POLITICAL SURVEYS BIAS SELF-REPORTED ECONOMIC PERCEPTIONS

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Abstract   If voters are to hold governments to account for the state of the economy, they must know how it has changed. Indeed, this is a prerequisite for democratic accountability. Yet the perceptions that voters report often show signs of clear partisan bias. At present, we do not know if this bias is real or instead due to priming in political surveys. To test this, I assign subjects at random to either a political or nonpolitical survey. I then record their economic perceptions and compare the results for each group. I show that political surveys do worsen partisan bias, though only among supporters of the incumbent party. Still, much partisan bias remains unexplained, even in the nonpolitical condition. So, while economic perception items remain biased, we can at least be sure that most people respond to them in a similar way no matter the survey context.

Introduction

To hold governments to account for the state of the economy, voters must first know how it has changed. Indeed, this is an essential prerequisite for democratic accountability (Ashworth 2012; Healy and Malhotra 2013). Thus, voters should notice that conditions improve when the economy grows and worsen when it shrinks. Just as this variation in perceptions is important, so too are its consequences. If voters are to reward and punish appropriately, then they should be more likely to support the incumbent where they also

think that the economy has improved. This two-step process—first, of economic updating; second, of electoral sanctioning—is crucial for good governance. Rather than force voters to suffer fools, it lets them "kick the rascals out" when they fail to live up to expectations (Stegmaier, Lewis-Beck, and Brown 2019).

Though this idea has great normative appeal, reality often falls short. Voters are not so dispassionate when it comes to judging economic management. Instead, all manner of considerations influence the decisions that they make. For instance, voters use their pre-existing beliefs to process new information. Evidence of this behavior is rife in political surveys, where respondents often report economic perceptions that show clear signs of partisan bias: those who support the incumbent tend to be more positive, and those who support the opposition more negative, than those who support no party at all (for recent evidence of this phenomenon, see De Vries, Hobolt, and Tilley 2018; Bailey 2019; Bisgaard 2019).

Given the potential ramifications, much work now focuses on mitigating this bias. Yet we still do not know if it is meaningful or, instead, the result of partisan priming in political surveys. I test this possibility in this paper using new survey experimental data collected during the 2019 UK General Election campaign.

I find that political surveys worsen partisan bias in voters' self-reported economic perceptions. But this is true only for those who voted for the incumbent at the last election. What's more, much partisan bias remains unexplained. Thus, while economic perception items are far from perfect, survey researchers and economic voting scholars can at least be sure that most partisan bias remains no matter the survey context.

Economic Perceptions, Partisan Bias, and Political Surveys

Party identification biases the economic perceptions that voters report (see, for example, Conover, Feldman, and Knight 1987; Bartels 2002; De Vries, Hobolt, and Tilley 2018). What's more, this bias serves to undermine accountability mechanisms that make democracy possible (Anderson 2007; Healy and Malhotra 2013). The reason for this is simple. Partisan voters report economic perceptions that paint their party in a positive light. For example, incumbent supporters tend to report more positive economic perceptions. Opposition supporters, instead, tend to report more negative ones. Thus, we cannot be sure that voters will hold their party to account on the basis of economic management when in power.

While we know that party identification affects reported economic perceptions, we are less certain *why* this is the case. Clearly, this is an important gap in our understanding. If we do not know what causes partisan bias, we cannot hope to mitigate its worst effects. To this end, the literature on

economic perceptions offers four competing hypotheses. Though they are competing, they are not exclusive. Rather, all four likely influence the economic perceptions that voters report to some extent or another. I discuss each below in turn.

The first potential cause of partisan bias is consistency-motivated reasoning (Kunda 1990). This theory holds that voters weight new information based on its congruence with their existing beliefs (Hill 2017). This behavior weakens voters' ability to hold parties to account. Still, it is easy to see why it might play a role in their economic calculus. Existing research shows that the economy shapes wider perceptions of party competence (Green and Jennings 2017). And this can lead to much psychological discomfort for partisan voters (Groenendyk 2013). Consider an incumbent supporter during the middle of an economic downturn. Such a voter must contend with two conflicting beliefs: that the economy is doing badly and that their own party is managing the economy well. Consistency-motivated reasoning offers them a way out of this conundrum. By down-weighting incongruent information, they can either ignore evidence that the economy is doing badly or, instead, update their perceptions based only on information that pins the blame on someone else (Tilley and Hobolt 2011; Bisgaard 2015, 2019).

The second potential cause is partisan cuing (Brady and Sniderman 1985). Like consistency-motivated reasoning, this too is psychological in nature. It suggests that voters make use of cognitive shortcuts. These shortcuts are necessary because many voters pay little attention to politics (Campbell et al. 1960). As such, they have a hard time when it comes to making political decisions. The partisan cuing literature argues that they resolve this problem by making a simple substitution. Rather than derive their own belief, they rely on their favorite party's position on the issue instead. They may do this either out of party loyalty or the belief that they would have come to the same conclusion were they fully informed (Brader, Tucker, and Duell 2013; Ramirez and Erickson 2014). Evidence in favor of partisan cuing is most striking where it concerns party elites. Bisgaard and Slothuus (2018), for example, show that when the Danish government began to consider the budget deficit in a negative light, its supporters came to do so too, despite not having done so a short time before.

