

The Effect of Fact-checking on Elites: A field experiment on U.S. state legislators

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Abstract

Does external monitoring improve democratic performance? Fact-checking has come to play an increasingly important role in political coverage in the United States, but some research suggests it may be ineffective at reducing public misperceptions about controversial issues. However, fact-checking might instead help improve political discourse by increasing the reputational costs or risks of spreading misinformation for political elites. To evaluate this deterrent hypothesis, we conducted a field experiment on a diverse group of state legislators from nine U.S. states in the months before the November 2012 election. In the experiment, a randomly assigned subset of state legislators were sent a series of letters about the risks to their reputation and electoral security if they are caught making questionable statements. The legislators who were sent these letters were substantially less likely to receive a negative fact-checking rating or to have their accuracy questioned publicly, suggesting that fact-checking can reduce inaccuracy when it poses a salient threat.

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To what extent can external monitoring increase political accountability and improve democratic outcomes? Social scientists are increasingly using field experiments to understand the ways in which monitoring can shape political and bureaucratic behavior (e.g., Duflo, Hanna, and Rya 2012; Olken 2007; Ferraz and Finan 2008; Grose N.d.; Butler 2010; Humphreys and Weinstein N.d.; Malesky, Schuler, and Tran 2012). One important source of external monitoring for politicians in free societies is the press. In the United States, several fact-checking organizations have begun to systematically evaluate the accuracy of statements made by politicians at the national and state level using a journalistic approach (Graves and Glaisyer 2012), but little systematic evidence exists on the consequences of this practice.

Among the mass public, the evidence is mixed on whether fact-checking improves citizens' political knowledge. Some studies suggest that it can be effective. For instance, Pingree, Brossard, and McLeod (2014) show that journalistic adjudication of factual disputes in a fictional scenario can successfully affect readers' factual beliefs. Similarly, observational data indicates that visitors to fact-checking websites show higher levels of knowledge controlling for certain observable characteristics (Gottfried et al. 2013). And with enough negative information, even committed motivated reasoners can adjust their evaluations in a counter-attitudinal direction (e.g., Redlawsk, Civettini, and Emmerson 2010). There are also reasons for skepticism, however. Research in political science and psychology suggests that fact-checking may fail to reduce misperceptions, especially among those individuals who are most predisposed to believe in them (for reviews, see Nyhan and Reifler 2012 and Lewandowsky et al. 2012). First, people often seek to avoid unwelcome information about politics, which may reduce exposure to fact-checks that challenge their existing beliefs (Taber and Lodge 2006; Stroud 2008; Iyengar et al. 2008;

Iyengar and Hahn 2009). In addition, corrections may be ineffective or even make misperceptions worse among individuals who do encounter counter-attitudinal corrective information about controversial issues (Nyhan and Reifler 2010; Nyhan, Reifler, and Ubel 2013).¹

It is possible, however, that fact-checking might have positive effects on elite behavior by increasing the reputational costs or risks of spreading misinformation (Nyhan 2010). Though a pollster for a U.S. presidential candidate famously said “We’re not going to let our campaign be dictated by fact checkers” (Smith 2012) and some pundits have written off the practice as ineffective at the presidential level (Carr 2012; Balz 2012), the fact that factually questionable statements continue to be made does not demonstrate that fact-checking is ineffective. The relevant question is whether misleading or inaccurate statements would be more frequent or severe in the absence of fact-checkers. Just as greater accountability could help pundits make more accurate predictions (Tetlock 2005), scrutiny from fact-checkers could increase politicians’ concerns about accuracy and encourage them to make fewer misleading or inaccurate statements. Previous research indicates that elected officials are very concerned about threats to their re-election (Mayhew 1974; Fenno 2002). While most fact-checking likely has little effect on a politician’s re-election prospects, visible or salient monitoring by fact-checkers should increase the perceived risk of a damaging disclosure, particularly for Congressional or state candidates who attract less media coverage and advertise less extensively than presidential candidates.

To evaluate this deterrent hypothesis, we conducted the first field experiment to

¹Previous studies of the effects of televised “ad watches” on viewers found similarly mixed results (e.g., Pfau and Loudon 1994; Cappella and Jamieson 1994; Ansolabehere and Iyengar 1996; Jamieson and Cappella 1997).

evaluate the effects of fact-checking on elected officials. The experiment, which was conducted on a diverse group of state legislators from nine U.S. states in the months before the November 2012 election, randomized whether politicians were sent a series of letters designed to ensure that the reputational and electoral risks posed by fact-checking were visible and salient. Because we conducted the study in states where PolitiFact state affiliates were operating, the threat that dubious claims could be exposed should have been credible. Our results indicate that state legislators who were sent letters about the threat posed by fact-checkers were less likely to have their claims questioned as misleading or inaccurate during the fall campaign—a promising sign for journalistic monitoring in democratic societies.

The effect of fact-checking on politicians

While major news organizations in the United States have long sought to ensure the accuracy of the facts that they report (e.g., quoting public figures correctly), they frequently refrain from questioning the accuracy of contested claims made by public figures even when the statements are verifiable. The origins of this practice are contested. The practice has been attributed to the journalistic norm of objectivity, pressures to avoid bias accusations, and the political media's focus on horse race coverage (e.g., Jamieson and Waldman 2002; Cunningham 2003). The infrequency of fact-checking in the contemporary era may also be linked to financial pressures (including cutbacks in media staffing and resources) and demands for more rapid production of content (e.g., Bantz, McCorkle, and Baade 1980; Fallows 1997; Jamieson and Waldman 2002; Plasser 2005). Politicians appear to exploit the media's reluctance to adjudicate competing factual claims, which enables them

to publicize questionable claims with little risk of being contradicted (e.g., Fritz, Keefer, and Nyhan 2004).

The fact-checking movement takes a very different approach in focusing exclusively on evaluating the accuracy of claims made by politicians and political elites. Fact-checking by the three elite fact-checkers (PolitiFact and its state affiliates, Factcheck.org, and the *Washington Post* Fact Checker) and other media organizations has come to play an increasingly important role in political coverage in the United States (Graves and Glaisyer 2012; Amazeen 2013) and is now beginning to expand abroad (Adair 2013; Alcorn 2013). This movement represents a potentially radical change in how journalism is practiced with significant consequences for political accountability and democratic discourse. Rather than limiting itself to the “he said,” “she said” coverage and horse race analysis that dominate traditional political news, the fact-checkers devote their energies and resources to scrutinizing what politicians say and rendering public judgments about the correctness of their claims.

What effects does fact-checking have on politicians? This question has not been examined systematically, but previous research suggests that legislators may be sensitive to media scrutiny of the sort that fact-checkers provide. The literature on legislative behavior shows that elected officials are concerned about re-election and engage in risk-averse behavior to minimize potential electoral or reputational threats (Mayhew 1974; Fenno 2002). State legislators have been described as wary of career risks in domains ranging from running for higher office (Berkman and Eisenstein 1999) to redistricting (Schaffner, Wagner, and Winburn 2004).

