The Demand for News: Accuracy Concerns versus Belief Confirmation Motives

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May 13, 2022

Abstract

We examine the relative importance of accuracy concerns and belief confirmation motives in driving the demand for news. In experiments with US respondents, we first vary beliefs about whether an outlet reports the news in a right-wing biased, left-wing biased, or unbiased way. We then measure demand for a newsletter covering articles from this outlet. Respondents only reduce their demand for biased news if the bias is inconsistent with their own political beliefs, suggesting a trade-off between accuracy concerns and belief confirmation motives. We quantify this trade-off using a structural model and find a similar quantitative importance of both motives. (*JEL* D83, D91, L82)

Keywords: News Demand, Media Bias, Accuracy Concerns, Belief Confirmation

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*This article supersedes a previous draft circulated under the title "Do People Value More Informative News?" (Chopra et al., 2019). We thank participants at the 2019 CESifo Conference in Behavioral Economics for pointing out a confound in the experiments reported in that previous draft, which spurred us to design the new experiments reported in this paper. We are particularly grateful for very helpful discussions with Jesse Shapiro which shaped our thinking about the new design. We also thank Peter Andre, Roland Bénabou, Stefano DellaVigna, Armin Falk, Matthew Gentzkow, Salvatore Nunnari, Simon Quinn, Gautam Rao, Bertil Tungodden, Basit Zafar, and Florian Zimmermann for very helpful comments. Apoorv Kanongo provided excellent research assistance. IRB approval was obtained from the German Association for Experimental Economic Research (GfeW) and the ethics committee of the University of Cologne. The experiments were pre-registered in the AsPredicted registry (#78800, #80266, #87947, and #89081). Financial support from the Russell Sage Foundation (Small Awards in Behavioral Economics), the Research Council of Norway through its Centre of Excellence Scheme (FAIR project No 262675), the Institute on Behavior and Inequality (briq), and the German Research Foundation (DFG) through CRC TR 224 (Project B03) is gratefully acknowledged. Roth acknowledges funding by the Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany's Excellence Strategy – EXC 2126/1-390838866. Chopra: University of Bonn, Felix.Chopra@uni-bonn.de; Haaland: University of Bergen, Ingar.Haaland@uib.no; Roth: University of Cologne and ECONtribute, roth@wiso.uni-koeln.de.

1 Introduction

Mounting empirical evidence documents that news outlets often report the news in a politically biased way (Gentzkow and Shapiro, 2010). Economic models differ in their explanation for why media bias occurs in equilibrium. One class of models assumes that readers value accuracy but also have a preference for news that distort signals towards readers' prior beliefs (Mullainathan and Shleifer, 2005). A second class of models assumes that readers only value accuracy but instead face uncertainty about the accuracy of news outlets (Gentzkow and Shapiro, 2006). This uncertainty leads readers to attribute a higher accuracy to news outlets that provide signals that align with readers' prior beliefs.

These two major theories thus make fundamentally different assumptions about the relative importance of accuracy concerns versus belief confirmation motives in driving the demand for news. The relative importance of these two motives, in turn, has important implications for the optimal regulation of media markets, such as the welfare effects of regulations to increase competition. A major identification challenge when trying to quantify the relative importance of the two motives is that theories based on belief confirmation motives often make predictions that are observationally equivalent with Bayesian updating about source quality (Gentzkow and Shapiro, 2006). This makes it challenging to quantify the relative importance of the two motives with naturally occurring data where beliefs about the media's reporting strategies are unobserved.

To solve the identification challenge, we design experiments to directly vary beliefs about the reporting strategy of a news outlet. We vary beliefs about whether a news outlet selectively reports the facts most favorable to either the Democratic Party (left-wing bias) or to the Republican Party (right-wing bias) or whether it reports all facts from an underlying report containing facts favorable to both parties (no bias). Since our respondents observe the full report available to the news outlet and the underlying source of the report is fixed, the design allows our respondents to make direct inferences about the outlet's reporting strategy. While theories based on accuracy concerns predict that readers should decrease their demand for biased news irrespective of the direction of the bias, theories of belief confirmation predict political heterogeneity based on the direction of the bias.

To quantify the relative importance of accuracy concerns and belief confirmation motives in driving the demand for news, we conduct experiments with over 7,000

US respondents using the online survey platform Prolific. In our first experiment, we experimentally vary beliefs about whether a news outlet is either right-wing biased or unbiased. To do so, we first tell our respondents that the Congressional Budget Office (CBO), Congress's official nonpartisan provider of cost and benefit estimates for legislation, published a report about the "Democrats' $15 Minimum Wage Bill" (Raise the Wage Act of 2021) in which it estimated that the plan would lift 900,000 people out of poverty (contradicting claims made by Republicans) and reduce employment by 1.4 million jobs (contradicting claims made by Democrats). We next tell our respondents that The Boston Herald wrote an article about the CBO findings.

To generate exogenous variation in perceptions of the reporting strategy, we use the fact that The Boston Herald published two different articles about the bill: one article published on February 26, 2021, that only cited the unemployment statistic, and a second article published on March 2, 2021, that cited both statistics.[1] Our treatment varies whether our respondents are informed about the reporting in the February 26 article that only cited the employment statistic (*right-wing bias* treatment) while the remaining half of our respondents are informed about the reporting in the March 2 article that cited both statistics (*no bias* treatment). We administer the treatments in a neutral language without referring explicitly to bias, selective reporting, or accuracy in any way. To measure how this treatment affects the demand for news, we offer all respondents the chance to sign up for a weekly newsletter featuring the top three articles about economic policy published in The Boston Herald. Respondents who sign up for the newsletter receive weekly emails through their Prolific account for one month. Our main outcome of interest is whether our respondents sign up for the newsletter.

Our second experiment uses an analogous design to shift beliefs about left-wing bias. We first inform our respondents that the CBO had published a report about the "Republican Healthcare Plan" (the American Health Care Act of 2017) in which it estimated that the plan would decrease the federal deficit by over $100 billion (contradicting claims made by Democrats) and leave over 20 million more people uninsured (contradicting claims made by Republicans). We again exploit that The Boston Herald published two different articles that differed in their reporting: one article about the Senate version of the bill that only cited the statistic on the number of uninsured, and one article about the House version of the bill that cited both statistics.

[1] The Boston Herald is one of the oldest newspapers in the US and is based near Boston, MA. In 2020, its print edition had a circulation of about 25,000 and its reporting is considered slightly right-of-center.

The key difference compared to the previous experiment relates to the direction of the bias: half of our respondents are informed that The Boston Herald only cited the statistic about the number of uninsured in its coverage of the Senate version of the plan (*left-wing bias* treatment) while the remaining half are informed that The Boston Herald cited both statistics in its coverage of the House version of the plan (*no bias* treatment).

In our analysis of the results, we first confirm that our treatments generate a significant first stage on perceptions of accuracy and political bias of the newsletter among both Biden and Trump voters. In Experiment 1, both Biden and Trump voters in the *right-wing bias* treatment think that the newsletter has significantly lower accuracy and is more right-wing biased compared to respondents in the *no bias* treatment. In Experiment 2, both Biden and Trump voters in the *left-wing bias* treatment think that the newsletter has significantly lower accuracy and is more left-wing biased compared to respondents in the *no bias* treatment. The magnitudes of the first stage on accuracy and bias are economically significant in both experiments. For instance, Biden and Trump voters in the *left-wing bias* treatment think that the newsletter has between 54.2% to 72% of a standard deviation lower accuracy than respondents in the *no bias* treatment.

Turning to our main findings on newsletter demand, we find a striking political heterogeneity in treatment effects depending on the direction of the bias, consistent with theories based on belief confirmation motives. Specifically, the *right-wing bias* treatment has a close to zero impact on newsletter demand among Trump voters. If anything, the *right-wing bias* treatment increases newsletter demand among Trump voters by a non-significant 0.5 percentage points (95% C.I. [-3.55,4.48]; $p = 0.821$). By contrast, the *left-wing bias* treatment significantly reduces newsletter demand among Trump voters by 5.2 percentage points (95% C.I. [-10.01,-0.41]; $p = 0.033$), corresponding to a 27.3% reduction in demand compared to the control group mean of 19.1%. These patterns reverse for Biden voters who significantly reduce their demand in response to the *right-wing bias* treatment by 8.6 percentage points (95% C.I. [-11.94,-5.33]; $p < 0.001$)—corresponding to a 47.7% reduction in demand compared to the control group mean of 18.1%—yet only reduce their demand by a non-significant 2.6 percentage points (95% C.I. [-6.37,1.17]; $p = 0.176$) in response to the *left-wing biased* treatment. These asymmetric responses are consistent with readers having a preference for belief confirmation and inconsistent with the traditional conception of news demand in which readers only care about the accuracy of news. At the same time, we do not observe a significant increase in news demand in any of the treatments, suggesting that

readers also place some value on the accuracy of news. Taken together, our results are thus in line with readers making a trade-off between accuracy concerns and belief confirmation motives.

To quantify the relative importance of accuracy concerns and belief confirmation motives in driving news demand, we use the experimental variation in conjunction with a simple discrete-choice model. Intuitively, the model combines information about the relative magnitude of the treatment effects on perceived accuracy and political bias with information about the magnitude of treatment effects on newsletter subscriptions to identify the relative importance of the two motives. Our structural estimates suggest that preferences for belief confirmation and accuracy concerns are of similar quantitative importance for the demand for news in this context.

To shed more light on how our respondents interpreted our main treatment variation, we conducted a separate mechanism experiment. In this experiment, we use open-ended questions to elicit beliefs about the potential motives behind The Boston Herald's reporting of one statistic (*bias* treatments) or the reporting of both statistics (*no bias* treatments) from the CBO reports. The unprompted responses reveal that respondents in the *bias* treatments have thoughts about political bias on top of their minds: 53.9% of respondents in the *bias* treatments mention political bias as the explanation for The Boston Herald selectively reporting only one statistic and no one mentions balanced reporting. By comparison, in the *no bias* treatments, 20.7% of respondents mention balanced reporting and only 12.4% mention political bias. Our data also reveals that only a very small fraction of respondents mention other potential motives underlying the selective reporting, such as entertainment, cognitive constraints, or rational delegation. These results thus provide direct evidence that people intuitively interpret the action of selectively reporting only one statistic from the CBO reports as a clear sign of political bias and associate the action of reporting both statistics with balanced reporting. As such, this data supports the assumption from our structural model that our treatments mainly shifted beliefs about accuracy and bias.

To examine how people justify their demand for biased news, we collect direct data on people's motives for subscribing to the newsletter at the end of the main experiments. To get an unprompted response, we asked respondents to answer an open-ended question on their motives for subscribing or not subscribing to the newsletter. Respondents in the *no bias* treatments frequently mention getting accurate and unbiased news as a key motive for signing up for the newsletter, while respondents in both of the *bias*

treatments are significantly less likely to mention such accuracy concerns and more likely to provide a generic justification, such as wanting to follow the news cycle or their interest in economic policy. These responses underscore that people do not invoke justifications that are consistent with alternative theories for why people consumed biased news, such as diversification or delegation motives. Rather, our finding that respondents in both of the *bias* treatments are significantly less likely to mention accuracy concerns and more likely to provide generic justifications is consistent with people providing rationales that allow them to maintain a positive self-image (Benabou and Tirole, 2006).

Our results contribute to the literature on media bias (DellaVigna and Kaplan, 2007; Durante and Knight, 2012; Gentzkow et al., 2018; Mullainathan and Shleifer, 2005; Perego and Yuksel, 2022). To measure media bias, previous studies have developed text-based measures that rank newspapers according to the similarity of their language to that of politicians (Gentzkow and Shapiro, 2006). For example, more frequent use of the term "death tax" rather than "estate tax" might indicate a tendency to slant towards the right. However, it is not obvious that one term conveys more *information* than the other. Thus, while previous studies suggest that readers have a demand for slanted language (Garz et al., 2020; Gentzkow and Shapiro, 2010; Gentzkow et al., 2014), this finding does not allow for strong conclusions about whether readers make a trade-off between accuracy concerns and belief confirmation motives. Our main contribution is to provide direct evidence on the relative importance of accuracy concerns and belief confirmation motives in a clean and natural setting.[2] The relative importance of these motives plays a major role in theoretical analyses of media markets (Baron, 2006; Chan and Suen, 2008; Gentzkow and Shapiro, 2006; Mullainathan and Shleifer, 2005) and is of critical importance for the debate on whether policymakers should introduce regulations to increase competition in media markets (Foros et al., 2015).

We also contribute to a literature on people's demand for information (Bursztyn et al., 2021; Capozza et al., 2021; Faia et al., 2021; Falk and Zimmermann, 2017; Fuster et al., 2022; Ganguly and Tasoff, 2016; Montanari and Nunnari, 2019; Nielsen, 2020; Zimmermann, 2015).[3] Our key contribution to this literature is to identify

[2]Subsequent work by Chopra et al. (2022) examines how the demand for a newsletter changes when the newsletter includes a fact-checking service.

[3]More broadly our evidence relates to a literature on motivated belief updating (Charness et al., 2021; Exley, 2015; Schwardmann and van der Weele, 2019; Schwardmann et al., 2022; Di Tella et al., 2015; Thaler, 2019).

the relative importance of accuracy concerns and belief confirmation motives. To differentiate between accuracy concerns and belief confirmation motives, we employ a new identification strategy in which we vary beliefs about whether a news outlet reports the news in a right-wing biased, left-wing biased, or politically unbiased way. In contrast to much of the previous experimental literature on information demand, we vary perceptions of bias about a real-world news outlet rather than features of an abstract signal structure. Moreover, our main outcome provides high external validity by measuring subscriptions to a newsletter covering actual newspaper articles from a real-world outlet.

Finally, we contribute to a growing literature on structural behavioral economics (see DellaVigna, 2018, for a comprehensive review). Prior work has provided estimates of key behavioral parameters by combining parsimonious behavioral models with experimentally-induced variation (Allcott and Taubinsky, 2015; Allcott et al., 2021; Augenblick et al., 2015; DellaVigna and Pope, 2018; DellaVigna et al., 2022). We demonstrate how to use exogenous variation in perceptions of accuracy and bias in reporting to estimate the relative importance of different motives in shaping people's demand for news using a parsimonious discrete choice model. Our estimates underline an important quantitative role of both accuracy concerns and preferences for belief confirmation in driving news demand. An important benefit of the structural estimation is that it provides greater comparability with future studies that might try to quantify the relative importance of accuracy concerns compared to belief confirmation motives in other settings.

The remainder of the paper proceeds as follows. Section 2 describes the experimental design. Section 3 presents both the reduced form results and the structural estimates. Section 4 presents evidence on psychological mechanisms and discusses alternative mechanisms. Section 5 concludes. The Online Appendix provides a theoretical framework, additional empirical results, and the full set of experimental instructions.

2 Experimental design

Our study features two main experiments that examine how varying beliefs about the accuracy and political bias of a news outlet affect demand for a newsletter featuring articles from that outlet. Experiment 1 varies beliefs about whether a news outlet

selectively reports the facts most favorable to the Republican Party (*right-wing bias*) while Experiment 2 varies beliefs about whether it selectively reports the facts most favorable to the Democratic Party (*left-wing bias*). Figure 1 presents an overview of the main design features and Section E of the Online Appendix presents the full instructions for both experiments.

[Insert Figure 1 here]

2.1 Sample

We collected the data for our main experiments in collaboration with Prolific, a leading market research company commonly used in social science research (Haaland et al., 2021). We collect data with Prolific not only because of the high quality of responses compared to other survey platforms (Eyal et al., 2021) but also because of the ability to email respondents the newsletter via their Prolific account without the need for collecting email addresses. The data for our main experiments was collected in November and December 2021. We collected a sample of 1,464 Biden voters and 1,235 Trump voters for Experiment 1 and 1,466 Biden voters and 849 Trump voters for Experiment 2.[4] Our samples are heterogeneous and resemble the US population in terms of several observables (income, region, and gender; see Table B.2). In both experiments, the treatment and control group of respondents are balanced in terms of observable characteristics both in the full sample (Table B.3 and Table B.4) and within the subsamples of Biden and Trump voters (see Tables B.5, B.6, B.7 and B.8).

2.2 Experiment 1: Right-wing bias vs. no bias

We first describe the design of Experiment 1 in which we vary beliefs about whether a news outlet selectively reports the facts most favorable to the Republican Party (*right-wing bias*) or reports facts favorable to both the Republican Party and the Democratic Party (*no bias*).

[4]We aimed for gender-balanced samples of 1,500 Biden voters and 1,500 Trump voters in both experiments. Respondents could only participate in one of the two experiments, making it especially difficult to recruit enough Trump voters in Experiment 2 (there are about six times as many Biden voters as Trump voters active on the Prolific platform). In both experiments, the median time to complete the survey was about six minutes. We employed a simple attention check at the beginning of the survey, which over 95% of respondents pass, to screen out inattentive respondents.

Background characteristics We first measure basic demographics, such as age, gender, education, income, and the region of residence. We then elicit whether our respondents voted for Joe Biden or Donald Trump in the 2020 Presidential Election.[5] We then measure their news consumption during the last 12 months, their interest in economic news, and whether they currently subscribe to any newsletters.

Pre-treatment beliefs Subsequently, we elicit beliefs about how The Boston Herald reported about a CBO report containing facts favorable to both Democrats and Republicans. Specifically, we tell our respondents that the Congressional Budget Office (CBO), Congress's official nonpartisan provider of cost and benefit estimates for legislation, published a report about the "Democrats' $15 Minimum Wage Bill" (Raise the Wage Act of 2021) in which it estimated that the plan would lift 900,000 people out of poverty (contradicting claims made by Republicans) and reduce employment by 1.4 million jobs (contradicting claims made by Democrats).

We next tell our respondents that The Boston Herald wrote an article about the economic impact of the $15 Minimum Wage Bill after the CBO published its report. We then measure beliefs about how The Boston Herald covered the CBO findings by asking them to guess whether it only reported the statistic on the number of people lifted out of poverty (left-wing bias), only the statistic on the effects on reducing employment (right-wing bias), or both statistics (no political bias).

By making our respondents observe the full report available to the news outlet, our design allows our respondents to make direct inferences about its reporting strategy. We chose to make the CBO the source of the underlying report for two reasons. First, the CBO is known to be nonpartisan (to stay politically neutral, it only assesses the consequences of proposed policies and does not make its own policy recommendations). Second, all major newspapers in the US generally feature articles covering the CBO's evaluation of legislative proposals, making it a familiar and natural source for a newspaper article.

[5] When recruiting respondents on Prolific, we pre-screen on having voted for either Donald Trump or Joe Biden. We ask about voting status in the survey to identify respondents who provide responses inconsistent with the screening criteria. Only a few respondents provided responses inconsistent with the screening criteria, and we excluded these respondents from further analysis.

Treatments To generate exogenous variation in beliefs about selective reporting, we exploit the fact that The Boston Herald published two different articles about the $15 Minimum Wage Bill: one article published on February 26, 2021, that only cited the unemployment statistic, and a second article published on March 2, 2021, that cited both statistics.[6] 50% of our respondents are randomly assigned to learn about the selective reporting in the February 26 article that only mentioned the unemployment statistic (*right-wing bias* treatment). We frame the treatment information in a neutral way to minimize experimenter demand effects:

> The article, published in The Boston Herald on February 26, 2021, reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.

The remaining 50% of respondents are assigned to learn about the balanced reporting in the March 2 article that reported both statistics (*no bias* treatment):

> The article, published in The Boston Herald on March 2, 2021, reported that the bill would reduce employment by 1.4 million jobs **and** that it would lift 900,000 people out of poverty.

We had two main reasons to select The Boston Herald as the news outlet for the experiment. First, we wanted to feature a news outlet for which people had relatively weak priors compared to more popular news outlets, such as The New York Times or The Wall Street Journal. Weaker priors about accuracy and political bias make beliefs about the outlet's reporting strategy potentially more malleable to information about past reporting.

