Understanding and reducing the spread of misinformation online

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The spread of false and misleading news on social media is of great societal concern. Why do people share such content, and what can be done about it? In a first survey experiment (*N*=1,015), we demonstrate a disconnect between accuracy judgments and sharing intentions: even though true headlines are rated as much more accurate than false headlines, headline veracity has little impact on sharing. We argue against a "post-truth" interpretation, whereby people deliberately share false content because it furthers their political agenda. Instead, we propose that the problem is simply distraction: most people do not want to spread misinformation, but are distracted from accuracy by other salient motives when choosing what to share. Indeed, when directly asked, most participants say it *is* important to only share accurate news. Accordingly, across three survey experiments (total *N*=2775) and an experiment on Twitter in which we messaged *N*=5,482 users who had previously shared news from misleading websites, we find that subtly inducing people to think about the concept of accuracy increases the quality of the news they share. Together, these results challenge the popular post-truth narrative. Instead, they suggest that many people are capable of detecting low-quality news content, but nonetheless share such content online because social media is not conducive to thinking analytically about truth and accuracy. Furthermore, our results translate directly into a scalable anti-misinformation intervention that is easily implementable by social media platforms.

Key Words: misinformation; social media; fake news; partisanship; reasoning; decision making

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The spread of misinformation – including, but not limited to, blatantly false political "fake news" – on social media has become a major focus of public debate and academic study in recent years (*1*). Although misinformation is nothing new, the topic gained prominence in 2016 following the U.S. Presidential Election and the U.K.'s Brexit referendum during which entirely fabricated stories (presented as legitimate news) received wide distribution via social media. The apparent proliferation of misinformation on social media is of substantial concern given the increasing reliance on social media as a major source of news (*2*).

Misinformation is problematic for democracy because it leads to inaccurate beliefs and can exacerbate partisan disagreement over even basic facts. Indeed, false stories may spread as much (*3*) or more (*4*) than similar true stories, and merely reading false headlines – including partisan headlines that are extremely implausible and inconsistent with one's political ideology – makes them subsequently seem more true (*5*).

In addition to being concerning, the widespread sharing of misinformation on social media is also *surprising*, given the outlandishness of much of this content. It is hard to imagine that large numbers of people really believed, for example, that Hillary Clinton was operating a child sex ring out of a pizza shop or that Donald Trump was going to deport his wife, Melania Trump, after a fight at the White House. Nonetheless, these headlines and others like them have collectively received millions of shares on social media (*6*).

Here we investigate this willingness to share seemingly unbelievable content, and ask what the results suggest about interventions to reduce the spread of misinformation. First, we ask: Is the problem simply that people are unable to tell what news is true versus false? Or is there a dissociation between what people deem to be accurate and what they choose to share on social media?

In Study 1, we recruited N=1,015 Americans using Amazon Mechanical Turk (*7*) (MTurk) and presented them with the headline, lede, and image for 36 actual news stories taken from social media. We focus on headlines and ledes rather than full news stories as research suggests that, on social media, people often (or even typically) share news articles without clicking through to the actual story (*8*). Half of the headlines were entirely false (i.e., fabricated, as determined by the third-party fact-checking websites, Snopes.com and Factcheck.org) and half were true (taken from mainstream sources). Furthermore, to assess the importance of partisan alignment and "politically motivated reasoning" (Kahan, Peters, Dawson, & Slovic, 2017; Pennycook & Rand, 2019b), half of the headlines were chosen to be favorable to Democrats and the other half to be favorable to Republicans (as determined by a pretest; see Pennycook, Bear, Collins, & Rand, 2019). We classify headlines as politically concordant when participants who prefer the Democratic [Republican] party rate headlines that are attractive to Democrats [Republicans], and politically discordant in the opposite case. To test participants' ability to differentiate between true versus false news ("truth discernment"), half of the participants were asked to judge whether or not each headline was accurate (Accuracy condition). To test for a dissociation between accuracy and sharing, the other half of participants were instead asked if they would consider sharing the headline online (Sharing condition). For full methodological details and explanations of preregistrations for all studies, see SI.

In the Accuracy condition, true headlines received much higher accuracy ratings than false headlines (Fig 1a). Conversely, politically concordant headlines were only rated as slightly more accurate than politically discordant headlines. This suggests that, perhaps surprisingly, politically motivated reasoning does not play a particularly large role in these accuracy judgments. The pattern was the opposite, however, in the Sharing condition (Fig 1b): participants were only slightly more likely to consider sharing true headlines than false headlines, but much more likely to consider sharing politically concordant headlines than politically discordant headlines (Significant interactions between veracity and condition, F=258.6, p<.0001, and between concordance and condition, F=18.56, p<.0001; for full statistical details, see SI). For example, consider the headline “Over 500 ‘Migrant Caravaners’ Arrested With Suicide Vests”, which only 15.7% of Republicans rated as accurate, but 51.1% said they would consider sharing. In fact, overall our participants were substantially more likely to consider sharing concordant but false headlines (37.4%) than discordant but true headlines (24.0%, F=19.94, p<.0001). Together, these results indicate that our participants *can* effectively identify the accuracy of true versus false headlines when asked to do so – but they are nonetheless willing to share many false headlines that align with their partisanship.

What explains this dissociation between accuracy judgments and sharing intentions when it comes to politically concordant but false headlines? One explanation is offered by a common narrative arising in both scholarly work (*12–14*) and the popular press (*15–17*): that we are in a "post-truth" era where people place little value on accuracy, and thus knowingly share misinformation on social media. By this account, people are explicitly aware of the non-veracity of misleading content, but do not place a substantial amount of weight on veracity when making sharing decisions. At first blush, our data seem consistent with this idea: We find that participants can discern between true and false news content, but seem to barely take this information into account when considering what to share on social media.

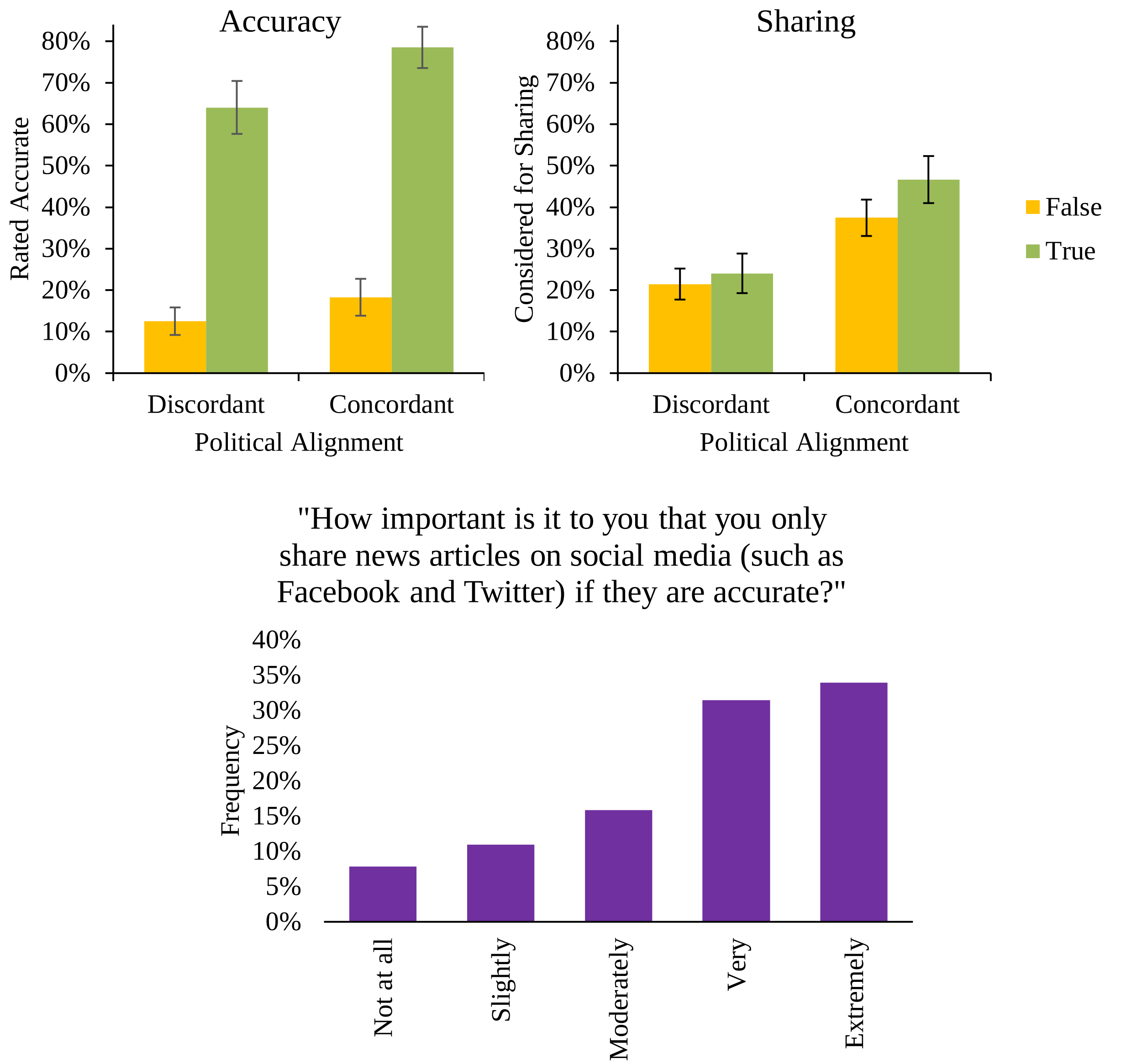

Figure 1. Participants can easily identify false headlines when asked to judge accuracy, but veracity has little impact on sharing intentions – despite participants' explicit commitment to only sharing accurate content. In Study 1, N=1002 Americans from Amazon Mechanical Turk were presented with a set of 36 headlines and either asked to indicate if they thought the headlines were accurate or if they would consider sharing them social media. (A) Shown is the fraction of headlines rated as accurate in the Accuracy condition, by the veracity of the headline and political alignment between the headline and the participant. Participants were much more likely to rate true headlines as accurate compared to false headlines, whereas the partisan alignment of the headlines had a much smaller impact. (B) Shown is the fraction of headlines participants said they would consider sharing in the Sharing condition, by the veracity of the headline and political alignment between the headline and the participant. In contrast to the Accuracy condition, headline veracity had little impact on sharing intentions, whereas partisan alignment played a larger role. Error bars indicate 95% confidence intervals based on standard errors clustered on participant and headline. (C) Nonetheless, when asked at the end of the study, participants overwhelmingly said they thought it was important to only share accurate content. Shown is the distribution over responses collapsing over conditions; for disaggregated responses, see SI.

This post-truth perspective, however, is not the only possible explanation of these findings – and we will argue it is probably not the dominant one. A first (suggestive) piece of evidence against the post-truth perspective comes from the questionnaire administered at the end of Study 1: When asked whether it is important to *only* share content that is accurate on social media (Fig 1c), the modal response was "extremely important". Conversely, only 7.9% of participants said it was "not at all important". Thus, most participants do not espouse a post-truth view, but instead report that they *do* think substantial weight should be put on accuracy when making sharing decisions.

Why, then, were participants in Study 1 – and millions of other Americans in recent years – so willing to share misinformation? In answer, we advance an alternative to the post-truth perspective. We argue that although most people do not want to spread inaccurate information, this accuracy motive may be overshadowed by other (often social) motives in the context of social media sharing (*18*, *19*). For example, the desire to attract and please followers/friends (*20*), to signal one's group membership (*21*), or to engage with emotionally or morally evocative content (*22*) may distract people from attending to headlines' veracity when deciding what to share. Indeed, even those participants who said it was very or extremely important to *only* share accurate content indicated that, on average, they would consider sharing 27.7% of the false headlines they were shown. Thus, even people with a strong regard for the truth may wind up sharing inaccurate headlines – because they fail to consider accuracy when making their sharing decisions. This distraction-based account stands in stark contrast to the post-truth perspective whereby people are *aware* of veracity but explicitly choose not to prioritize it when making sharing decisions.

We differentiate between these views by inducing people to think about accuracy and examining the effect on their subsequent sharing decisions. Importantly, our key experimental manipulation merely makes the *concept* of accuracy salient, rather than suggesting that accuracy is important (or advancing any other normative concerns related to accuracy). If people knowingly discount accuracy when choosing what to share – as per the post-truth perspective – then merely nudging them to think more about accuracy should have no effect: they already recognize whether the content is accurate, they just don't care that much. If, on the other hand, the sharing of misinformation is rooted in a distraction-based failure to consider accuracy, then our accuracy salience manipulation should help people overcome distraction and implement their (otherwise overlooked) desire to not share inaccurate content.

We first test these competing predictions in two survey experiments using Americans recruited from MTurk (Study 2, N=727; Study 3, N=780). The Control condition of these experiments was similar to the Sharing condition of Study 1: participants were shown 24 news headlines (balanced on veracity and partisanship, as in Study 1) and asked how likely they would be to share each headline on Facebook. In the Treatment, participants were asked to rate the accuracy of a single non-partisan news headline at the outset of the study (ostensibly as part of a pretest for stimuli for another study). They then went on to complete the same sharing intentions task completed by subjects in the Control condition – but with the concept of accuracy more likely to be top-of-mind. (The design of Studies 2 and 3 were identical, with the exception of using a different set of 24 headlines to demonstrate the generalizability of findings across headlines.) For full methodological details, see SI.

As predicted by our distraction-based account, in both experiments the Treatment increased sharing discernment (Fig 2a,b): Participants in the Treatment were significantly less likely to consider sharing false headlines compared to the Control (S2, F=14.08, p=.0002; S3, F=11.99, p=.0005), but equally likely to consider sharing true headlines (S2, F=.01, p= .92; S3, F=.23, p=.63; interaction between veracity and condition: S2, F=24.21, p<.0001; S3, F=19.53, p<.0001). As a result, the Treatment roughly doubled the difference in sharing of true versus false headlines (i.e., truth discernment) relative to the Control. Furthermore, the Treatment effect was even larger for politically concordant headlines compared to politically discordant headlines (significant three-way interaction between concordance, veracity, and condition: S2, F=8.46, p=.004; S3, F=14.69, p=.0001), which suggests that considering accuracy did not lead to politically motivated reasoning (*10*) (i.e., triggering people to reflect on accuracy did not merely increase their partisan inclinations – if anything, it did the opposite); and the Treatment reduced sharing intentions of false content for Democrats and Republicans alike (no significant interaction between partisanship, veracity, and condition: S2, F=2.80, p=.094; S3, F=2.19, p=.139). See SI for full statistical details.

