# Expressive Responding and Trump's Big Lie

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Do surveys measure sincere belief in Donald Trump's claims that fraud decided the 2020 election? We apply a multi-method approach to detecting expressive responding to the case of Trump's "big lie." Our evidence includes two versions of an honesty encouragement design, a list experiment, two opportunities to express related sentiments, and two opportunities to bet on prominent predictions about the future that were spawned by the big lie. We find minimal evidence of expressive responding. Nearly all survey respondents who directly endorse the big lie appear to genuinely believe it. These "believers" are about evenly split between those who confidently accept the big lie and those who find it to be plausible but are not deeply convinced. Similarly, large majorities of those who predicted that evidence of fraud would enable Trump to retain power in January 2021 or be reinstated in August 2021 appear to have been sincere.

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*I will totally accept the results of this great and historic presidential election ... if I win.*

— *Donald Trump, October 20, 2016*

Despite years of telegraphing his intent to dispute the result of any election he lost, polls suggest that former U.S. president Donald Trump has convinced a sizeable proportion of the American public, especially supporters of the Republican party, that his election loss to Joe Biden in 2020 was a consequence of fraud. The myth of a stolen election has taken on a sufficiently central role in contemporary American politics that observers have dubbed it "the big lie." Many observers question the notion that so many have accepted Trump's predictable and baseless claims, suggesting that some who do not believe the big lie may use survey questions about it to express their more general support for Trump.[1] Do surveys measure sincere belief in the big lie, or are respondents misrepresenting their belief in Trump's claims?

This paper examines whether measured belief in Trump's big lie is an artifact of expressive responding. We begin by borrowing three established methods from existing research, none of which yield evidence that respondents who endorse the big lie do not sincerely believe their answers. First, to address the possibility that respondents may not be sufficiently motivated to reveal their sincere beliefs, we test two versions of an encouragement to be honest (Berinsky 2018). Second, to address the possibility that respondents may endorse the big lie to maintain their self-image or comply with group norms, we anonymize responses using a list experiment (Miller 1984; Blair et al. 2020). Third, to address the possibility that respondents who endorse the big lie are really trying to express some other sentiment, we test two interventions designed to mitigate "response substitution" (Gal and Rucker 2011; Yair and Huber 2020). Across these five interventions, we find no evidence of expressive responding.

[1] For example, see Emily Badger, "Most Republicans Say They Doubt the Election. How Many Really Mean It?," *The New York Times*, November 30, 2020. Elizabeth Connor, "Do Republicans really believe the election was stolen – or are they just saying that?," *The Washington Post*, December 22, 2020. Musa Al-Gharbi, "No, America is not on the brink of civil war: it's time to tell the truth about the big lie," *The Guardian*, January 27, 2022.

None of our estimates are statistically significant. Among Republicans, no estimate is larger than two percentage points and three of the five take the wrong sign.

We next introduce a novel approach to financial incentive designs, *betting on the future*. Relative to honesty encouragement, payment for correct answers has a stronger track record of mitigating expressive responding (Bullock et al. 2015; Prior et al. 2015; Berinsky 2018; Peterson and Iyengar 2020). Yet for the study of partisan controversies and conspiratorial beliefs, payment for correct answers has an important downside: if researchers and respondents do not share a common basis for determining the truth, payment for correct answers may encourage respondents to say what they believe the researcher thinks is true rather than what the respondent themselves believe is true (Berinsky 2018; Malka and Adelman 2022). We circumvent this challenge by taking advantage of a central feature of the big lie: a series of prominent predictions that once the big lie was proven to be true, Trump would be able to overturn the election results or be restored to power. In two such circumstances, we allowed respondents to place bets on whether Trump would be president on the predicted date. Both studies suggest that large majorities of those who endorsed prominent predictions associated with the big lie were being sincere. In November 2020, about 80 percent of Republicans who predicted that Trump would successfully overturn the election result appear to have been sincere. In July 2021, more than 90 percent of Republicans who predicted that evidence of fraud would lead to Trump's reinstatement were sincere.

Even as our experiments yield limited evidence of expressive responding, they leave open another important question about the sense in which respondents "believe" their answers: do these individuals confidently accept the big lie, or is it simply their sincere best guess that the big lie is more likely than not to be true (Kuklinski et al. 2000; Pasek et al. 2015; Graham 2020)? In a supplemental analysis, we find that those who endorse the big lie are split about 50-50 between confident acceptance and uncertain guesses by those who suspect that the big lie is probably true, but have not fully accepted it. Unlike most measures of political misperceptions (Graham 2022), we find that confident beliefs are stable over time. This stands in contrast to Trump's previously most notorious claim of election fraud, that millions of illegal votes were cast in the 2016 presidential election. Relative to other measured misperceptions, the big lie appears to be unique in terms of the veracity with which it is believed.

In the bigger picture, our findings yield three key lessons for observers of politics. First, expressive responding is not an all-or-nothing proposition. Though robust evidence of expressive responding has been found in some cases (Bullock et al. 2015; Prior et al. 2015; Schaffner and Luks 2018; Peterson and Iyengar 2020; Yair and Huber 2020; Graham and Coppock 2021), it fails to appear in others (Berinsky 2018; Bullock and Lenz 2019; Malka and Adelman 2022). Second, testing for expressive responding paints and incomplete picture of whether respondents "believe" their answers. Respondents who are being sincere may still harbor doubts (Pasek et al. 2015; Graham 2022). Third, the robustness of measured belief in the big lie relative to other measures of misperceptions suggests that careful, item-level validation is required to determine which beliefs are meaningful and which are insincere or ephemeral. Unfortunately, validation exercises like ours tend to follow rather than lead prominent narratives regarding the veracity of measured beliefs. To reverse this state of affairs, we conclude by suggesting that survey researchers should proactively incorporate multi-method validation exercises into their initial assessments of the public's beliefs regarding important topics. This would allow evidence to shape narratives from the outset, providing observers of politics with more timely and accurate information regarding the nature of public sentiment.

# Expressive Responding

Survey researchers would usually like to measure their subjects' genuine beliefs. Inconveniently, however, survey respondents sometimes misrepresent their beliefs: that is, they select a response option other than the option that most accurately reflects their underlying beliefs. We define *partisan expressive responding* as instances in which survey respondents misrepresent their beliefs in order to convey a partisan sentiment.[2] In this paper, we take a multi-method approach that addresses two different plausible motives for expressive responding: subjects may want to reap the psychological benefits of expressing a partisan sentiment (Bullock et al. 2015; Schaffner and Luks 2018; Malka and Adelman 2022) or avoid the costs, psychological and otherwise, of expressing sentiments that are inconsistent with one's self-image or the norms of one's social group (Blair et al. 2020).

Our first and simplest approach is *honesty encouragement* in the form of two written prompts that ask respondents to reveal their beliefs honestly. The goal of honesty encouragement is to increase the value that respondents place on revealing their true beliefs, by either increasing the saliency of the expectation from the survey conductors of an honest survey response and/or by increasing the salience of the norm of being truthful. If successful, the honesty encouragement outweighs other influences on the response.

Our second approach is *list experiments*, which are also known as the item count technique. Rather than ask questions directly, list experiments ask subjects to count the number of statements with which they agree. For some randomly selected subjects, the list omits the belief of interest, in this case belief in the big lie. Comparing the average level of agreement with the two sets of statements allows us to estimate the percentage who hold the belief of interest (see also Berinsky 2018). The defining feature of list experiments is that they break the link between subjects and their response. Doing so is thought to shield survey respondents from a number of costs of endorsing socially undesirable beliefs, such as damage to their self image or the disapproval of others (Blair, Coppock and Moor 2020, Table 1). In our context, we would expect list experiments to work if one's position on the big lie is important to one's self-image or self-presentation as a partisan (also see Berinsky 2018; Bullock and Lenz 2019). In current politics, we would expect these pressures to encourage Republicans (Democrats) to endorse the big lie at higher (lower) rates than their genuine beliefs support.

[2]Expressive responding has also been documented in apolitical contexts (Gal and Rucker 2011). We restrict our attention to political cases.

Our third approach tests for *response substitution*, which is the phenomenon that survey respondents often answer the question they want to answer rather than the question that was actually asked. Gal and Rucker (2011) use the example of a restaurant with good food and terrible service. If asked to answer a one-question survey about the food, one might be tempted to provide a lower rating in order to express disapproval of the service, thereby "substituting" one's rating of the service for the rating of the food. Adding a question about the service would reverse the response substitution effect. Analogous effects have been documented in the study of politics (Yair and Huber 2020; Graham and Coppock 2021). For example, partisans tend to say that members of the opposite party are less attractive (Nicholson et al. 2016; cf. Huber and Malhotra 2017). However, when given the chance to rate the potential partner's values, apparent differences in physical attractiveness shrink considerably (Yair and Huber 2020). In both of these examples, response substitution emerges because truthfully answering that question does not allow respondents to express another sentiment that they wish to convey. In our context, we would expect response substitution treatments to work if subjects are using questions about the big lie to express closely related sentiments.

Our fourth and final approach is *financial incentives* in the form of payment for correct answers. Like honesty pledges, financial incentives seek to outweigh other factors that may influence the response. Yet the standard approach of paying for correct answers has a downside: if respondents believe that the researcher does not share their definition of truth, the incentive will motivate respondents to say what they believe the researcher believes to be true, not what the respondents themselves believe to be true (Berinsky 2018; Malka and Adelman 2022). This concern is especially important in the case of politicized controversies in polarized societies, which leave no common authority to appeal to. To circumvent this challenge, we allow respondents to bet on two concrete predictions about the future that are closely related to belief in the big lie. The first study was conducted in late November 2020, at which time Trump and his allies claimed that soon-to-emerge evidence of fraud would allow them to overturn the election results through the courts. The second was conducted in July 2021, at which time Trump and his allies claimed that evidence of fraud would lead to his restoration to the presidency. We describe the two cases in more detail below.

Strategies for mitigating expressive responding share an important limitation: they provide no information about how confidently respondents hold their beliefs (e.g., Kuklinski et al. 2000; Pasek et al. 2015). For example, someone who does not know much about the arguments on either side of the big lie could still make a sincere guess that fraud probably determined the result. Most measures of political misperceptions appear to capture the latter sort of belief: respondents who endorse falsehoods generally find them plausible but have not accepted them outright (Graham 2022). Consequently, after determining that our respondents are by and large sincere, we examine their degree of confidence in their beliefs and the temporal stability of these claims to be confident.

# Data

We collected our data across four surveys conducted on Amazon Mechanical Turk (MTurk) between November 28, 2020 and September 23, 2021. MTurk is a widely-used convenience sample vendor that tends to produce experimental treatment effects that are close to the general population (Mullinix et al. 2016; Coppock et al. 2018; Coppock and McClellan 2019), including in studies specifically related to partisanship (Levendusky 2018; Skytte 2021) and partisan expressive responding (Yair and Huber 2020). In this section, we briefly describe each survey. Table 1 lists the dates, sample size, partisan composition, and accuracy inducements included in each survey. Further information appears in the appendix.

Survey 1 was fielded shortly after the 2020 U.S. presidential election, on November 28-30, 2020. It included the first betting on the future study. For budgetary reasons, the survey was offered only to respondents who had indicated in separate surveys, conducted in March and May 2020, that they are Republicans. A total of 1,049 respondents completed the survey. Of these, 934 again identified themselves as Republicans while 115 identified

Table 1: Survey information.

| | | | Partisanship | | | |
|---|---|---|---|---|---|---|
| Survey | Dates | N | Dem | Rep | Ind | Interventions |
| Survey 1 | Nov. 28-30, 2020 | 1049 | 0 | 934 | 115 | Betting on the future |
| Survey 2 | May 10-23, 2021 | 2958 | 1754 | 934 | 270 | Response substitution |
| Survey 3 | July 7-31, 2021 | 4885 | 2278 | 2064 | 543 | List experiment, betting on the future |
| Survey 4 | Sep. 22-23, 2021 | 5005 | 2972 | 1739 | 294 | Honesty encouragement |
| Total | | 13897 | 7004 | 5671 | 1222 | |

as independents or Democrats. In our analysis, we pool these ex-Republicans into an "all others" category. Unlike our subsequent surveys, this survey was not pre-registered.

Survey 2 was fielded on May 10-23, 2021. Respondents first completed a baseline survey consisting of background questions and a pre-treatment measure of the dependent variable ($N = 3,599$). All respondents were recontacted for a second survey containing the response substitution experiment. Our pre-registered stopping rule was 3,000 respondents, and we stopped at almost that number, as 2,958 respondents completed this survey (recontact rate = 82.1 percent). Our analysis of this survey was pre-registered at https://aspredicted.org/blind.php?x=GDZ_UAN. A copy of the preregistration also appears in Appendix G.

Survey 3 was fielded on July 10-31, 2021. A total of 4,885 respondents completed the survey, which featured the list experiment and the second betting on the future study. Because list experiments yield less precise estimates than our other interventions, we recruited respondents from a pool of individuals who had indicated their partisanship in previous surveys, including but not limited to the Survey 2 baseline survey. This allowed us to recruit a larger share of Republicans than is typically present in online convenience samples. Our analysis of this survey was pre-registered at https://aspredicted.org/blind.php?x=HSX_CLG. A copy of the preregistration also appears in Appendix G.