The third potential cause is expressive responding (Bullock et al. 2015; Schaffner and Luks 2018). Unlike the two previous explanations, it does not rely on voter psychology to explain partisan bias. Instead, it contends that survey respondents use survey items to signal their support for a particular party. For example, a respondent might report that the economy has gotten better not because they really believe it, but because they support the incumbent party. Recent survey experimental evidence shows that expressive responding almost certainly occurs. Though concerned with factual questions, Bullock et al. (2015) and Prior, Sood, and Khanna (2015) run similar

experiments where they manipulate the incentive to engage in expressive responding. Respondents in the treatment groups received a small cash reward where they either admitted to not knowing the answer or happened to give the correct answer to a series of factual questions about the economy and other policy-related topics. Respondents in the control groups received no such reward. In both cases, the authors find that partisan disagreement was lower under the treatment than under the control, implying that some responses serve only to signal respondents' party preferences.

The fourth and final potential cause is item-order effects. These occur where the order in which survey questions are asked affects the answers that respondents give. If nonpolitical items precede political ones, they may *personalize* respondents' answers. Likewise, where political items precede nonpolitical ones, they may *politicize* them instead (Sears and Lau 1983). Item-order effects are both large and long-lasting. Indeed, even where several buffer items separate them, political questions still come to bias the economic perceptions that respondents report (Wilcox and Wlezien 1993). Further, as many electoral surveys begin by asking their respondents how they voted or how they intend to vote, this politicization of economic perception items is probably common.

Though distinct, all four causes share a common catalyst: the political survey context. That is to say, partisan priming in political surveys might worsen their effects. For the sake of illustration, consider expressive responding. If the survey context implies that the survey administrator does not care about politics, then respondents face fewer incentives to engage in partisan cheerleading. Likewise, consider motivated reasoning and partisan cuing. If the survey context does not encourage respondents to consider the economy through a partisan lens, then it seems reasonable to expect them to be less likely to rely on partisanship to determine what they think about the state of the economy. It is for this reason that most consumer confidence surveys rarely ask for respondents' party affiliations (Curtin 2019).

As a result, the political survey context *itself* might moderate how party identification affects the economic perceptions that voters report. And, given that partisan bias varies direction based on party identification, so too should political survey effects. Thus, we should expect incumbent supporters to be *more* likely to report positive and *less* likely to report negative economic perceptions in political compared to nonpolitical surveys. We should expect opposition supporters, instead, to do the opposite. This implies the two following hypotheses:

Hypothesis 1: Incumbent partisans report *more positive* economic perceptions in political surveys than do similar incumbent partisans in nonpolitical surveys.
Hypothesis 2: Opposition partisans report *more negative* economic perceptions in political surveys than do similar opposition partisans in nonpolitical surveys.

Experimental Design

I use a simple survey experiment to test my hypotheses. The market research and polling company YouGov collected the corresponding data from its panel of eligible British voters.[1] Data collection occurred between November 6 and 8, 2019.

The British case is especially useful and provides a strong test of my argument for two reasons. First, data collection coincided with the start of the 2019 UK General Election campaign. Thus, my subjects were exposed to a general politicization of the information environment that we might expect to bias their responses in *nonpolitical* surveys too. Any differences between my treatment and control groups are, therefore, likely conservative. Second, data collection also occurred at a time of economic uncertainty. Though the economy was not in recession, it was not growing much either. At the time, GDP data showed that the UK economy had contracted by 0.2 percent in the previous quarter. This is important, as new evidence shows that even strong partisans "get it" when the going gets tough (De Vries, Hobolt, and Tilley 2018; Bisgaard 2019) and that this leads partisan bias to diminish (Stanig 2013; Bailey 2019). As such, it seems reasonable to expect the economic circumstances at the time to provide less overall partisan bias for my treatment to manipulate.

In the first stage of the experiment, I drew a blocked sample from YouGov's online panel.[2] The first block contained only those panelists who had voted for the incumbent Conservative Party at the last election in 2017, the second only those who had voted for an opposition party, and the third only those who had not voted at all.[3] To determine my sample size, I conducted a simulation-based power analysis. The results from 6,000 simulated experiments showed that I would need a sample of around 2,500 respondents to reach a power level of 80 percent.[4]

I did not have my participants report their voting behavior during the experiment, but instead relied on contemporaneous data that YouGov collected after the 2017 election. As such, misreporting bias or other related issues should be low. Some might argue that it would be better to use participants'

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1. YouGov uses nonprobability samples, *not* convenience samples. It ensures that its data are nationally representative using "active sampling" (Twyman 2008). This approach has proven robust, and the company's surveys often yield results substantively similar to those collected using random probability sampling (Sanders et al. 2007). Note, however, that because active sampling uses nonprobability samples, it is not possible to provide a conventional response rate.

2. The design is deliberately nonrepresentative to maximize power. As such, I do not weight my data. Regardless, this likely makes little difference. As Miratrix et al. (2018) show, also using YouGov data, "sample quantities, which do not rely on weights, are often sufficient" (p. 275).