Second, we wanted an active control group design in which respondents would receive different pieces of truthful information about how a news outlet covered the CBO findings. The Boston Herald was the only news outlet we identified that had written multiple articles about the same CBO reports that also differed in whether or not it selectively reported about the CBO findings. Active control group designs have several advantages compared to passive control group designs (Haaland et al., 2021).

[6] See "Who wins, who loses with higher minimum wage" by Farren, Michael and Forzani, Agustin. *The Boston Herald*, March 2, 2021, and "$15 minimum wage hurts vulnerable workers the most" by Buhajla, Stefani. *The Boston Herald*, February 26, 2021.

First, an active control group allows for a cleaner identification of treatment effects because it holds more features of the environment constant compared to passive control group designs, such as respondents' attention and exposure to new information. In a design with a passive control group, respondents who do not learn about how the outlet reported about the CBO findings might be more curious to learn about the answer. That is, with a passive control group, curiosity motives could plausibly differ between the treatment and control group, while these motives are less likely to differ in an active control group design. Second, with an active control group, identification does not depend on people's prior beliefs, allowing us to identify causal effects of beliefs about selective reporting for a broader population. Furthermore, since prior beliefs are not exogenously assigned, interpretation of heterogeneous treatment effects is more difficult in designs with a passive control group.

Main outcome measure: Newsletter demand After giving respondents differential information about whether The Boston Herald reported in a balanced or selective way about the CBO findings, we measure demand for a weekly newsletter featuring stories from The Boston Herald:

> We would like to offer you the opportunity to sign up for our weekly newsletter.
>
> Our **Weekly Economic Policy Newsletter** will cover the **top three articles** about economic policy published in **The Boston Herald**.
>
> If you say "Yes" below, we will message you the newsletter on your Prolific account on a weekly basis over the next month.

Our main outcome of interest is the binary decision to sign up for this newsletter. Our focus on newsletter subscriptions is motivated by the fact that newsletters are a popular way of staying informed about politics, with 21% of Americans receiving news from a newsletter over the course of a week (Newman et al., 2020). Moreover, by including only the three top articles in our newsletter, we reduce the expected cost of our respondents to stay up to date about economic policies—both in terms of time costs and search efforts.

On the decision screen, we also clarify that the articles included in the newsletter can be accessed for free by visiting The Boston Herald's website. To fix beliefs about the

researchers' political leanings, we clarify that we are non-partisan academic researchers who provide the newsletter as a free service for people to stay informed about the most important news related to economic policy. Finally, we explain that the newsletter is a non-commercial product.

In practice, we sent the newsletter to our respondents on the Mondays of each of the four weeks after they decided to subscribe to the newsletter. A key advantage of conducting our experiment on Prolific is that we can administer the newsletter to respondents via direct messages on Prolific without eliciting any personally identifiable information. Instead, respondents receive an email notification when we message them the newsletter. This, in turn, ensures that we can measure newsletter demand irrespective of privacy concerns. Appendix Section D provides information about the logistical details and the newsletter's design.[7]

Post-treatment beliefs about accuracy and political bias of the newsletter After choosing whether to subscribe to the newsletter, we measure post-treatment beliefs about the accuracy and political bias of the newsletter. We also elicit perceptions about the trustworthiness, entertainment value, quality, and complexity of the newsletter. We measure these beliefs using five-point Likert scales.

2.3 Experiment 2: Left-wing bias vs. no bias

In Experiment 2, we vary beliefs about whether a news outlet selectively reports the fact most favorable to the Democratic Party (*left-wing bias*) or reports facts favorable to both the Republican Party and the Democratic Party (*no bias*). The design of this experiment closely resembles the design of Experiment 1, and most questions and outcomes are identical across the two experiments. We highlight the key design differences below (see also Figure 1).

Pre-treatment beliefs We measure beliefs about how The Boston Herald reports about the "Republican Health Care Plan" (the American Health Care Act of 2017). Respondents are told that the CBO estimated that the plan would decrease the fed-

[7]Each week we received a large number of thank you messages from respondents. A much smaller number of subscribers wrote to us that they would like to unsubscribe from the newsletter. Overall, this feedback from subscribers illustrates both the benefits and costs of receiving the newsletter.

eral deficit by $119 billion (contradicting claims made by Democrats) and leave 23 million more people uninsured (contradicting claims made by Republicans). 50% of respondents are asked about their beliefs about the *Senate* version of the Republican Healthcare Plan, while the remaining 50% are asked about the *House* version of the Republican Healthcare Plan.[8] This design choice is motivated by the fact that The Boston Herald reported different CBO statistics for these two versions of the Republican Health Care Plan, as explained below.

Treatments The Boston Herald published two articles about the Republican Healthcare Plan. In the article about the Senate version of the Republican Healthcare Plan, The Boston Herald reported only that the plan would leave over 20 million more people uninsured (*left-wing bias* treatment). In the other article about the House version of the Republican Healthcare Plan, The Boston Herald reported both CBO statistics (*no bias* treatment).[9] In our design, 50% of respondents are randomly assigned to learn about the coverage of the article that only mentioned the consequences on the number of uninsured people (*left-wing bias* treatment), which we again frame in a neutral way to minimize experimenter demand effects:

> The Boston Herald article about the Senate Republican Healthcare Plan reported that the plan would leave over 20 million more people uninsured **but not** that it would decrease the deficit by over $100 billion.

The remaining 50% of respondents learn about the article that mentioned both statistics (*no bias* treatment):

> The Boston Herald article about the House Republican Healthcare Plan reported that the plan would leave over 20 million more people uninsured **and** that it would decrease the deficit by over $100 billion.

Newsletter and post-treatment beliefs We then employ the same main outcome variable as in Experiment 1, namely the binary decision to subscribe to a newsletter

[8] Prior beliefs about reporting are virtually identical for the Senate version and the House version of the Republican Healthcare Plan (as shown in Figure B.1).

[9] See "CBO: 22 million more uninsured by 2026 under Senate health bill" (Associated Press), published in *The Boston Herald*, June 26, 2017, and "CBO House GOP health bill projection: 23 million more uninsured" (Associated Press), published in *The Boston Herald*, May 24, 2017.

featuring the three top stories about economic policy from The Boston Herald. We also measure post-treatment beliefs about accuracy and political bias as well as other beliefs about newsletter characteristics as in Experiment 1.

2.4 Hypotheses

Our design allows us to study whether and how people trade off the accuracy of news against the political bias in reporting. To provide a benchmark, we use standard arguments to formally show that both the *right-wing bias* treatment (in Experiment 1) and the *left-wing bias* treatment (in Experiment 2) should decrease the perceived Blackwell informativeness of the newsletter compared to the *no bias* treatments in the narrow context of reporting about CBO findings (as shown in Section A of the Online Appendix). The intuition underlying this observation is that both treatments increase the perceived likelihood of selective reporting—i.e., a coarsening of the signal—in a setting where full information disclosure would have been possible.[10]

Hypothesis 1. If respondents have preferences in line with the standard conception of news demand in which people only care about the accuracy of news, we would expect a significantly lower newsletter demand in both the *right-wing* and *left-wing* treatments compared to the associated *no bias* treatments.

Next, we turn to the predictions of behavioral models of news demand that posit a trade-off between accuracy and belief confirmation motives. The predictions of these models depend on the political beliefs of our respondents. Specifically, models of belief confirmation assume that Biden voters have a preference for reading left-wing biased news, while Trump voters have a preference for reading right-wing biased news. We thus expect patterns of heterogeneous treatment effects by respondents' political views. First, these models predict a decrease in the demand for news whenever the alignment between respondents' political views and the perceived political bias in reporting decreases. Second, in cases where the alignment between respondents' political views and the perceived political bias in reporting increases at the cost of lower accuracy

[10]While it seems reasonable that reporting both statistics is normatively better than selectively reporting only one statistic in our context, it is important to emphasize that there is in general no normative benchmark for how to select which facts to report when full disclosure is *not* possible (e.g., when a report includes many different statistics and a news outlet by necessity have to engage in selective reporting).

in reporting, the predictions of models of belief confirmation motives are ambiguous: Respondents have to trade off the gains from an increase in their preferred bias in reporting against the cost of lower accuracy in reporting. The sign of the overall effect on the demand for news depends on (i) the relative importance of accuracy compared to belief confirmation motives, and (ii) the underlying magnitude of first-stage changes in perceptions of accuracy and bias in reporting.

Hypothesis 2. If people have a preference for reading news that confirms their prior beliefs, we would expect the following patterns of treatment effects:

(a) In Experiment 1 (2), we would expect a negative effect of the *right-wing bias* (*left-wing bias*) treatment on the demand for news among Biden (Trump) voters.

(b) In Experiment 2 (1), the sign of the effect of the left-wing bias (right-wing bias) treatment on the demand for news among Trump (Biden) voters is ambiguous. If people have a sufficiently strong preference for belief confirmation, we would expect either no change in demand or an increase in demand.

3 Main results

This section presents our main results. We first present evidence on the first stage of the treatment on perceptions of accuracy and the political bias of the newsletter. We then present the main treatment effects on demand for the newsletter. We finally use a discrete choice model to estimate the relative importance of accuracy concerns compared to belief confirmation motives.

3.1 Beliefs about the accuracy and political bias of the newsletter

Table 1 shows treatment effects on beliefs about the accuracy and political bias of the newsletter separately for Trump voters (Panel A) and Biden voters (Panel B). Columns 1 and 4 show that Trump voters in the *right-wing bias* treatment think that the newsletter has 16.5% of a standard deviation lower accuracy ($p = 0.003$) while Trump voters in the *left-wing bias* think that the newsletter has 54.2% of a standard deviation lower accuracy ($p < 0.001$). We also observe treatment heterogeneity in accuracy perceptions among Biden voters: Biden voters in the *right-wing bias* treatment think that the

newsletter has 90.3% of a standard deviation lower accuracy ($p < 0.001$) while Biden voters in the *left-wing bias* treatment think that the newsletter has 72% of a standard deviation lower accuracy ($p < 0.001$).[11] The political heterogeneity in treatment effects on accuracy perceptions is consistent with the mechanism in Gentzkow and Shapiro (2006) and motivates our structural approach (outlined in Section 3.3) that accounts for heterogeneous treatment effects on perceptions.

We next examine treatment effects on perceptions of political bias. Column 2 of Table 1 shows that Trump voters in the *right-wing bias* treatment think that the newsletter has 49% of a standard deviation lower left-wing bias ($p < 0.001$) while Trump voters in the *left-wing bias* treatment think that the newsletter has 26.6% of a standard deviation higher left-wing bias ($p < 0.001$). Biden voters in the *right-wing bias* treatment think that the newsletter has 84.9% of a standard deviation lower left-wing bias ($p < 0.001$) while Biden voters in the *left-wing bias* treatment think that the newsletter has 30.5% of a standard deviation higher left-wing bias ($p < 0.001$).

Our experiments thus generate situations in which perceptions of accuracy always decrease but in which perceptions of political bias move in opposite directions. Experiment 1 creates a potential conflict between accuracy concerns and belief confirmation motives for Trump voters but not for Biden voters. Conversely, Experiment 2, creates a potential conflict between accuracy concerns and belief confirmation motives for Biden voters but not for Trump voters. This exogenous variation in accuracy and political bias allows us to test for the presence of belief confirmation motives in the demand for news.

3.2 Reduced form results on newsletter demand

Columns 3 and 6 of Table 1 present treatment effects on the demand for the newsletter in Experiment 1 and 2, respectively (Figure 2 displays these treatment effects graphically without control variables). As shown in Panel A of Table 1, we find virtually no impact of the *right-wing bias* treatment on newsletter demand among Trump voters in Experiment 1. If anything, the treatment *increases* newsletter demand among Trump voters by 0.5 percentage points (95% C.I. [-3.55,4.48]; $p = 0.821$). In Experiment 2,

[11] Table B.11 shows that treatment effects on accuracy perceptions are robust to using conceptually related outcomes: both Biden and Trump voters assigned to the bias treatments display lower trust in the newsletter and associate it with lower quality. On top of this, the first stage on accuracy perceptions looks very similar if we construct an "accuracy index" combining the accuracy, quality, and trust outcomes.

the *left-wing bias* treatment significantly reduces newsletter demand among Trump voters by 5.2 percentage points (95% C.I. [-10.01,-0.41]; $p = 0.033$), corresponding to a 27.3% reduction in demand compared to the control group mean of 19.1%.[12]

These patterns reverse for Biden voters. As shown in Panel B of Table 1, in contrast to the muted effects of the *right-wing bias* treatment among Trump voters, Biden voters significantly reduce their demand for the newsletter by 8.6 percentage points in response to the *right-wing bias* treatment (95% C.I. [-11.94,-5.33]; $p < 0.001$), corresponding to a 47.7% reduction in demand compared to the control group mean of 18.1%. However, in response to the *left-wing bias* treatment, Biden voters only reduce their demand by a non-significant 2.6 percentage points (95% C.I. [-6.37,1.17]; $p = 0.176$).[13]

The political heterogeneity in treatment effects, in which our respondents only significantly reduce their demand for biased news if the bias is inconsistent with their own political beliefs, is inconsistent with the traditional conception of news demand in which readers only care about the accuracy of news. At the same time, that we do not observe a significant increase in demand for the newsletter in any of the treatments suggests that our respondents also care about the accuracy of news. Taken together, our results are thus in line with behavioral models where readers face a trade-off between accuracy concerns and belief confirmation motives. Our first main result follows.

Result 1. People strongly reduce their demand for biased news, but only if the political bias in reporting is inconsistent with their own political beliefs.

[Insert Figure 2 here]
[Insert Table 1 here]

3.3 Structural estimates of preference parameters

Our reduced form results suggest that people's demand for news are driven by both accuracy concerns and belief confirmation motives, but they do not allow us to quantify the *relative* importance of these motives. In this section, we fill this gap by using the exogenous variation in perceptions of accuracy and bias induced by our treatments to estimate a parsimonious discrete choice model. This structural model allows us to combine the quantitative information on the effects of the treatments on both accuracy

[12]The p-value for equality of treatment effects across experiments for Trump voters is 0.072.
[13]The p-value for equality of treatment effects across experiments for Biden voters is 0.017.

and bias perceptions alongside with our quantitative estimates on the effects on news demand.

Discrete choice model We use a binary choice framework where the agent subscribes to the newsletter ($y = 1$) if the utility u from subscribing to the newsletter exceeds the reservation utility r of his outside option, which implies that $y = \mathbf{1}(u \geq r)$. To focus on the trade-off between accuracy and belief confirmation, we decompose u as follows:

$$u = \alpha\sigma + \beta b + \varepsilon \tag{1}$$

Here, σ denotes the accuracy of the newsletter; b denotes the ideological alignment of the newsletter ("belief confirmation"), where larger values indicate a greater alignment between the political bias of the newsletter and the respondents' own political ideology; and $\varepsilon \sim N(0,1)$ denotes a random utility shock with a standard normal distribution capturing idiosyncratic tastes. The parameters α and β capture the marginal utility of reading the newsletter from changes in accuracy and belief confirmation.

Estimation and identification We estimate the model parameters, $\theta = (\alpha, \beta, r)$, for the full sample as well as separately for Biden and Trump voters. To quantify σ and b, we use the z-scored post-treatment belief measures of perceived accuracy and political bias in reporting.[14] In particular, we recode the perceived political bias such that larger values correspond to a stronger left-wing (right-wing) bias for Biden (Trump) voters. Next, if perceptions of accuracy and bias were uncorrelated with the error term, one could simply use newsletter subscription choices and the belief data from Experiments 1 and 2 to estimate equation 1 using a Probit regression. However, this exclusion restriction is unlikely to hold in practice. We therefore estimate an IV probit model as outlined by the set of equations below in which the binary dependent variable is the decision to sign up to our newsletter:

$$y_i = \mathbf{1}(\alpha\sigma_i + \beta b_i + \varepsilon_i \geq r) \tag{2}$$

$$\sigma_i = Z_i'\gamma_\sigma + \varepsilon_i^{'} \tag{3}$$

$$b_i = Z_i'\gamma_b + \varepsilon_i^{''} \tag{4}$$

[14] We normalize the post-treatment belief measures such that they have a mean of zero and a standard deviation of one among respondents in the *no bias* treatment arms.

Here, we instrument respondents' perceptions with a saturated set of treatment indicators, Z_i (see equations 3 and 4). We use Newey's (1987) two-step estimator to estimate the parameters of interest.[15] In specifications where we pool both Democrats and Republicans, the set of instruments, Z_i, also includes interactions between the treatment arm indicators and a binary indicator for whether the respondent voted for Trump to account for heterogeneous first-stage effects on perceptions across political groups. We also include a binary indicator for whether the respondent voted for Trump as a control variable to allow for differences in the outside option across political groups in the pooled specification.

The main advantage of this estimation strategy is that we exploit *only* exogenous variation in perceptions to disentangle people's accuracy and belief confirmation motives: While both experimental treatments decrease the perceived accuracy relative to the *no bias* treatment, the *right-wing bias* treatment in Experiment 1 shifts the perceived bias to the right, while the *left-wing bias* treatment in Experiment 2 shifts the perceived bias to the left.

Discussion of assumptions First, we focus only on the accuracy-belief confirmation trade-off. While demand for our newsletter could also reflect other motives—such as a demand for entertainment—our estimation strategy remains valid if these motives do not differentially affect demand across treatment arms.[16] Second, we assume that there is no internal saturation point in terms of the newsletter's political bias. An alternative approach would be to assume that people receive disutility from the difference between their preferred level of media bias, b^*, and the perceived bias of the newsletter, $-\beta|b-b^*|$. This is equivalent to equation (1) if $b_D^* \leq b \leq b_R^*$, i.e., whenever the newsletter is perceived to be more centric than the preferred level of bias among Biden voters (b_D^*) and Republicans (b_R^*). In practice, we expect this to hold as our respondents perceive The Boston Herald as a relatively unbiased news outlet to begin with. Third, we assume that our survey measures of accuracy and bias capture underlying perceptions well and are comparable to each other. We designed our survey measures to be as comparable as possible by eliciting them both on the

[15] In practice, we use Stata's `ivprobit` routine.

[16] The open-ended data from the mechanism experiment (Experiment 3), which we present in Section 4.1, suggests that the treatment indeed mostly sparked thoughts about bias and accuracy and furthermore did not trigger many thoughts related to entertainment, cognitive constraints, or other features of news articles. This motivates an approach that focuses only on perceptions of accuracy and political bias.

same type of scale with the same number of response options. In addition, we only use z-scored perceptions in our estimation to further ensure the comparability of our survey measures.

Results Table 2 presents the parameter estimates of the discrete choice model.[17] Consistent with the predictions of standard models, the estimates using the full sample suggest a preference for accurate news ($p < 0.01$, column 1). At the same time, the model estimates suggest that the demand for news is also driven by a preference for belief confirmation ($p < 0.01$, column 1), which corroborates our reduced form results. Indeed, our estimates imply a relative weight on accuracy of $\alpha/(\alpha+\beta) = 0.255/(0.255+0.360) = 0.414$, and we cannot reject the null hypothesis that respondents assign equal weights to accuracy and belief confirmation ($p > 0.10$). Columns 2 and 3 of Table 2 examine heterogeneity in preferences between Biden and Trump voters. Among Biden voters, we again find both a preference for accurate news ($p < 0.01$) as well as a preference for belief confirmation ($p < 0.01$, column 2). The estimates for Trump voters are qualitatively similar but more noisily estimated. If anything, we find that Biden voters assign a smaller weight to accuracy compared to belief confirmation motives than Trump voters. However, we cannot reject the null hypothesis that both groups care equally about accuracy and belief confirmation ($p > 0.10$). Our second main result can thus be summarized as follows:

Result 2. Both accuracy and belief confirmation motives are important drivers of the demand for news, and our model estimates suggest that people assign about equal weight to both motives in the context of our experiment.

