The Effects of Unsubstantiated Claims of Voter Fraud on Confidence in Elections[†]

Nicolas Berlinski[1], Margaret Doyle[1], Andrew M. Guess[2], Gabrielle Levy[1], Benjamin Lyons[3], Jacob M. Montgomery[4], Brendan Nyhan[1], and Jason Reifler[5]

[1]Department of Government, Dartmouth College
[2]Department of Politics and Woodrow Wilson School, Princeton University
[3]Department of Communication, University of Utah
[4]Department of Political Science, Washington University in St. Louis
[5]Department of Politics, University of Exeter

Abstract

Political elites sometimes seek to delegitimize election results using unsubstantiated claims of fraud. Most recently, Donald Trump sought to overturn his loss in the 2020 U.S. presidential election by falsely alleging widespread fraud. Our study provides new evidence demonstrating the corrosive effect of fraud claims like these on trust in the election system. Using a nationwide survey experiment conducted after the 2018 midterm elections — a time when many prominent Republicans also made unsubstantiated fraud claims — we show that exposure to claims of voter fraud reduces confidence in electoral integrity, though not support for democracy itself. The effects are concentrated among Republicans and Trump approvers. Worryingly, corrective messages from mainstream sources do *not* measurably reduce the damage these accusations inflict. These results suggest that unsubstantiated voter fraud claims undermine confidence in elections, particularly when the claims are politically congenial, and that their effects cannot easily be mitigated by fact-checking.

[†]We thank Democracy Fund and the European Research Council (ERC) under the European Union's Horizon 2020 research and innovation programme (grant agreement No. 682758) for funding support. We are also grateful for funding support from the Weidenbaum Center on the Economy, Government, and Public Policy at Washington University in St. Louis. All conclusions and any errors are our own.

After Donald Trump lost the 2020 U.S. presidential election, he and his allies made sweeping and unsupported claims that the election had been stolen. These unsubstantiated assertions ranged from familiar voter-fraud tropes (claims that illegitimate ballots were submitted by dead people) to the fanciful (voting machines were part of a complicated conspiracy involving the late Venezuelan leader Hugo Chávez). Amid increasingly heated rhetoric, a January 6, 2021 "Stop the Steal" rally was followed by a violent insurrection at the U.S. Capitol that sought to disrupt the certification of President-elect Biden's victory, a tragic event many observers partially attributed to the false claims of fraud made by President Trump and his allies.

Claims of voter fraud like this are not uncommon, especially outside United States. In early February 2021, the Myanmar military justified its coup against the civilian government by alleging voter fraud in the November elections (Goodman 2021). In other cases, elites have made unsubstantiated claims of voter fraud in order to cast doubt on unfavorable or potentially damaging electoral results. For instance, Jair Bolsonaro, the president of Brazil, expressed fears of voter fraud during his presidential campaign in 2018 to preemptively cast doubt on an unfavorable electoral outcome (Savarese 2018). Prabowo Subianto, a presidential candidate who lost the 2019 Indonesian election, used this tactic even more aggressively, claiming that he had been the victim of voter fraud and refusing to concede (Paddock 2019).

Though accusations of misconduct are a frequent feature of electoral politics, the effects of this phenomenon on voter beliefs and attitudes have not been extensively studied. To date, research has largely focused on the effects of *actual* irregularities (e.g., Norris 2014) — and institutions intended to constrain malfeasance — on electoral confidence, particularly in less established democracies (e.g., Hyde 2011). Less is known about the effects of *unfounded* assertions of voter fraud on public faith in free and fair elections, especially in advanced democracies such as the United States. Can elites delegitimize a democratic outcome by asserting that electoral irregularities took place? Our motivating examples, particularly recent events in the United States, suggest reasons for concern. Given the centrality of voter fraud to Trump's rhetoric in the weeks leading up the January 6 insurrection, it is essential to better understand whether and how baseless accusations of election-related

illegalities affect citizens.[1]

While recent events and the voluminous elite cues literature (e.g., Zaller 1992) lead us to expect that fraud claims would have a deleterious effect, several streams of previous research suggest that political leaders may have a limited ability to alter citizen's attitudes about the legitimacy of foundational political institutions like elections by inventing accusations of fraud. First, previous work in this area suggests that unsubstantiated claims of widespread voter fraud may have little effect on public attitudes. Most notably, recent studies of the 2016 U.S. presidential election using panel designs provide mixed evidence on the effect of voter fraud claims. Despite Donald Trump's frequent (and unsubstantiated) claims of voter fraud before that election, Trump voters' confidence in elections did not measurably change and Democrats' confidence in elections actually *increased* pre-election, possibly in response to Trump's claims (Sinclair, Smith and Tucker 2018). After the election, confidence in elections actually *increased* and belief in illicit voting *decreased* among Trump supporters (a classic "winner effect") while confidence of Clinton's voters remained unchanged (Levy 2020).

Second, there is reason to be skeptical about claims that political leaders can alter citizens' attitudes so easily by alleging fraud. Studies find, for instance, that presidents struggle to change public opinion on most topics despite extensive efforts to do so (Edwards 2006; Franco, Grimmer and Lim N.d.). Moreover, they may face electoral sanctions for challenging democratic norms. Reeves and Rogowski (2016; 2018), for instance, argue that leaders are punished for acquiring or exercising power in norm-violating ways. Similarly, conjoint studies by Carey et al. (2020) and Graham and Svolik (2020) show that voters punish candidates for democratic norm violations, though the magnitude of these punishments are modest and voters may be more willing to apply them to the opposition party.

[1]We use data collected in the aftermath of the 2018 U.S. midterm election to examine how exposure to Republican claims of voter in fraud affect confidence in election and support for democracy. An advantage of this design is that while such claims were made, they were far less common than in 2020, allowing us to better isolate the effect of exposure.

Finally, even psychological factors and message effects that make people vulnerable to claims of fraud such as directionally motivated reasoning, framing, and elite cues face boundary conditions (Cotter, Lodge and Vidigal 2020; Druckman 2001; Nicholson 2011). Trusted sources and accuracy motivations are two such limiting factors; fact-checking could be persuasive due to source effects alone or by inducing accuracy motivations. The content of corrections may be also be effective given the growing consensus that exposure to corrections or fact-checking can improve belief accuracy for specific factual claims (Wood and Porter 2019; Nyhan and Reifler 2017), though the effect of corrections on broader attitudes and behavioral intentions is less clear (Nyhan et al. 2019, 2014).

In short, while politicians undoubtedly *make* unfounded claims of voter fraud, available evidence is less clear about whether such claims affect citizens' faith in elections. Our study addressing the limitations of prior panel studies and provides direct experimental evidence of the effects of unfounded accusations of voter fraud on citizens' confidence in elections. This approach is most closely related to that of Albertson and Guiler (2020), who show that telling respondents that "experts" believe that the 2016 election was vulnerable to manipulation and fraud increased perceptions of fraud, lowered confidence in the electoral system, and reduced willingness to accept the outcome. However, our study differs in that the accusations we test come from political leaders, a more common source in practice (experts believe voter fraud is exceptionally rare in the U.S.).

We specifically evaluate the effects of exposure to voter fraud claims from politicians in the context of the aftermath of the 2018 U.S. midterm elections. Notably, we not only test the effects of such accusations in isolation, but also examine the effects of such exposure when fraud claims are paired with fact-checks from independent experts. This design approach is critical for evaluating potential real-world responses by, e.g., social media companies that seek to mitigate harm from voter fraud claims (Klar 2020).

Our results show that exposure to unsubstantiated claims of voter fraud from prominent Republicans reduces confidence in elections, especially among Republicans and individuals who approve of Donald Trump's performance in office. Worryingly, exposure to fact-checks that show these claims to be unfounded does not measurably reduce the damage from these accusations. The results

suggest that unsubstantiated claims of voter fraud undermine the public's confidence in elections, particularly when the claims are politically congenial, and that these effects cannot easily be ameliorated by fact-checks or counter-messaging. However, we find no evidence that exposure to these claims reduces support for democracy itself.

From this perspective, unfounded claims of voter fraud represent a dangerous attack on the legitimacy of democratic processes. Even when based on no evidence and countered by non-partisan experts, such claims can significantly diminish the legitimacy of election outcomes among allied partisans. As the Capitol insurrection suggests, diminished respect for electoral outcomes presents real dangers for democracy (e.g., Minnite 2010). If electoral results are not respected, democracies cannot function (Anderson et al. 2005). And even if losers step down, belief in widespread voter fraud threatens to undermine public trust in elections, delegitimize election results, and promote violence or other forms of unrest.

Experimental design

We conducted our experiment among 4,283 respondents in the U.S. who were surveyed in December 2018/January 2019 by YouGov (see Online Appendix A for details on the demographic characteristics of the sample and question wording).[2] After a pre-treatment survey, respondents were randomly assigned to view either a series of non-political tweets (*placebo*); four tweets alleging voter fraud (*low dose*); the four tweets alleging voter fraud from the low dose condition plus four additional tweets alleging voter fraud (*high dose*); or the four tweets from the low dose condition alleging voter fraud plus four fact-check tweets (*low dose* + *fact-check*). Respondents then completed post-treatment survey questions measuring our outcome.

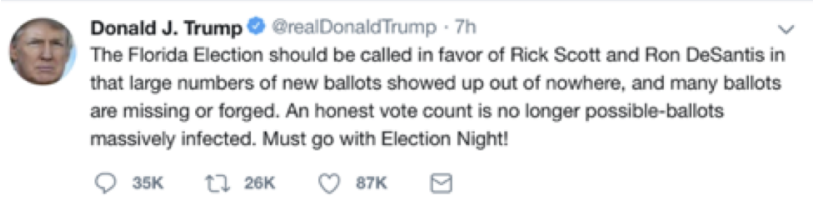
Immediately after the election, several prominent Republicans, including Florida governor Rick Scott, Senators Lindsey Graham and Marco Rubio, and Trump himself, made unfounded allegations of voter fraud while counts were still ongoing (Lopez 2018). Tweets from these political elites and fact-checks of the claims were used as the treatment stimuli (see Figure 1 for an example). This

[2]This survey also included orthogonal studies reported in (omitted for peer review).

design has high external validity, allowing us to show actual claims of voter fraud made by party elites to respondents in the original format in which they were seen by voters.

To match this format's external validity, we draw on actual corrections produced by the Associated Press, PBS NewsHour, and NYT Politics, again in the form of tweets (see Online Appendix A). Though these messages do not come from dedicated fact-checking outlets per se, these standalone articles fit within the larger diffusion of the format through the mainstream press (Graves, Nyhan and Reifler 2015) and follow prior work on journalistic corrections (Nyhan et al. 2019; Nyhan and Reifler 2010; Pingree et al. 2018).

Figure 1: Example stimulus tweet from the experiment



Hypotheses and research questions

We expect that exposure to unfounded voter fraud claims reduces confidence in elections (e.g., Alvarez, Hall and Llewellyn 2008; Hall, Quin Monson and Patterson 2009), the immediate object of criticism, and potentially undermines support for democracy itself (Inglehart 2003). This expectation leads to four preregistered hypotheses and two research questions.[3]

[3]We provide a "populated pre-analysis plan" (Duflo et al. 2020) and a link to the anonymized preregistration in Online Appendix E (the relevant preregistered hypotheses and analysis plan for this study appear in Section E). It is important to clarify that the preregistration is time-stamped February 20, 2019 even though data were collected in December 2018/January 2019. However, it was filed prior to data delivery from YouGov, which was withheld until February 27, 2019 — after the preregistration was filed. (The anonymized letter documenting the delivery date is provided here: `https://www.dropbox.com/s/1co33ljx54ddk5g/yougov-statement.pdf`.)

Our first three preregistered hypotheses concern the effect of exposure to voter fraud allegations. We expect that low (H1a) and high (H2a) doses of exposure to allegations of voter fraud will reduce confidence in elections and that a high dose will have a stronger effect (H3a). The idea that increased message dosage should lead to greater effects is long-standing and intuitive (Arendt 2015; Cacioppo and Petty 1979) and has received some empirical support (e.g., Ratcliff et al. 2019), but evidence is limited for this claim the domain of politics (Miller and Krosnick 1996; Arendt, Marquart and Matthes 2015; Lecheler and de Vreese 2013; Baden and Lecheler 2012). Higher doses may have diminishing returns in political messaging, with large initial effects among people who have not previously been exposed to similar messages but less additional influence as exposure increases (Markovich et al. 2020).

We also expect the effects of exposure to be greater when the claims are politically congenial (H1b–H3b) given the way pre-existing attitudes affect the processing of new information (e.g., Kunda 1990; Taber and Lodge 2006), including on election/voter fraud (Edelson et al. 2017; Udani, Kimball and Fogarty 2018).

Fact-checks can be effective in counteracting exposure to misinformation (Chan et al. 2017; Fridkin, Kenney and Wintersieck 2015). Our fourth hypothesis therefore predicts that fact-checks can reduce the effects of exposure to a low dose of voter fraud misinformation on perceived electoral integrity (H4a). We also expect fact-checks will reduce the effects of voter fraud misinformation more for audiences for whom the fraud messages are politically congenial simply because the initial effects are expected to be larger (H4b).