One potential threat that elected officials may be especially concerned about is critical media coverage. While state legislators receive relatively little coverage

(e.g., Lynch 2000; Kaplan, Goldstein, and Hale 2003), many members of Congress receive quite modest levels of coverage as well. Nonetheless, the variation in media scrutiny that has been observed (e.g., Arnold 2004; Schaffner 2006; Fogarty 2008), including at the state level (Carpini, Keeter, and Kenamer 1994; Campante and Do 2014), seems to be consequential. It has been shown that coverage of legislators at both the state and federal level can have significant consequences for citizen political knowledge (Carpini, Keeter, and Kenamer 1994), the incumbency advantage in elections (Prior 2006; Schulhofer-Wohl and Garrido 2013; Gentzkow, Shapiro, and Sinkinson 2011), legislator behavior in office (Snyder and Strömberg 2010; Hogan N.d.), and state-level corruption (Campante and Do 2014). We argue that the threat of fact-checking has the potential to create career risks for politicians by generating negative coverage that could damage their reputation and credibility and thereby harm their prospects for re-election, entering party leadership, or seeking higher office.² Anecdotal evidence suggests that some candidates and political operatives seek to avoid the negative ratings given out by fact-checkers or alter claims that come under fire, though others disavow such concerns (Graves 2013; Gottfried et al. 2013).³

Experimental design

Observational analyses of the effects of fact-checking on politician behavior could easily lead to incorrect conclusions. For instance, fact-checks may be more widely

²Fact-checks may be exploited by opposing candidates — see, e.g., Kessler (2014).

³As an example of how fact-checking can change incentives, consider this evocative quote from GOP media consultant Rick Wilson (Khan 2014): “There is less and less latitude for B.S. in ads these days, and there’s more and more ‘How do you deliver a message that will move the numbers when you don’t want to be in the weeds of Politifact crapping all over you for three days?’”

used in states that already had stronger accuracy norms in public discourse, a confound which might falsely suggest that fact-checking reduced inaccuracy in those states. Alternatively, fact-checking may spread in areas where accuracy norms are particularly weak, which could falsely make fact-checks seem ineffective if legislators in those areas frequently make questionable statements despite receiving negative ratings. In either case, we cannot determine which legislators are *not* making false statements because of the presence of fact-checkers using observational data alone.

To overcome these inferential difficulties, we conducted a field experiment in fall 2012 in nine U.S. states in which PolitiFact affiliates were operating (Florida, New Jersey, Ohio, Oregon, Rhode Island, Tennessee, Texas, Virginia, and Wisconsin), excluding only the two states where the authors were based at the time of the study (Georgia and New Hampshire) due to concerns about treatment effect heterogeneity. (See Supporting Materials [SM] for the distribution of legislators across states.) Because we could not randomize the activities of fact-checking organizations, we instead employed randomized correspondence, which has frequently been used in previous field experimental studies of elite political behavior (Bergan 2009; Broockman 2013; Butler and Broockman 2011; Butler and Nickerson 2011; Butler, Karpowitz, and Pope 2012; Loewen and MacKenzie N.d.; Loewen and Rubenson 2011; McClendon N.d.). In this case, we randomized whether legislators were sent a series of letters about the reputational or electoral consequences of receiving a negative rating from a fact-checking organization.

We chose to conduct our study with state legislators for several reasons. First, we could assemble a very large sample — far larger than would be possible in the U.S. Congress. Second, theory suggests that state legislators should be more

sensitive to an individual fact-check than a member of Congress because they are covered by the media less frequently, which means that a single bad story or negative rating will be a larger proportion of their total coverage. State legislators also have more limited financial resources and thus cannot rely on televised advertising or direct mail to the same extent as members of Congress. By targeting a lower-level politician in this way, we increase the effect of our treatment. Finally, it was more feasible to reach state legislators through correspondence than members of Congress, who typically have much larger offices and more professional staff and interns. In a Congressional office, for instance, it would be rare for a legislator to directly open and read incoming mail, which is primarily handled by staff whose primary responsibility is constituent correspondence. Our mailings would therefore be less likely to have a direct effect on Congressional behavior. However, state legislators often have few or no professional staff, substantially increasing the likelihood that they would encounter and read our mailings.

While the scope of our study is constrained by the availability of PolitiFact affiliates,⁴ we believe our results have substantial external validity. The states in our sample offer substantial political, institutional, and regional diversity. Though they are not necessarily representative of the country as a whole, the nine states in question include presidential battleground states as well as solidly Republican and Democratic states, vary significantly in their levels of legislative professionalism (Squire 2007), and include one or more states from each of the four Census regions. We therefore have no a priori theoretical reason to expect that our results would vary if PolitiFact affiliates were operating in a different set of states.

⁴The process is seemingly driven by affiliate demand. State affiliates must reach a licensing agreement with PolitiFact, go through a training process in the site's fact-checking method, and dedicate resources to produce fact-checking content (Spivak 2011).

Experimental conditions

Our experiment randomized whether state legislators were sent a series of letters about the reputational or electoral consequences of fact-checking. 1169 legislators from the nine states in our sample⁵ were randomly assigned to one of three conditions: a treatment condition in which legislators were sent letters reminding them of the risks to their reputation and electoral security if they are caught making questionable statements, a placebo (Hawthorne) condition in which legislators were sent letters stating that we were monitoring campaign accuracy, and a control condition. While legislators of course vary in the likelihood that they will make misleading or inaccurate statements or be the target of fact-checking, this randomization procedure (which is described further below) ensures that these individual-level differences are independent of treatment assignment, which allows us to obtain an unbiased estimate of the treatment effect.

Specifically, legislators in the treatment and placebo (Hawthorne) conditions were sent three separate mailings (mail dates: August 23, September 18, and October 12, 2012), while those in the control condition were not contacted. Because the state legislatures in question were out of session during our study, we sent a copy of each mailing to legislators' capitol and district addresses (see SM for details). By conducting the study when all of the states were out of legislative session, we minimized the possibility of treatment spillovers. When legislators are not interacting closely on a daily basis, there are fewer opportunities for information about the treatments to spread from treated to untreated legislators. Second, we avoid possible treatment effect heterogeneity from treating legislators when some are in

⁵See SM for the distribution of legislators across states.

session but not others.⁶ Finally, we conducted the study during election season — a time when legislators may be particularly sensitive to threats to their reputation.