[Insert Table 2 here]

Robustness To assess the robustness of our structural estimates, we also estimate an analogous *linear* probability model using a two-stage least-squares estimator where we again use our treatment assignments to instrument endogenous perceptions of accuracy and belief confirmation motives. Panel A of Table B.17 shows that we obtain quantitatively very similar estimates of the relative importance of accuracy compared to belief confirmation motives using the linear model. Thus, while the choice of a linear

[17] Our results are robust to replacing the z-scored post-treatment accuracy beliefs with an index based on post-treatment beliefs about accuracy, quality, and trustworthiness (as shown in Table B.18).

versus a non-linear second stage model affects the scale of the coefficients, the implied relative magnitudes are quantitatively robust across specifications.

One potential concern is that perceptions of accuracy and bias are endogenous to choices. Specifically, people's desire for consistency may shape their post-treatment beliefs in ways that are unrelated to the perceptions and motives actually shaping choices. To address this concern, we conducted an additional pre-registered experiment on Prolific in February 2022 (Experiment 4; see Table B.1).[18] In this experiment, we administer the same treatments as in Experiment 1 and 2 but respondents are not offered the chance to subscribe to the newsletter. Instead, we inform them about the existence of the newsletter and then elicit respondents' post-treatment beliefs about the newsletter's accuracy and bias using the same survey measures as in our previous experiments.[19]

We then use a *two-sample* instrumental variables strategy to estimate a linear probability model where the binary dependent variable is the decision to sign up to our newsletter. Specifically, we use OLS to estimate equations 3 and 4 using the belief data from Experiment 4 (where we only elicit perceptions). We then use data from Experiments 1 and 2 (where we elicit choices) and estimate a linear probability model using the predicted perceptions of accuracy and belief confirmation obtained from the first-stage regression as regressors. For inference, we obtain standard errors using a bootstrap procedure that resamples the choice data (from Experiments 1 and 2) and the belief data (from Experiment 4) with replacement.

Panel B of Table B.17 presents the parameter estimates from this robustness exercise. The estimates using the full sample support the quantitative importance of people's preference for belief confirmation ($p < 0.01$, column 1). Again, the implied weight on accuracy is close to and not statistically significantly different from 0.5, corroborating the robustness of our model estimates. If anything, the point estimates are closer to 0.5 and exhibit less heterogeneity across political groups (columns 2 and 3). Taken together, this underscores that both accuracy concerns and belief confirmation motives have a similar quantitative importance on the demand for news in our context.

[18] Our sample includes 968 Biden voters and 942 Trump voters. The median response time was 3.5 minutes. To recruit enough Trump voters, we allowed 624 Trump voters who had participated in the main experiments three to four months prior to participate in Experiment 4. Reassuringly, we see no treatment heterogeneity based on the original treatment assignment.

[19] While this addresses concerns about consistency bias in survey responses (Falk and Zimmermann, 2015), the absence of a choice might lower engagement with the survey. We therefore view this as a complementary robustness check.

4 Psychological mechanisms and robustness

In this section, we provide evidence on psychological mechanisms underlying our treatment effects and discuss some potential alternative mechanisms behind our results.

4.1 Mechanism experiment: Interpretation of treatment

To shed light on the psychological mechanisms, we measure respondents' thoughts about the motives behind different reporting decisions by the news outlet. For this purpose, we conducted an additional pre-registered experiment on Prolific (Experiment 3; see Table B.1). The experiment was conducted in February 2022 with 388 respondents (240 Biden voters and 148 Trump voters).[20]

Design Half of the respondents are informed that the CBO evaluated the consequences of the "$15 Minimum Wage Bill" while the remaining half of the respondents are informed that the CBO evaluated the consequences of the "Republican Healthcare Plan." We also tell our respondents about the competing claims made by Democrats and Republicans about the respective plans. We then randomly assign respondents to the same *bias* and *no bias* treatments on the respective plans as described in sections 2.2 and 2.3. We then elicit people's thoughts on why The Boston Herald reported only one statistic (in the *bias* treatment) or both statistics (in the *no bias* treatment) using open-ended text responses. To ensure high levels of effort, we ask our respondents to write two to three sentences. For example, respondents assigned to the $15 Minimum Wage Bill and the *right-wing bias* treatment were asked:

> Why do you think that The Boston Herald reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty?

Respondents assigned to the Republican Healthcare Plan received analogous instructions tailored to that plan (see Section E of the Online Appendix for the instructions).

[20]The median response time was four minutes and we excluded respondents from previous experiments. We aimed for a politically balanced sample of Trump and Biden voters but we found it challenging to recruit enough Trump voters after excluding previous survey respondents from participation. As noted previously, there are about six times as many Biden voters as Trump voters active on the Prolific platform.

Hand-coded data We hand-code the open-ended responses about the reporting strategy using a pre-specified procedure. We assign each response to one of the following three categories: First, if respondents mention that the outlet was politically biased, we assign them to the "bias" category (for instance, the following example responses were classified as "biased": "I think it's biased reporting," "Perhaps they are a Republican newspaper," "I believe it is a left-leaning newspaper," or "They clearly support the Democrats"). Second, if respondents mention that the newspaper was trying to provide a balanced view of the facts, we assign them to the "balanced" category (for instance, the following example responses were classified as "balanced": "They were probably trying to report fairly without bias," "They were trying to give the full picture," and "They tried to report fairly and accurately" would all be classified). Third, all other responses are assigned the "other" category. In addition to the pre-specified categories, we also categorized responses that mentioned motives related to entertainment, complexity, or rational delegation.

Results based on hand-coded data Figure 3 shows that respondents assigned to the *bias* treatments are 41.1 percentage points more likely to refer to political bias ($p < 0.001$) compared to a mean of 12.4% in the *no bias* treatments. Respondents assigned to the *no bias* treatment, on the other hand, are 20.1 percentage points more likely to talk about balanced reporting ($p < 0.001$) compared to a mean of 0% in the *no bias* treatments. These effects are both statistically and economically significant and highlight that respondents interpret the reporting decision to be either driven by motives to deliver accurate or biased reporting.[21] Respondents' unprompted responses also reveal that other perceived motives, such as rational delegation, entertainment, or cognitive constraints, only play a very minor role. Only four out of 388 respondents provide responses consistent with rational delegation in which the newspaper selectively reports statistics considered more important by their readers. Another three respondents mention entertainment motives. Finally, two respondents thought the selective reporting was motivated to reduce the complexity of the reporting. These findings thus corroborate the idea that our experiment is well-suited to quantify the relative importance of accuracy concerns and belief confirmation motives in driving the demand for news.

[Insert Figure 3 here]

[21] We find consistent patterns for whether people mentioned balanced reporting in their open-ended responses (as shown in Figure B.3).

Text analysis As a complement to the hand-coded data, we also use a more unstructured approach to analyze the text data. We use the methodology proposed by Gentzkow and Shapiro (2010) to determine the words that are most characteristic of being in the *no bias* or the *bias* treatment arms. Specifically, given two groups A and B of respondents, we calculate Pearson's χ^2 statistic for each word w,

$$\chi^2_{wAB} = \frac{(f_{wA}f_{\sim wB} - f_{wB}f_{\sim wA})^2}{(f_{wA}+f_{wB})(f_{wA}+f_{\sim wA})(f_{wB}+f_{\sim wB})(f_{\sim wA}+f_{\sim wA})} \tag{5}$$

where f_{wA} and f_{wB} denote the total number of times that the word w was mentioned by respondents in group A and B, respectively. Similarly, $f_{\sim wA}$ and $f_{\sim wB}$ refer to the total number of times words *other* than w were mentioned. We then focus on the words with the largest χ^2.

Figure 4 presents the 50 words that are most characteristic of responses by Biden voters (Panel A) and by Trump voters (Panel B). We find that words related to "bias" are more characteristic of responses in the *bias* treatment arms, while words, such as "non-partisan", "unbiased", "fair", and "factual" are more typical of responses in the *no bias* treatment arms. This corroborates the findings from the hand-coding exercise that our treatments seem to put thoughts about bias and accuracy on top of people's minds.

[Insert Figure 4 here]

4.2 Motives for news demand

Our experimental findings and our model-based preference estimates suggest that both Biden and Trump voters have a preference for reading like-minded news that sometimes conflicts with their desire for accuracy in reporting.

To examine how people justify their demand for news, we collect direct data on people's motives for subscribing to the newsletter at the end of the main experiments. To get an unprompted response, we asked our respondents to answer an open-ended question on their motives for subscribing or not subscribing to the newsletter (the full instructions are provided in Section E.1.4 of the Online Appendix). This data provides us with a direct lens into people's reasoning about the motives underlying their subscription decision.

Text analysis In a first step, we use the methodology proposed by Gentzkow and Shapiro (2010) to identify phrases that are characteristic of responses to the open-ended questions of subscribers and non-subscribers across treatment arms (which we describe in detail at the end of Section 4.1). Figure 5 presents the 50 words with the largest χ^2 statistic for subscribers (Panel A) and non-subscribers (Panel B). Words that are more characteristic of justifications provided by respondents in the *left-wing* or *right-wing biased* treatments are shown in green, while words more characteristic of respondents in the *unbiased* treatments are shown in red. Panel A reveals that words related to "unbiased" are more diagnostic for subscribers in the *no bias* treatments, while subscribers in the *bias* treatments avoid using terms related to bias or accuracy. Panel B shows that these patterns are reversed for non-subscribers: Non-subscribers in the bias treatments justify their non-subscription with the political bias of the newsletter, while respondents in the *no bias* treatment arms again bring up more generic reasons, such as wanting to follow the news cycle or their interest in economic policy.

[Insert Figure 5 here]

Frequency of mentioning bias Motivated by the previous findings, we more closely examine respondents' tendency to justify their decision by referring to the political bias of the newsletter. Table 3 presents OLS regression estimates pooling respondents from Experiments 1 and 2. The dependent variable in columns 1–3 is a binary indicator taking value one if respondents mention the word "unbiased" or any of its synonyms in their responses to the open-ended question.[22] Subscribers who were assigned to the *left-wing bias* or the *right-wing bias* treatment arms are 4.1 percentage points less likely to utilize synonyms of "unbiased" (column 1, $p = 0.013$), a substantial effect compared to a control group mean of 7.8%. On the other hand, respondents in the *bias* treatments who did not subscribe to our newsletter are marginally more likely to mention synonyms of "unbiased" in their responses (column 2, $p = 0.051$). The opposite pattern emerges once we consider synonyms of "biased" and construct an analogous dependent variable taking value one if respondents utilized any of the following words: "biased", "partisan", "tendentious", or "slanted." Column 4 shows that subscribers are not significantly more

[22] The synonyms were obtained from the website thesaurus.com and include: "disinterested", "dispassionate", "equitable", "honest", "impartial", "neutral", "nonpartisan", "open-minded", "aloof", "cold", "equal", "even-handed", "fair", "nondiscriminatory", "objective", "on-the-fence", "straight", "unbigoted", "uncolored", "uninterested", "unprejudiced."

likely to mention synonyms of "biased." Yet, non-subscribers are 4.4 percentage points more likely to mention terms related to "biased" (column 5, $p < 0.001$), which is a substantial effect compared to the control group mean of 1.9%. Our data thus suggests that our treatments either affect the composition of respondents selecting into the newsletter subscription, or that respondents flexibly adjust their rationales for subscription in response to our treatments (see Bursztyn et al., 2022a,b, for evidence on the role of rationales in justifying socially stigmatized behavior). People's rationales for choosing to consume biased news do not actively feature the political bias of the newsletter, consistent with people providing rationales that allow them to maintain a positive self-image (Benabou and Tirole, 2006).

[Insert Table 3 here]

4.3 Discussion of alternative mechanisms

A key assumption for our structural model is that the treatments only affect people's news demand through their impact on beliefs about accuracy and political bias. In this section, we discuss potential alternative mechanisms driving our treatment effects, including cognitive constraints, cross-learning about entertainment, diversification motives, and experimenter demand effects.

Cognitive constraints Respondents in the *no bias* treatments might expect the articles from The Boston Herald to be more cognitively demanding as these articles may be more likely to cover more facts compared to respondents in any of the *bias* treatments. Alternatively, respondents might associate the unbiased newsletter with higher complexity if they think it is psychologically more costly to process and integrate conflicting pieces of evidence. The open-ended responses from Experiment 3 demonstrate that complexity was not on top of people's minds when interpreting the treatment variation: Only two out of 388 respondents thought The Boston Herald only reported one statistic to reduce the complexity of the article or to make it easier to understand. If we consider the structured post-treatment beliefs measures from the main experiments, there is some evidence that Biden voters in the *bias* treatments associate the newsletter with lower complexity (as shown in Table B.12). However, since people did not talk about complexity in the open-ended responses, a likely explanation is that

these respondents changed their beliefs about the complexity of the newsletter only when prompted to think about it *after* deciding whether to subscribe to the newsletter. Furthermore, several patterns in our data are inconsistent with cognitive constraints driving the treatment effects. First, explanations based on cognitive constraints predict a similarly sized decrease in demand irrespective of the direction of the political bias. As shown in columns 2 and 6 of Table B.12, the magnitudes of treatment effects on perceptions of complexity among Biden voters are almost identical across the two experiments. Yet, inconsistent with a story based on cognitive constraints, Biden voters only significantly reduce their demand for the newsletter in response to the *right-wing bias* treatment. Second, Trump voters do not significantly update their beliefs about the complexity of the newsletter—even when prompted to think about it—making it unlikely that cognitive constraints differentially affected newsletter demand across treatment groups.

Entertainment motives It is conceivable that the treatments may affect perceptions of the newsletter's entertainment value. For instance, people might think that balanced reporting is less likely to lead to feelings of surprise and suspense (Ely et al., 2015). The open-ended responses from Experiment 3 demonstrate that entertainment was not on top of people's minds when interpreting the treatment variation: Only three out of 388 respondents mentioned entertainment in their responses. Turning to the structured post-treatment beliefs measures, we find some evidence that respondents update about the entertainment value of the newsletter (as shown in Table B.12). However, the lack of references to entertainment motives in Experiment 3 suggests that people only adjusted their beliefs about entertainment *ex-post* when they were prompted to specifically think about this dimension. Furthermore, the structured post-treatment belief measures in Experiment 4 show that only Biden voters significantly updated their beliefs about the entertainment value of the newsletter when there was no scope for ex-post rationalization of the newsletter subscription decision (Table B.16). Finally, conceptually disentangling belief utility and entertainment utility is not straightforward since part of the utility from belief confirmation might relate to the entertainment value of having your beliefs confirmed. If biased news were perceived to be more entertaining in general, unrelated to any form of belief confirmation utility, we would not expect to see any political heterogeneity in treatment effects.

Diversification motive People's news demand might be driven by the objective to read news articles from a diversified portfolio of outlets with an average ideological bias that is close to zero. Even if any individual outlet covers the news with a political bias, combining the signals across sources might allow people to obtain a more objective assessment of the state of the world. Importantly, this motive hinges on people's news consumption outside the experiment, but not on people's political views. To assess the plausibility of this mechanism, we asked respondents pre-treatment to indicate all news outlets from which they have received news over the past 12 months using a list of 21 popular outlets across the political spectrum. We then classify each outlet as either left-wing or right-wing biased, and then split the sample into respondents who, (i), do not read news from any of these outlets, (ii), who read more left-wing than right-wing sources, and, (iii), those who read more right-wing than left-wing sources. We then separately estimate treatment effects on people's newsletter demand in Experiment 1 and 2 for each of these three subgroups (as shown in Table B.14).

First, the diversification motive would predict a positive treatment effect whenever the perceived bias of The Boston Herald shifts away from the bias of the majority of outlets that a respondent currently reads. In contrast, column 2 shows a statistically significant decrease in demand among respondents who mainly read left-wing biased outlets in Experiment 1 where we increase the perceived right-wing bias of The Boston Herald. In the symmetric case in Experiment 2, we find a negative point estimate, although the small sample size limits the statistical power in this case (column 6). Second, diversification would predict a negative treatment effect among people who do not read news from any other source, for which we only find mixed empirical support (columns 1 and 4). Taken together, this suggests that a diversification motive alone is insufficient to rationalize our patterns of treatment effects.

Experimenter demand effects It is possible that respondents in the different treatment groups hold different beliefs about the experimenter's expectations. However, we do not believe that experimenter demand is a major concern in our setting. First, our experimental manipulation is implicit in nature as we only factually state the newspaper reporting without framing it in terms of bias or accuracy. Second, the patterns of heterogeneity by political ideology and experiments suggest that our patterns could only be explained by heterogeneously occurring demand effects. Third, trying to please the experimenter by signing up for an unwanted newsletter is a costly action as it

entails receiving weekly emails with unwanted content for a month. Fourth, recent evidence suggests that experimental subjects respond only moderately to explicit signals about the experimenter's expectations, indicating a limited quantitative importance of experimenter demand effects (de Quidt et al., 2018; Mummolo and Peterson, 2018).

5 Concluding remarks

In this paper, we conducted several large-scale experiments with American voters to quantify the relative importance of accuracy concerns and belief confirmation motives in driving the demand for news. For this purpose, we designed experiments in which we vary whether a news outlet reports the news in a right-wing biased, left-wing biased, or politically unbiased way. This allows us to examine cases where there is a trade-off between accuracy concerns and belief confirmation motives as well as cases where there is no such trade-off and the two motives should shift demand in the same direction. We then study people's demand for a newsletter featuring articles from this outlet. Our main finding is that both Biden and Trump voters strongly reduce their demand for politically biased news, but only if the bias is inconsistent with their own political views: Trump voters strongly reduce their demand for left-wing biased news, but not for right-wing biased news. The reverse patterns hold for Biden voters. The political heterogeneity is consistent with the predictions of behavioral models of news demand in which readers trade off accuracy concerns against belief confirmation motives. In a second step, we quantify the relative importance of accuracy and belief confirmation motives by using the experimental variation in perceptions of accuracy and political bias to estimate a parsimonious discrete-choice model. The estimates of the key preference parameters reveal that people attach about equal weights to accuracy and belief confirmation motives, suggesting that both motives play a key role in shaping news demand.

These findings provide empirical support for demand-side explanations of media bias, such as behavioral models where media bias is the equilibrium outcome of firms catering to the demand-side preference for like-minded news. Using the stylized model by Mullainathan and Shleifer (2005) as an example, the preference estimates from our structural model imply that a monopolist will slant the news 50% towards the prior belief of the *average* reader. In such models, efforts to increase competition—such as limiting ownership concentration—tend to exacerbate media bias. Of course, it seems

plausible that the amount of media bias in equilibrium will be shaped by other forces as well. For example, uncertainty about a news outlet's quality creates incentives to build up a reputation for accurate reporting (Gentzkow and Shapiro, 2006). Competition tends to reinforce these reputation incentives, thus decreasing equilibrium bias. Whether regulation aimed at increasing competition in media markets will ultimately increase or decrease media bias thus depends on the relative importance of these different forces shaping media bias and how competition interacts with these forces. While determining the relative importance of these forces is beyond the scope of our study, our estimates of people's relative preferences over accuracy and political bias in reporting represent a necessary input for such an exercise.

This paper studies the demand for political news, where the relative importance of accuracy concerns and belief confirmation motives is of particular interest as informed citizens are a necessary input to the functioning of democratic institutions. However, it is plausible to expect that the relative preference for accuracy in reporting is stronger in news domains where the costs of being misinformed are primarily borne by the reader—rather than arising in the form of a political externality. Future research should thus explore how people resolve the trade-off between accuracy concerns and belief confirmation in other domains, such as financial news.