Finally, we also consider preregistered research questions. First, we ask whether exposure to both a low dose of allegations of voter fraud and fact-checks affects confidence in elections compared to the placebo condition baseline per Thorson (2016) (RQ1a). Second, we test whether this result differs when the claims are politically congenial (RQ1b).[4] Finally, we examine whether these effects extend beyond attitudes towards electoral institutions and affect support for democracy itself

[4]These RQs compare the *low dose + fact-check* condition to the *placebo* condition while H4a and H4b compare the *low dose condition* to the *low dose + fact-check condition*.

Table 1: Measures of confidence in elections

Question	Mean	SD	Range
Confidence all entitled allowed to vote	2.58	1.00	[1, 4]
Confidence own vote was counted†	3.18	0.88	[1, 4]
Confidence officials manage counting votes	2.57	0.92	[1,4]
System works despite problems casting and counting votes	2.75	0.89	[1,4]
Trust elections	4.60	1.63	[1,7]
Ballots secure from tampering	2.86	1.02	[1,4]
Voting machines accurate	3.42	0.96	[1, 5]
Composite measure§	0	1	[−2.52, 1.99]

Complete question wordings for all items is provided in Appendix A. † indicates that the item was only asked of respondents who indicated they voted. § indicates a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details).

(RQ2).[5]

Methods

To test our main hypotheses, we examine seven survey items that tap into different aspects of election integrity (for example, "How confident are you that election officials managed the counting of ballots fairly in the election this November?"). Descriptive statistics for all items are shown in Table 1 and complete question wording is shown in Appendix A. On average, respondents indicated modestly high levels of confidence in U.S. electoral institutions and election integrity.

Exploratory factor analysis (EFA) showed that these items scaled together; we therefore created a standardized outcome measure of confidence in the electoral system. All seven items loaded

[5]These RQs deviate from our preregistration by splitting confidence in elections and support for democracy into separate outcome variables. Originally, we preregistered that H1–H4 and RQ1 would apply to "confidence in elections *and* (emphasis added) support for democracy." However, this statement was based on a preregistered factor analysis of the individual items reported in Online Appendix B. As the factor analysis distinguished between these items, we include both (as per our preregistration) but examine them separately. Adding an RQ is meant to aid the reader's understanding of which analyses apply to which outcome variable. See also Footnote 6.

onto a single factor; the absolute value of the factor loadings was greater than 0.6 for all cases and typically larger than 0.8. To identify the latent space, we set the variance of the latent factor to one, allowing all treatment effects to be interpreted as sample standard deviations. A full discussion of this process is presented in Online Appendix B.[6]

We estimate linear regression models that include only main effects for experimental conditions as well as models that interact treatment indicators with measures for whether voter fraud misinformation was congenial for respondents. In our original preregistration, we stated we would test the hypotheses related to congeniality by including an interaction term with an indicator for whether or not a respondent is a Republican, which implicitly combines Democrats and independents into a single category. We found that Democrats and Independents actually responded quite differently to the treatments and therefore deviate from our preregistration to estimate results separately using all three categories below (the preregistered analysis is provided in Table C3 in the Appendix). In addition, we also conducted exploratory analyses using approval of President Trump as an alternative moderator of whether the fraud messages were congenial.

Finally, for RQ2, we relied on a separate five-item battery measuring commitment to democratic governance reported in Online Appendix A. We analyze both the individual items and two

[6]As noted in Appendix B, our preregistered approach was to include seven items measuring election confidence and five additional items measuring support for democracy. We noted that if these separate batteries "represent a single construct" we would combine them into a single composite measure. Our preregistration did not specify what would be done if the items did not scale onto a single dimension. As shown in the appendix, EFA indicated that the seven election confidence items did relate to a single underlying construct. Our main analysis therefore focuses on this measure. However, the five remaining items scaled onto two separate dimensions. For the sake of completeness, we therefore analyze both those five individual items and the two composite measures that correspond to the indicated dimensions in Online Appendix C (Tables C4–C7). This approach represents a deviation from our preregistration in that we did not specify how we would proceed in this circumstance.

composite scales suggested by our EFA (see Footnote 6).

Deviations from preregistered analysis plan

For transparency, we provide a summary of the deviations from our preregistration here. First, per above, we now examine potential congeniality effects for Republicans, Democrats, and independents separately rather than examining differences between Republicans and all others. Online Appendix C contains the preregistered specification in which Democrats and independents are analyzed together. Second, we present an additional, exploratory test of congeniality using Trump approval as a moderator. Third, we present main effects below for individual items from our outcome measure of election confidence in addition to the composite measure; our preregistration stated that we would report results separately for each dependent variable included in the composite measure in the appendix, but we have included these models in the main text. Fourth, RQ2 deviates from our preregistration in that effects on both election confidence and support for democracy were included as outcomes of interest for H1–H4 and RQ1 pending a preregistered factor analysis of the individual items. As this factor analysis distinguished between these outcomes (see online Appendix B), we conduct separate analyses for support for democracy. These results are discussed briefly below, but are reported in full in Online Appendix C. A complete discussion of our preregistered analyses as well as deviations are shown in Online Appendix E.

Results

We focus our presentation below on estimated treatment effects for our composite measure of election confidence. However, we present treatment effects for each component outcome measure (exploratory) as well as the composite measure of election confidence (preregistered) in Table 2. Figure 2 shows the effects for the composite measure. Since the composite measure is standardized, the effects can be directly interpreted in terms of standard deviations (SDs).

We find that exposure to the *low dose* condition significantly reduced confidence in elections compared to the placebo condition (H1a: $\beta = -0.147$ SD, $p < .005$). This pattern also held in

the high-dose condition (H2a: $\beta = -0.168$ SD, $p < .005$).[7] However, we fail to reject the null hypothesis of no difference in effects (H3a); the effects of exposure to low versus high doses of tweets alleging voter fraud are not measurably different. This result, which we calculate as the difference in treatment effects between the *low dose* and *high dose* conditions, is reported in the row in Table 2 labeled "Effect of higher dosage."[8]

A crucial question in this study is whether the effect of fact-check tweets can offset the effect of the tweets alleging fraud. We find that exposure to fact-checks after a low dose of unfounded voter fraud claims did not measurably increase election confidence relative to the low dose condition. As a result, the negative effects of exposure remain relative to the placebo condition.

Specifically, we *can* reject the null hypothesis of no difference in election confidence between participants exposed to the *low dose* + *fact-check tweets* versus those in the placebo condition (RQ1a: $\beta = -0.092$ SD, $p < 0.05$). This effect is negative, indicating the fact-check tweets do not eliminate the harmful effects of exposure to unfounded allegations of fraud on election confidence. Substantively, the effect estimate is smaller than the effect for the *low dose* condition with

[7]We did not conduct any power analyses in advance. However, Online Appendix F uses the `DeclareDesign` approach to approximate the power of our design and provide context for interpreting these results (Blair et al. 2019). These simulations show that we are well powered to detect main effects of approximately -0.11 or larger, which is consistent with our estimates for H1a and H2a. However, the *low dose* + *corrections* condition (RQ1a) falls below this threshold. In addition, despite our large sample, we are powered to detect only fairly large interaction terms (larger than approximately 0.25). The design is sufficiently powered to detect an estimand similar in magnitude to the estimate we report for the *high dose* $\times$ *Democrat* interaction in Table C1 ($\beta = 0.252$). However, we are not powered to detect interactions if the true estimand is as small as the estimate for the *low dose* $\times$ *Democrat* interaction ($\beta = 0.099$).

[8]Effects for individual outcome measures are generally but not uniformly consistent with these patterns. Most notably, none of the treatments had an effect on beliefs that ballots are secure from tampering, a claim that was not questioned in the stimuli shown to respondents.

Table 2: Effect of exposure to voter fraud allegations on election confidence

	Confidence eligibles can vote	Confidence officials fairly manage	Confidence own vote counted	System works	Trust in elections	Ballot security	Voting machines accurate	Composite measure
Low dose (H1a)	-0.209***	-0.120***	-0.150***	-0.138***	-0.155*	-0.081	-0.038	-0.147***
	(0.043)	(0.042)	(0.039)	(0.038)	(0.069)	(0.044)	(0.040)	(0.042)
High dose (H2a)	-0.195***	-0.137***	-0.146***	-0.172***	-0.173*	-0.080	-0.095*	-0.168***
	(0.044)	(0.043)	(0.040)	(0.038)	(0.070)	(0.044)	(0.042)	(0.043)
Low dose+fact-check tweets (RQ1a)	-0.088*	-0.122**	-0.059	-0.095*	-0.074	-0.062	-0.057	-0.092*
	(0.043)	(0.044)	(0.040)	(0.038)	(0.070)	(0.045)	(0.042)	(0.043)
Constant	2.706***	3.274***	2.656***	2.848***	4.704***	2.913***	3.469***	0.102***
	(0.031)	(0.029)	(0.028)	(0.026)	(0.048)	(0.031)	(0.029)	(0.030)
Effect of higher dosage (H3a)								
High dose - Low dose	0.015	-0.017	0.004	-0.038	-0.018	0.001	-0.057	-0.021
	(0.043)	(0.043)	(0.040)	(0.039)	(0.071)	(0.044)	(0.042)	(0.043)
Effect of fact-check exposure (H4a)								
(Low dose + fact-check) - Low dose	0.121***	-0.001	0.090*	0.043	0.081	0.018	-0.200	0.055
	(0.043)	(0.044)	(0.040)	(0.038)	(0.071)	(0.044)	(0.041)	(0.043)
N	4278	3283	4279	4279	4277	4273	4250	4283

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided). OLS models with robust standard errors. The outcome variable is a composite measure of election confidence created using confirmatory factor analysis (see Online Appendix B for estimation details).

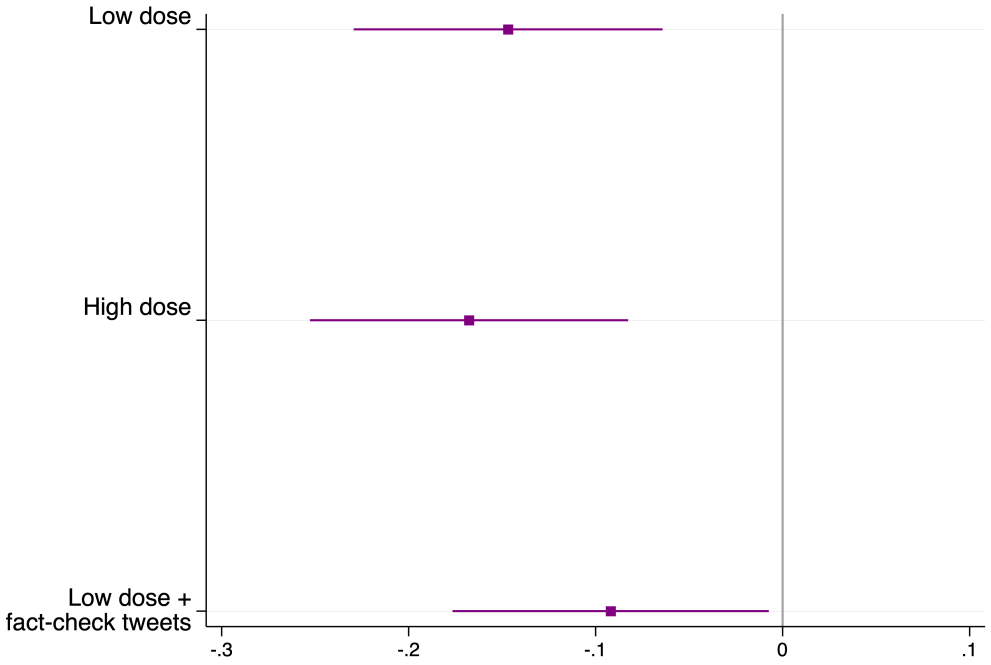
no fact-check tweets described above (H1a: $\beta = -0.147$ SD, $p < .005$) but the difference is not reliably distinguishable from zero (H4a: $\beta_{\text{low dose + fact check}} - \beta_{\text{low dose}} = 0.055$ SD, $p > .05$).

Next, we examined the effect of voter fraud messages on respondents for whom the content of those messages (and the sources who endorse them) would be congenial — the Republican identifiers and leaners whose party was seen as losing the 2018 midterm elections. We estimate how our treatment effects vary by party and by approval of President Trump in Tables C1 and C2 in Online Appendix C. The resulting marginal effect estimates are presented in Figure 3.[9]

We first analyze the results based on party identification. We find that the effects of exposure to a high dose of voter fraud misinformation vary significantly by party (H2b; $p < .01$), decreasing voter confidence significantly only among Republicans. By contrast, the effect of the low dosage of four tweets of voter fraud misinformation is not measurably different between Democrats and

[9]Our analysis of effects by party deviates from our preregistered analysis by examining Democrats and independents separately. We discuss this in greater detail in Online Appendix C, which also contains the preregistered specification in which they are analyzed together. In addition, our analysis of effect by Trump approval is exploratory.

Figure 2: Marginal effect of exposure to claims of voter fraud on confidence in elections



Difference in means (with 95% CIs) for composite measure of election confidence relative to the placebo condition.

Republicans (H1b), though the message's marginal effect is significant for Republicans ($p < .01$) and not for Democrats. Similarly, the effect of greater dosage of fraud allegations (i.e., high versus low dosage) does not vary measurably by party (H3b).