Treatment condition

In the treatment condition, legislators were sent a series of letters that emphasized the risks of having misleading or inaccurate statements exposed by fact-checkers. The treatment mailing to legislators had several key elements: (1) a reminder of the presence of a PolitiFact affiliate in their state to establish the credibility of the threat of being fact-checked; (2) a description of the potential electoral and reputational consequences of negative fact-check ratings; and (3) two sample PolitiFact “pants on fire” fact-checks (balanced by party) to heighten legislators’ concerns about being fact-checked from one of the states excluded from our study. (See SM for the full text of a sample letter.)

Placebo (Hawthorne) condition

In addition to our treatment letter, we designed a placebo letter that alerted legislators that we were conducting a study of the accuracy of statements made by politicians, but excluded any language about fact-checking or the consequences of inaccurate statements. We included this additional condition to account for what is known as a Hawthorne effect—the tendency for experimental participants to behave differently when they know they are being studied, which can confound treatment effect estimates (Levitt and List 2011). By including a placebo condition, we can determine whether legislators responded to the specific content of the treatment let-

⁶We could have treated legislators only when they were in session, but doing so would likely have required treating different states at different points in time, introducing time as a confound.

ter or the fact that they were being studied. (See SM for full text of a sample letter.)

Control condition

Legislators in the control condition were not sent mailings or contacted in any way.

Randomization and balance

Using the R package `blockTools` (Moore N.d.), we block randomized assignment to ensure balance between conditions on state, political party, legislative chamber (state house/state senate), and whether or not a legislator had previously received a PolitiFact rating. We also used multivariate continuous blocking to maximize balance between conditions on two continuous covariates that could be related to being fact-checked: previous vote share and fundraising (Moore 2012). In this way, we sought to minimize variance in factors other than our experimental conditions that would influence whether or not legislators would be fact-checked during the study period, which increases the precision of experimental treatment effect estimates (Duflo, Glennerster, and Kremer 2007).

This block randomization resulted in near-perfect balance between conditions among the 1169 legislators included in the study, as Table 1 and Figure 1 indicate.⁷ Importantly, this blocking also results in balance on observables that were not included in the blocking like being a party leader or a committee chair.⁸

⁷Of the 1197 legislators in our sample, 1169 were randomized within 69 blocks; 28 were dropped to maximize balance. Of the 69 blocks formed, 50 had equal numbers of legislators in each condition and 19 had one fewer legislator in a single condition. In this latter group, the probability of assignment to treatment varies slightly by block, which we account for in our analysis below by weighting by the inverse probability of treatment (Gerber and Green 2012, 117).

⁸Texas state representative J.M. Lozano was mistakenly coded as a Democrat; he actually switched to the Republican Party in spring 2012. The randomization procedure was carried out with him coded as a Democrat. After his affiliation was corrected, however, the sample remains balanced by party (56% in both the treatment and placebo/control conditions).

Table 1: Covariate balance by experimental condition

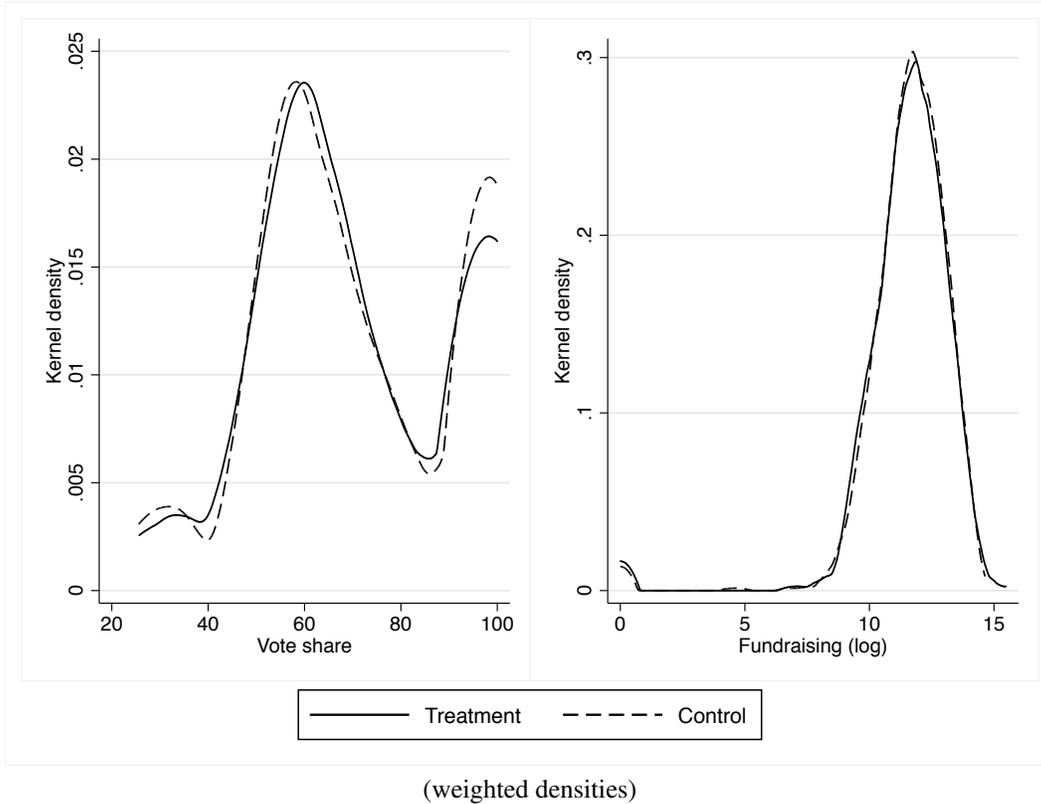
	Treatment	Placebo/Control
GOP	0.56	0.56
State senate	0.26	0.26
Previous fact-check	0.21	0.21
Log fundraising	11.6	11.5
Previous voteshare	71.6	71.3
Party leader	0.07	0.07
Committee leader	0.53	0.50

(weighted means)

Estimand: Assignment to treatment condition

Because it is impossible to know with certainty which legislators received the treatment, our experiment estimates the effect of being *assigned* to the treatment condition. As a partial indicator of which legislators read our mailings (and to further indicate the importance of the letter), we asked recipients of treatment and placebo letters to sign and return an enclosed postage-paid acknowledgment postcard. However, those postcards cannot be used to estimate the actual effect of reading the treatment letter. First, many legislators may have read the letter but not bothered to return the postcard. In addition, the postcards themselves provide suggestive evidence that the content of the treatment letter had a significant effect—only 21% of legislators in the treatment group returned a signed postcard compared to 34% of those in the placebo condition, suggesting that it may have displeased its recipients ($p < .01$; see SM). Alternatively, we could use successful delivery of the letter as an indicator of treatment receipt, but only 0.4% ($n = 18$) of the letters we sent were returned as undeliverable, so the treatment effect estimates would be virtually identical. We therefore estimate the expected difference in outcomes resulting from

