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Partisan Bias in Surveys

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Abstract

If citizens are to hold politicians accountable for their performance, they probably must have some sense of the relevant facts, such as whether the economy is growing. In surveys, Democrats and Republicans often claim to hold different beliefs about these facts, which raises normative concerns. However, it is not clear that their divergent survey responses reflect actual divergence of beliefs. In this review, we conclude that partisan divergence in survey responses is often not due to sincere, considered differences of belief that fall along party lines—but determining what it *is* due to is difficult. We review the evidence for possible explanations, especially insincere responding and congenial inference. Research in this area is still nascent, and much more will be required before we can speak with precision about the causes of partisan divergence in responses to factual questions.

INTRODUCTION

Ignorance of politics among ordinary citizens has long been one of the best-established findings of the social sciences. The study of misinformation—not a lack of awareness of the truth, but confident belief in falsehoods—was slower to develop. Findings about partisan differences in beliefs arrived even more recently. But when they came, they came in a torrent.

Bartels (2002, pp. 133–38) was among the first to write about the topic in a systematic way. He noted that Republican and Democratic respondents in the American National Election Studies (ANES) reported different beliefs on a variety of arguably objective matters, such as whether unemployment and inflation had improved in the previous eight years. Shortly afterward, stark partisan differences emerged over whether Iraq had weapons of mass destruction (Kull et al. 2004, pp. 3–5; Jacobson 2010), even though the United States had announced that it did not (Duelfer 2005). Furthermore, the divergence on this topic may have increased, instead of moving toward consensus, in the years after the invasion (Jacobson 2010). Larger partisan differences also emerged over the positions that presidential candidates had taken, even when the candidates' positions had been strong and consistent (Kull et al. 2004, pp. 11–12). More sensationally, many more Democrats than Republicans claimed to believe that George W. Bush “allowed the 9/11 attacks to take place because he wanted the United States to go to war in the Middle East” (Public Policy Polling 2009, p. 9). And Republicans seemed much more likely to believe that Barack Obama was a Muslim (57% of Republicans versus 12% of Democrats), that he “wants to turn over the sovereignty of the United States to a one-world government” (51% to 12%), and that he “may be the Antichrist” (24% to 6%; all figures from Harris Interactive 2010). Partisan differences in responses to factual questions are now common (Flynn 2016, p. 14; Ramsay et al. 2010, pp. 17–18; Shani 2006; see also Shapiro & Bloch-Elkon 2008).

With few exceptions, partisan differences in survey responses have been taken to reflect straightforward differences in partisans' beliefs. For example, in an article on both partisan and educational-attainment differences in beliefs, the *New York Times* reports that “the more education Republicans have, the less they tend to believe in climate change” (Quealy 2017). A major polling firm tells readers that answers to its questions about factual beliefs reflect “the actual percentages of adults who believe these things are true” (Harris Interactive 2010). Claims like these suggest a simple interpretation of survey results among journalists and pollsters: People have decided beliefs, and surveys elicit those beliefs.

To some extent, scholars have interpreted partisan gaps differently. In political science, for example, there is widespread recognition that many survey responses do not reflect well-formed prior beliefs; they are instead constructed on the spot, which calls into question conventional notions of “belief” in the matters about which respondents are asked (Tourangeau et al. 2000, Zaller 1992, Zaller & Feldman 1992). But even so, scholars too have often taken partisan differences in surveys as more or less direct evidence of differences in beliefs. For example, writing just before the 2004 presidential election, Kull et al. (2004, p. 13) maintain that survey results show that “Bush supporters and Kerry supporters have profoundly different perceptions of reality.” Jerit & Barabas (2012, p. 673) find “strong support” for the claim that partisans tend to “*see the world* in a manner that is consistent with their political views” (emphasis added). And Bartels (2002, pp. 135–36) argues that “[a]bsent some complicated just-so story...these large differences can only be interpreted as evidence of substantial partisan biases in perceptions.” All of these arguments suggest that partisan differences in survey responding reflect real differences in partisans' beliefs. They allow that partisans' thinking may be biased, but they do not seem to allow that partisans' answers are anything other than a straightforward reflection of their thinking.

Arguments like these may be correct, but they do not help us to distinguish between the extent to which survey responses reflect predetermined beliefs and the extent to which they are constructed on the spot. They also do not allow the possibility that partisans' responses reflect *cheerleading*: When asked a factual question, respondents may believe one answer but give a different answer to support their party. New research has begun to tackle these possibilities (Bullock et al. 2015, Prior et al. 2015, Schaffner & Luks 2018), but much more work remains to be done.

No previous article has been devoted to sifting the evidence on these points and attempting to adjudicate disputes, to determine where the evidence supports one argument and where it supports another. That is the task that we take up here. Of course, scholars have studied partisan differences in attitudes and evaluations for decades (e.g., Campbell et al. 1960, Palmer & Duch 2001, Sears & Lau 1983, Wilcox & Wlezien 1993). But our focus is on partisan differences in responses to factual questions, which are more tractable objects of study because they have correct answers. To what extent do partisan differences in responses to factual questions reflect sincere differences in beliefs between members of different parties? And to the extent that partisan differences in responses reflect something other than differences in beliefs, what is it, exactly, that they reflect?