Results are similar when we consider attitudes towards President Trump as a moderator. The effects of exposure to tweets varies significantly by approval in the *high dose* condition ($p < .005$), significantly reducing election confidence only among respondents who approve of Trump. The interaction is not significant for the *low dose* condition, though again the effect of the treatment is only significant among Trump approvers. Further, there is insufficient evidence to conclude that the additional effect of exposure to fact-check tweets (versus just the low dose of fraud tweets) varied by Trump approval. However, the dosage effect (*low* versus *high dosage*) varied significantly by approval ($\beta = -0.191$ SD, $p < .05$). Among disapprovers, additional dosage had no significant effect, but it reduced election confidence significantly among approvers ($\beta = -0.128$ SD, $p < .05$).

The size of the effects reported in Figure 3 are worth emphasizing. The *high dose* condition,

which exposed respondents to just eight tweets, reduced confidence in the electoral system by 0.27 standard deviations among Republicans and 0.34 standard deviations among Trump approvers. Even if these treatment effects diminish over time, these results indicate that a sustained diet of exposure to such unfounded accusations could substantially reduce faith in the electoral system.

We also consider whether the effects of fact-check exposure vary between Democrats and Republicans. We find the marginal effect of exposure to fact-checks (comparing the *low dose* + *fact-check* condition to the *low dose* condition) does not vary significantly by party (H4b). As a result, the negative effects of the *low dose* condition on trust and confidence in elections among Republicans ($\beta = -0.184$ SD, $p < .01$) persist if they are also exposed to fact-checks in the *low dose* + *fact-check* condition ($\beta = -0.176$ SD, $p < .05$). This pattern replicates when we instead disaggregate by Trump support. We find no measurable difference in the effects of the fact-checks by Trump approval, but the *low dose* + *fact-check* reduces election confidence among Trump supporters ($\beta = -0.190$ SD, $p < .005$) despite the presence of corrective information, mirroring the effect in the *low dose* condition ($\beta = -0.211$ SD, $p < .005$).[10]

Finally, we explore whether these treatments affect broader attitudes toward democracy itself. Table C4 in Online Appendix C shows that the effects of the low and high dosage voter fraud treatments were overwhelmingly null on "Having a strong leader who does not have to bother with Congress and elections," "Having experts, not government, make decisions," "Having the army rule;" "Having a democratic political system," and the perceived importance of living in a country

[10]As preregistered, we include additional analyses of other possible moderators of the effects of voter fraud message exposure in Online Appendix D (see Tables D3–D9). These moderators include trust in and feelings toward the media, feelings toward Trump, conspiracy predispositions, political interest and knowledge, and pre-treatment visits to fake news sites and fact-checking sites. We find little evidence of additional heterogeneity, suggesting that the primary moderator is partisanship. A fully populated preregistration is reported in Online Appendix E (Duflo et al. 2020).

Figure 3: Effect of exposure to claims of voter fraud on election confidence by predispositions

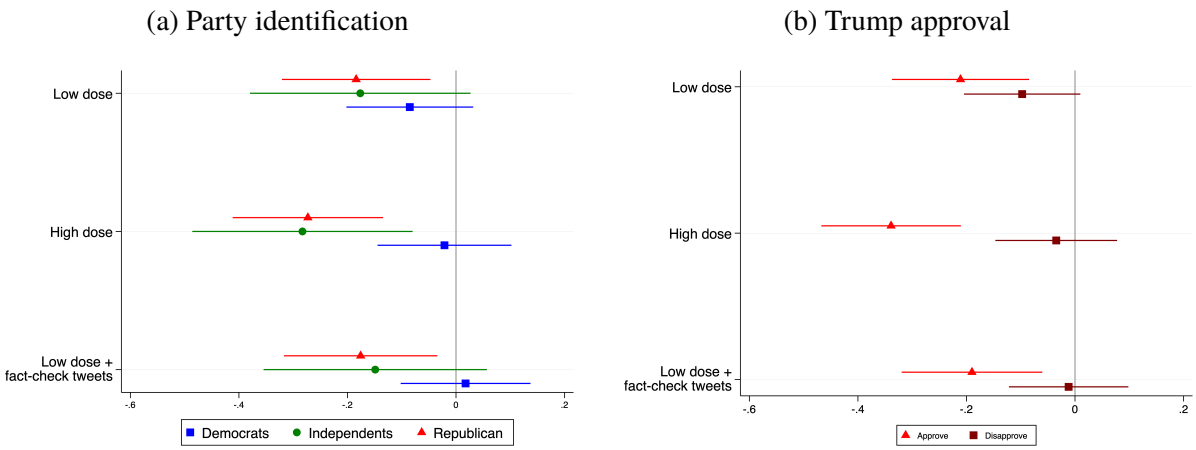


Figure 3a shows the marginal effect by party of exposure to claims of voter fraud on election confidence (Table C1), while Figure 3b shows the marginal effect by Trump approval (Table C2).

that is governed democratically.[11] These null effects were mirrored in analyses of heterogeneous treatment effects by party and Trump approval in Online Appendix C.

Conclusion

This study presents novel experimental evidence of the effect of unsubstantiated claims of voter fraud on public confidence in elections. Using a large, nationally representative sample collected after the 2018 U.S. elections, we show that respondents exposed to either low or high doses of voter fraud claims reported less confidence in elections than those in a placebo condition, though

[11] We find reduced support at the $p < .05$ level for a composite measure of support for alternatives to democracy among respondents exposed to four tweets claiming voter fraud and four fact-check tweets. All results in Table C4 are otherwise null. To assess the precision of these estimates, we estimate results from two one-sided equivalence tests at the 95% level. Across the outcome measures for which we obtain null results (all of which are measured on a 1–4 scale), we can confidently rule out effects of 0.09 or smaller for the low dose condition (0.11 s.d.), 0.11 or smaller for the high dose condition (0.11 s.d.), and 0.16 or smaller for the low dose + fact-check condition (0.20 s.d.).

there was no evidence that the treatments affected attitudes towards democracy more generally. These effects varied somewhat by party. Exposure significantly reduced confidence in elections only among Republicans and Trump supporters, though these effects only differed measurably by party or Trump approval in the high-dosage condition.

Worryingly, we found little evidence that fact-check tweets measurably reduced the effects of exposure to unfounded voter fraud allegations. Adding corrections to the *low dose* condition did not measurably reduce the effects of exposure. As a result, both Republicans and Trump approvers reported significantly lower confidence in elections after exposure to a low dose of voter fraud allegations even when those claims were countered by fact-checks (compared to those in a placebo condition). These findings reinforce previous research on the potential lasting effects of exposure to misinformation even after it is discredited (e.g., Thorson 2016). Our findings also contribute to the growing understanding of the seemingly powerful role of elites in promoting misinformation (Weeks and Gil de Zúñiga 2019) and other potentially damaging outcomes such as conspiracy beliefs (Enders and Smallpage 2019) and affective polarization (Iyengar et al. 2019).

Future work could address a number of limitations in our study and build on our findings in several important ways. First, our treatment and dosage designs were solely based on social media posts. Additional research could explore whether media reports or editorials echoing accusations from political elites have greater effects (e.g., Coppock et al. 2018). Second, journalistic corrections could likewise be strengthened. Corrections from in-group media may be more influential; in the present case, dismissal of fraud claims by outlets like *The Weekly Standard* or *The Daily Caller* could be more credible among Republican respondents. Similarly, dismissals from prominent Republican officials themselves might be more influential as they signal intra-party disagreement (Lyons 2018) — a costly signal, particularly for those who have shifted positions on the issue (Baum and Groeling 2009; Lyons et al. 2019; Benegal and Scruggs 2018). However, such messengers may alternatively be subject to negative evaluation by way of a "black sheep effect" (Matthews and Dietz-Uhler 1998) and could be less effective for Republicans in particular (Agadjanian 2020). Third, our study examines messages that were congenial for Republicans. Though we sought to test

the effects of fraud claims from the sources who have most frequently made them, a future study should also test the congeniality hypotheses we develop using Democrats as well.

While our data focuses on the U.S. case, we strongly encourage future work to examine both the prevalence of electoral fraud claims and the effects of such claims comparatively. We suspect that there is important cross-national variation in how frequently illegitimate electoral fraud claims are made. While we would expect our central finding — exposure to elite messages alleging voter fraud undermines confidence in elections — would be replicated in other locales, there may be important nuance or scope conditions that additional cases would help to reveal. For instance, variation in electoral rules and candidates' resulting relative dependence on the media to communicate with voters may shape the nature of fraud claims themselves (Amsalem et al. 2017). Journalistic fact-checking may be generally more effective in countries with less polarized attitudes towards the media (Lyons et al. 2020). Moreover, variation in party systems may affect the consequences to party elites face for making fraudulent claims; when parties control ballot access, it may be easier to constrain problematic rhetoric in the first place (Carson and Williamson 2018). In addition, proportional representation systems may change the strategic calculus of using rhetoric that attacks election legitimacy because losing parties still may have access to power through coalition bargaining (and voters in PR systems may be able to more easily punish norm violations by defecting to ideologically similar parties). Finally, many countries have dramatically more fluid party attachments than the U.S.; when party attachment is consistently weaker (Huddy, Bankert and Davies 2018), fraud claims may simply carry less weight.

It is also important to consider the potential for expressive responding (Schaffner and Luks 2018) (but see Berinsky 2018), which future work might rule out by soliciting higher stakes outcomes of interest (e.g., willingness to pay additional taxes to improve election security). Future research could also test the effects of allegations in a pre-election context and possibly examine effects on turnout or participation intentions. Finally, the COVID-19 pandemic highlights the importance of considering the effect of fraud allegations directed at mail voting and ballot counting, which may be especially vulnerable to unfounded allegations.

Still, our study provides new insight into the effects of unsubstantiated claims of voter fraud. We demonstrate that these allegations can undermine confidence in elections, particularly when the claims are politically congenial, and may not be effectively mitigated by fact-checking. In this way, the proliferation of unsubstantiated claims of voter fraud threatens to undermine confidence in electoral integrity and contribute to the erosion of U.S. democracy.

References

Agadjanian, Alexander. 2020. "When Do Partisans Stop Following the Leader?" *Political Communication* pp. 1–19.

Albertson, Bethany and Kimberly Guiler. 2020. "Conspiracy theories, election rigging, and support for democratic norms." *Research & Politics* 7(3):2053168020959859.

Alvarez, R Michael, Thad E Hall and Morgan H Llewellyn. 2008. "Are Americans confident their ballots are counted?" *The Journal of Politics* 70(3):754–766.

Amsalem, Eran, Tamir Sheafer, Stefaan Walgrave, Peter John Loewen and Stuart N Soroka. 2017. "Media motivation and elite rhetoric in comparative perspective." *Political Communication* 34(3):385–403.

Anderson, C.J., A. Blais, S. Bowler, T. Donovan and O. Listhaug. 2005. *Losers' Consent: Elections and Democratic Legitimacy*. Oxford University Press.

Arendt, Florian. 2015. "Toward a dose-response account of media priming." *Communication Research* 42(8):1089–1115.

Arendt, Florian, Franziska Marquart and Jörg Matthes. 2015. "Effects of right-wing populist political advertising on implicit and explicit stereotypes." *Journal of Media Psychology* .

Baden, Christian and Sophie Lecheler. 2012. "Fleeting, fading, or far-reaching? A knowledge-based model of the persistence of framing effects." *Communication Theory* 22(4):359–382.

Baum, Matthew A and Tim Groeling. 2009. "Shot by the messenger: Partisan cues and public opinion regarding national security and war." *Political Behavior* 31(2):157–186.

Benegal, Salil D and Lyle A Scruggs. 2018. "Correcting misinformation about climate change: The impact of partisanship in an experimental setting." *Climatic change* 148(1):61–80.

Berinsky, Adam J. 2018. "Telling the truth about believing the lies? Evidence for the limited prevalence of expressive survey responding." *Journal of Politics* 80(1):211–224.

Blair, Graeme, Jasper Cooper, Alexander Coppock and Macartan Humphreys. 2019. "Declaring and diagnosing research designs." *American Political Science Review* 113(3):838–859.

Cacioppo, John T and Richard E Petty. 1979. "Effects of message repetition and position on cognitive response, recall, and persuasion." *Journal of personality and Social Psychology* 37(1):97.

Carey, John, Katherine Clayton, Gretchen Helmke, Brendan Nyhan, Mitchell Sanders and Susan Stokes. 2020. "Who will defend democracy? Evaluating tradeoffs in candidate support among partisan donors and voters." *Journal of Elections, Public Opinion and Parties* pp. 1–16.

Carson, Jamie L and Ryan D Williamson. 2018. "Candidate emergence in the era of direct primaries." *Routledge Handbook of Primary Elections* p. 22.

Chan, Man-pui Sally, Christopher R Jones, Kathleen Hall Jamieson and Dolores Albarracín. 2017. "Debunking: A meta-analysis of the psychological efficacy of messages countering misinformation." *Psychological science* 28(11):1531–1546.

Coppock, Alexander, Emily Ekins, David Kirby et al. 2018. "The long-lasting effects of newspaper op-eds on public opinion." *Quarterly Journal of Political Science* 13(1):59–87.