Figure 1: Covariate balance: Vote share and fundraising (log)



assignment to the treatment condition (rather than receipt of treatment).⁹

In addition, we assume that outcomes are unaffected by the experimental conditions to which other legislators are assigned. A violation of this assumption would occur, for example, if a legislator who received our treatment letter showed the letter to a legislator in the control group and thereby affected the likelihood that she would make misleading or inaccurate statements. We believe this assumption is justified for two reasons. First, all of the state legislatures in our study were out of session during the study period, which should have dispersed legislators across

⁹However, we summarize what our estimates would be for the average treatment effect on the treated under different assumptions about the (unmeasurable) probability that legislators actually received the treatment in the results section below.

their districts, substantially reducing the opportunity for treatment spillover. In addition, any spillover would likely bias our treatment effects toward zero, reducing the likelihood that we would find significant effects.¹⁰

Outcome measures

Due to the complexities of language and politics, no perfectly objective measure of statement accuracy has yet been created. It was also infeasible to code every statement during the study period by all the state legislators in our data, especially since most are unobservable to researchers. As such, we examine three measures of public criticism by fact-checkers or others that question the validity or accuracy of statements made by state legislators in our sample. Such factual criticism should be more likely as the frequency of misleading or inaccurate statements by state legislators in our sample increases.

The study period in which these outcome measures were collected is defined as statements made between August 24–November 6, 2012, which runs from the first

¹⁰Though previous field experimental research has found some evidence of spillovers among people with close personal ties (e.g., Nickerson 2008), they appear to be more rare in settings that are more comparable to geographically dispersed state legislators (Sinclair, McConnell, and Green 2012; Baird et al. N.d., e.g.). The direction and magnitude of spillovers are ultimately theoretical questions (Aronow and Samii N.d.). In our study, we believe it is most plausible that spillovers would reduce our estimated treatment effect by making control/placebo legislators who were not sent the treatment letter and learned of its contents through a colleague more concerned about fact-checking and thereby more careful in their public statements than they otherwise would have been. It is also possible (though in our view less likely) that spillover could bias treatment effects towards zero by making legislators who received the treatment letter less concerned about the reputational cost of fact-checking if they learn that other legislators are not receiving the same letter. However, as an anonymous reviewer pointed out, spillover may not bias the effect estimate towards zero. Suppose that a legislator who received the treatment learned that another legislator who did not receive the treatment. It is possible — though we believe much less likely — that the legislator who received the treatment would become more concerned about reputational costs after learning others did not receive it *and* that the legislator who did not receive the treatment letter would become less concerned upon learning that she was not assigned to the treatment group. In this hypothetical scenario, spillovers would bias the treatment effect away from zero rather than towards zero.

day on which legislators could have received a letter (the day after the first mailing) to Election Day. The period before the election is the time in which the treatment should have been most salient to legislators due to the ongoing campaign, which should increase concern over potential electoral or reputational threats. It is also the period in which the effect of the treatment is most likely to be measurable due to the treatment group having recently received mailings (mail dates: August 23, September 18, and October 12) and fact-checks being produced more frequently.

The first dependent variable is whether a legislator received a negative rating from the PolitiFact affiliate in their state.¹¹ PolitiFact uses a six-point scale to rate the accuracy of statements, ranging from “True” to “Pants on Fire.” Because our unit of analysis is the state legislator, we created a binary measure (*Negative PolitiFact rating*) of whether a state legislator was rated by PolitiFact as having made a misleading or inaccurate statement. This measure, which is based on PolitiFact’s description of the meaning of their rating categories, takes a value of 1 if the state legislator received a rating of “half true” (“partially accurate but leaves out important details or takes things out of context”), “mostly false” (“contains some element of truth but ignores critical facts that would give a different impression”), “false” (“not accurate”), or “pants on fire” (“not accurate and makes a ridiculous claim”) during the study period and 0 otherwise, which could include PolitiFact ratings of “true” or “mostly true” or, most commonly, if the legislator had no statements publicly evaluated by PolitiFact (PolitiFact.com N.d.).

One potential concern is whether PolitiFact truth ratings are consistent and accurate. The franchise training model used by PolitiFact for its state affiliates suggest

¹¹As far as we know, the PolitiFact affiliates were not aware of our experiment or the treatment conditions to which legislators were assigned except in the case of one legislator in one state discussed in the SM.

that these ratings should be comparable across states (Myers N.d.; Nyhan 2013a). While individual fact-checks sometimes veer into punditry or semantic disputes (Marx 2012; Nyhan 2012, 2013b), an academic analysis of the ratings by elite fact-checking organizations finds a very high level of agreement when they evaluate identical or similar claims (Amazeen 2012, 66–68).

For our second dependent variable, a research assistant who was blind to the experimental randomization performed a search of LexisNexis Academic for media coverage in which the accuracy of specific claims made by a legislator were questioned. This *Accuracy questioned* measure is coded 1 if the research assistant found one or more articles or blog posts published during the study period in which specific factual claims made by the legislator were questioned by the author or other sources (including citations of past PolitiFact ratings) and 0 otherwise (intercoder reliability: 95% agreement, Krippendorff’s $\alpha=.876$; see SM for further details on the search protocol, coding procedure, and types of articles found).

The third dependent variable is a binary measure that combines the first two dependent variables. It is coded as 1 if the accuracy of a statement by the legislator was questioned by PolitiFact or in an article in LexisNexis and 0 otherwise.

Results

Even with a dataset of nearly 1200 state legislators across nine states, fact-checks were relatively rare — only 26 state legislators in our data received ratings from PolitiFact state affiliates during the study period. Among these legislators, 18 received a rating of “half true” or worse. Even with such small numbers, however, an inspection of the marginal distributions suggest that assignment to treatment had a

substantial effect, reducing the prevalence of negative ratings from 14 in the placebo and control conditions (1.8%) to 4 in the treatment condition (1.0%). Likewise, the number of legislators who had the accuracy of a claim questioned in media indexed in LexisNexis decreased from 8 in the placebo and control conditions (1.0%) to 1 in the treatment group (0.3%). There was no overlap in accuracy criticism between the measures. In all, 22 legislators in the placebo and control conditions had the accuracy of their claims questioned by PolitiFact or in Nexis (2.8%) compared with 5 in the treatment condition (1.3%; one-sided Fisher's exact test $p < .07$).

To more formally evaluate our hypothesis, we estimated a series of weighted least squares regression models.¹² We found no significant differences between the placebo and control conditions (see SM), suggesting that the differences in behavior we observe in the treatment condition are due to the fact-checking content in those mailings rather than a Hawthorne effect. To simplify exposition, we thus combine legislators in these conditions in the analyses below and estimate treatment effects relative to the control and placebo conditions. (All analyses are robust to estimating treatment effects relative to the placebo condition directly; see SM for details.)

In each model, we regressed our dependent variable on the treatment indicator using weighted least squares. Table 2 presents weighted means for the treatment and control groups as well as the results of these regression models, which estimate the average effect of being assigned to the treatment condition (the average treatment effect [ATE]) on our three dependent variables.

