The Demand for News: Accuracy Concerns versus Belief Confirmation Motives

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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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1 Introduction

Mounting empirical evidence shows that news outlets report the news in a politically biased way and that readers tend to consume like-minded news (Durante and Knight, 2012; Gentzkow and Shapiro, 2010). This has led to growing concerns about the news media contributing to increasing political polarization (Campante, Durante and Tesei, 2022; Durante, Pinotti and Tesei, 2019; Levy, 2021). Furthermore, since biased news could lead to less informed voters and increase social fragmentation, Sunstein (2018) warns that media bias and the emergence of political echo chambers could threaten the functioning of democracies.

Economic models differ in their explanation for why readers tend to consume like-minded news. 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.

Existing models of media bias thus make fundamentally different assumptions about the relative importance of accuracy concerns and belief confirmation motives in driving the demand for news. The relative importance of these two motives determines how competition affects media bias and is thus key to understand the dynamics between regulation of media markets, media bias, and political polarization. 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 with naturally occurring data where beliefs about media bias are either unobserved or endogenous.[1]

To quantify the relative importance of accuracy concerns and belief confirmation motives in driving the demand for news, we design experiments to directly vary beliefs

[1] Indeed, researchers disagree about how to interpret the finding that readers prefer confirming news (Gentzkow and Shapiro, 2008, 2010). For instance, Golman, Loewenstein, Moene and Zarri (2016) view this finding as evidence of "people's distaste for having their beliefs challenged." By contrast, in line with Gentzkow and Shapiro (2006), Shleifer (2015) considers it "perhaps more plausible" that readers prefer confirming news because they sincerely believe that confirming news is more accurate.

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 all main findings from the report that were 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.

Our experiments were conducted using the online survey platform Prolific and include over 7,000 respondents. 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.[2] 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 without referring explicitly to bias, selective reporting, or accuracy. To measure how this treatment affects the demand for news, we offer all respondents the chance to sign up for a weekly newsletter that we created for the purpose of the experiment. The

[2]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. An auxiliary experiment shows that people have dispersed priors about the political bias of The Boston Herald. Importantly, 44% are "unsure" about its political bias (compared to only 8% for Fox News).

newsletter features the top three articles about economic policy published in The Boston Herald and 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.[3]

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. 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 document a striking political heterogeneity in treatment effects depending on the direction of the bias. Specifically, the *right-wing bias* treatment has a close to zero impact on newsletter demand among

[3]An auxiliary experiment with a different news outlet shows that willingness to sign up for a newsletter is strongly correlated with incentivized willingness to pay for a newspaper subscription.

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 no bias 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 no bias 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 models 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 validate the core assumptions underlying our structural approach that changes in demand are driven primarily by beliefs about accuracy and political bias, we conduct 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 hand-coded, 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.

How do people justify their demand for biased news? At the end of the main experiments, we collect data on people's motives for subscribing to the newsletter. 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. 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 study contributes to an ongoing debate surrounding the origins and drivers of media bias in news markets (Demsetz and Lehn, 1985; Gentzkow and Shapiro, 2006; Gentzkow, Shapiro and Sinkinson, 2014; Mullainathan and Shleifer, 2005), which has been motivated by the media's influence on the public discourse (King, Schneer and White, 2017), voting behavior (DellaVigna and Kaplan, 2007), and, ultimately, the quality of elected officials and implemented policies (Perego and Yuksel, 2022).[4] While there is an emerging consensus on the role of supply-side explanations based on media owners' political agenda (Puglisi and Snyder, 2015), there is no consensus around how to interpret the tendency for individuals to read news from like-minded

[4] A complementary literature focuses on the measurement of media bias (Gentzkow and Shapiro, 2010; Groseclose and Milyo, 2005).

sources (Campante et al., 2022; Shleifer, 2015). A major empirical challenge is that this pattern can be explained both by a preference for confirmatory news (Mullainathan and Shleifer, 2005) and by differences in beliefs about the accuracy of information from aligned versus non-aligned news sources (Gentzkow and Shapiro, 2006; Gentzkow, Wong and Zhang, 2018). Understanding why consumers tend to read like-minded news has important implications for both theory and practice as it determines whether introducing regulations that promote competition will increase or decrease media bias in equilibrium (Foros, Kind and Sørgard, 2015).

The best-available evidence on consumer's news preferences comes from observational studies exploiting natural variation in changes in the ideological leanings of media outlets (e.g., Durante and Knight, 2012). Higher demand for ideologically aligned news could then reflect both preferences for belief confirmation as well as changes in accuracy perceptions and trust.[5] Moreover, changes in ideological leanings can coincide with other structural shifts in programming, such as a reorientation towards entertainment and soft news (Durante et al., 2019). We thus cannot draw strong conclusions about why people tend to read like-minded news from observational evidence (Tappin, Pennycook and Rand, 2020).

Our experimental approach overcomes this fundamental challenge by creating situations where theories based on accuracy concerns or belief confirmation motives make opposing predictions. Our main contribution is to provide the first experimental evidence on the relative importance of accuracy concerns and belief confirmation motives. We demonstrate that both accuracy concerns and belief confirmation motives play a key role in shaping the demand for news, which demonstrates the importance of integrating both motives into theories of news demand (Gentzkow and Shapiro, 2006; Golman et al., 2016).

We also contribute to a literature on people's demand for information (Bursztyn, Rao, Roth and Yanagizawa-Drott, 2022a; Faia, Fuster, Pezone and Zafar, 2021; Fuster, Perez-Truglia and Zafar, 2022; Ganguly and Tasoff, 2016; Montanari and Nunnari, 2019; Nielsen, 2020).[6] Chopra, Haaland and Roth (2022) examine how the demand for news changes in response to an added fact-checking service, demonstrating that

[5]A robust finding in surveys on news consumption is that people report substantially higher levels of trust in politically aligned compared to non-aligned news outlets (Mitchell and Weisel, 2014).

[6]More broadly our evidence relates to a literature on motivated belief updating (Schwardmann, Tripodi and Van der Weele, 2022; Di Tella, Perez-Truglia, Babino and Sigman, 2015; Thaler, 2019).

fact-checking is not necessarily an effective tool to reduce ideological segregation in news consumption. Our key contribution to the information demand literature is to identify the relative importance of accuracy concerns and belief confirmation motives in the news domain. 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 measure—willingness to sign up for a newsletter featuring actual newspaper articles from a real-world outlet—is tightly linked to actual news consumption decisions.[7]

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 (DellaVigna, List, Malmendier and Rao, 2022). We extend this literature by estimating a behavioral parameter governing people's informational preferences. Specifically, we 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. Interestingly, we find support for a substantial role of accuracy concerns despite the comparatively small instrumental value of political news, which suggests that people might place an intrinsic value on the accuracy of news. 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 discusses alternative mechanisms. Section 5 concludes. The Online Appendix provides a theoretical framework, additional empirical results, and the full

[7] An auxiliary experiment demonstrates that newsletter subscriptions strongly predict willingness to pay for newsletter subscriptions and thereby provides further evidence on the external validity of our main outcome measure.

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.[8]

[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, Roth and Wohlfart, 2023). We collect data with Prolific not only because of the high quality of responses compared to other survey platforms (Eyal, David, Andrew, Zak and Ekaterina, 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.[9] Our samples are heterogeneous and resemble the US population in terms of several observables (income, region, and gender; see Table F.1). In both experiments, the two treatment groups are balanced in terms of observable characteristics in the full sample (Table F.2).

[8]The full instructions for both experiments are available here: https://raw.githubusercontent.com/cproth/papers/master/AccuracyBeliefConfirmation_instructions.pdf.

[9]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.

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*).

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.[10] 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 informing our respondents about all main findings from the CBO report that The Boston Herald could have reported about, our design allows our respondents to make direct inferences about its reporting strategy. We chose to make the CBO the source

[10] 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.

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.

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.[11] 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:

> 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 Fox News or The New York Times.[12] Weaker priors about accuracy and political bias make beliefs about

[11] 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. See Table 1 for an overview of all articles.

[12] In Auxiliary Experiment 1(AsPredicted# 113035), we validate our assumption that people have relatively weak priors about the bias of The Boston Herald. Specifically, we ask a separate sample of 500 respondents about their bias perceptions of 12 major US news outlets. As shown in Figure F.2, 44% of respondents say they are "Unsure" about the bias of The Boston Herald compared to only 8% for Fox News and 16% for The New York Times.

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., 2023). 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.

Our design varies beliefs about media bias by *filtering*, i.e., the notion of selectively reporting only a subset of the available information. We chose to focus on this form of media bias—rather than bias by *distortion*, i.e., outright lying—as it is one of the main manifestations of media bias in practice (Puglisi and Snyder, 2015). Knowledge of how the demand for news responds to changes in perceptions of filtering bias is thus essential for our understanding of the market for news.