Importantly, there was no significant difference between conditions in responses to the post-experimental question regarding the importance of only sharing accurate content (t-test: t(1498)=.42, p=.68, 95% CI [-0.075,0.115] points on a 1-5 scale), and when controlling for responses to this question, the treatment effect on sharing was virtually unchanged (interaction between condition and veracity: S2, F=24.4, p<.0001; S3, F=20.6, p<.0001). These results are consistent with the treatment not *manufacturing* a motive for accurate sharing, but rather helping people *attend* to an existing (but latent) accuracy motive. Study 3 also included a post-experimental question regarding participants' perceptions of the importance their *friends* place on only sharing accurate content. Again, no significant treatment effect was observed (t-test: t(768)=-.57, p=.57, 95% CI [-0.205,0.113] points on a 1-5 scale), and the treatment effect on sharing remained significant when controlling for this question (interaction between condition and veracity: F=19.51, p<.0001). These results are consistent with the treatment not operating by changing perceived social norms around sharing. (We also note that participants rated their own concern for accuracy as significantly higher than their friends' concern for accuracy, one-sample t-test: t(1222)=31.52, p<0.0001.)

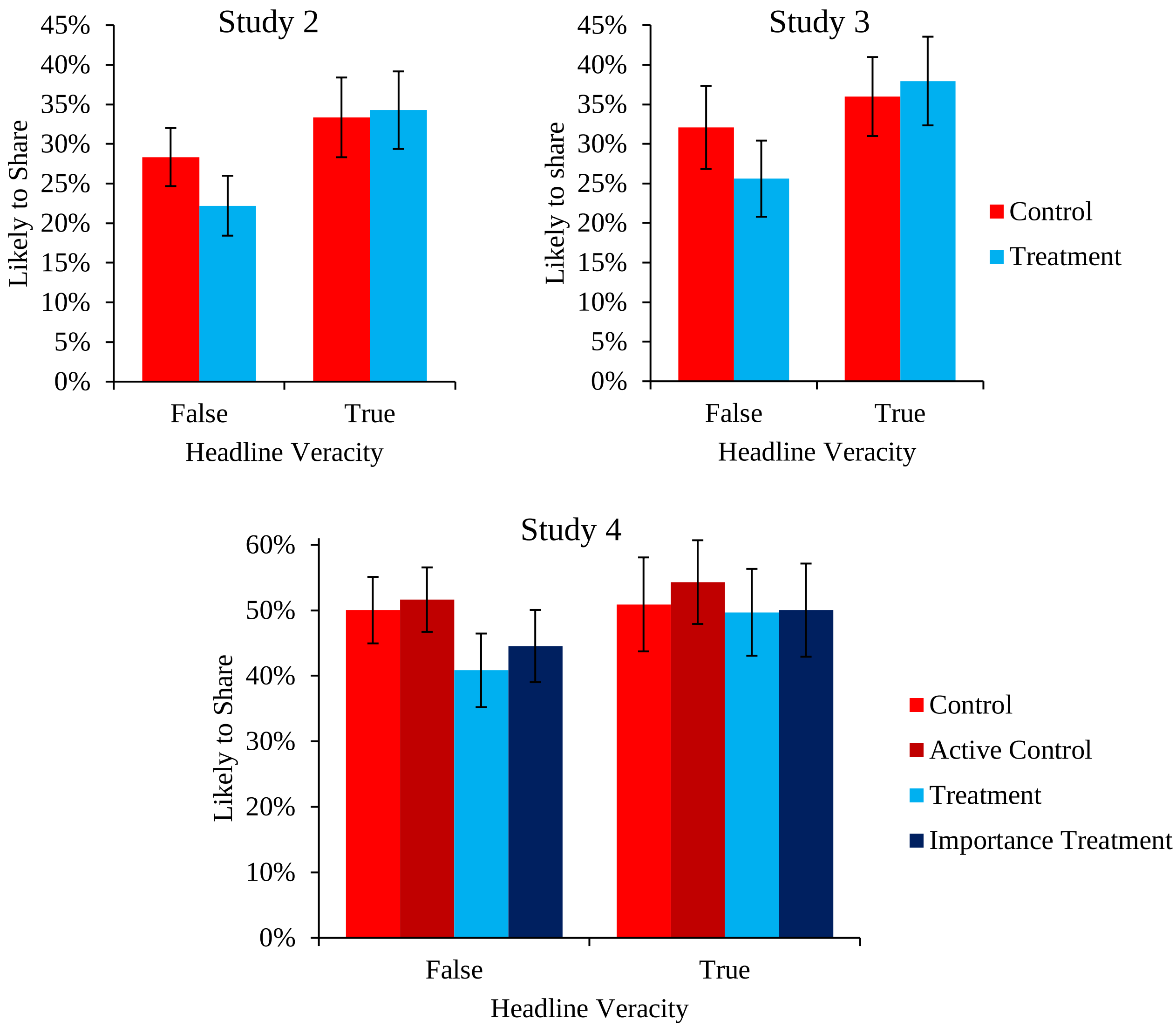

Figure 2. Inducing survey respondents to think about accuracy reduces willingness to share blatantly false news headlines. Participants in Studies 2 (A; N=727 Americans from MTurk), Study 3 (B; N=780 Americans from MTurk), and Study 4 (C; N=1,268 Americans from Lucid, nationally representative on age, gender, ethnicity, and geographic region) indicated how likely they would be to consider sharing a series of actual headlines from social media. Participants in the Treatment rated the accuracy of a single non-political headline at the outset of the study, thus increasing the likelihood that they would think about accuracy when indicating sharing intentions relative to the Control. In Study 4, we added an Active Control (in which participants rated the humorousness of a single headline at the outset of the study) and an Importance Treatment (in which participants were asked at the study outset how important they thought it was so only share accurate content). For interpretability, shown here is the fraction of "likely" responses (responses above the midpoint of the 6-point Likert scale) by condition and headline veracity; raw means are shown in the SI. As per our preregistered analysis plans, these analyses focus only on participants who indicated that they sometimes consider sharing political content on social media; for analysis of all participants, see SI. Error bars indicate 95% confidence intervals based on standard errors clustered on participant and headline.

Next, in Study 4, we provide additional support for our distraction-based account in another survey experiment that builds on Studies 2 and 3 in several ways. First, Study 4 tested whether the previous results generalize to a more representative sample by recruiting N=1268 participants from Lucid (*23*) that were quota-sampled to match the distribution of American residents on age, gender, ethnicity, and geographic region. Second, in addition to the same Control and Treatment conditions from the previous experiments, Study 4 included an Active Control condition in which participants were asked to rate the *humorousness* (rather than accuracy) of a single non-partisan news headline at the outset of the study. The Active Control allows us to test whether the treatment effect found in Studies 2 and 3 was specifically the result of asking about *accuracy* at the outset of the study, or merely the result of asking participants to rate any feature of the content. Finally, Study 4 tested whether the treatment effect generalized to another approach for making accuracy salient: instead of rating the accuracy of a headline at the outset of the study, participants in the Importance Treatment began the study by answering the question about the importance of only sharing accurate content that the other subjects completed at the end of the study. For full methodological details, see SI.

Study 4 (Figure 2c) successfully replicated Studies 2 and 3. As expected, there were no significant differences in sharing intentions between the Control and the Active Control conditions (no significant simple effect of condition and no significant interaction between condition and veracity, $p \geq 0.20$ for all). Collapsing over the two control conditions, we observed that each treatment condition significantly increased sharing discernment relative to control (Interaction between veracity and condition: Treatment, F=11.98, p=.0005; Importance Treatment, F=9.76, p=.0018). See SI for full statistical details.

Finally, to test whether our findings generalize to natural social media use settings (rather than laboratory experiments), actual (rather than hypothetical) sharing decisions, and misinformation more broadly (rather than just blatantly false "fake news"), we conducted a digital field experiment on social media (*24*). To do so, we delivered the same treatment used in the survey experiments to Twitter users, and observed its impact on the quality of the news content they subsequently shared. Specifically, we sent users private messages asking them to rate the accuracy of a single non-political headline (Figure 3B). We did not expect users to *respond* to our message – our intervention was based on the idea that merely reading the opening line ("How accurate is this headline?") would make the concept of accuracy more top-of-mind. Accordingly, we perform intent-to-treat analyses including all subjects regardless of whether they responded to (or even opened) the message. Furthermore, to avoid demand effects, users were not informed that the message was being sent as part of a research study, and the accounts from which we sent the messages had innocuous descriptions (e.g. "Cooking Bot").

To allow for causal inference, we used a stepped-wedge (randomized roll-out) design in which users were randomly assigned to a date on which to receive the treatment message. Within each

24-hour time-window, we can then compare the links shared by users randomly assigned to receive the treatment message at the beginning of that time window to the links shared by all the users who had not yet been messaged (and who thus serve as a control). We then combined estimates across dates to arrive at an overall treatment effect. (In addition to improving statistical power, this stepped-wedge design is required because the rate limits imposed by Twitter forced us to only send a small number of messages per account per day.) To quantify the quality of the news shared in any given post, we follow recent work (*3*, *25*) and assess quality based on the domain from which a news link comes (given the large volume of data, analyzing the details of the specific articles was infeasible). Specifically, we used a previously published list of 60 news websites whose trustworthiness had been rated by professional fact-checkers (*26*); Figure 3B. For full methodological details, see SI.

Our subject pool for Study 5 consisted of *N*=5,482 Twitter users who had previously shared links to websites that publish misleading or false news content. Given that Republicans have been found to share substantially more misinformation than Democrats (*3*, *27*), we specifically constructed a pool of users who had shared links to two particularly well-known sites that professional fact-checkers have rated as highly untrustworthy (*26*): Breitbart.com and Infowars.com. Examining baseline (pre-treatment) sharing behavior shows that we were successful in identifying users with relatively low-quality news sharing habits: The average quality score of news sources from pre-treatment posts was 0.34. (For comparison, the fact-checker-based quality score was 0.02 for *Infowars*; 0.16 for *Breitbart*; 0.39 for *Fox News*, and 0.93 for the *New York Times*). Moreover, 48.9% of shared news sites were sites that publish false or misleading content (0.9% fake news sites, 48.0% hyperpartisan sites). Together with other work suggesting that the incidence of blatantly false content on social media may be quite low (*3*, *27*), these data emphasize the practical importance of knowing whether our treatment generalizes to the sharing of hyperpartisan content. This test is also of substantial theoretical importance, as hyperpartisan news is a context where motivated reasoning may be more likely to occur (because the claims being made are less implausible) (*28*).

Given the greater complexity of the experimental design and tweet data, there are numerous reasonable ways to analyze the data. For simplicity, we focus on an analysis in which both primary tweets and retweets are analyzed, data is excluded from one day on which a technical issue led to randomization failure, and the simplest admissible model structure is used (wave fixed effects, p-values calculated in the standard fashion using linear regression with robust standard errors clustered on user); and then assess robustness to varying the specification.

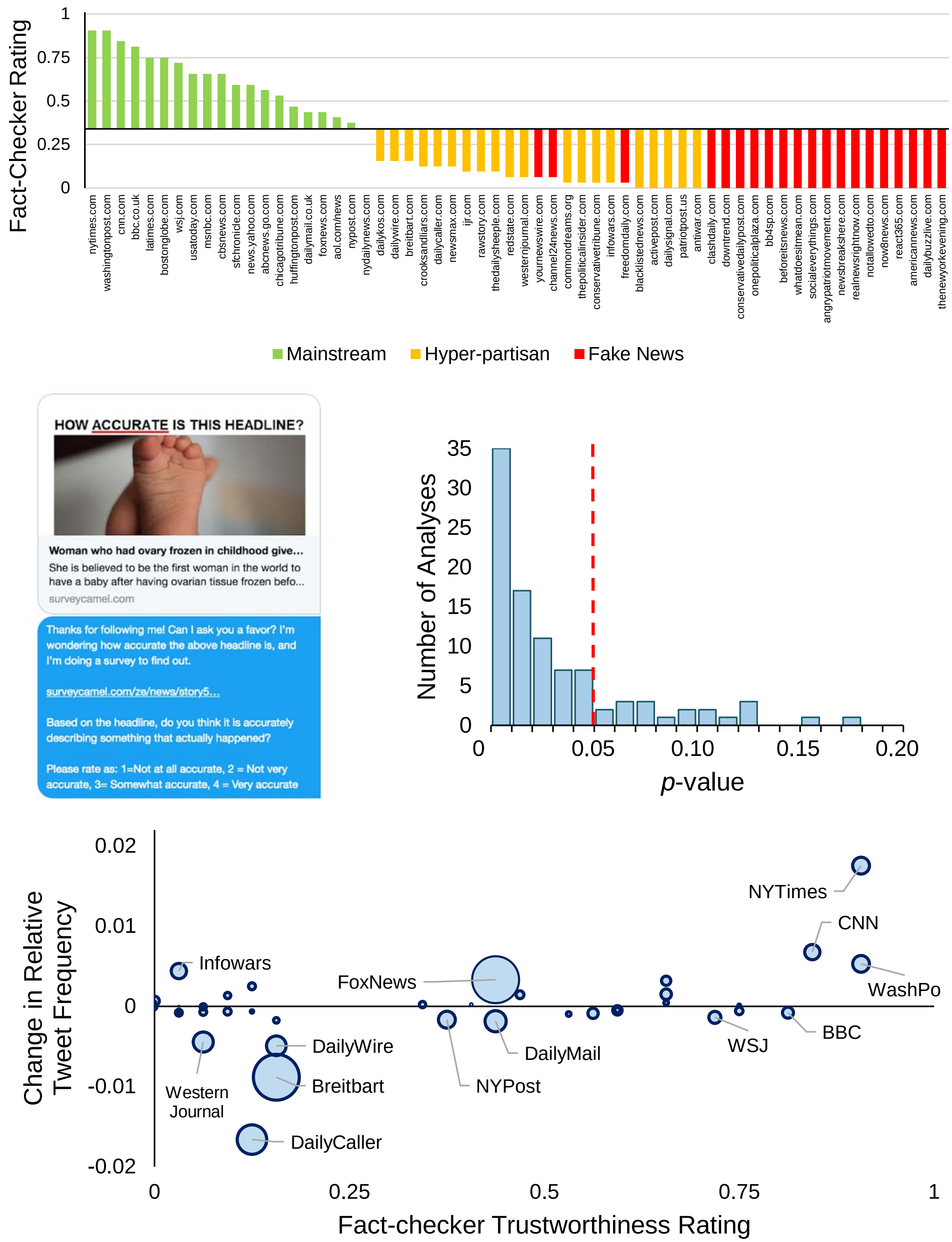

Figure 3. Sending Twitter users a message asking for their opinion about the accuracy of a single non-political headline increases the quality of the news they subsequently share. In Study 5, we conducted an experiment on the Twitter platform involving N=5,482 Twitter users who had recently shared links to websites that

regularly produce misleading and hyperpartisan content. We randomized the date on which users were sent an unsolicited message asking them to rate the accuracy of a single non-political headline. We then compared the quality of the news sites shared in the 24 hours after receiving the message to the sites shared by participants who had not yet received the message. (A) As our measure of quality, we used trust ratings given to 60 news websites by 8 professional fact-checkers (from (*26*)). The websites span the full trust range. The baseline is set to a quality score or 0.34, which is the average pre-treatment quality score among the users in our experiment. (B) The private message sent to the users is shown here. We did not expect most users to respond to the message, or even read it in its entirety. Thus we designed it such that reading only the top line should be sufficient to make the concept of accuracy salient. (C) To test the robustness of our results, we conducted 98 analyses that differed in their dependent variable, inclusion criteria and model specifications. Shown here is the distribution of p-values resulting from each of these analyses. Over 80% of approaches yield $p<0.05$. (D) A domain-level analysis provides a more detailed picture of the effect of the intervention. The x-axis indicates the trust score given to each outlet by the fact-checkers. The y-axis indicates the fraction of rated links to each outlet in the 24 hours after the intervention minus the fraction of links to each outlet among not-yet-treated users. The size of each dot is proportional to the number of pre-treatment posts with links to that outlet. Domains with more than 500 pre-treatment posts are labeled.