Survey 4 was fielded on September 22-23, 2021. A total of 5,005 respondents completed the survey. After completing an unrelated study, all respondents participated in the honesty encouragement experiment. Our analysis of this survey was pre-registered at https://aspredicted.org/blind.php?x=7YB_LLV. A copy of the preregistration also appears in Appendix G.

We selected our four experimental approaches during the months between Surveys 1 and 2. After devising the strategy, we fielded each component in the order that best-suited our logistical and cost constraints. Consequently, the following sections in an order that we believe makes conceptual sense, not in the temporal order in which the surveys were conducted.

## Honesty Encouragement

We begin by examining two interventions designed to encourage respondents to report their beliefs honestly and accurately. Our interventions are closely modelled after the first two studies reported by Berinsky (2018) and are similar to honesty or accuracy encouragements used by Bullock et al. (2015), Prior et al. (2015), and Hanmer et al. (2014). Both were fielded as part of Survey 4 (Table 1).

With equal probability, subjects were randomly assigned to a control condition or to one of the two treatments (simple random assignment, p = 1/3). The *honesty request* treatment asked respondents to honestly report their true beliefs. Before answering the question, respondents read the following text: "Regardless of how you feel about the people and events mentioned in the question below, we want you to tell us what you believe to be true. Again, we ask that you try and ignore your personal feelings." The *subtle pipeline* treatment endeavors to strengthen this intervention by adding the suggestion that researchers may somehow learn about dishonest responding. Respondents read the text, "We sometimes find that people choose answers that they do not really believe so that they can say something good or bad about the people and events mentioned in the question," followed by the same text as the honesty request.

Following their assigned treatment, subjects indicated their beliefs about whether the election was fraudulent. Subjects were asked, "Do you think that Joe Biden only won the 2020 presidential election due to voter fraud, or would he have won either way?" Responses were recorded on a five-point Likert scale: "Definitely due to voter fraud," "Probably due to voter fraud," "Not sure," "Probably would have won either way," "Definitely would have won either way." We coded this measure to range from 0 to 1, where 0 corresponds to "definitely would have won either way" and 1 corresponds to "definitely due to voter fraud." The control condition included only this question, with no preceding text.

To estimate the effect of the honesty encouragement treatments on endorsements of the big lie, we use OLS to estimate the parameters in

$$Y_i = \alpha + \beta_1 \text{Request treatment}_i + \beta_2 \text{Pipeline treatment}_i + \epsilon_i, \tag{1}$$

where $i$ indexes subjects, Request treatment$_i$ and Pipeline treatment$_i$ are indicators of our subjects' treatment assignment. $\alpha$ estimates our subjects' baseline tendency to endorse the big lie, $\beta_1$ estimates the effect of the request treatment, and $\beta_2$ estimates the effect of the subtle pipeline treatment. As we expect expressive pressures to point in different directions depending on the subject's partisanship, we report results separately for Democrats, Republicans, and independents. In keeping with the preregistration, we use one-tailed tests throughout the paper, unless noted otherwise.

The honesty encouragement treatments yield no evidence of expressive responding. Table 2 presents our estimates of (2). At baseline, the mean scale score was 0.404 among Republicans, 0.148 among Democrats, and 0.314 among independents. No statistically significant evidence emerges that either honesty encouragement altered these expressions of belief. The request treatment's estimated effect was -0.004 for Democrats (s.e. = 0.009), -0.005 for Republicans (s.e. = 0.019), and 0.025 for independents (s.e. = 0.045). The subtle pipeline treatment's estimated effect was -0.003 for Democrats (s.e. = 0.009), 0.019 for

Table 2: Honesty encouragement estimates.

| | Dem. | Indep. | Repub. | Partisan diff. |
|---|---|---|---|---|
| Constant | $0.148^{**}$ | $0.314^{**}$ | $0.404^{**}$ | $0.148^{**}$ |
| | (0.006) | (0.033) | (0.013) | (0.006) |
| Request treatment | −0.004 | 0.025 | −0.005 | −0.004 |
| | (0.009) | (0.045) | (0.019) | (0.009) |
| Pipeline treatment | −0.003 | 0.002 | 0.019 | −0.003 |
| | (0.009) | (0.046) | (0.019) | (0.009) |
| Republican | | | | $0.256^{**}$ |
| | | | | (0.015) |
| Republican × request treatment | | | | −0.000 |
| | | | | (0.021) |
| Republican × pipeline treatment | | | | 0.022 |
| | | | | (0.021) |
| Adj. $R^2$ | −0.001 | −0.006 | −0.000 | 0.197 |
| Num. obs. | 2969 | 294 | 1737 | 4706 |

*Note:* First three columns display estimates of (2). Fourth column interacts all terms with a Republican indicator; independents are excluded from this column. Robust standard errors in parentheses. One tailed tests preregistered. $^{*}p < 0.05$, $^{**}p < 0.01$.

Republicans (s.e. = 0.019), and 0.002 for independents (s.e. = 0.046).

For comparability with other studies, we also estimate the honesty encouragement treatment's effect on partisan differences. We do this by subsetting our data to Democrats and Republicans only, then interacting each term in 2 with a Republican indicator. We use OLS to estimate the parameters in

$$\begin{aligned} Y_i = \alpha + \beta_1 \text{Request treatment}_i + \beta_2 \text{Pipeline treatment}_i + \beta_3 \text{Republican}_i + \\ \beta_4 \text{Republican}_i \times \text{Request treatment}_i + \beta_5 \text{Republican}_i \times \text{Pipeline treatment}_i + \epsilon. \end{aligned} \quad (2)$$

Here, $\beta_3$ estimates the control group partisan difference and $\beta_4$ and $\beta_5$ estimate the two treatments' effect on partisan differences. If $\beta_4$ or $\beta_5$ take the opposite sign as $\beta_3$ and are statistically significant, we can infer that expressive responding occurred,

The results appear in the fourth column of Table 2. Consistent with the party-by-party results, we find no evidence of expressive responding. At baseline, the partisan difference was 0.256 scale points on the 0 to 1 scale. The average scale score was almost the same in the request treatment group ($\hat{\beta}_4 = 0.000$, s.e. $= 0.021$) and was slightly larger in the subtle pipeline treatment group ($\hat{\beta}_5 = 0.022$, s.e. $= 0.021$). Neither estimate is statistically significant.

## List Experiment

We next compare a list experiment estimate of the belief that Biden's victory was fraudulent to an identically worded direct question. As list experiments yield statistically imprecise estimates of the quantity of interest (Miller 1984; Blair et al. 2020), we conducted a simulation study prior to the experiment to identify the probabilities of treatment assignment that would minimize the standard error of our comparison. Using simple random assignment, we assigned about one-fifth of our respondents to the direct question ($p = 0.21$) and split the remaining respondents between the two list experiment conditions ($p = 0.395$).

The list experiment proceeded as follows. Respondents were first told, "Here is a list of statements about the 2020 U.S. presidential election. We want to know how many you agree with." Respondents in the control group were presented with the following list:

- I voted in the election.
- I did not know where to vote.
- I requested an absentee ballot, but it did not arrive.
- The Green Party came close to winning the election.

In the treatment group, the list also included:

- Joe Biden only won the election due to voter fraud.

The items were listed in random order. Below the list, respondents were asked "How many statements do you agree with?" and presented with a scale from 0 to 4 (control group) or 0 to 5 (treatment group).

Table 3: Distribution of responses, list experiment.

| | | *Response distribution* | | | | | |
|---|---|---|---|---|---|---|---|
| | | 0 | 1 | 2 | 3 | 4 | 5 |
| Control | N | 193 | 1439 | 148 | 108 | 55 | |
| | % | 9.9 | 74.1 | 7.6 | 5.6 | 2.8 | |
| Treatment | N | 154 | 1119 | 442 | 87 | 81 | 43 |
| | % | 8.0 | 58.1 | 22.9 | 4.5 | 4.2 | 2.2 |

List experiments do not ensure anonymity for individuals who disagree with all or none of the statements (Blair and Imai 2012). This threat can be minimized by selecting statements that are negatively correlated. Consequently, we paired "I voted in the election" with two statements that are unlikely to apply to people who voted: "I did not know where to vote" and "I requested an absentee ballot, but it did not arrive." The distribution of responses appears in Table 4. About 12.7 percent (control group) and 10.2 percent (treatment group) choose the highest or lowest option.

We compare the list experiment estimate to a direct question that matches the wording of the list experiment as closely as possible. Respondents assigned to the direct question condition were asked, "Do you agree or disagree with this statement about the 2020 U.S. presidential election? [Paragraph break.] Joe Biden only won the election due to voter fraud" and responded with "agree" or "disagree." This exactly matches the list experiment item in its wording and response options.

We estimate the degree of expressive responding within party by comparing the list experiment and direct question estimates. The effect on partisan differences is the difference between the list experiment estimates for Democrats and Republicans. We bootstrap standard errors and confidence intervals and calculate p-values using the percentile method.[3]

The list experiment finds no statistically significant evidence of expressive responding.

[3] To estimate each statistic's sampling distribution, we resampled our respondents with replacement 10,000 times and calculated the direct question estimate, the list experiment estimate, and the difference between them. Standard errors are calculated as the standard deviation of this distribution of estimates. One-sided hypothesis tests are conducted by calculating the percentage that fall above or below zero.

Table 4: List experiment estimates.

| Term | Dem. | Indep. | Repub. | Partisan diff. |
|---|---|---|---|---|
| Direct question estimate | 0.086 | 0.250 | 0.550 | 0.464 |
| | (0.013) | (0.041) | (0.024) | (0.027) |
| List experiment estimate | 0.083 | 0.149 | 0.527 | 0.444 |
| | (0.043) | (0.071) | (0.044) | (0.061) |
| Difference | -0.003 | -0.101 | -0.023 | -0.020 |
| | (0.045) | (0.082) | (0.050) | (0.067) |
| N | 2278 | 543 | 2064 | 4342 |

*Note:* First three columns display list experiment estimates by party. Fourth column calculates the partisan difference by subtracting Democrats from Republicans; independents are excluded from this column. Bootstrap standard errors in parentheses. One tailed tests preregistered. $^{*}p < 0.05$, $^{**}p < 0.01$. Asterisks omitted from first two rows.

Table 4 compares the list experiment and direct question estimates. Among Republicans assigned to the direct question condition, 55.0 percent agreed that Biden only won due to fraud. The list experiment estimates that 52.7 percent of Republicans agree with this statement, a difference of 2.3 percentage points (s.e. = 5.0). Among Democrats, the direct question estimate is 8.6 percent, compared with 8.3 percent in the list experiment (difference = 0.3, s.e. = 4.5). Among independents, the direct question estimate is 25.0 percent compared with 14.9 percent in the list experiment (difference = 10.1, s.e. = 8.2). There is also no evidence of an effect on partisan differences (difference = 2.0, s.e. = 6.7).

## Response Substitution

Our third test for expressive responding considers the possibility that endorsements of the big lie are due to response substitution. In other words, respondents may endorse the big lie in surveys not because they believe it specifically, but because they want to express another related sentiment. We considered two sentiments to be especially likely candidates for response substitution. First, Republican subjects may believe that election fraud occurred, but did not occur at a scale sufficient to alter the results of the election. Second, Republican subjects may not believe the election was fraudulent at all, but want to express their disappointment that Trump did not prevail. In the latter case, we also find it plausible that some Democrats believe that Biden's victory was fraudulent but want to express their satisfaction with that outcome.

To test this possibility, Survey 2 included a three-armed trial, assigning subjects either to a control group or to one of two treatment conditions designed to detect response substitution (simple random assignment, $p = 1/3$). All subjects answered the same question about belief in the big lie that was used in the honesty pledge experiment. Immediately before this, respondents assigned to one of the two response substitution treatment conditions answered a question designed to reduce response substitution.

The *fraud occurred* treatment examined the possibility that questions about whether Biden's victory was fraudulent serve as an expressive outlet for the belief that fraud occurred, regardless of whether it determined the election result. Just before the outcome variable, respondents who were randomly assigned to this condition were asked, "Which comes closest to your view?" and given the response options, "There was **no voter fraud** in the 2020 presidential election," "There was **a little voter fraud** in the 2020 presidential election," and "There was **a lot of voter fraud** in the 2020 presidential election." To the degree that this treatment reduces measured belief that Biden's victory was fraudulent, one would conclude that some respondents want to express that fraud occurred but do not believe that fraud determined the election result.

The *wrong decision* treatment examined the possibility that questions about election fraud serve as an outlet for expressing disapproval of the result. Just before the outcome variable, respondents assigned to this condition were asked "Which comes closest to your view?" with the response options "Electing Joe Biden was the **right decision** for the country" and "Electing Joe Biden was the **wrong decision** for the country." To the degree that this treatment reduces measured belief that Biden's victory was fraudulent, one would conclude that some respondents want to express disapproval of the result but do not believe that it was fraudulent.