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 two factors. First, the fact that newsletters are a popular means of staying informed about politics, with 21% of Americans receiving news from a newsletter over the course of a week (Newman, Fletcher, Schulz, Andi and Nielsen, 2020). Second, subscription decisions are behaviorally incentivized: 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. Yet, at the same time, subscribing to an unwanted newsletter is a costly action as it entails receiving weekly emails.[13]

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 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

[13] An alternative measure of news demand could have been willingness to pay for newspaper subscriptions. While The Boston Herald does not offer anonymous gift subscriptions, making a willingness to pay measure infeasible in our context, it is possible to buy anonymous gift subscriptions for other news outlets. In Auxiliary Experiment 2 (AsPredicted #113054), we examine whether the willingness to sign up for a newsletter featuring the top three stories about economic policy from The New York Times correlates with willingness to pay for a 12-month newsletter subscription to The New York Times (the decision was incentivized using the Becker–DeGroot–Marschak method). Those who signed up for the newsletter had a 25.9% (corresponding to 40.7% of a standard deviation) higher willingness to pay for the newsletter subscription ($p = 0.003$), validating our assumption that newsletter demand is an externally valid proxy for broader news demand.

concerns. Appendix Section E provides information about the logistical details and the newsletter's design.[14]

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 federal 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.[15] 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.

[14]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.

[15]Prior beliefs about reporting are virtually identical for the Senate version and the House version of the Republican Healthcare Plan ($p = 0.899$; two-sample Kolmogorov–Smirnov test).

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).[16] 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:

> 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 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.

[Insert Table 1 here]

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 by testing the predictions of three classes of models:

[16]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. See also Table 1 for an overview of all articles.

(i) models where people only care about accuracy, (ii) models where people only care about belief confirmation, and (iii) models where both accuracy and belief confirmation motives shape the demand for news. To fix ideas, let Y_i^g denote the demand for news in treatment arm $i \in \{L,N,R\}$ and political group $g \in \{B,T\}$, where B represents Biden voters, T represents Trump voters, and L, N and R denote the *left-wing bias*, *no bias* and *right-wing bias* treatment arm, respectively.

First, we consider models where the demand for news only depends on the perceived informativeness (e.g., Gentzkow and Shapiro, 2006). These models would predict that the demand for news is strictly larger in the *no bias* treatment arm compared to the other treatment arms, i.e., $Y_L^g < Y_N^g$ and $Y_R^g < Y_N^g$ for both political groups $g \in \{B,T\}$ (as shown in Section A.1 of the Online Appendix). The intuition underlying this observation is that the *right-wing* and the *left-wing bias* treatment increase the perceived likelihood of selective reporting in a setting where full information disclosure would have been possible.[17]

Second, we turn to models where the demand for news is driven only by belief confirmation motives (as discussed in Loewenstein and Molnar, 2018). The predictions of such models depend on the political preferences 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. In short, demand should increase whenever the perceived political bias moves towards the political belief of our respondents. We would thus expect $Y_L^B > Y_N^B > Y_R^B$ among Biden voters, and the opposite pattern $Y_L^T < Y_N^T < Y_R^T$ among Trump voters.

We finally turn to models in which people make a trade-off between accuracy and belief confirmation motives (e.g., Mullainathan and Shleifer, 2005). When the bias in reporting is not aligned with respondents' political views, we obtain the unambiguous prediction that $Y_R^B < Y_N^B$ and $Y_L^T < Y_N^T$ because there is no conflict between accuracy and belief confirmation motives. However, such a conflict arises whenever the alignment between respondents' political views and the perceived political bias in reporting increases at the cost of lower accuracy in reporting. The sign of the overall effect on

[17] 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 has to engage in selective reporting).

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. Without knowing these quantities, the comparison between Y_L^B and Y_N^B and the comparison between Y_R^T and Y_N^T are ambiguous. Note that if both motives are equally important drivers of the demand for news, one would expect similar levels of demand in these cases: $Y_L^B \approx Y_N^B$ and $Y_R^T \approx Y_N^T$. Appendix Table 2 provides a summary of the predictions.

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 before presenting the main treatment effects on demand for the newsletter. We then use a discrete choice model to estimate the relative importance of accuracy concerns compared to belief confirmation motives. Finally, we shed light on how people justify their consumption of biased news using text data.

3.1 Beliefs about the accuracy and political bias of the newsletter

Table 3 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 ($p < 0.001$).[18] The political heterogeneity in treatment effects on accuracy perceptions is consistent with the mechanism in Gentzkow and Shapiro

[18]Table F.3 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.

(2006) and motivates our structural approach (outlined in Section 3.3) that accounts for heterogeneous treatment effects on perceptions about the newsletter.

We next examine treatment effects on perceptions of political bias. Column 2 of Table 3 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 3 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 3, we find no statistically significant effect 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]; $p = 0.821$). However, while the point estimate is close to zero and not statistically significant, the confidence interval is consistent with economically significant changes in demand in both directions. In Experiment 2, 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 no bias group mean of 19.1%.[19]

[19] The p-value for a test of equality of treatment effects across experiments for Trump voters is 0.072.

These patterns reverse for Biden voters. As shown in Panel B of Table 3, 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 no bias 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]; $p = 0.176$).[20]

The political heterogeneity in treatment effects, in which our respondents only significantly reduce their demand for biased news if the change in bias is inconsistent with their own political beliefs, is inconsistent with models in which readers only care about the accuracy of news (as discussed in Appendix Section A.1). 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 3 here]

3.3 Structural estimates of preference parameters

Our reduced form results suggest that people's demand for news is 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. We quantify the preferences of a representative agent by combining the quantitative information on the effects of the treatments on both accuracy and bias perceptions alongside with our quantitative estimates of the effects on news demand.

[20] The p-value for a test of equality of treatment effects across experiments for Biden voters is 0.017.

Discrete choice model Agent i has to decide whether to subscribe to our newsletter $(y_i = 1)$ or not ($y_i = 0$). The agent will subscribe to the newsletter if his expected utility u_i from subscribing is positive, such that $y_i = \mathbf{1}(u_i \geq 0)$. Following Mullainathan and Shleifer (2005), we focus on the trade-off between accuracy and belief confirmation, and thus assume that the agent's expected utility from subscribing to the newsletter consists of a component capturing a preference for accuracy in reporting, a component capturing a preference for belief confirmation, and the price of the newsletter. As we offer the newsletter free of charge, the expected utility from subscribing is

$$u_i = \bar{u} + \alpha s_i + \beta b_i + \varepsilon_i \tag{1}$$

where $s_i = E_i(\tilde{s}_i)$ is the agent's subjective belief about the newsletter's accuracy; $b_i = E_i(\tilde{b}_i)$ is the agent's subjective belief about how much the newsletter will confirm his prior beliefs; and ε_i is a random taste shock. The parameters α and β capture the agent's willingness to trade off accuracy against belief confirmation.

As we elicit subjective beliefs about accuracy and belief confirmation in our experiment, we will directly substitute them for s_i and b_i in our structural estimation of the above utility function.[21] This is a key advantage of our approach compared to other identification strategies based on observational data where researchers do not observe beliefs, and thus have to impose specific assumptions on the structure of perceptions of accuracy and political bias of news.

Estimation and identification We estimate the model parameters, $\theta = (\alpha, \beta, \bar{u})$, both for the full sample as well as separately for Biden and Trump voters to explore heterogeneity in preferences. As proxies for s_i and b_i, we use the z-scored post-treatment belief measures of perceived accuracy and political bias in reporting.[22] 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. This captures the notion that belief

[21]We are deliberately agnostic about the underlying information-theoretic decision problem giving rise to a potential preference for accuracy because revealed preferences in our experiment should be a function of respondents' subjective beliefs about the accuracy and expected belief confirmation of the newsletter. However, one possibility is that the agent has to learn about the state of the economy ω, and take a subsequent action a_i with a payoff $v(a_i, \omega) = -\alpha(a_i - \omega)^2$ after reading the newsletter n (or not), which would give rise to a demand for accuracy in reporting.

[22]We normalize these measures to have a mean of zero and a standard deviation of one among respondents in the *no bias* treatment arms. The results are robust to using the non-z-scored beliefs.

confirmation depends on the perceived alignment between one's own political ideology and the perceived political bias in reporting. 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 the parameters θ using a probit model. However, this exclusion restriction is unlikely to hold in practice without relying on an exogenous shifter. We therefore estimate an IV probit model as outlined by the set of equations below in which the binary dependent variable y_i is the decision to sign up to our newsletter:

$$y_i = \mathbf{1}\left(\bar{u} + \alpha s_i + \beta b_i + \varepsilon_i \geq 0\right) \tag{2}$$

$$s_i = Z_i' \gamma_s + \varepsilon_i^{'} \tag{3}$$

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

Here, we instrument respondents' perceptions with a saturated set of treatment arm indicators, Z_i (see equations 3 and 4). We use Stata's `ivprobit` routine to estimate the parameters of interest using the efficient estimator proposed by Newey (1987). 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 beliefs 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 ($\bar{u}$) 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 the bias treatments in both experiments 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. Section B.2 provides an extended discussion of the assumptions required for identification of the model's structural parameters.