Consistent with our survey experiments, we find clear evidence that the accuracy message made users more discerning in their subsequent sharing decisions. Relative to baseline, the accuracy message increased the average quality of the news sources shared, $t(5481)=2.61$, $p=0.009$, and the total quality of shared sources summed over all posts, $t(5481)=2.68$, $p=0.007$, by 1.9% and 3.5% respectively. Furthermore, the treatment roughly doubled the level of sharing discernment (0.05 more mainstream than misinformation links shared per user-day pre-treatment; 0.10 more mainstream than misinformation links shared per user-day post-treatment; interaction between post-treatment dummy and link type, $t(5481)=2.75$, $p=0.006$). Conversely, we found no significant treatment effect on the number of posts *without* links to any of the 60 rated news sites, $t(5481)=.31$, $p=0.76$, which is consistent with the specificity of the treatment.

This pattern of results is not unique to one particular set of analytic choices. Figure 3C shows the distribution of p-values observed in 96 different analyses assessing the treatment effect on average quality, summed quality, or discernment under a variety of analytic choices. Of these analyses, 80.2% indicate a significant positive treatment effect (and none of 32 analyses of posts without links to a rated site find a significant treatment effect). For statistical details, see SI.

Finally, we examine the data at the level of the domain (Figure 3D). We see that the treatment effect is driven by increasing the fraction of rated-site posts with links to mainstream new sites with strong editorial standards such as the *New York Times*, and decreasing the fraction of rated-site posts that linked to relatively untrustworthy hyperpartisan sites such as the *Daily Caller*. Indeed, a domain-level pairwise correlation between fact-checker rating and change in sharing due to the intervention shows a very strong positive relationship (domains weighted by number of pre-treatment posts; r=0.70). In sum, our accuracy message successfully induced Twitter users who regularly shared misinformation to increase the quality of the news they shared.

Together, these studies shed new light on why people share misinformation, and introduce a new class of interventions aimed at reducing its spread. Our results suggest that, at least for many people, the misinformation problem is not driven by a basic inability to tell which content is inaccurate, or a desire to purposefully share inaccurate content. Instead, our findings implicate inattention on the part of people who are able to determine the accuracy of content (if they put their mind to it) and who are motivated to avoid sharing inaccurate content (if they realize it is inaccurate). It seems as though people are often distracted from considering the content's accuracy by other motives when deciding what to share on social media – and therefore, drawing attention to the concept of accuracy can nudge people toward reducing their sharing of misinformation.

These findings have important implications for theories of partisan bias, political psychology, and motivated reasoning. First, at a general level, the dissociation we observed between accuracy judgments and sharing intentions suggests that just because someone shares a piece of news on social media does not necessarily mean that they believe it – and thus, that the widespread sharing of false or misleading partisan content should not necessarily be taken as an indication of the widespread adoption of false beliefs or explicit agreement with hyperpartisan narratives. Furthermore, our results sound a rather optimistic note in an arena which is typically much more pessimistic: rather than partisan bias blinding our participants to the veracity of claims (Kahan, 2017; Kahan et al., 2017), or making them knowing disseminators of ideologically-confirming misinformation (Hochschild & Einstein, 2016; McIntyre, 2018; Petersen et al., 2018), our results suggest that many people mistakenly choose to share misinformation because they were merely distracted from considering the content's accuracy.

Identifying which particular motives are most active when on social media – and thus are most important for distracting people from accuracy – is an important direction for future work. Another issue for future work is more precisely identifying people's state of belief when not reflecting on accuracy: Is it that people hold no particular belief one way or the other, or that they tend to assume content is true by default (*31*)? Although our results do not differentiate between these possibilities, prior work suggesting that intuitive processes support belief in false headlines (*10*, *32*) lends some credence to the latter possibility. Similarly, future work should investigate *why* most people think it is important to only share accuracy content (*33*) – differentiating, for example, between an internalized desire for accuracy versus reputation-based concerns. Finally, future work should examine how these results generalize across different subsets of the American population, and – even more importantly – cross-culturally, given that misinformation is a major problem in areas of the world that have very different cultures and histories from the United States.

From an applied perspective, our results highlight an often overlooked avenue by which social media fosters the spread of misinformation. Rather than (or in addition to) the phenomenon of echo chambers and filter-bubbles (*34*, *35*), social media platforms may actually discourage people from reflecting on accuracy (*36*). These platforms are designed to encourage users to rapidly scroll and spontaneously engage with feeds of content, and mix serious news content with emotionally engaging content where accuracy is not a relevant feature (e.g., photos of babies, videos of cats knocking objects off tables for no good reason). Social media platforms also provide immediate quantified social feedback (e.g., number of likes, shares, etc.) on users' posts and are a space which users come to relax rather than engage in critical thinking. These factors imply that social media platforms may, *by design*, tilt users away from considering accuracy when making sharing decisions.

But this need not be the case. Our treatment translates easily into interventions that social media platforms could employ to increase users' focus on accuracy. For example, platforms could periodically ask users to rate the accuracy of randomly selected headlines (e.g. "to help inform algorithms") – thus reminding them about accuracy in a subtle way that should avoid reactance. The platforms also have the resources to optimize the presentation and details of the messaging, likely leading to effect sizes much larger than what we observed here in the proof-of-concept offered by Study 5. This optimization should include investigations of which messaging and sample headlines lead to the largest effects for which subgroups, how the effect decays over time (our stepped-wedge design did not provide sufficient statistical power to look beyond a 24 hour window), how to minimize adaptation to repeated exposure to the intervention (e.g. by regularly changing the form and content of the messages), and whether adding a normative component to our primarily cognitive intervention can increase its effectiveness. Approaches such as the one we propose could potentially increase the quality of news circulating online without relying on a centralized institution to certify truth and censor falsehood.

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Supplementary Materials

for

Understanding and reducing the spread of misinformation online

Contents

Study 1

In our first study, we sought to investigate whether individuals fail to consider the plain inaccuracy of false news relative to true news when making judgments about sharing content on social media. Specifically, we expect that people are relatively good at discerning between false and true news when asked to judge whether the headlines are accurate or inaccurate, but they will be relatively poor at discerning between false and true news when asked to indicate whether they would consider sharing the content on social media. Thus, in Study 1, participants were presented with a pretested set of false and true headlines (in "Facebook format", see Figure 1) and were either asked to indicate whether they thought they were accurate or not, or whether they would consider sharing them on social media or not. Our prediction is that the difference in 'yes' responses between false and true news (i.e., discernment) will be greater when individuals are asked about accuracy than when they are asked about sharing, whereas the difference between ideological discordant and concordant news (i.e., bias) will be greater when they are asked about sharing than when they are asked about accuracy. Our preregistration is available at https://osf.io/p6u8k/. This study was approved by the MIT COUHES (Protocol #1806400195).

Figure S1*. Example false (left) and true (right) news headlines in "Facebook format", as shown to participants in Studies 1-4.*

Method

Participants

We preregistered a target sample of 1,000 complete responses, using participants recruited from Amazon's Mechanical Turk (MTurk) but noted that we would retain individuals who completed the study above the 1,000-participant quota. In total, 1,825 participants began the survey. However, an initial screener only allowed American participants who indicated having a Facebook or Twitter account (when shown a list of different social media platforms) and indicated that they would consider sharing political content (when shown a list of different content types) to continue and complete the survey. The purpose of these screening criteria was to focus our investigation on the relevant subpopulation – those who share political news. The accuracy judgments of people who never share political news on social media are not relevant here, given our interest in the sharing of political misinformation. Of the participants who entered the survey, 153 indicated that they had neither a Facebook nor Twitter account, and 651 indicated that they did have either a Facebook or Twitter account but would not consider sharing political content. A further 16 participants passed the screener but did not finish the survey and thus were removed from the data set. The full sample (Mean age = 36.7) included 475 males, 516 females, and 14 participants who selected another gender option.

Materials

We presented participants with 18 false ("fake") and 18 true ("real") news headlines in a random order for each participant. The false news headlines were originally selected from a third-party fact-checking website, Snopes.com, and were therefore verified as being fabricated and untrue. The true news headlines were all accurate and selected from mainstream news outlets to be roughly contemporary with the false news headlines. Moreover, the headlines were selected to be either Pro-Democratic or Pro-Republican (and equally so). This was done using a pretest, which confirmed that the headlines were equally partisan across the categories (for a similar approach, see Pennycook, Bear, Collins, & Rand, 2019; Pennycook, Cannon, & Rand, 2018; Pennycook & Rand, 2019). The pretest asked participants to (among other things) rate the political partisanship of 10 randomly selected news headlines (from a corpus of 70 false or 70 true) using the following question: "Assuming the above headline is entirely accurate, how favorable would it be to Democrats versus Republicans" – 1 = More favorable for Democrats, 5 = More favorable for Republicans). We then selected the items used in Study 1 such that the Pro-Democratic items were equally different from the scale midpoint as the Pro-Republican items.

Participants in Study 1 were also asked: "How important is it to you that you only share news articles on social media (such as Facebook and Twitter) if they are accurate", to which they responded on a 5-point scale from 'not at all important' to 'extremely important'. We also asked participants about their frequency of social media use, along with several exploratory questions about media trust. At the end of the survey, participants were asked if they responded randomly at any point during the survey or searched for any of the headlines online (e.g., via Google). As noted in our preregistration, we did not intend on excluding these individuals. Participants also completed several additional measures as part of separate investigations (this was also noted in the preregistration); namely, the 7-item Cognitive Reflection Test (Pennycook & Rand, 2019), a political knowledge questionnaire, and the positive and negative affective schedule (Watson, Clark, & Tellegen, 1988). In addition, participants were asked several demographic questions (age, gender, education, income, and a variety of political and religious questions). The most central political partisanship question was "Which of the following best describes your political preference" followed by the following response options: Strongly Democratic, Democratic, Lean Democratic, Lean Republican, Republican, Strongly Republican. For purposes of data analysis, this was converted to a Democratic/Republican binary. The survey was completed on August 13th-14th, 2019. The full survey is available online in both text format and as a Qualtrics file, along with all data (https://osf.io/p6u8k/).

Procedure

Participants in the accuracy condition were given the following instructions: "You will be presented with a series of news headlines from 2017 to 2019 (36 in total). We are interested in whether you think these headlines describe an event that actually happened in an accurate and unbiased way. Note: The images may take a moment to load." In the sharing condition, the middle sentence was replaced with "We are interested in whether you would consider sharing these stories on social media (such as Facebook or Twitter).” We then presented participants with the full set of headlines in a random order. In the accuracy condition, participants were asked “To the best of your knowledge, is this claim in the above headline accurate?” In the sharing condition, participants were asked “Would you consider sharing this story online (for example, through Facebook or Twitter)?” In both conditions, the response options were simply “No” and “Yes.” Moreover, participants either saw the response options listed as Yes/No or No/Yes (randomized across participants – i.e., an individual participant only ever saw ‘yes’ first or ‘no’ first).

Analysis plan

Our preregistration specified that all analyses would be performed at the level of the individual item (i.e., one data point per item per participant; 0 = No, 1 = Yes) using linear regression with robust standard errors clustered on participant. However, we subsequently realized that we should also be clustering standard errors on headline (as multiple ratings of the same headline are non-independent in a similar way to multiple ratings from the same participant), and thus deviated from the preregistrations in this minor way (all key results are qualitatively equivalent if only clustering standard errors on participant). The linear regression was preregistered to have the following independent variables: a condition dummy (-0.5=accuracy, 0.5=sharing), a news type dummy (-0.5=false, 0.5=true), a political concordance dummy (-0.5=discordant, 0.5=concordant), and all 2-way and 3–way interactions. [Political concordance is defined based on the match between content and ideology. Specifically, political concordant = Pro-Democratic [Pro-Republican] news (based on a pretest) for American individuals who prefer the Democratic [Republican] party over the Republican [Democratic]. Politically discordant is the opposite.] Our key prediction was that there would be a negative interaction between condition and news type, such that the difference between false and true is smaller in the sharing condition than the accuracy condition. A secondary prediction was that there would be a positive interaction between condition and concordance, such that the difference between concordant and discordant is larger in the sharing condition than the accuracy condition. We also said we would check for a 3-way interaction, and use a Wald test of the relevant net coefficients to test how sharing likelihood of false concordant headlines compares to true discordant headlines. Finally, as robustness checks, we said we would repeat the main analysis using logistic regression instead of linear regression, and using ratings that are z-scored within condition.

Results

The fraction of “yes” responses by condition, veracity, and headline concordance are shown in the main text Figure 1, and reproduced here in Figure S2. The regression results are in shown in Table S1. We observe a significant main effect of condition in Models 1 and 2, such that overall, participants were more likely to rate headlines as true than to say they would consider sharing them (this difference is eliminated by design in Model 3 because responses are z-scored within condition). Across all 3 models, we unsurprisingly observe significant positive main effects of veracity and concordance ($p < .001$ for both main effects in all models).

Critically, as predicted, across all models we observe a significant negative interaction between condition and veracity, and a significant positive interaction between condition and headline concordance ($p < .001$ for both interactions in all models). Thus, participants are less sensitive to veracity, and more sensitive to concordance, when making sharing decisions than accuracy judgments. We also observe no significant 3-way interaction (*p* > .100 in all models). Finally, we see inconsistent evidence regarding a positive interaction between veracity and concordance, such that veracity may play a bigger role among concordant headlines than discordant headlines.