Cotter, Ryan G, Milton Lodge and Robert Vidigal. 2020. The Boundary Conditions of Motivated Reasoning. In *The Oxford Handbook of Electoral Persuasion*. Oxford University Press, USA p. 66.

Druckman, James N. 2001. "The Implications of Framing Effects for Citizen Competence." *Political Behavior* 23(3):225–256.

Duflo, Esther, Abhijit Banerjee, Amy Finkelstein, Lawrence F Katz, Benjamin A Olken and Anja Sautmann. 2020. "In praise of moderation: Suggestions for the scope and use of pre-analysis plans for RCTs in economics." National Bureau of Economic Research working paper, April 2020.

Edelson, Jack, Alexander Alduncin, Christopher Krewson, James A Sieja and Joseph E Uscinski. 2017. "The effect of conspiratorial thinking and motivated reasoning on belief in election fraud." *Political Research Quarterly* 70(4):933–946.

Edwards, George C. 2006. *On deaf ears: The limits of the bully pulpit*. Yale University Press.

Enders, Adam M and Steven M Smallpage. 2019. "Informational Cues, Partisan-Motivated Reasoning, and the Manipulation of Conspiracy Beliefs." *Political Communication* 36(1):83–102.

Franco, Annie, Justin Grimmer and Chloe Lim. N.d. "The Limited Effect of Presidential Public Appeal." Downloaded May 15, 2020 from `https://www.dropbox.com/s/5ym94ggasbff260/public.pdf?dl=0`.

Fridkin, Kim, Patrick J Kenney and Amanda Wintersieck. 2015. "Liar, liar, pants on fire: How fact-checking influences citizens' reactions to negative advertising." *Political Communication* 32(1):127–151.

Goodman, Jack. 2021. "Myanmar coup: Does the army have evidence of voter fraud? Downloaded from `https://www.bbc.com/news/55918746` on Februrary 13, 2021." BBC, February 2, 2021.

Graham, Matthew H and Milan W Svolik. 2020. "Democracy in America? Partisanship, polarization, and the robustness of support for democracy in the United States." *American Political Science Review* 114(2):392–409.

Graves, Lucas, Brendan Nyhan and Jason Reifler. 2015. *The Diffusion of Fact-checking*. Vol. 22 American Press Institute.

Hall, Thad E, J Quin Monson and Kelly D Patterson. 2009. "The human dimension of elections: How poll workers shape public confidence in elections." *Political Research Quarterly* 62(3):507–522.

Huddy, Leonie, Alexa Bankert and Caitlin Davies. 2018. "Expressive versus instrumental partisanship in multiparty European systems." *Political Psychology* 39:173–199.

Hyde, Susan D. 2011. *The Pseudo-Democrat's Dilemma: Why Election Observation Became an International Norm*. Cornell University Press.

Inglehart, Ronald. 2003. "How solid is mass support for democracy: And how can we measure it?" *PS: Political Science and Politics* 36(1):51–57.

Iyengar, Shanto, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra and Sean J Westwood. 2019. "The origins and consequences of affective polarization in the United States." *Annual Review of Political Science* 22:129–146.

Klar, Rebecca. 2020. "Warning label added to Trump tweet over potential mail-in voting disinformation." *The Hill*, September 17, 2020. Downloaded November 6, 2020 from `https://thehill.com/policy/technology/516890-warning-label-added-to-trump-tweet-over-potential-mail-in-voting`.

Kunda, Ziva. 1990. "The case for motivated reasoning." *Psychological bulletin* 108(3):480–498.

Lecheler, Sophie and Claes H de Vreese. 2013. "What a difference a day makes? The effects of repetitive and competitive news framing over time." *Communication Research* 40(2):147–175.

Levy, Morris. 2020. "Winning cures everything? Beliefs about voter fraud, voter confidence, and the 2016 election." *Electoral Studies* p. 102156.

Lopez, German. 2018. "The Florida voter fraud allegations, explained." Vox, November 12, 2018. Downloaded February 16th, 2020 from https://www.vox.com/policy-and-politics/2018/11/12/18084786/florida-midterm-elections-senate-governor-results-fraud.

Lyons, Ben, Vittorio Mérola, Jason Reifler and Florian Stoeckel. 2020. "How politics shape views toward fact-checking: Evidence from six European countries." *The International Journal of Press/Politics* 25(3):469–492.

Lyons, Benjamin A. 2018. "When readers believe journalists: Effects of adjudication in varied dispute contexts." *International Journal of Public Opinion Research* 30(4):583–606.

Lyons, Benjamin A, Ariel Hasell, Meghnaa Tallapragada and Kathleen Hall Jamieson. 2019. "Conversion messages and attitude change: Strong arguments, not costly signals." *Public Understanding of Science* 28(3):320–338.

Markovich, Zachary, Matthew A Baum, Adam J Berinsky, Justin de Benedictis-Kessner and Teppei Yamamoto. 2020. "Dynamic Persuasion: Decay and Accumulation of Partisan Media Persuasion.".

Matthews, Douglas and Beth Dietz-Uhler. 1998. "The Black-Sheep Effect: How Positive and Negative Advertisements Affect Voters' Perceptions of the Sponsor of the Advertisement 1." *Journal of Applied Social Psychology* 28(20):1903–1915.

Miller, Joanne M and Jon A Krosnick. 1996. "News media impact on the ingredients of presidential evaluations: A program of research on the priming hypothesis." *Political persuasion and attitude change* pp. 79–100.

Minnite, Lorraine C. 2010. *The myth of voter fraud*. Cornell University Press.

Nicholson, Stephen P. 2011. "Dominating cues and the limits of elite influence." *The Journal of Politics* 73(4):1165–1177.

Norris, Pippa. 2014. *Why electoral integrity matters*. Cambridge University Press.

Nyhan, Brendan, Ethan Porter, Jason Reifler and Thomas J Wood. 2019. "Taking fact-checks literally but not seriously? The effects of journalistic fact-checking on factual beliefs and candidate favorability." *Political Behavior* pp. 1–22.

Nyhan, Brendan and Jason Reifler. 2010. "When corrections fail: The persistence of political misperceptions." *Political Behavior* 32(2):303–330.

Nyhan, Brendan and Jason Reifler. 2017. "Do people actually learn from fact-checking? Evidence from a longitudinal study during the 2014 campaign." *Unpublished manuscript* .

Nyhan, Brendan, Jason Reifler, Sean Richey and Gary L Freed. 2014. "Effective messages in vaccine promotion: a randomized trial." *Pediatrics* 133(4):e835–e842.

Paddock, Richard C. 2019. "Indonesia Court Rejects Presidential Candidate's Voting Fraud Claims." *New York Times*, June 27, 2019. Downloaded May 4, 2020 from `https://www.nytimes.com/2019/06/27/world/asia/indonesia-widodo-prabowo-election-fraud.html`.

Pingree, Raymond J, Brian Watson, Mingxiao Sui, Kathleen Searles, Nathan P Kalmoe, Joshua P Darr, Martina Santia and Kirill Bryanov. 2018. "Checking facts and fighting back: Why journalists should defend their profession." *PloS one* 13(12):e0208600.

Ratcliff, Chelsea L, Jakob D Jensen, Courtney L Scherr, Melinda Krakow and Kaylee Crossley. 2019. "Loss/gain framing, dose, and reactance: a message experiment." *Risk Analysis* 39(12):2640–2652.

Reeves, Andrew and Jon C Rogowski. 2016. "Unilateral powers, public opinion, and the presidency." *The Journal of Politics* 78(1):137–151.

Reeves, Andrew and Jon C Rogowski. 2018. "The public cost of unilateral action." *American Journal of Political Science* 62(2):424–440.

Savarese, Mauricio. 2018. "Leading Brazil candidate says he fears electoral fraud." Associated Press. September 17, 2018. Downloaded February 16th, 2020 from https://apnews.com/d75824e19eac49d9b6f2cfec8d9daf12/Leading-Brazil-candidate-says-he-fears-electoral-fraud.

Schaffner, Brian F and Samantha Luks. 2018. "Misinformation or expressive responding? What an inauguration crowd can tell us about the source of political misinformation in surveys." *Public Opinion Quarterly* 82(1):135–147.

Sinclair, Betsy, Steven S Smith and Patrick D Tucker. 2018. "'It's Largely a Rigged System': Voter Confidence and the Winner Effect in 2016." *Political Research Quarterly* 71(4):854–868.

Taber, Charles S. and Milton Lodge. 2006. "Motivated Skepticism in the Evaluation of Political Beliefs." *American Journal of Political Science* 50(3):755–769.

Thorson, Emily. 2016. "Belief echoes: The persistent effects of corrected misinformation." *Political Communication* 33(3):460–480.

Udani, Adriano, David C Kimball and Brian Fogarty. 2018. "How local media coverage of voter fraud influences partisan perceptions in the United States." *State Politics & Policy Quarterly* 18(2):193–210.

Weeks, Brian E and Homero Gil de Zúñiga. 2019. "What's next? Six observations for the future of political misinformation research." *American Behavioral Scientist* .

Wood, Thomas and Ethan Porter. 2019. "The elusive backfire effect: Mass attitudes' steadfast factual adherence." *Political Behavior* 41(1):135–163.

Zaller, John. 1992. *The Nature and Origins of Mass Opinion*. Cambridge University Press.

Online Appendix A: Survey details

Survey details

Participants in the U.S. study were YouGov panel members who consented to participate in an online study (YouGov determines the specific eligibility and exclusion criteria for their panel). Researchers have no role in selecting the participants. This study was conducted among a representative sample of the U.S. population by YouGov, which recruits a large panel of opt-in respondents and then uses a weighting and matching algorithm to construct a final sample that mirrors the demographic composition of the U.S. population. Our participants closely resemble the U.S. population in both demographics and political attitudes and affiliations (see demographics reported in the Table A1). The experimental results we present do not use survey weights per Franco et al. (2017) and Miratrix et al. (2018).

The survey was a two-wave panel conducted from November 20–December 27, 2018 (Wave 1, N=4,907) and December 14, 2018–January 3, 2019 (Wave 2, N=4,283) as part of a larger study reported in a different paper. The voter fraud experiment reported in the main text took place almost exclusively in Wave 2, although we use a few background questions listed below from Wave 1.

We coded respondents' Pulse data, categorizing mainstream news visit, fact-checking visit, and fake news visits (see Appendix D) computed as a binary measure of exposure to the aforementioned types of content, as well as a a count of total webpages visited from each category during the 7 days following Wave 1. Data was collected by YouGov via anonymized web traffic data from respondents. However, the Pulse data is used only as a moderator in our exploratory analysis in Appendix D and was not used for any results in the main text.

Figure A1: Experimental design and process

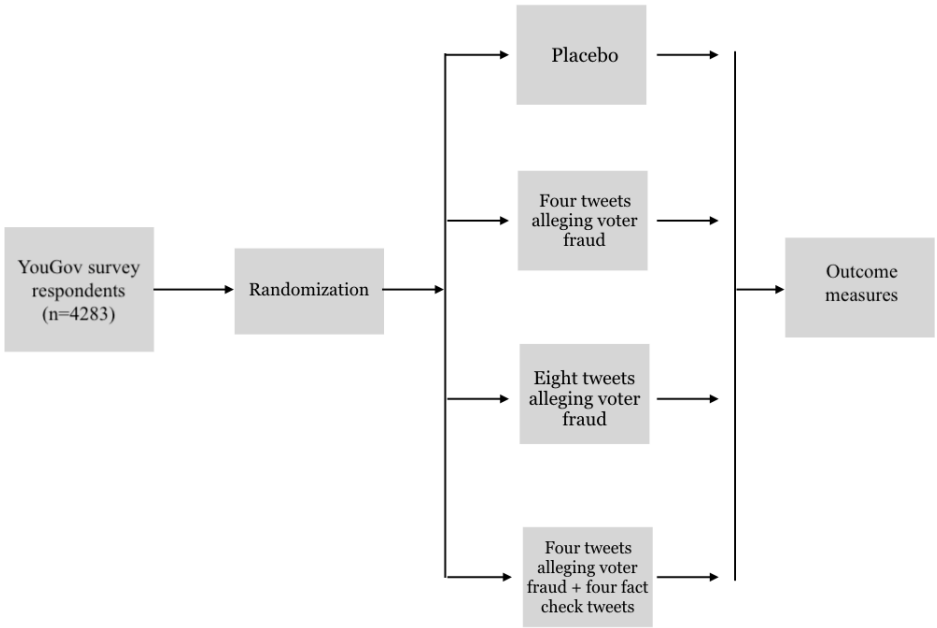


Table A1: Characteristics of YouGov sample

Characteristic	Sample	Census	Gallup
Education			
Less than high school	4.2%	15.2%	-
High school graduate	31.4%	27.3%	-
Some college/less than four-year degree	32.5%	26.8%	-
Bachelor's degree	20.3%	19.6%	-
Postgraduate degree	11.6%	11.0%	-
Age			
18–24	6.7%	13.1%	-
25–44	34.4%	35.0%	-
45–64	35.9%	34.7%	-
65 and older	23.1%	17.2%	-
Gender			
Male	45.4%	48.4%	-
Female	54.6%	51.6%	-
Party			
Democrats	36.8%	-	34.0%
Republicans	26.2%	-	25.0%
Independents	37.0%	-	39.0%
Trump approval			
Disapprove	43.1%	-	37.0%
Approve	56.9%	-	59.0%

Unweighted YouGov survey sample. Sources for population benchmarks: education (United States Census Bureau 2020), age and gender (Howden and Meyer 2011), party (Gallup 2020*a*), and Trump approval (Gallup 2020*b*).