Discussion of assumptions First, we focus on the accuracy-belief confirmation trade-off. While demand for our newsletter could also reflect other motives, our estimation strategy remains valid if these motives do not differentially affect demand across treatment arms.[23] 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: Auxiliary Experiment 1 (AsPredicted #113035) confirms that US Americans hold weak and dispersed priors about the political bias of The Boston Herald, with 44% expressing uncertainty about its political bias compared to only 8% for Fox News. 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 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 by accounting for any differences in scale use.

Results Table 4 presents the parameter estimates of the discrete choice model. 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 $\alpha/(\alpha+\beta) = 0.241/(0.241+0.345) = 0.412$, and we cannot reject the null hypothesis that respondents assign equal weights to standard deviation changes in perceived accuracy and belief confirmation ($p > 0.10$). Columns 2 and 3 of Table 4 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

[23]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. In Section B.3, we discuss the implications of allowing for potentially differential effects of such motives across treatments, which can still yield bounds on the relative preference for accuracy.

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 4 here]

Heterogeneity by education While we pre-specified looking at political heterogeneity, our survey data allows us to further explore heterogeneity by different background characteristics. Recent voting trends show that political polarization is increasing faster among highly educated voters. More selective exposure to information among highly educated voters could be an important driver of this pattern.[24] We therefore separately estimate the structural model on four groups of respondents defined by whether they voted for Biden or Trump in the 2020 presidential election, and by whether they have completed a college degree (66%) or not (34%).

We document a pronounced education gradient in respondents' relative preference for accuracy compared to belief confirmation motives. Specifically, the parameter estimates suggest that the demand for news among Biden voters with lower levels of educational attainment is almost entirely driven by a preference for belief confirmation (column 1, Table F.8). The parameter capturing the preference for belief confirmation is an order of magnitude larger than the parameter capturing respondents' preference for accuracy in reporting. In contrast, Biden voters with a college degree seem to care more equally about perceived accuracy and belief confirmation motives, with a relative weight on accuracy that is close to and not statistically significantly different from 0.5 (column 2). We find similar qualitative patterns by educational attainment among Trump voters (columns 3 and 4). If anything, Trump voters without a college degree even display a distaste for perceived accuracy in reporting, although this should be interpreted cautiously as the model parameters are less precisely estimated (column 3). Taken together, these findings suggest that heterogeneity in preference can partially explain the segmented demand for news across different levels of education.

[24] See, e.g., `https://www.pewresearch.org/fact-tank/2016/09/15/educational-divide-in-vote-preferences-on-track-to-be-wider-than-in-recent-elections/` (accessed November 24, 2022).

Robustness We obtain similar structural estimates across a series of robustness checks. First, we obtain similar results when we re-estimate the model without z-scoring beliefs about accuracy and belief confirmation (as shown in Table F.4), which suggests that this normalization procedure does not affect our estimates of the relative importance of accuracy concerns and belief confirmation motives. Second, our results are robust to replacing the z-scored accuracy belief measure with an index based on post-treatment beliefs about accuracy, quality, and trustworthiness (as shown in Table F.5). Third, we also estimate an analogous *linear* probability model using a two-stage least-squares estimator where we again use our treatment assignments as instruments. Panel A of Table F.6 shows that we obtain quantitatively very similar estimates of the relative importance of accuracy compared to belief confirmation motives using a linear probability model. Thus, while the choice of a linear versus a non-linear second stage model affects the scale of the coefficients, the implied relative magnitudes are quantitatively robust across specifications. Fourth, we mitigate concerns about consistency bias in survey responses affecting our structural estimates. The results from a robustness exercise addressing this concern are presented in Panel B of Table F.6. Appendix Section B.1 provides more details about this concern and how we address it. Fifth, we examine whether a preference for accuracy in combination with, (i), a preference for simplicity in reporting or, (ii), a preference for entertainment could also explain the treatment effects with plausible parameter values. To do so, we repeat our main estimation but replace our measure of belief confirmation with a measure of, (i), the perceived simplicity (reverse-coded complexity belief) or, (ii), perceived entertainment value of the newsletter. Table F.7 provides the results from this exercise. The coefficients are mostly statistically insignificant and unstable in sign across political subgroups. This indicates that it is difficult to rationalize the patterns of treatment effects based on preferences for simplicity or entertainment on top of a preference for accuracy.

Taken together, these five additional checks underscore the robustness of the main findings from the structural model.

3.4 Motives for news demand

Our experimental findings and our model-based preference estimates suggest that 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[25] This data provides us with a direct lens into people's reasoning about the motives underlying their subscription decision.

We manually categorize the 4,991 open-ended text responses by determining whether political bias was cited as a reason for subscribing or not subscribing to the newsletter (e.g., subscribers who said "It seems informative and unbiased" or non-subscribers who said "They didn't report the news fairly in my opinion"). Panel A of Figure 3 shows that subscribers in the *bias* treatment arms are 10.8 percentage points less likely to justify their demand for the newsletter in a way related to the political bias of the newsletter compared to respondents in the *no bias* treatments ($p < 0.001$), which corresponds to a decrease by 62.7%. On the flip side, Panel B of Figure 3 shows that among non-subscribers, respondents in the "bias" treatment arms are almost three times as likely to justify their non-subscription with the political bias of the newsletter compared to respondents in the "no bias" treatment arms ($p < 0.001$).

[Insert Figure 3 here]

These patterns are robust to alternative means of analyzing the text data, such as classifying responses based on whether synonyms of "biased" and "unbiased" are mentioned by respondents (Appendix Section C.1), or using the method proposed by Gentzkow and Shapiro (2010) to identify phrases that are characteristic of open-ended responses of subscribers and non-subscribers across treatments (Appendix Section C.2).

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, Egorov, Haaland, Rao and Roth, 2022b, 2023, 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).

[25] The full instructions are available here: `https://raw.githubusercontent.com/cproth/papers/master/AccuracyBeliefConfirmation_instructions.pdf`.

4 Robustness

We present evidence using text data from open-ended responses that suggest that our treatment mainly operates by changing people's perceptions of the accuracy and political bias of The Boston Herald, and discuss other potential mechanisms in light of this evidence.

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 1). The experiment was conducted in February 2022 with 388 respondents (240 Biden voters and 148 Trump voters).[26]

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?

[26] 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.

Respondents assigned to the Republican Healthcare Plan received analogous instructions tailored to that plan.

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 4 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 *bias* treatments. These effects are both statistically and economically significant and highlight that our respondents interpret the reporting decision to be either driven by motives to deliver accurate or biased reporting.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 4 here]

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. Figure 5 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 systematically create variation in thoughts about bias and accuracy on top of people's minds.

[Insert Figure 5 here]

4.2 Discussion of alternative mechanisms

While we document a robust treatment effect on people's beliefs about the accuracy and political bias of The Boston Herald, other factors—such as cognitive constraints, cross-learning about entertainment, experimenter demand effects, or diversification motives—-could in principle explain some of the treatment effects on news demand. In this section, we examine the explanatory power of these mechanisms and explain why we think they play at most a negligible role in our setting. In Section B.3, we further argue that our estimates are relatively robust to small changes in perceptions of factors other than accuracy and political bias.

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 F.10). 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 F.10, 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 arms.

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, Frankel and Kamenica, 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 F.10). 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 (see Appendix Section B.1) show that only Biden voters significantly updated their beliefs about the entertainment value of the newsletter when there was less scope for ex-post rationalization of the newsletter subscription decision (Table F.11). Finally, conceptually disentangling belief utility and entertainment utility is not straightforward since the two concepts might be intertwined. That is, reading news that confirms your beliefs might feel more entertaining than reading news that challenges your beliefs. For instance, as a Republican, it might not feel very "entertaining" to read that the Republican Health Care Plan will lead to more than 20 million more people uninsured. Part of the utility from belief confirmation might thus relate to the entertainment value of having your beliefs confirmed. Importantly, 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. Furthermore, Section B.3 provides a discussion on how we can recover bounds on the parametric estimates of the relative importance of accuracy concerns even if our treatment would change the perceived entertainment value.

Experimenter demand effects It is possible that respondents in the different treatment groups hold different beliefs about the experimenter's expectations, although 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, Haushofer and Roth, 2018). In our setting, we do not believe that experimenter demand is a major concern for several reasons. First, our experimental manipulation is implicit in nature as we only factually state the newspaper reporting in a neutral way without framing it in terms of bias or accuracy. Moreover, as we employ an active control group design that informs all respondents about The Boston Herald's past reporting, any potential demand effects arising from the information provision *itself* are held constant across treatment arms by design. 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.