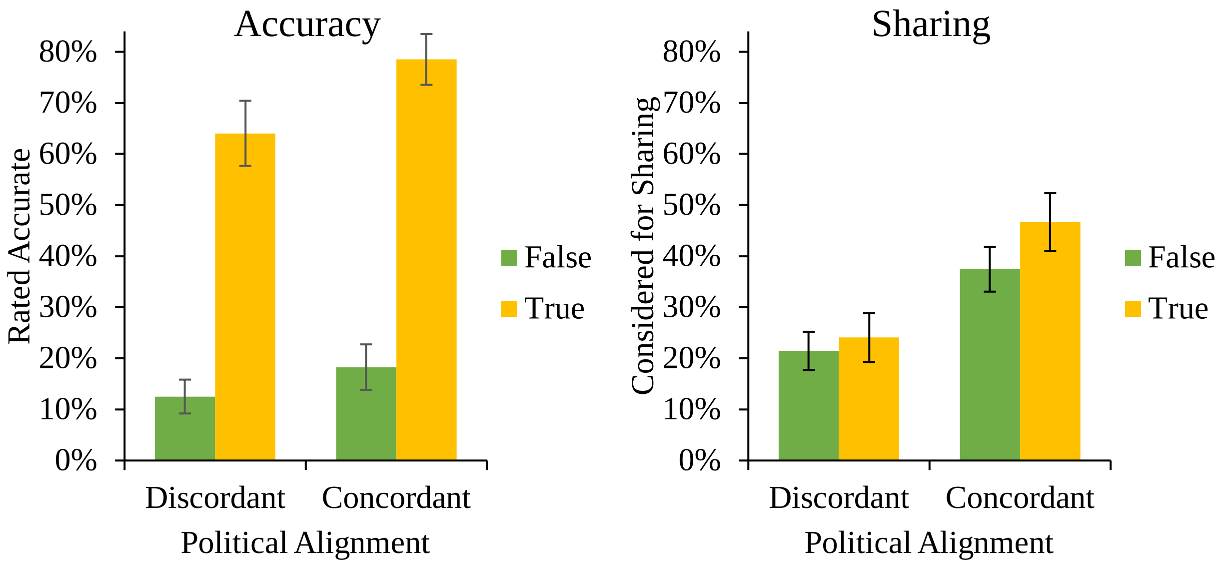

Figure S2. Fraction of "Yes" responses in Study 1 by condition, veracity, and concordance. Error bars indicate 95% confident intervals.

	(1) Linear Rating	(2) Logistic Rating	(3) Linear *z*-Rating
Condition (Accuracy=-0.5, Sharing=0.5)	-0.111***	-0.396***	0.000386
	(0.0182)	(0.102)	(0.0377)
Veracity (False=-0.5, True=0.5)	0.308***	1.449***	0.624***
	(0.0203)	(0.109)	(0.0420)
Concordance of headline (-0.5=discordant, 0.5=concordant)	0.145***	0.727***	0.304***
	(0.0181)	(0.0994)	(0.0378)
Condition X Veracity	-0.499***	-2.378***	-0.999***
	(0.0310)	(0.180)	(0.0638)
Condition X Concordance	0.0954***	0.345**	0.216***
	(0.0221)	(0.115)	(0.0464)
Veracity X Concordance	0.0788*	0.275	0.163*
	(0.0349)	(0.191)	(0.0725)
Condition X Veracity X Concordance	-0.0227	-0.0707	-0.0376
	(0.0396)	(0.202)	(0.0826)
Constant	0.380***	-0.575***	0.00128
	(0.0112)	(0.0592)	(0.0233)
Observations	36,428	36,428	36,428
Participant clusters	1005	1005	1005
Headline clusters	36	36	36
R-squared	0.206		0.187
Standard errors in parentheses			
*** p<0.001, ** p<0.01, * p<0.05			

Table S1. Regressions predicting responses (0 or 1) in Study 1. Models 1 and 3 use linear regression; Model 2 uses logistic regression. Models 1 and 2 use the raw responses; Model 3 uses responses that are z-scored within condition. Robust standard errors clustered on participant and headline.

Finally, we conduct a post hoc test of whether responses to the post-experimental question “How important is it to you that you only share news articles on social media (such as Facebook and Twitter) if they are accurate” vary across conditions. In particular, one might expect that being assigned to the Sharing condition, and thus making many sharing decisions involve false headlines, might make people report less concern with only sharing accurate content. Somewhat in line with this hypothesis, we find that average responses to this importance question are slightly lower in the Sharing condition (M = 3.65, SD = 1.25) compared to the Accuracy condition (M = 3.80, SD = 1.25), although the difference is only marginally significant, t(1003) = 1.83, p = .067. Be that as it may, only a small fraction of participants in either condition indicated that they thought only sharing accurate content was not at all important or only slightly important (17.3% in Accuracy, 20.3% in Sharing). Figure S3 shows the distribution of responses within each condition.

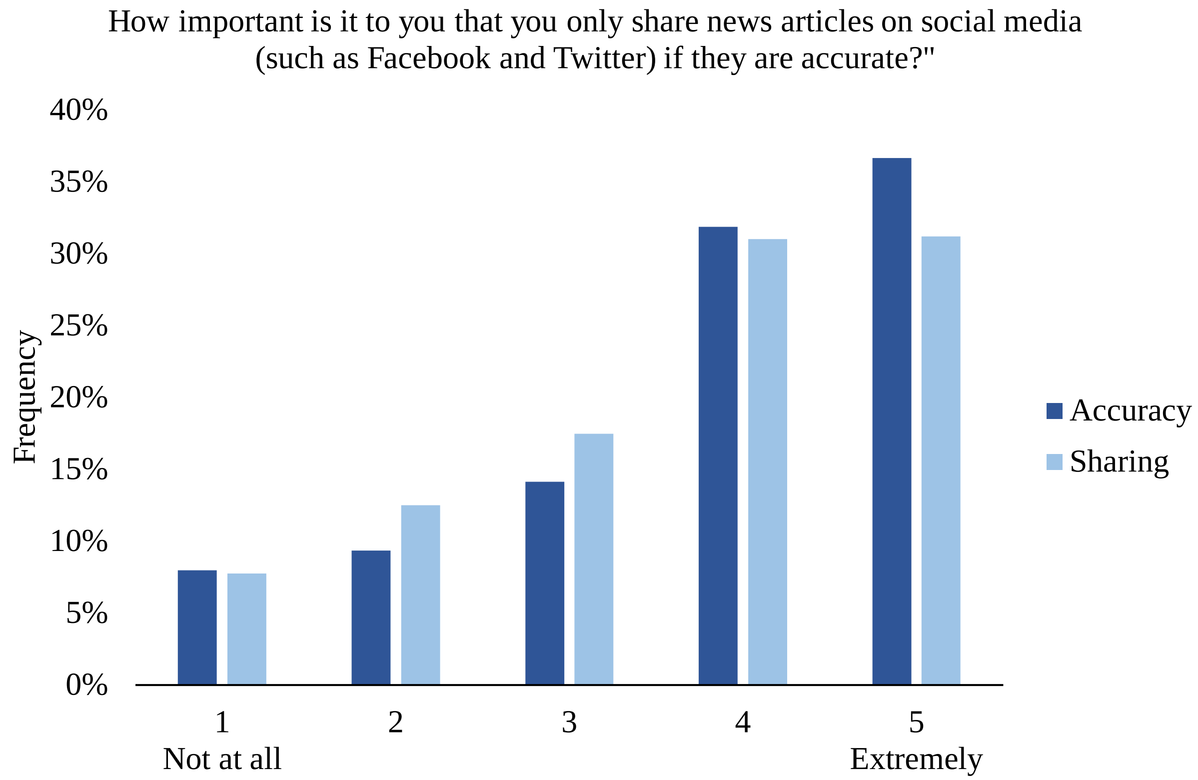

Figure S3. Distribution of responses to the post-experimental question in Study 1 regarding the importance of only sharing accurate content on social media.

Studies 2 and 3

Study 1 established that participants were far better at discerning between false and true news when making judgments about accuracy relative to judgments about social media sharing. In Studies 2 and 3, we investigate whether priming people to reflect about accuracy selectively decreases the willingness to share false news (relative to true news) on social media. In particular, participants were asked to judge the accuracy of a single (politically neutral) news headline at the beginning of the study, ostensibly as the part of a pretest for another study. We then tested whether this subtle accuracy-cue impacts individuals' ability to discern between false and true news when making judgments about social media sharing. The only difference between Studies 2 and 3 was the set of headlines used, to demonstrate the generalizability of these findings. Our preregistrations for both experiments are available at https://osf.io/p6u8k/. These study was approved by the Yale University Committee for the Use of Human Subjects (IRB protocol #1307012383).

Method

Participants (Study 2)

We preregistered a target sample of 1200 participants from MTurk. In total, 1254 participants began the survey. However, 21 participants reporting not having a Facebook profile at the outset of the study and, as per our preregistration, were not allowed to proceed; and 71 participants did not complete the survey. The full sample (Mean age = 33.7) included 453 males, 703 females, and 2 who did not answer the question. Following the main task, participants were asked if they "would ever consider sharing something political on Facebook" and were given the following response options: 'Yes', 'No', and 'I don't use social media'. As per our preregistration, only participants who selected 'Yes' to this question were included in our main analysis. Following Pennycook et al. (2019), the purpose of this exclusion is to estimate the effect of the treatment on individuals for which there is a possible treatment effect (i.e., people who are not willing to share political content online do not display the behavior that we are attempting to influence). This excluded 431 people and the sample of participants who would consider sharing political content (Mean age = 34.5) included 274 males, 451 females, and 2 who did not to answer the gender question. Unlike in Study 1, because this question was asked after the experimental manipulation (rather than at the outset of the study), there is the possibility that this exclusion may introduce selection effects and undermine causal inference (Montgomery, Nyhan, & Torres, 2018). While there was no significant difference in responses to this political sharing question between conditions (χ^2 test; S2: $\chi^2(1, N = 1{,}158) = .156$, $p = .69$; S2: χ^2 $(1, N = 1{,}248) = .988$, $p = .32$; S2 and S3 combined, χ^2 $(1, N = 2{,}406) = .196$, $p = .66$), for completeness we show that all of our results are robust to including all participants.

Participants (Study 3)

We preregistered a target sample of 1200 participants from MTurk. In total, 1328 participants began the survey. However, 8 participants did not report having a Facebook profile and 72 participants did not finish the survey. The full sample (Mean age = 33.3) included 490 males, 757 females, and 1 who did not answer the question. Restricting to participants were responded "Yes" when asked if they "would ever consider sharing something political on Facebook" excluded 468 people, such that the sample of participants who would consider sharing political content (Mean age = 33.6) included 282 males, 497 females, and 1 who did not answer the gender question.

Materials (Study 2)

We presented participants with 12 false and 12 true news headlines from (Pennycook et al., 2019) in a random order for each participant. The false news headlines were originally selected from a third-party fact-checking website, Snopes.com, and were therefore verified as being fabricated and untrue. The true news headlines were all accurate and selected from mainstream news outlets to be roughly contemporary with the false news headlines. Moreover, the headlines were selected to be either Pro-Democratic or Pro-Republican (and equally so). This was done using a pretest, which confirmed that the headlines were equally partisan across the categories (Pennycook & Rand, 2019). The pretest asked participants to rate a subset of news headlines on the following dimensions: Partisanship ("Assuming the above headline is entirely accurate, how favorable would it be to Democrats versus Republicans" – 1 = More favorable for Democrats, 5 = More favorable for Republicans), plausibility ("What is the likelihood that the above headline is true" – 1 = Extremely unlikely, 7 = Extremely likely), and familiarity ("Are you familiar with the above headline (have you seen or heard about it before)?" – Yes/Unsure/No) (see Pennycook, Cannon, & Rand, 2018; Pennycook & Rand, 2019, for further details on the pretest).

As in Study 1, following the main task participants were asked about the importance of only sharing accurate news articles on social media, along with several exploratory questions about media trust. Participants were also asked if they responded randomly at any point during the survey or searched for any of the headlines online (e.g., via Google). Participants also completed the 7-item Cognitive Reflection Test (Pennycook & Rand, 2019). Finally, participants were asked several demographic questions (age, gender, education, income, and a set of political and religious questions). The most central political partisanship question was "If you absolutely had to choose between only the Democratic and Republican party, which would do you prefer?" followed by the following response options: Democratic Party, Republican Party. The survey was completed on October 4th-6th, 2017. The full survey is available online in both text format and as a Qualtrics file, along with all data (https://osf.io/p6u8k/).

Materials (Study 3)

We used a different set of 24 headlines in E3 (the materials were otherwise identical to Study 2). As in Studies 1 and 2, the headlines were selected (via pretest) to be either Pro-Democratic or Pro-Republican (and equally so). Moreover, the false and true news headlines were selected from the same superset that the Study 2 headlines came from, which means that the false news headlines were originally selected from a third-party fact-checking website, Snopes.com, and were therefore verified as being fabricated and untrue. Moreover, the true news headlines were all accurate and selected from mainstream news outlets to be roughly contemporary with the false news headlines. We selected headlines that would be relevant at the time the study was completed (November 28th-30th, 2017). The full survey is available online in both text format and as a Qualtrics file, along with all data (https://osf.io/p6u8k/).

Procedure

In both studies, participants were first asked if they have a Facebook account and those who did not were not permitted to complete the study. Participants were then randomly assigned to one of two conditions.

In the Control condition, participants were told: "You will be presented with a series of news headlines from 2016 and 2017 (24 in total). We are interested in whether you would be willing to share the story on Facebook. Note: The images may take a moment to load."

In the Treatment condition, participants were instead given the following instructions: "First, we would like to pretest an actual news headline from 2016 and 2017 for future studies. We are interested in whether people think it is accurate or not. We only need you to give your opinion about the accuracy of a single headline. We will then continue on to the primary task. Note: The image may take a moment to load." Participants were then shown a politically neutral headline and were asked: "To the best of your knowledge, how accurate is the claim in the above headline?" and were given the following response scale: "Not at all accurate, Not very accurate, Somewhat accurate, Very accurate." One of two headlines (1 true, 1 false) was randomly selected. (The actual accuracy rating that participants provided is not of importance to the current paper – rather, our goal was to investigate whether asking about accuracy selectively decreased the willingness to share false relative to true news; we therefore do not report the accuracy results, although the full data is available online, https://osf.io/p6u8k/). Following their accuracy judgment, participants in the Treatment were then given the same introductory blurb (described above) that was provided to participants in the Control.

Participants in both conditions then proceeded to the main task in which they were presented with the 24 headlines and for each were asked "If you were to see the above article on Facebook, how likely would you be to share it" and given the following response scale: "Extremely unlikely, Moderately unlikely, Slightly unlikely, Slightly likely, Moderately likely, Extremely likely". We used a continuous scale, instead of binary scale used in Study 1, to increase the sensitivity of the measure.