3. Retention was high. Just 3.5 percent (91) of respondents failed to finish the survey. Of these, 48 left before being assigned to a condition, 18 left after being assigned to the treatment, and 25 left after being assigned to the control.

4. For more information, see Supplementary Material, section A.

current party identification and not how they voted in the past. After all, attitudes and choices change over time. While this is a reasonable objection, it is not possible to include such an item without undermining the nonpolitical survey context. Further, using past voting behavior has one particular advantage: voters cannot undo it. This may explain why it appears to exert such a considerable effect on the economic perceptions that voters report in political surveys (Anderson, Mendes, and Tverdova 2004).

In the second stage of the experiment, I exploited YouGov's day-to-day operations to administer my treatment. As a large commercial polling company, YouGov runs many simultaneous political and nonpolitical surveys. It also runs them in tandem. As a result, panelists are used to surveys that concern one topic then switch to another. My treatment group first completed a version of YouGov's standard voting intention poll. This includes five questions that concern voting behavior and the perception of party leaders. The control group, instead, completed a survey on dental hygiene. This had an almost identical structure to the political survey. For example, it asked the same number of questions, the same type of questions, and included the same number of response options in all cases. Further, it also used only questions that YouGov had fielded in the past to ensure that it was believable.[5] In all cases, participants had an equal chance of being assigned to the treatment or to the control.

In the third and final stage of the experiment, I again exploited YouGov's day-to-day operations, this time to measure my participants' economic perceptions. After receiving their treatment, both groups saw the topic of the survey switch from politics or dental hygiene to the economy. I then asked them to report their own retrospective economic and financial perceptions. As I used a sample of eligible British voters, I followed the lead of the British Election Study Internet Panel (Fieldhouse et al. 2020) and had my participants answer the two following questions:

- Now, a few questions about economic conditions. How does the *financial situation of your household* now compare with what it was 12 months ago?
- How do you think the *general economic situation in this country* has changed over the *last 12 months*?

These items have their origins in consumer confidence surveys (Katona 1951), entered political science via *The American Voter* (Campbell et al. 1960), and are now ubiquitous in economic voting research (Lewis-Beck and Stegmaier 2007).[6] By and large, the literature on economic perceptions and

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5. The full questionnaire is available in the Appendix.

6. Note that the consumer confidence surveys from which these items originate rarely field questions of partisanship, as they are known to engender emotional states that bias how survey respondents answer economic perception questions (Curtin 2019).

partisan bias focuses only on national-level items (for recent examples, see Hansford and Gomez 2015; Anson 2017; Dassonneville and Lewis-Beck 2019). While I do include this item, I also asked my subjects to report their personal financial perceptions. Doing so serves two useful purposes: it provides a benchmark for any national-level effects and helps prevent an unusual one-question-long topic.

In both cases, my subjects faced exactly the same response options. They could answer each question on a five-point ordinal scale that ranged from *"1–Got a lot worse"* to *"5–Got a lot better."* They could also report that they did not know how either the national or their own personal economic situation compared to what it was 12 months ago. Where this was the case, I removed these participants using list-wise deletion.[7]

Figure 1 shows the raw percentages for each response option stratified by party and treatment status. As we would expect, these show that incumbent partisans are more positive than are nonvoters. Further, this is true under both the treatment and the control. For example, 17.6 percent of incumbent partisans in the treatment condition (a political survey) said that the economy had gotten a lot or a little better, while only 8.9 percent of nonvoters in the treatment condition said the same. Likewise, opposition partisans were more negative than nonvoters. Among opposition partisans in the treatment condition, 77.2 percent said that the economy had gotten a lot or a little worse, whereas only 61.6 percent of similar nonvoters made the same judgment.

Though still descriptive, figure 1 also suggests evidence in favor of my first hypothesis. Incumbent partisans in the political survey treatment condition were 3.3 percent more likely to say that things had gotten better than similar incumbent partisans in the control condition. They were also 10.0 percent less likely to say that the economy had gotten worse. The data suggest little evidence in favor of my second hypothesis. Opposition partisans in the political survey treatment condition were 1.4 percent more likely to say that things had gotten better and 2.6 percent less likely to say that things had gotten worse than opposition partisans in the nonpolitical control condition. While informative, any inferences that we make from these descriptive statistics do not account for the uncertainty inherent in the sample. To do so requires a more rigorous approach, which I describe in greater detail below.