While we consider experimenter demand effects unlikely, we cannot rule out that they still play a role. To further alleviate potential concerns, we therefore use almost 5,000 hand-coded responses based on participants' guesses about the study's purpose from an open-ended question that was elicited at the end of the main experiments. As shown Appendix Figure F.1, only 4.1% of our respondents correctly guess the study's purpose (i.e., how perceptions of bias shape people's news consumption). Most participants guess that the purpose is related to understanding perceptions of bias (36.2%), measuring opinions and attitudes (16.9%), and studying political views (10.4%). A sizable fraction of respondents also express that they simply do not know (11.5%). We re-run our main specifications for the subsample of respondents who did not correctly guess the study purpose and also exclude all respondents who think that the purpose was to understand "perceptions of bias." This robustness check shows that results are virtually unchanged for the subsample of respondents for which demand effects are particularly unlikely to confound treatment effects (as shown by Appendix Table F.12).

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 F.9).

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.

5 Concluding remarks

Using large-scale experiments with American voters, we quantify the relative importance of accuracy concerns and belief confirmation motives in driving the demand for news. Our experiments vary whether a news outlet reports the news in a right-wing biased, left-wing biased, or politically unbiased way. We then study people's demand for a newsletter featuring articles from this outlet. 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. 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.

While a key concern about all structural models is that the results might be specific to a particular context and sensitive to the assumptions that go into the estimation (DellaVigna, 2018), a key advantage of the structural estimation is that it provides a benchmark estimate for the importance of the two motives that can be compared across studies. Furthermore, model-based estimates of behavioral parameters can be important for welfare and policy evaluations, especially when parameter estimates are similar across different settings and studies. Our paper takes the first step in this direction by estimating the relative importance of the two motives with experimental data. Given a strong disagreement in the literature about the relative importance of the two motives in driving the demand for news (Gentzkow and Shapiro, 2006; Golman et al., 2016; Loewenstein and Molnar, 2018; Shleifer, 2015), we believe it will be very important to examine the robustness of our findings in future studies to converge on a common view.

There are growing concerns that biased news contributes to increasing political polarization, increased social fragmentation, and the rise of populism (Levy, 2021; Sunstein, 2018). It is thus important to understand why media bias occurs in equilibrium. Our findings contribute to this debate by providing direct experimental evidence on potential demand-side drivers: People value accuracy, but also demand news that confirms their existing beliefs. This result lends empirical support to demand-side explanations of media bias, such as behavioral models where firms cater to people's preference for like-minded news by slanting their reporting towards the beliefs of their readers. While other factors may also contribute to the origin of media bias, our findings suggest that accounts that assume that consumers only value the accuracy of news are likely to be incomplete. A preference for like-minded news has important implications for regulation and other efforts aimed at fighting media bias and fake news. First, competition among media outlets should increase media bias in equilibrium if consumers have a demand for biased news (Mullainathan and Shleifer, 2005). Regulatory efforts to increase the competitive pressure in media markets—such as limiting ownership concentration—may thus backfire. Second, a preference for biased news complicates the welfare analysis of efforts aimed at reducing media bias. Specifically, it creates a trade-off between satisfying consumers' preference for like-minded news and mitigating the negative political externalities of media bias. Our findings thus demonstrate the complexity of optimal regulation.

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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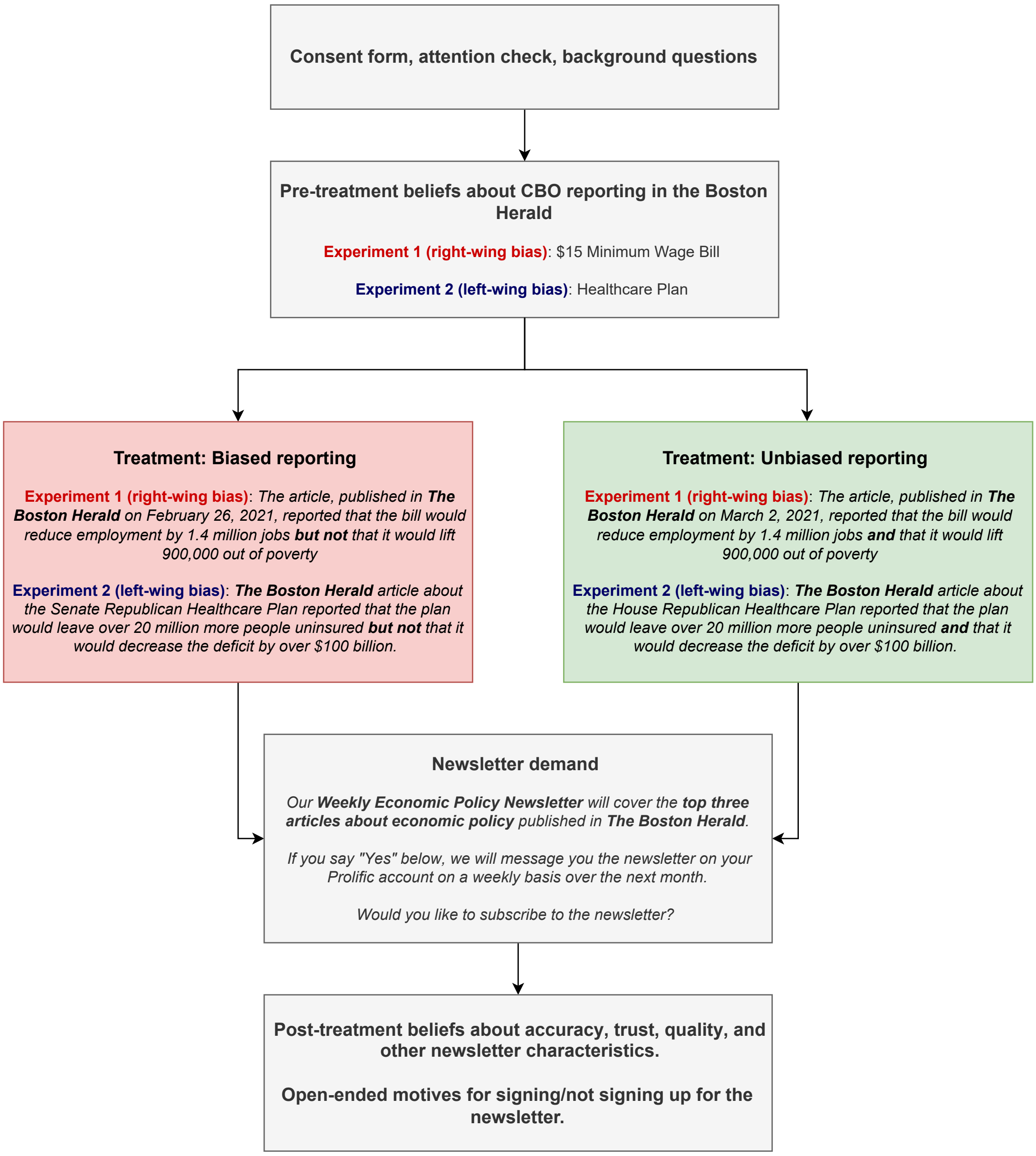
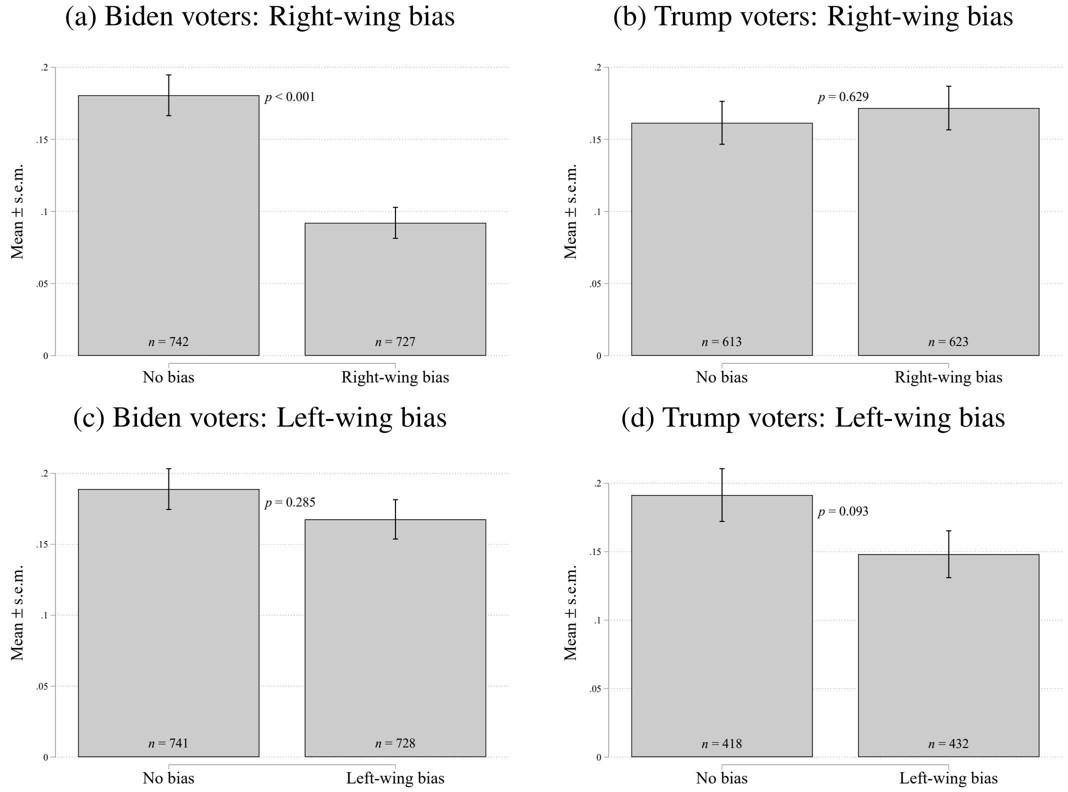
Main figures and tables

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). The full instructions for both experiments are available here: `https://raw.githubusercontent.com/cproth/papers/master/AccuracyBeliefConfirmation_instructions.pdf`.