¹²We use weighted least squares because the block randomization procedure results in slightly differing probabilities of treatment across blocks as noted above. Weighted least squares is necessary to obtain an unbiased estimate of the average effect of assignment to treatment for our experiment (Gerber and Green 2012, 117). Specifically, we follow Gerber and Green (2012) and weight treated observations by the inverse probability of treatment within each block while weighting placebo/control observations by $1/(1-\text{probability of treatment within each block})$. These weights were used in the weighted least squares regression estimates presented in the article text and supplementary materials.

Table 2: Treatment effects of fact-checking threat letter

Outcome	Weighted means		ATE	<i>p</i> -val.	Risk reduction	
	Treatment	Control			Absolute effect	% max possible
Negative PF rating	0.010	0.018	-0.008 (0.007)	.125	-0.8%	-44%
Acc. questioned	0.003	0.010	-0.008 (0.004)	.042	-0.8%	-75%
Combined measure	0.013	0.028	-0.016 (0.008)	.028	-1.6%	-55%

Study sample consists of 1169 state legislators from nine states. Weighted means provided for treatment group and controls (the combined placebo/control group). Average treatment effect (ATE) estimated using weighted least squares regression with robust standard errors in parentheses; estimated *p*-values are one-sided due to the directional nature of our hypothesis. See text and supplementary materials for further details on treatments and outcome measures.

Our results indicate that legislators who were sent our treatment letters were substantially less likely to receive a negative PolitiFact rating or to have their accuracy questioned publicly in the study period (August 24–November 6, 2012). While the treatment effect falls short of significance for the negative PolitiFact rating, the effect is in the expected direction. For the *Accuracy questioned* variable, the treatment is statistically significant ($p < .05$ one-sided). Finally, when we combine the two outcome measures into a broader indicator of whether the accuracy of the legislator’s claims are questioned, the treatment effect is statistically significant ($p < .05$). These results are consistent in a series of robustness checks presented in the SM (restricting the *Negative PolitiFact rating* measure to only take a value of 1 for “mostly false”, “false”, and “pants on fire” ratings; estimating treatment effects relative to the placebo condition; using logistic regression instead of weighted least squares; including block fixed effects; and using standard errors that are clustered by block).¹³

¹³Not surprisingly, the effect of being sent the treatment letters on negative PolitiFact ratings in the post-election period (when the threat of fact-checking is less salient and the treatment effect has decayed) is not significant (results available upon request).

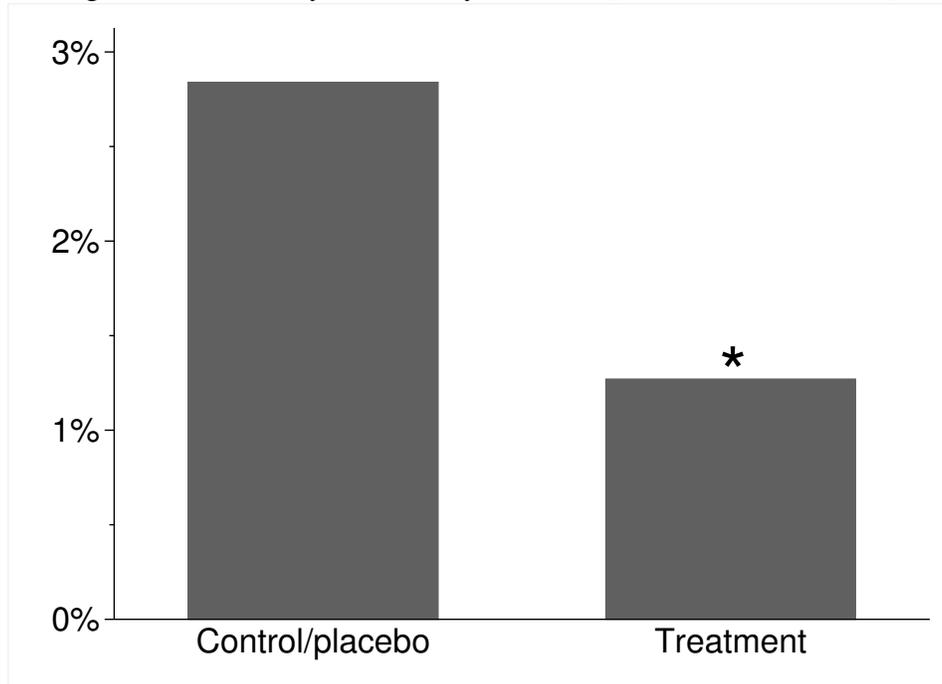
In addition to the estimated treatment effects and standard errors, Table 2 also provides two estimates of the changes in predicted probability. Because we used a least squares estimator on a binary outcome measure, the treatment effect can be directly interpreted as a difference in means. The coefficients thus tell us how much the treatment reduced the probability of a negative outcome relative to the combined placebo and control conditions. The estimated absolute risk reductions are relatively low (0.8–1.6%) due to the infrequency with which state legislators were rated by PolitiFact or had their statements questioned in media or online outlets — a base rate that almost certainly understates the proportion of politicians who make misleading or inaccurate claims.

While the absolute decline in accuracy criticism may seem small, one way to assess the substantive magnitude of the treatment effect is to compare the observed effect with the maximum effect that we could observe relative to the placebo/control conditions. In this case, if our treatment were perfectly effective, it would reduce accuracy criticism from its current (untreated) levels down to zero. The last column of Table 2 shows our realized effect size as a proportion of this theoretical maximum. Judged by this standard, our treatments reduce accuracy criticism by 44–75% of the amount possible given their observed incidence among untreated legislators.¹⁴ Figure 2 highlights these substantively significant effects by contrasting the weighted means for our composite measure of reported inaccuracy between the treatment and placebo/control groups.

To rule out the possibility that these effects were the result of the treatment suppressing public statements by legislators more generally, we estimated weighted

¹⁴Of course, the relative effect of the treatment on accuracy criticism is likely to differ by context and measurement strategy.