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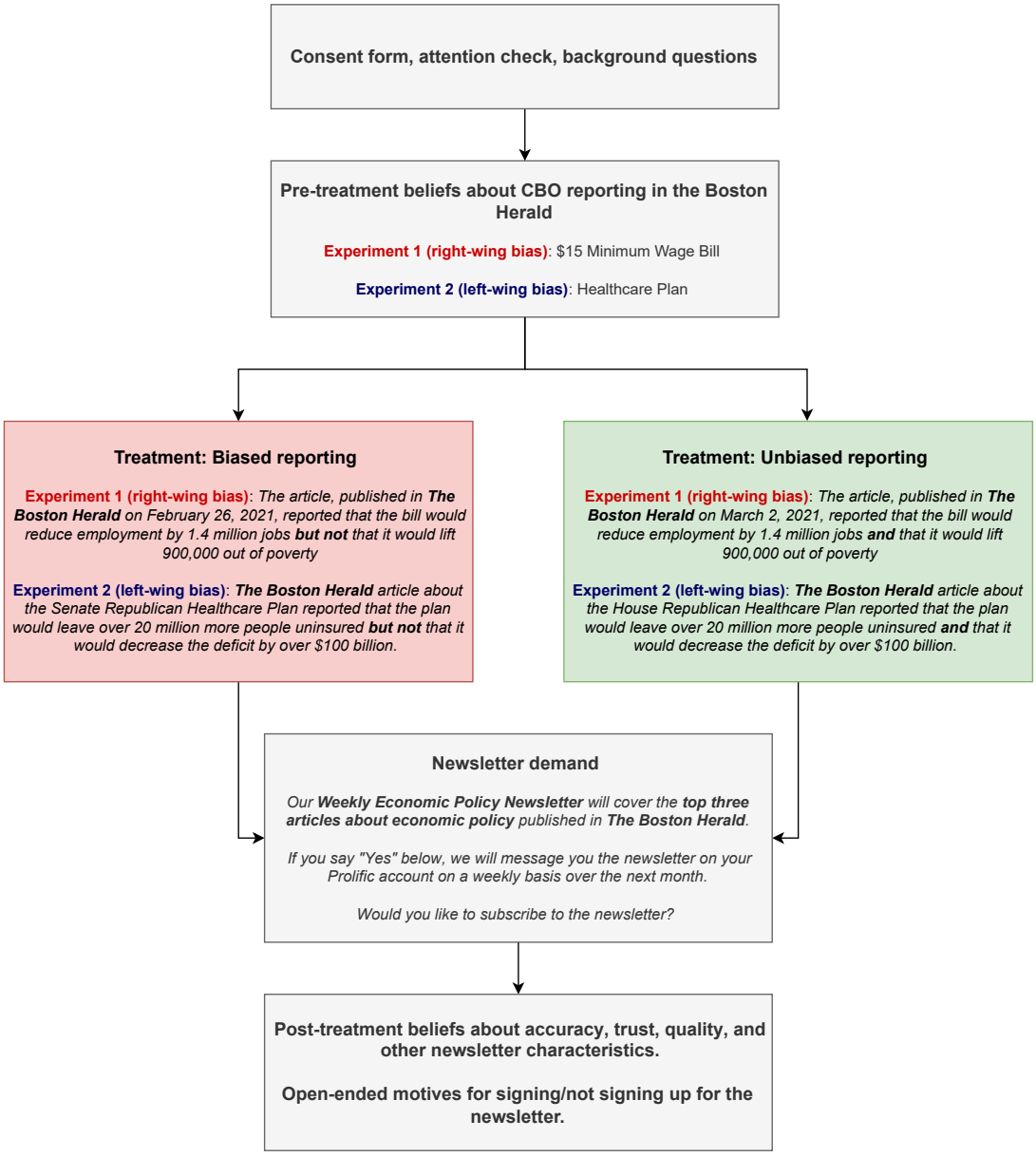
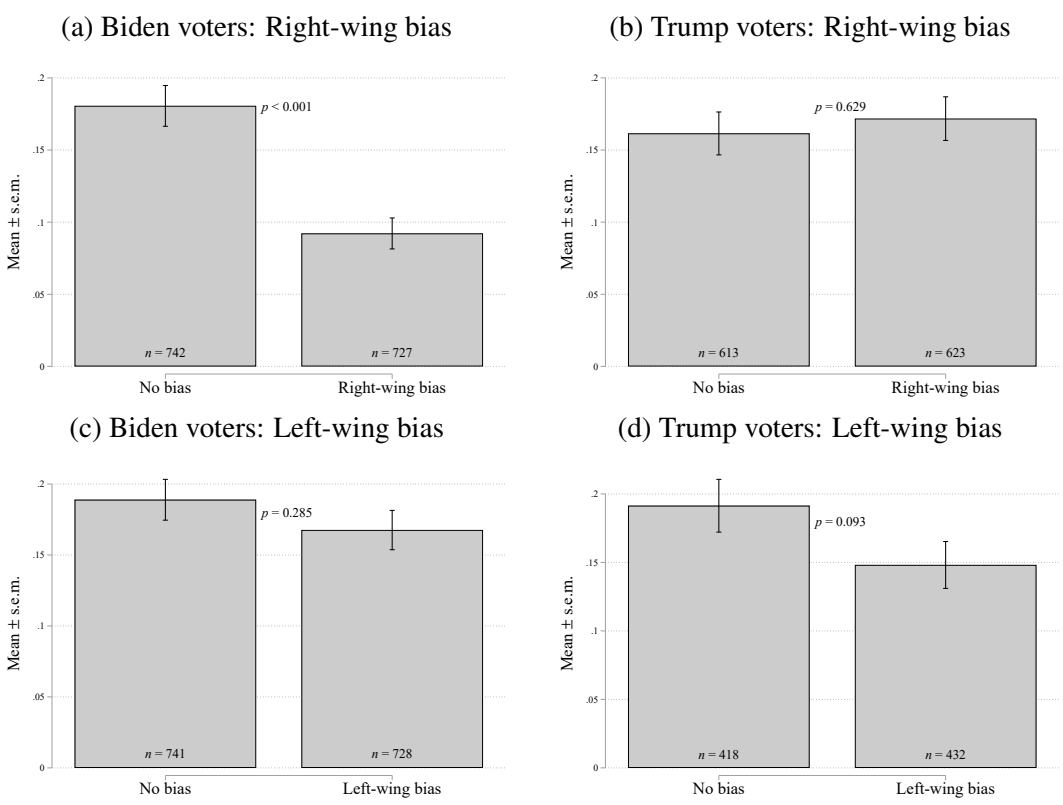
Main figures and tables

Figure 1: Overview of the experimental design



Note: This figure provides an overview of the main design features of Experiment 1 (right-wing bias) and Experiment 2 (left-wing bias). Appendix Section E contains the full experimental instructions.

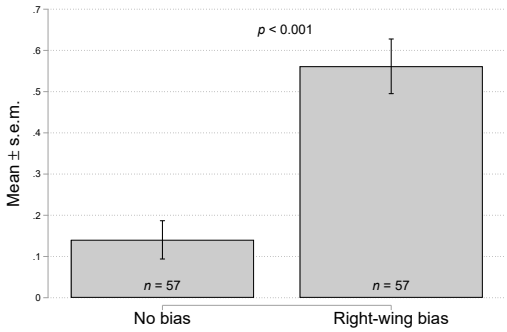
Figure 2: Newsletter demand by treatment and voting status



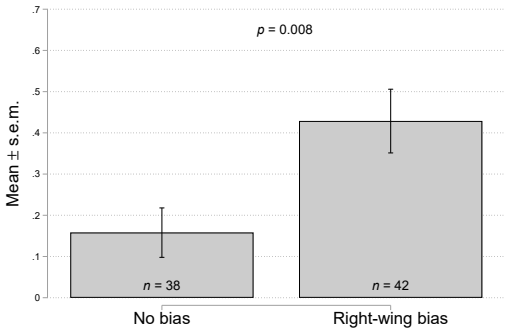
Note: This figure presents the share of respondents who chose to subscribe to the weekly politics newsletter by treatment and voting status. Panel (a) and Panel (b) present results from Experiment 1. Panel (c) and Panel (d) present results from Experiment 2. Panel (a) and Panel (c) focus on the subsample of respondents who voted for Joe Biden, while Panel (b) and Panel (d) focus on respondents who voted for Donald Trump. The p-values are obtained from a two-sample t-test of equality of means. Standard errors of the mean are shown.

Figure 3: Treatment effects on mentioning political bias in the open-ended responses

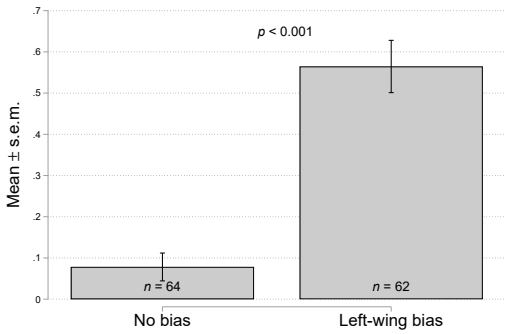
(a) Biden voters: Right-wing bias



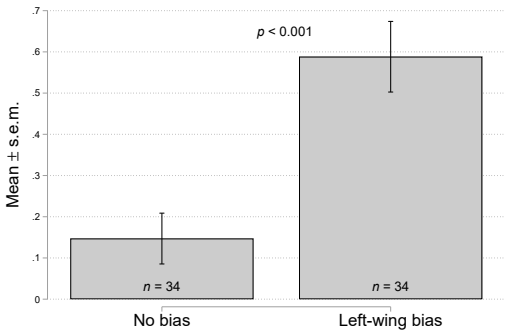
(b) Trump voters: Right-wing bias



(c) Biden voters: Left-wing bias



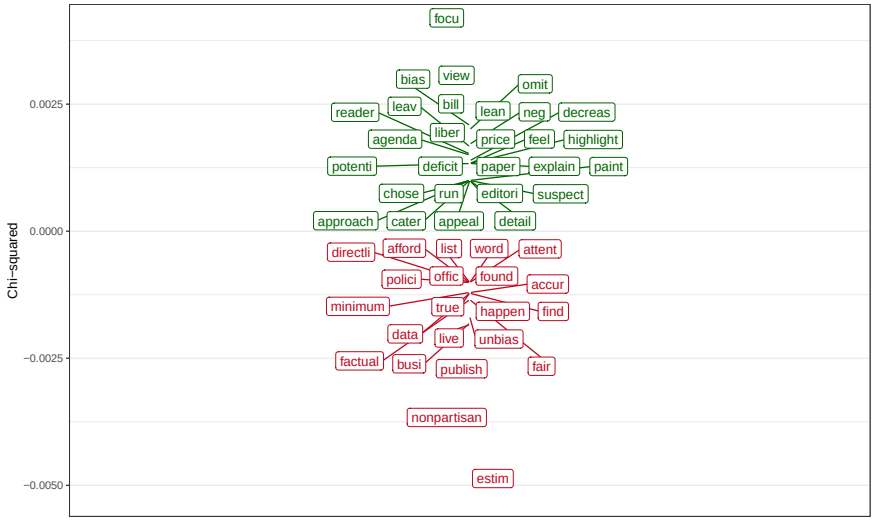
(d) Trump voters: Left-wing bias



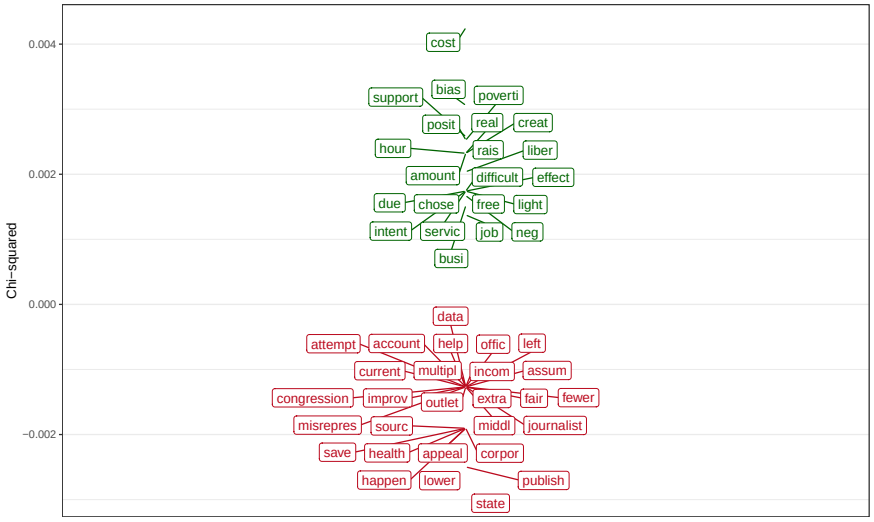
Note: The figure presents treatment effects on whether respondents mention political bias in their responses to the open-ended motives question in Experiment 3 (see Table B.1). Specifically, respondents were asked why they think The Boston Herald reported in the way it did. Each panel displays the share of respondents whose responses were hand-coded to the "bias" category (example responses: "I think it's biased reporting," "Perhaps they are a Republican newspaper," "I believe it is a left-leaning newspaper," or "They clearly support the Democrats" would all be classified as "biased"). Panel (a) and Panel (b) compare the *right-wing bias* treatment to the *no bias* treatment (analogous to Experiment 1). Panel (c) and Panel (d) compare the *left-wing bias* treatment to the *no bias* treatment (analogous to Experiment 2). Panel (a) and Panel (c) focus on the subsample of respondents who voted for Joe Biden, while Panel (b) and Panel (d) focus on respondents who voted for Donald Trump. The p-values are obtained from a two-sample t-test of equality of means. Standard errors of the mean are shown.

Figure 4: Perceived motives for reporting: Most distinctive phrases

(a) Biden voters



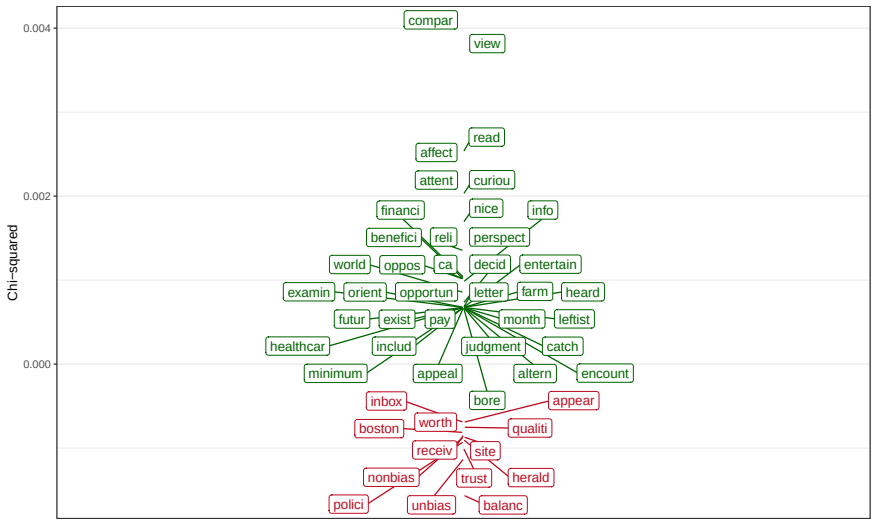
(b) Trump voters



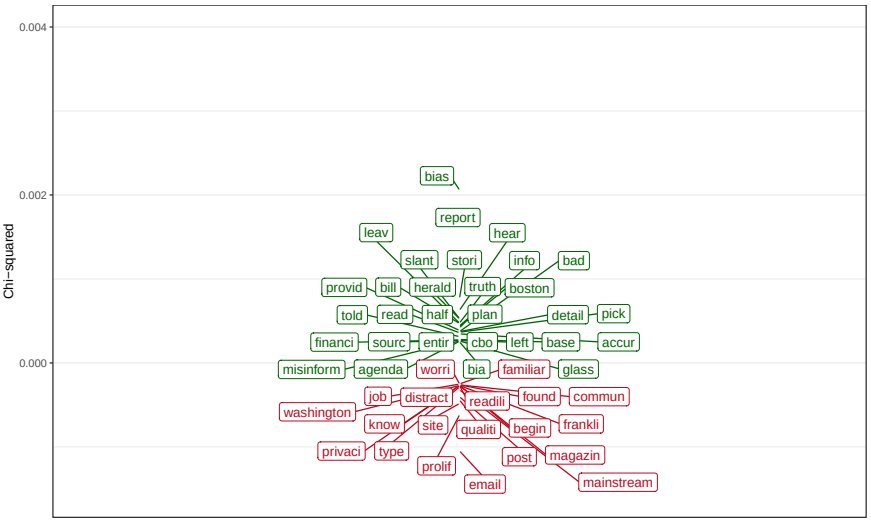
Note: This figure uses data from the mechanism experiment in which we measured perceived motives for the reporting strategy of The Boston Herald using open-ended questions (Experiment 3, see Table B.1). The figure displays the 50 phrases with the largest χ^2 statistic using the method proposed by Gentzkow and Shapiro (2010). We exclude stop words and reduce all words to their stem using the Porter stemmer. Panel (a) uses responses to the open-ended motives question from Biden voters, while Panel (b) uses responses from Trump voters to calculate the χ^2 statistics. Phrases with a positive χ^2 statistic are more distinctive of responses in the *biased* treatment arms (in green). Phrases with a negative χ^2 statistic are more distinctive of responses in the *unbiased* treatment arm (in red). The terms "cbo" and "report", which have χ^2 values of -0.0126 and -0.0098, were omitted to better scale the other phrases.

Figure 5: Motives for news demand: Most distinctive phrases

(a) Distinctive phrases of subscribers by treatment status



(b) Distinctive phrases of non-subscribers by treatment status



Note: This figure uses respondents' answers to the open-ended question of why they subscribed (or did not subscribe) to the newsletter from Experiment 1 and 2 (see Table B.1). Panel (a) uses responses to the open-ended questions from respondents who subscribed to the newsletter on why they subscribed to the newsletter, while Panel (b) uses responses from respondents who did not subscribe on why they did not subscribe to the newsletter. Each panel displays the 50 phrases with the largest χ^2 statistic using the method proposed by Gentzkow and Shapiro (2010). We exclude stop words and reduce all words to their stem using the Porter stemmer. Phrases with a positive χ^2 statistic are more distinctive of open-responses in the *left-wing* or *right-wing biased* treatment arms (in green). Phrases with a negative χ^2 statistic are more distinctive of responses in the *unbiased* treatment arm (in red).