To statistically test for the effect of these treatments, we use OLS to estimate the parameters in

$$Y_i = \alpha + \beta_1 F_i + \beta_2 W_i + \beta_3 X_i + \epsilon_i, \quad (3)$$

where $i$ indexes respondents, $Y_i$ is belief in the big lie, $F_i$ indicates assignment to the fraud occurred treatment, $W_i$ indicates assignment to the wrong decision treatment, and $X_i$ is a pre-treatment measure of belief in the big lie collected in the baseline survey.[4] To aid the interpretability of our parameter estimates, we de-mean $X_i$ within each party.[5] Consequently, $\alpha$ can be interpreted as average endorsement of the big lie in the control group. $\beta_1$ and $\beta_2$ estimate the effect of the response substitution treatments. We estimate the effect on partisan differences by interacting each term with a Republican indicator.[6]

Neither treatment yields evidence that response substitution affects measured belief in the big lie. On the 0 to 1 scale, Republicans assigned to the fraud occurred treatment said that the big lie was 0.008 scale points more likely to be true (s.e. = 0.019), while Republicans assigned to the wrong decision treatment also said that it was 0.008 scale points more likely to be true (s.e. = 0.017). Democrats assigned to the fraud occurred treatment said the big lie was 0.010 scale points more likely to be true (s.e. = 0.011), while those assigned to the wrong decision treatment were 0.006 scale points less supportive of it (s.e. = 0.010). Independents in the fraud occurred treatment said that it was 0.058 scale points less likely to be true (s.e. = 0.027), while those in the wrong decision condition were 0.015 scale points less supportive (s.e. = 0.026). As we have no expectation that response substitution should systematically shift independents in one direction or the other, we attribute the statistically significant result to noise. Similarly, no evidence emerges that the treatments affected partisan differences. In column 4, both estimates for the effect on partisan differences take the wrong sign and are

[4] Appendix D reports estimates without covariate adjustment, which are similar to those presented here.

[5] That is, after splitting our data by party, we calculate the party mean of $X$ and subtract it.

[6] A detailed explanation of our strategy for examining partisan differences appears above in the "Honesty Encouragement" section.

Table 5: Response substitution estimates.

| | Dem. | Indep. | Repub. | Partisan diff. |
|---|---|---|---|---|
| Constant | $0.130^{**}$ | $0.355^{**}$ | $0.506^{**}$ | $0.034^{**}$ |
| | (0.007) | (0.019) | (0.014) | (0.007) |
| Fraud occurred treatment | 0.010 | $-0.058^{*}$ | 0.008 | 0.011 |
| | (0.011) | (0.027) | (0.019) | (0.011) |
| Wrong decision treatment | −0.006 | −0.015 | 0.008 | −0.004 |
| | (0.010) | (0.026) | (0.017) | (0.010) |
| Republican | | | | $0.117^{**}$ |
| | | | | (0.018) |
| Republican × fraud occurred treatment | | | | 0.001 |
| | | | | (0.022) |
| Republican × wrong decision treatment | | | | 0.014 |
| | | | | (0.020) |
| Pre-treatment DV | $0.553^{**}$ | $0.870^{**}$ | $0.792^{**}$ | $0.684^{**}$ |
| | (0.029) | (0.032) | (0.021) | (0.019) |
| Adj. $R^2$ | 0.323 | 0.716 | 0.587 | 0.640 |
| Num. obs. | 1744 | 267 | 929 | 2673 |

*Note:* First three columns display estimates of (3). Fourth column interacts all terms with a Republican indicator; independents are excluded from this column. Robust standard errors in parentheses. One tailed tests preregistered. $^{*}p < 0.05$, $^{**}p < 0.01$.

statistically insignificant.

## Betting on the Future

We next turn to our financial incentive design. Beliefs that are rooted in partisan misinformation challenge the classic financial incentive paradigm, which relies on payment for correct answers about facts that are not widely disputed (Bullock et al. 2015; Prior et al. 2015). By its nature, partisan misinformation casts doubt on authoritative sources of truth, leading to the concern that payment for correct answers will lead respondents to try to guess what the researcher believes is true rather than revealing their own beliefs (Berinsky 2018). Consequently, research that examines expressive responding about belief in false claims made by partisan actors generally shies away from the use of financial incentives (Berinsky 2018; Schaffner and Luks 2018; but see Peterson and Iyengar 2020).

To apply financial incentive treatments to the big lie, we took advantage of a unique feature of this case: highly salient predictions about the future. Specifically, the period after Trump's election loss was characterized by a series of widely disseminated predictions that evidence of fraud would enable Trump to hang onto the presidency or be reinstated to it. Rather than having our subjects bet directly on whether the big lie is true, we allowed our subjects to bet on whether these predictions would bear out. Regardless of what the subjects believe about the researcher's beliefs, the *betting on the future* strategy gives subjects an incentive to rely only on their best guess about the truth.

The central drawback of this technique is that it does not allow us to directly measure survey respondents' beliefs in Trump's big lie, but rather tangent beliefs that depend in some way belief in the big lie. For example, a genuine belief that Trump would be reinstated in August 2021, as in Study 2 below, is extremely likely to depend on a belief that credible evidence of voter fraud will be revealed. However, expressive responding could still occur due to a lack of genuine belief in some other component of the theory. For example, one who endorses the idea of reinstatement might sincerely believe that evidence of fraud will emerge, but might not sincerely believe that this will be enough to get Trump reinstated. Thus, while the betting on the future technique can provide strong evidence as to whether partisans are willing to put their money where their mouth is, it leaves some ambiguity as to precisely which belief drives whatever expressive responding is detected.

**Study 1**

Our first implementation of the betting on the future strategy was conducted at the end of November 2020, shortly after the contested presidential election. At this time, Trump and his associates were predicting that evidence of fraud would enable him to prevail in his attempts to overturn the results.

To leverage these circumstances, we recruited a sample of 939 Republican subjects. The sample also included 115 subjects who had identified themselves as a Republican in March or May 2020, but identified as an independent or a Democrat in our survey (Survey 1, Table 1). To begin, all subjects were asked the following question: "On January 20, the winner of the presidential election will be inaugurated and begin his term. [Paragraph break.] Who do you expect to be President the next day, on January 21?" The response options were "Joe Biden" and "Donald Trump." Next, all subjects were asked which of two tickets they would like to enter into a drawing for a $100 bonus, to be conducted on January 21, 2021. One ticket read "Win if Donald Trump is President on January 21, 2021" while the other read "Win if Joe Biden is President on January 21, 2021." This within-person design enables us to estimate the frequency with which individuals change their prediction when money is on the line.

To estimate the difference between the two question formats, we use OLS to estimate the parameters in

$$Y_{ik} = \alpha + \beta \text{Incentive}_{ik} + \epsilon_{ik} \tag{4}$$

where $i$ indexes subjects, $k$ indexes the two questions (no incentive or incentive), and $\text{Incentive}_{ik}$ is an indicator for the incentivized question. Because we have two observations per subject, we cluster our standard errors at the subject level. This yields exactly

Table 6: Betting on the future: Study 1.

| | Republican | All others |
|---|---|---|
| Constant | 0.266** | 0.079** |
| | (0.014) | (0.025) |
| Incentivized question | −0.057** | −0.018 |
| | (0.010) | (0.012) |
| Adj. $R^2$ | 0.004 | −0.003 |
| Num. obs. | 1864 | 228 |
| Num. clusters | 932 | 114 |

*Note:* Table displays estimates of (4) with clustered standard errors in parentheses. One tailed tests preregistered. $^*p < 0.05$, $^{**}p < 0.01$. Asterisks omitted from first two rows.

the same estimates that result from subtracting the two measures and taking the mean (see Appendix E.2), but with the benefit of explicitly showing the mean of the unincentivized condition.

This test yields our only statistically significant evidence of expressive responding (Table 6). Among Republicans, 26.6 percent predicted that Trump would remain president when there was no money on the line. This fell to 20.9 percent when incentives were introduced, a statistically significant decline of 5.7 percentage points (s.e. = 1.4; in this study we use two-tailed tests because it was not-preregistered). This suggests that about one-fifth of Republicans who initially claimed that Trump would prevail in his effort to overturn the election results did not actually think that this was the most likely outcome. Among those who did not identify as Republicans, 7.9 percent predicted that Trump would retain power when there was no money on the line, compared with 6.1 percent when the incentive was introduced. This decline of 1.8 percentage points (s.e. = 1.2) falls on the borderline of statistical significance.

The key weakness of Study 1 is the within-person design. To address this shortcoming, we took advantage of another prominent prediction that evidence supporting Trump's big lie would enable him to serve a second term as president.

**Study 2**

Our second betting on the future study was conducted in summer 2021. At this time, rumors swirled in Republican circles that Trump would be reinstated by the end of August. Though the institutional mechanism varied, the prediction always started with the seismic impact of soon-to-be-released evidence of voter fraud.[7] Public-facing polls reported widespread belief among Republicans that Trump would be reinstated.[8] The potential for these rumors could lead to violence sparked concern at the U.S. Departments of Homeland Security and Justice, as well as the Federal Bureau of Investigation (FBI).[9]

To leverage this second set of circumstances, we recruited a sample of 4,885 subjects, including 2,278 Democrats and 2,064 Republicans (Survey 3, Table 1). Respondents were first assigned to one of two experimental conditions (simple random assignment, p = 1/2). Those in the control (unincentivized) condition were asked, "Which statement is most likely to be true?" Those in the treatment (incentivized) condition were asked to select which of two tickets they would like to enter into a drawing for a $500 bonus, to be conducted on September 1, 2021. Both conditions had the same response options: "Donald Trump will be restored as President of the United States by the end of August." and "Donald Trump will **not** be restored as President of the United States by the end of August." This gives us a between-person experiment on the effect of financial incentives. Next, subjects initially assigned to the control condition were asked the treatment version of the question. This

[7]The most popular version held that the Supreme Court would reinstate Trump through unspecified institutional means. See Ewan Palmer, "Why Mike Lindell Thinks Donald Trump Will Return as President in August," *Newsweek*, June 3, 2021, and Jason Leman, "Mike Lindell Insists There Are 'Two Pathways' to Change 2020 Election Results," *Newsweek*, July 11, 2021. Another version held that following the emergence of evidence of fraud, the U.S. House of Representatives would remove its Speaker, Nancy Pelosi, and elect Trump in her place. This would have placed Trump second in the line of presidential succession and in position to lead the impeachment of those ahead of him in the line of succession. See Aliya Shob, "A 7-point plan to reinstate Donald Trump as president 'in days, not weeks' was handed out at CPAC," *Business Insider*, July 10, 2021.

[8]Eli Yokley, "29% of GOP Voters Say It's Likely Trump Will Be Reinstated as President This Year," *The Morning Consult*, June 10, 2021.

[9]For example, see Betsy Woodruff Swan, "DHS is concerned about Trump reinstatement conspiracy theory, top official says," *Politico*, June 25, 2021. Marshall Cohen, "Justice Department says Trump's reinstatement talk could fuel more violence from his supporters, *CNN*, July 9, 2021." Chris Strohm, "Conspiracy Theory About Trump Comeback Puts FBI on Alert for Violence," *Bloomberg*, July 13, 2021.

gives us a within-person estimate comparable to our first study.

To estimate the causal effect of the incentive treatment, we use OLS to estimate the parameters in

$$Y_i = \alpha + \beta \text{Incentive}_i + \epsilon_i, \tag{5}$$

where $\text{Incentive}_i$ indicates that the subject was randomly assigned to the financial incentive condition. We estimate the within-person design using the same strategy as our first study (Equation 4). This gives us two comparable sets of parameter estimates, one from the experiment and one from the within-person design. We estimate the effect on partisan differences by interacting each term with an indicator for Republican partisanship.[10]

We find little evidence of expressive responding among Republicans (Table 7). Among Republicans who were randomly assigned to the no-incentive condition, 14.9 percent predicted that Trump would be reinstated by the end of August. Among those assigned to the incentive condition, 14.5 percent made the same prediction, a difference of 0.4 percent (s.e. = 1.6; Table 7a, third column). When the no-incentive respondents were given an opportunity to make the same prediction with financial stakes, 13.8 predicted that Trump would be reinstated, a drop-off of 1.1 percent (s.e. = 0. 7; Table 7b, third column). This is on the borderline of statistical significance (one-tailed $p = 0.06$) and equal to less than 10 percent of the baseline. In sum, the between-person experiment yielded no statistically significant evidence of expressive responding among Republicans, while the within-person design suggested that a small amount of expressive responding may have been present.

Likewise, no evidence of expressive responding emerges among Democrats or independents. Among Democrats who were randomly assigned to the no-incentive condition, 7.6 percent predicted that Trump would be reinstated. In the incentive condition, the comparable figure was 8.3 percent (difference = 0.7, s.e. = 1.1). In the within-person test, the

[10]A detailed explanation of our strategy for examining partisan differences appears above in the "Honesty Encouragement" section.

Table 7: Betting on the future: Study 2.

