.015)	.011 (.011)
Income					.001 (.06)	.0002 (.05)
Full-time employment					-.25 (.56)	-.11 (.39)
Constant	-4.8** (.6)	-3.6** (.4)	-5.0** (.6)	-3.7** (.4)	-5.5** (1.7)	-5.4** (1.0)
Weights?	no	no	yes	yes	yes	yes
Log Likelihood	-85.5	-111.1	-83.8	-108.4	-77.8	-103.6
Pseudo R ²	.51	.37	.52	.39	.55	.42
N	252	256	252	256	252	256

** $p < .01$, * $p < .05$

Cell entries are logit coefficients with robust standards errors in parentheses. Bolded cells indicate that coefficients in the two experimental conditions are statistically different from each other at $p < .05$ (one-tailed).

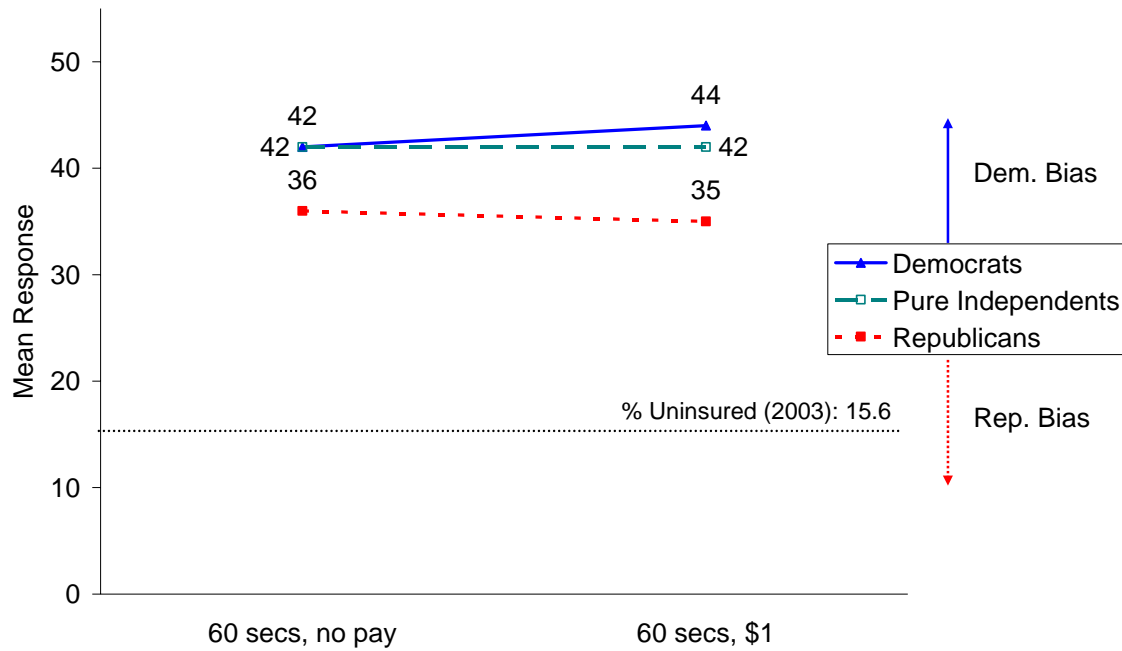
Figure 1: Reported Beliefs about the Level of Public Debt



Question wording: “The outstanding public debt of the United States is the total amount of money owed by the federal government. Every year the government runs a deficit, the size of the public debt grows. Every year the government runs a surplus, the size of the public debt shrinks. In January of 2001, when President Bush took office, the outstanding public debt of the United States was approximately 5.7 trillion dollars. Which of the following responses is closest to the outstanding public debt today?”