Analysis plan

Our preregistrations specified that all analyses would be performed at the level of the individual item (i.e., one data point per item per participant, with the 6-point sharing Likert scale rescaled to the interval [0,1]) using linear regression with robust standard errors clustered on participant. However, we subsequently realized that we should also be clustering standard errors on headline (as multiple ratings of the same headline are non-independent in a similar way to multiple ratings from the same participant), and thus deviated from the preregistrations in this minor way (all key results are qualitatively equivalent if only clustering standard errors on participant). For both studies, the key preregistered test was an interaction between a condition dummy (0 = Control, 1 = Treatment) and a news veracity dummy (0 = False, 1 = True). This is to be followed-up by tests for simple effects of news veracity in each of the two conditions; and, specifically, the effect was predicted to be larger in the Treatment condition. We also planned to test for simple effects of condition for each of the two types of news; and, specifically, the effect was predicted to be larger for false relative to true news.

In addition to these preregistered analyses, we also conducted a post-hoc analysis using a linear regression with robust standard errors clustered on participant and headline to examine the potential moderating role of a dummy for the participant's partisanship (preference for the Democratic versus Republican party) and a dummy for the headline's ideological concordance (Pro-Democratic [Pro-Republican] headlines scored as concordant for participants who preferred the Democratic [Republican] party; Pro-Republican [Pro-Democratic] headlines scored as discordant for participants who preferred the Democratic [Republican] party). For ease of interpretation, we z-scored the partisanship and concordance dummies, and then included all possible interactions in the regression model. To maximize statistical power for these moderation analyses, we pooled the data from Study 2 and Study 3.

Results

The average sharing intention values by condition and veracity are shown in Figure S4; note that whereas the main text figures for ease of interpretation discretize the sharing intention variable such that 0-3, the various "unlikely" responses are scored as 0 and 4-6, here we instead saw the means of the raw sharing intentions measure (as the raw sharing intentions measure is the DV in the regression models). The regression results are in shown in Table S2. The p-values associated with the various simple effects are shown in Table S3.

Whether considering only participants who indicated that they sometimes consider sharing political content (Cols 1 and 2) or considering all participants (Cols 4 and 5), we observed (i) the predicted significant positive interaction between condition (0 = Control, 1 = Treatment) and news veracity (0 = False, 1 = True), such that sharing discernment was higher in the Treatment compared to the Control; (ii) the predicted negative simple effect of condition for false headlines, such that participants were less likely to consider sharing false headlines in the Treatment compared to the Control; and (iii) no significant simple effect of condition for true headlines, such that participants were no less likely to consider sharing true headlines in the Treatment compared to the Control. We also find that these results are unaffected by controlling for participants' responses to the post-experiment questions regarding their own perceptions, and their friends' perceptions, of the importance of only sharing accurate content (Table S4).

Turning to potential moderation effects, we begin by examining the regression models in columns 3 and 6 of Table S2. We see that the Treatment has a significantly larger effect on sharing discernment for concordant headlines (significant positive 3-way Treatment X Veracity X Concordance interaction); but that this moderation effect is driven by Democrats more so than Republicans (significant negative 4-way Treatment × Veracity × Concordance × Party interaction). These effects are visualized in Figure S5. Interactions notwithstanding, sharing of false headlines was significantly lower in the Treatment than the control for every combination of participant partisanship and headline concordance ($p < .05$ for all), with the exception of Republicans sharing concordant headlines when including all participants ($p = .36$).

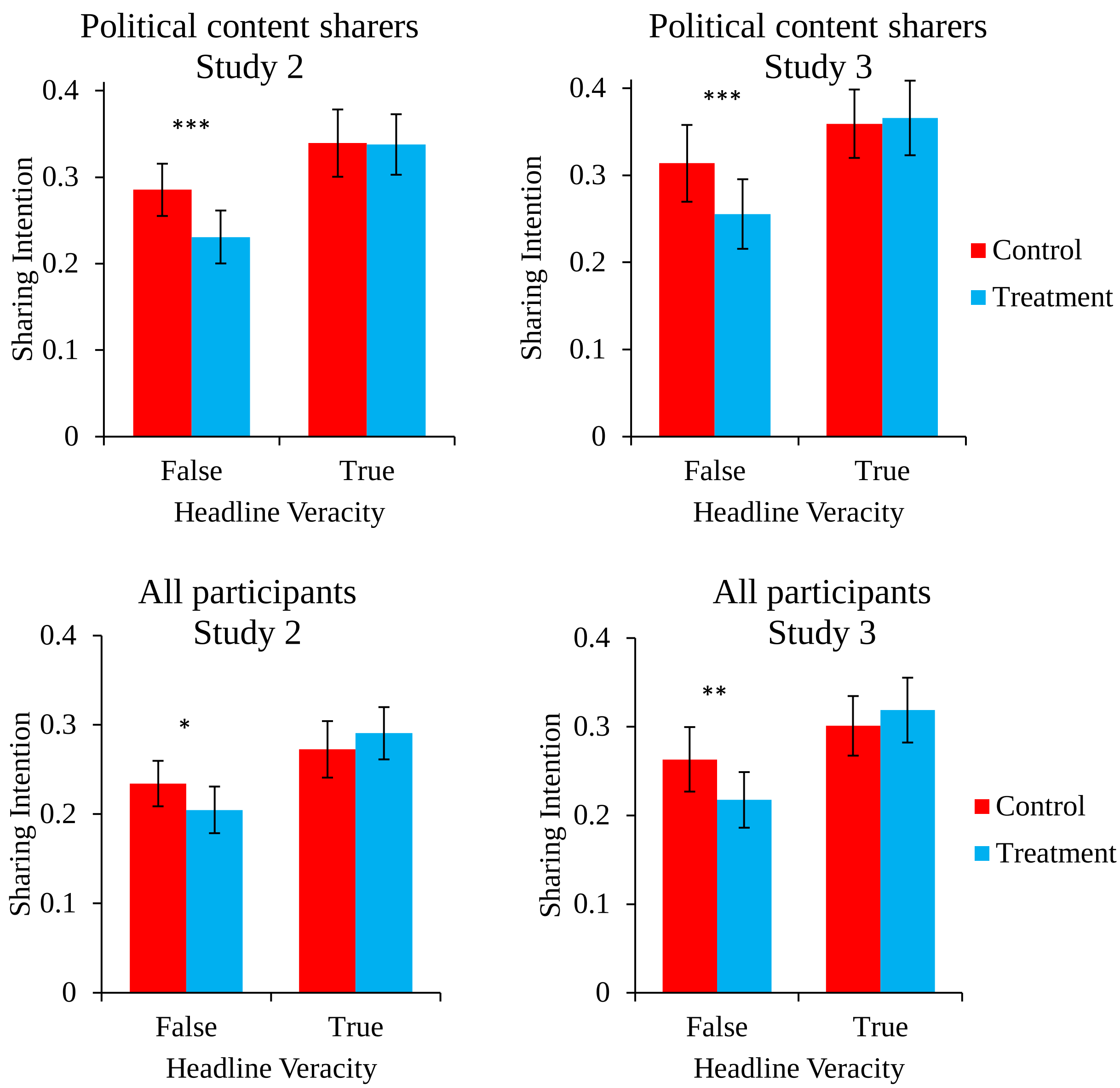

*Figure S4. Average values of the sharing intention variable by condition and news veracity for Studies 2 and 3. Top row considers only participants who indicated in the post-experimental questionnaire that they sometimes share political content on Facebook. Bottom row considers all participants. Error bars indicate 95% confident intervals generated used robust standard errors clustered on participant and headline. *p<.05, **p<.01, ***p<.001.*

	(1)	(2)	(3)	(4)	(5)	(6)
	Participants that share political content			All participants		
	S2	S3	S2+S3	S2	S3	S2+S3
Treatment	-0.0545***	-0.0582***	-0.0557***	-0.0294*	-0.0457***	-0.0372***
	(0.0145)	(0.0168)	(0.0110)	(0.0117)	(0.0139)	(0.00902)
Veracity (0=False, 1=True)	0.0540**	0.0455	0.0494**	0.0383*	0.0378	0.0380**
	(0.0205)	(0.0271)	(0.0161)	(0.0169)	(0.0225)	(0.0138)
Treatment × Veracity	0.0529***	0.0648***	0.0589***	0.0475***	0.0635***	0.0557***
	(0.0108)	(0.0147)	(0.00857)	(0.00818)	(0.0117)	(0.00681)
z-Party (Prefer Republicans to Democrats)			0.0169			0.00902
			(0.00939)			(0.00804)
Veracity × Party			0.00322			0.00249
			(0.00930)			(0.00792)
Treatment × Party			0.00508			0.0111
			(0.0106)			(0.00809)
Treatment × Veracity × Party			-0.0159			-0.0113*
			(0.00864)			(0.00573)
z-Concordance of Headline			0.0684***			0.0524***
			(0.00723)			(0.00625)
Veracity × Concordance			0.00351			0.00396
			(0.0107)			(0.00897)
Treatment × Concordance			-0.0156***			-0.00723*
			(0.00462)			(0.00315)
Treatment × Veracity × Concordance			0.0224***			0.0163***
			(0.00527)			(0.00290)
Party × Concordance			-0.00352			-0.00547
			(0.00928)			(0.00834)
Treatment × Party × Concordance			0.00725			0.00930*
			(0.00471)			(0.00440)
Veracity × Party × Concordance			0.0157			0.0159
			(0.0135)			(0.0123)
Treatment × Veracity × Party × Concordance			-0.0136**			-0.0132***
			(0.00448)			(0.00382)
Constant	0.285***	0.314***	0.300***	0.234***	0.263***	0.249***
	(0.0152)	(0.0221)	(0.0125)	(0.0128)	(0.0182)	(0.0106)
Observations	17,417	18,677	36,094	27,732	29,885	57,617
Participant clusters	727	780	1,507	1,158	1,248	2,406
Headline clusters	24	24	48	24	24	48
R-squared	0.019	0.016	0.063	0.012	0.014	0.045

Robust standard errors in parentheses
*** p<0.001, ** p<0.01, * p<0.05

Table S2. Linear regressions predicting sharing intentions (1-6 Likert scale rescaled to [0,1]) in Studies 2 and 3. Robust standard errors clustered on participant and headline.

Simple effect	Net coefficient	Participants that share political content		All participants	
		S2	S3	S2	S3
Treatment on false headlines	Treatment	0.0002	0.0005	0.0117	0.0010
Treatment on true headlines	Treatment+Treatment×Veracity	0.9185	0.6280	0.1535	0.1149
Veracity in Control	Veracity	0.0083	0.0934	0.0237	0.0935
Veracity in Treatment	Veracity+Treatment×Veracity	<.0001	0.0001	<.0001	<.0001

Table S3. P-values associated with the various simple effects from the regression models in Table S2.

	(1)	(2)	(3)	(4)	(5)	(6)
	Participants that share political content			All participants		
	S2	S3	S3	S2	S3	S3
Treatment	-0.0529***	-0.0574***	-0.0585***	-0.0267*	-0.0472***	-0.0458***
	(0.0139)	(0.0162)	(0.0167)	(0.0114)	(0.0136)	(0.0137)
Veracity (0=False, 1=True)	-0.0676	-0.140***	0.0376	-0.0495	-0.0940***	0.00756
	(0.0437)	(0.0413)	(0.0354)	(0.0269)	(0.0236)	(0.0239)
Treatment x Veracity	0.0523***	0.0653***	0.0647***	0.0452***	0.0654***	0.0656***
	(0.0106)	(0.0144)	(0.0147)	(0.00794)	(0.0116)	(0.0116)
AccuracyImportance (1-5)	-0.0573***	-0.0684***		-0.0304***	-0.0447***	
	(0.00874)	(0.00921)		(0.00602)	(0.00702)	
Treatment x AccuracyImportance	0.0286***	0.0426***		0.0215***	0.0306***	
	(0.00738)	(0.0116)		(0.00413)	(0.00716)	
FriendsAccuracyImportance (1-5)			0.0199**			0.0217***
			(0.00698)			(0.00532)
Treatment x FriendsAccuracyImportance			0.00277			0.00961*
			(0.00585)			(0.00430)
Constant	0.529***	0.609***	0.250***	0.359***	0.455***	0.196***
	(0.0398)	(0.0381)	(0.0300)	(0.0280)	(0.0293)	(0.0233)
Observations	17,393	18,533	18,439	27,684	29,645	29,527
R-squared	0.038	0.035	0.022	0.019	0.025	0.023
Standard errors in parentheses *** p<0.001, ** p<0.01, * p<0.05						

Table S4. Linear regressions predicting sharing intentions (1-6 Likert scale rescaled to [0,1]) in Studies 2 and 3 while controlling for participants' perceptions of the importance of only sharing accurate content. Robust standard errors clustered on participant and headline.

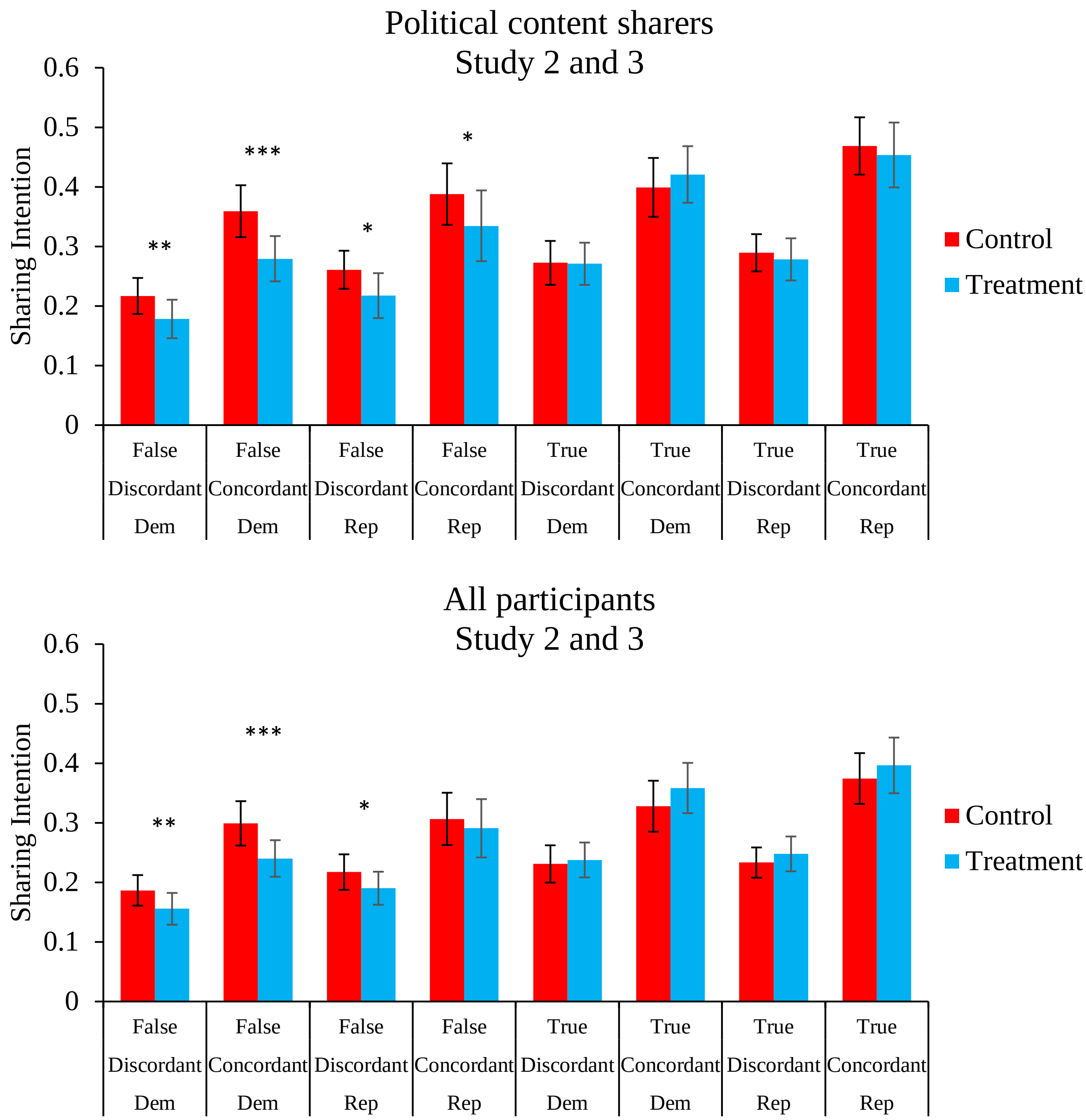

Figure S5. Average values of the sharing intention variable by condition, news veracity, headline concordance, and participant partisanship. Top panel considers only participants who indicated in the post-experimental questionnaire that they sometimes share political content on Facebook. Bottom panel considers all participants. Error bars indicate 95% confident intervals generated used robust standard errors clustered on participant and headline.