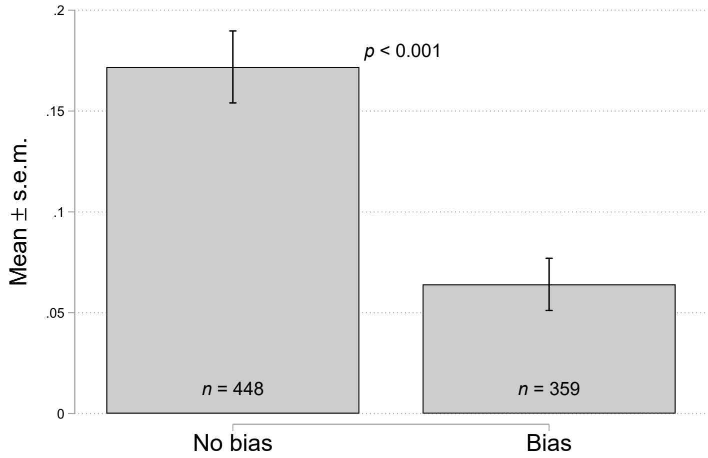
Figure 2: Newsletter demand by treatment and voting status



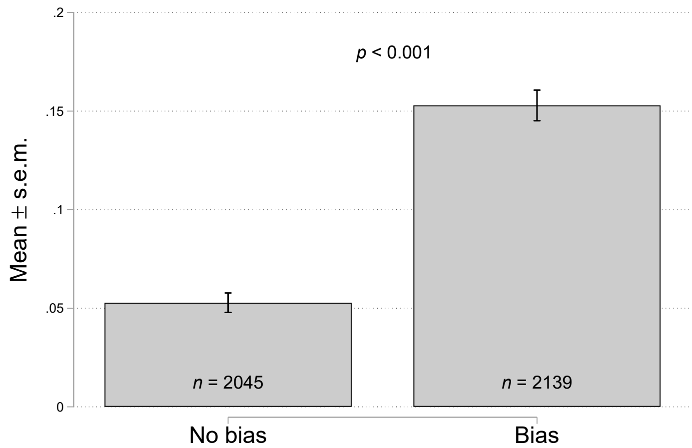
Note: This figure presents the share of respondents who chose to subscribe to the weekly politics newsletter by treatment and voting status. Panel (a) and Panel (b) present results from Experiment 1. Panel (c) and Panel (d) present results from Experiment 2. Panel (a) and Panel (c) focus on the subsample 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 two-sample t-tests of equality of means. Standard errors of the mean are shown.

Figure 3: Motives for news demand

(a) Fraction among subscribers mentioning **unbiased news** as a motive for subscribing to the newsletter, by treatment status

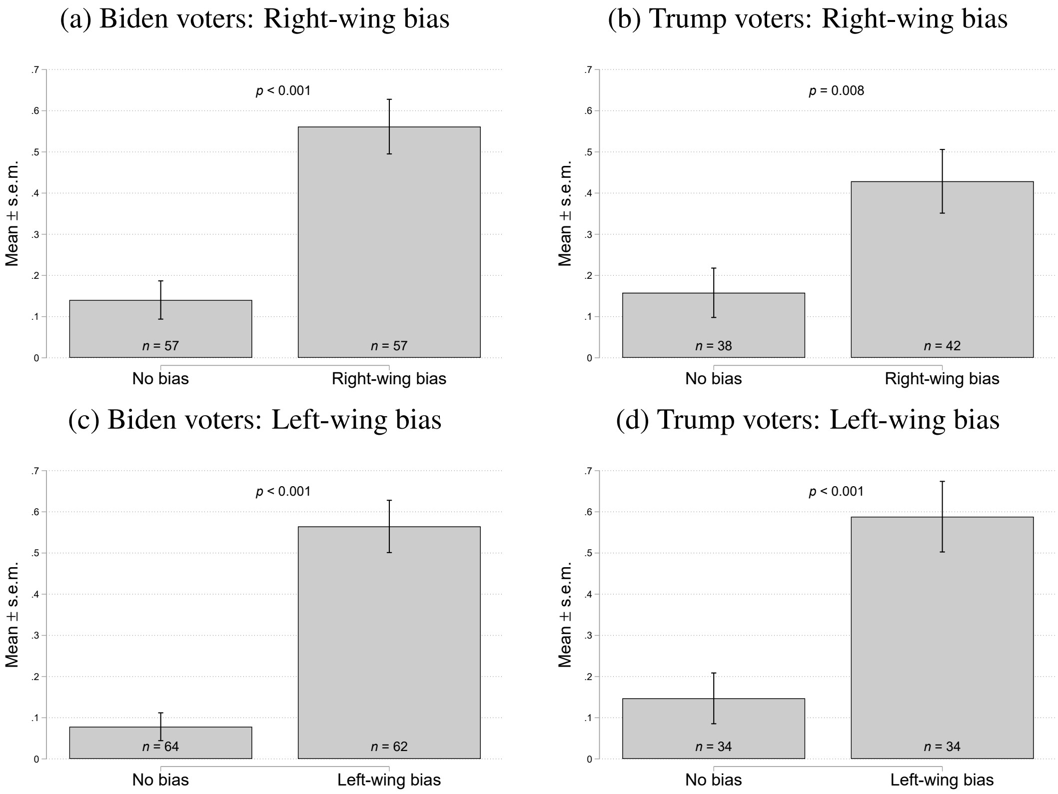


(b) Fraction among non-subscribers mentioning **biased news** as a motive for not subscribing to the newsletter, by treatment status



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 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. We hand-code all responses and create a dummy equal to one for respondents who mention unbiased news among subscribers (e.g., "It seems informative and unbiased") and biased news among non-subscribers (e.g, "Why subscribe to something that is not going to give me all the facts").

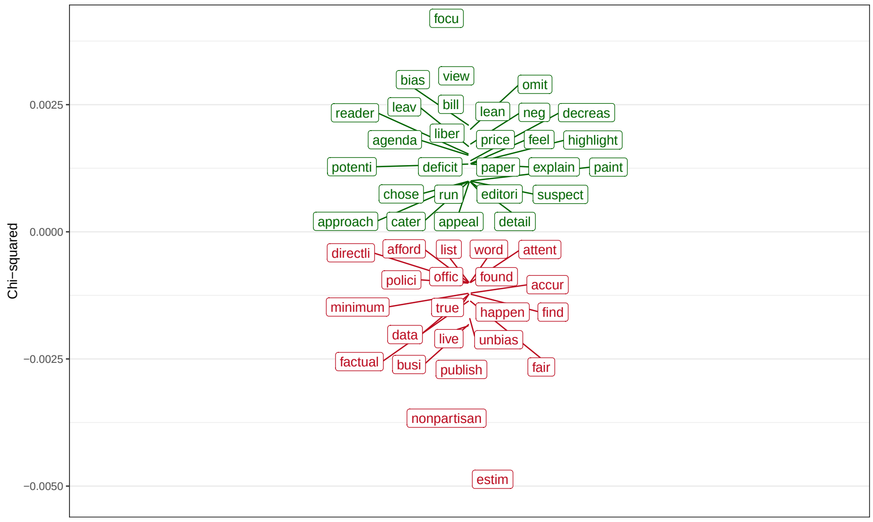
Figure 4: Treatment effects on mentioning political bias in the open-ended responses



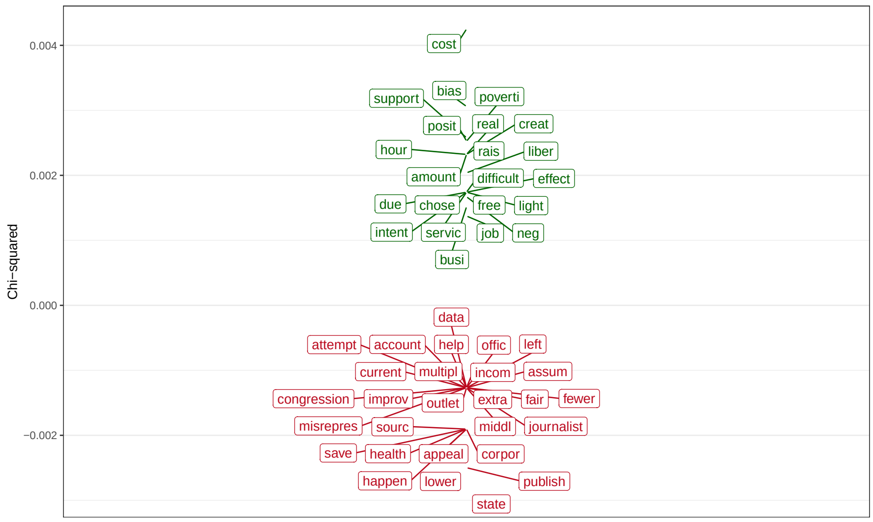
Note: The figure presents treatment effects on whether respondents mention political bias in their responses to the open-ended motives question in Experiment 3 (see Table 1). Specifically, respondents were asked why they think The Boston Herald reported in the way it did. Each panel displays the share 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 5: 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 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 stem all words 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.