Table 1: Main results: The demand for biased news

	Experiment 1: Right-wing bias			Experiment 2: Left-wing bias		
	(1) Accuracy	(2) Left-wing bias	(3) Demand	(4) Accuracy	(5) Left-wing bias	(6) Demand
Panel A: Trump voters						
Treatment (a)	-0.165***	-0.490***	0.005	-0.542***	0.266***	-0.052**
	(0.056)	(0.063)	(0.020)	(0.072)	(0.072)	(0.024)
N	1,235	1,235	1,236	849	849	850
Z-scored	Yes	Yes	No	Yes	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0	0	0.162	0	0	0.191
Panel B: Biden voters						
Treatment (b)	-0.903***	-0.849***	-0.086***	-0.720***	0.305***	-0.026
	(0.057)	(0.061)	(0.017)	(0.055)	(0.059)	(0.019)
N	1,464	1,464	1,469	1,466	1,466	1,469
Z-scored	Yes	Yes	No	Yes	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0	0	0.181	0	0	0.189
p-value: a = b	0.000	0.005	0.001	0.073	0.947	0.395

Note: This table presents OLS regression estimates using data from Experiment 1 (columns 1–3) and Experiment 2 (columns 4–6) where the dependent variables are post-treatment beliefs about accuracy (columns 1 and 4), the perceived left-wing bias of the newsletter (columns 2 and 5), and newsletter demand (columns 3 and 6). Panel A and Panel B present results for Biden and Trump voters, respectively. "Treatment" is a binary variable taking value one for respondents assigned the right-wing bias (columns 1–3) or the left-wing bias (columns 4–6) treatment arm, and zero for respondents in the no bias treatment arm. "Demand" is a binary variable taking value one for respondents who said "Yes" to receiving the weekly newsletter, and zero for those who said "No." "Accuracy" of the newsletter is measured on a 5-point Likert scale from "Very inaccurate" to "Very accurate." "Left-wing bias" is measured on a 5-point Likert scale from "Very right-wing biased" to "Very left-wing biased." "Accuracy" and "Left-wing bias" have been z-scored using the relevant control group mean and standard deviation. All regressions include a set of basic control variables: gender, age, education, race and ethnicity, log income, employment status, Census region, voting, political affiliation, ideology, interest in economic news, whether they have read any of a list of 21 newspapers during the last 12 months, whether they have read The Boston Herald, whether they currently subscribe to any newsletters, and their pre-treatment beliefs about how The Boston Herald reported about the CBO findings.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 2: Structural model: Preferences for accuracy and biased news

	Parameter estimates:		
	(1) Full sample	(2) Biden voters	(3) Trump voters
Preference for accuracy (α)	0.255***	0.225**	0.267
	(0.078)	(0.088)	(0.189)
Preference for belief confirmation (β)	0.360***	0.405***	0.191
	(0.096)	(0.116)	(0.163)
Implicit weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	0.414***	0.357***	0.584**
	(0.111)	(0.131)	(0.270)
N	5,014	2,930	2,084

Note: This table presents the parameter estimates of the discrete choice model outlined in equations 2, 3 and 4 in Section 3.3. Column 1 presents parameter estimates for the full sample, while columns 2 and 3 present estimates for Biden and Trump voters, respectively. Specifically, we estimate an IV probit model using Newey's (1987) two-step estimator as implemented by Stata's `ivprobit` routine. We use data from Experiments 1 and 2 where we elicit newsletter subscription choices and perceptions within-subject. The dependent variable is a binary indicator taking value one for respondents who choose to sign up to the newsletter. The endogenous regressors are z-scored perceptions of quality and belief confirmation. We instrument these perceptions with a saturated set of treatment status indicators. In column 1, we also include interactions of the treatment assignment with a binary indicator for whether a respondent voted for Trump as instruments to capture differential first-stage effects of the treatments. We include a binary indicator for whether a respondent voted for Trump as a control variable in column 1.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table 3: Motives for subscription vs. non-subscription to the newsletter

	Mentions at least one synonym of:					
	Unbiased			Biased		
	(1)	(2)	(3)	(4)	(5)	(6)
Biased	-0.041**	0.009*	0.009*	0.002	0.044***	0.044***
	(0.016)	(0.005)	(0.005)	(0.015)	(0.006)	(0.006)
News demand			0.061***			0.026**
			(0.013)			(0.010)
Biased x News demand			-0.049***			-0.042**
			(0.017)			(0.016)
N	789	4,052	4,841	789	4,052	4,841
Sample	Subscriber	Non-subscriber	All	Subscriber	Non-subscriber	All
Control group mean	0.078	0.017	0.028	0.046	0.019	0.024

Note: This table presents OLS regression estimates pooling respondents from Experiment 1 and 2 where the dependent variables are binary indicators for whether respondents mentioned synonyms of "unbiased" (columns 1–3) or "biased" (columns 4–6). Specifically, the dependent variable in columns 1–3 is a binary indicator taking value one if respondents mention the word "unbiased" or any of its synonyms in their open response to the question why they subscribed (did not subscribe) to the newsletter. The synonyms are "disinterested", "dispassionate", "equitable", "honest", "impartial", "neutral", "nonpartisan", "open-minded", "aloof", "cold", "equal", "even-handed", "fair", "nondiscriminatory", "objective", "on-the-fence", "straight", "unbigoted", "uncolored", "uninterested", "unprejudiced." Synonyms are taken from the website thesaurus.com. The dependent variable in columns 4–6 is constructed analogously using "biased" and any of the following synonyms: "partisan", "tendentious", "slanted." "Biased" is a binary indicator taking value one for respondents assigned to the "left-wing bias" or the "right-wing bias" treatment arms, and zero otherwise. "News demand" is a binary variable taking value one for respondents who said "Yes" to receiving the weekly newsletter, and zero for those who said "No." Columns 1 and 4 focus on the subsample respondents who subscribed to the newsletter, while columns 2 and 5 focus on those who did not subscribe. Columns 3 and 6 include all respondents. All regressions include experiment fixed effects.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

For online publication only:

The Demand for News: Accuracy Concerns versus Belief Confirmation Motives

Felix Chopra, Ingar Haaland, and Christopher Roth

Section A presents theoretical results.

Section B contains additional tables and figures.

Section C contains information about our preregistration and the ethics approval.

Section D contains additional details about the publication and distribution of our weekly newsletter.

Section E provides screenshots of the experimental instructions.

A Theoretical appendix

This section formalizes the intuition that our active control designs in Experiment 1 and 2 (see Section 2) should *decrease* the perceived Blackwell informativeness of Boston Herald articles in the narrow context of reporting about CBO findings. Proposition 3 below outlines sufficient conditions for the *left-wing biased* and *right-wing biased* treatment to strictly *decrease* the perceived Blackwell informativeness compared to the respective *no bias* treatment. As a result, this provides us with the empirical prediction that for neoclassical agents that care only about the accuracy of news reporting, our treatments should decrease newsletter demand.

While it seems intuitively reasonable that reporting both statistics is more informative than selectively reporting only one statistic in our context, it is important to emphasize that there is in general no normative benchmark on the reporting of facts when full disclosure is *not* possible. For example, suppose that a newsletter receives three signals, $(s_1, s_2, s_3) = (L, R, R)$, about an unobserved state θ, but can only report one signal. From the reader's perspective, the optimal reporting rule will depend on the prior beliefs and the cost of making a Type I and Type II error when conditioning actions on one's belief about θ (a point made by Suen 2004). Thus, readers with different priors prefer different reporting rules, making it not possible to define a complete ordering of reporting rules in terms of their informativeness.

We therefore chose to focus on a setting where it seems ex-ante very likely that news outlets are not constrained in whether they report only one or both of the main findings from the CBO reports.[1] Thus, when evaluating the reporting of The Boston Herald only in the narrow sense of how it covers CBO reports, an increase in the probability of reporting both statistics necessarily increases its informativeness in the Blackwell sense. Below, we outline the formal argument.

Setup There is a binary state space $\Theta = \{L, R\}$ with a typical element denoted by θ and an agent with prior belief $q \in \Delta(\Theta)$ about the hidden state. The agent has the option to acquire information from a news outlet (The Boston Herald), which

[1] For example, we verified that all top 15 US newspapers by circulation (as of June 2019) reported both findings from the CBO report about the Healthcare Plan, suggesting that news outlets do not face binding constraints that would require them to choose between reporting either the effects on the deficit or the effects on the number of uninsured.

publishes a newsletter n that is informative about the state θ. To introduce scope for information suppression, we assume that the news outlet receives a set of private signals $s = \{s_1, \ldots, s_K\} \in S$ about θ. The set consists of K binary signals $s_i \in \Theta$, where $K \in \mathbb{N}$ is drawn randomly and independently of θ. The signals, s_i, take value L with probability p_θ where $p_R < p_L$, and value R otherwise. The news outlet can disclose any subset of s in its newsletter n, i.e. $n \subseteq s$. Note that this implies that it cannot distort individual signals but only choose to suppress a subset of signals. In our experiments, The Boston Herald received two conflicting signals from the Congressional Budget Office about the consequences of the $15 Minimum Wage Bill (Experiment 1) or the consequences of the healthcare plan (Experiment 2), i.e. $s = \{L, R\}$ in both experiments.

Informativeness The source signal can thus be represented as an information structure (S, π) with state-dependent likelihood $\pi : \Theta \to \Delta(S)$. We are agnostic about the news outlet's incentives to suppress information, subsuming them in the reader's belief $\rho : S \to \Delta(N)$ about how the news outlet reports conditional on s. From the agent's perspective, the informativeness of n is an invariant of the state-dependent distribution over news articles, $\sigma : \Theta \to \Delta(N)$, induced by the agent's belief about the quality of the news outlet's source, π, and the belief about how the news outlet reports, ρ. Consider two articles n and n' with distributions $\sigma, \sigma' : \Theta \to \Delta(N)$. We use Blackwell's (1951) notion of informativeness and say that n is *(Blackwell) more informative* than n' if (n, σ) is *sufficient* for (n', σ'), that is: there is a stochastic transformation τ such that n' and $\tau(n)$ are identically distributed. Intuitively, we obtain n' by adding noise to n. This is the benchmark for evaluating the informativeness of an information structure: any agent with access to an article n that is more informative than n' can attain an expected payoff at least as large as the maximal expected payoff attainable with n', regardless of the prior q and the decision problem $a \in A$ with payoffs $u(a, \theta)$ (Blackwell, 1953). This provides the prediction that the demand for news should be strictly increasing in the perceived informativeness of the news.

How does strategic suppression of signals affect the informativeness of news? Suppose the news outlet received the signals $s = \{s_1, \ldots, s_K\}$ and let $\sigma(s' \mid s)$ denote the agents' belief that the news outlet would report $s' \subseteq s$ after receiving s. Intuitively, the informativeness of the article n should be strictly increasing in the probability of fully conveying the set of signals. Indeed, the Blackwell informativeness strictly increases if we decrease the probability $\sigma(s' \mid s)$ of reporting a filtered signal $s' \subsetneq s$ and instead

increase the probability of full information transmission, $\sigma(s \mid s)$.

Proposition 3 (Informativeness)**.** Fix $s = \{s_1, \ldots, s_K\} \in S$ and two reporting strategies $\rho, \rho' : S \to \Delta(N)$. Let $\sigma, \sigma' : \Theta \to \Delta(N)$ be the information structures induced by combining the source signal $\pi : \Theta \to \Delta(S)$ with the reporting strategies, respectively. Suppose that

(i) $\rho(s \mid s) \geq \rho'(s \mid s)$,

(ii) $\rho(t \mid s) \leq \rho'(t \mid s)$ for all $t \subsetneq s$,

(iii) $\rho(\cdot \mid s') = \rho'(\cdot \mid s')$ for all $s' \neq s$.

Then the information structure σ is Blackwell more informative than σ'.

Proof. It suffices to show that the conclusion obtains if we strengthen the assumption by additionally assuming that $\rho(t \mid s) < \rho'(t \mid s)$ for some $t \subsetneq s$ and that for all other $t' \subsetneq s$ with $t' \neq t$, we have $\rho(t' \mid s) = \rho'(t' \mid s)$. The general case then follows by applying the result to the sequence $\rho = \rho_1, \ldots, \rho_L = \rho'$ where ρ_k and ρ_{k+1} differ at most on the set $\{s, s'\}$ for some $s' \subseteq s$ and $L = |\mathscr{P}(s)|$. Suppose that $n \in N$ is a random variable with state-dependent distribution σ. To show that σ is Blackwell more informative than σ', it suffices to construct an n-measurable random variable $n' \in N$ with state-dependent distribution σ', thereby establishing statistical sufficiency. We construct n' as follows: let $n' = n$ whenever $n \neq s$ and set $\beta = \rho'(s \mid s)/\rho(s \mid s)$. If $n = s$, then n' takes value s with probability β and value t with probability $1 - \beta$. One can then verify that conditional on the state $\theta \in \Theta$, the distribution of n' is $\sigma'(\cdot \mid \theta)$. This concludes the proof. □

In our active control group designs, the *right-wing bias* and the *left-wing bias* treatment exogenously decrease the probability $\rho(s \mid s)$ of reporting both statistics from the CBO report compared to the *no bias* treatment, while increasing the probability of selective reporting. By Proposition 3, this means that respondents in the *right-wing bias* and the *left-wing bias* should perceive the newsletter as strictly less informative compared to respondents in the *no bias* treatment.

B Additional tables and figures

Table B.1: Overview of experiments

Experiment	Sample	Treatment Arms	Main Outcomes
Experiment 1: Right-wing bias vs. no bias (November 2021)	Prolific: n = 2,705 AsPredicted ID: #78800	**Right-wing bias treatment**: Information about how The Boston Herald covered only one statistic from the CBO report on the Minimum Wage Bill **No bias treatment**: Information about how The Boston Herald covered both statistics from the CBO report on the Minimum Wage Bill	Demand for a newsletter covering the top 3 articles from The Boston Herald; Post-treatment beliefs about newsletter characteristics
Experiment 2: Left-wing bias vs. no bias (December 2021)	Prolific: n = 2,319 AsPredicted ID: #80266	**Left-wing bias treatment**: Information about how The Boston Herald covered only one statistic from the CBO report on the Healthcare Bill **No bias treatment**: Information about how The Boston Herald covered both statistics from the CBO report on the Healthcare Bill	Demand for a newsletter covering the top 3 articles from The Boston Herald; Post-treatment beliefs about newsletter characteristics
Experiment 3: Mechanisms on interpretation of treatment (February 2022)	Prolific: n = 388 AsPredicted ID: #87947	**Bias treatments**: Information about how The Boston Herald covered one statistic from the CBO report on the Healthcare Bill/Minimum Wage Bill **No bias treatments**: Information about how The Boston Herald covered both statistics from the CBO report on the Healthcare Bill/Minimum wage bill	Open-ended question on why The Boston Herald reported the statistics in this particular way
Experiment 4: First-stage Experiment (February 2022)	Prolific: n = 1,910 AsPredicted ID: #89081	**Bias treatments**: Information about how The Boston Herald covered one statistic from the CBO report on the Healthcare Bill/Minimum Wage Bill **No bias treatments**: Information about how The Boston Herald covered both statistics from the CBO report on the Healthcare Bill/Minimum wage bill	Post-treatment beliefs about accuracy and bias

Note: This table provides an overview of all experiments.

Table B.2: Summary statistics

	(1) Experiment 1	(2) Experiment 2	(3) Experiment 3	(4) Experiment 4
Male	0.468	0.436	0.479	0.481
Age	35.487	36.304	35.737	38.829
White	0.834	0.840	0.827	0.821
Income	70857.671	68799.051	69574.742	69979.058
College degree	0.649	0.678	0.683	0.695
Full-time work	0.490	0.535	0.534	0.497
Northeast	0.174	0.194	0.157	0.189
Midwest	0.231	0.235	0.206	0.204
West	0.206	0.173	0.224	0.211
South	0.389	0.398	0.412	0.396
Voted for Trump	0.457	0.367	0.381	0.493
Observations	2,705	2,319	388	1,910

Note: This table displays the mean value of basic covariates for each experiment (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time work" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise. "Voted for Trump" is a binary variable taking value one if the respondent voted for Donald Trump in the 2020 US presidential election, and zero if the respondent voted for Joe Biden.

Table B.3: Test of balance of treatment vs. control: Experiment 1

	Treatment (T)	Control (C)	P-value (T - C)	Observations
Male	0.47	0.47	0.949	2705
Age	35.80	35.18	0.239	2705
White	0.84	0.83	0.789	2705
Income (midpoint)	72051.85	69667.90	0.169	2705
College degree	0.65	0.65	0.817	2705
Full-time employee	0.49	0.49	0.953	2705
Northeast	0.17	0.18	0.795	2705
Midwest	0.23	0.24	0.589	2705
West	0.21	0.20	0.774	2705
South	0.39	0.39	0.666	2705

Note: This table provides a balance test between the treatment and control group using all respondents from Experiment 1 (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income (midpoint)" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time employee" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise.

Table B.4: Test of balance of treatment vs. control: Experiment 2

	Treatment (T)	Control (C)	P-value (T - C)	Observations
Male	0.41	0.46	0.018	2319
Age	36.24	36.36	0.837	2319
White	0.85	0.83	0.456	2319
Income (midpoint)	68377.16	69221.31	0.643	2319
College degree	0.67	0.68	0.514	2319
Full-time employee	0.55	0.52	0.253	2319
Northeast	0.20	0.19	0.682	2319
Midwest	0.24	0.23	0.816	2319
West	0.17	0.18	0.776	2319
South	0.39	0.40	0.754	2319

Note: This table provides a balance test between the treatment and control group using all respondents from Experiment 2 (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income (midpoint)" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time employee" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise.

Table B.5: Test of balance of treatment vs. control: Biden voters in Experiment 1

	Treatment (T)	Control (C)	P-value (T - C)	Observations
Male	0.50	0.50	0.940	1469
Age	31.50	31.56	0.911	1469
White	0.78	0.75	0.206	1469
Income (midpoint)	70818.43	69524.93	0.590	1469
College degree	0.66	0.67	0.780	1469
Full-time employee	0.47	0.48	0.761	1469
Northeast	0.18	0.19	0.634	1469
Midwest	0.23	0.22	0.517	1469
West	0.26	0.25	0.640	1469
South	0.33	0.34	0.544	1469

Note: This table provides a balance test between the treatment and control group focusing only on respondents in Experiment 1 who voted for Joe Biden (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income (midpoint)" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time employee" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise.

Table B.6: Test of balance of treatment vs. control: Trump voters in Experiment 1

	Treatment (T)	Control (C)	P-value (T - C)	Observations
Male	0.43	0.43	0.966	1236
Age	40.81	39.55	0.128	1236
White	0.90	0.93	0.081	1236
Income (midpoint)	73491.17	69840.95	0.144	1236
College degree	0.63	0.63	0.985	1236
Full-time employee	0.51	0.50	0.822	1236
Northeast	0.16	0.15	0.848	1236
Midwest	0.22	0.26	0.137	1236
West	0.15	0.15	0.968	1236
South	0.47	0.44	0.243	1236

Note: This table provides a balance test between the treatment and control group focusing only on respondents in Experiment 1 who voted for Donald Trump (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income (midpoint)" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time employee" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise.

Table B.7: Test of balance of treatment vs. control: Biden voters in Experiment 2

	Treatment (T)	Control (C)	P-value (T - C)	Observations
Male	0.47	0.51	0.150	1469
Age	36.49	37.28	0.264	1469
White	0.80	0.79	0.827	1469
Income (midpoint)	68990.38	70006.75	0.660	1469
College degree	0.72	0.74	0.462	1469
Full-time employee	0.57	0.55	0.423	1469
Northeast	0.24	0.23	0.665	1469
Midwest	0.23	0.23	0.854	1469
West	0.16	0.17	0.735	1469
South	0.37	0.38	0.782	1469

Note: This table provides a balance test between the treatment and control group focusing only on respondents in Experiment 2 who voted for Joe Biden (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income (midpoint)" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time employee" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise.

Table B.8: Test of balance of treatment vs. control: Trump voters in Experiment 2

	Treatment (T)	Control (C)	P-value (T - C)	Observations
Male	0.31	0.37	0.053	850
Age	35.83	34.74	0.254	850
White	0.92	0.90	0.317	850
Income (midpoint)	67343.75	67828.95	0.870	850
College degree	0.58	0.58	0.991	850
Full-time employee	0.51	0.48	0.370	850
Northeast	0.13	0.13	0.823	850
Midwest	0.25	0.25	0.904	850
West	0.18	0.18	0.960	850
South	0.44	0.44	0.828	850

Note: This table provides a balance test between the treatment and control group focusing only on respondents in Experiment 2 who voted for Donald Trump (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income (midpoint)" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time employee" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise.

Table B.9: Test of balance of treatment vs. control: Experiment 3

	Treatment (T)	Control (C)	P-value (T - C)	Observations
Male	0.49	0.47	0.758	388
Age	35.12	36.36	0.364	388
White	0.82	0.84	0.533	388
Income (midpoint)	71576.92	67551.81	0.383	388
College degree	0.70	0.67	0.540	388
Full-time employee	0.54	0.53	0.845	388
Northeast	0.16	0.16	0.924	388
Midwest	0.23	0.18	0.230	388
West	0.19	0.26	0.102	388
South	0.42	0.40	0.744	388

Note: This table provides a balance test between the treatment and control group using all respondents from Experiment 3 (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income (midpoint)" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time employee" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise.