(a) Experiment.

| | Dem. | Indep. | Repub. | Partisan diff. |
|---|---|---|---|---|
| Constant | $0.076^{**}$ | $0.042^{**}$ | $0.149^{**}$ | $0.076^{**}$ |
| | (0.008) | (0.013) | (0.011) | (0.008) |
| Incentive treatment | 0.007 | 0.007 | −0.004 | 0.007 |
| | (0.011) | (0.018) | (0.016) | (0.011) |
| Republican | | | | $0.073^{**}$ |
| | | | | (0.014) |
| Republican × incentive treatment | | | | −0.011 |
| | | | | (0.019) |
| Adj. $R^2$ | −0.000 | −0.002 | −0.000 | 0.011 |
| Num. obs. | 2278 | 543 | 2064 | 4342 |

(b) Within subjects.

| | Dem. | Indep. | Repub. | Partisan diff. |
|---|---|---|---|---|
| Constant | $0.076^{**}$ | $0.042^{**}$ | $0.149^{**}$ | $0.076^{**}$ |
| | (0.008) | (0.013) | (0.011) | (0.008) |
| Incentive question | −0.003 | 0.019 | −0.011 | −0.003 |
| | (0.005) | (0.012) | (0.007) | (0.005) |
| Republican | | | | $0.073^{**}$ |
| | | | | (0.014) |
| Republican × incentive question | | | | −0.009 |
| | | | | (0.009) |
| Adj. $R^2$ | −0.000 | −0.000 | −0.000 | 0.012 |
| Num. obs. | 2248 | 518 | 2096 | 4344 |
| Num. clusters | 1124 | 259 | 1048 | 2172 |

*Note:* Part (a) displays estimates of equation (5) with robust standard errors in parentheses. Part (b) displays estimates of equation (4) with clustered standard errors in parentheses. Fourth column interacts all terms with a Republican indicator; independents are excluded from this column. One-sided tests pre-registered. $^{*}p < 0.05$, $^{**}p < 0.01$.

share predicting a Trump reinstatement dropped to 7.3 percent (difference = -0.3, s.e. = 0.5). Among independents in the no-incentive condition, 4.2 percent predicted a Trump reinstatement. The comparable figure in the incentive condition was 4.9 percent (difference = 0.7, s.e. = 1.8); in the within-person test, 6.1 percent (difference = 1.9, s.e. = 1.2).

We similarly find little evidence of expressive responding when examining partisan differences. Relative to the baseline, the experimental estimate indicates that the partisan difference was 1.1 percentage points smaller in the incentive condition (s.e. = 1.9). The within-person estimate indicates that the difference was 0.9 percentage points smaller (s.e. = 0.9). Neither estimate is statistically significant.

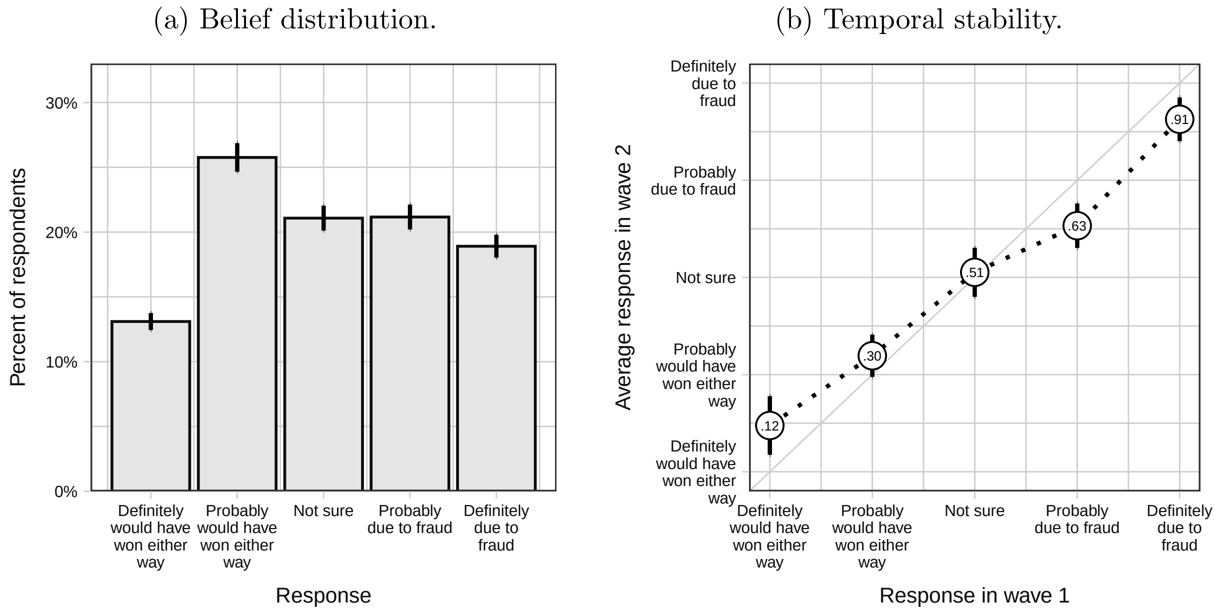
In sum, we find limited evidence for expressive responding in the case of Trump's bid for reinstatement in August 2021. Our strongest evidence in favor of expressive responding in this case is one estimate that is substantively small (less than 10 percent of the baseline) and falls on the borderline of statistical significance.

## Confidence and Temporal Stability

Though we find limited evidence of expressive responding, there is another sense in which survey respondents may not "believe" their answers: they may make guesses even when they lack confidence in them. Research on political misperceptions holds that identifying true believers requires measuring respondents' confidence in their answers (Kuklinski et al. 2000; Pasek et al. 2015; Graham 2020). Even this step is often insufficient: respondents who endorse falsehoods tend to be unstable in their responses over time, even when they at first report a high level of confidence in their answer (Graham 2022). To examine the degree to which our apparently sincere respondents believe their answers, this section presents a supplemental analysis that takes advantage of Survey 2's two-wave design.[11] We find that Republicans who endorse the big lie are about evenly split between individuals who confidently believe it and individuals who find it plausible, but are not deeply convinced.

[11] The analysis in this section was not pre-registered, but is modelled after Graham's (2022) critique.

Figure 1: Measured belief and temporal stability among Republicans, Survey 2.




*Note:* Panel (a) displays the distribution of responses from Study 2's baseline survey. Panel (b) displays the average response in the second survey conditional on the response in the first survey, using only data for the control group. Vertical bars represent 95 percent confidence intervals. Appendix F presents these results in tabular form.

To begin, we examine the pre-treatment distribution of Republican respondents' answers to the survey item designed to measure belief in the big lie (Figure 1a). Recall that respondents answered on a five-point scale labelled "Definitely would have won either way," "Probably would have won either way," "Not sure," "Probably due to fraud," and "Definitely due to fraud." Even when included in bipolar scales of this kind, probabilistic scale point labels capture meaningful information about respondent confidence (Graham 2022). Republicans who endorsed the big lie were about evenly split between the "probably due to fraud" and "definitely due to fraud" categories. In other words, about half of the individuals who endorse the big lie are confident that it is definitely true, while the other half find it plausible but are not deeply convinced.

Before placing too much weight on the meaning of these scale point labels, it is important to examine respondents' degree of commitment to their answers. To do so, we adopt Graham's (2022) strategy of examining the average response in wave 2 of a panel survey, conditional on the wave 1 response (Figure 1b).[12] To the degree that respondents indicate the same belief in both waves (as indicated by the 45-degree line), one can take respondents' claims to be confident in their answers at face value. For this step of the analysis, we only use the experimental control group.[13]

We find a high degree of response stability in measured belief in Trump's big lie. Figure 1b shows that on average, respondents to the second wave of the survey held very close to the beliefs they reported in the first wave. Among respondents who said in wave 1 that Biden's victory was "definitely due to fraud," the average scale score in wave 2 was 0.91. This is closer to "definitely due to fraud" (1) than it is to "probably due to fraud" (0.75). This estimate represents a substantially greater degree of temporal stability than appears in any of the measures of misperceptions examined by Graham (2022). Similarly, among respondents who said in wave 1 that Biden's victory was "probably due to fraud," the average scale score in wave 2 was 0.63, solidly above the 0.5 score that indicates complete uncertainty. This indicates that measurement error makes only a modest contribution to Republicans' measured confidence in Trump's big lie.

For Democrats and independents, Appendix F presents equivalent estimates of confidence and response stability. Independents are similar to Republicans in their degree of response stability. By contrast, Democrats who endorse the big lie in wave 1 tend to reject it in wave 2, exhibiting substantially less stability than Republicans. The typical Democrat who at first says that the big lie is "probably" or "definitely" true switches to saying that it is probably false. This indicates that the appearance that some Democrats have confidently accepted the big lie arises largely due to measurement error.

In Appendix F, we use data from the 2020 ANES Social Media Study to benchmark belief in the big lie against Trump's previously most notorious claim of election fraud, that millions of illegal votes were cast in the 2016 election. We find substantially less response

[12]Figure 1b is modelled after the left panel of Graham's (2022) Figure 1.

[13]The wave 2 responses were collected after an experimental treatment. Though we found little evidence of expressive responding using these data, we think it is prudent to avoid calculating descriptive statistics using post-treatment data.

stability among Republicans who endorse that claim (Figure F.1). For example, Republicans who endorsed the 2016 claim with complete confidence in wave 1 had an average wave 2 scale score of 0.64. The big lie appears distinct from most measures of misperceptions—including Trump's previous claims of voter fraud—in that many people have outright accepted the falsehood at hand, as opposed to finding it plausible.

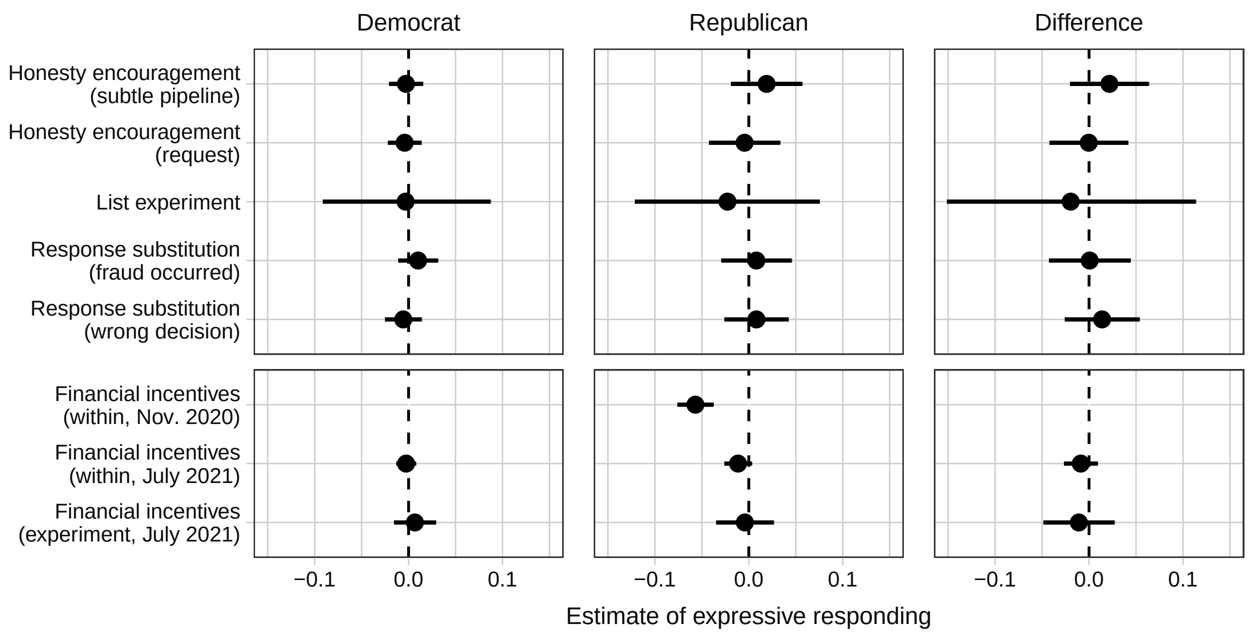
## Summary

The evidence presented above constitutes a thorough investigation into the possibility that survey respondents who endorse the big lie do not really believe it. Our four-pronged strategy for detecting expressive responding yields minimal evidence that survey respondents are being insincere when they endorse the big lie, and a supplemental analysis found that about half of the big lie's endorsers believe in it confidently. In this section, we present a holistic summary of this evidence.

To summarize our evidence for expressive responding, Figure 2 plots all of our estimates of the prevalence of expressive responding among both Democrats and Republicans. As all of our items were scaled to range between 0 and 1, we report each estimate in its natural units, without any further standardization. We refrain from combining the estimates into a meta-analysis for two reasons. First, because the response substitution and list experiment estimates are tests for specific pathways through which expressive responding might be occurring, average of the three would not represent an estimate of the total prevalence of expressive responding. Second, whereas typical meta-analytic approaches assume independence between estimates, some of our estimates share the same control group.

Our primary interest was in expressive responding among Republicans. Across our eight estimates, only one yields statistically significant evidence consistent with the expressive responding hypothesis (Figure 2, center panel). That estimate suggested that about 5.7 percent of Republicans misreported their best guess about whether Trump would be able to hang onto power after the 2020 election. This was equal to about 20 percent of the baseline

Figure 2: Summary of evidence for expressive responding.




rate. The other seven estimates, all based on data collected in 2021, were substantively small (2.3 percentage points or less) and statistically insignificant. We think the phrase "limited at best" fairly captures this pattern of results. Not all of our estimates are null, but they suggest that at most, expressive responding is confined to a relatively small minority of those who endorse the big lie.