Table 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. Article: `www.bostonherald.com/2021/02/26/15-minimum-wage-hurts-vulnerable-workers-the-most` **No bias treatment**: Information about how The Boston Herald covered both statistics from the CBO report. Article: `www.bostonherald.com/2021/03/02/who-wins-who-loses-with-higher-minimum-wage`)	Demand for a newsletter covering the top three 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. Article: `www.bostonherald.com/2017/06/26/cbo-22-million-more-uninsured-by-2026-under-senate-health-bill` **No bias treatment**: Information about how The Boston Herald covered both statistics from the CBO report on the Healthcare Bill. Article: `www.bostonherald.com/2017/05/24/cbo-house-gop-health-bill-projection-23-million-more-uninsured`	Demand for a newsletter covering the top three 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
Auxiliary Experiment 1: Beliefs about biases across outlets (November 2022)	Prolific: $n = 500$ AsPredicted ID: #113035	No treatments	Perceptions of media bias of 12 major news outlets
Auxiliary Experiment 2: Validation Experiment (November 2022)	Prolific: $n = 298$ AsPredicted ID: #113054	No treatments	Demand for a newsletter covering the top three articles from The New York Times Willingness to pay for 12-month NYT subscription

Note: This table provides an overview of all experiments. Links to the Boston Herald articles that we used to generate the information treatments are included in the table. Links to the underlying CBO reports are as follows: `https://www.cbo.gov/system/files/2021-02/56975-Minimum-Wage.pdf` (Experiment 1) and `https://www.cbo.gov/publication/52752` (Experiment 2). Respondents did not receive access to any of the articles. Instead, we provided them with a summary of which of the main findings from the underlying CBO report in the Boston Herald article.

Table 2: Predictions of different models of the demand for news

	Experiment 1: Right-wing bias		Experiment 2: Left-wing bias	
	Prediction (1)	Results (2)	Prediction (3)	Results (4)
Panel A: Accuracy				
Biden voters	$Y_R^B < Y_N^B$	✓	$Y_L^B < Y_N^B$	X
Trump voters	$Y_R^T < Y_N^T$	X	$Y_L^T < Y_N^T$	✓
Panel B: Belief confirmation				
Biden voters	$Y_R^B < Y_N^B$	✓	$Y_L^B > Y_N^B$	X
Trump voters	$Y_R^T > Y_N^T$	X	$Y_L^T < Y_N^T$	✓
Panel C: Both motives				
Biden voters	$Y_R^B < Y_N^B$	✓	ambiguous	$Y_L^B = Y_N^B$
Trump voters	ambiguous	$Y_R^T = Y_N^T$	$Y_L^T < Y_N^T$	✓

Note: This table summarizes the predictions outlined in Section 2.4 (columns 1, 3) and compares them to the experimental results (columns 2, 4). Panel A summarizes the predictions of models where people only care about accuracy. Panel B summarize the predictions of models where people only care about belief confirmation. Panel C summarizes the predictions of models where both accuracy and belief confirmation motives shape the demand for news. Y_i^g denote the demand for news in treatment arm $i \in \{L, N, R\}$ and political group $g \in \{B, T\}$, where B represents Biden voters, T represents Trump voters, and L, N and R denote the *left-wing bias*, *no bias* and *right-wing bias* treatment arm, respectively. The equality signs in columns 2 and 4 indicate that the differences in demand across conditions are not statistically significant ($p > 0.010$).

Table 3: 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: Biden voters						
Bias treatment (a)	-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
No bias treatment mean	0	0	0.181	0	0	0.189
p-value: Ex. 1 = Ex. 2	0.026	0.000	0.017	0.026	0.000	0.017
Panel B: Trump voters						
Bias treatment (b)	-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
No bias treatment mean	0	0	0.162	0	0	0.191
p-value: Ex. 1 = Ex. 2	0.000	0.000	0.072	0.000	0.000	0.072
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. "Bias 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 no bias group mean and standard deviation. "p-value: Ex. 1 = Ex. 2" provides p-values for tests of the equality of coefficients between Experiment 1 and Experiment 2. "p-value: a = b" provides p-values for tests of the equality of coefficients between Trump and Biden voters. 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 4: Structural model: Preferences for accuracy and biased news

	Parameter estimates:		
	(1) Full sample	(2) Biden voters	(3) Trump voters
Preference for accuracy (α)	0.241*** (0.076)	0.204** (0.085)	0.266 (0.190)
Preference for belief confirmation (β)	0.345*** (0.081)	0.374*** (0.091)	0.190 (0.160)
Relative weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	0.412*** (0.111)	0.353*** (0.131)	0.583** (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.

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The Demand for News: Accuracy Concerns versus Belief Confirmation Motives

A Theoretical appendix

A.1 Benchmark

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 1 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

[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.

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 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 $s = \{s_1, \dots, s_K\} \in S$ 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 $s = \{s_1, \dots, s_K\}$ 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 1 (Informativeness)**.** Fix $s = \{s_1, \dots, s_K\} \in S$ 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 $\rho = \rho_1, \dots, \rho_L = \rho'$ 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. $\square$

In our active no bias 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 1, 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 Structural model

B.1 Robustness

One potential concern is that perceptions of accuracy and bias are endogenous to choices. Specifically, respondents might have a taste for providing survey responses that are internally consistent (Falk and Zimmermann, 2015). In our main experiments, we elicit demand for our newsletter *before* asking respondents to state their beliefs about the newsletter's accuracy and political bias. A taste for consistency would thus imply that the act of subscribing to our newsletter has an effect on respondents' stated belief that is *independent* from our treatments, which would imply that we do not obtain unbiased estimates of respondents' beliefs in our main experiments. This could potentially bias our structural estimates of the relative importance of accuracy compared to belief confirmation motives. However, note that this cannot affect our results if the magnitude of the consistency bias in survey responses is identical for the survey measures eliciting beliefs about accuracy and beliefs about the political bias.

To address this concern, we conducted an additional pre-registered experiment on Prolific in February 2022 (Experiment 4; see Table 1).[2] 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. While this addresses concerns about consistency bias in survey responses, the absence of an active choice might lower engagement with the survey. We therefore view this as a complementary robustness check.

We then use a *two-sample* instrumental variables strategy to estimate a linear

[2]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.

probability model where the binary dependent variable is the decision to sign up to our newsletter (Angrist and Krueger, 1995; Inoue and Solon, 2010). Specifically, we use OLS to estimate equations the first-stage effect of our treatments on perceptions of accuracy and belief confirmation (see equations 3 and 4) using the belief data from Experiment 4 (where we only elicit perceptions). We then use the choice data from Experiments 1 and 2 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 F.6 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).

This suggests that consistency bias in survey responses is unlikely to account for our structural finding that accuracy and belief confirmation motives are approximately equally important drivers of people's demand for news.

B.2 Identification

This section discusses the set of assumptions that are necessary for the structural model to be identified from the experimental variation in beliefs about the Boston Herald's reporting strategy. To focus on the key arguments, we discuss the case where the propensity to subscribe to the newsletter (y_i) is *linear* in respondents' perceived accuracy (s_i) and political bias (b_i) with the understanding that analogous arguments carry over to the probit model:

$$y_i = \bar{u} + \alpha_i s_i + \beta_i b_i + \varepsilon_i \tag{6}$$

The parameters (α_i, β_i) capture the subjective value of perceived accuracy and perceived bias in reporting, respectively. We allow for unobserved heterogeneity in preferences at this stage. Ideally, we would have access to an instrument z_i^s for s_i and an additional instrument z_i^b for b_i. However, perceptions of accuracy and political bias are naturally intertwined in most settings, including ours.