Wave 1

Party ID questions

Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent,or something else? (Options: Republican, Democrat, Independent, Something else)

[if Democrat selected] Would you call yourself a strong Democrat or not a very strong Democrat? (Options: Strong Democrat or Not very strong Democrat)

[if Republican selected] Would you call yourself a strong Republican or not a very strong Republican? (Options: Strong Republican or Not very strong Republican)

Political interest

Some people seem to follow what's going on in government and public affairs most of the time, whether there's an election going on or not. Others aren't that interested. Would you say you follow what's going on in government and public affairs? (Options: Most of the time, Some of the time, Only now and then, Hardly at all, Don't know)

Political knowledge

Questions below used to create a scale measuring political knowledge that ranges from 0 (no questions correct) to 8 (all questions correct)

How many times can an individual be elected President of the United States under current laws? (Options: Once, Twice, Four times, Unlimited number of terms, Don't know)

How many U.S. Senators are there from each state? (Options: One, Two, Depends on which state, Don't know)

Who is currently the Prime Minister of the United Kingdom? (options: Richard Branson, Nick Clegg, David Cameron, Theresa May, Margaret Thatcher, Don't know)

For how many years is a member of the United States House of Representatives elected - that is, how many years are there in one full term of office for a U.S. House member? (Options: Two years, Four years, Six years, Eight years, For life, Don't know)

Conspiracy predispositions - mean of four items:

Much of our lives are being controlled by plots hatched in secret places. (Options: Strongly agree (5), Somewhat agree (4), Neither disagree nor disagree (3), Somewhat disagree (2), Strongly disagree (1)

Even though we live in a democracy, a few people will always run things anyway. (Options: Strongly agree (5), Somewhat agree (4), Neither disagree nor disagree (3), Somewhat disagree (2), Strongly disagree (1)

The people who really 'run' the country are not known to the voter. (Options: Strongly agree (5), Somewhat agree (4), Neither disagree nor disagree (3), Somewhat disagree (2),Strongly disagree (1)

Big events like wars, recessions, and the outcomes of elections are controlled by small groups of people who are working in secret against the rest of us. (Options: Strongly agree (5), Somewhat agree (4), Neither disagree nor disagree (3), Somewhat disagree (2), Strongly disagree (1)

Trust in confidence in mass media in reporting news

In general, how much trust and confidence do you have in the mass media – such as newspapers,

TV and radio – when it comes to reporting the news fully, accurately and fairly? (Options: A great deal, A fair amount, Not very much, None at all)

Other background variables
In what year were you born? (open text response)
What is your gender? (Options: Male, Female, Other)
What racial or ethnic group best describes you? (Options: White, Black or African-American, Hispanic or Latino, Asian or Asian-American, Native American, Middle Eastern, Mixed Race, Other)
What is the highest level of education you have completed? (Options: Did not graduate from high school; High school graduate; Some college, but no degree (yet); 2-year college degree; 4-year college degree; Postgraduate degree (MA, MBA, MD, JD, PhD, etc.))
Who did you vote for in the election for President? (Options: Hillary Clinton, Donald Trump, Gary Johnson, Jill Stein, Evan McMullin, Other, Did not vote)

Wave 2

In general, how much trust and confidence do you have in the mass media - such as newspapers, TV and radio - when it comes to reporting the news fully, accurately and fairly? (Options: A great deal, A fair amount, Not very much, None at all)

In general, how much trust and confidence do you have in the information you see on Facebook when it comes to reporting the news fully, accurately, and fairly? (Options: A great deal, A fair amount, Not very much, None at all)

We would like to get your feelings toward some groups, leaders, and institutions who are in the news these days using something we call the feeling thermometer. Ratings between 50 degrees and 100 degrees mean that you feel favorable and warm toward the group, leader, or institution. Ratings between 0 degrees and 50 degrees mean that you don't feel favorable toward them and that you don't care too much for them. You would rate them at the 50 degree mark if you don't feel particularly warm or cold toward them. If we come to a group, leader, or institution whose name you don't recognize, you don't need to rate them. (Respondents click on thermometer to give ratings for: White people, Hispanic or Latino people, Christians, Muslims)

Experimental manipulation:

Control - series of non-political, control tweets

Condition 1 - random subset of 4 of 8 election fraud/meddling tweets below in random order

Condition 2 - all 8 of the election meddling tweets below in random order

Condition 3 - random subset of 4 of 8 election fraud/meddling tweets below in random order and all 4 of the no fraud/no meddling tweets (in random order)

Election fraud/meddling tweets:

Marco Rubio @marcorubio · Nov 10

#BrowardElections office admits the vote count they submitted to state includes 22 illegal votes.

We know about these 22 because they got caught breaking law in reviewing 202 ballots. How can anyone trust more illegal votes aren' in their final count?

Broward elections office included 22 void ballots in its final total sent ...

Broward Supervisor of Elections Brenda Snipes had accidentally mixed the valid ballots with the invalid ones after she had initially removed the ballo...

miamiherald.com

5.0K 8.0K 14K

Donald J. Trump @realDonaldTrump · 7h

The Florida Election should be called in favor of Rick Scott and Ron DeSantis in that large numbers of new ballots showed up out of nowhere, and many ballots are missing or forged. An honest vote count is no longer possible-ballots massively infected. Must go with Election Night!

35K 26K 87K

Dinesh D'Souza @DineshDSouza · Nov 9

Trump is showing he is not the standard GOP invertebrate who gasps in impotent frustration as **Democrats** coolly **steal** elections. The crooks are going to be exposed, thwarted and—if warranted— arrested!

723 8.9K 23K

The Senate Majority Retweeted

Camille M. Gallo @camillegallo · 29m

Now @SenBillNelson is suing to count ballots that were received AFTER Election Day.

How desperate can this guy get?

nrsc.org/press-releases… #FLSEN #SAYIE

reeks of desperation - NRSC

Bill Nelson just keeps getting more and more desperate. Now Nelson and his lawyers are suing to count ballots that were received AFTER Election...

nrsc.org

4 17 13

Lindsey Graham @LindseyGrahamSC · Nov 9

When it comes to confirming judges and counting votes, Democrats are amazingly consistent – The law is NO obstacle to the outcome they seek.

> **Rick Scott** @ScottforFlorida
> US Senate candidate, FL
> Every day since the election, Broward county have been coming up with more and more ballots out of nowhere. We all know what is going on. I will not sit idly by while unethical liberals try to steal this election from the people of Florida.

6.7K 14K 35K

Rick Scott
@ScottforFlorida
US Senate candidate, FL

Follow

Every day since the election, Broward county have been coming up with more and more ballots out of nowhere. We all know what is going on. I will not sit idly by while unethical liberals try to steal this election from the people of Florida.

11:45 AM - 9 Nov 2018

10,638 Retweets **28,283** Likes

2.4K 11K 28K

No fraud/no meddling tweets:

PBS NewsHour @NewsHour · 16h

The state's law enforcement arm and elections monitors have found no evidence of wrongdoing.

'Ramp down the rhetoric,' judge in Florida recount case says

"We need to be careful of what we say. Words mean things these days," the judge told lawyers representing the warring sides in the Florida recount

pbs.org

7 51 82

NYT Politics @nytpolitics · 15h

Fact Check of the Day: Trump Makes a Baseless Claim About 'Massively Infected' Ballots in Florida

Trump Makes a Baseless Claim About 'Massively Infected' Ballots in ...

Without evidence, President Trump asserted that some ballots in Florida "showed up out of nowhere" and that others were missing or forged.

nytimes.com

19 31 44

Control tweets:

Food & Wine @foodandwine · 10 Oct 2017

MTN is **Travis Lett**'s most ambitious project yet: trib.al/2HoQzyF

9 28

JBF Taste America LA @TasteAmericaLA · Aug 24

James Beard Nominated Chef **Travis Lett** puts together delicious dishes at his restaurant, Gjelina. This grilled squid, chicory, mandarinquat and salsa verde is a must try here. Hit the link in bio for more!

1

Attention check (varies by condition)

What news event was mentioned in the tweets you just read? [shown to respondents in treatment conditions] (Options: The elections in November 2012, The elections in November 2016, The elections in November 2018, The elections in November 2020)

Which of these individuals was featured in the tweets you just read? [shown to respondents in control condition] (Options: Travis Lett, Los Angeles chef; James Johnson, New York chef; Steve Clifford, Chicago chef; John Wright, Miami chef)

Confidence measures:

How confident are you that everyone who was legally entitled to vote and sought to do so was able to successfully cast a ballot in the election this November? (Options: Very confident, Somewhat confident, Not too confident, Not at all confident)

[If they said they voted][1] How confident are you that your vote was accurately counted in the election this November? (Options: Very confident, Somewhat confident, Not too confident, Not at all confident)

How confident are you that election officials managed the counting of ballots fairly in the election this November? (Options: Very confident, Somewhat confident, Not too confident, Not at all con-

[1]Only respondents who indicated "I am sure I voted" in response to the following question ("In talking to people about elections, we often find that a lot of people were not able to vote because they weren't registered, they were sick, or they just didn't have time. Which of the following statements best describes you?") were shown this question. All other confidence measure items were asked to every respondent.

fident)

Do you agree or disagree with the following statement?
At the end of the day, in spite of all the problems casting and counting the votes, the system worked. (Options: Strongly agree, Somewhat agree, Somewhat disagree, Strongly disagree)

To what extent do you trust elections in this country? Please respond on the scale below where 1 means "not at all" and 7 means "a lot." (Options: 1 Not at all, 2, 3, 4, 5, 6, 7 A lot)

How secure are ballots from tampering in this country's elections? (Options: Extremely secure, Very secure, Moderately secure, Not too secure, Not at all secure)

How often are voting machines accurate in counting the votes? (Options: Extremely often, Very often, Moderately often, Not too often, Not at all often)

Thermometer ratings
We would like to get your feelings toward some groups, leaders, and institutions who are in the news these days using something we call the feeling thermometer. Ratings between 50 degrees and 100 degrees mean that you feel favorable and warm toward the group, leader, or institution. Ratings between 0 degrees and 50 degrees mean that you don't feel favorable toward them and that you don't care too much for them. You would rate them at the 50 degree mark if you don't feel particularly warm or cold toward them. If we come to a group, leader, or institution whose name you don't recognize, you don't need to rate them. (Respondents click on thermometer to give ratings for: Democratic Party, Republican Party, President Trump, The news media)

How important is it for you to live in a country that is governed democratically? Please respond below on this scale where 1 means it is "not at all important" and 10 means "absolutely important." (Options: 1 Min, 2, 3, 4, 5, 6, 7, 8, 9, 10 Max)

Various types of political systems are described below. Please think about each choice in terms of governing this country and indicate if you think it would be a very good, fairly good, fairly bad, or very bad way of governing the United States.
Having a strong leader who does not have to bother with Congress and elections: (Options: Very good, Fairly good, Fairly bad, Very bad)
Having experts, not government, make decisions according to what they think is best for the country: (Options: Very good, Fairly good, Fairly bad, Very bad)
Having the army rule: (Options: Very good, Fairly good, Fairly bad, Very bad)
Having a democratic political system: (Options: Very good, Fairly good, Fairly bad, Very bad)

Online Appendix B

The survey instrument included twelve survey items measuring attitudes towards the integrity of the elections (see Online Appendix A). These items measured perceptions of perceived electoral integrity including ballot security, machine accuracy, and fairness. Following our preregistration we conducted an exploratory factor analysis, which indicated three underlying dimensions. The results for this analysis are shown in Table B1. We selected the seven items marked with a † in Table B1 as the components of our composite outcome.

Table B1: Preregistered factor analysis of all measured outcomes

	Factor 1	Factor 2	Factor 3	Uniqueness
Confidence entitled allowed to vote†	0.6103	-.0102	0.2473	0.5663
Confidence own vote was counted†	0.6875	-0.1451	-0.1471	0.4847
Confidence officials manage counting votes†	0.8193	.0791	-.06810	0.3178
System works despite problems casting and counting votes†	0.8221	.0280	-.0533	0.3205
Trust in elections†	-0.8265	-.0178	0.1088	0.3048
Security of ballots from tampering†	0.8287	.0845	-.0760	0.3003
Frequency voting machines accurate in counting votes†	0.7604	-.0962	-.0910	0.4042
Importance of living in democratically governed country	-0.1218	0.1380	0.8431	0.2554
Having a strong leader who doesn't have to both with parliament/elections	0.0296	0.8191	0.2352	0.2729
Having experts, not government, make decisions	-0.0419	0.7572	-0.1416	0.4048
Having the army rule	0.0551	0.8079	0.2133	0.2988
Having a democratic political system	0.0798	-0.1139	-0.8647	0.2330

Exploratory factor analysis of the outcome measures that we preregistered that we would consider to determine if they scaled together after varimax rotation. Question wording for each item is presented in Online Appendix A. † indicates items chosen for final measure.

Based on these results, we fit a confirmatory factor analysis reported in Table B2. This model was identified by setting the mean of the latent trait to zero and the variance to unity. All factor loadings were large and significant, indicating an adequate fit.