The summary figure also includes estimates for Democrats and partisan differences (Figure 2, left and right panels). Across the fourteen estimates, none were statistically significant. Though one would expect weaker pressure to respond expressively among Democrats than among Republicans, these null findings provide some further evidence for the limited prevalence of expressive responding.

Finding little evidence of expressive responding, we turned to the question of whether Republicans endorse the big lie with confidence and remain temporally stable in their endorsements. We found that about half of Republicans who endorse the big lie are confident that it is true, and that these respondents are stable in their professions of confidence. This outperforms other measures of misperceptions, including measured belief in Trump's lies about 2016 election fraud.

## Implications

Our evidence suggests that by and large, survey respondents are being sincere when they endorse "big lie" that Trump lost the 2020 presidential election due to voter fraud. These beliefs are split between confident acceptance and sincere suspicions (i.e., guesses by people who are not deeply convinced of the big lie, but think it is more likely than not to be true).

Our findings highlight the misleading nature of one-size-fits-all interpretations of survey data. Though we found minimal evidence of expressive responding in this case, our findings do not indicate that other research that finds stronger evidence of expressive responding is flawed. Instead, it suggests that the presence or absence of expressive responding varies from topic to topic. This echoes recent work on measures of political misperceptions (Graham 2022) as well as a much longer tradition of examining variation in response stability across attitudinal items (Converse 1964; Dodd and Svalastoga 1952; Schuman and Presser 1981). Measures of beliefs need to be validated at the item level before they can be fully trusted. Absent such evidence, researchers should refrain from strong judgments about the veracity with which respondents believe their answers. In this sense, observers have been right to worry that expressive responding *could* influence measured belief in the big lie, even as our evidence suggests that it does not.

To promote nuanced, evidence-based interpretation of survey responses, the research community should consider making a standard practice out of the sort of comprehensive validation exercise we have carried out here. Rather than "going wide" by asking ten or twenty different questions about the same topic, researchers should "go deep" by allocating resources toward understanding the measures they consider to be most important. At present, such evaluations tend to be carried out in reaction to prominent narratives in public opinion discourse, leaving hard evidence in the uncomfortable position of having to confirm or correct the narrative. Ideally, prevailing interpretations of survey data would be informed by validation exercises. Today, the relationship is precisely the opposite. Strong interpretations of survey data serve as the motivation for validation exercises, functioning as a precondition for the collection of hard evidence rather than emerging as a function of that evidence.

Our betting on the future design highlights another benefit to proactive validation: the ability to leverage fleeting, context-specific opportunities. In our case, prominent predictions about the big lie's implications for future events provided an opportunity to get around a critical limitation of the typical financial incentive design, the need for researcher and respondent to share a common ground for determining which answer is correct. Our ability to do so was time-bound: what was then the future has become the past. Some of the most valuable evidence for expressive responding shares. Schaffner and Luks (2018) struck the case of Trump's false claims about his 2016 inauguration crowd while that iron was hot, taking advantage of the combination of photographic evidence and a fleeting window of media attention. This produced unique and convincing evidence for expressive responding. By contrast, Allcott et al. (2020) found that amid the initial COVID-19 outbreak March 2020, financial incentives had no effect on the difference between Democrats' and Republicans' predictions about how many cases would occur in April, suggesting that early differences in threat perception were sincere. Yet the recent past also contains missed opportunities. For example, the early veracity of "birther" beliefs could have been probed through bets on whether then-President Barack Obama would release his birth certificate. Similarly, before Robert Mueller's investigation into the 2016 election was politicized, predictions about its outcome could have been leveraged as a way to test the veracity of Democrats' and Republicans' belief differences over Russia's involvement in the election and with Trump. Greater emphasis on proactive validation would enable the research community to spot and seize more opportunities for gauging whether partisan proclivities contrast with strong accuracy motivations.

The evidence presented here also highlights the benefits of integrating tests for expressive response, confidence, and response stability into the same validation exercise. Although our primary interest was in expressive responding, we designed our surveys in a manner that was mindful of the multiple senses in which a survey question may fail to measure a respondent's beliefs. Consequently, after determining that expressive responding was unlikely to be an important influence on our data, we were able to conduct supplemental tests that spoke to other concerns.

Though our evidence is internally consistent, it leaves room for further work on measured belief in Trump's big lie. Methods not tested here have shown promise, including the use of functional magnetic resonance imaging (fMRI) technology (Leong et al. 2019). Resource constraints limited us to a single survey vendor; though treatment effects do typically generalize across samples (Mullinix et al. 2016; Coppock et al. 2018; Coppock and McClellan 2019), there is always room for out-of-sample replication. Replication of our strategies could support an eventual meta-analysis, allowing researchers to draw a finer distinction between small amounts of expressive responding and an absence altogether. Through our use of the words "limited" and "minimal," we have been intentionally vague in this respect.

In sum, by providing the most comprehensive evidence to date that measured belief in Trump's big lie is minimally affected by expressive responding, our effort highlights the benefits of comprehensive approaches to case-by-case validation of survey items. By combining multiple approaches and taking advantage of contextual factors, validation exercises can support rigorous, topic-specific interpretation of polling about the most important issues of the day.

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*Appendix to*

# Expressive Responding and Trump's Big Lie

## Contents

# A Survey Information

### Survey 1

*Platform:* MTurk.

*Dates:* November 28-30, 2020.

*Sample size:* 1,049.

*Screeners:* Captcha verification.

*Consent:* Subjects read an IRB-approved consent form, then voluntarily consented to participate in a research study.

*Refusal rate:* 0 percent.

*Compensation:* \$0.50. As the vendor does not set any standard rate, this amount was chosen to exceed the hourly minimum wage in the United States.

*Preanalysis plan:* None.

### Survey 2

*Platform:* MTurk.

*Dates:* May 10-23, 2021.

*Sample size:* 3,599 (first wave); 2,958 (second wave).

*Screeners:* Captcha verification.

*Consent:* Subjects read an IRB-approved consent form, then voluntarily consented to participate in a research study.

*Refusal rate:* 0.2 percent.

*Compensation:* \$0.60. As the vendor does not set any standard rate, this amount was chosen to exceed the hourly minimum wage in the United States.

*Preanalysis plan:* An anonymous version of our preanalysis plan is publicly available at https://aspredicted.org/blind.php?x=GDZ_UAN. The pre-registered hypotheses are tested in the following locations:

- *Hypothesis 1: partisan differences.* Tested in main text Table 5. Robustness check in Table D.3.
- *Hypothesis 1a: expressive responding by Democrats.* Tested in main text Table 5.

Robustness check in Table D.3.

- *Hypothesis 1b: expressive responding by Republicans.* Tested in main text Table 5. Robustness check in Table D.3.

- *Hypothesis 2: comparison between treatments.* Tested in Table D.5.

The preanalysis plan applies these hypotheses to two additional topic areas. These will be examined in a separate manuscript. As we note, "[t]hough we may examine these topics in separate manuscripts, we are registering them together because the studies all draw on the same theoretical framework."

**Survey 3**

*Platform:* MTurk.

*Dates:* July 7-31, 2021.

*Sample size:* 4,885.

*Screeners:* Captcha verification.

*Consent:* Subjects read an IRB-approved consent form, then voluntarily consented to participate in a research study.

*Refusal rate:* 0.4 percent.

*Compensation:* $0.50. As the vendor does not set any standard rate, this amount was chosen to exceed the hourly minimum wage in the United States.

*Preanalysis plan:* An anonymous version of our preanalysis plan is publicly available at https://aspredicted.org/blind.php?x=HSX_CLG. The pre-registered hypotheses are tested in the following locations:

- *Hypothesis 1: list experiment reduces Republicans' measured belief in the big lie.* Tested in main text Table 4.

- *Hypothesis 1a: list experiment reduces partisan differences in the big lie.* Tested in main text Table 4.

- *Hypothesis 2: financial incentives reduce Republicans' tendency to predict Trump's reinstatement.* Tested in main text Table 7a and 7b.

- *Hypothesis 2a: financial incentives reduce partisan differences regarding Trump's reinstatement.* Tested in main text Table 7a and 7b.

The preanalysis plan applies these hypotheses to two additional topic areas. These will be examined in a separate manuscript. As we note, "[t]hough we may examine these topics in separate manuscripts, we are registering them together because the studies all draw on the same theoretical framework."

We deviated from the PAP in two respects. First, for the list experiment, the PAP states that we will use block random assignment. However, this failed when we launched the survey. As a backup, the survey automatically reverted to using simple random assignment. Second, for betting on the future, the PAP states that we will conduct the within-person analysis by scoring respondents as a -1, 0, or 1, then taking the average. We instead used a different procedure that yields identical estimates (see Appendix E.2).

**Survey 4**

*Platform:* MTurk.

*Dates:* September 22-23, 2021.

*Sample size:* 5,005.

*Screeners:* Captcha verification.

*Consent:* Subjects read an IRB-approved consent form, then voluntarily consented to participate in a research study.

*Refusal rate:* 0.1 percent.

*Compensation:* $0.75. As the vendor does not set any standard rate, this amount was chosen to exceed the hourly minimum wage in the United States.

*Preanalysis plan:* An anonymous version of our preanalysis plan is publicly available at https://aspredicted.org/blind.php?x=7YB_LLV. The pre-registered hypotheses are tested in the following locations:

- *Hypothesis 1: honesty encouragement reduces Republicans' measured belief in the big lie.* Tested in main text Table 2.
- *Hypothesis 1a:honesty encouragement reduces partisan differences in belief in the big lie.* Tested in main text Table 2.

# B Honesty Encouragement

## B.1 Balance tests

Table B.1: Balance test, request treatment versus control.

| Variable | Z=0 | Z=1 | Diff | SE | Std. Diff | z | p |
|---|---|---|---|---|---|---|---|
| white | 0.786 | 0.780 | -0.006 | 0.014 | -0.014 | -0.417 | 0.676 |
| black | 0.131 | 0.143 | 0.012 | 0.012 | 0.035 | 1.017 | 0.309 |
| asian | 0.060 | 0.061 | 0.001 | 0.008 | 0.004 | 0.112 | 0.911 |
| hispanic | 0.000 | 0.000 | 0.000 | 0.000 | NaN | 0.000 | 1.000 |
| female | 0.454 | 0.434 | -0.020 | 0.017 | -0.040 | -1.149 | 0.251 |
| pid7 | 3.340 | 3.355 | 0.015 | 0.084 | 0.006 | 0.180 | 0.857 |
| educ_n | 0.707 | 0.701 | -0.005 | 0.006 | -0.030 | -0.858 | 0.391 |
| age | 39.323 | 38.997 | -0.326 | 0.413 | -0.027 | -0.789 | 0.430 |
| female(missing) | 0.001 | 0.000 | -0.001 | 0.001 | -0.034 | -0.997 | 0.319 |
| educ_n(missing) | 0.230 | 0.218 | -0.012 | 0.014 | -0.029 | -0.829 | 0.407 |

Overall: Chi-squared statistic= 6.071(df=9,p=0.733)

Table B.2: Balance test, pipeline treatment versus control.

| Variable | Z=0 | Z=1 | Diff | SE | Std. Diff | z | p |
|---|---|---|---|---|---|---|---|
| white | 0.786 | 0.782 | -0.004 | 0.014 | -0.009 | -0.253 | 0.800 |
| black | 0.131 | 0.134 | 0.003 | 0.012 | 0.009 | 0.248 | 0.804 |
| asian | 0.060 | 0.058 | -0.002 | 0.008 | -0.008 | -0.234 | 0.815 |
| hispanic | 0.000 | 0.000 | 0.000 | 0.000 | NaN | 0.000 | 1.000 |
| female | 0.454 | 0.437 | -0.017 | 0.017 | -0.033 | -0.966 | 0.334 |
| pid7 | 3.340 | 3.377 | 0.037 | 0.084 | 0.015 | 0.438 | 0.662 |
| educ_n | 0.707 | 0.704 | -0.003 | 0.006 | -0.017 | -0.486 | 0.627 |
| age | 39.323 | 39.292 | -0.031 | 0.412 | -0.003 | -0.075 | 0.940 |
| female(missing) | 0.001 | 0.000 | -0.001 | 0.001 | -0.034 | -0.996 | 0.319 |
| educ_n(missing) | 0.230 | 0.222 | -0.008 | 0.014 | -0.018 | -0.531 | 0.595 |

Overall: Chi-squared statistic= 2.787(df=9,p=0.972)

Note: Diff refers to the difference in means for a covariate. SE denotes the standard error of the difference in means. Std. Diff refers to the standardized difference in means.

### B.2 Supplemental results

Table B.3: Estimates with treatment conditions pooled, honesty encouragement.

| | Dem. | Indep. | Repub. | Partisan Diff. |
|---|---|---|---|---|
| Constant | $0.148^{**}$ | $0.314^{**}$ | $0.404^{**}$ | $0.148^{**}$ |
| | (0.006) | (0.033) | (0.013) | (0.006) |
| Either treatment | $-0.004$ | 0.014 | 0.007 | $-0.004$ |
| | (0.008) | (0.040) | (0.017) | (0.008) |
| Republican | | | | $0.256^{**}$ |
| | | | | (0.015) |
| Republican × either treatment | | | | 0.011 |
| | | | | (0.018) |
| Adj. $R^2$ | $-0.000$ | $-0.003$ | $-0.000$ | 0.197 |
| Num. obs. | 2969 | 294 | 1737 | 4706 |

*Note:* Table replicates main text Table 2 with the two treatment conditions pooled. Robust standard errors in parentheses. One tailed tests preregistered. $^{*}p < 0.05$, $^{**}p < 0.01$.

