

The Expressive Value of Answering Survey Questions

Matthew H. Graham

Gregory A. Huber

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Abstract

Do survey respondents view answering survey questions as a costly or a beneficial activity? This chapter introduces a new approach to understanding the expressive value of answering survey questions: a revealed preference measure that allows respondents to choose whether to answer five extra survey questions. Randomly assigning two features of this choice — the question used to “tease” the extra five questions, and the description of what those five questions would be used for — provides a range of insights. Most of our respondents choose to answer extra questions, suggesting that they gain an expressive benefits from doing so. These benefits are most widespread when respondents expect the extra questions to be about matters that are easily connected to partisanship. The anticipation of partisan political content introduces a sorting effect: respondents who choose to answer questions about politicized rumors are also more partisan in their responses than those who prefer not to answer such questions. The results have implications for research designs that aim to alter the expressive context of surveys, such as paying for correct answers, list experiments, and designs that use the threat of a longer survey to vary the cost of selecting different response options.

Introduction

In the last several years, political scientists have focused new attention on the expressive value of responding to political surveys. This chapter introduces a new design for studying who derives expressive benefits from surveys, which types of questions they derive expressive benefits from, and what about the process of responding to survey questions provides that benefit. The findings have implications research designs that seek to alter the expressive context of surveys and provide suggestive new evidence as to the effect of “recruiting on the dependent variable.”

Attention to the expressive value of surveys has focused on the expressive benefit respondents might derive from selecting particular responses. Expressive responding is the notion that individuals may answer questions not simply on the basis of what they truly believe, but also because they get more expressive benefit from some responses than others. For example, one might report that the economy is doing better (worse) than one believes to express one’s support for (opposition to) a co-partisan president. If expressive responding is widespread, correlations between reported economic perceptions and vote choice might arise not because perceptions affect voting (Fiorina 1978) or because partisan preferences affect economic perceptions (Bartels 2002; Evans and Andersen 2006), but because partisanship affects *reported* economic perceptions.

Situations that lack an objective benchmark against which to evaluate responses create a particular challenge for understanding expressive responding. The first studies to document expressive responding found that when randomly assigned to be paid for correct answers, partisan response differences decreased by about half (Bullock et al. 2015; Prior et al. 2015). Designs like this are less credible when there is no unambiguous truth to appeal to. In response, scholars have developed designs for examining expressive response tendencies that do not depend on incentivizing a correct answer. Even after Schaffner and Luks (2018) present respondents with unambiguous evidence that President Barack Obama’s 2008 inaugural crowd was larger than President Donald Trump’s crowd in 2016, 15 percent

of Republicans claim that Trump’s crowd was larger. [Berinsky \(2018\)](#) deploys a series of non-monetary incentive designs, finding little evidence of expressive responding.

The rise of designs like these has created a need for a more substantial evidentiary basis for understanding the nature of expressive motivations in survey-taking. This chapter contributes a new strategy to this effort. Rather than infer expressive motivation from the content of responses, we introduce a revealed preference measure of the utility subjects expect to receive from answering survey questions. The design “teases” respondents with a survey question, then presents them with a simple choice: would you like to answer some extra survey questions, or would you like to go straight to the survey’s final question? Designs of this kind enable four types of inferences. First, examining the characteristics of respondents who choose to answer additional questions provides an understanding of who derives expressive utility from the act of answering survey questions. Second, variation in these relationships according to the “teaser question,” sheds light on how expressive utility varies with question content. Third, the extent to which interest in answering extra questions varies with the implied purpose of the questions — a test survey or a national poll — enables inferences about whether internal or external motivations drive expressive benefits. Fourth, because all respondents answer at least one more question before the survey ends, we can compare the distribution of responses between respondents who choose to answer survey questions and those who do not. Together, these features provide a look at who gets expressive benefits, what they get expressive benefits from, and how differences in the distribution of expressive benefits predict differences in responses.

We produce several novel results. First, most survey respondents derive expressive benefits from answering survey questions, over and above the incentive payment they receive (which is held constant in our design). Depending on the content of the randomly-assigned teaser question, between 50 and 80 percent of our respondents chose to answer five additional questions. This demonstrates that answering additional questions should not be conceptualized as a purely costly activity.

Second, expressive benefits vary with the expected content of the survey. As a baseline, we used two “neutral” teaser questions that we expected to be difficult for respondents to connect to partisanship or other factors that may influence expressive benefits. On the most obscure of these, a question about job approval of the administrator of the General Services Administration (GSA), about 50 percent of respondents chose to answer extra questions. By comparison, three teaser questions involving rumors about Presidents Obama and Trump had about 70 percent uptake. A teaser about approval of the same presidents reached nearly 80 percent .

Third, expressive benefits vary according to respondent characteristics — but not always in the ways one might expect. Across the board, respondents who choose to answer more questions also self-report higher levels of political interest, higher voter turnout in the 2018 midterm, stronger partisanship, greater approval of their party’s policies, and a larger gap in favorability toward Trump and Obama. Surprisingly, however, we find no evidence of greater interest in answering questions among respondents who score higher on partisan social identity measure modelled after [Greene \(1999\)](#) and [Huddy et al. \(2015\)](#), or who more strongly endorse the “secret cabal” question that [Oliver and Wood \(2014\)](#) find to be predictive of rumor endorsement.

Fourth, our results suggest that expressive benefits may be more internal than external. Respondents who were randomly assigned to see the extra questions described as a “national poll” were no more likely to choose to answer them than respondents who saw them described as a “test survey.” This result is consistent with self-reported reasons among those who chose to answer more questions: these respondents are more likely to report that they enjoy answering the questions than to report that they wanted to express their beliefs to others.

Fifth, we find that the content of the teaser questions affects the response distribution of those who choose to answer the questions. Relative to respondents who chose to answer more questions after being exposed to a neutral teaser, we find that respondents who chose to answer questions after a rumor teaser gave responses that were 0.06 points more congenial

to their partisanship on a 0 to 1 scale. This suggests that people who gain more expressive utility from answering rumor questions also offer more partisan responses to those questions. Crucially, this does not necessarily mean that expressive utility is what caused the greater response congeniality — it could be that people who derive greater expressive utility from answering rumor questions also sincerely hold more-partisan beliefs. To our knowledge, however, our results constitute the first affirmative demonstration that respondents gain expressive benefits from answering rumor questions, and that these expressive benefits are associated with partisan rumor endorsement.

Research Design

We recruited 3,813 subjects using Lucid, a vendor that quota samples online survey respondents to Census demographic benchmarks.¹ We first obtained informed consent, after which subjects completed the remainder of the survey. Respondents who agreed to take our survey were not informed of its political content ahead of time. We did not collect any personally identifying information; consequently, subjects remained anonymous to us. The survey contained two experimental manipulations.

Our survey was divided into three sections. The first section collected a broad range of demographics and political attitudes, as well as responses for unrelated projects (attitudes about tax policy, economic perceptions, etc.). These items allow us to characterize the composition of our sample and understand whether these “pre-treatment” covariates predict subsequent post-treatment decisions and expressed opinions.

The second section asked respondents to choose between answering five extra questions or going straight to the end of the survey. This section had one item, which read:

The last part of the survey is a **five-question [Audience]**. One of the questions will be:

[Teaser question]

¹Table 2 includes a distribution of respondent demographics and political characteristics.

Would you like to answer these five questions or go straight to the final question?

[Audience] and [Teaser question] are independent randomizations and their potential values are described below.

The third section consisted of either six questions or one question, depending on whether the respondent chose to answer five additional questions. The last question for respondents who elected not to answer extra questions was always a randomly-selected question about one of the four rumor teasers. There was one restriction on the randomization: to avoid deceiving our subjects, respondents who were assigned a rumor teaser and chose not to answer a question were ineligible to answer that same question as their final question.² For respondents who chose to answer five extra questions, the last six questions were (1) the teaser question, (2-4) three additional rumor questions, and (5-6) two debrief questions about why they chose to answer more questions.³ The two debrief items asked respondents, “Why did you want to answer five extra questions? Please write a sentence or two.”, and, “Which of the following are reasons why you wanted to answer five more questions? Please select all that apply.” As these items are (a) self-reports and (b) are asked only of respondents who chose to answer more questions, we use them only as suggestive evidence of our respondents’ motivations for answering questions.