The answers are important for two reasons. First, they speak to the venerable claim that partisanship is a "perceptual screen" such that "the stronger the party bond, the more exaggerated the process of selection and perceptual distortion will be" (Campbell et al. 1960, p. 133). The stronger the evidence for cheerleading, the greater the challenge to the view of partisanship as a perceptual screen. Second, the answers speak to arguments for democratic government. Some of the strongest defenses of democracy are predicated on theories of retrospective voting: Even if voters know little, these theories maintain, they can at least administer rough justice by rewarding or punishing incumbents for things that have happened during their terms (Fiorina 1981, p. 4). But inasmuch as partisan differences in responses to factual questions reflect inaccurate beliefs among at least some partisans, they call into question partisans' abilities to vote retrospectively. For example, antitax voters who believe that an incumbent has raised taxes when he has instead lowered them will be unable to reward him for his stance on taxes (Cooper 2010). Such errors also call into question a common assumption about legislators' duties: Should legislators heed their constituents' views if those views are rooted in false beliefs?

We begin this review with a theoretical discussion of survey responses, focusing on factors that may give rise to partisan differences in those responses. We then turn a critical eye to the evidence, focusing on the distinction between cheerleading and other forms of motivated responding. We conclude by discussing the implications of the evidence and points on which further research is especially needed.

THEORY

To structure thinking about partisan differences in responses to factual questions, we first present a general theory of survey response. We make no large innovation here, and we borrow from theoretical work on consideration sampling (Tourangeau et al. 2000, Zaller 1992, Zaller & Feldman 1992) and motivated reasoning (Kunda 1990).

Definitions

A *belief* is a probability distribution over a set of mutually exclusive and exhaustive statements about a state of the world. For example, if Donald Trump can be only good or bad, your belief about Trump's goodness is fully characterized by the subjective probability that Trump is good. Thus, if 50% of the mass of your distribution falls on the statement "Trump is good," we know that

the remaining 50% falls on the statement “Trump is bad,” and the probability distribution—your belief about Trump—is fully characterized.

If most of the mass of your belief probability distribution (BPD) falls on a particular statement, we say that you *hold a belief* that the statement is true or *believe* that it is true. To continue with the example given above, if 50% of the mass of your BPD is centered on “Trump is good” and 50% on “Trump is bad,” we cannot say that you hold any particular belief about Trump’s goodness or badness. But if 51% of the mass is centered on “Trump is good,” we may say that you believe Trump is good.

The more that the mass of your BPD falls on a particular statement, the more confident you are in your belief that the statement is true. If 90% of your BPD falls on “Trump is good,” but only 50% of our BPD falls on “Trump is good,” then you are more confident of Trump’s goodness than we are. *Knowledge* is confident belief in true statements.

A *factual question* is a question about a statement or set of statements whose truth can be learned, and a *factual belief* is a belief about such a statement. Statements like these are often binary: Either unemployment was higher in January 2017 than in January 2009, or it was not. But the statements need not be binary. For example, we can imagine a large or even infinite set of statements about the unemployment rate in January 2017: It was 4.0%, it was 4.1%, it was 4.2%, and so on. One may have a belief over this set of statements, too.

A factual belief need not be accurate. One may believe, for example, that the unemployment rate in January 2017 was 20%. It was actually 4.8% (see Bureau of Labor Statistics 2018).

Cheerleading, Congenial Inference, and Motivated Responding

When people are asked factual questions, they think of answers. As they do, they may be spurred by an accuracy motive and by directional motives (Kunda 1990). The accuracy motive is the desire to give a correct response. Directional motives are desires to offer particular answers regardless of whether those answers are correct. Our interest is in partisan directional motives: motives to support one’s own political party by answering in a certain way.

An example will illustrate the ways in which accuracy and directional motives can coincide and conflict. When a Democrat is asked whether the unemployment rate declined from the beginning to the end of the Obama administration, the motives should coincide. The unemployment rate did decline, and so the accuracy motive should spur the Democrat to give the correct answer. The correct answer also casts Obama in a positive light, and so, to the extent that the Democrat is moved by partisan motives, they should also spur him to give the correct answer. But when a knowledgeable Republican is asked the same question, the two motives are unlikely to coincide. The accuracy motive will incline our Republican, too, to give the correct answer, but the directional motive may point him in the opposite direction. That is, it may incline him to say that the unemployment rate rose during Obama’s administration, even though he knows it fell.

Critically, accuracy motives may operate in more than one way. Either of our respondents may know the correct answer before being asked the question. In this case, the accuracy motive simply inclines them to say what they already know. Alternatively, the respondents may not know the answer. In this case, the accuracy motive may spur them to think harder about it. If we imagine that one’s response is a function of the considerations that one can call to mind when trying to think of an answer (Tourangeau et al. 2000, Zaller 1992), “thinking harder” may take the form of trying harder to recall relevant considerations.

Directional motives, too, may operate in more than one way. If our Republican respondent is confident that unemployment rate declined under Obama, a directional motive may simply incline him to give a response that he believes to be false. This is insincere responding, or

cheerleading. On the other hand, our Republican respondent may not be confident of the correct answer. In this case, he may canvas his memory for considerations related to the unemployment rate—but do so in a way that makes him especially likely to retrieve considerations that cast Obama in a negative light. This is biased consideration sampling.² Alternatively, the Republican may rely on a pro-party heuristic (e.g., Khanna & Sood 2018) to determine his answer: for example, a heuristic that says “give the response that makes Obama look bad unless faced with incontrovertible contrary evidence.” Following Prior et al. (2015, especially p. 494), we use the term *congenial inference* to refer to either biased consideration sampling or the use of pro-party heuristics.¹

Cheerleading and congenial inference are both forms of motivated responding (Khanna & Sood 2018, especially pp. 83–86). Many have overlooked this simple, critical point: There are multiple types of motivated responding. And while estimating the extent of motivated responding to a given question is relatively easy, distinguishing between types of motivated responding is hard. We shall return to this problem.