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7. List-wise deletion can produce biased estimates if data are not missing completely at random. Still, simulation studies show that list-wise deletion yields less biased estimates than multiple imputation where data are missing not at random (Pepinsky 2018). Even so, I include these data as a robustness check (see Supplementary Material, section C). This does not change my results. Further, participants were no more likely to answer "Don't know" under the treatment than under the control condition.
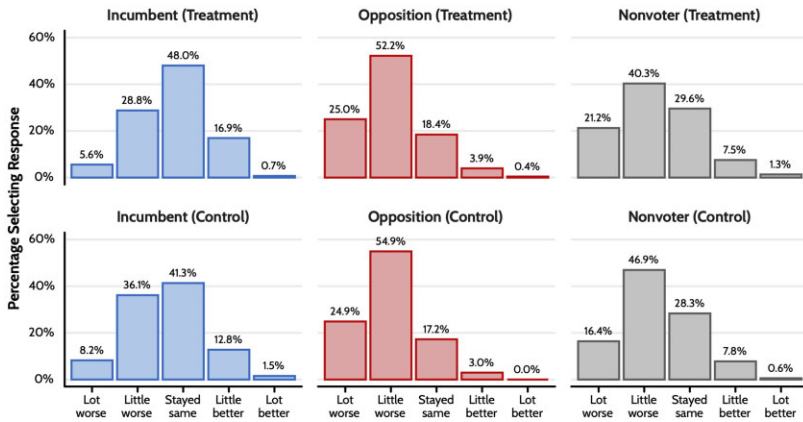
Figure 1. Distribution of responses under the treatment and control. Incumbent partisans (left column) tend to be more positive than nonvoters (right column). Likewise, opposition partisans (middle column) tend to be more negative than nonvoters. Further, these figures also suggest evidence in favor of my first hypothesis that incumbent partisans in the treatment would be more positive than incumbent partisans in the control, though not my second hypothesis that opposition partisans in the treatment would be more negative than opposition partisans in the control.

Modeling Ordinal Outcome Variables

Economic perception items yield ordinal data. Yet many researchers treat them as continuous. This is convenient, as it allows them to estimate treatment effects using only a simple comparison of means. But this simplicity belies drawbacks that include false positives, false negatives, and even estimates with incorrect signs (Liddell and Kruschke 2018).

One argument for treating these items as continuous is that while the outcome is ordinal, subgroup means and their differences are continuous. This is true. But it is not clear what such treatment effects even imply. Indeed, when ordinal variables have three or more response options, there are an *infinite* combination of response distributions that could produce any given difference in means.

Better then to model the choices that survey respondents really face: the ordinal variable's various response options. To do so, one might expect to use ordered regression. But these models face similar problems. Figure 2 shows why. Ordered regression treats the ordinal distribution that we observe (bottom row) as a function of a continuous one that we do not (top row). It then uses a set of threshold parameters (gray dotted lines) to convert between the two. These divide the latent continuous distribution into as many

segments as there are response options. The area between two thresholds then gives the probability of each response occurring.

The first column shows the baseline case. Here, each response has an equal probability. To change this, we can adjust either the latent distribution's mean or its variance. This has three consequences. When we adjust the mean, the latent distribution *shifts* up or down the scale (second column). This alters the area between the thresholds and moves the ordinal distribution in the same direction. When we instead adjust the variance, the latent distribution either *compresses*, squeezing the ordinal distribution's probability mass (third column), or *disperses*, piling up probability mass at the extremes (fourth panel).

As figure 2 shows, compression and dispersion can have large effects on the ordinal distribution. Yet conventional ordered regression accounts only for shift. This is a problem, as treatments may affect the outcome without shifting the probability mass to one end or the other. Dealing with this is difficult using Frequentist methods. Thus, in line with recent recommendations (Liddell and Kruschke 2018), I use Bayesian methods instead.[8]

My model is as follows. Let E_i be person i's reported retrospective economic perceptions. In line with existing economic voting research, this item is measured on a five-point ordinal scale, as described above, and which takes a value that varies between 1 = "Got a lot worse" and 5 = "Got a lot better." In order to model the data as ordinal, I assume that the observed ordered variable, E_i, is a function of some latent continuous variable, E_i^*. I then assume that this latent continuous variable follows a normal distribution with mean, μ_i, and standard deviation, σ_i:

$$E_i^* \sim \text{Normal}(\mu_i, \sigma_i)$$

Likewise, the observed ordinal outcome variable, E_i, takes a particular value as follows:

$$E_i = k \text{ if } \tau_{k-1} \leq E_i^* \leq \tau_k \text{ for } k = 1, \ldots, K$$

Here, τ_k for $k \in \{0, \ldots, K\}$ represent threshold parameters which segment the latent continuous distribution. We fix the 0th and k^{th} thresholds equal to $-\infty$ and $+\infty$, such that $-\infty = \tau_0 \; \tau_1 \; \ldots \; \tau_{k-1} \; \tau_k = \infty$. As such, the probability that $E_i = k$ is

$$\Pr(E_i = k) = \Phi\left(\frac{\tau_k - \mu_i}{\sigma_i}\right) - \Phi\left(\frac{\tau_{k-1} - \mu_i}{\sigma_i}\right),$$

where Φ is the cumulative distribution function of the normal distribution with mean μ_i and standard deviation σ_i. As I discuss above, both influence