The essential feature of the third section is this: because all subjects answered at least one randomly selected rumor question, we can examine differences in responses between those who did and did not choose to answer extra questions. The random assignment of the teaser questions provides an exogenous source of variation: though the difference between respondents who did and did not choose to answer questions is observational, the randomly-assigned rumor questions may have a causal effect on the nature and extent of

²Due to a programming error, respondents assigned to the “Trump-Russia” teaser were eligible to answer this same question. Consequently, 60 respondents who said they did not want to answer this question nonetheless had to answer it.

³This means that respondents who saw a rumor teaser answered all four rumor questions (1-4), while those who saw a neutral or partisan teaser answered a randomly-selected three of the four rumor question (2-4).

these compositional differences.⁴

The survey concluded at this time. We did not debrief subjects because the survey did not include any deception.

Details of Assigned Treatments

The first manipulation was the audience for the extra questions. The two conditions, assigned with equal probability, were “Test Survey” and “National Poll”. The “National Poll” condition is designed to enhance the perceptions that the survey results will be communicated to a third party, potentially increasing expressive value by letting the respondent know that the results would be communicated to others. By contrast, the “Test Survey” is ambiguous as to audience and implies the results are for our own research purposes, potentially reducing expressive rewards.

The second manipulation determined the sample survey item we displayed to “tease” the content of the additional questions. These *teaser questions* were randomly selected from eight different items, assigned with equal probability, which we grouped ex ante into three categories. Our baseline consisted of two *neutral* teasers that lack obvious ties to partisanship:

- *GSA administrator approval*: Do you approve or disapprove of [Emily Webster Murphy / Denise Turner Roth]’s performance as administrator of the General Services Administration?
- *Policy (hard)*: Carried interest income, which is the income accruing to the general partner of a private investment fund from return on assets managed by the fund, can currently be treated as capital gains income for tax purposes.

Do you support or oppose treating carried interest as ordinary wage and salary income?

Because most individuals know little about the GSA or its administrator, we viewed the GSA item as unlikely to attract individuals interested in communicating a partisan sentiment. We viewed the second as neutral because it contained no party cues or explanation of who benefits from the current tax condition.

⁴This assumption is valid if the teaser, combined with the decision to answer more questions, does not also affect responses to the rumor items.

At the other extreme, the two *partisan* teasers were designed to be easily mapped onto contemporary party divisions:

- *Policy (easy)*: Carried interest earnings, which go overwhelmingly to wealthy hedge fund managers, are currently taxed at the capital gains rate of 24%. Congressional Democrats have proposed to treat this income like ordinary wage and salary income, which would increase the tax rate to 37%

Do you support or oppose treating carried interest as ordinary wage and salary income?

- *Presidential approval*: [Do / Did] you approve or disapprove of [Donald Trump / Barack Obama]’s performance as President of the United States?

Endorsing the president of one’s own party and criticizing the performance of an out-party president are self-evidently connected to partisanship. “Policy (easy)” was similar to “policy (hard)”, but featured language about the intended targets of the measure (wealthy hedge fund managers) and party divisions.

Finally, our *rumor* teasers served a dual purpose in our design: they were used as both teaser questions *and* as the rumor questions that were asked after the randomly-assigned teaser. We sought to understand whether these items were more similar to the neutral or partisan teasers in the types of respondents induced to answer more questions. The rumor teasers were:

- *Bush 9/11*: True or false? Members of George W. Bush’s administration either assisted in the September 11, 2001 attacks or took no action to stop the attacks because they wanted the United States to go to war in the Middle East.
- *Trump-Russia collusion*: True or false? Donald Trump personally colluded with Russia to influence the 2016 election.
- *Obama Muslim*: True or false? Barack Obama is a Muslim.
- *Obama spied on Trump*: True or false? As president, Barack Obama ordered the FBI to spy on Donald Trump’s campaign.

Two of the items, Bush 9/11 and Obama Muslim, have been studied in prior work on partisan rumors (Berinsky 2018; Krosnick et al. 2014). We selected the other two based on our observations of salient rumors in contemporary politics. In a pre-test of items for this and other work, we found substantially larger partisan gaps on the Obama Muslim item

than the Bush 9/11 item, suggesting that over time, the latter has lost some of its bite as a partisan-tinged controversy. The other two items relate to more recent controversies. The Trump-Russia item draws on an exaggeration of the truth rooted in speculation by left-leaning pundits. Though there is ample evidence of Russian efforts to influence the 2016 presidential election, there is no extant evidence that Trump personally worked with Russia. At the same time, endorsing (or refuting) this item provides a general chance to express an attitude about the legitimacy of Trump’s electoral victory. The Obama spied on Trump item originated in a 2017 (post-election) claim that then-President Obama had directed the FBI to spy on Trump’s campaign to advantage the Democratic candidate, Hillary Clinton.

Results

Do Respondents Want to Answer Extra Questions?

The first set of results examines how many of our respondents viewed the prospect of answering five extra survey questions as a benefit rather than a cost. Pooling across the eight teaser questions, 64 percent of respondents chose to answer an additional five questions rather than going straight to the last question (Table 1).⁵ For almost two-thirds of our respondents, the benefit of answering additional survey questions outweighed the cost.

The benefit of answering additional survey questions is partly a function of the expected content of those questions. Among the eight teaser questions, our respondents appeared to expect the most benefit from answering the question about presidential approval. When presidential approval was “teased,” just more than three-quarters (77 percent) of our respondents chose to answer five additional questions. Our respondents expected the least benefit from answering a similarly-worded approval question about Denise Roth Turner or

⁵Included among the 1,366 respondents we classify as refusing to answer more questions are 21 respondents who left the survey entirely at this point (0.6 percent). We detect some differences across treatments ($\chi^2 = 35.1, p < 0.001$). Seven of the 21 were assigned to policy (easy), and another four to policy (hard); at least one respondent in each treatment arm left the survey. In all of our analysis of post-treatment responses, we treat these respondents as being missing at random.

Table 1: Percentage of respondents choosing to answer five more questions.

Category	Teaser	All Respondents	Political Party		
			Democrat	Republican	Independent
Pooled	Pooled	0.64 (0.63, 0.66)	0.65 (0.62, 0.67)	0.66 (0.64, 0.69)	0.58 (0.54, 0.62)
Neutral	GSA Admin. approval	0.51 (0.47, 0.56)	0.49 (0.43, 0.56)	0.53 (0.45, 0.60)	0.55 (0.44, 0.66)
	Policy (hard)	0.59 (0.55, 0.64)	0.61 (0.55, 0.67)	0.60 (0.53, 0.68)	0.49 (0.37, 0.62)
Partisan	Policy (easy)	0.62 (0.58, 0.67)	0.62 (0.55, 0.68)	0.65 (0.58, 0.72)	0.58 (0.48, 0.69)
	President approval	0.77 (0.73, 0.80)	0.78 (0.72, 0.83)	0.78 (0.72, 0.84)	0.69 (0.59, 0.80)
Rumor	Bush 9/11	0.58 (0.53, 0.62)	0.55 (0.49, 0.61)	0.62 (0.55, 0.70)	0.55 (0.42, 0.67)
	Trump-Russia collusion	0.67 (0.63, 0.71)	0.71 (0.65, 0.77)	0.68 (0.60, 0.75)	0.55 (0.45, 0.66)
	Obama Muslim	0.70 (0.66, 0.74)	0.72 (0.66, 0.78)	0.70 (0.63, 0.77)	0.65 (0.54, 0.75)
	Obama spied on Trump	0.70 (0.66, 0.74)	0.73 (0.67, 0.79)	0.72 (0.65, 0.78)	0.57 (0.45, 0.69)

Emily Webster Murphy, the administrators of the General Services Administration under Obama and Trump, respectively. Even when teased with the opportunity to express an opinion of these presumably unknown figures, 51 percent of respondents chose to answer the additional questions. Our other two benchmarks fell between these two. Surprisingly, our respondents were similarly interested in answering a policy question about taxes on carried interest when it was framed technically (“policy (hard)”) and when it had an accessible framing featuring partisan cues (“policy (easy)”).

These questions benchmark our respondents’ interest in answering rumor questions. Three of the politically-salient rumors attracted similar levels of answering additional questions: when teased with the opportunity to say whether “Donald Trump personally colluded with Russia to influence the 2016 election,” whether “Barack Obama is a Muslim,” or whether “[a]s president, Barack Obama ordered the FBI to spy on Donald Trump’s campaign,” just over two-thirds of our respondents chose to answer the additional five questions. The fourth rumor appears to have lost some of its bite. When teased with the opportunity

to say whether “Members of George W. Bush’s administration either assisted in the September 11, 2001 attacks or took no action to stop the attacks because they wanted the United States to go to war in the Middle East,” 58 percent of our respondents chose to answer additional questions — closer to GSA administrator approval and policy “hard” than to the other rumors or to presidential approval.