Study 4

Studies 2 and 3 found that a subtle reminder of the concept of accuracy decreased sharing of false (but not true) news. In Study 4, we build on these results in several ways. First, we added an active control condition for Study 4 where people were asked to rate the funniness (rather than accuracy) of the single headline at the outset of the study. This allows us to rule out the possibility that asking individuals to rate *any* aspect of an initial headline will increase attention and thus decrease false news sharing. Second, we tested an additional treatment condition which uses a different method to induce participants to think about accuracy: in this "accuracy importance" treatment, participants are asked out the outset of the study to indicate how important it is to only share accurate content on social media. Third, we tested whether the results would generalize beyond MTurk by recruiting participants from Lucid for Academics, delivering a sample that matches the distribution of American residents on age, gender, ethnicity, and geographic region. Our preregistration is available at https://osf.io/p6u8k/. This study was approved by the MIT COUHES (Protocol #1806400195).

Method

Participants

We preregistered a target sample of 1200 participants from Lucid. In total, 1628 participants began the survey. However, 236 participants reported not having a Facebook profile (and thus were not allowed to complete the survey) and 105 participants did not finish the survey. The full sample (Mean age = 45.5) included 626 males and 661 females. At the end of the survey, participants were asked if they "would ever consider sharing something political on Facebook" and were given the following response options: Yes, No, I don't use social media. As per our preregistration, our main analysis only includes participants who selected 'Yes' to this question. This excluded 616 people, such that the sample of participants who would consider sharing political content (Mean age = 44.3) included 333 males and 338 females. As in Study 2 and 3, there was no significant difference in responses to this political sharing question between conditions (χ^2 test; χ^2 (3, N = 1,287) = 2.320, p = .51), but again for completeness we show that all of our results are robust to including all participants.

Materials

We used yet another set of 20 different headlines for Study 4. As in our earlier studies, the headlines were selected (via pretest) to be either Pro-Democratic or Pro-Republican (and equally so). Moreover, the false and true news headlines were selected from among the same set, which means that the false news headlines were originally selected from a third-party fact-checking website, Snopes.com, and were therefore verified as being fabricated and untrue. Moreover, the true news headlines were all accurate and selected from mainstream news outlets to be roughly contemporary with the false news headlines. We selected headlines that would be relevant at the time the study was completed (April 30th-May 2nd, 2019). As in our earlier studies, after the main task participants were asked about the importance of only sharing accurate news articles on social media (except in the Importance Treatment, where this item was completed at the outset of the study) and about their frequency of social media use, along with several exploratory questions about media trust. At the end of the survey, participants were asked if they responded randomly at any point during the survey or searched for any of the headlines online (e.g., via Google). Participants also completed the 7-item Cognitive Reflection Test (Pennycook

& Rand, 2019). Finally, participants were asked several demographic questions (age, gender, education, income, and a set of political and religious questions). The most central political partisanship question was “If you absolutely had to choose between only the Democratic and Republican party, which would do you prefer?” followed by the following response options: Democratic Party, Republican Party. The full survey is available online in both text format and as a Qualtrics file, along with all data (https://osf.io/p6u8k/).

Procedure

Participants were first asked if they have a Facebook account and those who did not were not permitted to complete the study. Participants were then randomly assigned to one of four conditions. In the (passive) Control condition, participants were simply told: “You will be presented with a series of news headlines from 2017 and 2018 (24 in total). We are interested in whether you would be willing to share the story on Facebook. Note: The images may take a moment to load.” In the Active Control condition, participants were told: “First, we would like to pretest an actual news headline for future studies. We are interested in whether people think it is funny or not. We only need you to give your opinion about the funniness of a single headline. We will then continue on to the primary task. Note: The image may take a moment to load.” They were then presented with one of four neutral news headlines (as in Study 2) and asked: “In your opinion, is the above headline funny, amusing, or entertaining? (response options: Extremely unfunny, moderately unfunny, slightly unfunny, slightly funny, moderately funny, extremely funny). In the Treatment condition, participants were given the following instructions: “First, we would like to pretest an actual news headline for future studies. We are interested in whether people think it is accurate or not. We only need you to give your opinion about the accuracy of a single headline. We will then continue on to the primary task. Note: The image may take a moment to load.” Participants in the Treatment condition were then given one of four neutral news headlines and asked to rate how accurate they believed it to be (as in previous studies). In the Importance Treatment condition, participants were asked the following question at the outset of the study: “Do you agree or disagree that ‘it is important to only share news content on social media that is accurate and unbiased’?” (response options: strongly agree to strongly disagree). Participants in all conditions were then presented with the 20 true and false news headlines and indicated their willingness to share them on social media (as in previous studies).

Analysis plan

As in Studies 2 and 3, our preregistration indicated that we use analyses performed at the level of the individual item (i.e., one data point per item per subject) using linear regression with robust standard errors clustered on subject; but, for the reason given above, we deviate from that plan by also clustering standard errors on headline. We first testing whether the active and passive control conditions differ by testing for main effect or interaction between condition (0=passive, 1=active) and news veracity (0=fake, 1=real). If these did not differ, we preregistered that we would combine the two control conditions for subsequent analyses. We then tested whether the two treatment conditions differ from the control condition(s) by testing for an interaction between dummies for each treatment (0=passive or active control, 1=treatment being tested) and a news veracity (0=fake, 1=real). This is to be followed-up by tests for simple effects of news veracity in each of the conditions; and, specifically, the effect was predicted to be larger in the treatment conditions. We also planned to test for simple effects of condition for each of the two types of news; and, specifically, the effect was predicted to be larger for false relative to true news.

Results

The average sharing intention values by condition and veracity are shown in Figure S6; note that whereas the main text figures for ease of interpretation discretize the sharing intention variable such that 0-3, the various “unlikely” responses are scored as 0 and 4-6, here we instead saw the means of the raw sharing intentions measure (as the raw sharing intentions measure is the DV in the regression models). The regression results are in shown in Table S5. The p-values associated with the various simple effects are shown in Table S6.

We begin by comparing the passive and active controls. Whether considering only participants who indicated that they sometimes consider sharing political content (Col 1) or considering all participants (Col 3), we see no significant simple effect of condition (0 = Passive Control, 1 = Active Control) or interaction between condition and news veracity (0= False, 1 = True). Therefore, as per our preregistered analysis plan, we collapse across control conditions for our main analysis.

We next consider the impact of our main Treatment. Whether considering only participants who indicated that they sometimes consider sharing political content (Col 2) or considering all participants (Col 4), we observed (i) the predicted significant positive interaction between Treatment and news veracity, such that sharing discernment was higher in the Treatment compared to the controls; (ii) the predicted negative simple effect of Treatment for false headlines, such that participants were less likely to consider sharing false headlines in the Treatment compared to the controls; and (iii) no significant simple effect of Treatment for true headlines, such that participants were no less likely to consider sharing true headlines in the Treatment compared to the controls. (Equivalent results are observed if comparing the Treatment only to the Active control.)

Finally, we consider our alternative Importance Treatment. Whether considering only participants who indicated that they sometimes consider sharing political content (Col 2) or considering all participants (Col 4), we observed the predicted significant positive interaction between Importance Treatment and news veracity, such that sharing discernment was higher in the Importance Treatment compared to the controls. However, the negative simple effect of Importance Treatment for false headlines was only marginally significant when considering sharer participants and non-significant when considering all participants; and the simple effect of Importance Treatment for true headlines was non-significant in both cases.

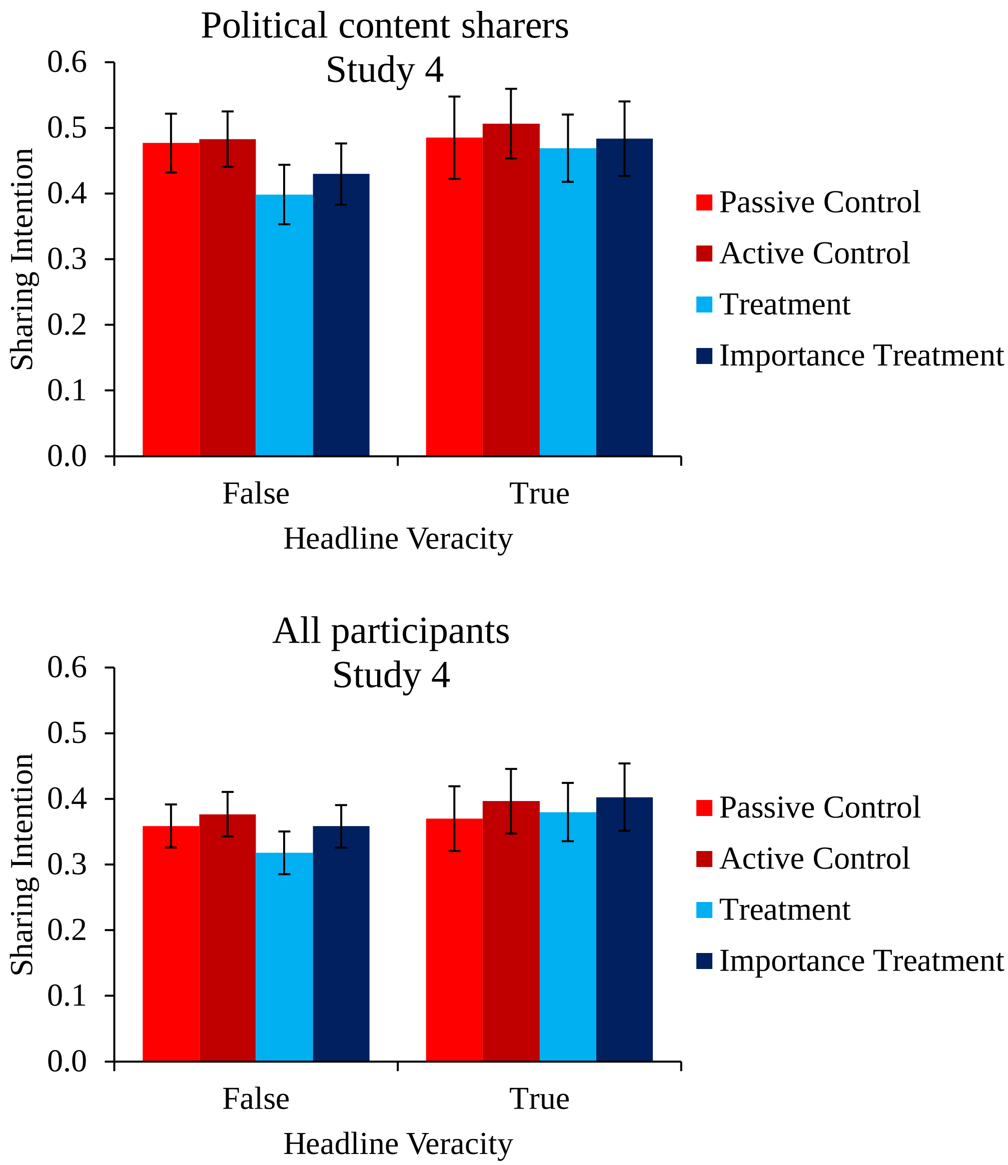

Figure S6. Average values of the sharing intention variable by condition and news veracity for Study 4. Top row considers only participants who indicated in the post-experimental questionnaire that they sometimes share political content on Facebook. Bottom row considers all participants. Error bars indicate 95% confident intervals generated used robust standard errors clustered on participant and headline.

	(1)	(2)	(3)	(4)
	Participants that share political content		All participants	
	Controls only	All conditions	Controls only	All conditions
Veracity (0=False, 1=True)	0.00812	0.0163	0.0111	0.0154
	(0.0262)	(0.0234)	(0.0206)	(0.0212)
Active Control	0.00606		0.0179	
	(0.0303)		(0.0223)	
Active Control X Veracity	0.0155		0.00856	
	(0.0120)		(0.00660)	
Treatment		-0.0815**		-0.0500**
		(0.0261)		(0.0185)
Treatment X Veracity		0.0542***		0.0466***
		(0.0157)		(0.00914)
Importance Treatment		-0.0504		-0.00966
		(0.0274)		(0.0193)
Importance Treatment X Veracity		0.0376**		0.0291***
		(0.0120)		(0.00634)
Constant	0.477***	0.480***	0.359***	0.368***
	(0.0227)	(0.0160)	(0.0166)	(0.0127)
Observations	6,776	13,340	12,847	25,587
Participant clusters	341	671	646	1286
Headline clusters	20	20	20	20
R-squared	0.001	0.007	0.001	0.004
Standard errors in parentheses				
*** *p*<0.001, ** *p*<0.01, * *p*<0.05				

Table S5. Linear regressions predicting sharing intentions (1-6 Likert scale rescaled to [0,1]) in Study 4. Robust standard errors clustered on participant and headline.