Table B2: Structural equation model for latent election confidence measure

	Coefficient	Constant	Variance
Confidence entitled allowed to vote	0.535	2.574	0.714
	(0.012)	(0.032)	(0.013)
Confidence own vote was counted	0.639	3.523	0.592
	(0.011)	(0.047)	(0.014)
Confidence officials manage counting votes	0.790	2.782	0.376
	(0.007)	(0.034)	(0.011)
System works despite problems casting and counting votes	0.795	3.088	0.368
	(0.007)	(0.037)	(0.011)
Trust in elections	0.798	2.819	0.363
	(0.067)	(0.034)	(0.011)
Security of ballots from tampering	0.816	2.789	0.334
	(0.006)	(0.034)	(0.010)
Frequency voting machines accurate in counting votes	0.725	3.568	0.474
	(0.008)	(0.042)	(0.012)

All coefficients are statistically significant ($p < .001$). Structural equation model of the outcome measures that we identified as scaling together in Table B1. Latent variable estimated using maximum likelihood with missing values. Question wording for each item is presented in Online Appendix A. N=4,280; $\chi^2(df = 14)$=487.36, $p < .001$; CFI=0.967; TLI=0.950; RMSEA = 0.089

As noted in the main text, our preregistration was ambiguous as to how to handle the remaining five items in the event they did not load onto the main factor. Because there are too few items to estimate latent variables for the second and third factor from the exploratory factor analysis, we take the average for the items in each factor when modeling effects of our treatments (responses to importance of living in a democracy are rescaled to 1–4 to match the other outcome variables). It is possible to also estimate these composite scores using some sort of latent trait analysis, but the two-item battery would be unidentified (without adding additional parameter constraints) and the three-item battery would be just-identified making it impossible to adequately asses fit. We therefore rely on the simpler additive model, but also examine each component separately in Table C4 below. As we cannot clearly articulate what makes these latent traits distinguishable, we use the agnostic labels "composite 1" and "composite 2" below (both refer to general support for democracy and democratic institutions).

Online Appendix C: Full results for main text

The model specification in Table C1 deviates from our preregistration. Our preregistered analysis, which is reported in Table C3, pools Democrats and independents and analyzes them separately from Republicans. However, independents reacted to the messages somewhat differently from Democrats. We therefore disaggregate Democrats and independents in our analysis and consider them separately from Republicans, who are the omitted category in our heterogeneous effects model (see Table C1). As described below, the analysis of how treatment effects vary by Trump approval is exploratory and was not preregistered. To mirror the party interaction model, we make Trump approvers the omitted category in that model (Table C2).

Our original hypotheses concerned how the treatments would affect "confidence in elections and support for democracy" pending the factor analysis reported in Appendix B. As noted in the main text, the seven election confidence scores did load onto a single trait but the five "support for democracy" items loaded onto two separate dimensions. We therefore created two additive composite scores as specified in Appendix B. This choice was not preregistered in the sense that we failed to specify how we would handle these five items if they did not load onto the main underling dimension. However, we do test all of our hypotheses for both composite scores in Tables C4–C7. With one exception, there is insufficient evidence to conclude that exposure to these claims measurably affected support for democracy.

Table C1: Effect of exposure to voter fraud allegations on election confidence by party

	Coefficient (SE)
Low dose	-0.184**
	(0.070)
High dose	-0.273***
	(0.071)
Low dose + fact-check tweets	-0.176*
	(0.072)
Democrat	-0.287***
	(0.065)
Independent	-0.450***
	(0.088)
Low dose × Democrat	0.099
	(0.092)
High dose × Democrat	0.252**
	(0.095)
Low dose + fact-check × Democrat	0.193*
	(0.094)
Low dose × independent	0.008
	(0.125)
High dose × independent	-0.010
	(0.125)
Low dose + fact-check tweets × independent	0.027
	(0.127)
Constant	0.309***
	(0.049)
Effect of high dose (versus low dose)	
Democrats	0.064
	(0.062)
Republicans	-0.089
	(0.071)
Difference (H3b)	0.153
	(0.094)
Effects of low dose + fact-check (versus low dose)	
Democrats	0.103
	(0.060)
Republicans	0.008
	(0.072)
Difference (H4b)	0.094
	(0.094)
N	4283

* $p < 0.05$, * $p < 0.01$, *** $p < .005$ (two-sided). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Partisan leaners are treated as members of the party in question.

Table C2: Effect of exposure to voter fraud allegations on election confidence by Trump approval

	Coefficient (SE)
Low dose	-0.211***
	(0.064)
High dose	-0.339***
	(0.066)
Low dose + fact-check	-0.190***
	(0.066)
Disapprove of Trump	-0.333***
	(0.060)
Low dose × disapprove of Trump	0.114
	(0.085)
High dose × disapprove of Trump	0.304***
	(0.087)
Low dose + fact-check × disapprove of Trump	0.178*
	(0.087)
Constant	0.288***
	(0.046)
Effect of high dosage (versus low)	
Disapprover	0.063
	(0.057)
Approver	-0.128*
	(0.065)
Difference	-0.191*
	(0.087)
Effects of low dose + fact-check (versus low dose)	
Disapprover	0.086
	(0.057)
Approver	0.021
	(0.065)
Difference	0.065
	(0.087)
N	4281

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Trump approval indicator are respondents who do not approve).

Table C3: Effect of exposure to voter fraud allegations on election confidence by party (Republicans vs. Democrats/independents)

	Coefficient (SE)
Low dose	-0.115*
	(0.052)
High dose	-0.106
	(0.054)
Low dose + fact-check	-0.035
	(0.053)
Republican	0.330***
	(0.062)
Low dose × Republican	-0.069
	(0.087)
High dose × Republican	-0.167
	(0.089)
Low dose + fact-check × Republican	-0.141
	(0.090)
Constant	-0.020
	(0.037)
Effect of high dosage (versus low)	
Republican	-0.089
	(0.071)
Non-Republican	0.010
	(0.054))
Difference	-0.099
	(0.089)
Effects of low dose + fact-check (versus low dose)	
Republican	-0.008
	(0.072)
Non-Republican	0.080
	(0.053))
Difference	-0.072
	(0.089)
N	4283

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Republican indicator includes both Democrats and independents). Partisan leaners are treated as members of the party in question. This table is included in addition to Table C1 per the preregistration.

Table C4: Main treatment effects for support for democracy

	Strong leader	Experts make decisions	Army rule	Democratic pol. system	Importance of living in democ.	Composite (cols. 1–3)	Composite (cols. 4–5)
Low dose	0.005	0.017	-0.007	0.025	-0.016	0.005	0.004
	(0.044)	(0.043)	(0.037)	(0.035)	(0.021)	(0.034)	(0.015)
High dose	-0.029	-0.027	-0.023	0.038	-0.015	-0.027	0.011
	(0.044)	(0.043)	(0.037)	(0.035)	(0.021)	(0.034)	(0.014)
Low dose + fact-check	-0.086	-0.047	-0.067	0.006	-0.013	-0.070*	-0.003
	(0.045)	(0.043)	(0.038)	(0.035)	(0.021)	(0.035)	(0.014)
Constant	3.182***	2.741***	3.450***	1.587***	2.186***	3.124***	1.889***
	(0.031)	(0.031)	(0.027)	(0.024)	(0.015)	(0.024)	(0.010)
Effect of higher dosage	-.034	-.044	-.017	.012	.003	-.031	.005
	(.044)	(.042)	(.038)	(.036)	(.085)	(.033)	(.038)
Effect of fact-check	-.091	-.064	-.061	-.020	.009	-.075*	.002
	(.044)	(.043)	(.038)	(.035)	(.084)	(.034)	(.038)
N	4240	4240	4229	4237	4264	4250	4278

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided). OLS models with robust standard errors. Responses to importance of living in a democracy rescaled to 1–4 to match the other outcome variables. Composites are based on the results of the exploratory factor analysis in Online Appendix B. The first is the average of the outcomes measured in columns 1–3. The second is the average of the outcomes measured in columns 4–5.

Table C5: Effect of exposure to voter fraud allegations on support for democracy by party

	Composite 1	Composite 2
Low dose	-0.006	-0.005
	(0.060)	(0.027)
High dose	-0.046	-0.000
	(0.059)	(0.026)
Low dose + fact-check	-0.023	-0.019
	(0.061)	(0.027)
Democrat	0.070	-0.157***
	(0.054)	(0.021)
Independent	-0.100	-0.005
	(0.072)	(0.035)
Low dose × Democrat	0.016	0.020
	(0.075)	(0.032)
High dose × Democrat	0.065	0.024
	(0.076)	(0.031)
Low dose + fact-check × Democrat	-0.101	0.050
	(0.078)	(0.031)
Low dose × Independent	0.028	-0.001
	(0.101)	(0.048)
High dose × Independent	-0.026	-0.017
	(0.098)	(0.047)
Low dose + fact-check × Independent	0.015	-0.039
	(0.102)	(0.047)
Constant	3.108***	1.963***
	(0.042)	(0.018)
Effect of high dose (versus low dose)		
Democrats	0.009	0.009
	(0.046)	(0.018)
Republicans	-0.040	0.005
	(0.059)	(0.027)
Difference (H3b)	0.049	0.004
	(0.075)	(0.032)
Effects of low dose + fact-check (versus low dose)		
Democrats	-0.134***	0.016
	(0.047)	(0.018)
Republicans	-0.017	-0.014
	(0.061)	(0.028)
Difference (H4b)	-0.117	0.030
	(0.077)	(0.033)
N	4250	4278

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided). OLS models with robust standard errors. Outcome variables are composite measures of support for democracy (see Online Appendix B for estimation details). Partisan leaners are treated as members of the party in question.

Table C6: Effect of exposure to voter fraud allegations on support for democracy by Trump approval

	Composite 1	Composite 2
Low dose	0.026	-0.007
	(0.056)	(0.025)
High dose	-0.029	-0.004
	(0.056)	(0.024)
Low dose + fact-check	-0.031	-0.036
	(0.057)	(0.024)
Disapprove of Trump	0.151***	-0.153***
	(0.050)	(0.020)
Low dose × disapprove of Trump	-0.037	0.020
	(0.069)	(0.030)
High dose × disapprove of Trump	0.003	0.028
	(0.069)	(0.029)
Low dose + fact-check × disapprove of Trump	-0.070	0.061*
	(0.071)	(0.029)
Constant	3.040***	1.975***
	(0.040)	(0.017)
Effect of high dosage (versus low)		
Disapprover	-0.016	0.012
	(0.041)	(0.017)
Approver	-0.055	0.003
	(0.055)	(0.025)
Difference	0.039	0.009
	(0.068)	(0.030)
Effects of low dose + fact-check (versus low dose)		
Disapprover	-0.091*	0.012
	(0.041)	(0.017)
Approver	-0.057	-0.029
	(0.065)	(0.025)
Difference	-0.034	0.041
	(0.070)	(0.030)
N	4248	4276

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided). OLS models with robust standard errors. Outcome variables are composite measures of support for democracy (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Trump approval indicator are respondents who do not approve).

Table C7: Effect of exposure to voter fraud allegations on support for democracy by party (Republicans vs. Democrats/Independents)

	Composite 1	Composite 2
Low dose	0.010	0.013
	(0.040)	(0.017)
High dose	-0.015	0.018
	(0.041)	(0.017)
Low dose + fact-check	-0.096*	0.010
	(0.042)	(0.016)
Republican	-0.026	0.118***
	(0.051)	(0.022)
Low dose × Republican	-0.016	-0.018
	(0.072)	(0.032)
High dose × Republican	-0.031	-0.018
	(0.072)	(0.031)
Low dose + fact-check × Republican	0.072	-0.029
	(0.074)	(0.031)
Constant	3.134***	1.846***
	(0.029)	(0.011)
Effect of high dosage (versus low)		
Republican	-0.040	0.005
	(0.059)	(0.027)
Non-Republican	-0.025	0.005
	(0.040)	(0.017)
Difference	-0.015	0.000
	(0.071)	(0.032)
Effects of low dose + fact-check (versus low dose)		
Republican	-0.017	-0.014
	(0.061)	(0.028)
Non-Republican	-0.106**	-0.003
	(0.041)	(0.017)
Difference	0.088	-0.011
	(0.073)	(0.032)
N	4250	4278

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided). OLS models with robust standard errors. Outcome variables are composite measures of support for democracy (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Republican indicator is Democrats and independents). Partisan leaners are treated as members of the party in question.

Online Appendix D: Exploratory analysis of additional preregistered moderators

This appendix reports exploratory analyses of potential moderators of the effect of fraud messages on beliefs about and confidence in elections and democracy. These potential moderators include trust in and feelings toward the media, feelings toward Trump, conspiracy predispositions, political interest and knowledge, and pre-treatment visits to fake news sites and fact-checking sites. We control the false discovery rate with the Benjamini-Hochberg procedure given the risk of false positives. We find limited evidence in support of these heterogeneous treatment effects.

We do not discuss the one significant interaction term we find — untrustworthy website visits (the only significant one in Online Appendix D after adjusting p-values for the interaction terms using the Benjamini and Hochberg 1995 procedure) — in the main text because only 74 respondents visited an untrustworthy website during the sample period. The results below are thus underpowered and likely reflect the correlation between party identification and exposure to untrustworthy websites during the study period (46 of the 69 respondents who visited an untrustworthy website identify as or lean Republican).