Our two main experiments allow us to overcome this challenge. Let z_i^k take the value one if respondent i is assigned to the "unbiased" treatment arm in experiment $k \in \{1,2\}$, and zero otherwise. Assuming that z_i^k only affects perceptions of accuracy and political bias, we can express the subscription propensity in experiment k as

$$y_i = \bar{u} + \alpha_i(s_i + \Delta s_i^k z_i^k) + \beta_i(b_i + \Delta b_i^k z_i^k) + \varepsilon_i \tag{7}$$

where Δs_i^k and Δb_i^k capture the treatment effect on respondent i's perceptions of accuracy and bias. We can then express the treatment effect on demand in experiment k as

$$\mathbb{E}(\Delta y_i^k) = \mathbb{E}(y_i \mid z_i^k = 1) - \mathbb{E}(y_i \mid z_i^k = 0) = \mathbb{E}(\alpha_i \Delta s_i^k) + \mathbb{E}(\beta_i \Delta b_i^k) \tag{8}$$

To achieve identification, we have to assume that the treatment effects on beliefs are uncorrelated with respondents' valuation of accuracy and political bias. Specifically, we assume that

$$\mathrm{Corr}(\alpha_i, \Delta s_i^k) = \mathrm{Corr}(\beta_i, \Delta b_i^k) = 0. \tag{9}$$

Under this assumption, the treatment effect on demand in experiment k simplifies to

$$\mathbb{E}(\Delta y_i^k) = \mathbb{E}(\alpha_i)\mathbb{E}(\Delta s_i^k) + \mathbb{E}(\beta_i)\mathbb{E}(\Delta b_i^k) \tag{10}$$

We can write the treatment effects in both experiments in stacked form as

$$\begin{pmatrix} \mathbb{E}(\Delta y_i^1) \\ \mathbb{E}(\Delta y_i^2) \end{pmatrix} = \begin{pmatrix} \mathbb{E}(\Delta s_i^1) & \mathbb{E}(\Delta b_i^1) \\ \mathbb{E}(\Delta s_2^1) & \mathbb{E}(\Delta b_i^2) \end{pmatrix} \begin{pmatrix} \mathbb{E}(\alpha_i) \\ \mathbb{E}(\beta_i) \end{pmatrix} \equiv A \begin{pmatrix} \mathbb{E}(\alpha_i) \\ \mathbb{E}(\beta_i) \end{pmatrix} \tag{11}$$

In both experiments, we increase perceived accuracy relative to the *biased* treatment arms, implying that $\mathbb{E}(\Delta s_i^1), \mathbb{E}(\Delta s_i^2) > 0$. In experiment 1, we increased the perceived right-wing bias of the newsletter: $\mathbb{E}(\Delta b_i^1) > 0$. In experiment 2, we decreased the perceived right-wing bias instead: $\mathbb{E}(\Delta b_i^2) < 0$. This implies that

$$\det(A) = \mathbb{E}(\Delta s_i^1)\mathbb{E}(\Delta b_i^2) - \mathbb{E}(\Delta b_i^1)\mathbb{E}(\Delta s_2^1) < 0 \tag{12}$$

This is equivalent to A having full rank, which in turn implies that we can identify $\mathbb{E}(\alpha_i)$ and $\mathbb{E}(\beta_i)$ in Equation (11) by inverting A.

Key assumptions The above identification relies on three main assumptions:

(A1) The distribution of preferences across experiments are identical.

(A2) $\text{Corr}(\varepsilon_i, \Delta s_i^k) = 0$

(A3) $\text{Corr}(\alpha_i, \Delta s_i^k) = \text{Corr}(\beta_i, \Delta b_i^k) = 0$

Assumption (A1) is necessary for us to pool the empirical moments from both experiments. Assumption (A2) is the standard exclusion restriction and Section 4.1, among others, provides evidence supporting the assumption that our treatments only affect respondents' demand for news through changes in perceptions of accuracy and political bias in reporting. Assumption (A3) rules out that treatment effects on perceptions are correlated with preferences. For example, (A3) rules out that respondents that highly value accuracy change their perceptions of accuracy more than respondents with a weaker preference for accuracy.

Relative weight on accuracy Under assumptions (A1) to (A3), we can use our empirical estimates of $\hat{\mathbb{E}}(\alpha_i)$ and $\hat{\mathbb{E}}(\beta_i)$ to characterize the relative weight on accuracy of the *representative* agent in our sample as follows:

$$\text{Relative weight} = \frac{\hat{\mathbb{E}}(\alpha_i)}{\hat{\mathbb{E}}(\alpha_i) + \hat{\mathbb{E}}(\beta_i)}. \tag{13}$$

B.3 Estimation bias in the absence of the exclusion restriction

This section discusses how changes in perception of newsletter characteristics other than its accuracy and political bias would affect our structural estimates of the relative weight on accuracy compared to belief confirmation motives among Biden voters and Trump voters. For example, we will argue that among Trump voters, our structural estimate of the relative weight on accuracy can be interpreted as a lower bound, suggesting that accuracy concerns play a major role as a driver of their demand for news.

Trump voters Panel B of Table F.10 shows that Trump voters do not report any differences in the perceived complexity of the newsletter in the left-wing or the right-wing bias treatment arm relative to the no bias treatment arms (columns 2 and 6). However, the newsletter in the right-wing bias treatment is perceived to be 15% of a standard deviation more entertaining (column 1), while the newsletter in the left-wing bias treatment is perceived to have 15% of a standard deviation lower entertainment value (column 5). One important caveat to keep in mind is that the notion of entertainment has a nontrivial conceptual overlap with the notion of belief confirmation, which will contribute to a positive correlation in perceptions of entertainment value and political bias *even* if our treatment *only* affects beliefs about political bias. Indeed, the analysis of respondents' open-ended responses in Section 4.1 suggests that our treatments puts thoughts of accuracy and political bias on top of people's minds, suggesting that the above correlations are driven primarily by the conceptual overlap of these constructs. Moreover, the effect sizes on perceptions of accuracy and bias are substantially larger (as shown in Table 3), suggesting that the effect of any changes in perceptions of entertainment may only be of second-order importance.

But taking the effects on perceived entertainment at face value, we can investigate how this factor would affect our structural estimates of the relative weight on accuracy compared to belief confirmation motives as reported in Table 4. Assuming that Trump voters prefer more entertaining news ceteris paribus, the asymmetric treatment effects on entertainment across experiments would introduce a positive omitted variable bias in our estimate of the preference for belief confirmation: Accuracy perceptions are negatively affected in both experiments, but the directional effects on entertainment coincide with the directional changes in perceived right-wing bias. The measure of perceived right-wing bias would thus pick up the utility changes due to changes in the entertainment value, thus potentially overstating the relevance of belief confirmation motives among Trump voters.

This suggests that our estimate of the relative weight on accuracy among Trump voters should be interpreted as a lower bound. More specifically, even if our treatment affected the perceived entertainment value in ways other than by altering perceived belief confirmation, our structural estimates would suggest that accuracy concerns are *at least as important* as belief confirmation motives among Trump voters.

Biden voters The direction of a potential omitted variable bias in our structural estimation is ambiguous among Biden voters. First, we observe that Biden voters in both the right-wing and the left-wing bias treatment arms report a lower perceived complexity of the newsletter (columns 2 and 6 in Panel A of Table F.10). A natural assumption would be that, ceteris paribus, Biden voters prefer lower complexity in reporting. As the effect size is constant across treatment arms, this would negatively bias our structural estimate of Biden voter's preference for accuracy: Our measure of perceived accuracy would pick up both the expected utility losses from lower accuracy in both treatment arms, and the partially offsetting utility gains from lower complexity. Second, Biden voters report a lower entertainment value in both of the bias treatment arms, with larger effects in the right-wing bias treatment (columns 1 and 5). This might affect our structural estimates by introducing a positive omitted variable bias on our estimate of the weight on accuracy. However, it is important to keep the caveats outlined above in the paragraph on Trump voters in mind when interpreting the effects on the perceived entertainment value.

Taken together, the net effect of the potential negative omitted variable bias from changes in perceived complexity and the positive bias from changes in the entertainment value on our estimate of the relative importance of accuracy compared to belief confirmation motives is ambiguous for Biden voters. However, given the three to six times larger effect sizes on perceptions of accuracy and bias compared to perceptions of entertainment and complexity, we would expect that the net effect is comparatively small. It is thus unlikely to overturn the insights from the structural model that both accuracy and belief confirmation motives are important drivers of people's demand for news.

C Text analysis of subscription motives

We present complementary analyzes that corroborate our results on people's subscription motives. We use alternative ways of classifying respondents that do not draw on our hand-coding of respondents' text responses, which we presented in Section 3.4.

C.1 Frequency of mentioning bias

In this section, we examine respondents' tendency to justify their decision by referring to the political bias of the newsletter using an alternative procedure to classify responses. Specifically, instead of relying on the hand-coding of text responses, we use a rule-based approach that classifies responses based on whether synonyms of "biased" and "unbiased" appear in respondents' open-ended text responses. Table C.1 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.[3] 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 no bias 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 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 no bias group mean of 1.9%.