# C List Experiment

## C.1 Balance tests

Table C.1: Balance test, treatment versus control lists.

| Variable | Z=0 | Z=1 | Diff | SE | Std. Diff | z | p |
|---|---|---|---|---|---|---|---|
| white | 0.717 | 0.710 | -0.006 | 0.015 | -0.014 | -0.442 | 0.659 |
| black | 0.091 | 0.087 | -0.005 | 0.009 | -0.017 | -0.525 | 0.600 |
| asian | 0.068 | 0.071 | 0.003 | 0.008 | 0.011 | 0.336 | 0.737 |
| hispanic | 0.060 | 0.059 | -0.001 | 0.008 | -0.004 | -0.127 | 0.899 |
| female | 0.528 | 0.528 | 0.001 | 0.016 | 0.001 | 0.033 | 0.973 |
| pid7 | 4.232 | 4.090 | -0.142 | 0.072 | -0.063 | -1.977 | 0.048 |
| educ_n | 0.542 | 0.543 | 0.001 | 0.006 | 0.005 | 0.164 | 0.870 |
| age | 41.555 | 41.135 | -0.420 | 0.412 | -0.033 | -1.021 | 0.307 |
| hispanic(missing) | 0.035 | 0.045 | 0.010 | 0.006 | 0.050 | 1.542 | 0.123 |
| female(missing) | 0.004 | 0.005 | 0.002 | 0.002 | 0.024 | 0.749 | 0.454 |
| pid7(missing) | 0.002 | 0.002 | 0.001 | 0.001 | 0.013 | 0.391 | 0.696 |
| educ_n(missing) | 0.227 | 0.212 | -0.015 | 0.013 | -0.037 | -1.157 | 0.247 |
| age(missing) | 0.031 | 0.040 | 0.009 | 0.006 | 0.046 | 1.445 | 0.148 |

Overall: Chi-squared statistic= 12.546(df=13,p=0.483)

Table C.2: Balance test, treatment list versus direct question.

| Variable | Z=0 | Z=1 | Diff | SE | Std. Diff | z | p |
|---|---|---|---|---|---|---|---|
| white | 0.696 | 0.710 | 0.014 | 0.018 | 0.032 | 0.815 | 0.415 |
| black | 0.087 | 0.087 | 0.000 | 0.011 | -0.001 | -0.031 | 0.975 |
| asian | 0.088 | 0.071 | -0.017 | 0.010 | -0.063 | -1.633 | 0.102 |
| hispanic | 0.057 | 0.059 | 0.002 | 0.009 | 0.008 | 0.199 | 0.842 |
| female | 0.520 | 0.528 | 0.008 | 0.019 | 0.017 | 0.426 | 0.670 |
| pid7 | 4.111 | 4.090 | -0.020 | 0.087 | -0.009 | -0.235 | 0.815 |
| educ_n | 0.551 | 0.543 | -0.009 | 0.007 | -0.050 | -1.290 | 0.197 |
| age | 41.500 | 41.135 | -0.365 | 0.487 | -0.029 | -0.750 | 0.453 |
| hispanic(missing) | 0.043 | 0.045 | 0.001 | 0.008 | 0.005 | 0.141 | 0.888 |
| female(missing) | 0.012 | 0.005 | -0.007 | 0.003 | -0.077 | -1.994 | 0.046 |
| pid7(missing) | 0.000 | 0.002 | 0.002 | 0.001 | 0.056 | 1.450 | 0.147 |
| educ_n(missing) | 0.245 | 0.212 | -0.033 | 0.016 | -0.079 | -2.036 | 0.042 |
| age(missing) | 0.032 | 0.040 | 0.008 | 0.007 | 0.044 | 1.134 | 0.257 |

Overall: Chi-squared statistic= 16.987(df=13,p=0.2)

Note: Diff refers to the difference in means for a covariate. SE denotes the standard error of the difference in means. Std. Diff refers to the standardized difference in means.

Table C.3: Balance test, control list versus direct question.

| Variable | Z=0 | Z=1 | Diff | SE | Std. Diff | z | p |
|---|---|---|---|---|---|---|---|
| white | 0.696 | 0.717 | 0.021 | 0.018 | 0.046 | 1.184 | 0.236 |
| black | 0.087 | 0.091 | 0.004 | 0.011 | 0.016 | 0.403 | 0.687 |
| asian | 0.088 | 0.068 | -0.020 | 0.010 | -0.075 | -1.923 | 0.054 |
| hispanic | 0.057 | 0.060 | 0.003 | 0.009 | 0.012 | 0.304 | 0.761 |
| female | 0.520 | 0.528 | 0.008 | 0.019 | 0.015 | 0.399 | 0.690 |
| pid7 | 4.111 | 4.232 | 0.121 | 0.086 | 0.055 | 1.408 | 0.159 |
| educ_n | 0.551 | 0.542 | -0.010 | 0.007 | -0.055 | -1.419 | 0.156 |
| age | 41.500 | 41.555 | 0.056 | 0.497 | 0.004 | 0.112 | 0.911 |
| hispanic(missing) | 0.043 | 0.035 | -0.009 | 0.007 | -0.045 | -1.156 | 0.247 |
| female(missing) | 0.012 | 0.004 | -0.008 | 0.003 | -0.103 | -2.669 | 0.008 |
| pid7(missing) | 0.000 | 0.002 | 0.002 | 0.001 | 0.048 | 1.249 | 0.212 |
| educ_n(missing) | 0.245 | 0.227 | -0.018 | 0.016 | -0.041 | -1.069 | 0.285 |
| age(missing) | 0.032 | 0.031 | 0.000 | 0.007 | -0.002 | -0.043 | 0.966 |

Overall: Chi-squared statistic= 16.659(df=12,p=0.163)

Note: Diff refers to the difference in means for a covariate. SE denotes the standard error of the difference in means. Std. Diff refers to the standardized difference in means.

# D Response Substitution

## D.1 Balance tests

Table D.1: Balance test, fraud occurred treatment versus control.

| Variable | Z=0 | Z=1 | Diff | SE | Std. Diff | z | p |
|---|---|---|---|---|---|---|---|
| white | 0.730 | 0.763 | 0.033 | 0.020 | 0.075 | 1.661 | 0.097 |
| black | 0.121 | 0.103 | -0.018 | 0.014 | -0.056 | -1.250 | 0.211 |
| asian | 0.113 | 0.107 | -0.007 | 0.014 | -0.021 | -0.464 | 0.642 |
| hispanic | 0.129 | 0.128 | -0.001 | 0.015 | -0.002 | -0.035 | 0.972 |
| female | 0.468 | 0.496 | 0.029 | 0.023 | 0.057 | 1.268 | 0.205 |
| pid7 | 4.857 | 4.651 | -0.206 | 0.098 | -0.095 | -2.104 | 0.035 |
| educ_n | 0.703 | 0.697 | -0.006 | 0.011 | -0.023 | -0.518 | 0.605 |
| age | 39.307 | 40.300 | 0.993 | 0.554 | 0.081 | 1.792 | 0.073 |
| hispanic(missing) | 0.001 | 0.000 | -0.001 | 0.001 | -0.045 | -1.002 | 0.316 |
| female(missing) | 0.004 | 0.006 | 0.002 | 0.003 | 0.028 | 0.627 | 0.530 |
| educ_n(missing) | 0.016 | 0.017 | 0.001 | 0.006 | 0.007 | 0.164 | 0.870 |
| age(missing) | 0.002 | 0.000 | -0.002 | 0.001 | -0.064 | -1.417 | 0.156 |

Overall: Chi-squared statistic= 13.69(df=12,p=0.321)

Table D.2: Balance test, wrong decision treatment versus control.

| Variable | Z=0 | Z=1 | Diff | SE | Std. Diff | z | p |
|---|---|---|---|---|---|---|---|
| white | 0.730 | 0.771 | 0.040 | 0.020 | 0.093 | 2.061 | 0.039 |
| black | 0.121 | 0.093 | -0.028 | 0.014 | -0.090 | -1.991 | 0.046 |
| asian | 0.113 | 0.102 | -0.011 | 0.014 | -0.037 | -0.817 | 0.414 |
| hispanic | 0.129 | 0.136 | 0.007 | 0.015 | 0.020 | 0.449 | 0.653 |
| female | 0.468 | 0.480 | 0.012 | 0.023 | 0.025 | 0.545 | 0.586 |
| pid7 | 4.857 | 4.607 | -0.250 | 0.099 | -0.115 | -2.540 | 0.011 |
| educ_n | 0.703 | 0.698 | -0.005 | 0.011 | -0.020 | -0.452 | 0.651 |
| age | 39.307 | 39.984 | 0.676 | 0.542 | 0.056 | 1.248 | 0.212 |
| hispanic(missing) | 0.001 | 0.000 | -0.001 | 0.001 | -0.045 | -1.001 | 0.317 |
| female(missing) | 0.004 | 0.002 | -0.002 | 0.002 | -0.037 | -0.820 | 0.412 |
| educ_n(missing) | 0.016 | 0.016 | 0.000 | 0.006 | 0.000 | -0.006 | 0.995 |
| age(missing) | 0.002 | 0.000 | -0.002 | 0.001 | -0.064 | -1.416 | 0.157 |

Overall: Chi-squared statistic= 14.615(df=12,p=0.263)

Note: Diff refers to the difference in means for a covariate. SE denotes the standard error of the difference in means. Std. Diff refers to the standardized difference in means.

### D.2 Additional results

This section contains the following tables, all of which are versions of main text Table 5.

- Table D.3 presents estimates without covariate adjustment.
- Table D.4 presents estimates with the treatment conditions pooled.
- Table D.5 compares the two response substitution treatments to one another. The reference group is the fraud occurred treatment.

Table D.3: Response substitution estimates without covariate adjustment.

| | Dem. | Indep. | Repub. | Partisan Diff. |
|---|---|---|---|---|
| Constant | 0.128** | 0.356** | 0.501** | 0.128** |
| | (0.008) | (0.033) | (0.020) | (0.008) |
| Fraud occurred treatment | 0.006 | −0.074 | 0.034 | 0.006 |
| | (0.013) | (0.048) | (0.028) | (0.013) |
| Wrong decision treatment | −0.013 | 0.020 | 0.021 | −0.013 |
| | (0.012) | (0.048) | (0.028) | (0.012) |
| Republican | | | | 0.373** |
| | | | | (0.022) |
| Republican × fraud occurred treatment | | | | 0.027 |
| | | | | (0.031) |
| Republican × wrong decision treatment | | | | 0.033 |
| | | | | (0.030) |
| Adj. $R^2$ | 0.000 | 0.008 | −0.001 | 0.331 |
| Num. obs. | 1745 | 268 | 929 | 2674 |

*Note:* Table replicates main text Table 5 without covariate adjustment (i.e., dropping the pre-treatment measure of the dependent variable). $^{*}p < 0.05$, $^{**}p < 0.01$.

Table D.4: Estimates with treatment conditions pooled, response substitution.

| | Dem. | Indep. | Repub. | Partisan Diff. |
|---|---|---|---|---|
| Constant | 0.130** | 0.355** | 0.506** | 0.034** |
| | (0.007) | (0.019) | (0.014) | (0.007) |
| Either treatment | 0.002 | −0.036 | 0.008 | 0.004 |
| | (0.009) | (0.023) | (0.016) | (0.009) |
| Republican | | | | 0.117** |
| | | | | (0.018) |
| Republican × either treatment | | | | 0.007 |
| | | | | (0.019) |
| Pre-treatment DV | 0.553** | 0.875** | 0.792** | 0.684** |
| | (0.029) | (0.032) | (0.021) | (0.019) |
| Adj. $R^2$ | 0.323 | 0.714 | 0.587 | 0.640 |
| Num. obs. | 1744 | 267 | 929 | 2673 |

*Note:* Table replicates main text Table 5 with the two treatment conditions pooled. Robust standard errors in parentheses. One tailed tests preregistered. $^{*}p < 0.05$, $^{**}p < 0.01$.

Table D.5: Response substitution: test for differences between treatments.

| | Dem. | Indep. | Repub. | Partisan Diff. |
|---|---|---|---|---|
| Constant | 0.133** | 0.313** | 0.528** | 0.041** |
| | (0.008) | (0.020) | (0.013) | (0.008) |
| Wrong decision treatment | −0.016 | 0.041 | 0.000 | −0.015 |
| | (0.010) | (0.026) | (0.017) | (0.010) |
| Republican | | | | 0.105** |
| | | | | (0.019) |
| Republican × wrong decision treatment | | | | 0.013 |
| | | | | (0.020) |
| Pre-treatment DV | 0.592** | 0.889** | 0.806** | 0.713** |
| | (0.036) | (0.037) | (0.025) | (0.022) |
| Adj. $R^2$ | 0.359 | 0.741 | 0.598 | 0.663 |
| Num. obs. | 1131 | 180 | 650 | 1781 |

*Note:* Table tests for differences in effects between the two response substitution treatments. In all other respects it is comparable to main text Table 5. $^{*}p < 0.05$, $^{**}p < 0.01$.