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8. Note that Bayesian models require prior distributions. In this case, I specify a set of conservative and weakly informative priors for each parameter. I discuss my choices in greater detail in Supplementary Material section B.
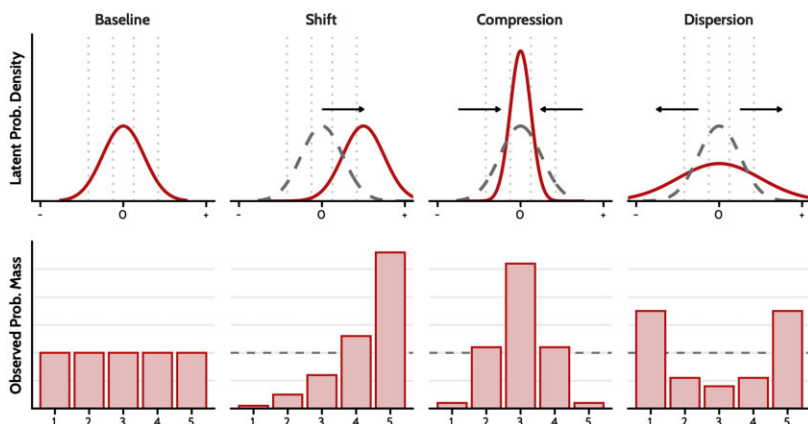
Figure 2. A graphical description of the assumptions of ordered regression. Ordered regression assumes that the observed ordinal scale is a function of a latent continuous one. When the latent scale shifts, so too does the probability of selecting a higher value on the observed scale. Likewise, when it compresses or disperses, the observed scale follows suit. As we can see, each may have a large effect. Yet most ordered regression models account only for shift.

the ordinal distribution that we observe. Likewise, both may also vary according either to party preference or treatment status:

$$\mu_i = \beta_1 T_i + \beta_2 I_i + \beta_3 O_i + \beta_4 T_i \times I_i + \beta_5 T_i \times O_i$$

$$\log\left(\frac{1}{\sigma_i}\right) = \delta_1 T_i + \delta_2 I_i + \delta_3 O_i + \delta_4 (T_i \times I_i) + \delta_5 (T_i \times O_i)$$

Here, T_i takes the value 1 where person i is in the treatment group. Likewise, I_i and O_i take the value 1 where person i voted for the incumbent or an opposition party at the last election, respectively. Rather than model σ_i, I instead model $\log\left(\frac{1}{\sigma_i}\right)$, thereby fixing σ_i to 1 for the baseline category (nonvoters) for the sake of identification.

Both my first and second hypotheses assume heterogeneous treatment effects. This is why the linear models I fit on μ_i and σ_i above include interactions between treatment status, T_i, and incumbent and opposition voting, I_i and O_i. Thus, I test my hypotheses based on the value of β_4 (the shift in latent mean for incumbent supporters under the treatment) and test my second hypothesis based on the value of β_5 (the shift in latent mean for opposition supporters under the treatment). As I use Bayesian methods, the standard decision criterion—a *p*-value less than 0.05—makes little sense, as Bayesian

statistics has no equivalent to statistical significance. Instead, I base my decision criterion on each parameter's posterior distribution; in particular, whether or not the parameter's 95 percent credible interval includes zero.

Though complex, the method that I use is robust to the various problems I discuss above. Still, like any ordered regression model, the parameters that it produces are hard to interpret. Fortunately, as Bayesian models are generative (Lambert 2018), we can have them estimate the treatment's effect on the more intuitive probability scale while also incorporating any inherent uncertainty. I do this below, and compute treatment effects for each response category as follows:

$$\text{ATE}_k = \Pr(E_i = k | T_i = 1) - \Pr(E_i = k \mid T_i = 0)$$

Results

Table 1 shows the resulting parameter estimates from my model. Here, the various mean parameters shift the latent continuous distribution. As we can see, and as we would expect, the political survey treatment appears to have had no effect on the economic perceptions that nonvoters reported (-0.02, 95 percent CI -0.16 to 0.12). Likewise, and again as we would expect, incumbent partisans tended to report more positive (0.41, 95 percent CI: 0.29 to 0.54) and opposition partisans tended to report more negative (-0.35, 95 percent CI: -0.49 to -0.22) economic perceptions no matter their treatment status.

In line with my expectations, it appears that political surveys *do* affect the economic perceptions that respondents report in political surveys. As my first hypothesis suggests, incumbent partisans who first completed a political survey tended also to report more positive economic perceptions (0.20, 95 percent CI 0.02 to 0.38). There was, however, little support for my second hypothesis. Unlike incumbent partisans, opposition partisans showed little to no difference in the economic perceptions that they reported under the treatment and the control (0.05, 95 percent CI 0.14 to 0.22). Political survey treatment effects may, thus, be limited only to those respondents who voted for the incumbent Conservatives in 2017.[9]

It is interesting to note that the treatment also caused differences in compression and dispersion too. For example, the treatment caused the range of responses that incumbent supporters reported to compress (0.18, 95 percent CI: 0.03 to 0.33), giving their latent economic perceptions less variance. As a consequence, incumbent partisans were not only more positive under the treatment, they showed a greater consensus too.

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9. This finding is robust to a range of tests. See Supplementary Material, section C.