We interpret these results to mean that in the aggregate, people expect to derive more benefit from answering questions concerning rumors about politically salient figures than they expect to derive from answering many other types of questions. This benefit falls short of the expected benefit of stating one’s opinion of the president, but exceeds the benefit of answering policy questions, questions about stale rumors, or questions about obscure political figures.

Who Wants to Answer Extra Questions?

To understand how the benefits of answering survey questions vary within the population, we collected pre-treatment information about our respondents’ demographic and political characteristics. Among the non-political background characteristics, age is the clearest predictor of answering additional questions. Among respondents between the ages of 18 and 29, about 57 percent chose to answer additional questions (Table 2). This percentage rises steadily to 70 percent among respondents who are 70 years or older. Little heterogeneity emerged across levels of education, gender, household income, or race and ethnicity.

Turning to political attitudes and characteristics, the strongest predictor is political interest. About 50 percent of those “not at all interested” in politics chose to answer more questions, compared with 63 percent of the “somewhat interested” and 72 percent of the “very interested.” Self-reported voter turnout also predicted the desire to answer more questions. About 69 percent of those who reported voting in 2018 chose to answer more questions, compared with 57 percent of those who said that they did not vote.

Another set of predictors examined partisanship. Using the seven-point partisan iden-

Table 2: Percent choosing to answer more questions by respondent characteristic.

Category	Sample size	Estimate	Category	Sample size	Estimate
Age			Party (7-point)		
18-29	724 (19.0%)	56.8 (53.2, 60.4)	Strong R	912 (23.9%)	67.9 (64.8, 70.9)
30-39	715 (18.8%)	63.9 (60.4, 67.4)	Not strong R	573 (15.0%)	57.6 (53.5, 61.7)
40-49	690 (18.1%)	63.9 (60.3, 67.5)	Lean R	353 (9.3%)	67.7 (62.8, 72.6)
50-59	655 (17.2%)	65.3 (61.7, 69.0)	Independent	609 (16.0%)	58.1 (54.2, 62.1)
60-69	615 (16.1%)	68.5 (64.8, 72.1)	Lean D	261 (6.8%)	67.4 (61.7, 73.2)
70+	414 (10.9%)	69.8 (65.4, 74.2)	Not strong D	410 (10.8%)	63.2 (58.5, 67.9)
Education			Strong D	695 (18.2%)	67.6 (64.1, 71.1)
<HS	107 (2.8%)	73.8 (65.4, 82.3)	Party (folded)		
HS	1122 (29.4%)	63.8 (61.0, 66.6)	Indep	609 (16.0%)	58.1 (54.2, 62.1)
Some college	849 (22.3%)	65.7 (62.5, 68.9)	Lean	614 (16.1%)	67.6 (63.9, 71.3)
Associate	331 (8.7%)	64.7 (59.5, 69.8)	Weak	983 (25.8%)	59.9 (56.8, 63.0)
BA/BS	981 (25.7%)	62.3 (59.2, 65.3)	Strong	1607 (42.1%)	67.8 (65.5, 70.1)
Graduate	410 (10.8%)	63.7 (59.0, 68.3)	Party's policies*		
Missing	13 (0.3%)	61.5 (30.9, 92.1)	Quintile 1	832 (26.2%)	61.3 (58.0, 64.6)
Gender			Quintile 2	394 (12.4%)	61.9 (57.1, 66.7)
Female	1949 (51.1%)	65.1 (63.0, 67.2)	Quintile 3	1100 (34.7%)	66.4 (63.6, 69.2)
Male	1864 (48.9%)	63.2 (61.0, 65.4)	Quintile 4	403 (12.7%)	70.2 (65.7, 74.7)
Household income			Quintile 5	442 (13.9%)	69.0 (64.7, 73.3)
0 to 25k	1287 (33.8%)	64.7 (62.1, 67.3)	Party social ID*		
25 to 50k	1101 (28.9%)	64.6 (61.7, 67.4)	Quintile 1	540 (17.1%)	68.1 (64.2, 72.1)
50 to 75k	646 (16.9%)	61.8 (58.0, 65.5)	Quintile 2	420 (13.3%)	62.9 (58.2, 67.5)
75 to 100k	344 (9.0%)	66.9 (61.9, 71.9)	Quintile 3	735 (23.2%)	60.8 (57.3, 64.4)
100k+	405 (10.6%)	62.5 (57.7, 67.2)	Quintile 4	791 (25.0%)	66.0 (62.7, 69.3)
Missing	30 (0.8%)	70.0 (52.6, 87.4)	Quintile 5	677 (21.4%)	68.4 (64.9, 71.9)
Race and ethnicity			Obama favorability		
Asian	230 (6.0%)	61.7 (55.4, 68.1)	Very unfav.	735 (19.3%)	67.9 (64.5, 71.3)
Black	418 (11.0%)	66.5 (62.0, 71.1)	Unfavorable	584 (15.3%)	61.6 (57.7, 65.6)
Hispanic	456 (12.0%)	66.7 (62.3, 71.0)	Favorable	1121 (29.4%)	60.0 (57.2, 62.9)
Other	126 (3.3%)	65.1 (56.6, 73.5)	Very fav.	1368 (35.9%)	66.7 (64.2, 69.2)
White	2575 (67.7%)	63.4 (61.6, 65.3)	Trump favorability		
Interest in politics			Very unfav.	1700 (44.8%)	65.1 (62.8, 67.3)
Not at all	315 (8.3%)	50.2 (44.6, 55.7)	Unfavorable	600 (15.8%)	58.3 (54.4, 62.3)
Not very	574 (15.1%)	55.4 (51.3, 59.5)	Favorable	782 (20.6%)	65.0 (61.6, 68.3)
Somewhat	1635 (42.9%)	63.5 (61.2, 65.9)	Very fav.	716 (18.9%)	66.5 (63.0, 69.9)
Very	1289 (33.8%)	72.3 (69.9, 74.8)	Secret cabal		
Voted 2018			Strong dis.	194 (5.1%)	68.0 (61.4, 74.7)
Yes	2297 (60.3%)	68.7 (66.8, 70.6)	Disagree	481 (12.6%)	69.0 (64.9, 73.2)
No	1511 (39.7%)	57.2 (54.7, 59.7)	Neither	894 (23.5%)	56.5 (53.2, 59.7)
			Agree	1550 (40.7%)	64.1 (61.7, 66.5)
			Strong agree	692 (18.2%)	69.7 (66.2, 73.1)

Note: Pooling across all teaser questions, the “estimate” column presents the percentage of respondents who chose to answer five additional questions, with a 95 percent confidence interval in parentheses. The “sample size” column lists the number of respondents, with their percentage of the sample in parentheses. Variables marked with a * exclude pure independents (i.e., those who do not lean toward a party). These two variables are not split into perfectly even quintiles because their distributions are discrete.

tity scale, 58 percent of independents chose to answer more questions, compared with 68 percent of strong partisans. This pattern is largely symmetric across parties. Measured according to the folded partisanship scale, the relationship between strength of partisanship and the desire to answer questions is not linear: 68 percent of independents who lean toward a party chose to answer more questions, compared with 60 percent of those who chose a party but did not strongly prefer it.

For a closer look at partisanship, we asked respondents who indicated a partisan identity to place themselves on two more scales. The two-item policy similarity scale separately asked how similar the party’s social and economic policies were to the respondent’s ideal policies, while the partisan social identity scale used three statements from [Greene \(1999\)](#) and [Huddy et al. \(2015\)](#). The policy scale’s relationship to the desire to answer questions was fairly linear: 61 percent of respondents in the lowest quintile chose to answer more questions, compared with 69 percent in the highest quintile. The partisan identity scale was closer to U-shaped: 68 percent of both the lowest and highest quintiles chose to answer more questions, compared with 61 percent in the middle.

The relationships between measures of partisanship and the desire to answer more questions defied the expectations we had extrapolated from the literature. Accounts of expressive responding frame the act of stating an insincere, party-congenial response as “partisan cheerleading” ([Bullock et al. 2015](#)), a reference accounts of partisan affiliation as an emotional attachments or social identity (e.g., [Green et al. 2002](#)). Yet in our results, the measure of partisanship with the least connection to these accounts — the policy similarity scale — was the most predictive of the desire to answer more questions. This is consistent with recent research demonstrating that measured partisan animosity originates in policy differences rather than pure “teamism” ([Orr and Huber 2020](#)).