As we discuss below, people often lack confidence in their understanding of politics. This lack of confidence has important and underappreciated implications for interpretation of partisan differences in survey responses. All else equal, partisans who are confident of the correct answer to a factual question may be less likely to give an incorrect answer that favors their party. Accuracy motives may incline them to answer correctly. But among those who are not confident of the correct answer, partisan responding seems more likely. The accuracy motive does not impel these people to answer correctly because they have little notion of what the correct answer is. In contrast, directional motives are still likely to affect these partisans, who may derive some benefit from giving an answer that favors their party. Thus, partisan differences in survey responses will sometimes be due neither to cheerleading nor to sincere differences in beliefs, but to the use of pro-party responses by those who have little notion about the correct answers. To date, the effects of variation in confidence have been insufficiently appreciated (though see Ortoleva & Snowberg 2015).

Given low levels of knowledge and confidence, it is even possible that accuracy motivations lead to partisan differences. This possibility arises when people have so little confidence in the correct answers to questions that a party default provides their best guess. When answering a question about how inflation changed under President Reagan, for instance, the only consideration that may come to mind is that Reagan was a Republican. Republican respondents may then report that inflation fell under Reagan because they think Republican officeholders generally perform well, while Democrats may report that inflation rose because they believe the opposite about Republican officeholders. A likely mechanism is “attribute substitution,” and it, rather than directional motives, may underpin many cognitive biases (Kahneman 2003; 2011, ch. 8). Since accuracy motives can therefore also produce partisan differences, distinguishing cheerleading from its alternatives is an especially difficult task.

The question before us is why, on average, Democrats and Republicans give different answers to many factual questions about politics. They may hold different beliefs in a straightforward

¹The term “expressive belief” has no place in our framework. It has been used in other works about partisan bias in surveys, but it has never been defined, and it is deeply ambiguous. The ambiguity arises because it is obvious that statements can be expressive, less obvious that beliefs can be (though see, for example, Abelson & Prentice 1989). When someone speaks of “expressive beliefs,” then, it is hard to say whether he is simply confusing “statements” and “beliefs” or is instead trying to say something subtle about beliefs.

²Cheerleading is compatible with consideration sampling. For example, we may never have thought about whether Barack Obama is the Antichrist. When asked whether he is, we may quickly infer that he is not, drawing on relevant considerations, stored in our memories, about both Obama and the Antichrist. Partisan motives may then lead us to respond that Obama is the Antichrist, even though we are now confident that he is not.

sense, or they may be cheerleading, or they may be relying on congenial inference, or they may be relying on an accuracy-based party heuristic. Each of these answers has distinct implications for theories of accountability. We turn now to the evidence, to see how well it supports each answer.

EVIDENCE: PARTISAN BIAS IN EXPRESSIONS OF FACTUAL BELIEFS

Partisan differences in responses to political survey questions are often large. Consider, for example, differences in the extent to which Democrats and Republicans approve of the performance of the incumbent president, or the consistent partisan differences in support for the Iraq War mentioned in the Introduction. In these cases and many others, there is a great difference between the answers given by Republicans and the answers given by Democrats.

One might imagine that large partisan differences are also common in responses to factual questions about politics. But despite burgeoning interest in partisan differences of this sort, there have been few attempts to generalize about the size of these differences. That said, three recent papers shed light on the question.

Two of the papers are similar to each other. Prior et al. (2015) and Bullock et al. (2015) both describe experiments in which subjects are asked numerous factual questions about politics. The questions are partisan in the sense that the correct answer may be taken to favor either the Democratic or the Republican Party. Prior et al. focus on economic perceptions (for example, the percentage of Americans living in poverty), while Bullock et al. ask about both economic perceptions and the American wars in Iraq and Afghanistan. In all of the experiments in these articles, the control conditions are akin to ordinary survey conditions. And in all of the experiments, the authors find that the partisan gaps in responses are generally modest. Averaging across all ten of their questions about the economy, Prior et al. (2015) find that the difference between the “congenial error rate” and the “uncongenial error rate” is 12%. That is, partisans were 12% more likely to offer incorrect answers that favored their party than to offer incorrect answers that favored the other party. Similarly, Bullock et al. (2015) find average partisan differences of 12% and 15% in their two studies. In all of these cases, the maximum possible difference was 100%. In other words, under ordinary survey conditions, partisan differences in responses to questions about factual beliefs are rather modest.

The finding of modest partisan differences is corroborated by the most systematic review of partisan differences in retrospective items. Sood (2015) analyzes all retrospective factual questions in the ANES that were conducted in presidential years between 1988 and 2008, including questions about the economy, budget priorities, inequality, and crime. He finds that partisan differences in answers to these questions are highly variable—but that, on average, they are small. The mean gap in answers to these questions is 15%; the median gap is 12% (Sood 2015, p. 10). Again, we see that despite the partisan differences in responses to outlandish or sensational questions, the differences in responses to the critical everyday questions of American politics are often smaller than many imagine.³

³Differences between strong Democrats and strong Republicans may be larger than average differences between all Democrats and all Republicans. For example, Bartels (2002, pp. 135–36) finds large differences between strong Republicans and strong Democrats in response to retrospective questions about inflation and unemployment in the 1988 ANES. However, he finds smaller differences in response to eight other retrospective questions, and 1988 was a high-water mark for partisan differences in response to the ANES questions about unemployment and inflation (Sood 2015, p. 10). Partisan differences in responses to inflation questions nearly vanished in the ensuing 20 years, and during that same period, partisan differences in responses to unemployment questions were higher only in 2004.