Response options: “Less than 3.5 trillion dollars,” “4.5 trillion dollars,” “5.5 trillion dollars,” “6.5 trillion dollars,” “7.5 trillion dollars,” “8.5 trillion dollars,” “More than 9.5 trillion dollars”

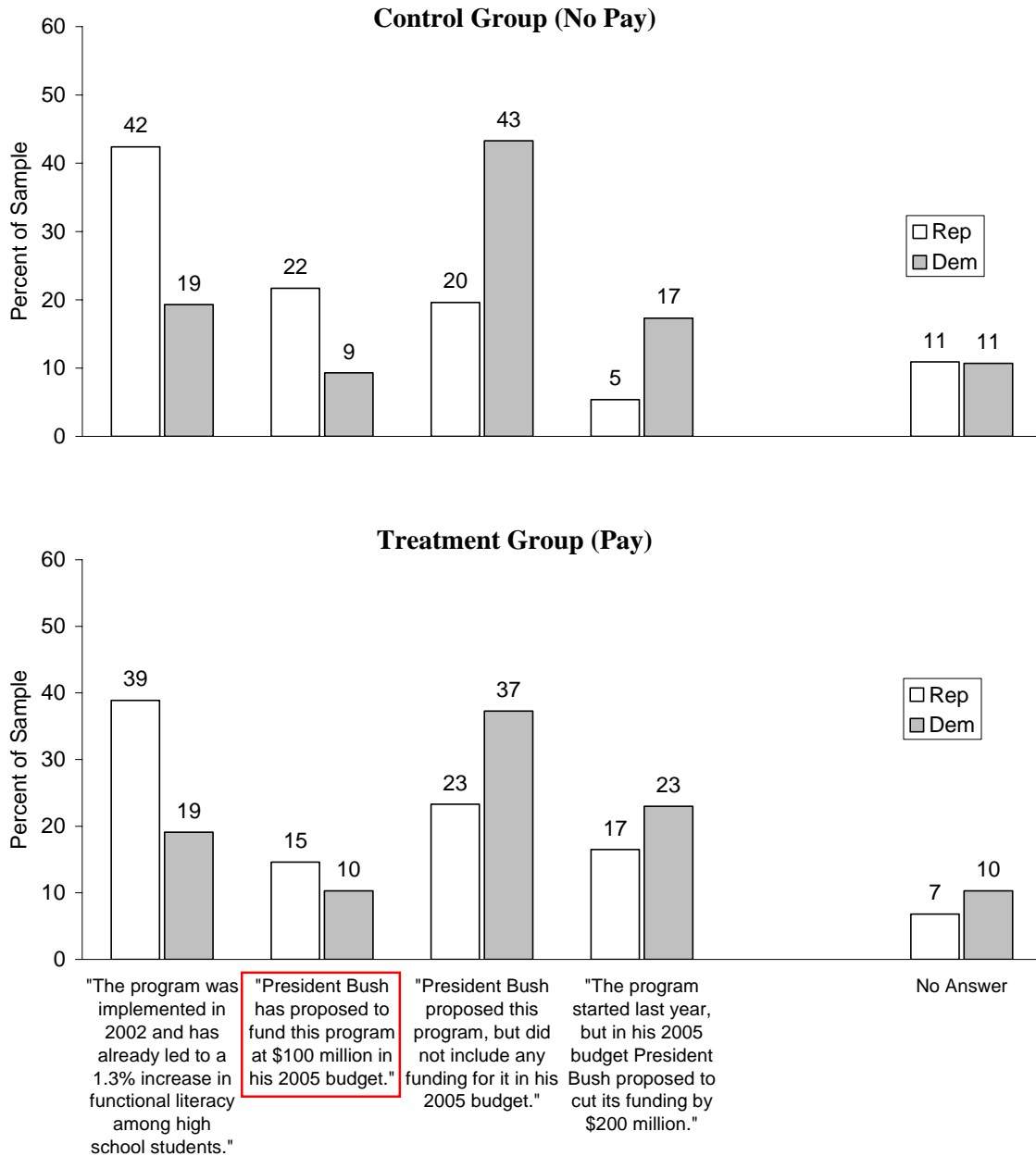
Figure 2: Reported Beliefs about the Percentage of Uninsured Americans



Question wording: “In August 2004, the United States Census Bureau reported an estimate of the number of Americans without health insurance. The Census Bureau classified people as uninsured if they were not covered by any type of health insurance at any time in 2003. By this definition, what percentage of Americans did not have health insurance in 2003?”