The remaining political predictors that relate to partisanship are favorability toward Obama and Trump. Both relationships are roughly U-shaped: people with more intense attitudes toward presidents are more interested in answering more questions. About two-

thirds of respondents with a very favorable or very unfavorable view of Obama chose to answer additional questions, compared with 60 percent of those who chose one a middle option (favorable or unfavorable). A less pronounced version of this pattern appears using ratings of Trump.

Heterogeneity by Teaser Question: An Illustrative Example

Before examining the final predictor, it is useful to introduce the next phase of our analysis, which examines heterogeneity according to the type of teaser question. We expected not just that different types of people are more and less interested in answering questions, but that those patterns would covary with question content. To introduce this portion of the analysis, we focus on one respondent characteristic. Below, we apply this same approach to the other variables listed in Table 2.

The question most intentionally selected for this portion of the analysis was the “secret cabal” question, which [Oliver and Wood \(2014\)](#) found to be a strong predictor of conspiracy endorsement. The question asks respondents the extent to which they agree or disagree with the statement, “Much of what happens in the world today is decided by a small and secretive group of individuals.” Our expectation was that an attitude that predicts the willingness to endorse conspiracies would also be more predictive of the desire to answer rumor questions than of the desire to answer other questions.

The results departed from this expectation. Overall, the secret cabal question produced a U-shaped pattern reminiscent of that seen on the partisan social identity scale and presidential approval questions: a hair over two-thirds of those in the most extreme categories chose to answer more questions, compared with 57 percent of those who took no position (Table 2). To examine how this relationship varies across questions, the online appendix plots the same information presented in Table 2 separately for each of the eight teaser questions (Figure A2, bottom row). The separate patterns for each teaser question look quite similar to the overall relationship: people with strong opinions about the secret

cabal statement want to answer more questions, while those with less strong opinions are less interested.

For a more formal test of differences across teaser questions, we used OLS regression to examine heterogeneity in the linear relationship between the political characteristics in Table 2 and the desire to answer more questions. Our estimate of the linear relationship is β_1 from

$$\text{More Qs}_i = \beta_0 + \beta_1 x_i + \epsilon_i \quad (1)$$

where x_i is the covariate of interest.⁶

We are interested in whether our estimate of the linear relationship, β_1 , varies across teaser questions. To test for such heterogeneity, we used OLS to fit two separate linear models that pool across teaser questions, then use an F test to compare their goodness of fit. The models are

$$\text{More Qs}_i = \sum_{k=1}^K \alpha_k \text{Teaser Q}_{ik} + \beta_1 x_i + \epsilon_i \quad (2)$$

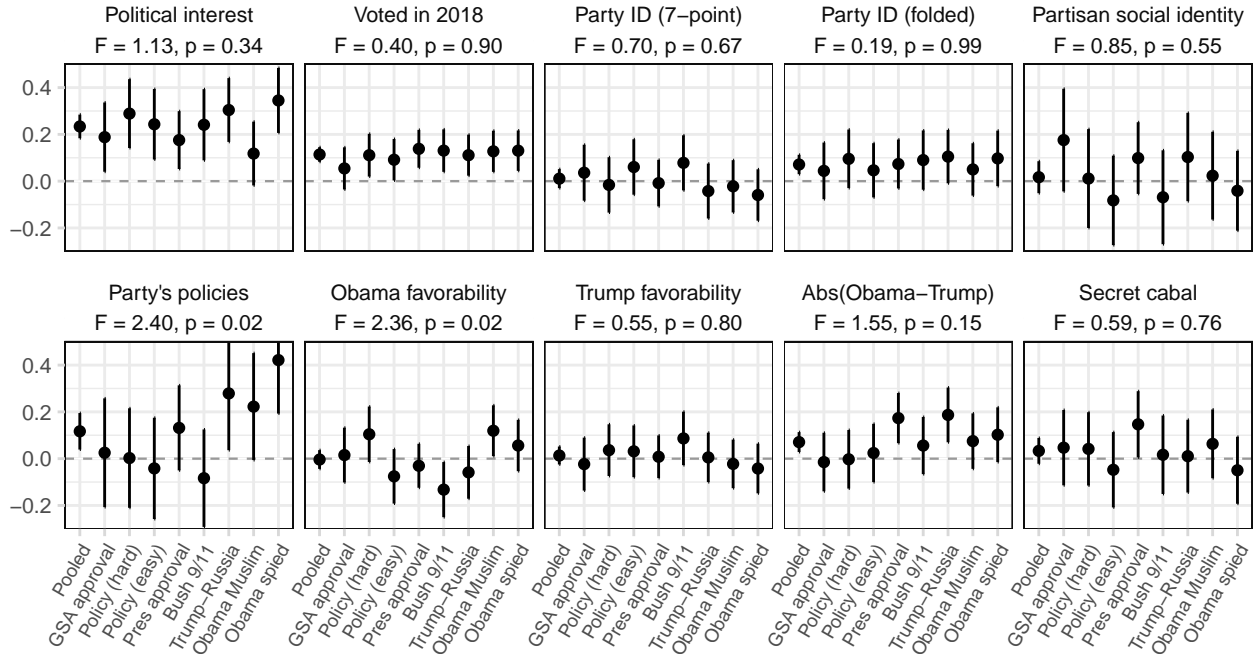
$$\text{More Qs}_i = \sum_{k=1}^K \alpha_k \text{Teaser Q}_{ik} + \beta_1 x_i + \sum_{k=2}^K \beta_k x_i \times \text{Teaser Q}_{ik} + \epsilon_i \quad (3)$$

where x_i is the covariate, k indexes the eight teaser questions, and the Teaser Q_{ik} are indicators for the teaser questions. The restricted model (2) has a separate mean for each teaser question but requires the slope to be the same. By adding the δ_k , the unrestricted model (3) allows the slope to vary. An F test for the difference between these asks, “does the linear relationship between the secret cabal question and saying “yes” to more questions depend on what question has been teased?”

In the case of the secret cabal question, this test finds no evidence of heterogeneity

⁶Note that this does not test for the U-shaped relationships described above. To us, the most obvious interpretation of U-shaped relationships is a generic relationship between stronger opinions and a desire to answer questions.

Figure 1: Regression test: political predictors of choosing to answer more questions.



Note: Estimates are β_1 from equation (1). Each dot is a coefficient estimate. Whiskers represent 95 percent confidence intervals based on robust standard errors. At the top of the figure, the F statistic is a comparison between the pooled estimate (the rightmost dot in each facet) and a model that allows the slope to vary across questions (as it does in the leftmost eight dots in each facet). See text for a discussion of the F test.

across teaser questions. Figure 1 presents nine estimates of β_1 from equation (1): one that pools across all eight teasers and eight for each of the individual teasers. Above each facet is the result of the F test for the difference between equations (2) and (3). We find no statistical evidence of heterogeneity in the linear relationship between the secret cabal item and the desire to answer additional questions ($F = 0.59, p = 0.76$).

Heterogeneity by Teaser Question: Further Analysis

We now apply the same approach to examine heterogeneity across teaser questions for the other respondent characteristics listed in Table 2. For each of the non-political variables, the online appendix plots the same estimates as Table 2, separated by teaser question (Figures A1 and A2). Overall, we see little evidence of substantial heterogeneity in the extent to which non-political demographics predict the desire to answer additional questions. The

lone exception is age, which was the strongest demographic predictor of overall desire to answer additional questions. Younger peoples' lower desire to answer questions is especially pronounced for the Bush 9/11 rumor and the policy (easy) question. This accords with our intuitions. Most people who were under the age of 29 at the time of the survey were age ten or younger on September 11, 2001. These individuals are also less likely than older people to have substantial financial investments or tax burdens.

For several of the political characteristics, the most striking pattern is the lack of heterogeneity across teaser questions. Net of the overall average difference in desire to answer more questions after each teaser, there is little difference in the extent to which political interest, self-reported 2018 voter turnout, the 7-point party identity scale, or folded party identity predict the desire to answer questions. In the aggregate, interest in answering questions is higher among those who are more interested in politics, said they voted, or are stronger partisans — but these relationships do not vary much across questions. The top row of Figure 1 shows that in all four of these cases, the F test finds no evidence of demographic heterogeneity.

Turning again to our two alternative measures of strength of partisan preference, we find greater heterogeneity according to perceived policy closeness than according to partisan social identity. Relative to the other teaser questions, policy proximity to one's party is more predictive of interest in answering the three rumor questions that concern Obama and Trump — and, to a lesser extent, of interest in answering a presidential approval question. The F test detects heterogeneity in these linear relationships ($F = 2.40$, $p = 0.02$). By contrast, the relationships between the partisan social identity scale and the desire to answer additional question largely maintains its U-shaped character across teaser questions. We cannot think of a reason to interpret this U-shaped relationship as theoretically significant, outside the aforementioned generic relationship between stronger attitudes and greater interest in answering questions.