Still, partisan differences in responses to factual questions do exist. And in some cases—particularly those that attract a lot of elite attention, like the existence of weapons of mass destruction in Iraq—the differences are sizable (see Jacobson 2010). Partisan gaps may also be large for factual items that are immediately preceded by questions about vote choice or other highly politicized matters (Sears & Lau 1983, Wilcox & Wlezien 1993). What should we make of these partisan gaps in answers to factual questions? To what extent do they reflect motivated responding?

The Extent of Motivated Responding

Incentives for accurate responding are the chief tools that scholars have used to investigate partisan bias in factual beliefs. Typically, some subjects in an experiment are assigned to a condition in which they are offered a payment or an entry into a lottery for each correct answer. This treatment is intended to heighten subjects' accuracy motives.

The two experiments in Prior et al. (2015) are paradigmatic. In each experiment, all subjects were asked five questions about economic perceptions: questions about the price of gas, the size of the federal debt, the percentage of Americans living in poverty, and so on. Control-group subjects were asked these questions under ordinary survey conditions. Treatment-group subjects were asked the questions under similar conditions, but they were randomly assigned to receive \$1 or \$2 for each correct answer. Responses to each question were coded as correct, as a congenial error (favoring one's own party), or as an uncongenial error (favoring the other party). For example, all subjects were asked to estimate the unemployment rate in 2004, when the Republican Party controlled both the White House and Congress. Republican subjects who underestimated the true unemployment rate were coded as making a congenial error. Democrats who underestimated the true unemployment rate were coded as making an uncongenial error.⁴

The authors' main outcome of interest is the difference between congenial and uncongenial error rates. They call this difference "partisan bias." Averaging across both studies, they find that their monetary incentives reduce partisan bias from 12% to 6% (Prior et al. 2015). In other words, the small monetary incentives reduced partisan bias by about 50%.

Bullock et al. (2015) report similar results from two similar experiments. In their first study, treated subjects received, for each correct answer, an entry into a lottery. The lottery was complex, but under reasonable assumptions, the expected payoff for each correct answer was approximately 17 cents (Bullock et al. 2015, p. 539n13). In the second study, subjects simply received a fixed amount for each correct answer; the amount varied randomly across subjects from 10 cents to \$1.

Bullock et al. scaled responses to each question so that they ranged from 0 to 1. Their main outcome of interest is the difference, on this 0-to-1 scale, between the average Republican answer and the average Democratic answer. They find that the lottery treatment reduced this partisan gap from 12% to 5%, or by 56% (Bullock et al. 2015, p. 537). The direct-payment treatment reduced the partisan gap from 15% to 6%, or by 60% (Bullock et al. 2015, p. 553). In general, larger payments led to greater reductions, and the maximum payment of \$1 led to the maximum reduction: When subjects were offered \$1 per correct answer, the partisan gap was reduced by 80%.

Five other studies use similar designs with subtler treatments. Both Bullock et al. (2015) and Prior et al. (2015, p. 497) assign some subjects to receive not a payment for correct answers but an admonition to answer correctly. Berinsky (2018) does the same in two studies. And Robbett & Matthews (2018) ask groups of subjects to vote on the correct answers, and they vary the extent

⁴This discussion elides some details in the authors' coding procedure. In short, slight underestimates or overestimates of the unemployment rate were not coded as errors of any kind. See Prior et al. (2015, pp. 499–500).

to which subjects are pivotal. In some conditions, the subject's answer will determine the group's collective decision; in others, it will not. If the group decision is correct, the entire group will be rewarded.

All of these studies attempt to heighten subjects' desires to respond accurately. But the results from the subtler treatments in these studies are mixed. Prior et al. (2015, p. 503) find that the effects of their admonition are as great as the effects of offering \$2 for each correct answer. In contrast, Bullock et al. (2015, p. 559n28) and Berinsky (2018) find no clear effect of brief admonitions to answer accurately. Finally, Robbett & Matthews (2018) find that their pivotality manipulation is as effective as the monetary incentives in other authors' studies: It reduces the gap in average responses between Democrats and Republicans by 60% of the range of the scale.

Collectively, these nine experiments suggest that modest treatments can reduce the partisan gap a lot. Six of the nine studies find that partisan differences in responses fall by at least 50% when subjects receive small inducements to answer accurately. If we grant that the treatments change responses exclusively by deterring motivated responding—an idea to which we shall return—these results suggest that motivated responding accounts for a large portion of partisan differences in responses to factual questions. Given that only the subtlest treatments fail to reduce partisan gaps by 50% or more, we might conclude that 50% is a lower bound on the extent to which partisan differences in responses to questions like these are driven by motivated responding. The bound is a lower bound because, if the treatments had been stronger (for example, if the incentives had been larger), the effects might have been greater still. Indeed, in one study discussed below, Bullock et al. (2015, p. 555) find that a combination of treatments was sufficient to eliminate all partisan responding. But before we can repose confidence in these conclusions, we need to consider several objections to studies of this sort.

Some of the objections are easy to dismiss. One might worry, for example, that subjects in online experiments will simply look up the answers to questions via an online reference source. But careful online studies like those mentioned above are designed to prevent just such a problem, partly through the use of strict time limits for each question. One might also worry that the factual questions in these studies are ambiguous, leading to confusion about which answers count as correct. But ambiguity of this sort, if it exists, should weaken the effects of incentives. In effect, it would suggest that the already-large estimates of the effects of incentives are smaller than those that we would find with unambiguous questions (Bullock et al. 2015, p. 529n10). Finally, one might worry that the results obtain with only a particular kind of sample—for example, a Mechanical Turk sample. But the results have been found across samples from multiple firms, not just with samples from a single provider.