Figure 2: Probability of accuracy criticism (PolitiFact/LexisNexis)



Legislators assigned to treatment condition are less likely to receive accuracy criticism. Study sample consists of 1169 state legislators from nine states. Weighted probabilities provided for treatment and placebo/control group. See text and supplementary materials for further details on treatments and outcome measures.

least squares regression models of the probability of receiving *any* rating from PolitiFact, the total number of articles found for each legislator in Nexis excluding the accuracy-related keywords used in *Accuracy questioned*, and the number of pages on each legislator's website when scraped approximately one week after the election (a proxy for the total volume of content provided). As Table 3 indicates, none of these results were statistically significant, suggesting that the treatment did not

suppress speech, but changed it.¹⁵

Table 3: Treatment effects for indicators of volume of speech

Model	Received PF rating	Total Nexis articles	Number webpages
Treatment	-0.003 (0.009)	1.806 (1.221)	0.075 (3.131)
Constant	0.023 (0.005)	9.317 (0.605)	21.354 (2.099)
R ²	0.00	0.00	0.00
N	1169	1169	789

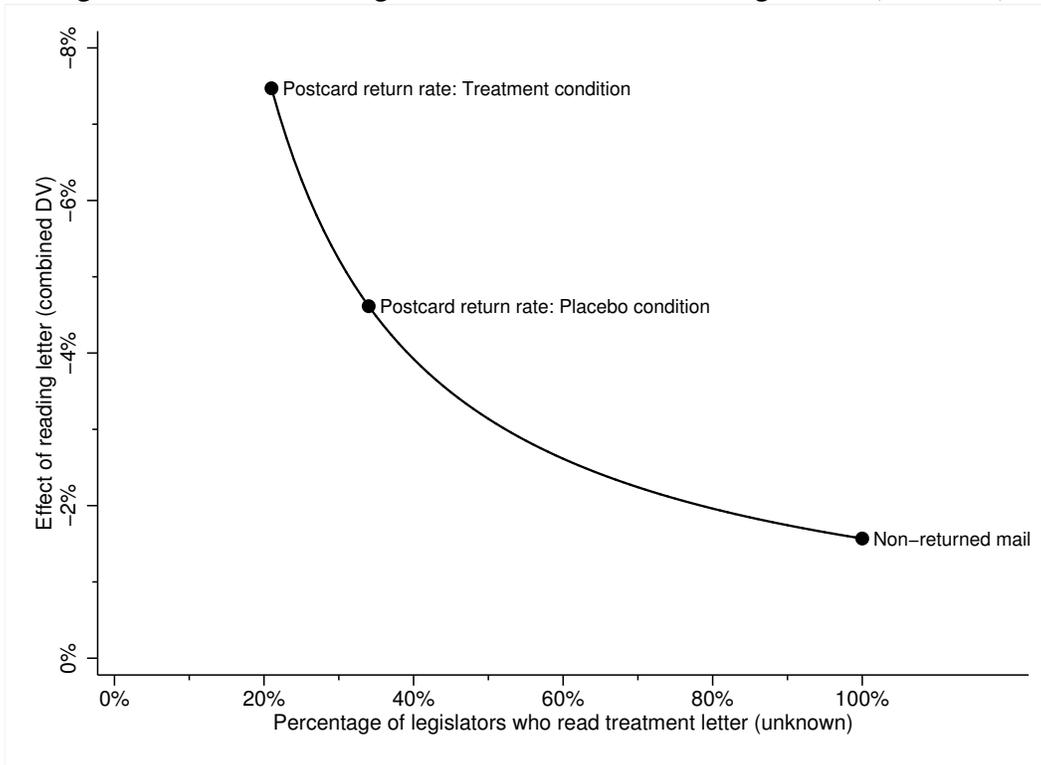
Weighted least squares regression with robust standard errors in parentheses. Number of webpages calculated as the number of files ending in .html, .shtml, .php, .htm, .asp, or .php that were successfully scraped by an automated program on November 13, 2012 from the candidate’s website when one could be located. Facebook or Twitter accounts were excluded.

Finally, it is important to reiterate that our experiment estimates the effect of *assigning* a legislator to the treatment condition. An important quantity of interest is the effect of actually *receiving* the treatment – that is reading the letter. We cannot observe who has read the letter. However, we can compute how effect size changes as the proportion successfully treated (i.e. read the letter) also changes. As Figure 3 illustrates, the estimated effect of *receiving* the treatment increases as the assumed level of non-compliance increases. In other words, as fewer legislators actually read the letter, the effect is larger for those who did read it. Figure 3 provides three possible measures for the proportion successfully treated: (1) the rate at which recipients of the treatment condition signed and returned an enclosed postage-paid acknowledgment postcard, (2) the rate at which recipients of the placebo condition signed and returned an enclosed postage-paid acknowledgment postcard, and (3)

¹⁵It is possible that legislators responded to the treatment by making fewer specific factual claims in their public statements rather than speaking less frequently (we are grateful to a reviewer for this suggestion). Testing this possibility, which would require directly coding legislators’ public statements rather than assessing the frequency with which their accuracy was questioned, is a possible topic for future research.

the proportion of letters that were *not* returned as undeliverable.

Figure 3: Effect of reading treatment letter on treated legislators (PF/Nexis)



Possible range of absolute effect estimates for the average effect of treatment on the treated. Calculated by dividing the effect size from combined PolitiFact/LexisNexis measure in Table 2 by the hypothetical proportion of legislators in treatment condition who actually read the letter. See text and supplementary materials for further details on treatments and outcome measures.

The magnitude of the average treatment effect on the treated is easily calculated under various assumptions about the ratio of legislators who were successfully treated (the treatment effect for all others is assumed to be zero). For instance, if 50% of legislators in the treatment group did not read the letter, the treatment effect on the combined measure for those who did read the letter is twice as large as the effect of assignment to treatment. Similarly, the effect is four times as large if 75% did not read the letter.

Discussion

Does external monitoring reduce inaccuracy in statements made by political elites? In the first field experiment of its kind, we find that the randomized provision of a series of letters highlighting the electoral and reputational risks of having questionable statements exposed by fact-checkers significantly reduced the likelihood that legislators in nine U.S. states would receive a negative fact-checking rating or have the accuracy of their claims questioned publicly. We found no evidence that these results were driven by legislators speaking less frequently or receiving less coverage, suggesting instead that they were less likely to make inaccurate statements rather than being silenced more generally.

Moreover, these results, while encouraging, may understate the magnitude of the potential effects of fact-checking on the behavior of politicians or other elites. Our experiment estimates the effect of being *assigned* to receive the treatment letter. It is unlikely that every state legislator to whom we sent the treatment letter received it and read it carefully. If the negative consequences of inaccurate statements were salient and accessible to all elites, the potential effects on their behavior would likely be even larger. In addition, the magnitude of the treatment effects are scaled relative to the low base rate of fact-checking or articles questioning a legislator's accuracy, which is likely to capture only a tiny fraction of the deceptive or inaccurate statements that politicians make. If the frequency of inaccurate statements is much higher in practice, our estimates suggest that the potential effect of fact-checking threat is sizable.