# E Betting on the Future

## E.1 Balance test

Table E.1: Balance test, betting on the future study 2, treatment versus control.

| Variable | Z=0 | Z=1 | Diff | SE | Std. Diff | z | p |
|---|---|---|---|---|---|---|---|
| white | 0.705 | 0.715 | 0.010 | 0.013 | 0.022 | 0.773 | 0.439 |
| black | 0.088 | 0.089 | 0.001 | 0.008 | 0.004 | 0.136 | 0.892 |
| asian | 0.075 | 0.072 | -0.004 | 0.007 | -0.014 | -0.488 | 0.625 |
| hispanic | 0.066 | 0.052 | -0.013 | 0.007 | -0.056 | -1.973 | 0.049 |
| female | 0.529 | 0.524 | -0.005 | 0.014 | -0.010 | -0.335 | 0.738 |
| pid7 | 4.119 | 4.182 | 0.063 | 0.064 | 0.028 | 0.986 | 0.324 |
| educ_n | 0.543 | 0.545 | 0.002 | 0.005 | 0.010 | 0.354 | 0.724 |
| age | 41.419 | 41.337 | -0.083 | 0.364 | -0.006 | -0.226 | 0.821 |
| hispanic(missing) | 0.037 | 0.044 | 0.006 | 0.006 | 0.031 | 1.084 | 0.278 |
| female(missing) | 0.004 | 0.008 | 0.004 | 0.002 | 0.047 | 1.647 | 0.099 |
| pid7(missing) | 0.001 | 0.002 | 0.001 | 0.001 | 0.032 | 1.121 | 0.262 |
| educ_n(missing) | 0.212 | 0.237 | 0.025 | 0.012 | 0.059 | 2.060 | 0.039 |
| age(missing) | 0.033 | 0.037 | 0.004 | 0.005 | 0.020 | 0.710 | 0.478 |

Overall: Chi-squared statistic= 15.538(df=13,p=0.275)

## E.2 Additional results

In the main text, we use regression to estimate the difference between the unincentivized and incentivized questions, with errors clostered at the respondent level (Tables 6 and 7). However, our PAP states that we will estimate the difference by subtracting the two measures and taking the average. These procedures yield identical results. We verify this in Table F.2. The estimates are identical to the second row of Tables 6 and 7).

Table E.2: Average within-person change, betting on the future.

| Study | Party | Estimate | SE | t-stat |
|---|---|---|---|---|
| Study 1 | Neither | -0.018 | (0.012) | -1.421 |
| | Republican | -0.057 | (0.010) | -5.850 |
| Study 3 | Democrat | -0.003 | (0.005) | -0.507 |
| | Independent | 0.016 | (0.011) | 1.417 |
| | Republican | -0.012 | (0.007) | -1.550 |

# F Confidence and Temporal Stability

## F.1 Additional results

This section contains a tabular version of the estimates plotted in main text Figure 1, as well as the equivalent estimates for Democrats and independents.

Table F.1: Estimates displayed in Figure 1a.

| Party | Response | Estimate | SE | 95% CI | N |
|---|---|---|---|---|---|
| Rep. | Definitely would have won either way (0) | 0.131 | 0.003 | (0.124, 0.138) | 151 |
| | Probably would have won either way (0.25) | 0.258 | 0.006 | (0.247, 0.269) | 297 |
| | Not sure (0.5) | 0.211 | 0.005 | (0.201, 0.220) | 243 |
| | Probably due to fraud (0.75) | 0.212 | 0.005 | (0.202, 0.221) | 244 |
| | Definitely due to fraud (1) | 0.189 | 0.005 | (0.180, 0.198) | 218 |
| Dem. | Definitely would have won either way (0) | 0.648 | 0.005 | (0.639, 0.658) | 1374 |
| | Probably would have won either way (0.25) | 0.204 | 0.004 | (0.197, 0.211) | 432 |
| | Not sure (0.5) | 0.092 | 0.002 | (0.088, 0.096) | 195 |
| | Probably due to fraud (0.75) | 0.045 | 0.001 | (0.043, 0.047) | 96 |
| | Definitely due to fraud (1) | 0.010 | 0.000 | (0.010, 0.011) | 22 |
| Indep. | Definitely would have won either way (0) | 0.283 | 0.011 | (0.261, 0.305) | 92 |
| | Probably would have won either way (0.25) | 0.277 | 0.011 | (0.255, 0.299) | 90 |
| | Not sure (0.5) | 0.252 | 0.010 | (0.232, 0.273) | 82 |
| | Probably due to fraud (0.75) | 0.114 | 0.006 | (0.103, 0.125) | 37 |
| | Definitely due to fraud (1) | 0.074 | 0.004 | (0.066, 0.081) | 24 |

Table F.2: Estimates displayed in Figure 1b.

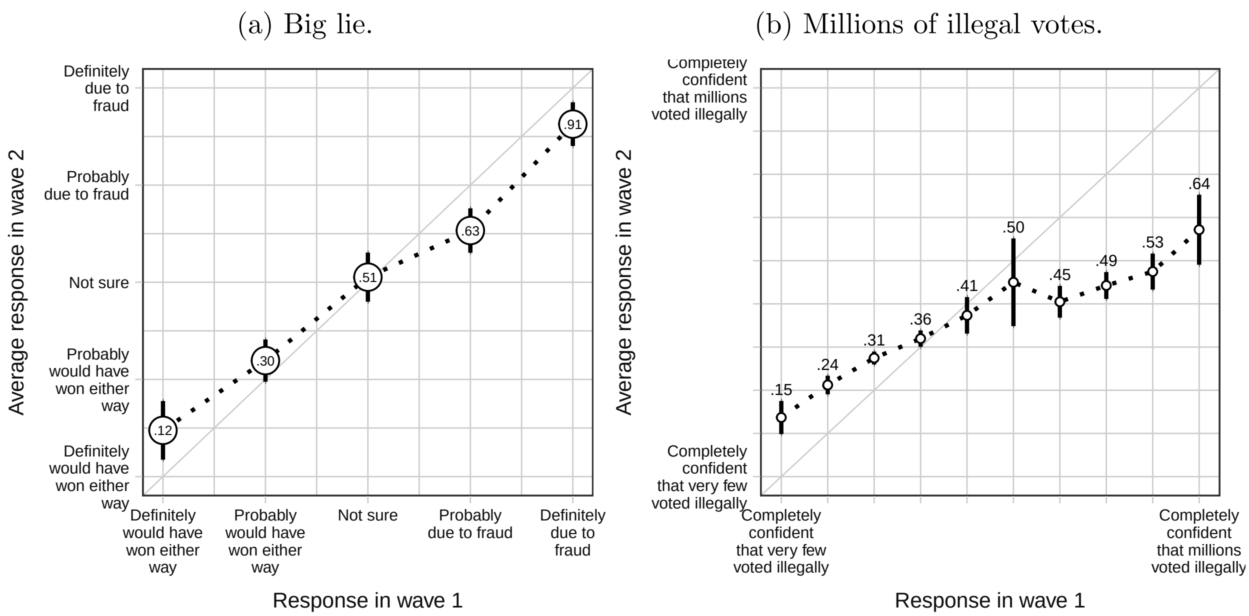
| Party | Response | Estimate | SE | 95% CI | N |
|---|---|---|---|---|---|
| Rep. | Definitely would have won either way (0) | 0.119 | 0.037 | (0.044, 0.195) | 42 |
| | Probably would have won either way (0.25) | 0.299 | 0.027 | (0.244, 0.353) | 67 |
| | Not sure (0.5) | 0.513 | 0.032 | (0.450, 0.576) | 57 |
| | Probably due to fraud (0.75) | 0.633 | 0.029 | (0.576, 0.690) | 62 |
| | Definitely due to fraud (1) | 0.907 | 0.028 | (0.851, 0.963) | 51 |
| Dem. | Definitely would have won either way (0) | 0.046 | 0.007 | (0.032, 0.060) | 398 |
| | Probably would have won either way (0.25) | 0.235 | 0.018 | (0.199, 0.271) | 133 |
| | Not sure (0.5) | 0.358 | 0.032 | (0.295, 0.421) | 51 |
| | Probably due to fraud (0.75) | 0.391 | 0.058 | (0.270, 0.513) | 23 |
| | Definitely due to fraud (1) | 0.219 | 0.088 | (0.012, 0.426) | 8 |
| Indep. | Definitely would have won either way (0) | 0.065 | 0.036 | (-0.009, 0.140) | 23 |
| | Probably would have won either way (0.25) | 0.269 | 0.039 | (0.189, 0.350) | 26 |
| | Not sure (0.5) | 0.467 | 0.033 | (0.400, 0.535) | 23 |
| | Probably due to fraud (0.75) | 0.687 | 0.078 | (0.502, 0.873) | 8 |
| | Definitely due to fraud (1) | 0.893 | 0.074 | (0.711, 1.075) | 7 |

### F.2 Comparison to 2016 fraud claim

To benchmark our results, we examined a topically similar false claim: Trump's allegation that millions of illegal votes were cast in the 2016 U.S. presidential election. A question designed to measure belief in this claim was included in the 2020 ANES Social Media Study. In both survey waves, the question asked, "Which of these do you think is most likely to be true?" with the response options, "Millions of people voted illegally in the 2016 election" and "Few people voted illegally in the 2016 election." Next, respondents were asked "How confident are you about that?" and presented with a 5-point scale with the labels "not at all," "a little," "moderately," "very," and "completely." We recoded these responses to a 0-1 scale where 0 represents completely confident rejection of the claim and 1 represents completely confident acceptance.

Using these data, we computed the same temporal stability statistics. The results appear in Figure F.1b. For comparison, the results from the main text are reprinted in Figure F.1a. We observe a substantially lesser degree of response stability among endorsers of the 2016 fraud claim. In the second wave, respondents who claim to be completely confident that millions of illegal votes were cast have an average scale score of 0.64. This indicates that these individuals find the claim plausible, but are not nearly as committed as those who endorse the big lie with complete confidence. Among all other respondents who endorse the 2016 fraud claim, the average scale scores range between 0.45 and 0.53, and are not distinguishable from 0.5 in any case. This indicates that even those who claim to be "very" confident in the 2016 fraud claim are indifferent to it on average, and only appear confident due to measurement error.

Figure F.1: Temporal stability among Republicans, big lie vs. millions of illegal votes




*Note:* The x-axis displays the response in the first wave of a panel survey. The y-axis is the average response in the second wave, conditional on the response given in the first wave. The right panel presents these quantities using our data; it is identical to the left panel of Figure 1a. The left panel presents an identical analysis using the 2020 ANES Social Media Study's question about whether millions of illegal votes were cast in the 2016 election. Printed numbers are point estimates. Vertical bars represent 95 percent confidence intervals.

# G Pre-Analysis Plans

The following pages contain anonymized versions of our preanalysis plans.

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# As Predicted:*Response substitution: Economy, vaccinations, and voter fraud* (#65446)

**Created:** 05/09/2021 08:57 AM (PT)

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**1) Have any data been collected for this study already?**
No, no data have been collected for this study yet.

**2) What's the main question being asked or hypothesis being tested in this study?**
We examine whether partisan gaps in response to survey items concerning political issues are caused by "response substitution" on behalf of partisan respondents. "Response substitution" in the survey context can be considered a systematic measurement error, as respondents provide "an answer to a question that reflects attitudes or beliefs that they want to convey but that the researcher has not asked about" (Gal and Rucker 2011, 186) in response to a given survey item. In our study, we test whether reducing "response substitution" indeed reduces the partisan gap in responses to politically contested items. More specifically, we will test whether allowing respondents to answer "an unasked question" (i.e., convey "the attitudes or beliefs that they want to convey") prior to the item of interest changes their response to the item of interest, thereby reducing the partisan gap in the latter item.

As detailed below, we will examine response substitution in three topic areas: economic evaluations, vaccination campaigns, and voter fraud in the 2020 election. Though we may examine these topics in separate manuscripts, we are registering them together because the studies all draw on the same theoretical framework. In all areas we will test the following hypothesis. "Treatment" always refers to allowing the respondent to answer an unasked question.
H1: Treatment will reduce the difference in average responses between Democrats and Republicans.

To provide context for H1, we will also test the following hypotheses.
H1a: Treatment will reduce pro-Democrat responses among Democrats.
H1b: Treatment will reduce pro-Republican responses among Republicans.
When it comes to voter fraud, we may privilege our test of H1b over H1 and H1a. See section 8 for a brief discussion of the relative substantive importance of these hypotheses as they apply to each topic.

For the two topics with more than one treatment (i.e., all but the vaccine topic), we will also compare the effect of the two treatments.
H2: Different treatments have different effects.