Table B.10: Test of balance of treatment vs. control: Experiment 4

	Treatment (T)	Control (C)	P-value (T - C)	Observations
Male	0.48	0.48	0.968	1910
Age	38.63	39.03	0.554	1910
White	0.82	0.82	0.680	1910
Income (midpoint)	71156.05	68794.64	0.248	1910
College degree	0.69	0.70	0.954	1910
Full-time employee	0.51	0.48	0.272	1910
Northeast	0.18	0.20	0.240	1910
Midwest	0.20	0.21	0.855	1910
West	0.22	0.21	0.665	1910
South	0.40	0.39	0.465	1910

Note: This table provides a balance test between the treatment and control group using all respondents from Experiment 4 (see Table B.1 for an overview of the experiments). "Male" is a binary variable taking value one for male respondents, and zero otherwise. "Age" is the numerical age of the respondent in years. "White" is a binary variable taking value one if the respondent selected "Caucasian/White," and zero otherwise. "Income (midpoint)" is coded continuously as the income bracket's midpoint (Less than $15,000, $15,000 to $24,999, $25,000 to $49,999, $50,000 to $74,999, $75,000 to $99,999, $100,000 to $149,999, $150,000 to $200,000, $200,000 or more). "College degree" is a binary variable taking value one if the respondent has a college degree, and zero otherwise. "Full-time employee" is a binary variable taking value one if the respondent is a full-time employee, and zero otherwise. "Northeast," "Midwest," "West" and "South" are binary variables with value one if the respondent lives in the respective region, and zero otherwise.

Table B.11: Treatment effects on perceptions of accuracy: Robustness

	Experiment 1: Right-wing bias				Experiment 2: Left-wing bias			
	(1) Accuracy	(2) Trust	(3) Quality	(4) Index	(5) Accuracy	(6) Trust	(7) Quality	(8) Index
Panel A: Biden voters								
Treatment (a)	-0.903***	-0.824***	-0.545***	-0.842***	-0.720***	-0.662***	-0.504***	-0.703***
	(0.057)	(0.056)	(0.054)	(0.056)	(0.055)	(0.053)	(0.053)	(0.054)
N	1,464	1,464	1,464	1,464	1,466	1,466	1,466	1,466
Z-scored	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Trump voters								
Treatment (b)	-0.165***	-0.143**	-0.135**	-0.162***	-0.542***	-0.522***	-0.376***	-0.546***
	(0.056)	(0.056)	(0.057)	(0.056)	(0.072)	(0.072)	(0.069)	(0.072)
N	1,235	1,235	1,235	1,235	849	849	849	849
Z-scored	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value: a = b	0.000	0.000	0.000	0.000	0.073	0.166	0.304	0.118

Note: This table presents OLS regression estimates using data from Experiment 1 (columns 1–4) and Experiment 2 (columns 4–8) where the dependent variables are post-treatment beliefs about the newsletter. Panel A and Panel B show results for Biden and Trump voters, respectively. "Treatment" is a binary variable taking value one for respondents assigned to the right-wing biased (columns 1–4) or left-wing biased (columns 5–8) treatment arm. "Accuracy" of the newsletter is measured on a 5-point Likert scale from "Very inaccurate" to "Very accurate." "Trust" is the trustworthiness of the newsletter and measured on a 5-point Likert scale from "Not trustworthy at all" to "Very trustworthy." "Quality" of the newsletter is measured on a 5-point Likert scale from "Very low quality" to "Very high quality." "Index" is a simple average of the accuracy, trust, and quality outcomes. All outcomes are z-scored using the relevant control group mean and standard deviation. All regressions include the standard set of control variables.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table B.12: Secondary results: Beliefs about other newsletter characteristics

	Experiment 1: Right-wing bias				Experiment 2: Left-wing bias			
	(1) Entertainment	(2) Complexity	(3) Easy	(4) No outlet bias	(5) Entertainment	(6) Complexity	(7) Easy	(8) No outlet bias
Panel A: Biden voters								
Treatment (a)	-0.306***	-0.281***	0.118**	-0.551***	-0.141***	-0.272***	0.139***	-0.548***
	(0.051)	(0.053)	(0.052)	(0.022)	(0.050)	(0.052)	(0.053)	(0.021)
N	1,464	1,464	1,464	1,469	1,466	1,466	1,466	1,469
Z-scored	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0	0	0	0.806	0	0	0	0.870
Panel B: Trump voters								
Treatment (b)	0.150***	-0.076	-0.012	-0.359***	-0.155**	-0.049	-0.105	-0.407***
	(0.058)	(0.055)	(0.057)	(0.026)	(0.067)	(0.069)	(0.069)	(0.031)
N	1,235	1,235	1,235	1,236	849	849	849	850
Z-scored	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control group mean	0	0	0	0.664	0	0	0	0.780
p-value: a = b	0.000	0.008	0.094	0.000	0.706	0.008	0.005	0.000

Note: This table presents OLS regression estimates using data from Experiment 1 (columns 1–4) and Experiment 2 (columns 5–8) where the dependent variables are post-treatment beliefs about the newsletter and The Boston Herald's reporting. Panel A and Panel B show results for Biden and Trump voters, respectively. "Treatment" is a binary variable taking value one for respondents assigned to the right-wing biased (columns 1–3) or left-wing biased (columns 4–6) treatment arm. "Entertainment" of the newsletter is measured on a 5-point Likert scale from "Not entertaining at all" to "Very entertaining." "Complex" is the belief about the complexity of the newsletter and measured on a 5-point Likert scale from "Very simple" to "Very complex." "Easy" is the belief about the difficulty of understanding the newsletter and measured on a 5-point Likert scale from "Very easy" to "Very difficult." "No outlet bias" is a binary variable taking value one for respondents who think that The Boston Herald would disclose both key findings from a CBO report, and zero otherwise (see Section E.1.1 for the instructions we used). The outcome variables in columns 1–3 and 5–7 are z-scored using the relevant control group mean and standard deviation. All regressions include the standard set of control variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table B.13: Heterogeneity by ideology

	Dependent variable: Newsletter demand	
	(1) Experiment 1	(2) Experiment 2
Treatment	0.018	-0.023
	(0.035)	(0.043)
Treatment × Liberal	-0.020**	-0.003
	(0.010)	(0.012)
Liberal	0.030**	0.030**
	(0.013)	(0.014)
N	2705	2319

Note: This table presents OLS regression estimates using data from Experiment 1 (column 1) and Experiment 2 (column 2) where the dependent variables are newsletter demand. "Newsletter demand" is a binary variable taking value one for respondents who said "Yes" to receiving the weekly newsletter, and zero for those who said "No." "Treatment" is a binary variable taking value one for respondents assigned the right-wing bias (column 1) or the left-wing bias (column 2) treatment arm, and zero for respondents in the no bias treatment arm. "Liberal" is measured on a 5-point Likert scale from 1: *Very conservative* to 5: *Very liberal*. All regressions include the standard set of control variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table B.14: Heterogeneity in effects by news demand outside the experiment

	Dependent variable: Newsletter demand					
	Experiment 1: Right-wing bias			Experiment 2: Left-wing bias		
Respondents who read:	(1) No other outlet	(2) Mainly left-wing	(3) Mainly right-wing	(4) No other outlet	(5) Mainly left-wing	(6) Mainly right-wing
Treatment	0.004	-0.046**	-0.130***	-0.034	-0.050**	-0.031
	(0.024)	(0.018)	(0.042)	(0.029)	(0.020)	(0.052)
N	599	1,515	340	447	1,408	241
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Mean of dep. var.	0.093	0.158	0.179	0.107	0.196	0.187

Note: This table presents OLS regression estimates using data from Experiment 1 (columns 1–3) and Experiment 2 (columns 4–6) where the dependent variable is a binary indicator taking value one for respondents who said "Yes" to receiving the weekly newsletter, and zero for those who said "No.". Columns 1 and 4 restrict to respondents who indicated pre-treatment that they do not read news from any of the 21 news outlets that we listed. Columns 2 and 5 restrict to respondents who read more left-wing than right-wing biased outlets, while columns 3 and 6 restrict to respondents who read more right-wing than left-wing biased outlets. We used a classification of outlet ideology from the website mediabiasfactcheck.com as of January 26, 2022. "Treatment" is a binary variable taking value one for respondents assigned the right-wing biased (Experiment 1) or the left-wing biased treatment arm (Experiment 2), and zero otherwise. All regressions include the standard set of control variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table B.15: Experiment 4: Treatment effects on perceptions of accuracy and bias

	Left-wing bias		Right-wing bias	
	(1) Accuracy	(2) Left-wing bias	(3) Accuracy	(4) Left-wing bias
Panel A: Biden voters				
Treatment	-0.576***	0.384***	-0.980***	-0.822***
	(0.093)	(0.111)	(0.095)	(0.107)
N	486	486	477	477
Controls	Yes	Yes	Yes	Yes
Panel B: Trump voters				
Treatment	-0.477***	0.408***	-0.231**	-0.308***
	(0.094)	(0.100)	(0.096)	(0.104)
N	473	473	460	460
Controls	Yes	Yes	Yes	Yes

Note: This table presents OLS regression estimates using data from Experiment 4 (see Table B.1 for an overview of experiments) where the dependent variables are perceptions of the newsletter's accuracy (columns 1 and 3) and the perceived left-wing bias of the newsletter (columns 2 and 4). Panel A shows results for Biden voters and Panel B shows results for Trump voters. "Treatment" is a binary indicator for whether respondents were informed that The Boston Herald reported the news in a left-wing biased way (columns 1 and 2) or in a right-wing biased way (columns 3 and 4), and zero otherwise. "Accuracy" of the newsletter is measured on a 5-point Likert scale from "Very inaccurate" to "Very accurate." "Left-wing bias" is measured on a 5-point Likert scale from "Very right-wing biased" to "Very left-wing biased." All outcomes have been z-scored using the relevant control group mean and standard deviation. All regressions include standard control variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table B.16: Experiment 4: Treatment effects on secondary outcomes

	Left-wing bias				Right-wing bias			
	(1) Trust	(2) Quality	(3) Entertainment	(4) Complexity	(5) Trust	(6) Quality	(7) Entertainment	(8) Complexity
Panel A: Biden voters								
Treatment	-0.495***	-0.313***	-0.157*	-0.300***	-0.980***	-0.825***	-0.419***	-0.271***
	(0.088)	(0.092)	(0.084)	(0.091)	(0.100)	(0.095)	(0.093)	(0.089)
N	486	486	486	486	477	477	477	477
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Trump voters								
Treatment	-0.393***	-0.446***	-0.123	-0.064	-0.145	-0.081	0.039	-0.037
	(0.093)	(0.102)	(0.099)	(0.093)	(0.094)	(0.099)	(0.100)	(0.095)
N	472	472	472	472	460	460	460	460
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table shows OLS regression estimates using data from Experiment 4 where the dependent variables are post-treatment beliefs about the newsletter (see Table B.1 for an overview of experiments). Panel A shows results for Biden voters and Panel B shows results for Trump voters. "Treatment" is a binary indicator for whether respondents were informed that The Boston Herald reported the news in a left-wing biased way (columns 1–4) or in a right-wing biased way (columns 5–8). "Trust" is the trustworthiness of the newsletter and measured on a 5-point Likert scale from "Not trustworthy at all" to "Very trustworthy." "Quality" of the newsletter is measured on a 5-point Likert scale from "Very low quality" to "Very high quality." "Entertainment" of the newsletter is measured on a 5-point Likert scale from "Not entertaining at all" to "Very entertaining." "Complex" is the belief about the complexity of the newsletter and measured on a 5-point Likert scale from "Very simple" to "Very complex." All outcomes are z-scored using the relevant control group mean and standard deviation. All regressions include the standard set of control variables.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table B.17: Robustness: Structural estimates from a linear probability model

	Parameter estimates:		
	(1) Full sample	(2) Biden voters	(3) Trump voters
Panel A: 2SLS			
Preference for accuracy (α)	0.053***	0.047**	0.057
	(0.018)	(0.021)	(0.045)
Preference for belief confirmation (β)	0.071***	0.082***	0.040
	(0.020)	(0.024)	(0.038)
Implicit weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	0.424***	0.365**	0.588*
	(0.132)	(0.153)	(0.305)
N	5,014	2,930	2,084
Panel B: Two-sample 2SLS			
Preference for accuracy (α)	0.054***	0.055**	0.035
	(0.019)	(0.024)	(0.058)
Preference for belief confirmation (β)	0.048**	0.059**	0.025
	(0.022)	(0.030)	(0.045)
Implicit weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	0.527***	0.481**	0.583**
	(0.194)	(0.218)	(0.229)
N: Choice data	5,014	2,930	2,084
N: Belief data	1,896	963	933

Note: This table presents the parameter estimates of a linear probability model where the dependent variable is a binary indicator taking value one for respondents who choose to sign up to the newsletter. Column 1 presents parameter estimates for the full sample, while columns 2 and 3 present estimates for Biden and Trump voters, respectively. Panel A ("2SLS") presents two-stage least-squares estimates where we instrument the endogeneous regressors (z-scored perceptions of accuracy and belief confirmation) with a saturated set of treatment arm indicators. We use data from Experiments 1 and 2 where we elicit newsletter subscription choices and perceptions within-subject. In column 1, we also include interactions of the treatment assignment with a binary indicator for whether a respondent voted for Trump as instruments to capture differential first-stage effects of the treatments. We include a binary indicator for whether a respondent voted for Trump as a control variable in column 1. Robust standard errors are shown in parentheses. Panel B ("Two-sample 2SLS") presents analogous *two-sample* two-stage least squares estimates. The endogenous regressors are again z-scored perceptions of accuracy and belief confirmation. However, to estimate the first stage, we only use the "belief data" from Experiment 4 (where we only elicit perceptions) and regress z-scored perceptions of accuracy and belief confirmation on a saturated set of treatment indicators. To estimate the second stage model, we use data from Experiments 1 and 2 and estimate a linear probability model using the predicted perceptions based on the first-stage estimates. We use the same set of instruments and controls as in Panel A. Standard errors in Panel B are obtained from a bootstrap procedure that resamples both the choice data (from Experiment 1 and 2) and the belief data (from Experiment 4) with replacement.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table B.18: Structural model: Preferences for accuracy and biased news — Robustness to using an index of accuracy-related beliefs

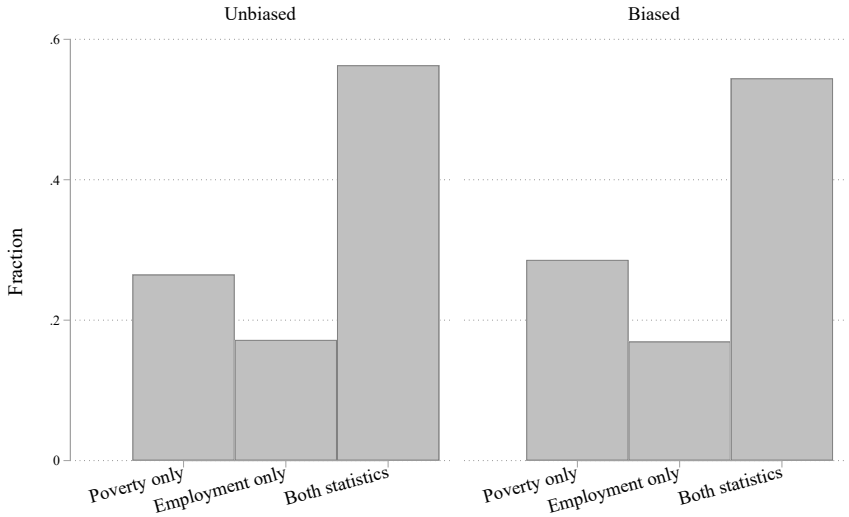
	Parameter estimates:		
	(1) Full sample	(2) Biden voters	(3) Trump voters
Preference for accuracy (α)	0.270***	0.244***	0.274
	(0.082)	(0.093)	(0.191)
Preference for belief confirmation (β)	0.367***	0.412***	0.192
	(0.097)	(0.117)	(0.166)
Implicit weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	0.424***	0.372***	0.588**
	(0.111)	(0.130)	(0.274)
N	5,014	2,930	2,084

Note: This table presents parameter estimates that are analogous to the IV probit estimates presented in Table 2 except for one difference: Instead of using the z-scored post-treatment measure of perceived accuracy (which is measured on a 5-point Likert scale), we use a z-scored index based on the perceived accuracy, quality and trustworthiness of the newsletter. Trustworthiness of the newsletter is measured on a 5-point scale from "Not trustworthy at all" to "Very trustworthy." Quality of the newsletter is measured on a 5-point scale from "Very low quality" to "Very high quality."

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

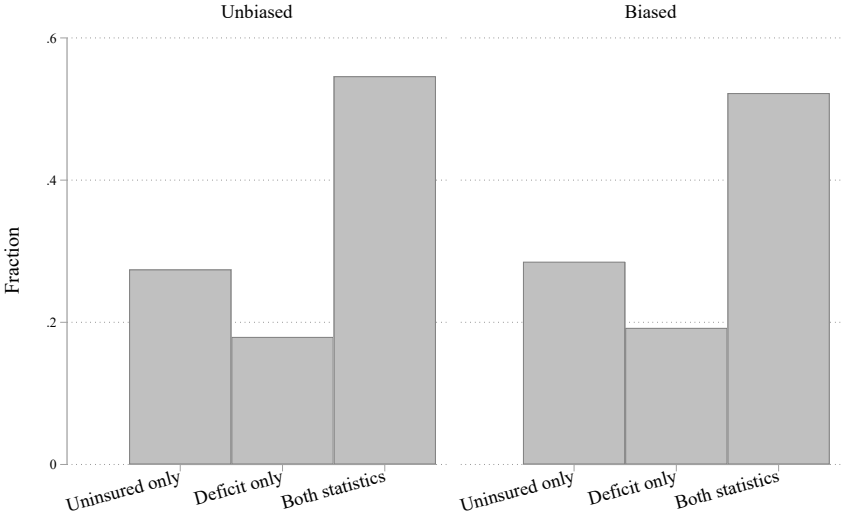
Figure B.1: Pre-treatment beliefs about bias by treatment status

(a) Experiment 1: Beliefs about the coverage of the Healthcare Plan



P-value obtained from a Kolmogorov-Smirnov test for equality of distribution functions: 0.927

(b) Experiment 2: Beliefs about the coverage of the $15 Minimum Wage Bill

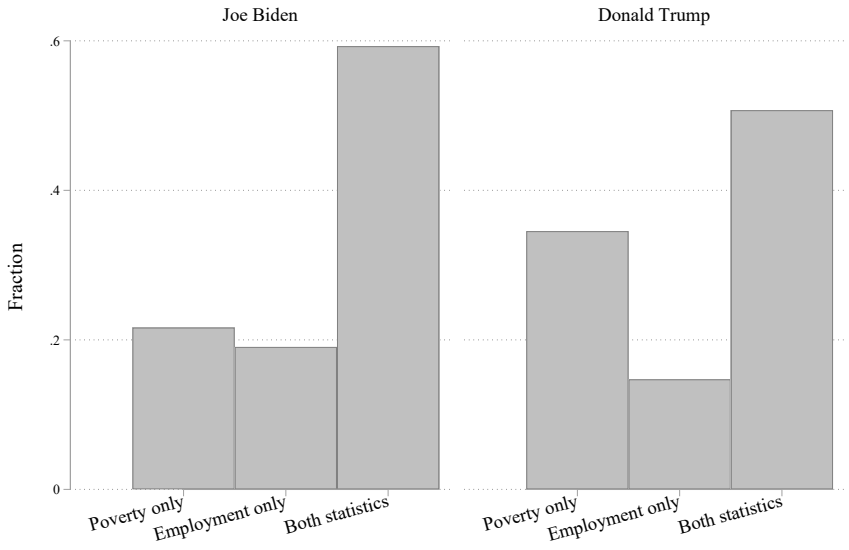


P-value obtained from a Kolmogorov-Smirnov test for equality of distribution functions: 0.899

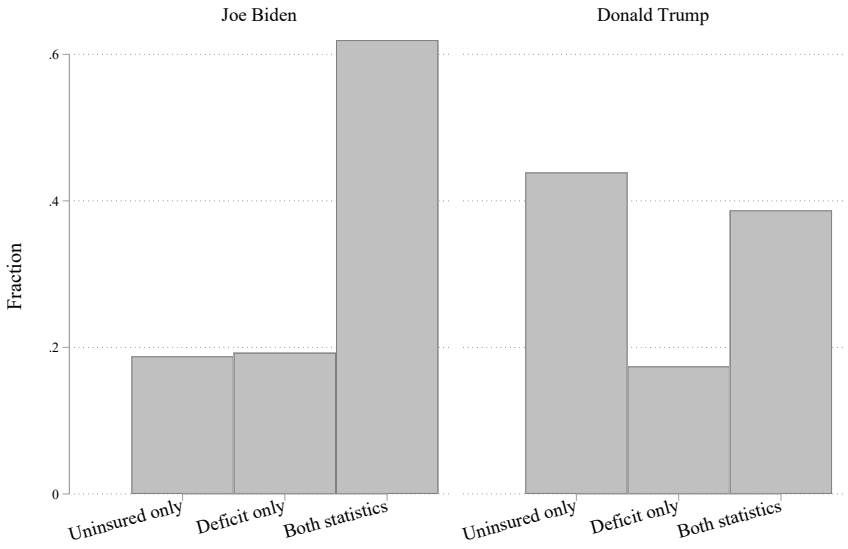
Note: Panel (a) and Panel (b) display the distribution of pre-treatment beliefs about reporting by The Boston Herald for Experiment 1 and 2, respectively. Each panel displays the distribution of pre-treatment beliefs separately for respondents in the no bias treatment arm ("unbiased") and the biased treatment arm ("biased"), i.e., the right-wing bias treatment in Experiment 1 and the left-wing bias treatment in Experiment 2 (see Table B.1 for an overview of experiments).