The scope of these results should be noted, however. First, because our study is limited to states with PolitiFact affiliates, the treatment effect we estimate is the

result of a salient accountability threat in states in which an affiliate is already operating, not the direct effect of the creation of a PolitiFact affiliate itself (a different question and one that would require a different research design). Second, the study provides a direct reminder to legislators that is typically not delivered by fact-checkers. However, to the extent that the treatment highlights a genuine reputational threat, our estimates should capture the effect that salient fact-checking can have outside of an experimental context.¹⁶ Finally, as in any experimental study, our estimate is a partial equilibrium result (Acemoglu 2010); it is possible that politicians would be less sensitive to fact-checking if it were more common. Our theory suggests, however, that they should become more cautious in their public statements when fact-checking is more widespread. If legislators make fewer inaccurate or questionable statements as a result of this scrutiny, the general equilibrium result would therefore likely be a pattern of behavior that is consistent with our findings (even if accountability threat reminders might have less of an effect on the margin in those cases).

Future research should further investigate the mechanisms by which fact-checking changes elite behavior and the extent to which they are captured in our experimental design. For instance, fact-checkers may alter elite behavior by increasing the perceived risks of making misleading claims and/or priming normative concerns about truthfulness. Our experimental design does not allow us to evaluate potential mediators, though our treatment letter could plausibly have both effects. Another possibility is that the effects of fact-checking may vary due to state-level or contex-

¹⁶If the treatment did not capture a real-world threat, it likely would not have had a significant effect. Moreover, as fact-checking becomes a prominent part of the political and media landscape, the threat that it poses to legislators will likely become more salient, especially at the state legislative and Congressional levels. We are thus confident that the results should generalize to cases in which direct reminders are not provided.

tual factors such as whether the legislature is in session. With only nine states in our sample (all of which were out of session during our study), we cannot answer these questions, but they are worth considering as fact-checking continues to expand. For example, future research could examine whether there are certain time periods where politicians are especially sensitive to fact-checking such as campaigns.

More generally, these results indicate that fact-checking should not be discredited by the continued prevalence of misinformation and misperceptions. While fact-checking may be ineffective at changing public opinion, its role as a monitor of elite behavior may justify the continued investment of philanthropic and journalistic resources. Indeed, given the very small numbers of legislators whose accuracy is currently being questioned by fact-checkers or other sources, one could argue that fact-checking should be expanded in the U.S. so that it can provide more extensive and consistent monitoring to politicians at all levels of government.

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Supplementary materials

The Effect of Fact-checking on Elites:

A field experiment on U.S. state legislators

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July 30, 2014

States with PolitiFact affiliates

Table S1: Distribution of legislator sample by state

State	Legislators in study
Florida	157
New Jersey	116
Ohio	128
Oregon	88
Rhode Island	110
Tennessee	128
Texas	178
Virginia	136
Wisconsin	128

At the time of the study, there were eleven states with PolitiFact affiliates. We excluded the states where the authors were based at the time of the study (Georgia and New Hampshire) due to concerns about treatment effect heterogeneity. Table S1 shows the distribution of legislators across the remaining nine states in the study.

Treatment letter example

The Honorable Rodney Ellis
P.O. Box 12068
Capitol Station
Austin, TX 78711

August 20, 2012

Dear Senator Ellis:

We are writing to let you know about an important research project.

As you may know, the national fact-checking organization PolitiFact has created an affiliate in Texas. Our research project examines how elected officials in your state are responding to the presence of this fact-checking organization during this campaign season. PolitiFact examines statements made by politicians and then rates their accuracy and truthfulness on a scale that ranges from “true” to “pants on fire.” (Note: We are independent researchers who are not affiliated with PolitiFact in any way.)

In particular, we are writing to notify you that we are studying how elected officials react to the presence of a PolitiFact affiliate in their state. We have enclosed two recent fact-check articles from PolitiFact Georgia as examples of the type of coverage that you might expect to receive if you make a false or unsupported claim.

Politicians who lie put their reputations and careers at risk, but only when those

lies are exposed. That's why we are especially interested in the consequences of PolitiFact verdicts and other fact-checking efforts in your state. Here are examples of the types of questions we are interested in:

- Are “false” or “pants on fire” verdicts damaging to the reputation or political support of political candidates?
- Do election campaigns use “false” or “pants of fire” verdicts in their advertising to attack their opponents?
- Will state legislators lose their seats as a result of fact-checkers revealing that they made a false statement?

Because the legislature is out of session, we are sending this letter to your capitol and district addresses. Over the next two months, we will send you two additional reminder letters about our project so that you will keep thinking about these issues over the course of the campaign. We will seek to contact legislators in your state to discuss these issues further after the 2012 election.

It is essential for the validity of the study to determine whether this letter has reached you successfully. We have therefore enclosed a postage-paid acknowledgment postcard. We would be extremely grateful if you could sign and return the postcard once you have read this letter.

If you have any questions about this study, please contact Susan M. Adams in the Committee for the Protection of Human Subjects at Dartmouth College (cphs.tasks@dartmouth.edu) or Susan Vogtner in the Georgia State University Office of Research Integrity at 404-413-3513.

Sincerely,

Brendan Nyhan
Assistant Professor
Dept. of Government

Jason Reifler
Assistant Professor
Dept. of Political Science

Dartmouth College

Georgia State University

Placebo (Hawthorne) letter example

The Honorable Floyd Prozanski
900 Court St
S-417
Salem, OR 97301

August 20, 2012

Dear Senator Prozanski:

We are writing to let you know about an important research project.

We are studying the accuracy of the political statements made by legislators in Oregon.

Because the legislature is out of session, we are sending this letter to your capitol and district addresses. Over the next two months, we will send you two additional reminder letters about our project. We will seek to contact legislators in your state to discuss these issues further after the 2012 election.

It is essential for the validity of the study to determine whether this letter has reached you successfully. We have therefore enclosed a postage-paid acknowledgment postcard. We would be extremely grateful if you could sign and return the postcard once you have read this letter.

If you have any questions about this study, please contact Susan M. Adams in the Committee for the Protection of Human Subjects at Dartmouth College (cphs.tasks@dartmouth.edu) or Susan Vogtner in the Georgia State University Of-

Office of Research Integrity at 404-413-3513.

Sincerely,

Brendan Nyhan

Assistant Professor

Dept. of Government

Dartmouth College

Jason Reifler

Assistant Professor

Dept. of Political Science

Georgia State University

Mailing procedures

Mailings were sent to each legislator's district and capitol addresses on August 23, September 18, and October 12, 2012 for a total of six mailings each with two exceptions:

- District addresses could not be located for three legislators in the placebo group and four legislators in the treatment group. These legislators were only sent mail at their capitol address.
- Two legislators who complained to an institutional review board were removed from the study after the first wave and did not receive further mailings.