Table 1. Parameter estimates from my ordered regression model showing the effect of treatment condition by vote choice

	Median	Error	2.5%	97.5%
Mean				
Treatment	−0.02	0.07	−0.16	0.12
Incumbent	0.41	0.07	0.29	0.54
Opposition	−0.35	0.07	−0.49	−0.22
Treatment × Inc.	0.20	0.09	0.02	0.38
Treatment × Opp.	0.05	0.09	−0.14	0.22
Discrimination				
Treatment	−0.10	0.06	−0.21	0.02
Incumbent	0.05	0.06	−0.06	0.16
Opposition	0.14	0.06	0.02	0.25
Treatment × Inc.	0.18	0.08	0.03	0.33
Treatment × Opp.	0.01	0.08	−0.16	0.16
Threshold				
Threshold 1	−0.94	0.07	−1.07	−0.81
Threshold 2	0.31	0.05	0.21	0.42
Threshold 3	1.40	0.08	1.25	1.56
Threshold 4	2.52	0.13	2.26	2.79
N				2,487

NOTE.—Incumbent supporters report more positive economic perceptions when they answer in a political versus a nonpolitical survey. Data come from a survey experiment conducted by YouGov between November 6 and 8, 2019, the start of the campaign for the 2019 UK General Election.

Figure 3 shows how the treatment's consequences on the latent scale affect the choices that my respondents actually made. Here, density plots show the full posterior distribution of each treatment effect, black bars their 95 percent credible intervals, and point estimates their median. Each density curve reflects the difference in the probability of reporting a given response under the treatment versus the control. Thus, a positive value implies that the political survey treatment increased the probability of a respondent picking a given response by a given number of percentage points compared to similar respondents in the control group.

The leftmost panel shows how the treatment affected those who voted for the incumbent party in 2017. As discussed above, incumbent supporters tended to be more positive on the latent response scale. This would suggest that they should also be more positive on the observed one too. This is exactly what we see. Under the treatment, incumbent voters were 3.7 percentage points (95 percent CI: 1.3 to 6.2) less likely to say that the economy "got
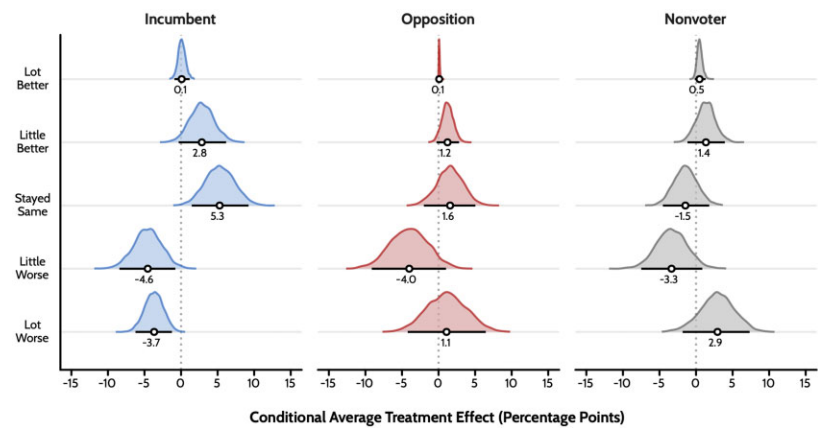
Figure 3. Conditional average treatment effects of the political survey condition. Political surveys cause incumbent voters to report different economic perceptions (left panel). They were less likely to say that the economy had gotten worse and more likely to say that it had "stayed the same" or "got a little better." Density plots show the posterior distribution of conditional average treatment effects. Further, black bars show their 95 percent credible intervals and point estimates their medians.

a lot worse" and 4.6 percentage points (95 percent CI: 0.8 to 8.4) less likely to say that it "got a little worse." In comparison, they were 2.8 percentage points (95 percent CI: -0.3 to 6.2) *more* likely to say that the economy "got a little better."

Interestingly, incumbent partisans appeared no more likely to say that the economy "got a lot better" (0.1, 95 percent CI: -0.9 to 1.2). This effect was also much more precise than for other responses. Though this may seem unusual, it arises only because almost no one reported that the economy "got a lot better." This is not uncommon, at least in the British case, even when the economy is booming (see Bailey 2019). Finally, those reporting that the economy "stayed the same" made up the difference. These participants were 5.3 percentage points (95 percent CI: 1.5 to 9.2) more likely to pick this option under the treatment compared to the control.

As the parameter estimates in table 1 suggest, the effect of taking a political survey was less clear where participants voted for an opposition party at the last election. These subjects were not much more likely to say that the economy "got a lot better" (0.1, 95 percent CI: 0.0 to 0.3), "got a little better" (1.2, 95 percent CI: -0.3 to 2.8), or "stayed the same" (1.6, 95 percent CI: -2.0 to 5.0) where they took the political survey treatment. And, while they were 1.1 percentage points (95 percent CI: -4.2 to 6.5) more likely to

say that the economy "got a lot worse," they were in fact 4.0 percentage points (95 percent CI: 0.3 to 9.1) *less* likely to say that it "got a little worse." Interestingly, nonvoters showed a similar pattern of treatment effects to opposition voters, though were even more muted. This is perhaps unsurprising, given that the participants who comprised this group presumably had little sense of party identification.

Political Surveys and Partisan Bias

One question remains unanswered: how much partisan bias do political surveys account for? With only a single experiment to draw upon, this is difficult to know. Yet we can approximate this proportion by assuming that my treatment effects represent upper bounds on the true effect. As I discuss above, my estimates are likely conservative. As such, treating them as an *upper* and not *lower* bounds is also conservative, as the true value may be larger.