Our measures of presidential favorability suggest that among Obama's supporters,

the overall U-shaped relationship between favorability and the desire to answer questions may obscure an impulse among Obama’s supporters to jump to his defense. Respondents who approve more strongly of Obama appear particularly interested in rejecting the notion that Obama is a Muslim, and particularly uninterested in weighing in on the Bush 9/11 rumor. The other two rumor questions suggest more muted versions of this pattern that fail to attain statistical significance: slightly more interest in answering the question about whether Obama ordered the FBI to spy on Trump, and slightly less interested in weighing in on whether Trump personally colluded with Russia. The literature on incorrect answers to factual survey questions tends to emphasize partisan motivations to *accept* a rumor. Here, we have suggestive evidence of a partisan impulse to reject rumors. Our readiness to fully accept this conclusion is somewhat tempered by the lack of a similar relationship with the Trump favorability measure. However, the Obama finding raises an important, unexplored question: do people experience expressive motivation to *reject* unfavorable or false claims about their favorite politicians?

In contrast to some of the other U-shaped relationships we observe between respondent characteristics and the desire to answer questions, we see a clear reason to unpack this pattern as it relates to favorability toward presidents: it may be that people with the most intense attitudes toward political figures are most interested in answering rumor questions about them. To examine this, we used the absolute value of the difference between Obama and Trump’s favorability ratings. Using this measure, we find stronger evidence of heterogeneity than with most of our other variables, but insufficient to reject the null hypothesis of no heterogeneity in the F test ($F = 1.55$, $p = 0.15$). In Figure 1, the linear relationship between the Obama/Trump contrast and the desire to answer questions is strongest on the questions that concern Obama and Trump and almost exactly zero on GSA approval and policy. Placing this result in contrast to the relative lack of heterogeneity on more conventional measures of partisanship (partisan identity and the partisan social identity scale) suggests another tentative conclusion: it may be that attitude strength toward the attitude object

(Obama and Trump), as opposed to generalized support for a party, is more predictive of the expressive benefits of answering rumor questions.

What Explains the Choice to Answer Extra Questions?

The results so far have suggested that people who choose to answer additional survey questions tend to be stronger partisans who are more interested in politics — but that with a few exceptions, these relationships do not vary much based on the expected content of the additional questions. What, then, explains the desire to answer additional questions? Beyond what we can glean from correlations with respondent characteristics, the survey included two features designed to shed light on this question. Both features were designed to speak to the relative extent to which respondents derive benefit from internal and external motivations.

The first look at the role of internal versus external motivations comes from the other randomized element of our survey. In addition to randomizing the teaser question, we randomly assigned whether respondents were told that the five extra questions were part of a test survey or a national poll. To estimate the average effect of this treatment, we used OLS to estimate

$$\text{More Qs}_i = \beta_0 + \beta_1 \text{National Poll}_i + \epsilon_i, \tag{4}$$

where β_1 is the difference between the two treatments in the percentage of respondents who wanted to answer more questions. We estimated β_1 for our entire dataset as well as several subsets of the data. We computed separate estimates for the three categories listed in Table 1: neutral teasers, partisan teasers, and rumor teasers. We also computed separate estimates for each level of three of the variables listed in Table 2: interest in politics, folded party identification, and respondent partisanship (with leaners classified as partisans).

Pooling across all teaser questions, we estimate that the effect of this manipulation was almost exactly zero ($\hat{\beta}_1 = -0.006$, s.e.= 0.016). The online appendix presents separate

estimates by teaser question category and demographic characteristic; in most cases, we find little evidence of a difference in means (Figure A3). Despite the large number of coefficient estimates, we wish to report one pattern that is deserving of further investigation. Among respondents who were randomly assigned to see a rumor teaser, there appears to be heterogeneity by political interest: the greater the respondent’s stated interest in politics, the larger effect the audience manipulation had on the desire to answer a rumor question.⁷

Together, the results from this experiment suggest that whatever benefits respondents derive from answering survey questions, the potential to broadcast one’s views to a wide audience may not be a major component. It is also possible, however, that our manipulation may have been too subtle, in that respondents may have (correctly) intuited that as academic researchers we would be analyzing their attitudes either way.

For a second look at motivations for answering additional questions, we turn to self-reports from respondents who chose to answer additional questions. As a reminder, these items appeared at the end of the post-treatment battery. Focusing on the closed format item, we found little heterogeneity across the eight teaser questions in the percentage of respondents who selected each statement (see the online appendix, Figure A4). The most important pattern is the relative unpopularity of the statements we designed to capture the desire to broadcast one’s views to a larger audience. These two statements, “I wanted to stand up for what I believe,” and “I wanted other people to know what I believe,” were respectively selected by just 26 and 19 percent of respondents, only coming in ahead of “I was bored or didn’t have anything else to do” (9 percent). The statements that were designed to tap internal motivations for enjoying expressing one’s views — “I like taking surveys in general” (37 percent) and “I like answering questions like the ones you showed me” (33 percent) — were each more popular, as were the statements that were designed to tap instrumental motivations (“I thought I might get extra pay or some other reward” and “I

⁷For a formal test of this relationship, we subsetting our data to respondents assigned to see a rumor teaser, then used OLS to estimate $\text{More Qs}_i = \beta_0 + \beta_1 \text{National Poll}_i + \beta_2 \text{Interest}_i + \beta_3 \text{National Poll}_i \times \text{Interest}_i + \epsilon_i$. We estimate $\hat{\beta}_3 = 0.150$ (s.e. = 0.072).

was trying to be helpful”, each with 34 percent). Though the difference between internal and external motivations is not enormous — the largest gap between any two of these statements is 18 percentage points — they suggest that at least as far as the respondents are concerned, the internal and instrumental benefits of answering questions outweigh the external benefits. The remaining statement was the most popular statement (“I was curious about what you would ask”, 64 percent).

Perhaps it is not surprising that we do not find large differences here, as most people chose to answer more items across all of the teasers. We revisit this design consideration below.

Do People Who Choose to Answer More Questions Respond Differently?

We now turn to another potentially distinguishing characteristic of respondents who choose to answer more questions: do they provide different answers than those who choose not to answer extra questions? As we note above, our design facilitates comparisons between respondents who did and did not choose to answer more questions by asking *all* respondents to answer at least one rumor question, including respondents who chose not to answer the extra five questions. This allows us to compare the responses of people who did and did not choose to answer more questions, and to examine how these patterns vary by teaser question.

The design resulted in a substantial imbalance in the number of observations in each category for this analysis. Recall from above that (1) regardless of the teaser question, less than half of respondents refused to answer additional questions and (2) respondents who chose not to answer extra questions answered only one rumor question, while respondents who chose to answer extra questions answered either three or four of them. This means that we have many fewer observations for people who did not want to answer questions than for people who did want to answer questions.⁸ Statistically speaking, the consequence is that

⁸In total, we have 1,340 answers to rumor questions from respondents who chose not to answer extra questions and 8,860 from respondents who did choose to answer more questions. This works out to about 112 respondents in each of the 12 cells of respondents who chose not to answer more questions (four rumor questions \times three categories of teasers), compared with 738 responses per cell for respondents who did choose

subjects were not optimally sorted to maximize our ability to detect differences between the groups (Gerber and Green 2012). We return to this issue below in our discussion of potential refinements to our design.

We use these data to answer two empirical questions. First, do people who choose to answer additional questions provide more partisan responses to rumor questions than those who do not? Second, relative to other types of teaser questions, is the desire to answer a rumor question more predictive of partisan response differences? The first question is purely descriptive, based on our observation of whether people want to answer more questions and their subsequent responses to questions about rumors. The second question is causal: relative to other teasers, what effect do rumor teasers have on the response differences between these two groups?

To answer these questions, we turn to linear regression. To avoid the interpretive complexity introduced by three-way interaction terms, we code our dependent variable, Y_i , to be the partisan congeniality of respondent i 's response to the rumor question. Consider the rumor that President Trump personally colluded with Russian agents to influence the 2016 election, which is more likely to be endorsed by Democrats. On this question, we code each Democrat's response of "true" as 1, "false" as 0, and "not sure" as 0.5; for Republicans, "true" is 0, "false" is 1, and "not sure" is 0.5. We applied this same coding to the Bush 9/11 rumor. For the Obama Muslim and Obama spied on Trump rumors, we reverse this coding for both parties.