Objections related to experimenter-induced demand effects are more serious but still rather limited. The natural concern in this vein is that subjects who are paid for correct answers will give liberal answers, even when they don't believe those answers, because they intuit that the scholars running the study want liberal answers. This would be a grave concern if all of the questions in a study had correct answers that favored, say, the Democratic Party. But it is a lesser concern when studies contain a mix of questions, some favoring the Democratic Party and others favoring the Republican Party. All of the studies discussed here do contain just such a mix of questions.⁵

⁵ Berinsky (2018) proposes an experimental design to identify the proportion of people in a sample who will "say anything to get the money." The intuition is that if we pay some subjects to express agreement with a statement (for example, "Barack Obama is a Muslim") and others to express disagreement with it, we can then use their responses to estimate the proportion of all subjects who will say anything to get the money. This design may work well with samples that are evenly divided between those who believe the statement and those who do not. But as the proportions of believers and nonbelievers become more unequal, the assumptions invoked by the design become more tenuous. See Bullock & Lenz (2018).

The mix of questions is important because it permits us to probe the extent to which incentives incline subjects to give liberal answers that they do not believe. If incentives have this effect, the effect is likely to be more pronounced when subjects answer questions that have liberal answers (i.e., questions whose correct answers favor the Democratic Party). In these cases, incentives may attract subjects (especially Republicans) who do not believe the liberal answer but offer it simply to win money. By contrast, when questions have conservative answers, no group of subjects is likely to offer a conservative answer just to win money. We may therefore expect that incentives do more to change people's responses when the correct answers are liberal. But that is not at all what we find. Among those questions in Bullock et al. (2015) whose correct answers favor the Republican or the Democratic Party, the effects of incentives are greater when the correct answers favor the Republican Party.⁶ Khanna & Sood (2018) report a similar finding.

One objection to the studies described above is more potent than any of those just named. It is that they cannot distinguish between types of motivated responding. In particular, they can tell us little about whether the motivated responding that we observe is due to cheerleading or to congenial inference. In a sense, the problem is that these studies are too powerful: They seem capable of reducing partisan differences that come about because of either factor. Thus, when we observe reduced partisan differences in these studies, we are unable to say whether the reduction is due to one factor or the other. In other words, these studies may reduce partisan differences in responses to survey questions, but they cannot tell us whether the partisan gap has narrowed because the studies reduced insincere responding among partisans or because they caused partisans to think differently (and perhaps more carefully) about the questions. This is not a flaw in the studies just described, but it is a limitation. The point is acknowledged by one set of authors (Bullock et al. 2015, pp. 527, 559), but there is little that any authors can do about it with the experimental designs described above.

A related objection is that these studies cannot distinguish between motivated responding and use of an accuracy-based heuristic. That is, when incentives reduce partisan gaps in answers to factual questions, they may do so by reducing motivated responding of one sort or another—or by reducing use of an accuracy-motivated heuristic that leads people astray. In the latter case, for example, incentives may lead partisans to think harder about the heuristic which suggests that “members of my party are good managers of the economy,” realize that it is not always accurate, and so rely less on it. To distinguish between these accounts of partisan differences, we must turn to other types of studies.

Cheerleading Versus Congenial Inference

The studies described above cannot distinguish cheerleading from its alternatives. This is so because they are based on treatments—for example, payments to subjects—that may induce both sincere responding and more careful and even-handed thinking about a question. By definition, to the extent that they induce sincere responding, they limit cheerleading. But to the extent that they induce more careful and even-handed thinking about a question, they may limit congenial

⁶Many questions in Bullock et al. (2015), and all of those in Prior et al. (2015), do not unambiguously favor either party. For example, average Democratic and Republican answers differ in response to “What percentage of U.S. soldiers killed in Iraq since the invasion in 2003 are black?”, but the correct answer does not obviously favor either party. Of the seven questions posed by Bullock et al. (2015, especially pp. 573–74) that do unambiguously favor one party, incentives for correct answers reduced the partisan gap by twelve percentage points when the correct answers favored the Republican Party and by seven percentage points when they favored the Democratic Party.

inference. Distinguishing between these possibilities is important: Cheerleading may raise little concern about voter competence, but congenial inference may raise serious concerns.

Not all studies are limited in the same way. We turn now to four studies that do seem able to distinguish cheerleading from congenial inference, at least to some extent. We present them in rough order of their ability to make this distinction, starting with the less able and proceeding to the more able.

Main evidence. Consider first the “modified balanced incentive design” of Berinsky (2018), in which some subjects are offered an incentive to agree with a rumor while others are offered an incentive to disagree with it. In this case, the incentive is time: Subjects are given an opportunity to finish the survey five minutes earlier than expected if they endorse or refuse to endorse a rumor, depending on the experimental condition. Berinsky applies the design to one question about Obama’s religion and another about the terrorist attacks of September 11, 2001. He finds no evidence of motivated responding in response to the first question and almost none in response to the second.⁷ Indeed, 42% of Republicans say that Obama is a Muslim even when incentivized to say he is not (against 44% who say the same in a control condition). If the incentive is strong enough to eliminate motivated responding, these results show that motivated responding does not underpin many responses to questions about Obama’s religion. That said, the strength of the incentive is unclear, and the modified balanced incentive design invokes other assumptions that may not hold (Bullock & Lenz 2018).⁸

Consider next the list experiment, often used to elicit honest answers to sensitive questions (e.g., Kuklinski et al. 1997). In a conventional list experiment, control-group subjects receive a list of statements. Treatment-group subjects receive a list that includes the same statements and one additional statement—the target statement. All subjects are asked how many statements they believe. Ideally, the difference between the mean numbers provided by the control and treatment groups is the proportion of treatment-group members who believe the target item. Random assignment will ensure that this proportion is also an unbiased estimate of the proportion of control-group subjects who believe the target statement.