Computing the bias *within* the experiment is simple if we use the parameters in table 1. One need only divide the treatment's main effect and its interaction with partisanship by its main effect, its interaction, and the main effect of partisanship. In the present case, this suggests that around 30.3 percent (95 percent CI: 11.4 to 47.3) of the partisan bias present in incumbent supporters' self-reported economic perceptions is due to the political survey context itself.

While informative, this estimate is limited only to a single case. It would be better to compute a *distribution* of proportions using data from many points in time. The British Election Study Internet Panel, 2014–2023 (Fieldhouse et al. 2020), provides one such source of data. The BESIP includes the national economic perceptions item in 15 separate waves. These cover the period between April 2014 and November 2019. I fit a similar ordered regression model to each wave of the data, then, as the data do not vary the survey context, use the treatment effect from my survey experiment to approximate the proportion of partisan bias due to the political survey context under the assumption that it remains constant.

Figure 4 shows the resulting estimates. For incumbent supporters, these range from a low of 20.1 percent (95 percent CI: 6.7 to 34.0) in wave 6 to a high of 31.7 percent (95 percent CI: 10.8 to 53.5) in wave 13. The average across all waves is 27.4 percent (95 percent CI: 9.4 to 45.8), suggesting that around one-quarter of all partisan bias in the economic perceptions that incumbent partisans report is due to political survey effects. The equivalent effects for opposition supporters are much weaker and much more uncertain.

Discussion and Conclusion

Survey research often proceeds as though survey respondents say what they mean. This is especially true when it comes to studying both the economic vote and voters' economic perceptions. Most often, this research assumes that differences between groups that exist *within the survey* reflect real differences that exist *outside of the survey* (Bullock and Lenz 2019). My results show that this is not always the case. Some partisan bias arises simply due to the political survey context itself. In particular, I show that incumbent partisans report more positive economic perceptions in political compared to nonpolitical surveys.

Why might this affect incumbent partisans but not opposition partisans? One explanation is that different partisans face different pressures when the economy is middling or poor. First, let us consider opposition partisans. When things are bad, these voters' primed and unprimed responses should coincide. Thus, they should show little difference in partisan bias. Now consider incumbent partisans, who face the opposite pressure. For this group, political priming leads them to report that things have gotten better. As a result, the economic climate causes a gap to open between the perceptions that they report in political and nonpolitical survey contexts. If this is correct, and the state of the economy moderates political survey treatment effects, then future research might find that its effect reverses when the economy is doing well.

As the political survey context worsens partisan bias in self-reported economic perceptions, the most pressing issue is to work out how this affects applied research. For the economic vote, the outcome is mixed. Measuring economic perceptions in political surveys almost certainly worsens economic voting's endogeneity problem (Evans and Andersen 2006; Evans and Pickup 2010; Visconti 2017). Yet any effects appear limited only to incumbent partisans, and much partisan bias remains constant. For political science in general, my results raise questions of measurement and validity. After all, these are likely not the only items sensitive to changes in survey context. Take items that measure attitudes toward immigration. It seems reasonable to expect voters to report different attitudes when primed to consider party politics or, say, changes in the labor market or even unrelated topics like dental hygiene.

The consequences are most serious for macro research. This is because small differences at the individual level can yield large differences at the aggregate level. Most often, this research aggregates these data in one of two ways. First, they compute the proportion of respondents who say that the economy has gotten better. However, my results suggest that doing so would overestimate how rosy incumbent supporters think things really are where these figures are split by partisanship (see, for example, Enns, Kellstedt, and McAvoy 2012). Second, they compute net economic perceptions; that is, the proportion of respondents who report that the economy has gotten better minus those who report that it has gotten worse. Yet using net figures both
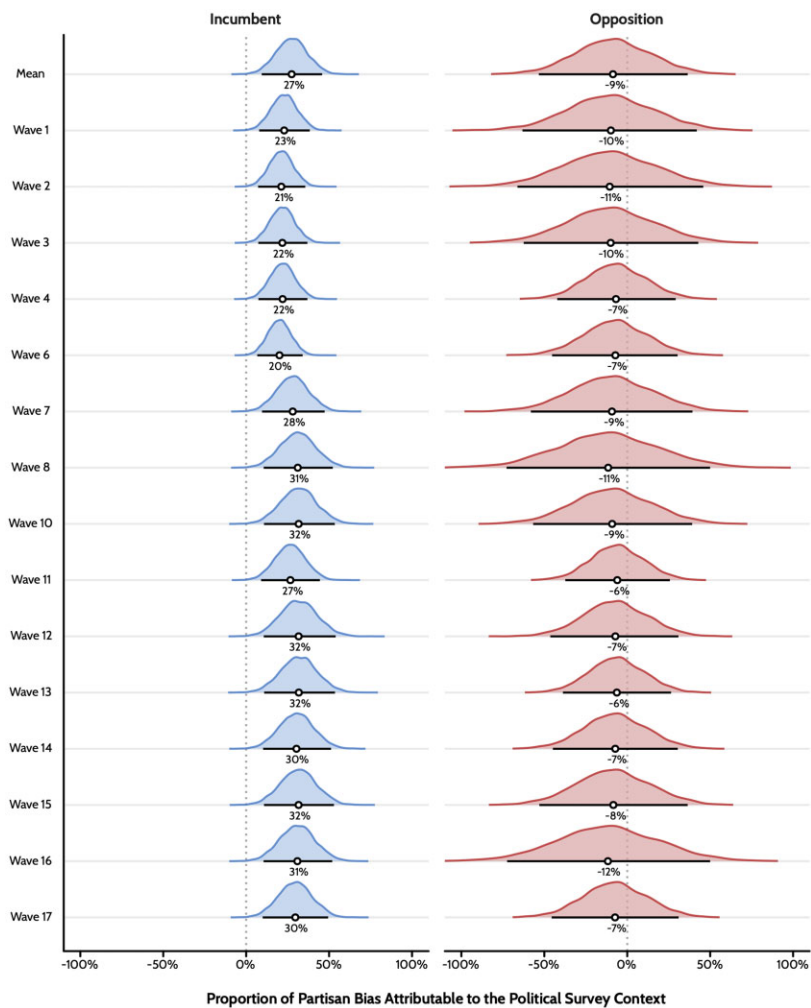
Figure 4. Estimated partisan bias due to the political survey condition in each wave of the BES Internet Panel. Assuming that my estimates represent upper bounds, the political survey context accounts for around one-quarter of all partisan bias in the economic perceptions that voters report in 15 recent waves of the British Election Study Internet Panel (BESIP). Density plots here show the posterior distribution of the proportion of partisan bias attributable to the political survey context. Further, black bars show their 95 percent credible interval and point estimates their median.