Using this dependent variable, we used OLS to estimate

$$Y_i = \alpha_D + \alpha_R \text{ Republican}_i + \beta_1 \text{ Partisan Teaser}_i + \beta_2 \text{ Rumor Teaser}_i + \beta_3 \text{ More Qs}_i + \beta_4 \text{ More Qs}_i \times \text{ Partisan Teaser}_i + \beta_5 \text{ More Qs}_i \times \text{ Rumor Teaser}_i + \epsilon_i \quad (5)$$

where More Qs_i is an indicator variable for choosing to answer more questions, Partisan

to answer more questions.

Teaser_{*i*} is an indicator for assignment to a partisan teaser, and Rumor Teaser_{*i*} is the equivalent for rumor teasers. We also fit a pooled version of this model that combines across all four rumor questions, with party-specific intercepts for each rumor question and type of teaser question.

We interpret these coefficients as follows. We emphasize that the dependent variable is the congeniality of the response to a rumor question, even for respondents who were teased with a neutral or partisan question.

β_1 : difference between respondents who said “no” to a neutral teaser and respondents who said “no” to a rumor teaser. If respondents who reject rumor teasers give less partisan responses to rumor questions, β_1 should be negative.

β_2 : difference between respondents who said “no” to a neutral teaser and respondents who said “no” to a partisan teaser.

β_3 : difference in response congeniality between those who said “no” to a neutral teaser and those who said “yes” to a neutral teaser. If respondents who said “yes” give more partisan responses to rumor questions, β_3 should be positive.

β_4 : difference in differences between neutral teasers and rumor teasers. If respondents who said “yes” to rumor teasers give more partisan responses than those who said “yes” to neutral teasers, β_4 should be positive.

β_5 : difference in differences between neutral teasers and partisan teasers.

The coefficients that describe neutral teasers (β_0 and β_3) have a purely descriptive interpretation: they simply describe the characteristics of people who did and did not choose to answer extra questions after seeing a neutral teaser. The other coefficients estimate the causal effect of a different teaser question on these differences: relative to the neutral baseline, how do different types of teaser questions change the composition of the groups that do and do not choose to answer extra questions, as measured by their responses?

Table 3 presents the results. Focusing on the pooled estimates in column (5), we see that relative to neutral teasers, rumor teasers generate larger differences in response congeniality between people who do and do not choose to answer extra questions. We estimate $\hat{\beta}_2 = 0.58$ (s.e.= 0.27) and $\hat{\beta}_5 = 0.57$ (s.e.= 0.30), meaning that relative to neutral teasers, rumor teasers cause about an 0.06 point larger gap in response congeniality on our

Table 3: Regression Test for Response Differences

	(1)	(2)	(3)	(4)	(5)
	Bush 9/11	Obama Muslim	Obama Spied	Trump Russia	Pooled
Constant	0.465*** (0.048)	0.832*** (0.043)	0.792*** (0.036)	0.791*** (0.039)	0.419*** (0.024)
α_1 Republican	0.278*** (0.019)	-0.245*** (0.018)	-0.246*** (0.017)	-0.002 (0.016)	0.277*** (0.019)
β_1 Rumor Teaser	-0.031 (0.067)	-0.117** (0.056)	0.012 (0.049)	-0.071 (0.049)	-0.058** (0.027)
β_2 Partisan Teaser	0.002 (0.076)	-0.018 (0.072)	0.011 (0.058)	-0.082 (0.063)	-0.022 (0.033)
β_3 More Qs	-0.074 (0.053)	-0.068 (0.048)	0.060 (0.041)	-0.041 (0.044)	-0.027 (0.024)
β_4 More Qs \times Rumor	0.022 (0.072)	0.125** (0.061)	-0.030 (0.054)	0.084 (0.054)	0.057* (0.030)
β_5 More Qs \times Partisan	0.000 (0.081)	0.053 (0.077)	-0.047 (0.064)	0.112* (0.068)	0.030 (0.037)
Adj. R ²	0.090	0.079	0.096	-0.000	0.115
Num. obs.	2113	2171	2167	2219	8670

Note: Estimates correspond to equation (5). The dependent variable is coded [0, 0.5, 1], where 0 is the response least congenial to one’s political party, 0.5 is “not sure,” and 1 is the most congenial response. Independents are excluded. Columns 1-4 correspond to each of the four rumor questions that respondents could answer after choosing whether or not to answer more questions. Column 5 pools across all four questions and includes separate Republican intercepts for each question. HC2 robust standard errors in parentheses. Standard errors are clustered by respondent. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

0-1 scale. The partisan teasers generate a more muted version of this pattern that is not statistically significantly different from either the neutral or the rumor teasers.

The first four columns of Table 3 examine how these relationships vary by teaser question. The Obama Muslim and Trump-Russia rumor questions drive the patterns we focused upon in our analysis of the pooled estimates. We interpret these results to mean that to the tendency among those who want to answer more rumor questions to provide more partisan responses is concentrated within the most politically salient rumors. Whereas we were not surprised to have obtained weaker results for the Bush 9/11 rumor (see above), we expected stronger results for the “Obama Spied” teaser, which is less prominent than the other rumors but involves two high-profile figures on opposite sides of the partisan divide.

Implications

These findings have implications for research designs that have been used to study a range of questions in political science, including expressive responding on rumor questions. Following [Dellavigna et al. \(2017\)](#), one such design varies the costs and benefits attached to response options. With support from self-reported respondent preferences, [Berinsky \(2018\)](#) argues that in his modified balance design, “the people who do not really believe the rumor and will easily forgo expressed support in exchange for an incentive” (218) because “a very small proportion of all respondents would view additional questions as a benefit” (219). In our sample, about two-thirds of respondents appeared to view answering extra questions as a benefit, as revealed by their choice to do so after being “teased” with the same rumor questions. This suggests that we should not expect everyone to be deterred by a longer survey; some may even see it as a benefit. In the case of [Dellavigna et al. \(2017\)](#), 41 percent of non-voters lied about having voted instead of taking a survey that was 8 minutes shorter. For these respondents — already part of the minority that opened the door and agreed to take a survey — taking the longer survey may have offered benefits that they would have had to forego by admitting that they had not voted.

Our attention to a potential difference between internal and external expressive utility has implications for a range of research designs that seek to alter the expressive costs and benefits of different response behaviors. Some sources of expressive utility, like personal pride one takes in being honest or the personal joy one takes in choosing absurd responses, may be internal. Others, like glee or fear at the prospect that someone might learn what one stated, are external. Viewed through this lens, list experiments effectively seek to shrink expressive utility (positive or negative) to zero by mostly anonymizing⁹ the subject’s response by adding noise to it. List experiment designs have been tried in a few cases, and are viewed as a potentially fruitful tool for learning about expressive responding ([Berinsky 2018](#); [Bullock](#)

⁹We say “mostly anonymizing” because subjects who agree with zero or all of the statements still reveal their opinion on the statement of interest. See [Blair and Imai \(2012\)](#).

and Lenz 2019). Yet to the extent that the expressive benefits of answering political questions are internal rather than external, list experiments may be less effective at eliminating the expressive benefits of rumor endorsement than they are at eliminating the expressive costs of expressing beliefs or traits that are socially undesirable or self-incriminating (Dalton et al. 1994; Kalinin 2016; Kuklinski et al. 1997; Matanock and Garcia-Sanchez 2018). In this respect, one potential advantage of treatments that seek to activate accuracy motivation (e.g., incentive payments or honesty encouragement) is that they may outweigh other sources of expressive utility, regardless of whether those other sources are internal or external. By contrast, treatments that seek to *de-activate* particular sources of expressive motivation may be effective in a more limited set of cases. Our results on internal and external motivations point toward an under-studied dimension of expressive motivations in survey taking.

Our results also raise a concern related to survey recruitment and consent practices that advertise the survey’s content. These concerns take on particular importance when individuals are given information about the content of the survey during the course of their choice about whether to participate. For example, a major national polling firm has invited one of the authors to surveys using taglines including “Trump Tweets – What do you think?” and “Alleged chemical attack in Syria – Do you support the U.S. response?”. Institutional Review Boards may introduce similar messages through requirements that consent forms describe the purpose of the research. Our results suggest that the practice of “recruiting on the dependent variable” is likely to affect the observed distribution of responses because they alter the pool of individuals who participate in the first place. This concern is related to but distinct from some other persistent concerns in survey recruitment. One is generalizability: much concern about convenience samples focuses on possible differences between the target population and other population (Best et al. 2001; Mullinix et al. 2016). Our results suggest that recruitment on the dependent variable could distort the distribution of attitudes relative to whatever target population has been chosen, convenience sample or not. Another set of concerns deals with the prospect that respondents may modify their response, either

to please the researcher (“demand effects,” see [Orne 1962](#)) or to avoid revealing socially undesirable beliefs. Recruitment on the dependent variable could have similar effects without any response falsification.