Table D1: Effect of exposure to voter fraud allegations on election confidence by feelings towards Trump

	Coefficient (SE)
Low dose	-0.095
	(0.061)
High dose	-0.029
	(0.064)
Low dose + fact-check	0.033
	(0.063)
Feelings towards Trump	0.003***
	(0.001)
Low dose × feelings towards Trump	-0.001
	(0.001)
High dose × feelings towards Trump	-0.003
	(0.001)
Low dose + fact-check × feelings towards Trump	-0.003
	(0.001)
Constant	-0.036
	(0.044)
N	4131

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; p-values of treatment × moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Republican indicator is Democrats and independents). Partisan leaners are treated as members of the party in question.

Table D2: Effect of exposure to voter fraud allegations on election confidence by feelings towards Trump (tercile indicators)

	Coefficient (SE)
Low dose	-0.091
	(0.072)
High dose	-0.039
	(0.077)
Low dose + fact-check	0.005
	(0.076)
Feels neutrally about Trump 2	0.316***
	(0.071)
Feels warmly about Trump	0.310***
	(0.077)
Low dose × feels neutrally	-0.072
	(0.102)
Low dose × feels warmly	-0.069
	(0.105)
High dose × feels neutrally	-0.103
	(0.105)
High dose × feels warmly	-0.235
	(0.109)
Low dose + fact-check × feels neutrally	-0.066
	(0.103)
Low dose + fact-check × feels warmly	-0.182
	(0.109)
Constant	-0.094
	(0.053)
N	4131

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; p-values of treatment × moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Republican indicator is Democrats and independents). Partisan leaners are treated as members of the party in question.

Table D3: Effect of exposure to voter fraud allegations on election confidence by media feelings

	Coefficient (SE)
Low dose	-0.271***
	(0.080)
High dose	-0.336***
	(0.082)
Low dose + fact-check	-0.189*
	(0.083)
Media feelings	0.002*
	(0.001)
Low dose × media feelings	0.003
	(0.001)
High dose × media feelings	0.004
	(0.001)
Low dose + fact-check × media feelings	0.002
	(0.001)
Constant	0.015
	(0.057)
N	4113

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; p-values of treatment × moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Media feelings measured using a 0–100 feeling thermometer.

Table D4: Effect of exposure to voter fraud allegations on election confidence by media trust

	Coefficient (SE)
Low dose	-0.339*
	(0.136)
High dose	-0.460***
	(0.136)
Low dose + fact-check	-0.259
	(0.140)
Trust in mass media	0.215***
	(0.036)
Low dose × trust in mass media	0.077
	(0.050)
High dose × trust in mass media	0.118
	(0.051)
Low dose + fact-check × trust in mass media	0.064
	(0.051)
Constant	-0.437***
	(0.097)
N	4282

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; p-values of treatment × moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Media trust measured using a four-point scale.

Table D5: Effect of exposure to voter fraud allegations on election confidence by conspiracy predispositions

	Coefficient (SE)
Low dose	-0.226
	(0.139)
High dose	-0.205
	(0.135)
Low dose + fact-check	-0.076
	(0.141)
Predisposed to conspiracy	-0.261***
	(0.029)
Low dose × predisposed to conspiracy	0.026
	(0.043)
High dose × predisposed to conspiracy	0.015
	(0.042)
Low dose + fact-check × predisposed to conspiracy	-0.003
	(0.044)
Constant	0.933***
	(0.094)
N	4263

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; p-values of treatment × moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Republican indicator is Democrats and independents). Partisan leaners are treated as members of the party in question.

Table D6: Effect of exposure to voter fraud allegations on election confidence by political interest

	Coefficient (SE)
Low dose	-0.367**
	(0.137)
High dose	-0.412***
	(0.139)
Low dose + fact-check	-0.215
	(0.141)
Politically interested	0.077***
	(0.026)
Low dose × politically interested	0.061
	(0.037)
High dose × politically interested	0.070
	(0.037)
Low dose + fact-check × politically interested	0.035
	(0.038)
Constant	-0.182
	(0.098)
N	4275

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; p-values of treatment × moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Republican indicator is Democrats and independents). Partisan leaners are treated as members of the party in question.

Table D7: Effect of exposure to voter fraud allegations on election confidence by political knowledge

	Coefficient (SE)
Low dose	-0.156
	(0.096)
High dose	-0.333***
	(0.098)
Low dose + fact-check	-0.043
	(0.098)
Politically knowledgeable	0.078***
	(0.020)
Low dose $\times$ politically knowledgeable	0.002
	(0.027)
High dose $\times$ politically knowledgeable	0.054
	(0.027)
Low dose + fact-check $\times$ politically knowledgeable	-0.014
	(0.028)
Constant	-0.141*
	(0.069)
N	4283

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; p-values of treatment $\times$ moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Republican indicator is Democrats and independents). Partisan leaners are treated as members of the party in question.

Due to the infrequency of visits to untrustworthy websites, we use a binary indicator of exposure below as the moderator in Table D8. We do not discuss the one significant interaction term we find (the only significant one in Online Appendix D after adjusting *p*-values for the interaction terms using the Benjamini and Hochberg 1995 procedure) in the main text because only 74 respondents visited an untrustworthy website during the sample period. The results below are thus underpowered and likely reflect the correlation between party identification and exposure to untrustworthy websites during the study period (46 of the 69 respondents who visited an untrustworthy website identify as or lean Republican).

Table D8: Effect of exposure to voter fraud allegations on election confidence by pre-treatment exposure to untrustworthy websites

	Coefficient (SE)
Low dose	-0.151
	(0.095)
High dose	-0.074
	(0.097)
Low dose + fact-check	0.094
	(0.098)
Visited untrustworthy websites (binary)	0.630***
	(0.205)
Low dose × visited untrustworthy websites	-0.646
	(0.314)
High dose × visited untrustworthy websites	-0.970
	(0.360)
Low dose + fact-check × visited untrustworthy websites	-0.979*
	(0.290)
Constant	0.134*
	(0.068)
N	923

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; *p*-values of treatment × moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Untrustworthy website exposure measured as a visit to one or more of the 673 domains identified in Allcott, Gentzkow and Yu (2018) as a fake news producer as of September 2018 excluding those with print versions (including but not limited to Express, the British tabloid) and also domains that were previously classified by Bakshy, Messing and Adamic (2015) as a source of hard news. In addition, we exclude sites that predominantly feature user-generated content (e.g., online bulletin boards) and political interest groups. All exposure measures are limited to the period observed in available behavioral data immediately before completing the survey among respondents who participate in the YouGov Pulse panel.

Table D9: Effect of exposure to voter fraud allegations on election confidence by pre-treatment visits to fact checking sites

	Coefficient (SE)
Low dose	-0.174
	(0.094)
High dose	-0.131
	(0.096)
Low dose + fact-check	0.012
	(0.097)
Visited fact checking site	0.336
	(0.259)
Low dose $\times$ visited fact check site	-0.605
	(0.399)
High dose $\times$ visited fact check site	-0.277
	(0.365)
Low dose + fact-check $\times$ visited fact check site	-0.135
	(0.352)
Constant	0.172*
	(0.067)
N	923

* $p < 0.05$, ** $p < 0.01$, *** $p < .005$ (two-sided; p-values of treatment $\times$ moderator interaction terms are adjusted to control the false discovery rate using the Benjamini and Hochberg 1995 procedure). OLS models with robust standard errors. Outcome variable is a composite measure of election confidence that was created using confirmatory factor analysis (see Online Appendix B for estimation details). Sample includes all respondents (i.e., the reference category for the Republican indicator is Democrats and independents). Partisan leaners are treated as members of the party in question.

Online Appendix E: Preregistration

This "populated pre-analysis plan" (Duflo et al. 2020) details the location of our preregistered results in the manuscript as well as departures from the plan. Our pre-analysis plan was filed in the EGAP registry and subsequently migrated to OSF at
`https://osf.io/tjq9w/?view_only=032681ce2a934c0494cdafd54616c36a`,
where all data and analysis scripts will be shared.

It is important to clarify that the preregistration is time-stamped February 20, 2019 even though data were collected in December 2018/January 2019. However, it was filed prior to data delivery from YouGov, which was withheld until February 27, 2019 — after the preregistration was filed. (See anonymized letter here from YouGov:
`https://www.dropbox.com/s/1co33ljx54ddk5g/yougov-statement.pdf`).

In order to facilitate comparing these models to our preregistration, hypothesis labelling in the below section reflects the original preregistration document, not the main manuscript text. However, the main text and the preregistration diverge in two ways. First, the main text hypotheses do not contain the "E" prefix.[2] Second, our preregistration discussed that the number/content of outcome variables would depend on a factor analysis of variables that focus on election confidence and support for democracy. RQ2 is designed to capture this particular aspect of the preregistration, even though we did not formally write it as a research question. In other words, RQ2 allows us to to account for separately analyze election confidence and support for democracy as outcome measures as specified in the preregistration.

Construction of outcome measures

Our outcome measures are confidence in elections and support for democracy. We measure these using items reported in Online Appendix A. We will analyze these items as a composite measure if they scale together using principal components factor analysis. If they do not scale together, we will analyze them separately (as separate composite measures and/or individual outcome measures). If we analyze one or more composite measures, we will also report results separately for each dependent variable included in the composite measure(s) in the appendix.

- For factor analysis of all measured outcomes, see Table B1 in Online Appendix B.

Preregistered hypotheses and research questions

Effects of tweet exposure

H-E1a/b. Exposure to four tweets including claims of voter or election fraud will reduce confidence in elections and support for democracy compared to a placebo condition (H-E1a), especially among respondents for whom those messages are pro-attitudinal (H-E1b).

H-E2a/b. Exposure to eight tweets including claims of voter or election fraud will reduce confidence in elections and support for democracy compared to a placebo condition (H-E1a), especially

[2]Results for Hypothesis Groups A—D in the preregistration concern orthogonal studies reported in (omitted for peer review). Including the "E" prefix is thus more likely to cause confusion than alleviate it.

among respondents for whom those messages are pro-attitudinal (H-E1b).

H-E3a/b. Exposure to eight tweets including claims of voter or election fraud will reduce confidence in elections and support for democracy more strongly than exposure to four tweets including such claims (H-E3a), especially among respondents for whom those messages are pro-attitudinal (H-E3b).

H-E4a/b. Exposure to four tweets including claims of voter or election fraud and four tweets fact-checking those claims will reduce confidence in elections and support for democracy less than exposure to four tweets including claims of voter or election fraud without fact-checks (H-E4a), especially among respondents for whom the voter or election fraud messages are pro-attitudinal (H-E4b).

RQ-E1a/b. Does exposure to four tweets including claims of voter or election fraud and four tweets fact-checking those claims reduce confidence in elections and support for democracy relative to a placebo (RQ-E1a), especially among respondents for whom the voter or election fraud messages are pro-attitudinal (RQ-E1b)?

Note: Based on the results of the preregistered factor analysis described above, effects on support for democracy items are separated out in the main text and described under RQ2.

Models

For each of the main effects hypotheses, we will estimate the following models using OLS regression (with robustness checks using ordered probit where appropriate): Main effects: Outcome = [constant] + 4 fraud tweet exposure + 8 fraud tweet exposure + 4 fraud/4 fact-check tweet exposure

For H-E1a: the coefficient for "4 fraud tweet exposure" will serve as the hypothesis test. A negative coefficient will support H-E1a. For H-E2a: the coefficient for "8 fraud tweet exposure" will serve as the hypothesis test. A negative coefficient will support H-E2a. For H-E3a: lincom "8 fraud tweet exposure" - "4 fraud tweet exposure" will serve as the hypothesis test. A positive coefficient will support H-E3a. For H-E4a: lincom "4 fraud tweet exposure" - "4 fraud/4 fact-check tweet exposure" will serve as the hypothesis test. A positive coefficient will support H-E4a. For RQ-E1a: the coefficient for "4 fraud/4 fact-check tweet exposure" will serve as the RQ test.

For the congeniality moderations, we will estimate the following models using OLS regression: Outcome = [constant] + 4 fraud tweet exposure + 8 fraud tweet exposure + 4 fraud/4 fact-check tweet exposure + Republican + 4 fraud tweet exposure*Republican + 8 fraud tweet exposure*Republican + 4 fraud/4 fact-check tweet exposure*Republican

For H-E1b: the coefficient for "4 fraud tweet exposure*Republican" will serve as the hypothesis test. A negative coefficient will support H-E1b. For H-E2b: the coefficient for "8 fraud tweet exposure*Republican" will serve as the hypothesis test. A negative coefficient will support H-E2b. For H-E3b: lincom "8 fraud tweet exposure*Republican" - "4 fraud tweet exposure*Republican" will serve as the hypothesis test. A positive coefficient will support H-E3b. For H-E4b: lincom "4 fraud tweet exposure*Republican" - "4 fraud/4 fact-check tweet exposure*Republican" will serve as the hypothesis test. A positive coefficient will support H-E4b. For RQ-E1b: the coefficient for "4 fraud/4 fact-check tweet. exposure*Republican" will serve as the RQ test.