overestimates incumbent positivity and underestimates incumbent negativity. For example, the results in figure 2 suggest a difference in net economic perceptions of almost 12 percentage points, depending on the survey context. Some might argue that this is no issue for economic voting research that most often relies on real economic statistics (though see Lewis-Beck, Martini, and Kiewiet 2013). This might be true. Even so, it could still be a problem for the broader analysis of attitudes and opinions in mass publics.

Future research should consider if political survey effects are constant or, instead, if they vary by external context. For example, I have alluded to the possibility that my results might change in good economic times. Ultimately, this remains to be tested. Nevertheless, my results suggest a fruitful avenue for future research: we might opt not to adjust our *models*, but rather to adjust our *designs*. Research in this vein has already begun (Visconti 2017). One obvious suggestion would be to field separate surveys to measure respondents' party political and nonparty political attitudes and beliefs. Though this might be more costly, the gains could be considerable if it were to reduce nuisance variation that muddies our inferences. As a result, students of the economic vote might gain both a better understanding of how the economy affects voters' behavior and how voters come to update their economic perceptions.

Appendix. Questionnaire

Political Survey	Nonpolitical Survey
Q1. If there were a general election held tomorrow, which party would you vote for?	Q1. Imagine that you need to buy toothpaste in the near future, which brand would you choose?
Conservative	Colgate
Labour	Sensodyne
Liberal Democrat	Aquafresh
Scottish National Party (SNP)	Oral-B
Plaid Cymru	Macleans
Brexit Party	Arm & Hammer
Green	Crest
Some other party	Some other brand
Would not vote	I would not buy toothpaste
Don't know	Don't know

Q2. On a scale of 0 (certain NOT to vote) to 10 (absolutely certain to vote), how likely would you be to vote in a general election tomorrow?

0 – Certain NOT to vote

1

2

3

4

5

6

7

8

9

10 – Absolutely certain to vote

Don't know

Q3. Who do you think would make the best Prime Minister?

Boris Johnson [Database]

Jeremy Corbyn

Jo Swinson

Don't know

Q4. In hindsight, do you think Britain was right or wrong to vote to leave the European Union?

Right to leave

Wrong to leave

Neither right nor wrong

Don't know

Q2. On a scale of 0 (not at all important) to 10 (very important), how important do you think dental hygiene is in everyday life?

0 – Not at all important

1

2

3

4

5

6

7

8

9

10 – Very important

Don't know

Q3. Generally speaking, what type of toothbrush do you use?

Manual

Electric

I do not have a toothbrush

Don't know

Q4. When brushing your teeth, do you...

Wet your toothbrush, then apply toothpaste?

Apply toothpaste, then wet your toothbrush?

Not wet your toothbrush at all

Don't know

Q5. How well or badly do you think the government are doing at handling Britain's exit from the European Union?

Very well

Fairly well

Neither well nor badly

Fairly badly

Very badly

Don't know

Q5. Generally speaking, on average how many times do you brush your teeth every day?

Never

Once

Twice

Three times

More than three times

Don't know

Treatment ends. Subsequent questions are identical for each group.

Q6. Now, a few questions about economic conditions. How does the financial situation of your household now compare with what it was 12 months ago?

Got a lot worse

Got a little worse

Stayed the same

Got a little better

Got a lot better

Don't know

Q7. How do you think the general economic situation in this country has changed over the last 12 months?

Got a lot worse

Got a little worse

Stayed the same

Got a little better

Got a lot better

Don't know.

Data Availability Statement

REPLICATION DATA AND DOCUMENTATION are available at https://doi.org/10.7910/DVN/GSEXCC.

Supplementary Material

SUPPLEMENTARY MATERIAL may be found in the online version of this article: https://doi.org/10.1093/poq/nfab054.

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