Looking Ahead

This chapter introduced a novel research design for examining the contours of expressive benefits — who experiences these benefits, how the benefits vary with question content, and whether respondents who enjoy answering questions respond differently than those who do not. This design produced a number of novel findings, which we summarized above. This concluding section considers an important issue for future research: what more could be learned from a refined version of this design? In our minds, three key changes could have sharpened the inferences we presented.

First, we recommend that future iterations of this design increase the number of additional questions that respondents must choose to answer. Overall, about two-thirds of our respondents chose to answer additional questions, leaving us with a relatively small group of respondents who chose not to. The gap in the number of observations of the two groups’ responses was even larger, however: all respondents who chose to answer additional questions were asked three or four rumor questions each, while respondents who chose not to answer additional questions were asked only one rumor question. All told, this meant that just 13 percent of our responses to rumor questions (1,340 of 10,200) came from the group that chose not to answer extra questions. Had we asked respondents to answer a larger number of questions, we might have induced a larger proportion of respondents to decline to answer additional questions. This would have made the number of responses more equal between the two groups, without necessarily reducing the total number of responses: fewer people would likely choose to answer extra questions, but each person making this choice would answer more of them. A stronger selection mechanism could also accentuate the differences between those who choose to answer more questions and those who do not.

Second, we recommend reframing the choice as one between answering all X additional questions and answering a random subset of those questions (e.g., answer ten more questions including the teaser, or two randomly selected questions). The key advantage of this change would be to provide a route to observing responses on the same question that a respondent chose not to answer. Our survey was designed to avoid asking respondents the same question they had just refused to answer; allowing this possibility may not cross the line into outright deception, but it would certainly have violated our sense of what it means to treat a survey respondent beneficently. Framing the choice as between a full set of questions and a random subset of the questions would have provided an alternate route around this problem.

Third, we recommend that researchers use stronger versions of our audience manipulation (test survey versus national poll) to study possible sources of expressive utility. We expected that if external motivations were the major driver of expressive benefits, our manipulation would work: though the switch from “test survey” to “national poll” was in some ways subtle, it was also bold, underlined words in a fairly concise statement framing a choice. Having failed to find evidence of an effect, it strikes us that there is no published causal evidence decisively showing that external motivations are an important component of expressive utility. We think the name “expressive” applies regardless of why people enjoy expressing their views, but greater understanding of what about expressing views gives people enjoyment could provide clues as to how to measure expressive tendencies and expunge responses of them.