Response options: open-ended question

Figure 3: Reported Beliefs about the “Striving Readers Initiative”



Question wording: “President Bush proposed a “Striving Readers initiative” to help high school students who are not reading as well as they should be for their age. What is the status of the Striving Readers program?”

Note: Graphs show the distribution of responses by Democrats and Republicans in the two experimental conditions. The χ^2 statistics for the control and treatment group are 32.7 and 14.4, respectively (each with 4 degrees of freedom).

Appendix Table 1: Knowledge Questions

Question ID	Question wording	Response options (Correct response in bold)
Unemployment rate	The U.S. Bureau of Labor Statistics counts a person as unemployed if they are not employed at any job and are looking for work. By this definition, what percentage of Americans was unemployed in August of 2004?	<ul style="list-style-type: none"> • around 11 percent • around 9 percent • around 7 percent • around 5 percent • around 3 percent
Estate tax	There is a federal estate tax – that is, a tax on the money people leave to others when they die. What percentage of Americans leaves enough money to others for the federal estate tax to kick in?	<ul style="list-style-type: none"> • About 95 of all Americans • About 70 of all Americans • About 50 of all Americans • About 25 of all Americans • Less than 5 of all Americans
Uninsured Americans	In August 2004, the United States Census Bureau reported an estimate of the number of Americans without health insurance. The Census Bureau classified people as uninsured if they were not covered by any type of health insurance at any time in 2003. By this definition, what percentage of Americans did not have health insurance in 2003?	open-ended, correct: 15.6
Federal Debt	The outstanding public debt of the United States is the total amount of money owed by the federal government. Every year the government runs a deficit, the size of the public debt grows. Every year the government runs a surplus, the size of the public debt shrinks. In January of 2001, when President Bush took office, the outstanding public debt of the United States was approximately 5.7 trillion dollars. Which of the following responses is closest to the outstanding public debt today?	<ul style="list-style-type: none"> • Less than 3.5 trillion dollars • 4.5 trillion dollars • 5.5 trillion dollars • 6.5 trillion dollars • 7.5 trillion dollars • 8.5 trillion dollars • More than 9.5 trillion dollars

Appendix Table 1 (cont.): Knowledge Questions

Kerry tax proposal	John Kerry says that he would eliminate the Bush tax cuts on families making how much money?	<ul style="list-style-type: none"> • Over 50,000 a year • Over 100,000 a year • Over 150,000 a year • Over 200,000 a year • Over 500,000 a year
Poverty rate	In August 2004, the Census Bureau reported how many Americans live in poverty. The poverty threshold depends on the size of the household. For example, a person under age 65 is considered to live in poverty if his or her 2003 income was below \$9,573 and a family of four is considered to live in poverty if its 2003 income was below \$18,810. By this definition, what percentage of Americans lived in poverty in 2003?	open-ended, correct 12.5
Striving Readers	President Bush proposed a “Striving Readers initiative” to help high school students who are not reading as well as they should be for their age. What is the status of the Striving Readers program?	<ul style="list-style-type: none"> • The program was implemented in 2002 and has already led to a 1.3 increase in functional literacy among high school students. • President Bush has proposed to fund this program at \$100 million in his 2005 budget. • President Bush proposed this program, but did not include any funding for it in his 2005 budget. • The program started last year, but in his 2005 budget President Bush proposed to cut its funding by \$200 million.
Al-Qaeda connection	As you may know, a special government commission—called the “9/11 Commission,” investigated the circumstances surrounding the September 11 attacks and recently issued its final report. Which statement most accurately represents the Commission’s conclusions about the relationship between Iraq and al Qaeda?	<ul style="list-style-type: none"> • They had no connection at all • A few al-Qaeda individuals visited Iraq or had contact with Iraqi officials • Iraq gave substantial financial support to al-Qaeda, but was not involved in the September 11th attacks • Iraq was directly involved in carrying out the September 11th attacks

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