Location of results

- For all models of main effects on election confidence (H-E1a, H-E2a, H-3a, H-4a, and RQ-1a), see main text Table 2, column 8 (composite measure).
- For all models of main effects on support for democracy (H-E1a, H-E2a, H-3a, H-4a, and RQ-1a), see Table C4, columns 6 and 7 (composite measures).
- For all models of effects on election confidence by Republican affiliation (H-E1b, H-E2b, H-3b, H-4b, and RQ-1b) see Table C3.
- For all models of effects on election confidence by Republican, Democrat, and independent affiliation (exploratory) see Table C1.
- For all models of effects on support for democracy by Republican affiliation (H-E1b, H-E2b, H-3b, H-4b, and RQ-1b) see Table C7.
- For all models of effects on support for democracy by Republican, Democrat, and independent affiliation (exploratory) see Table C5.

Heterogeneous treatment effects

We will also conduct exploratory analyses of potential moderators of the effect of fraud messages on beliefs about and confidence in elections and democracy: trust in and feelings toward the media, feelings toward Trump (entered as a linear term and with indicators for terciles or quartiles), conspiracy predispositions, political interest and knowledge, and pre-treatment visits to fake news sites and fact-checking sites. For these exploratory analyses of potential moderators, we control the false discovery rate with the Benjamini-Hochberg procedure given the risk of false positives. These analyses will be limited to the appendix or supplementary materials, but if any positive findings replicate in future studies, we may then use these data and analyses in the main text of a paper.

Models

For the exploratory analyses of possible moderators of the effects of fraud message exposure, the outcome measures are election confidence and support for democracy. Due to likely collinearity between the predictors, we will estimate separate models for each potential moderator for each outcome measure. E.g.: Outcome = [constant] + 4 fraud tweet exposure + 8 fraud tweet exposure + 4 fraud/4 fact-check tweet exposure + feelings toward Trump + 4 fraud tweet exposure*feelings toward Trump + 8 fraud tweet exposure*feelings toward Trump + 4 fraud/4 fact-check tweet exposure*feelings toward Trump

Location of results

- For exploratory tests of possible moderators, see Tables D1–D9 in Online Appendix D.

Online Appendix F: Power simulations for main effects and party interactions

As noted in our pre-registration, we did not conduct a power analysis for this study in advance. However, we can consider the power of our design to provide additional context for our results. While there are many ways to conduct power analyses for main effects, the literature remains relatively unsettled as to how to handle power calculations for interactions.

To put our entire discussion of power in a common framework, we use the `DeclareDesign` R package (Blair et al. 2019), which specifically allows us to analyze features of our design under various simulated conditions. Here we focus on the estimands whose estimates appear in the final column of Table 2 in the main text and the results in Figure 3a (and Table C1) as these are most consistent with our original pre-registration.

The full `DeclareDesign` analysis will be included in our replication archive, but it is worth sketching our approach first before showing our results. First, we use the fitted model to approximate σ_y, which is the residual error not explained by the variables included in our regression. In each simulation below, we draw $U_i \sim N(0, \sigma_y)$, which reflects the unmodeled variation in the outcome.

Second, we use the reported coefficients in Table 2 as our initial estimates for the average treatment effects (ATE) for the *Low dose*, *High dose*, and *Low dose + fact-check* conditions. We denote these as $D_i = L$, $D_i = H$, and $D_i = F$ respectively. Using these values, we set up a table of potential outcomes and define our estimands. So, for instance,

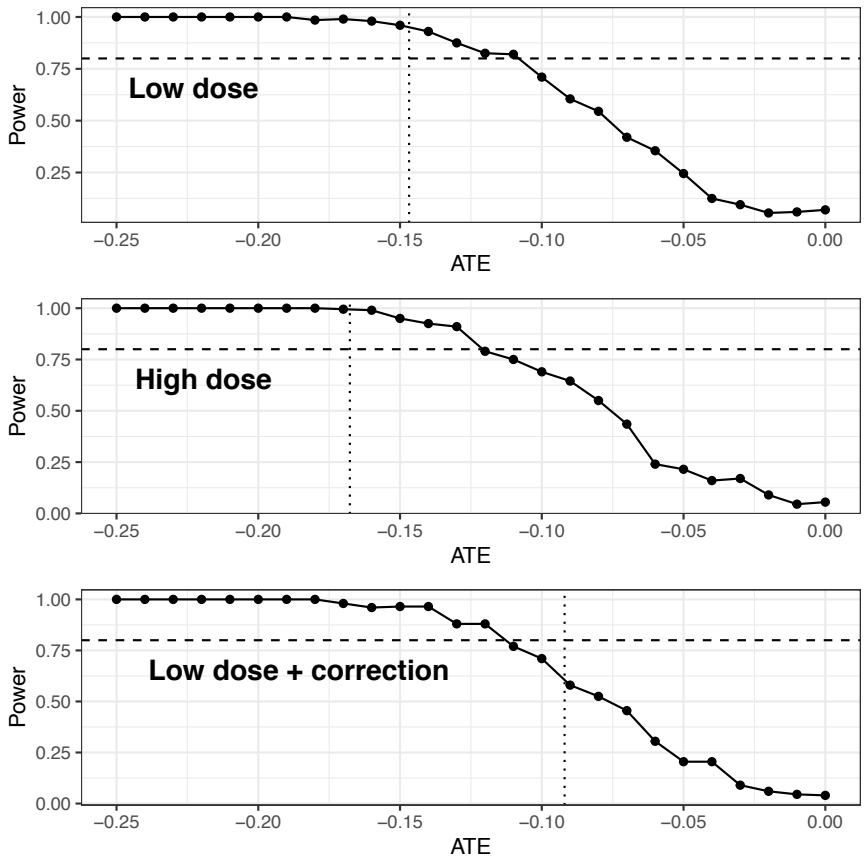
$$\begin{aligned} Y_{i[D_i=C]} &= U_i \\ Y_{i[D_i=L]} &= ATE_L + U_i, \\ Y_{i[D_i=H]} &= ATE_H + U_i, \\ Y_{i[D_i=F]} &= ATE_F + U_i. \end{aligned}$$

Our estimand is then just the difference in potential outcomes. Finally, the randomization is simulated and we can analyze the "revealed" dataset using the same model as in the main text.

The advantage of the `DeclareDesign` framework is that we can repeat this simulation multiple times under different hypothetical settings. Here we are interested in getting a sense of how large the *actual* treatment effects would have to be in order to achieve a power level of 0.8. For each ATE, we simulate 400 datasets incrementing the assumed estimand from 0 to -0.25 by 0.01 (holding all other parameters at their assumed values specified above). This procedure allows us to calculate the power of the complete design and analysis for different potential values of the ATE (the solid black line) and compare it to the value reported in the main text (the vertical dotted line). The results, which are provided in Figure 1, indicate that the design has sufficient power to reliably detect treatment effects of -0.11 or greater (i.e., in the range $[-\infty, -0.11]$). Both the *Low dose* and *High dose* ATEs are well above this threshold, although the *Low dose + correction* coefficient falls just below, indicating that it is somewhat underpowered.

Understanding power for heterogeneous treatment effects is more complicated — the exact power calculations needed depend on the question one is interested in answering. In our case, we focus on the interaction between party and treatments reported in Table C1. Here, we specif-

Figure 1: Implied power for assumed values of treatment effects

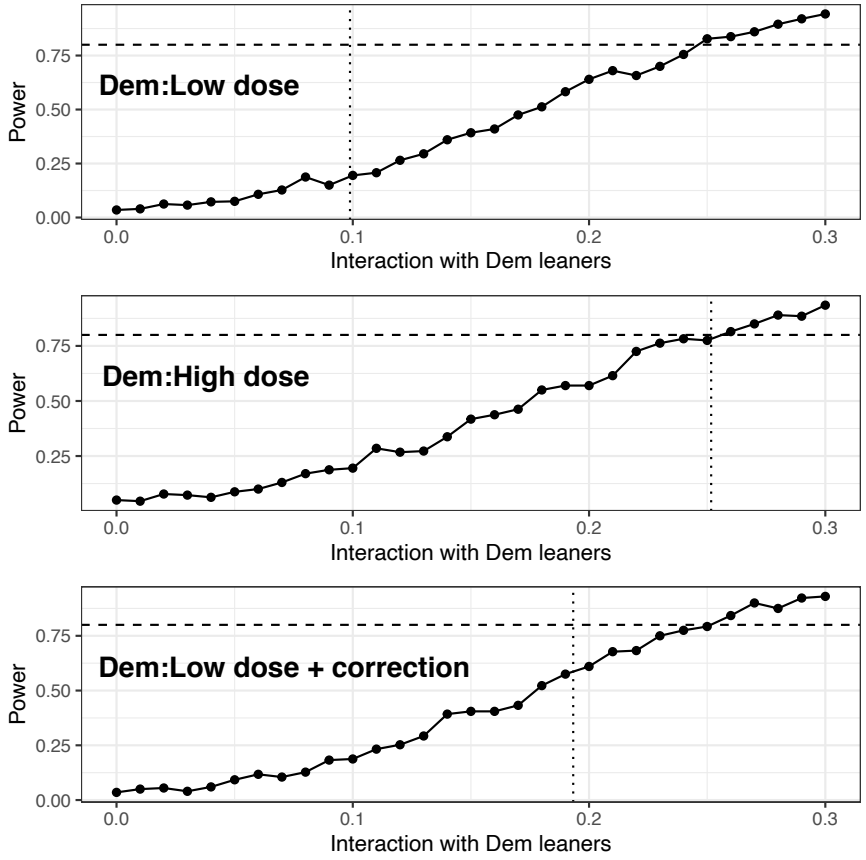


The solid black lines show the simulated power for the linear model reported in final column of Table 2 in the main text for different assumed values of the ATE. The horizontal line represents the traditional 0.8 power threshold. The vertical dotted lines are the reported ATE in the main text.

ically calculate power for the *Low dose* × *Democrat*, *High dose* × *Democrat*, and *Low dose* + *fact-check* × *Democrat* estimands. This is appropriate because the partisan difference in treatment effects is the focus of our discussion of these interactive models in the main text. We follow the basic strategy above but also must use the observed value of partisanship in our data to simulate potential outcomes.

The results are shown in Figure 2 and are consistent with existing research showing that interactions tend to have lower power. Here, the analyses indicate that interactions reach the traditional 0.8 threshold near magnitudes of 0.25. Thus the *High dose* × *Democrat* interaction appears to be sufficiently powered while the *Low dose* + *correction* × *Democrat* has less power (reflecting in part the lower power of the main effect). We are not sufficiently powered to detect interactions as small as the *Low dose* × *Democrat* coefficient reported in Table C1. This finding is not surprising given that the interaction itself is not statistically significant, but it does mean that we should refrain from simply concluding that the interaction is completely absent.

Figure 2: Implied power for assumed values of interaction terms for Democrats



The solid black lines show the simulated power for the linear model reported in Table C1. The horizontal line represents the traditional 0.8 power threshold. The vertical dotted lines are the reported interactions of the treatments with the indicator for being a Democrat.

References

Allcott, Hunt, Matthew Gentzkow and Chuan Yu. 2018. "Trends in the diffusion of misinformation on social media." Downloaded December 18, 2018 from `https://arxiv.org/abs/1809.05901`.

Bakshy, Eytan, Solomon Messing and Lada A. Adamic. 2015. "Exposure to ideologically diverse news and opinion on Facebook." *Science* 348(6239):1130–1132.

Benjamini, Yoav and Yosef Hochberg. 1995. "Controlling the false discovery rate: a practical and powerful approach to multiple testing." *Journal of the Royal Statistical Society: Series B* 57(1):289–300.

Blair, Graeme, Jasper Cooper, Alexander Coppock and Macartan Humphreys. 2019. "Declaring and diagnosing research designs." *American Political Science Review* 113(3):838–859.

Duflo, Esther, Abhijit Banerjee, Amy Finkelstein, Lawrence F Katz, Benjamin A Olken and Anja Sautmann. 2020. "In praise of moderation: Suggestions for the scope and use of pre-analysis plans for RCTs in economics." National Bureau of Economic Research working paper, April 2020.

Franco, Annie, Neil Malhotra, Gabor Simonovits and L.J. Zigerell. 2017. "Developing standards for post-hoc weighting in population-based survey experiments." *Journal of Experimental Political Science* 4(2):161–172.

Gallup. 2020*a*. "Party Affiliation." Downloaded November 9, 2020 from `https://news.gallup.com/poll/15370/party-affiliation.aspx`.

Gallup. 2020*b*. "Presidential Approval Ratings – Donald Trump." Downloaded November 9, 2020 from `https://news.gallup.com/poll/203198/presidential-approval-ratings-donald-trump.aspx`.

Howden, Lindsay M. and Julie A. Meyer. 2011. "Age and Sex Composition: 2010 Census Briefs." United States Census Bureau Report C2010BR-03.

Miratrix, Luke W., Jasjeet S. Sekhon, Alexander G. Theodoridis and Luis F. Campos. 2018. "Worth weighting? How to think about and use weights in survey experiments." *Political Analysis* 26(3):275–291.

United States Census Bureau. 2020. "Educational Attainment in the United States: 2018." Downloaded November 9, 2020 from `https://www.census.gov/data/tables/2018/demo/education-attainment/cps-detailed-tables.html`.