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Supplemental figures for

The Expressive Value of Answering Survey Questions

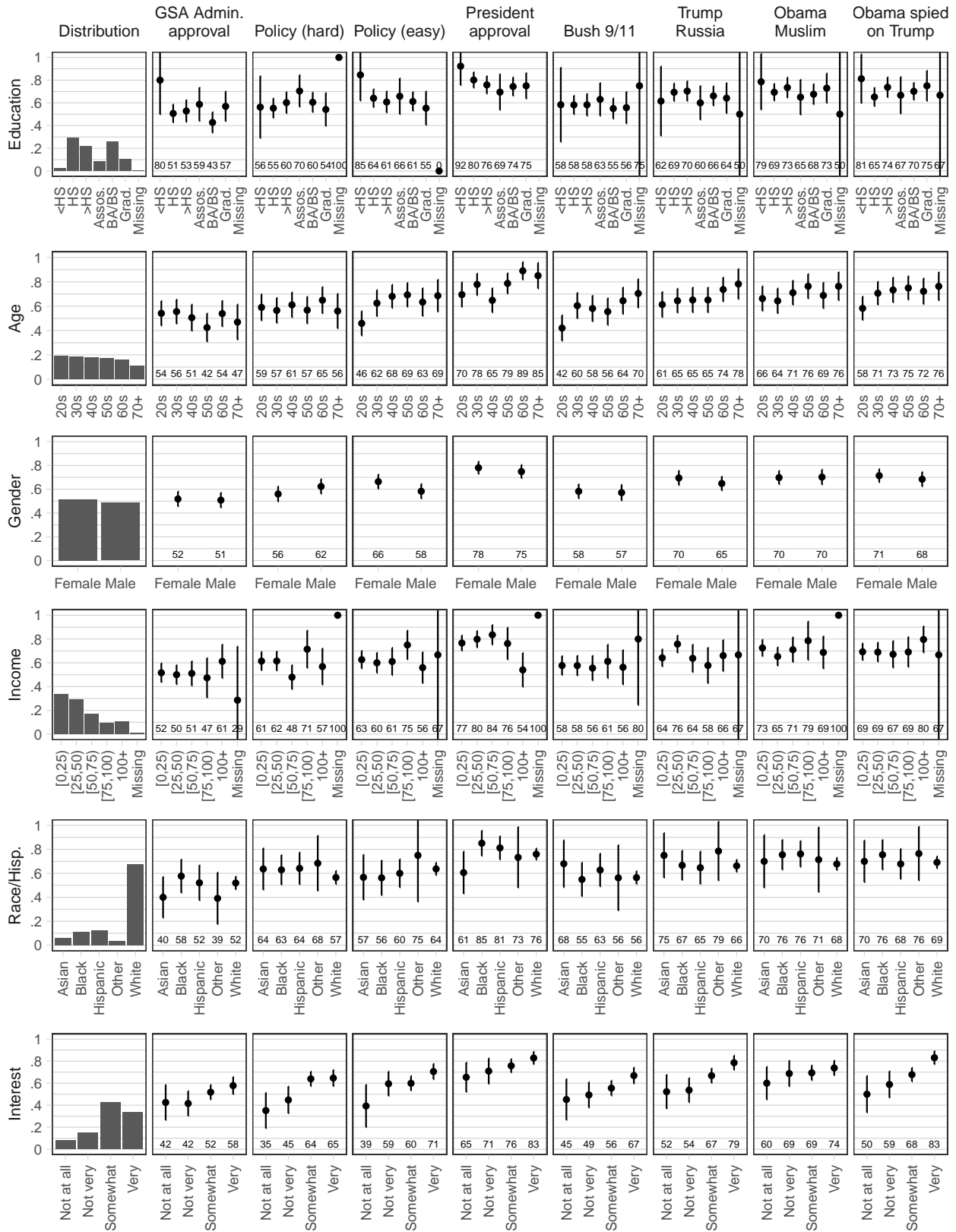
Matthew H. Graham

Gregory A. Huber

April 3, 2020

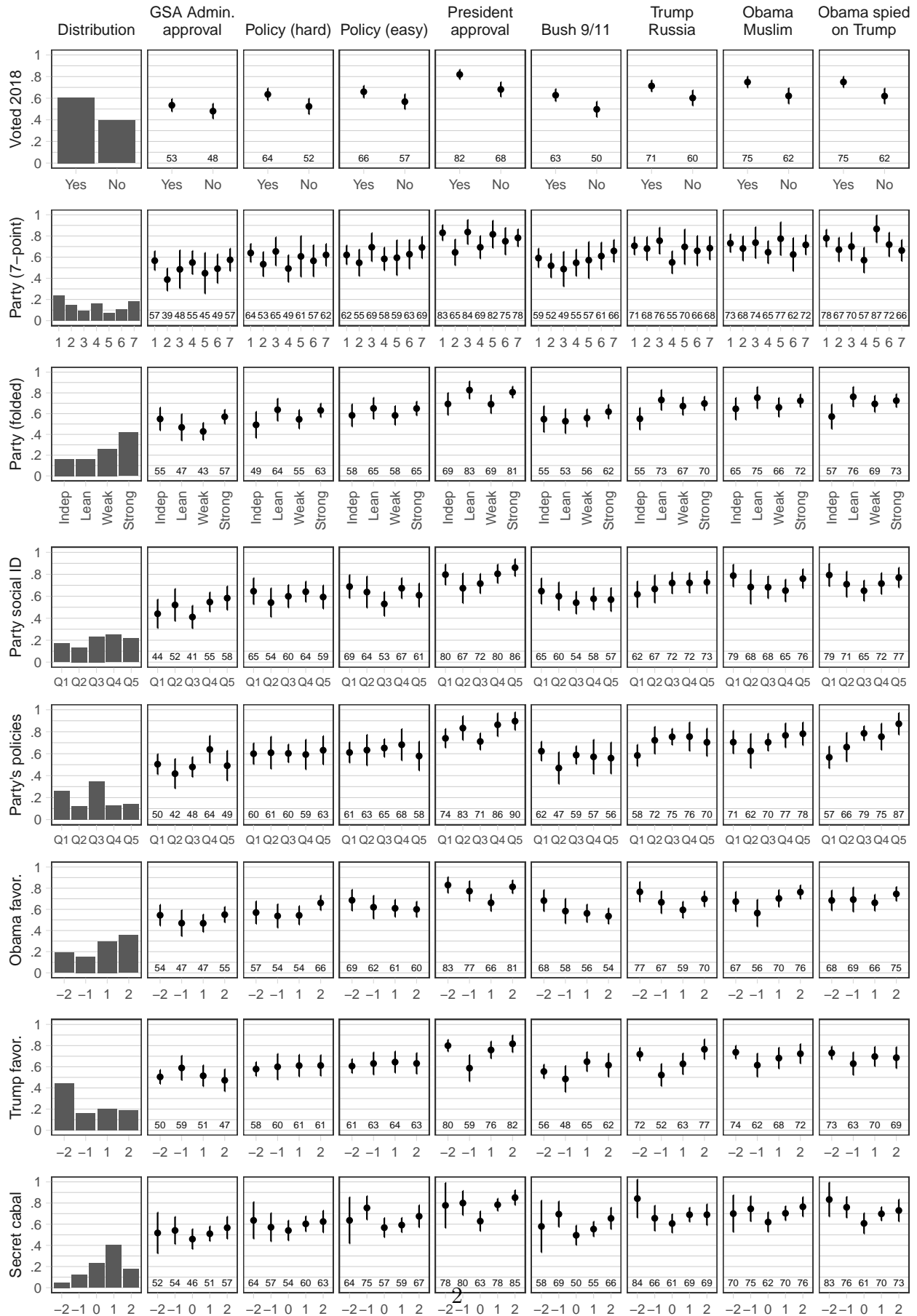
This online appendix contains a series of figures to accompany a chapter, by the same title and authors, of *The Politics of Truth*, edited by David Barker and Elizabeth Suhay and published in 2021 by the Oxford University Press. The chapter refers to each figure by its number. For more details, see the chapter and the notes that appear below each figure.

Figure 1: Predictors of choosing to answer more questions.



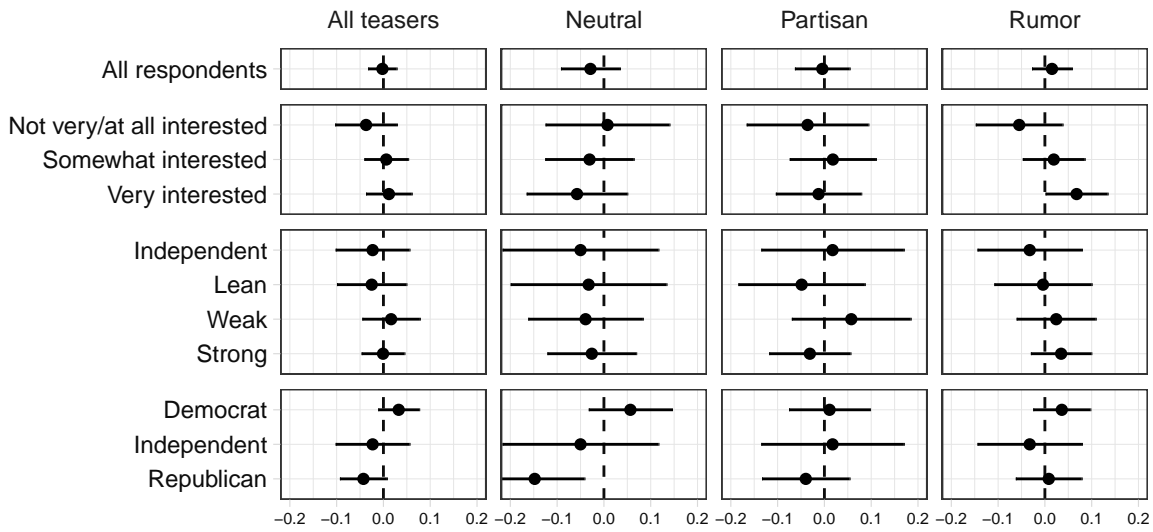
Note: In each row, the x-axis is values of the variable listed in the left margin. In the panels with bars, the y-axis is the overall percentage of respondents in each category. In the panels with dots and whiskers, the y-axis is the percentage of respondents with that characteristic who chose to answer more questions after being “teased” with the question listed at the top of the figure. The numbers along the bottom of each panel are equal to the value plotted on the y-axis.

Figure 2: Predictors of choosing to answer more questions, continued.



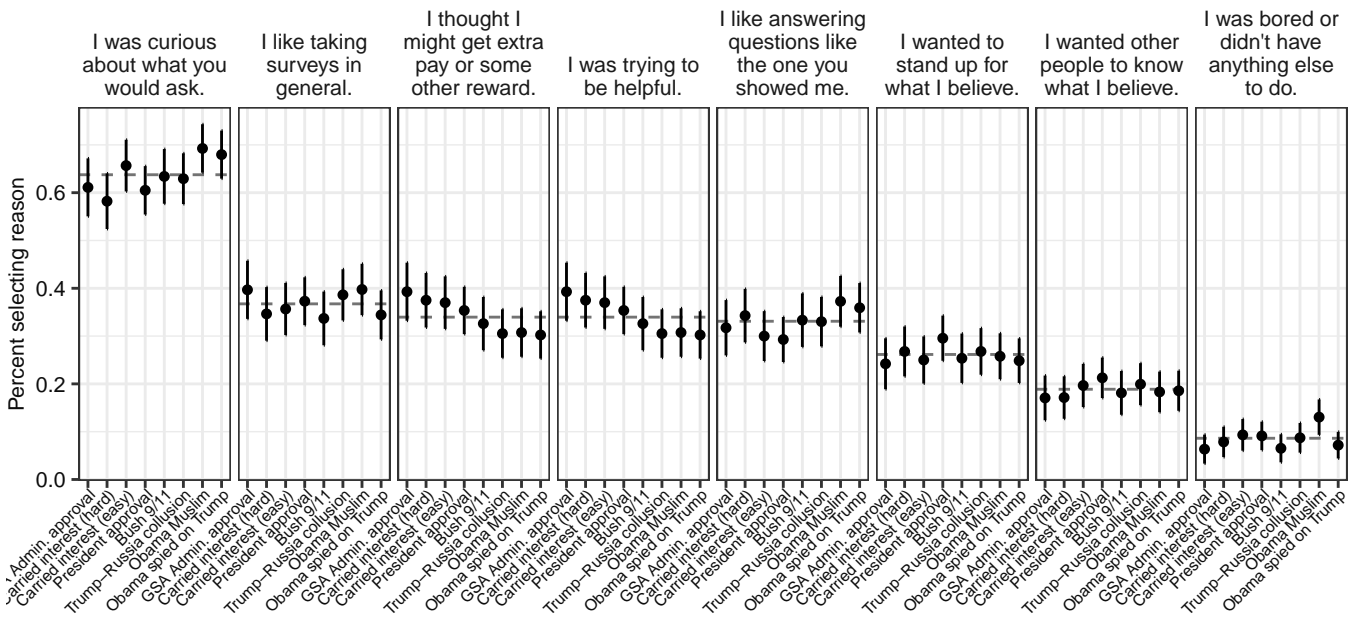
Note: See the note accompanying Figure 1.

Figure 3: Effect of national audience on desire to answer more questions.



Note: This figure plots the average treatment effect of the audience treatment on the percentage of respondents who wanted to answer more questions. The treatment group was told that the questions are for a national poll, while the control group was told that the questions are for a test survey. The columns correspond to the categories of teaser questions in Table 1. The rows display separate estimates for all respondents and within categories of political interest, strength of partisanship, and party identity (counting leaners as partisans). Dots are OLS estimates. Whiskers represent 95 percent confidence intervals.

Figure 4: Stated reasons for answering more questions.



Note: At the end of the survey, each respondent who chose to answer additional questions was asked, “Which of the following are reasons why you wanted to answer five more questions?” This figure plots the percentage of respondents in each treatment group who chose each answer. The vertical dashed lines are the pooled percentage (i.e., the overall percentage across all eight teaser questions).