Simple effect	Net coefficient	Participants that share political content	All participants
Treatment on false headlines	Treatment	0.0018	0.0068
Treatment on true headlines	Treatment+Treatment×Veracity	0.2411	0.8473
Importance Treatment on false headlines	Importance Treatment	0.0660	0.6166
Importance Treatment on true headlines	ImportanceTreatment+ImportanceTreatment×Veracity	0.5883	0.2700
Veracity in Controls	Veracity	0.4860	0.4665
Veracity in Treatment	Veracity+Treatment×Veracity	0.0032	0.0027
Veracity in Importance Treatment	Veracity+ImportanceTreatment×Veracity	0.0242	0.0470

Table S6. P-values associated with the various simple effects from the regression models in Table S5.

Study 5

In Study 5 we set out to test whether the results of the survey experiments in Studies 2 through 4 would generalize to real sharing decisions "in the wild", and to misleading but not blatantly false news. Thus, we conducted a digital field experiment on Twitter in which we delivered the same intervention from the Treatment condition of the survey experiments to users who had previously shared links to unreliable news sites. We then examined the impact of receiving the intervention on the quality of the news they subsequently shared. The experiment was approved by Yale University Committee of the Use of Human Subjects IRB protocol #2000022539 and MIT COUHES Protocol #1806393160. All analysis code is posted online at https://osf.io/p6u8k/. We did not publicly post the data due to privacy concerns (even with de-identified data, it is likely possible to back out which Twitter user corresponds with many of the users in the dataset). Researchers interested in accessing the data are asked to contact the corresponding author.

Method

Study 5 is an aggregation of three different waves of data collection, the details of which are summarized in Table S7. (These are all of the data we collected; nothing was left "in the file drawer".)

Wave	Date Range	Treatment Time	Treatment Days	Bots	Users Followed	Follow - backs	Qualified Users	DMs sent	Rated tweets analyzed	Total tweets analyzed
1	4/20/2018-4/28/2018	7:43pm EST	7 (no 4/25)	6	19,913	821	808	808	19,354	1,291,445
2	9/10/2018-9/13/2018	5:00pm EST	3	7	23,673	3,111	2,153	879	30,140	7,492,534
3	1/10/2019-1/22/2019	7:00pm EST	12	13	92,793	7,432	2,521	2,520	27,780	2,381,713
Total			22	13	136,379	11,364	5,482	4,550	77,274	11,165,692

Table S7. Details for the three waves of Study 5 data collection.

Participants

The basic experimental design involved sending a private direct message (DM) to users asking them to rate the accuracy of a headline (as in the Treatment condition of the survey experiments). Twitter only allows DMs to be sent from account X to account Y if account Y follows account X. Thus, our first task was to assemble a set of accounts with a substantial number of followers (who we could then send DMs to). In particular, we needed followers who were likely to share misinformation. Our approach was as follows.

First, we created a list of tweets with links to one of two news sites that professional fact-checkers rated as extremely untrustworthy (Pennycook & Rand 2019) but that are nonetheless fairly popular: Breitbart.com and infowars.com. We identified these tweets by (i) retrieving the timeline of the Breitbart Twitter account using the Twitter REST API (Infowars has been banned from Twitter and thus has no Twitter account) and (ii) searching for tweets containing a link to the corresponding domain using the Twitter advanced search feature and either collecting the tweet

IDs manually (wave 1) or via scraping (waves 2 and 3). Next, we used the Twitter API to retrieve lists of users who retweeted each of those tweets (we periodically fetched the list of "retweeters" since the Twitter API only provides the last 100 users "retweeters" of a given tweet). As shown in Table S7, across the three waves this process yielded a *potential participant list* of 136,379 total Twitter users with some history of retweeting links to misleading news sites.

Next, we created a series of accounts with innocuous names (for an example, see Figure S7); we created new accounts for each experimental wave. Each of the users in the potential participant list was then randomly assigned to be followed by one of our accounts. We relied on the tendency of Twitter users to reciprocally follow-back to create our set of followers. Indeed, 8.3% of the users that were followed by one of our accounts chose to follow our account back. This yielded a total 11,364 followers across the three waves.

Figure S7. One of the accounts used to deliver our intervention via direct message.

To determine eligibility and to allow blocked randomization, we then identified (i) users' political ideology using the algorithm from Barberá, Jost, Nagler, Tucker, and Bonneau (2015); (ii) their probability of being a bot, using the bot-or-not algorithm (Davis, Varol, Ferrara, Flammini, & Menczer, 2016); (iii) the number of tweets to one of the 60 websites with fact-checker ratings that will form our quality measure; and (iv) the average fact-checker rating (quality score) across those tweets.

For waves 1 and 2, we excluded users who tweeted no links to any of the 60 sites in our list in the two weeks prior to the experiment; who could not be given an ideology score; who could not be given a bot score; or who had a bot score above 0.5. In wave 3, we took a different approach to avoiding bots, namely avoiding high-frequency tweeters. Specifically, we excluded participants who tweeted more than 30 links to one of the 60 sites in our list in the two weeks prior to the experiment, as well as excluding those who tweeted less than 5 links to one of the 60 sites (to avoid lack of signal). This resulted in a total of 5,482 unique Twitter users across the three waves. (Note that these exclusions were applied ex ante, and excluded users were not included in the experiment, rather than implementing post hoc exclusions.)

Materials & Procedure

The treatment in Study 5 was very similar to the survey experiments: users were sent a DM asking them to rate the accuracy of a single non-political headline (see Figure S8). Because of DM rate limits imposed by Twitter, we could only send DMs to roughly 20 users per account per day.

Thus, we conducted each wave in a series of 24-hour blocks in which a small subset of users was DM'd on each day. All tweets and retweets posted by all users in the experiment were collected on each day of the experiment. All links in these tweets were extracted (including expanding shortened URLs). The dataset was then composed of the subset of these links that linked to one of the 60 sites with fact-checker ratings (with the data entry being the quality score of the linked site).

To allow for causal inference, we used a randomized roll-out (also called stepped-wedge) design in which users were randomly assigned to a treatment date. This allows us to analyze all tweets made during all of the 24-hour treatment blocks, comparing tweets from users who received the DM at the start of a given block (Treated) to tweets from users who had not yet been DM'd (Control). Because treatment date is randomly assigned, it can be inferred that any systematic difference revealed by this comparison was caused by the treatment. (Wave 2 also included a subset of users who were randomly assigned to never receive the DM.) To improve the precision of our estimate, random assignment to treatment date was balanced across bot accounts in all waves, and across political ideology, number of tweets to rated sites in the two weeks before the experiment, and average quality of those tweets across treatment dates in waves 2 and 3 (the code used to assign users to treatment dates is available at https://osf.io/p6u8k/).

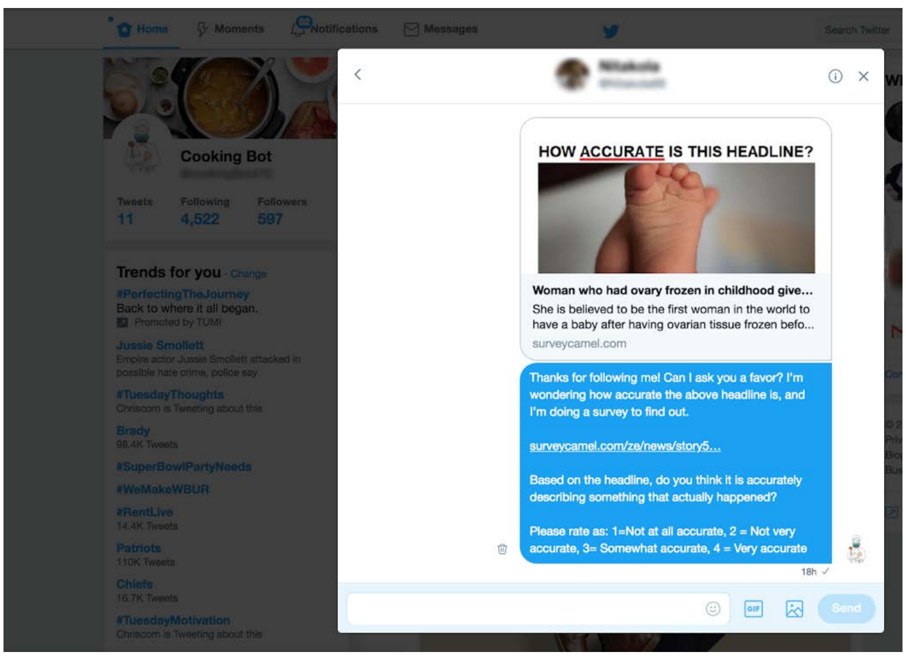

Figure S8. The message sent to users to induce them to think about accuracy. Note that in wave 1 of the experiment, some participants received a message with slightly different wording.

Because our treatment was delivered via the Twitter API, we were vulnerable to unpredictable changes to, and unstated rules of, the API. These gave rise to several deviations from our planned procedure. On day 2 of wave 1, fewer than planned DMs were sent as our accounts were blocked part way thru the day; and no DMs were sent on day 3 of wave 1 (hence, that day is not included in the experimental dataset). On day 2 of wave 2, Twitter disabled the DM feature of the API for the day, so we were unable to send the DMs in an automated fashion as planned. Instead, all 370 DMs sent on that day were sent manually over the course of several hours (rather than simultaneously). On day 3 of wave 2, the API was once again functional, but partway through sending the DMs, the credentials for our accounts were revoked and no further DMs were sent. As a result, only 184 of the planned 369 DMs were sent on that day. Furthermore, because we did not randomize the order of users across stratification blocks, the users on day 3 who were not DM'd were systematically different from those who were DM'd. (As discussed in detail below, we consider analyses that use an intent-to-treat approach for wave 2 day 3 – treating the data as if all 369 DMs had indeed been sent – as well as analyses that exclude the data from wave 2 day 3.)

Ethics of Digital Field Experimentation

Field experimentation necessarily involves engaging in people's natural activities to assess the effect of a treatment *in situ*. As digital experimentation on social media becomes more attractive to social scientists, there are increasing ethical considerations that must be taken into account (Desposato, 2015; Gallego, Martínez, Munger, & Vásquez-Cortés, 2019; Taylor & Eckles, 2018).

One such consideration is the nature of the interaction between Twitter users and our bot accounts. As discussed above, this involved following individuals who shared links to misinformation sites, and then sending a DM to those individuals who followed our bot accounts back. This procedure complies with Twitter's Terms of Service, which not only allow publicly visible accounts to be followed (within API rate limits) but also allow accounts to be DM'ed if they follow the account that sends the DM. Furthermore, we believe that the potential harm of an account following and sending a DM to an individual are minimal; and that the potential benefits of scientific understanding and an increase in shared news quality outweigh that negligible risk. Both the Yale University Committee for the Use of Human Subjects (IRB protocol #2000022539) and the MIT COUHES (Protocol #1806393160) agreed with our assessment. With regard to informed consent, it is standard practice in field experiments to eschew informed consent because much of the value of field experiments comes from participants not knowing they are in an experiment (thus providing ecological validity). As obtaining informed consent would disrupt the user's normal experience using Twitter, and greatly reduce the validity of the design – and the risks were minimal – both institutional review boards waived the need for informed consent. A final consideration is the ethical collection of individuals tweet histories for analysis. Since we are only considering publicly available tweets, and hence any collated dataset would be the product of secondary research, we believe this to be an acceptable practice.

There is the open question of how these considerations interact, and if practices that are separately appropriate can create ethically ambiguous situations when conducted conjointly. Data rights on social media are a complicated and ever-changing social issue with no clear answers. We hope Study 5 highlights some principles and frameworks for considering these issues in the context of digital experimentation, and helps create more discussion and future work on concretely establishing norms of engagement.

While we believe this intervention is ethically sound, we also acknowledge the fact that if this methodology was universalized as a new standard for social science research, it could further dilute and destabilize the Twitter ecosystem, which already suffers from fake accounts, spam, and misinformation. Future work should invest in new frameworks for digital experimentation that maintains social media's standing as a town square for communities to genuinely engage in communication, while also allowing researches to causally understand user behavior on the platform. These frameworks may involve, for example, external software libraries built on top of publicly available APIs, or explicit partnerships with the social media companies themselves.

Analysis plan

As the experimental design and the data were substantially more complex than the survey experiment studies and we lacked well-established models to follow, it was not straightforward to determine the optimal way to analyze the data in Study 5. This is reflected, for example, in the fact that wave 1 was not preregistered, two different preregistrations were submitted for wave 2 (one prior to data collection and one following data collection but prior to analyzing the data), and one preregistration was submitted for wave 3 (see https://osf.io/p6u8k/ for all preregistrations), and each of the preregistrations stipulated a different analysis plan. Moreover, after completing all three waves, we realized that all of the analyses proposed in the preregistrations do not actually yield valid causal inferences because of issues involving missing data (as discussed in more detail below in the "Dependent variable" section). Therefore, instead of conducting a particular preregistered analysis, we consider the pattern of results across a range of reasonable analyses.

All analyses are conducted at the user–day level using linear regression with heteroscedasticity-robust standard errors clustered on user. All analyses include all users on a given day who have not yet received the DM as well as users who received the DM on that day (users who received the DM more than 24 hours before the given day are not included). All analyses use a post-treatment dummy (0=user has not yet been DMed, 1=user received the DM that day) as the key independent variable. We note that this is an intent-to-treat approach that assumes that all DMs on a given day are sent at exactly the same time, and counts all tweets in the subsequent 24-hour block as post-DM. Thus, to the extent that technical issues caused tweets on a given day to be sent somewhat earlier or later than the specified time, this approach may somewhat underestimate the treatment effect.

The analyses we consider differ in the following ways: dependent variable, model specification, type of tweets considered, approach to handling randomization failure, and approach to determining statistical significance. We now discuss each of these dimensions in more detail.

1. Dependent variable: We consider three different ways of quantifying tweet quality. Across approaches, a key issue is how to deal with missing data. Specifically, on days when a given user does not tweet any links to rated sites, the quality of their tweeted links is undefined. The approach implied in our preregistrations was to simply omit missing user–days (or to conduct analyses at the level of the tweet). Because the treatment is expected to influence the probability of tweeting, however, omitting missing user–days has the potential to create selection and thus undermine causal inference (and tweet-level analyses are even more problematic). For example, if a user tweets as a result of being treated but would not have tweeted had they been in the control (or does not tweet as a result of treatment but would have tweeted have they been in the control), then omitting the missing user–days breaks the independence between treatment and potential outcomes ensured by random assignment. Given that only 47.0% of user-days contained at least one tweeted link

to a rated site, such issues are potentially quite problematic. We therefore consider three approaches to tweet quality that avoid this missing data problem.