**3) Describe the key dependent variable(s) specifying how they will be measured.**
Economic evaluations (7-point Likert)
Question text: What do you think about the state of the economy these days in the United States? Would you say the state of the economy is good or bad?
Valence of responses: Good = pro-Democrat, bad = pro-Republican

Vaccine evaluations (7-point Likert)
Question text: What do you think about COVID-19 vaccination in the United States? Would you say the country is doing a good job or a bad job?
Valence of responses: Good = pro-Democrat, bad = pro-Republican

Voter fraud (5-point Likert)
Question text: Would you say that Joe Biden only won the 2020 presidential election due to voter fraud, or do you think he would have won

either way?
Valence of responses: Either way = pro-Democrat, due to fraud = pro-Republican

**4) How many and which conditions will participants be assigned to?**
For all three topics, respondents will be assigned to the treatment arms with equal probability using simple random assignment. Control conditions display only the dependent variable (as listed above), while treatment conditions precede the dependent variable with items that we suspect may function as "unasked questions." The conditions are as follows:

Economic evaluations
• Control
• Approval condition (unasked question concerns approval of Biden's performance)
• Responsibility condition (unasked question concerns whether Biden or Trump had more influence on the current state of the economy)

Vaccine evaluations
• Control
• Responsibility condition (unasked question concerns whether Biden or Trump had more influence on the vaccine rollout)

Voter fraud
• Control
• Fraud beliefs condition (unasked question concerns whether fraud occurred)
• Right decision condition (unasked question concerns whether electing Biden was the right or wrong decision for the country)

**5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.**
For each of the individual topics, we will estimate all of our treatment effects using OLS regressions with robust standard errors.

To test H1, we will use OLS to estimate the parameters in a linear model:
(1) DV = B0 + B1×FirstTreatment + B2×SecondTreatment + B3×Republican + B4×FirstTreatment×Republican + B5×SecondTreatment×Republican + B6×PreTreatmentDV + epsilon

where:
• DV – the dependent variable in each topic, with higher values denoting responses that are more favorable to Republicans (see section 3).
• FirstTreatment – a dummy variable for our first treatment.
• SecondTreatment – a dummy variable for our second treatment.
• Republican – a dummy variable for a Republican respondent.
• PreTreatmentDV – the pre-treatment measure of the DV collected in the baseline survey.

For the vaccine topic, B2 and B5 will be excluded as there is only one treatment. Similarly, for topics in which we fail to confirm hypothesis 2, we will report a pooled estimate using a model that compares the control condition with the two treatments combined.

We expect that B1 and B2 will take the opposite sign as B4 and B5 and that B4 and B5 will be statistically significant. This would indicate that the treatment(s) reduced the partisan difference.

To illustrate where the estimates come from (e.g., to say that response substitution seemed to primarily occur among Democrats or Republicans), we will examine the separate party means and treatment effects.
To test H1a and H1b, we will use data for Democrats and Republicans only to estimate (1)
(2) DV = B0 + B1×FirstTreatment + B2×SecondTreatment + B3×PreTreatmentDV + epsilon
where all terms are defined above.

To test H2, we will use only the data on the treatment groups to estimate:
(3) DV = B0 + B1×SecondTreatment + B2×SecondTreatment×Republican + B3×PreTreatmentDV + epsilon
where all terms are defined above. We have no clear expectations as to whether B2 should be positive or negative.

For the hypothesis stated with clear directional expectations (i.e., all but H2), we will conduct one-tailed tests.

**6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.**
We will exclude from our hypothesis tests "pure" independents ("leaning" independents will be included in the analyses as either Democrats or Republicans).

**7) How many observations will be collected or what will determine sample size?**

**No need to justify decision, but be precise about exactly how the number will be determined.**

We will recruit 3,600 respondents from MTurk into a baseline/wave 1 survey that measures background characteristics and a pre-treatment measure of the economic, vaccine, and voter fraud DVs. We will recontact 3,000 of these respondents for the main/wave 2 survey, which will include the treatments.

**8) Anything else you would like to pre-register?**
**(e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)**

Although we are pre-registering the same set of tests in the same empirical framework for all three topics, we note above that our emphasis on the components of H1 may differ based on the context of each topic area. For the economic and vaccine topics, we do not have strong expectations about whether response substitution among Democrats or Republicans would be more important. We therefore plan to emphasize H1, which tests for the narrowing of partisan differences that are expected to occur if both H1a and H1b hold. Due to the voter fraud topic's political context, we think that response substitution is most important if it occurs among Republicans, and therefore plan to pay close attention to H1b.

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# As Predicted: *Belief in voter fraud + Trump reinstatement (July 2021)* (#70229)

**Created:** 07/08/2021 12:07 PM (PT)

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**1) Have any data been collected for this study already?**
No, no data have been collected for this study yet.

**2) What's the main question being asked or hypothesis being tested in this study?**
As detailed below, we will look for evidence of expressive responding in two topic areas: belief that voter fraud determined the results of the 2020 election, and belief that Donald Trump will be reinstated as the US president in August 2021.

For voter fraud, we will test the following hypothesis:
H1: An anonymity treatment (a list experiment) will reduce Republicans' reported belief that Biden only won due to election fraud.

For the Trump reinstatement prediction, we will test the following hypothesis:
H2: Financial incentives will reduce Republicans' tendency to predict that Trump will be reinstated as president.

To provide context, we will also test the following hypothesis:
H1a/H2a: Treatment (anonymity or financial incentives) will reduce the average difference in responses between Democrats and Republicans.

**3) Describe the key dependent variable(s) specifying how they will be measured.**
Voter fraud:
• Direct question: Do you agree or disagree with this statement about the 2020 U.S. presidential election? Joe Biden only won the election due to voter fraud. [Agree, disagree]
• List experiment, control condition: Here is a list of statements about the 2020 U.S. presidential election. We would like to know how many you agree with. [I voted in the election.; I did not know where to vote.; I requested an absentee ballot, but it did not arrive.; The Green Party came close to winning the election.]
• List experiment, treatment condition: Same as control with the addition of the following statement: Joe Biden only won the election due to voter fraud.

Trump reinstatement:
• Direct question: Which statement is more likely to be true?
• Incentivized question: Which ticket would you like to enter into the drawing?
Response options for both questions: Donald Trump will be restored as President of the United States by the end of August.; Donald Trump will not be restored as President of the United States by the end of August.

Before answering the dependent variable, respondents in the incentivized condition will see some explanatory text stating that they will be allowed to enter a drawing for a bonus that will be conducted on September 1, 2021.

**4) How many and which conditions will participants be assigned to?**
For the voter fraud list experiment, respondents will be assigned to one of three conditions using block random assignment conducted prior to recruitment. Treatment probabilities were selected using a simulation study to minimize the standard error of the estimated difference between the direct question and the list experiment.
• Direct question (p=0.21)

• List experiment, no fraud statement (p=0.395)
• List experiment with fraud statement (p=0.395)

For the Trump reinstatement experiment, respondents will be assigned to one of two conditions using block random assignment:
• Control: direct question / no incentive (p=1/2)
• Treatment: financial incentive (p=1/2)
After answering the direct question, control respondents will be routed to the treatment condition. This will allow us to conduct both experimental analysis and a within-subjects analysis.

**5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.**
For the voter fraud topic, we will estimate our treatment effect by comparing the difference in means from the list experiment to the mean of the direct question measure. If the difference in means from the list experiment is smaller, this would indicate expressive responding. We will test for statistical significance using a t-test, with p=0.05 (one-sided) as a guideline for statistical significance when testing H1. In all comparisons between the list experiment and direct questions, we will bootstrap p-values using the percentile method. We will conduct 10,000 simulations with the seed set to 0.

For the Trump reinstatement experiment, we will conduct a difference in means test. Specifically, per H2 we will examine whether the percentage of Republicans who will state in the "direct" question that they believe that Trump will be reinstated in August will be greater than the percentage of Republicans who will place a bet on Trump being reinstated in August. We will test for statistical significance using OLS regression, with p=0.05 (one-sided) as a guideline for statistical significance.

For the Trump reinstatement within-subjects analysis, we will subtract the direct question from the incentivized question, yielding a variable that takes the values -1 ('switch' from reinstated to not reinstated), 0 (no change), 1 ('switch' from not reinstated to reinstated). The within-person effect is the mean of this variable. A negative average among Republicans provides support for H2, with p=0.05 (one-sided) as a guideline for statistical significance.

To test for the effect on partisan differences (H1a/H2a), we estimate the difference in treatment effects between Democrats and Republicans using an interaction between the treatment and a dummy for Republican respondents.

**6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.**
We will exclude "pure" independents from our test of the partisan difference hypotheses (H1a/H2a). "Leaning" independents will be included in the analyses as either Democrats or Republicans.

**7) How many observations will be collected or what will determine sample size?**
**No need to justify decision, but be precise about exactly how the number will be determined.**
We will make the survey available to 8,647 respondents on MTurk whose partisanship is already known to the researcher. Of these, about 50 percent are Republicans, 10 percent are independents, and 40 percent are Democrats. We will end the survey when we reach 6,000 respondents or on July 31, whichever comes first. If the recontact rate is below 50 percent on July 31, we will extend the sampling period for the list experiment by two weeks.

**8) Anything else you would like to pre-register?**
**(e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)**

# As Predicted: *Belief in election fraud: Honesty treatments* (#72630)

**Created:** 08/12/2021 07:34 AM (PT)





**1) Have any data been collected for this study already?**
No, no data have been collected for this study yet.

**2) What's the main question being asked or hypothesis being tested in this study?**
As detailed below, we will look for evidence of expressive responding in the belief that voter fraud determined the results of the 2020 election. Specifically, we will test the following hypothesis:
H1: An honesty treatment will reduce Republicans' reported belief that Biden only won due to election fraud.

To provide context, we will also test the following hypothesis:
H1a: The honesty treatment will reduce the average difference in responses between Democrats and Republicans.

**3) Describe the key dependent variable(s) specifying how they will be measured.**
[1] Do you think that Joe Biden only won the 2020 presidential election due to voter fraud, or do you think he would have won either way?
• Definitely due to voter fraud
• Probably due to voter fraud
• Not sure
• Probably would have won either way
• Definitely would have won either way

We will rescale this variable such that higher values denote more support for the response that is congenial to Republicans (i.e., 'Definitely due to voter fraud')

**4) How many and which conditions will participants be assigned to?**
Respondents will be assigned to one of three conditions using simple random assignment.

• First treatment (request): Regardless of how you feel about the people and events mentioned in the question below, we want you to tell us what you believe to be true. Again, we ask that you try and ignore your personal feelings. [Respondents will then answer the dependent variable]

• Second treatment (pipeline): We sometimes find that people choose answers that they do not really believe so that they can say something good or bad about the people and events mentioned in the question. Regardless of how you feel about the people and events mentioned in the question below, please tell us what you believe to be true. [Respondents will then answer the dependent variable]

• Control: none [only the dependent variable.]

**5) Specify exactly which analyses you will conduct to examine the main question/hypothesis.**
We will estimate our treatment effects using OLS regressions with robust standard errors.

To test H1, we will use OLS to estimate the parameters in a linear model among republican respondents only. This model will separately estimate the effect of the two treatments:
(1) DV = B0 + B1× FirstTreatment + B2×SecondTreatment + epsilon

And to test H1a, we will use OLS to estimate the parameters in a linear model:
(2) DV = B0 + B1×FirstTreatment + B2×SecondTreatment + B3×Republican + B4×FirstTreatment×Republican + B5×SecondTreatment×Republican + epsilon

where:
• DV – the dependent variable in each topic, with higher values denoting responses that are more favorable to Republicans (see section 3).
• FirstTreatment – a dummy variable for our first treatment.
• SecondTreatment – a dummy variable for our second treatment.
• Republican – a dummy variable for a Republican respondent.

In Equation (1), we expect both B1 and B2 to be negative and statistically significant. This would indicate that each treatment reduced reported belief in voter fraud among Republicans.
In Equation (2), we expect that B1 and B2 will take the opposite sign as B4 and B5, and that B4 and B5 will be statistically significant. This would indicate that each treatment reduced the partisan difference.

As an additional test of H1 and H1a, respectively, we will use OLS models that combine the two treatments:
(3) DV = B0 + B1×AnyTreatment + epsilon

(4) DV = B0 + B1×AnyTreatment + B2×Republican + B3×AnyTreatment×Republican + epsilon

where:
• AnyTreatment – a dummy variable for any of the two treatment conditions.

In Equation (3), we expect that B1 will be negative and statistically significant. This would indicate that the treatments (pooled) reduced reported belief in voter fraud among Republican respondents.
And in Equation (4), we expect that B1 will take the opposite sign as B3 and that B3 will be statistically significant. This would indicate that the treatments (pooled) reduced the partisan difference.

All hypotheses are stated with clear directional expectations. Accordingly, we will conduct one-tailed tests.

**6) Describe exactly how outliers will be defined and handled, and your precise rule(s) for excluding observations.**
We will exclude "pure" independents from our test of the partisan difference hypotheses (H1a). "Leaning" independents will be included in the analyses as either Democrats or Republicans.

**7) How many observations will be collected or what will determine sample size?**
**No need to justify decision, but be precise about <u>exactly</u> how the number will be determined.**
We will recruit 5,000 respondents from MTurk.

**8) Anything else you would like to pre-register?**
**(e.g., secondary analyses, variables collected for exploratory purposes, unusual analyses planned?)**