Awareness of treatment assignment (PolitiFact)

The PolitiFact affiliates in the nine states in our study were not told of our experiment or the treatment conditions to which legislators were assigned. On October 18, 2012, we received word that that PolitiFact Texas had been shown one copy of our letter (Adair 2012). This disclosure suggested that a study was in progress, but it took place relatively late in the August 24–November 6, 2012 study period. Moreover, PolitiFact had no information about the treatment assignment of the other legislators in the state — they later confirmed they did not see any other letters, did not discuss the Texas letter with any of their other affiliates, and were not aware of

other treatment conditions (Adair 2014). As a result, including data from Texas in the sample should not bias our treatment effect estimates. Overall, only two fact-checks were published during the period between October 18 and November 6 in Texas. Of these, only one was a negative rating and the legislator who received a negative rating (Jason Isaac) was in the control condition. PolitiFact Texas could not have inferred that he was a control unless a reporter asked Isaac directly if he had received a letter. It seems highly unlikely that PolitiFact conferred with Isaac (one of 178 Texas legislators in our sample) about receiving letters from us prior to issuing a negative rating.

Coding of *Accuracy questioned*

To compile the information necessary for the *Accuracy questioned* variable, a research assistant who was blind to the experimental conditions of the state legislators used the Lexis-Nexis Academic database to search for articles with the the state legislator's name as a keyword. To narrow this large list of articles, they were instructed to include a search restriction that the legislator's name had to fall within 50 words of the following list of words:

bamboozle OR beguile OR hoax OR hoodwink OR hornswoggle OR misguide OR misinform OR mislead OR snooker OR sucker OR cheat OR defraud OR fleece OR hustle OR swindle OR fable OR fabricate OR fabrication OR false OR falsehood OR falsity OR fib OR mendacity OR prevarication OR prevaricate OR untruth OR whopper OR politifact OR "fact check" OR "fact-check" OR distort OR distortion OR exaggerate OR exaggeration OR half-truth OR ambiguity OR ambiguous OR equivocate OR defame OR libel OR slander OR perjure OR perjury OR fiction OR nonsense OR canard OR fallacy OR misconception OR myth OR falsification OR misinformation OR misreport OR misrepresentation OR misstatement OR deceive OR deceit OR deceitful OR dishonest OR duplicity OR fraudulent OR lie OR controversy OR unsupported OR "no evidence" OR "lacks evidence" OR unsubstantiated OR unproven OR unverified OR smear OR calumniate OR defame OR malign OR traduce OR vilify OR belittle OR denigrate OR disparage OR disgrace OR dishonor

OR abase OR debase OR degrade OR humiliate OR scorn OR belittle OR deprecate
 OR depreciate OR derogate OR decry OR trash-talk OR vilipend OR demagogic OR
 demagogue OR questionable OR fraud OR facts

This measure was coded 1 if the keyword was used to directly question the factual/evidentiary basis of specific claim by the legislator in the article and 0 otherwise (95% agreement in a test of intercoder reliability; Kappa = .875). Six of the articles came from newspapers or wire services and three came from blogs. Three were news articles and six were opinion articles, columns, or blog posts.

No differences between placebo and control

As Table S2 indicates, there were no statistically observable differences between the placebo (Hawthorne) and control conditions.

Table S2: Effects of placebo (Hawthorne) letter relative to controls

	Negative PolitiFact rating	Accuracy questioned	Combined
Placebo	0.011 (0.010)	0.005 (0.007)	0.016 (0.012)
Constant (Control)	0.013 (0.006)	0.008 (0.004)	0.020 (0.007)
R ²	0.00	0.00	0.00
N	777	777	777

Weighted least squares regression with robust standard errors in parentheses.

Results: Robustness checks

Results are unchanged if we restrict *Negative PolitiFact rating* to only take a value of 1 for mostly false, false, and pants on fire ratings (Table S3). They are also substantively identical if treatment effects are estimated using the following approaches: relative to the placebo (Hawthorne) condition (Table S4), using logistic regression (Table S5), including block fixed effects (Table S6), or with standard errors by block (Table S7).

Table S3: Treatment effects of fact-checking threat letter

Outcome	Weighted means		ATE	<i>p</i> -value	Risk reduction	
	Treatment	Control			Absolute	% max possible
PolitiFact MF/F/POF	0.008	0.014	-0.007 (0.006)	.142	-0.7%	-46%
Accuracy questioned	0.003	0.010	-0.008 (0.004)	.042	-0.8%	-75%
Combined measure	0.010	0.025	-0.014 (0.008)	.030	-1.4%	-58%

Weighted means provided for treatment group and controls (the combined placebo/control group). Average treatment effect (ATE) estimated using weighted least squares regression with robust standard errors in parentheses; estimated *p*-values are one-sided due to the directional nature of our hypothesis. MF=“Mostly false,” F=“False,” POF=“Pants on fire.”

Table S4: Treatment effect relative to placebo (Hawthorne) condition

	Negative PolitiFact rating	Accuracy questioned	Combined
Treatment	-0.014 (0.009) [0.073]	-0.010 (0.006) [0.050]	-0.024 (0.011) [0.016]
Constant (Placebo)	0.024 (0.008)	0.013 (0.006)	0.037 (0.010)
R ²	0.00	0.00	0.01
N	778	778	778

Weighted least squares regression with robust standard errors in parentheses; estimated p -values (in brackets) are one-sided due to the directional nature of our hypothesis.

Table S5: Logistic regression

	Negative PolitiFact rating	Accuracy questioned	Combined
Treatment	-0.577 (0.571) [0.156]	-1.403 (1.063) [0.093]	-0.813 (0.500) [0.052]
Constant	-3.998 (0.270)	-4.566 (0.356)	-3.536 (0.216)
Marginal effect	-0.008 (0.007) [0.131]	-0.008 (0.004) [0.040]	-0.016 (0.008) [0.029]
N	1169	1169	1169

Logistic regression with robust standard errors in parentheses; estimated p -values (in brackets) are one-sided due to the directional nature of our hypothesis.

Table S6: Block fixed effects

	Negative PolitiFact rating	Accuracy questioned	Combined
Treatment	-0.008 (0.006) [0.108]	-0.008 (0.004) [0.041]	-0.016 (0.008) [0.023]
Constant	0.004 (0.003)	0.029 (0.026)	0.033 (0.026)
Block fixed effects	Yes	Yes	Yes
R ²	0.18	0.07	0.14
N	1169	1169	1169

Weighted least squares regression with robust standard errors in parentheses; estimated p -values (in brackets) are one-sided due to the directional nature of our hypothesis.

Table S7: Standard errors clustered by block

	Negative PolitiFact rating	Accuracy questioned	Combined
Treatment	-0.008 (0.007) [0.133]	-0.008 (0.003) [0.014]	-0.016 (0.008) [0.028]
Constant	0.018 (0.006)	0.010 (0.003)	0.028 (0.007)
R ²	0.00	0.00	0.00
N	1169	1169	1169

Weighted least squares regression with robust standard errors in parentheses; estimated p -values (in brackets) are one-sided due to the directional nature of our hypothesis.

References

Adair, Bill. 2012. Personal communication. October 18, 2012.

Adair, Bill. 2014. Personal communication. June 10, 2014.