Figure B.2: Pre-treatment beliefs about reporting in The Boston Herald

(a) Experiment 1: Beliefs about the coverage of the Healthcare Plan



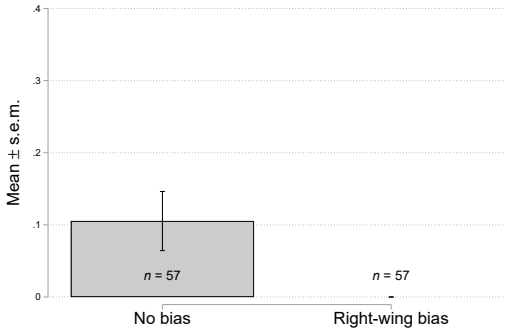
(b) Experiment 2: Beliefs about the coverage of the $15 Minimum Wage Bill



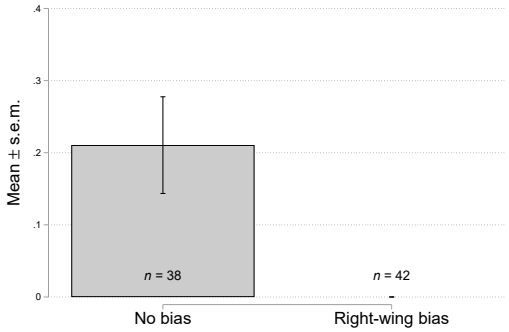
Note: Panel (a) and Panel (b) display the distribution of pre-treatment beliefs about reporting by The Boston Herald for Experiment 1 and 2, respectively. Each panel displays the distribution of pre-treatment beliefs separately for respondents who voted for Joe Biden and respondents who voted for Donald Trump.

Figure B.3: Treatment effects on mentioning balanced reporting in the open-ended responses

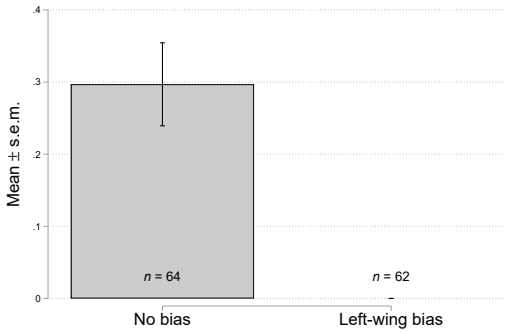
(a) Biden voters: Right-wing bias



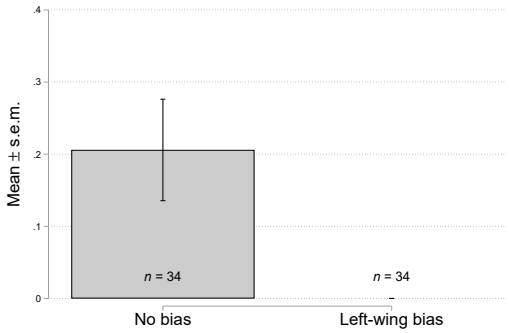
(b) Trump voters: Right-wing bias



(c) Biden voters: Left-wing bias



(d) Trump voters: Left-wing bias



Note: The figure presents treatment effects on whether respondents mentioned *balanced reporting* in their responses to the open-ended motives question in Experiment 3 (see Table B.1). Specifically, respondents were asked why they think The Boston Herald reported in the way that it did. Each panel displays the share of respondents whose responses were hand-coded to the "no bias" category (e.g., "They were probably trying to report fairly without bias," "They were trying to give the full picture," and "They tried to report fairly and accurately" would all be classified as "balanced"). Panel (a) and Panel (b) compare the *right-wing bias* treatment to the *no bias* treatment (analogous to Experiment 1). Panel (c) and Panel (d) compare the *left-wing bias* treatment to the *no bias* treatment (analogous to Experiment 2). Panel (a) and Panel (c) focus on the subsample of respondents who voted for Joe Biden, while Panel (b) and Panel (d) focus on respondents who voted for Donald Trump. The p-values are obtained from a two-sample t-test of equality of means. Standard errors of the mean are shown.

C Research transparency

Preregistration Our experiments were all preregistered in the AsPredicted registry (#78800, #80266, #87947, #89081). The preregistration includes details on the experimental design, the sampling process, planned sample size, exclusion criteria, and the main analyses. Below, we document deviations from the preregistration for our main experiments (Experiments 1 and 2):

- The set of control variables specified in our pre-analysis plan erroneously omitted respondents' pre-treatment belief about how The Boston Herald reported the news (two indicators). In our main specification, we control for pre-treatment beliefs.
- In both experiments, Prolific's subject pool was not large enough to achieve the targeted sample size of 1,500 Trump voters within the pre-specified sampling period of five days. In Experiment 1, we managed to recruit 1,236 Trump voters, while we managed to recruit 850 Trump voters in Experiment 2.
- In our main analysis, the treatment indicator takes value one for respondents in the "right-wing biased" or "left-wing biased" treatment arm, and value zero for respondents in the "unbiased" treatment arm. This is numerically equivalent to the specification we specified on AsPredicted.

Ethical approval The experimental study received ethics approval from the German Association for Experimental Economic Research, and the ethics committee of the University of Cologne.

Data and code availability The experimental data and the analysis code will be made available upon publication.

D Newsletter

This section provides additional details about how we published our newsletter.

Selection of news articles We employed the following procedure to select three articles for each edition of the weekly newsletter. On Mondays, when the next edition of the newsletter is to be published, we used a Firefox browser and went on `https://duckduckgo.com`. The advantage of this search engine over other engines, such as Google, is that search results are not biased by the researcher's own search history or interests. After setting the search engine's settings to "Region: US (English)" and "Time: Past week", we used the following search query: `site:bostonherald.com economic policy`. We then selected the top three articles matching the newsletter's focus on economic policy from the results page.

Newsletter editions Each edition of our newsletter had the same basic structure. Across editions, we exchanged the article headlines and links to the articles. The template we used for our newsletter editions is presented below:

> Thank you very much for participating in our survey *[last week, two weeks ago, three weeks ago, four weeks ago]*. According to our records, you also wanted to subscribe to our weekly newsletter featuring articles related to economic policy over the next month. This is the *[first, second, third, fourth and final]* of four editions of our newsletter. The newsletter includes the top three articles published in The Boston Herald based on readership. Individual links to the articles included this week are included below.
>
> Article 1: Biden's climate plan aims to reduce methane emissions
> Link: https://www.bostonherald.com/2021/11/02/bidens-climate-plan-aims-to-reduce-methane-emissions/
>
> Article 2: Fed pulls back economic aid in face of rising uncertainties
> Link: https://www.bostonherald.com/2021/11/03/fed-pulls-back-economic-aid-in-face-of-rising-uncertainties/
>
> Article 3: Biden hails infrastructure win as 'monumental step forward'
> Link: https://www.bostonherald.com/2021/11/06/biden-hails-infrastructure-win-as-monumental-step-forward/

Logistics We released the newsletter on Mondays on the following dates in 2022 at about 6 am Eastern Time: Nov 8, Nov 15, Nov 22, Nov 29, Dec 7, Dec 13, Dec 20. To provide respondents with our newsletter, we used the capability of Prolific to send direct messages to respondents on Prolific's platform. This allows us to distribute the newsletter without having to elicit any personally identifiable information. This, in turn, ensures that we can measure newsletter demand irrespective of privacy concerns. If respondents indicated that they wish to unsubscribe from our newsletter, we did not send them any additional editions of our newsletter in the following weeks.

Articles Here is a complete list of all articles we included across newsletters:

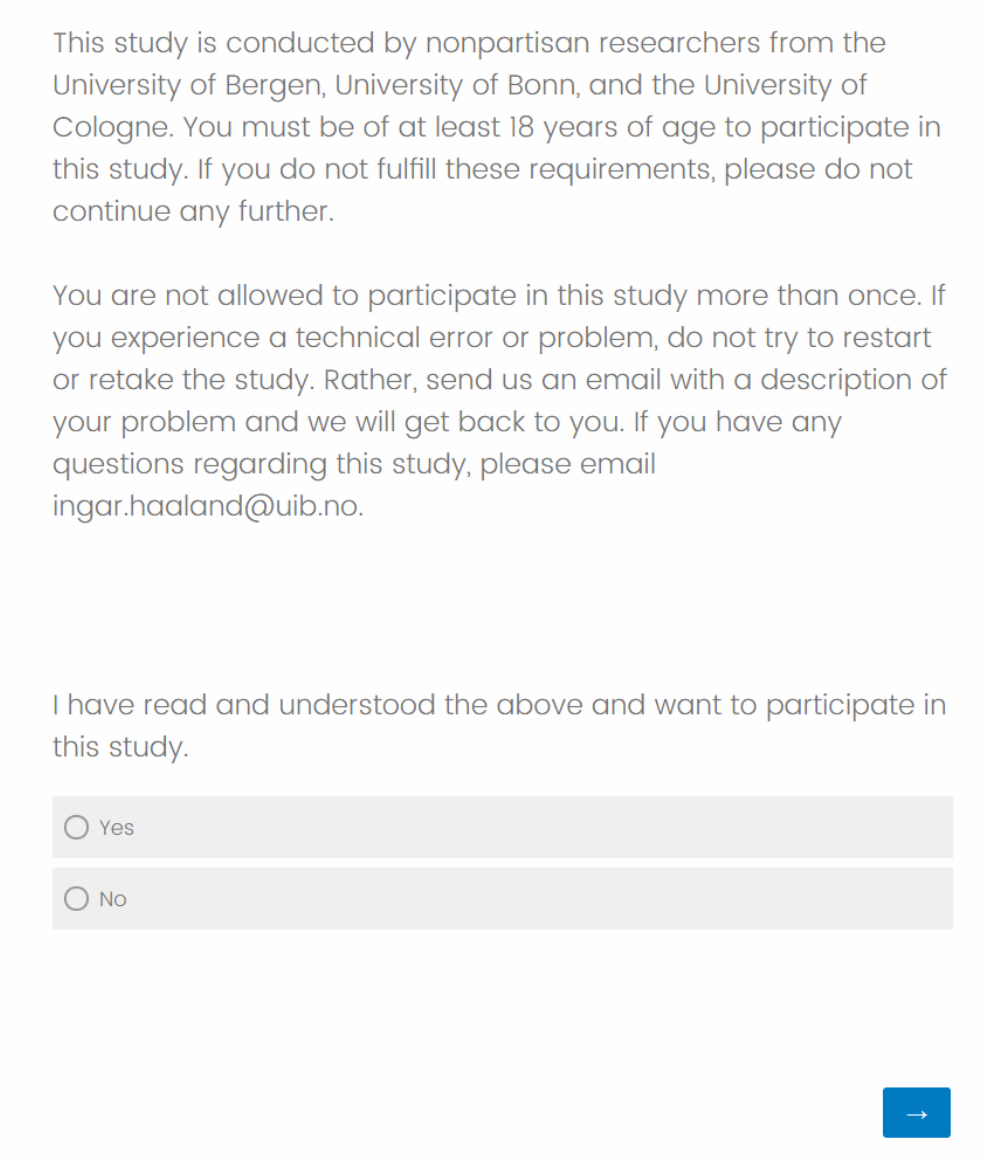
- Biden's climate plan aims to reduce methane emissions
- Fed pulls back economic aid in face of rising uncertainties
- Biden hails infrastructure win as 'monumental step forward'
- Yellen says quashing COVID is key to lowering inflation
- Biden bill would give local news outlets 'shot in the arm'
- Biden bill includes boost for union-made electric vehicles
- House OKs $2T social, climate bill in Biden win; Senate next
- Biden signs $1T infrastructure deal with bipartisan crowd
- No settlement for separated migrant families amid criticism
- Biden Administration approves 2nd large US offshore wind farm
- Some fear China could win from US spat with Marshall Islands
- Will Maine's anti-mining laws keep needed minerals underground?
- Massive $4 billion ARPA, surplus tax revenue bill set for passage
- Biden, Putin square off as tension grows on Ukraine border
- Auditor: Feds gave nearly $4 billion in pandemic relief to businesses that were probably ineligible

- New inflation number feeds angst about Democrats' $2T bill
- Job listings and new quitting remain near record highs
- Inflation hits a 39-year high and isn't going away
- SALT in the wound: Expanded state and local tax deduction stranded as bill dies
- People pressure governments worldwide to act on inflation
- Here come the rate hikes: Fed sees 3 in 2022

E Screenshots

E.1 Experiment 1: Right-wing biased news

E.1.1 Pre-treatment questions



Please indicate your gender.

- ◯ Male
- ◯ Female

What is your age?

Which category best describes your highest level of education?

- ◯ Eighth grade or less
- ◯ Some high school
- ◯ High school degree/GED
- ◯ Some college
- ◯ 2-year college degree
- ◯ 4-year college degree
- ◯ Master's degree
- ◯ Doctoral degree
- ◯ Professional degree (JD, MD, MBA)

Which of the following best describes your race or ethnicity?

- ◯ African American/Black
- ◯ Asian/Asian American
- ◯ Caucasian/White
- ◯ Native American, Inuit or Aleut
- ◯ Native Hawaiian/Pacific Islander
- ◯ Other

Are you of Hispanic, Latino, or Spanish origin?

- ○ Yes
- ○ No

What was your family's gross household income in 2020 in US dollars?

- ○ Less than $15,000
- ○ $15,000 to $24,999
- ○ $25,000 to $49,999
- ○ $50,000 to $74,999
- ○ $75,000 to $99,999
- ○ $100,000 to $149,999
- ○ $150,000 to $200,000
- ○ More than $200,000

Who did you vote for in the 2020 presidential election?

- ○ Donald Trump
- ○ Joe Biden
- ○ Other
- ○ Did not vote

In politics, as of today, do you consider yourself a Republican, a Democrat, or an Independent?

- ○ Republican
- ○ Democrat
- ○ Independent

→

Do you consider yourself a strong Republican or weak Republican?

- ◯ Strong Republican
- ◯ Weak Republican

Do you consider yourself a strong Democrat or weak Democrat?

- ◯ Strong Democrat
- ◯ Weak Democrat

In politics, as of today, do you lean towards the Republican Party or lean towards the Democratic Party?

- ◯ The Republican Party
- ◯ The Democratic Party

What is your region of residence?

- ◯ **Northeast** (CT, ME, MA, NH, RI, VT, NJ, NY,PA),
- ◯ **Midwest** (IL, IN, MI, OH, WI, IA, KS, MN, MO, NE, ND, SD)
- ◯ **South** (DE, DC, FL, GA,MD, NC, SC, VA, WV, AL, KY, MS, TN, AR, LA, OK, TX)
- ◯ **West** (AZ, CO, ID, NM, MT, UT,NV, WY, AK, CA, HI, OR, WA)

What is your current employment status?

- ◯ Full-time employee
- ◯ Part-time employee
- ◯ Self-employed or small business owner
- ◯ Unemployed and looking for work
- ◯ Student
- ◯ Not in labor force (for example: retired or full-time parent)

Are you liberal or conservative?

- ◯ Very liberal
- ◯ Liberal
- ◯ Neither liberal nor conservative
- ◯ Conservative
- ◯ Very conservative

→

Which of the newspapers below have you read at least once during the last 12 months? Please check all that apply.

- [ ] Breitbart
- [ ] BuzzFeed News
- [ ] Boston Herald
- [ ] Chicago Sun-Times
- [ ] Daily Mail
- [ ] Drudge Report
- [ ] InfoWars
- [ ] Los Angeles Times
- [ ] New Republic
- [ ] Newsmax
- [ ] New York Daily News
- [ ] New York Post
- [ ] Palmer Report
- [ ] The Denver Post
- [ ] The Huffington Post
- [ ] The Mercury News
- [ ] The New York Times
- [ ] The Wall Street Journal
- [ ] The Washington Post
- [ ] The Washington Times
- [ ] USA Today
- [ ] I have not read any of the newspapers above during the last 12 months

→

How interested are you in economic news?

- ◯ Very interested
- ◯ Interested
- ◯ Somewhat interested
- ◯ Not interested
- ◯ Not at all interested

→

Do you currently subscribe to any newsletters?

- ◯ Yes
- ◯ No

→

E.1.2 Treatment: Right-wing biased news

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In February 2021, the CBO analyzed the consequences of the Democrats' $15 Minimum Wage Bill.

When debating the $15 Minimum Wage Bill, **Democrats** claimed that the bill would reduce poverty without reducing employment. **Republicans**, by contrast, claimed that the bill would fail to reduce poverty and reduce employment.

In its published report, the CBO estimated that the bill would **lift 900,000 people out of poverty** and **reduce employment by 1.4 million jobs**.

On February 26, 2021, after the CBO published its report, **The Boston Herald** published an article about the economic impact of the bill.

If you had to guess, how do you think the article reported about the CBO findings?

- ○ The article reported that the bill would lift 900,000 people out of poverty **but not** that it would reduce employment by 1.4 million jobs.
- ○ The article reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.
- ○ The article reported that the bill would lift 900,000 people out of poverty **and** that it would reduce employment by 1.4 million jobs.

→

The article, published in **The Boston Herald** on February 26, 2021, reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.

E.1.3 Treatment: Unbiased news

Democrats' $15 Minimum Wage Bill

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In February 2021, the CBO analyzed the consequences of the Democrats' $15 Minimum Wage Bill.

When debating the $15 Minimum Wage Bill, **Democrats** claimed that the bill would reduce poverty without reducing employment. **Republicans**, by contrast, claimed that the bill would fail to reduce poverty and reduce employment.

In its published report, the CBO estimated that the bill would **lift 900,000 people out of poverty** and **reduce employment by 1.4 million jobs**.

On March 2, 2021, after the CBO published its report, **The Boston Herald** published an article about the economic impact of the bill.

If you had to guess, how do you think the article reported about the CBO findings?

- ○ The article reported that the bill would lift 900,000 people out of poverty **but not** that it would reduce employment by 1.4 million jobs.
- ○ The article reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.
- ○ The article reported that the bill would lift 900,000 people out of poverty **and** that it would reduce employment by 1.4 million jobs.