Because subjects are never asked to state their view of any particular statement, the experiment affords a degree of anonymity. This anonymity may lead people to reveal beliefs that they are normally unwilling to reveal. It may thereby produce an unusually accurate measure of the proportion of people who believe a particular claim. And unlike the treatments described in the previous section, anonymity seems unlikely to encourage more careful thinking. Thus, by comparing the list-experiment measure of belief in a statement to a normal survey measure of belief, we may be able to identify the extent of cheerleading, distinct from congenial inference.

To our knowledge, only two list experiments have been conducted to determine the extent of partisan cheerleading. The first is a never-published experiment by Brendan Nyhan; the second was conducted by Berinsky (2018). The target item in both experiments was “I believe Barack

⁷Interestingly, Berinsky introduces the modified balanced incentive design not to distinguish cheerleading from congenial inference, but to purge his estimates of the influence of respondents who will “say anything to get the money” (see footnote 5).

⁸Berinsky’s study, an online experiment, was inspired by DellaVigna et al. (2017). Those authors ran a face-to-face experiment that involved financial payments to some subjects and a “time discount” to others: They gave some subjects the opportunity to conclude a survey eight minutes earlier than they had anticipated. The results suggest that, to their subjects, saving eight minutes of survey time was worth nearly \$5 (DellaVigna et al. 2017). If we assume that Berinsky’s subjects were similar and that the contexts were similar—for example, if we assume that people were willing to pay the same amount to shorten a face-to-face interaction as they were to shorten an online survey interaction—the result implies that Berinsky’s five-minute time discount was worth about \$3, which is large by the standards of the experiments discussed here.

Obama is a Muslim.” Nyhan’s experiment produced no result, suggesting no cheerleading. Berinsky’s results indicated that 37% of Republicans in his sample believed that “Barack Obama is a Muslim.” But other subjects were directly asked whether they shared that belief, instead of being assigned to the list experiment, and when directly asked, 44% of Republicans agreed with the statement. The experiment thus suggests that 7% of Republicans are cheerleading when asked about Obama’s religion.

The list experiment is a clever design, but for three reasons, it may produce inaccurate estimates of cheerleading. First, the incentive offered by a list experiment may be weak in the context of partisan beliefs, suggesting that list-experiment estimates of cheerleading may be lower bounds. Indeed, list experiments generally seem to be underpowered (Blair et al. 2018), perhaps in part because they offer insufficient inducement to answer sincerely (Rosenfeld et al. 2016; Tourangeau & Yan 2007, pp. 872–73). Second, the list experiment relies on the assumption that people’s responses to the target item are unaffected by the control items. But this assumption is sometimes violated—for example, when control items spur anger, which in turn changes people’s response to the target item (Blair & Imai 2012). Third, list experiments may be ill-suited to the study of cheerleading when social desirability effects are also at work. For example, if some people are cheerleaders who stop claiming to believe that Obama is a Muslim under anonymity, while others believe that Obama is a Muslim but will say so only under anonymity, the two groups have cross-cutting effects. They may cancel each other out, producing list-experiment estimates that do not differ from the direct-question estimate (B. Nyhan, personal communication; Gelman 2014).

Two other studies may more cleanly isolate the effects of cheerleading from those of congenial inference. Schaffner & Roche (2017) find that, just after the 2012 announcement of a major drop in the unemployment rate, Republicans became more likely to say that unemployment had increased. Of course, this result could be due to congenial inference. For example, hearing positive news about unemployment may cause Republicans to canvass their memories for other considerations about unemployment. These other considerations may be negative, and collectively, they may outweigh the positive news. In this sense, the positive announcement about unemployment may have caused Republicans to believe that the unemployment rate was actually getting worse (Schaffner & Roche 2017, p. 104).

But the more likely explanation, we venture, is cheerleading on the part of Republicans. Respondents had heard that the unemployment rate declined, and as the announcement came only one month before a presidential election, its implications were stark. With the election in mind, some Republicans’ estimates of the unemployment rate were higher than they would have been in the absence of the announcement. This does not seem to be congenial inference of any sort; this seems to be cheerleading.

The second study that more cleanly isolates the effects of cheerleading is that of Schaffner & Luks (2018), who simply asked respondents which of two photographs had more people. They did so in the midst of a controversy over the size of crowds at the Obama and Trump inaugurations. The Obama inauguration had drawn a far larger crowd, and although Schaffner and Luks provided no context when they asked respondents about the photographs, it is likely that some respondents inferred (correctly) that the photographs were of the crowds at the two inaugurations. The authors found that 15% of Trump voters gave the wrong answer to the question about crowd size, but only 2% of Clinton voters did. It is hard to see a role for congenial inference in this result. When asked “Which photograph has more people?” one does not canvass one’s memory for relevant considerations; there are none in memory. The result is hard to reconcile with any process other than cheerleading: Approximately 15% of Trump voters knew the correct answer but chose to give the wrong one. Schaffner and Luks further argue that, had more Trump voters been aware of the controversy, more would have given the wrong answer.

Evidence from behavior. Another approach to distinguishing between cheerleading and its alternatives is to examine whether partisan differences in survey responses correspond to differences in behavior. For example, if Democrats really believe that the economy will fare better under a Democratic president, we may expect them to spend more money shortly after a Democratic candidate wins a close election. But if their stated views on the economy merely reflect cheerleading, we would not expect this pattern. Using tax receipts from US states, Gerber & Huber (2009) found exactly this pattern. A subsequent study, however, showed that their finding arose primarily because of anomalous spending data from one state in one year (McGrath 2017). The same study concluded that their finding does not appear in data from recent elections in the United States or Europe. At least on economic perceptions, then, the costly-behavior test seems more supportive of cheerleading.