The first measure is the *average relative quality score*. This measure assigns each tweeted link a relative quality score by taking Pennycook & Rand 2019's fact-checker trust rating (quality score, [0,1]) of the domain being linked to, and subtracting the baseline quality score of 0.34 (this corresponds to the average quality score of all pre-treatment tweets across all users in all of the experimental days if Study 5). Each user–day is then assigned an average relative quality score by averaging the relative quality score of all tweets made by the user in question on the day in question; and users who did not tweet on a given day are assigned an average relative quality score of 0 (thus avoiding the missing data problem). The average relative quality score is thus defined over the interval [-0.34, 0.66]. Importantly, this measure is quite *conservative* because the (roughly half of) post-treatment user–days where data is missing are scored as 0s. Thus, this measure assumes that the treatment had no effect on users who did not tweet on the treatment day. If, instead, non-tweeting users would have shown the same effect had they actually tweeted, the estimated effect size would be roughly twice as large as what we observe here. We note that this measure is equivalent to using average quality scores (rather than relative quality score) and imputing the baseline quality score to fill missing data (so assuming that on missing days, the user's behavior matches the subject pool average).

The second measure is the *summed relative quality score*. This measure assigns each tweeted link a relative quality score in the same manner described above. A given user–day's summed relative quality score is then 0 plus the sum of the relative quality scores of each link tweeted by that user on that day. Thus, the summed relative quality score increases as a user tweets more and higher quality links, and decreases as the user tweets more and lower quality links; and, as for the average relative quality score, users who tweet no rated links received a score of 0. As this measure is unbounded in both the positive and negative directions, and the distribution contains extreme values in both directions, we winsorize summed relative quality scores by replacing values above the 95th percentile with the 95th percentile, and replacing values below the 5th percentile with values below the 5th percentile (our results are qualitatively robust to alternative choices of threshold at which to winsorize).

The third measure is discernment, or the difference in the number of links to mainstream sites versus misinformation sites shared on a given user–day. This measure is mostly closely analogous to the analytic approach taken in Studies 2-4. To assess the impact of the intervention on discernment, we transform the data into long format such that there are two observations per user–day, one indicating the number of tweets to mainstream sites and the other indicating the number of tweets to misinformation sites (as defined in Pennycook & Rand, 2019). We then include a source type dummy (0=misinformation, 1=mainstream) in the regression, and interact the this dummy with each independent variable. The treatment increases discernment if there is a significant positive interaction between the post-treatment dummy and the source type dummy. As these count measures are unbounded in the positive direction, and the distributions contain extreme values, we winsorize by replacing values above the 95th percentile of all values with the 95th percentile of all values (our results are qualitatively robust to alternative choices of threshold at which to winsorize).

Finally, as a control analysis, we also consider the treatment effect on the number of tweets in each user–day that did not contain links to any of the 60 rated news sites. As this count measure is unbounded in the positive direction, and the distribution contains extreme values, we winsorize by replacing values above the 95^{th} percentile of all values with the 95^{th} percentile of all values (our results are qualitatively robust to alternative choices of threshold at which to winsorize).

2. Model specification: We consider four different model specifications. The first includes wave dummies. The second post-stratifies on wave by interacting centered wave dummies with the post-treatment dummy. This specification also allows us to assess whether any observed treatment effect significantly differs across waves by performing a joint significance test on the interaction terms. The third includes date dummies. The fourth post-stratifies on date by interacting centered date dummies with the post-treatment dummy.

3. Tweet type: The analysis can include all tweets, or can focus only on cases where the user retweets the tweet containing the link without adding any comment. The former approach is more inclusive, but may contain cases in which the user is not endorsing the shared link (e.g., someone debunking an incorrect story may still link to the original story). Thus, the latter case might more clearly identify tweets that are uncritically sharing the link in question.

4. Approach to randomization failure: As described above, due to issues with the Twitter API on day 3 of wave 2, there was a partial randomization failure on that day (many of the users assigned to treatment received no DM). We consider two different ways of dealing with this randomization failure. In the intent-to-treat approach, we include all users from the randomization-failure day (with the post-treatment dummy taking on the value 1 for all users who were assigned to be DMed on that day, regardless of whether they actually received a DM). In the exclusion approach, we instead drop all data from that day.

5. Determining statistical significance: We consider the results of two different methods for computing p-values for each model. The first is the standard regression approach, in which robust standard errors clustered on user are used to calculate p-values. The second employs Fisherian Randomization Inference (FRI) to compute a p-value that is exact (i.e., has no more than the nominal Type I error rate) in finite samples (Fisher, 1937; Imbens & Rubin, 2015; Rosenbaum, 2002; Rubin, 1980). FRI is non-parametric and thus does not require any modeling assumptions about potential outcomes. Rather, the stochastic assignment mechanism determined by permuting the treatment schedule determines the distribution of the test statistic (Imbens & Rubin, 2015). Based on our stepped-wedge design, our treatment corresponds to the day on which the user receives the DM. Thus, to perform FRI, we create 20,000 permutations of the assigned treatment day for each user by re-running the random assignment procedure used in each wave, and recompute the t-statistic for the coefficient of interest in each model in each permutation. We then determine p-values for

each model by computing the fraction of permutations that yielded t-statistics with absolute value larger than the t-statistic observed in the actual data. Note that therefore, FRI takes into account the details of the randomization procedure that balanced treatment date across bots in all waves, and across ideology, tweet frequency, and tweet quality in waves 2 and 3.

Results

The p-values for each of analyses described above are shown in Table S8. Taken together, the results support the conclusion that the treatment significantly increased the quality of news shared. For the average relative quality score, virtually all (31 out of 32) analyses found a significant effect. For the summed relative quality score, most analyses found a significant effect, except for the FRI-derived p-values when including all tweets which were all non-significant. Similarly, for discernment, most analyses found a significant effect, except for most of the FRI-derived p-values when including all tweets. That the analyses using these two variables were somewhat less stable than the analyses using the average relative quality score variable is perhaps not that surprising, given that summed relative quality score and discernment are both unbounded. Reassuringly, the results for all dependent variables and models were significant when only considering retweets without comment (which more clearly indicate endorsement of the link, rather than negations); and there was little qualitative difference between the two approaches for handling randomization failure, or across the four model specifications. Finally, when considering tweets that did not contain links to any of the rated news sites, we see no evidence of a treatment effect.

Tweet Type	Randomization Failure	Model Spec	Average Relative Quality			Summed Relative Quality			Discernment			Tweets without links to rated domains		
			Coeff	Reg *p*	FRI *p*	Coeff	Reg *p*	FRI *p*	Coeff	Reg *p*	FRI *p*	Coeff	Reg *p*	FRI *p*
All	Exclude	Wave FE	0.007	**0.009**	**0.027**	0.012	**0.007**	0.106	0.053	**0.006**	0.061	0.213	0.758	0.729
All	Exclude	Wave PS	0.007	**0.012**	**0.028**	0.011	**0.009**	0.105	0.051	**0.007**	0.069	0.150	0.818	0.808
All	Exclude	Date FE	0.005	**0.049**	**0.043**	0.009	**0.048**	0.154	0.042	**0.038**	0.093	-0.279	0.729	0.644
All	Exclude	Date PS	0.005	0.061	**0.035**	0.009	0.075	0.122	0.042	0.051	0.100	-0.083	0.917	0.883
All	ITT	Wave FE	0.007	**0.005**	**0.010**	0.010	**0.020**	0.122	0.052	**0.006**	**0.036**	0.476	0.486	0.387
All	ITT	Wave PS	0.007	**0.006**	**0.011**	0.010	**0.019**	0.112	0.050	**0.006**	**0.046**	0.358	0.574	0.510
All	ITT	Date FE	0.006	**0.021**	**0.015**	0.008	0.071	0.178	0.044	**0.027**	0.058	0.103	0.896	0.862
All	ITT	Date PS	0.006	**0.035**	**0.017**	0.008	0.088	0.130	0.043	**0.039**	0.070	0.160	0.836	0.776
RT only	Exclude	Wave FE	0.007	**0.005**	**0.004**	0.013	**0.002**	**0.012**	0.057	**0.001**	**0.005**	0.125	0.805	0.751
RT only	Exclude	Wave PS	0.007	**0.006**	**0.004**	0.013	**0.002**	**0.012**	0.055	**0.001**	**0.006**	0.093	0.844	0.815
RT only	Exclude	Date FE	0.005	**0.040**	**0.011**	0.010	**0.020**	**0.015**	0.047	**0.012**	**0.007**	-0.199	0.737	0.667
RT only	Exclude	Date PS	0.006	**0.047**	**0.011**	0.010	**0.037**	**0.014**	0.046	**0.022**	**0.015**	-0.128	0.825	0.743
RT only	ITT	Wave FE	0.007	**0.003**	**0.002**	0.011	**0.007**	**0.021**	0.056	**0.001**	**0.004**	0.301	0.549	0.426
RT only	ITT	Wave PS	0.007	**0.004**	**0.002**	0.011	**0.006**	**0.017**	0.054	**0.001**	**0.004**	0.227	0.624	0.535
RT only	ITT	Date FE	0.006	**0.021**	**0.005**	0.009	**0.035**	**0.028**	0.047	**0.009**	**0.005**	0.053	0.927	0.910
RT only	ITT	Date PS	0.006	**0.030**	**0.006**	0.009	**0.046**	**0.019**	0.046	**0.017**	**0.012**	0.035	0.951	0.930

Table S8. P-values associated with each model for Study 5. In the model specification column, FE represents fixed effects (i.e. just dummies) and PS represents post-stratification (i.e. centered dummies interacted with the post-treatment dummy). For Discernment, the p-value associated with the interaction between the post-treatment dummy and the source type dummy is reported;

for all other DVs, the p-value associated with the post-treatment dummy is reported. P-values below 0.05 are bolded. The model presented in the main text is shown on the first row, regression p-value columns.

The analyses presented in Table S8 collapse across waves to maximize statistical power. As evidence that this aggregation is justified, we examine the models in which the treatment effect is post-stratified on wave (i.e. the wave dummies are interacted with the post-treatment dummy). Table S9 shows the p-values generated by a joint significance test over the wave-post-treatment interactions (i.e. testing whether the treatment effect differed significantly in size across waves) for the four dependent variables crossed with the four possible inclusion criteria choices. As can be seen, in all cases the joint significance test is extremely far from significant. This lack of significant interaction between treatment and wave supports our decision to aggregate the data across waves.

Tweet Type	Randomization-Failure	Average Relative Quality	Summed Relative Quality	Discernment	Tweets without rated links
All	Exclude	0.805	0.413	0.778	0.773
All	ITT	0.8645	0.342	0.803	0.499
RT w/o comment	Exclude	0.866	0.591	0.637	0.727
RT w/o comment	ITT	0.876	0.625	0.760	0.485

Table S9. P-values generated by a joint significant test of the interaction between wave2 and post-treatment and wave3 and post-treatment, from the models in Table S7 where treatment effect is post-stratified on wave.

Finally, in Figure S9 we show the results of domain-level analyses. These analyses compute the fraction of pre-treatment rated links that link to each of the 60 rated domains, and the fraction of rated links in the 24 hours post-treatment that link to each of the 60 rated domains. For each domain, we then plot the difference between these two fractions on the y-axis, and the fact-checker trust rating from Pennycook & Rand (2019) on the x-axis.

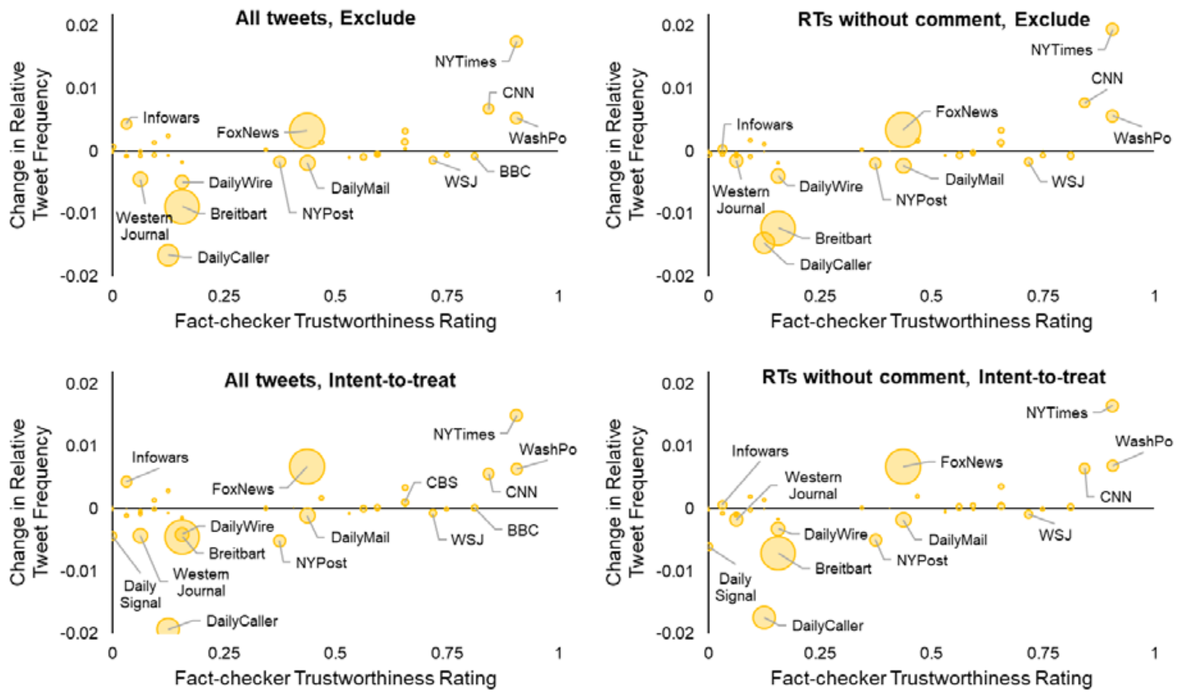

Figure S9. Domain-level analysis for each combination of approach to randomization failure (exclusion or intent-to-treat) and tweet type (all or only RTs without comment). Size of dots are proportional to pre-treatment tweet count. Outlets with at least 500 pre-treatment tweets are labeled.

Limitations

While the methodology and analysis plan discussed above provide a promising new framework for digital field experimentation, there are many limitations. First, our analysis involves an intent-to-treat (ITT) setup, since do not know when the user actually saw the DM treatment. Thus we must use the time we sent the DM as a proxy for exposure to it. Second, we assume 24-hour time blocks for our analyses. We do this since the DM schedule messaged individuals every 24 hours, which makes 24 hours a natural temporal resolution. In addition, given the ITT assumption stated above, we wanted a time window that not only was short enough to capture quick changes in behavior, but was also long enough to give people adequate time to see the message in that time block. Future work could use a more continuous shock-based model of how (and when) the treatment effects individual. Third, to comply with Twitter policies, we only direct messaged users who selected into the experiment by following back the account. This procedure introduces sample selection bias for the participant pool, and thus restricts our causal claims only to this specific subset of users. Future work could involve generalizing this sample average treatment effect to a population average treatment effect using such methods as post-stratification or weighting (Franco, Malhotra, Simonovits, & Zigerell, 2017).

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