→

The article, published in **The Boston Herald** on March 2, 2021, reported that the bill would reduce employment by 1.4 million jobs **and** that it would lift 900,000 people out of poverty.

E.1.4 Post-treatment outcomes

We would like to offer you the opportunity to sign up for our weekly newsletter.

Our **Weekly Economic Policy Newsletter** will cover the **top three articles about economic policy** published in **The Boston Herald**.

If you say "Yes" below, we will message you the newsletter on your Prolific account on a weekly basis over the next month.

Would you like to subscribe to the newsletter?

- ◯ Yes
- ◯ No

Note: You can access the articles included in the newsletter for free by visiting the Boston Herald website. We select the top three stories based on their readership. We, as nonpartisan academic researchers, provide the newsletter as a free service for people to stay informed about the most important news related to economic policy. The newsletter is a completely non-commercial product.

→

How accurate do you expect the newsletter to be?

- ○ Very accurate
- ○ Accurate
- ○ Somewhat accurate
- ○ Inaccurate
- ○ Very inaccurate

How trustworthy do you expect the newsletter to be?

- ○ Very trustworthy
- ○ Trustworthy
- ○ Somewhat trustworthy
- ○ Not trustworthy
- ○ Not trustworthy at all

What quality would you expect the newsletter to have?

- ○ Very high quality
- ○ High quality
- ○ Medium quality
- ○ Low quality
- ○ Very low quality

What kind of political bias do you expect the newsletter to have?

- ○ Very right-wing biased
- ○ Somewhat right-wing biased
- ○ Not biased
- ○ Somewhat left-wing biased
- ○ Very left-wing biased

How entertaining do you expect the newsletter to be?

- ○ Very entertaining
- ○ Entertaining
- ○ Somewhat entertaining
- ○ Not entertaining
- ○ Not entertaining at all

Do you expect the newsletter to have a simple or complex message?

- ○ Very simple
- ○ Simple
- ○ Neither simple nor complex
- ○ Complex
- ○ Very complex

Do you expect the newsletter to be easy or difficult to understand?

- ○ Very easy
- ○ Easy
- ○ Neither easy nor difficult
- ○ Difficult
- ○ Very difficult

→

Do you favor or oppose the Democrats' $15 Minimum Wage Bill?

- ◯ Strongly favor
- ◯ Favor
- ◯ Neither favor nor oppose
- ◯ Oppose
- ◯ Strongly oppose

→

Republican Healthcare Bill

In 2017, the CBO analyzed the consequences of the Republican Healthcare Bill to repeal and replace Obamacare.

When debating the Republican Healthcare Bill, **Republicans** claimed that the bill would decrease the federal deficit without increasing the number of people without health coverage. **Democrats**, by contrast, claimed that the bill would fail to decrease the deficit and increase the number of people without health coverage.

In its published report, the CBO estimated that the Republican Healthcare Bill would **decrease the deficit by $119 billion** and **leave 23 million more people uninsured**.

After the CBO published its report, **The Boston Herald** published an article about the economic impact of the bill.

If you had to guess, how do you think the article reported about the CBO findings?

- ○ The article reported that the bill would decrease the deficit by $119 billion **but not** that it would leave 23 million more people uninsured.
- ○ The article reported that the bill would leave 23 million more people uninsured **but not** that it would decrease the deficit by $119 billion.
- ○ The article reported that the bill would leave 23 million more people uninsured **and** that it would decrease the deficit by $119 billion.

→

Do you think the researchers behind this study are politically biased?

- ◯ Very right-wing biased
- ◯ Somewhat right-wing biased
- ◯ Not biased
- ◯ Somewhat left-wing biased
- ◯ Very left-wing biased

Do you think the researchers behind this study are trustworthy?

- ◯ Very trustworthy
- ◯ Somewhat trustworthy
- ◯ Neither trustworthy nor untrustworthy
- ◯ Somewhat untrustworthy
- ◯ Very untrustworthy

→

You said "Yes" to subscribe to our **Weekly Economic Policy Newsletter.**

Can you briefly explain why you wanted to subscribe to the newsletter?

→

You said "No" to subscribe to our **Weekly Economic Policy Newsletter.**

Can you briefly explain why you did not want to subscribe to the newsletter?

→

If you had to guess, what would you say was the purpose of this study?

→

If there are any remarks that you would like to make or clarifications that you would like to obtain, please do let us know by writing them into the field below.

→

E.2 Experiment 2

E.2.1 Treatment: Left-wing biased news

Senate Republican Healthcare Plan

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In 2017, the CBO analyzed the consequences of the Senate Republican Healthcare Plan to repeal and replace Obamacare.

When debating the Senate Republican Healthcare Plan, **Republicans** claimed that the plan would decrease the federal deficit without increasing the number of people without health coverage. **Democrats**, by contrast, claimed that the plan would fail to decrease the deficit and increase the number of people without health coverage.

In its published report, the CBO estimated that the Senate Republican Healthcare Plan would **decrease the deficit by over $100 billion** and **leave over 20 million more people uninsured**.

After the CBO published its report, **The Boston Herald** published an article about the economic impact of the plan.

If you had to guess, how do you think the article reported about the CBO findings?

- ○ The article reported that the bill would decrease the deficit by over $100 billion **but not** that it would leave over 20 million more people uninsured.
- ○ The article reported that the bill would leave over 20 million more people uninsured **but not** that it would decrease the deficit by over $100 billion.
- ○ The article reported that the bill would leave over 20 million more people uninsured **and** that it would decrease the deficit by over $100 billion.

→

The Boston Herald article about the Senate Republican Healthcare Plan reported that the plan would leave over 20 million more people uninsured **but not** that it would decrease the deficit by over $100 billion.

E.2.2 Treatment: Unbiased news

House Republican Healthcare Plan

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In 2017, the CBO analyzed the consequences of the House Republican Healthcare Plan to repeal and replace Obamacare.

When debating the House Republican Healthcare Plan, **Republicans** claimed that the plan would decrease the federal deficit without increasing the number of people without health coverage. **Democrats**, by contrast, claimed that the plan would fail to decrease the deficit and increase the number of people without health coverage.

In its published report, the CBO estimated that the House Republican Healthcare Plan would **decrease the deficit by over $100 billion** and **leave over 20 million more people uninsured**.

After the CBO published its report, **The Boston Herald** published an article about the economic impact of the plan.

If you had to guess, how do you think the article reported about the CBO findings?

- ○ The article reported that the bill would decrease the deficit by over $100 billion **but not** that it would leave over 20 million more people uninsured.
- ○ The article reported that the bill would leave over 20 million more people uninsured **but not** that it would decrease the deficit by over $100 billion.
- ○ The article reported that the bill would leave over 20 million more people uninsured **and** that it would decrease the deficit by over $100 billion.

→

The Boston Herald article about the House Republican Healthcare Plan reported that the plan would leave over 20 million more people uninsured **and** that it would decrease the deficit by over $100 billion.

E.3 Post-treatment outcomes

Democrats' $15 Minimum Wage Bill

In February 2021, the CBO analyzed the consequences of the Democrats' $15 Minimum Wage Bill.

When debating the $15 Minimum Wage Bill, **Democrats** claimed that the bill would reduce poverty without reducing employment. **Republicans**, by contrast, claimed that the bill would fail to reduce poverty and reduce employment.

In its published report, the CBO estimated that the bill would **lift 900,000 people out of poverty** and **reduce employment by 1.4 million jobs**.

After the CBO published its report, **The Boston Herald** published an article about the economic impact of the bill.

If you had to guess, how do you think the article reported about the CBO findings?

- ○ The article reported that the bill would lift 900,000 people out of poverty **but not** that it would reduce employment by 1.4 million jobs.
- ○ The article reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.
- ○ The article reported that the bill would lift 900,000 people out of poverty **and** that it would reduce employment by 1.4 million jobs.

→

Do you favor or oppose the Democrats' $15 Minimum Wage Bill?

- ◯ Strongly favor
- ◯ Favor
- ◯ Neither favor nor oppose
- ◯ Oppose
- ◯ Strongly oppose

→

E.4 Experiment 3: Open-ended motives

E.4.1 Treatment 1: No bias (minimum wage bill)

Democrats' $15 Minimum Wage Bill

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In February 2021, the CBO analyzed the consequences of the Democrats' $15 Minimum Wage Bill.

When debating the $15 Minimum Wage Bill, **Democrats** claimed that the bill would reduce poverty without reducing employment. **Republicans**, by contrast, claimed that the bill would fail to reduce poverty and reduce employment.

In its published report, the CBO estimated that the bill would **lift 900,000 people out of poverty** and **reduce employment by 1.4 million jobs**.

On March 2, 2021, after the CBO published its report, **The Boston Herald** published an article about the economic impact of the bill.

The article reported that the bill would reduce employment by 1.4 million jobs **and** that it would lift 900,000 people out of poverty.

Why do you think that The Boston Herald reported that the bill would reduce employment by 1.4 million jobs **and** that it would lift 900,000 people out of poverty? Please answer in two to three full sentences.

→

E.4.2 Treatment 2: Right-wing bias (minimum wage bill)

Democrats' $15 Minimum Wage Bill

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In February 2021, the CBO analyzed the consequences of the Democrats' $15 Minimum Wage Bill.

When debating the $15 Minimum Wage Bill, **Democrats** claimed that the bill would reduce poverty without reducing employment. **Republicans**, by contrast, claimed that the bill would fail to reduce poverty and reduce employment.

In its published report, the CBO estimated that the bill would **lift 900,000 people out of poverty** and **reduce employment by 1.4 million jobs**.

On February 26, 2021, after the CBO published its report, The Boston Herald published an article about the economic impact of the bill.

The article reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.

Why do you think that The Boston Herald reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty? Please answer in two to three full sentences.

E.4.3 Treatment 3: No bias (healthcare plan)

House Republican Healthcare Plan

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In 2017, the CBO analyzed the consequences of the House Republican Healthcare Plan to repeal and replace Obamacare.

When debating the House Republican Healthcare Plan, **Republicans** claimed that the plan would decrease the federal deficit without increasing the number of people without health coverage. **Democrats**, by contrast, claimed that the plan would fail to decrease the deficit and increase the number of people without health coverage.

In its published report, the CBO estimated that the House Republican Healthcare Plan would **decrease the deficit by over $100 billion** and **leave over 20 million more people uninsured**.

After the CBO published its report, **The Boston Herald** published an article about the economic impact of the plan.

The article reported that the plan would leave over 20 million more people uninsured **and** that it would decrease the deficit by over $100 billion.

Why do you think that The Boston Herald reported that the plan would leave over 20 million more people uninsured **and** that it would decrease the deficit by over $100 billion? Please answer in two to three full sentences.

→

E.4.4 Treatment 4: Left-wing bias (healthcare plan)

Senate Republican Healthcare Plan

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In 2017, the CBO analyzed the consequences of the Senate Republican Healthcare Plan to repeal and replace Obamacare.

When debating the Senate Republican Healthcare Plan, **Republicans** claimed that the plan would decrease the federal deficit without increasing the number of people without health coverage. **Democrats**, by contrast, claimed that the plan would fail to decrease the deficit and increase the number of people without health coverage.

In its published report, the CBO estimated that the Senate Republican Healthcare Plan would **decrease the deficit by over $100 billion** and **leave over 20 million more people uninsured**.

After the CBO published its report, **The Boston Herald** published an article about the economic impact of the plan.

The article reported that the plan would leave over 20 million more people uninsured **but not** that it would decrease the deficit by over $100 billion.

Why do you think that The Boston Herald reported that the plan would leave over 20 million more people uninsured **but not** that it would decrease the deficit by over $100 billion? Please answer in two to three full sentences.

→

E.5 Experiment 4: Beliefs about newsletter characteristics

E.5.1 Left-wing bias: Prior (control)

House Republican Healthcare Plan

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In 2017, the CBO analyzed the consequences of the House Republican Healthcare Plan to repeal and replace Obamacare.

When debating the House Republican Healthcare Plan, **Republicans** claimed that the plan would decrease the federal deficit without increasing the number of people without health coverage. **Democrats**, by contrast, claimed that the plan would fail to decrease the deficit and increase the number of people without health coverage.

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After the CBO published its report, **The Boston Herald** published an article about the economic impact of the plan.

If you had to guess, how do you think the article reported about the CBO findings?

- ○ The article reported that the bill would decrease the deficit by over $100 billion **but not** that it would leave over 20 million more people uninsured.
- ○ The article reported that the bill would leave over 20 million more people uninsured **but not** that it would decrease the deficit by over $100 billion.
- ○ The article reported that the bill would leave over 20 million more people uninsured **and** that it would decrease the deficit by over $100 billion.

→

E.5.2 Left-wing bias: Prior (treatment)

Senate Republican Healthcare Plan

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In 2017, the CBO analyzed the consequences of the Senate Republican Healthcare Plan to repeal and replace Obamacare.

When debating the Senate Republican Healthcare Plan, **Republicans** claimed that the plan would decrease the federal deficit without increasing the number of people without health coverage. **Democrats**, by contrast, claimed that the plan would fail to decrease the deficit and increase the number of people without health coverage.

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- ○ The article reported that the bill would leave over 20 million more people uninsured **but not** that it would decrease the deficit by over $100 billion.
- ○ The article reported that the bill would leave over 20 million more people uninsured **and** that it would decrease the deficit by over $100 billion.

→

E.5.3 Right-wing bias: Prior (control)

Democrats' $15 Minimum Wage Bill

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In February 2021, the CBO analyzed the consequences of the Democrats' $15 Minimum Wage Bill.

When debating the $15 Minimum Wage Bill, **Democrats** claimed that the bill would reduce poverty without reducing employment. **Republicans**, by contrast, claimed that the bill would fail to reduce poverty and reduce employment.

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- ○ The article reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.
- ○ The article reported that the bill would lift 900,000 people out of poverty **and** that it would reduce employment by 1.4 million jobs.

→

E.5.4 Right-wing bias: Prior (treatment)

Democrats' $15 Minimum Wage Bill

The Congressional Budget Office (CBO) is Congress's nonpartisan provider of cost and benefit estimates for legislation.

In February 2021, the CBO analyzed the consequences of the Democrats' $15 Minimum Wage Bill.

When debating the $15 Minimum Wage Bill, **Democrats** claimed that the bill would reduce poverty without reducing employment. **Republicans**, by contrast, claimed that the bill would fail to reduce poverty and reduce employment.

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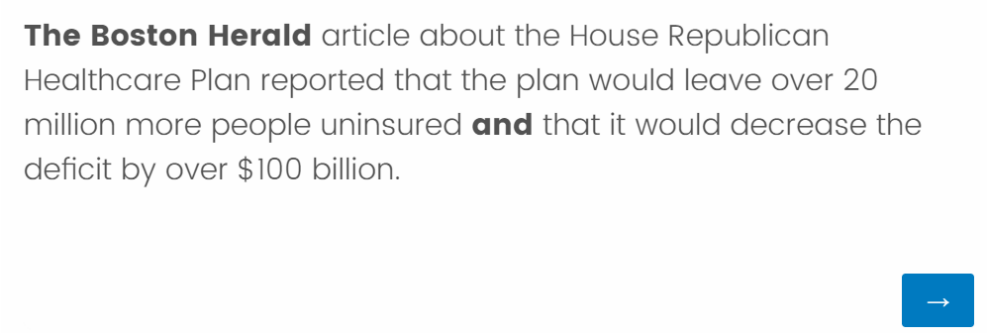
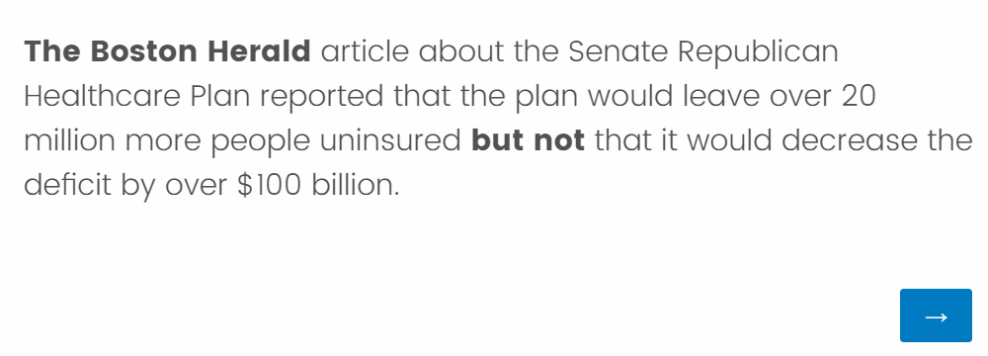
- ○ The article reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.
- ○ The article reported that the bill would lift 900,000 people out of poverty **and** that it would reduce employment by 1.4 million jobs.

→

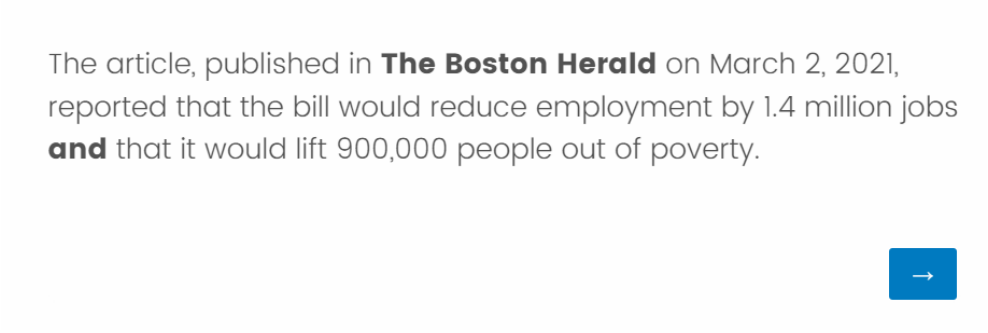
E.5.5 Left-wing bias: Information provision (control)



E.5.6 Left-wing bias: Information provision (treatment)



E.5.7 Right-wing bias: Information provision (control)



E.5.8 Right-wing bias: Information provision (treatment)

The article, published in **The Boston Herald** on February 26, 2021, reported that the bill would reduce employment by 1.4 million jobs **but not** that it would lift 900,000 people out of poverty.

→

E.5.9 Post-treatment outcomes

On the next page, we will ask you some questions about a weekly newsletter that we have offered to Prolific participants in the past.

You will not be able to subscribe to the newsletter, but we are interested in learning what you think about it.

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Our **Weekly Economic Policy Newsletter** covers the **top three articles about economic policy** published in **The Boston Herald**.

Note: Subscribers can access the articles included in the newsletter for free by visiting the Boston Herald website. We select the top three stories based on their readership. We, as nonpartisan academic researchers, have provided the newsletter as a free service for people to stay informed about the most important news related to economic policy. The newsletter is a completely non-commercial product.

What kind of political bias do you expect the newsletter to have?

- ◯ Very right-wing biased
- ◯ Somewhat right-wing biased
- ◯ Not biased
- ◯ Somewhat left-wing biased
- ◯ Very left-wing biased

How accurate do you expect the newsletter to be?

- ◯ Very accurate
- ◯ Accurate
- ◯ Somewhat accurate
- ◯ Inaccurate
- ◯ Very inaccurate

→

How trustworthy do you expect the newsletter to be?

- ○ Very trustworthy
- ○ Trustworthy
- ○ Somewhat trustworthy
- ○ Not trustworthy
- ○ Not trustworthy at all

Do you expect the newsletter to have a simple or complex message?

- ○ Very simple
- ○ Simple
- ○ Neither simple nor complex
- ○ Complex
- ○ Very complex

What quality would you expect the newsletter to have?

- ○ Very high quality
- ○ High quality
- ○ Medium quality
- ○ Low quality
- ○ Very low quality

How entertaining do you expect the newsletter to be?

- ○ Very entertaining
- ○ Entertaining
- ○ Somewhat entertaining
- ○ Not entertaining
- ○ Not entertaining at all

→