In contrast, another recent study does suggest that partisanship leads to costly economic behavior, and it thereby calls cheerleading explanations into question. Wintoki & Xi (2017) find that Republican- and Democratic-leaning mutual-fund managers disproportionately invest in businesses run by copartisans. They do so at relatively high levels—copartisanship accounts for a shift of about 7% of assets—even though doing so lowers their average returns and increases the volatility of their funds. The return and volatility results suggest that superior information cannot explain the bias. They also find that it is weaker among experienced managers and managers at larger firms.

At least three studies suggest that differences in beliefs lead to differences in behavior in the domain of healthcare. First, partisans do not just claim to hold different beliefs about the merits of the Affordable Care Act (ACA)—they act as though they really hold those beliefs, with Republicans enrolling at lower rates (Lerman et al. 2017). Second, Republican and Democratic physicians not only express different beliefs about the seriousness of certain patient reports—having multiple abortions, smoking marijuana regularly, or having multiple firearms in their homes—but also differ in the treatments that they say they would offer to patients making such reports (Hersh & Goldenberg 2016).⁹ Finally, depending on who is president, partisans both express different beliefs about vaccines and seem to vaccinate their children at different rates. That is, they seem to trust and to use vaccines more when their own party holds the presidency (Krupenkin 2018).¹⁰

Other evidence. The studies described above provide the best evidence to date on cheerleading versus its alternatives. We now briefly mention four other areas of evidence: studies of corrections of false beliefs, conspiracy theories, survey design, and the effect of accuracy incentives on accuracy itself.

Partisan differences in survey responses to factual questions must reflect some incorrect responding among members of at least one party. If these incorrect responses are due to full-throated cheerleading, providing respondents with correct information may do little to change their responses. By contrast, if incorrect responses are the product of congenial inference, providing respondents with correct information may decrease the rate of incorrect responding. Unfortunately, the literature on corrections of misinformation has yielded mixed findings. In some studies,

⁹Although Hersh & Goldenberg (2016) observe only reports of behavior on a survey, not the behavior itself, they made the survey realistic. They presented physicians with vignettes describing the patients, and to identify the party registration of physicians, they used public records rather than survey questions about party identification.

¹⁰Other studies find partisan differences across a range of behaviors. For example, McConnell et al. (2018) report a willingness to work for copartisans at lower wages. But these findings do not necessarily reflect partisan differences in factual beliefs. They may simply reflect a preference for copartisans, much like the preference for physically attractive individuals (Hamermesh 2011).

corrections meet stiff resistance (Nyhan & Reifler 2010, Nyhan et al. 2013, Thorson 2016). But in others, corrections lead to updating and diminished partisan differences (Nyhan et al. 2017, Weeks 2015, Wood & Porter 2018). Most of these studies look at politicized rumors, such as “Barack Obama is a Muslim.” Studies that examine nonrumor factual beliefs, such as the unemployment rate, may find clearer evidence that people update (Hill & Huber 2019; Robbett & Matthews 2018, pp. 116–17).

Numerous studies have examined the correlates of beliefs in rumors and conspiracy theories. These studies find that predispositions such as paranoia, anxiety, and authoritarianism correlate with belief in conspiracy theories (Abalakina-Paap et al. 1999, Allport & Postman 1947, Anthony 1973). For example, Oliver & Wood (2018, p. 122) find that “intuitive thinking,” as opposed to “rationalist thinking,” predicts belief in political conspiracy theories (see also Oliver & Wood 2014). These associations seem inconsistent with a pure cheerleading account of partisan divergence on such items. If partisan differences reflected cheerleading alone, we would not expect them to be correlated with a general tendency to believe in conspiracy theories.

In a creative new paper, Huber & Yair (2018) hypothesize that cheerleading may arise from the desire to send a partisan message. Given the opportunity to send that message before answering factual questions, they reason, partisans should be less likely to cheerlead. Examining judgments of candidate physical attractiveness, they find exactly that pattern: reduced partisan differences. Their findings, therefore, suggest that cheerleading lies behind some partisan differences.

If people know the right answer but give a false response because they are cheerleading, then accuracy incentives should not only decrease partisan differences but also increase accuracy. Although accuracy incentives used by Bullock et al. (2015) and Prior et al. (2015) clearly reduce partisan differences in survey responses, it is not as clear that they cause respondents to answer more accurately. Bullock et al. do not examine the effects of incentives on accuracy. Prior et al. (2015, especially p. 503) do examine the effects of incentives on accuracy, although these effects are not their focus; they find that incentives increase accuracy in their first study but not in their second. In general, the argument that incentives reveal cheerleading in ordinary survey responding is stronger if incentives lead people to answer more accurately, weaker (but still plausible) if incentives only reduce partisan differences in survey responses.¹¹

The tests described in this section are capable of distinguishing cheerleading from alternatives, at least to some extent. But these tests are few. In addition, some of the research designs used here—notably the modified balanced incentive design and the list experiment—impose strong assumptions that may not be met. In short, while existing research does permit some generalizations about the extent of motivated responding, it permits only the weakest generalizations about the extent of cheerleading, distinct from congenial inference.

UNCERTAINTY AND MOTIVATED RESPONDING

Although the evidence does little to distinguish between cheerleading and its alternatives, it speaks more strongly to another aspect of motivated responding: the extent to which apparent partisan differences in factual beliefs depend on a lack of confidence in the correct answers to factual questions. This point is deeply underappreciated in research on partisan differences.¹² As we have

¹¹Robbett & Matthews (2018, p. 114) do find evidence of increased accuracy, but only when the correct response “challenges” respondent partisanship, e.g., when Democrats have to admit that inflation decreased under Reagan.

¹²More broadly, the absence of confidence in responses to most survey questions about politics is underappreciated. But it has been recognized by some for decades (e.g., Alvarez & Franklin 1994, Bishop 2005, Ortoleva & Snowberg 2015).

argued, partisan differences in survey responses will sometimes be due neither to cheerleading nor to sincere differences in beliefs, but to the use of pro-party responses among those who have little notion about the correct answers. In support of these claims, Bullock et al. (2015, p. 541n15) find that when subjects are offered a “don’t know” response option but given no incentive to use it, only 15% of their answers to factual questions are “don’t know.” But in a different condition, subjects are paid to answer correctly—and are also offered a smaller incentive to say “don’t know.” Subjects in this condition stand to gain more from guessing at the correct answer than from saying “don’t know” if they have any idea at all about the correct answer. But even so, fully 46% of subjects in this condition reported that they didn’t know the answer, and the average reduction in partisan differences—relative to the no-incentive control condition—was 80%. The size of the reduction depended on the sizes of the incentives, and when incentives were at their largest—\$1 for each correct answer, 33 cents for each “don’t know” response—partisan differences vanished entirely (Bullock et al. 2015, pp. 550–55). The dramatic effects of a small incentive to say “don’t know,” even when coupled with a larger incentive to answer correctly, suggest that many people don’t know the answers to factual questions about politics, and know that they don’t know the answers, but will not admit their lack of knowledge under ordinary survey conditions.

Khanna & Sood (2018, p. 97) also find that a widespread lack of confidence changes the color of their results. When asked to interpret the data tables that they use in their studies, only 13% to 23% of their respondents report being “very confident” or “certain” of their interpretations. (The exact percentage varies from study to study.) The percentages of subjects who are both confident and incorrect are smaller still: 10% and 4%.

These results suggest not only that cheerleading may be less common among people who are confident that they know the truth, but also that ordinary surveys may lead us to overestimate the public’s confidence in its knowledge of the answers, as well as the degree of confidence that underpins partisan differences in surveys. In addition, the results suggest that many people who do not know the answers are aware of their own lack of knowledge (see also Hill & Huber 2019).

CONCLUSION

It is important to determine whether partisan differences in answers to factual questions reflect real differences in beliefs. If they do, it may pose a problem for democracy. If citizens are to hold their politicians accountable, they probably must have some sense of the relevant facts. And to the extent that they lack a sense of the facts, they may be less able to reward or punish incumbents for their performance (Fiorina 1981).

When partisans respond to survey questions, do they believe what they say? As this review makes clear, the question does not permit a simple answer. Scholars need to separate cheerleading from its alternatives, and they have not yet made much progress on this front. That said, the growing body of research does permit us to draw several conclusions.

First, for many factual questions about politics, a substantial portion of the partisan differences that we observe is not due to sincere, considered differences of belief. We have reached this conclusion in part because of incentives’ often large effect on responses and in part because people often express low confidence in their beliefs. This is not to say that cheerleading (i.e., insincere partisan responding) accounts for a substantial portion of the partisan differences. As we have explained, cheerleading is only one kind of motivated responding, and existing studies do not allow us to make strong generalizations about the extent to which any particular kind of motivated responding accounts for partisan differences. We have also argued that an accuracy-based partisan heuristic can explain partisan gaps. These alternative accounts make the study of cheerleading especially difficult, as interventions that reduce cheerleading may also reduce congenial inference or

reliance on heuristics. Because cheerleading and congenial inference look the same in survey data and may often respond similarly to interventions, they amount to something like an empiricist's nightmare.

Second, some partisans cheerlead at least some of the time. The Schaffner & Luks (2018) study clearly suggests that about 15% of Trump voters were cheerleading in response to a question about inauguration crowd sizes. And their 15% estimate may understate the percentage of Republicans who cheerlead at least occasionally. However, we do not yet know how well their result generalizes to other times, other topics, or other groups of people.

Third, people often lack confidence in their responses to factual questions about politics. As a result, the pattern of responding that Schaffner & Luks (2018) observe is probably rare: It is probably rare for partisans to believe one thing with confidence while saying another. Perhaps they would often engage in this sort of behavior if they often held beliefs with confidence. But they don't.

Fourth, and more preliminarily, the degree to which partisans believe unfounded political rumors seems higher than one might expect given the above conclusions. For example, Berinsky (2018) found that 42% of Republicans still said that Obama was a Muslim even when incentivized to say he was not.

Our review makes clear the need for more research. In particular, more attention should be paid to people's lack of confidence in their own factual beliefs about politics. Even when partisan differences in survey responses reflect sincere differences in factual beliefs, those beliefs may be too weakly held to matter in the voting booth or to otherwise affect partisans' behavior. In this case, concerns about accountability may be minimal. By contrast, if these weakly held beliefs nevertheless influence behavior, then concerns about accountability may be real. Put more generally, we need to know whether the accuracy incentives that people face when voting are large enough to offset the partisan biases that may sometimes affect their survey responses.

Most importantly, we need studies that distinguish cheerleading from its alternatives. The creative designs of Huber & Yair (2018) and Berinsky (2018) should be developed and applied to other factual questions. Other questions and topics need to be studied, too. For instance, the studies in which scholars pay for correct answers are focused on numerical facts, such as the unemployment rate. Incentives do reduce partisan differences in responses to questions about facts of this sort—but these studies do not examine the hyperpartisan conspiracies and rumors now present in American politics. It will be fascinating to see whether financial incentives reduce partisan differences on questions about these matters, too.

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