C.2 Characteristic phrases

This section contains the results from an alternative way of analyzing the open-ended responses to the question of why respondents chose to subscribe (or not subscribe) to our weekly newsletter. Instead of analyzing the codes assigned to responses by trained research assistants, 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. Specifically, given two groups A and B of respondents, we calculate Pearson's χ^2 statistic for each word w,

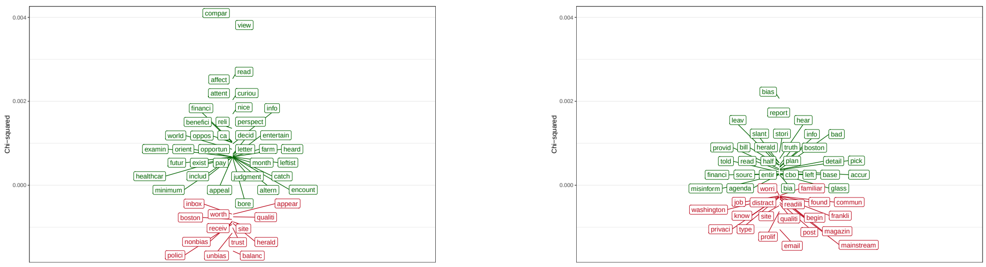
$$\chi^2_{wAB} = \frac{(f_{wA} f_{\sim wB} - f_{wB} f_{\sim wA})^2}{(f_{wA} + f_{wB})(f_{wA} + f_{\sim wA})(f_{wB} + f_{\sim wB})(f_{\sim wA} + f_{\sim wA})} \tag{14}$$

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.

[3] 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."

Figure C.1: Motives for news demand: Most distinctive phrases by treatment status

(a) Distinctive phrases of subscribers (b) Distinctive phrases of non-subscribers



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 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).

Figure C.1 presents the 50 words with the largest absolute 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.

Table C.1: 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
No bias treatment 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.

D Deviations from the pre-analysis plan

Our experiments were all preregistered in the AsPredicted registry (#78800, #80266, #87947, #89081, #113035, and #113054). 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.

E 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.

Willingness to pay experiment In auxiliary experiment 2 (see Table 1), we validated our main behavioral outcome of whether respondents are willing to sign up for a weekly newsletter by showing that it is strongly correlated with respondents' incentivized willingness to pay for 12-month subscription.[4] In this study, we elicited respondents willingness to subscribe to a weekly newsletter about economic policy featuring articles from The New York Times. We designed and delivered the newsletter in the same way as the newsletter that was part of our main experiments.

[4] See Section G.7 for the experimental instructions: `https://raw.githubusercontent.com/cproth/papers/master/AccuracyBeliefConfirmation_instructions.pdf`.

F Additional tables and figures

Table F.1: Summary statistics

	(1) US pop.	(2) Exp 1	(3) Exp 2	(4) Exp 3	(5) Exp 4	(6) Au. Exp. 1	(5) Au. Exp. 2
Male	0.492	0.468	0.436	0.479	0.481	0.508	0.497
Age (years)	47.78	35.487	36.304	35.737	38.829	41.390	39.332
White	0.763	0.834	0.840	0.827	0.821	0.836	0.738
Employed	0.620	0.681	0.724	0.724	0.715	0.774	0.691
College	0.329	0.649	0.678	0.683	0.695	0.684	0.708
High income	0.482	0.443	0.429	0.461	0.446	0.440	0.389
Northeast	0.17	0.174	0.194	0.157	0.189	0.266	0.195
Midwest	0.21	0.231	0.235	0.206	0.204	0.234	0.188
South	0.38	0.389	0.398	0.412	0.396	0.382	0.383
West	0.24	0.206	0.173	0.224	0.211	0.118	0.235
Vote Trump	0.469	0.457	0.367	0.381	0.493	0.236	0.178
Observations		2,705	2,319	388	1,910	500	298

Note: This table displays the mean value of basic covariates for the US population (column 1) as well as for each experiment (see Table 1 for an overview of the experiments). We obtained population data from the 2019 American Community Survey and the U.S. Census Bureau "QuickFacts" tool. "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. "Employed" is a dummy variable taking value one if the respondent is employed full-time, part-time, or self-employed. "High income" is a binary variable taking value one if the respondent has pre-tax household annual income above $75,000. "College degree" is a binary variable taking value one if the respondent has at least a bachelor's degree. "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 F.2: Balance tests

	(1)	(2)	(3)	(4)	(5)	(6)
	Experiment 1			Exeriment 2		
Variable	μ: NB	μ: B	NB - B	μ: NB	μ: B	NB - B
Male	0.47	0.47	-0.00	0.46	0.41	-0.05**
Age	35.18	35.80	0.62	36.36	36.24	-0.12
White	0.83	0.84	0.00	0.83	0.85	0.01
High income	0.43	0.45	0.02	0.42	0.43	0.01
College degree	0.65	0.65	-0.00	0.68	0.67	-0.01
Full-time employee	0.49	0.49	-0.00	0.52	0.55	0.02
Northeast	0.18	0.17	-0.00	0.19	0.20	0.01
Midwest	0.24	0.23	-0.01	0.23	0.24	0.00
West	0.20	0.21	0.00	0.18	0.17	-0.00
South	0.39	0.39	0.01	0.40	0.39	-0.01
Observations	1,355	1,350	2,705	1,159	1,160	2,319
	Experiment 3			Experiment 4		
Variable	μ: NB	μ: B	NB - B	μ: NB	μ: B	NB - B
Male	0.47	0.49	0.02	0.48	0.48	-0.00
Age	36.36	35.12	-1.23	39.03	38.63	-0.39
White	0.84	0.82	-0.02	0.82	0.82	-0.01
High income	0.41	0.51	0.09*	0.43	0.46	0.03
College degree	0.67	0.70	0.03	0.70	0.69	-0.00
Full-time employee	0.53	0.54	0.01	0.48	0.51	0.03
Northeast	0.16	0.16	0.00	0.20	0.18	-0.02
Midwest	0.18	0.23	0.05	0.21	0.20	-0.00
West	0.26	0.19	-0.07	0.21	0.22	0.01
South	0.40	0.42	0.02	0.39	0.40	0.02
Observations	193	195	388	952	958	1,910

Note: This table provides a balance test between the two treatment groups for all four experiments (see Table 1 for an overview of the experiments). The mean (μ) is indicated separately for the no bias (NB) and bias (B) treatment groups, while NB - B shows the difference in means between the two groups. Stars indicate if the difference in means is statistically significant (* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$). "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. "High income" is a binary variable taking the variable one if the respondent reported an income above $75,000, and zero otherwise. "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 F.3: 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								
Bias 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								
Bias 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. "Bias 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 no bias 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 F.4: Structural model: Preferences for accuracy and biased news — Robustness to using non-z-scored perceptions of accuracy and bias

	Parameter estimates:		
	(1) Full sample	(2) Biden voters	(3) Trump voters
Preference for accuracy (α)	0.337***	0.297**	0.353
	(0.103)	(0.117)	(0.250)
Preference for belief confirmation (β)	0.423***	0.475***	0.224
	(0.112)	(0.136)	(0.191)
Relative weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	0.443***	0.385***	0.612**
	(0.113)	(0.135)	(0.264)
N	5,014	2,930	2,084

Note: This table presents parameter estimates that are analogous to the IV probit estimates presented in Table 4 except for one difference: Instead of using the z-scored post-treatment measure of perceived accuracy and belief confirmation (which are measured on 5-point Likert scales), we use *non*-z-scored perceptions of accuracy and belief confirmation.

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

Table F.5: 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)
Relative 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 4 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.

Table F.6: Structural model: Robustness to using 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)
Relative 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 F.7: Structural model: Replacing the belief confirmation measure with perceived complexity or perceived entertainment value

	Parameter estimates:		
	(1) Full sample	(2) Biden voters	(3) Trump voters
Panel A: Complexity instead of bias			
Preference for accuracy (α)	0.278	-0.080	0.364
	(0.240)	(0.690)	(0.260)
Preference for simplicity (β)	-0.171	-1.257	0.701
	(0.696)	(1.936)	(1.102)
Weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	2.598	0.060	0.342
	(20.312)	(0.400)	(0.283)
N	5,014	2,930	2,084
Panel B: Entertainment instead of bias			
Preference for accuracy (α)	0.364***	0.526***	0.261
	(0.130)	(0.177)	(0.202)
Preference for entertainment (β)	0.009	-0.500	0.529
	(0.429)	(0.528)	(0.522)
Weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	0.975	20.335	0.331
	(1.129)	(306.462)	(0.311)
N	5,014	2,930	2,084

Note: This table presents parameter estimates that are analogous to the IV probit estimates presented in Table 4 except for two differences: Panel A replaces the z-scored post-treatment measure of belief confirmation with a z-scored post-treatment measure of perceived simplicity (i.e., the reverse-coded perception of complexity). Panel B replaces the z-scored post-treatment measure of belief confirmation with a z-scored post-treatment measure of entertainment value.

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

Table F.8: Structural model: Heterogeneity by educational attainment

	Parameter estimates:			
	Biden voters		Trump voters	
	(1) No college	(2) College	(3) No college	(4) College
Preference for accuracy (α)	-0.041	0.279***	-0.284	0.476**
	(0.183)	(0.094)	(0.350)	(0.190)
Preference for belief confirmation (β)	0.390***	0.371***	0.177	0.147
	(0.148)	(0.115)	(0.283)	(0.197)
Relative weight on accuracy $\left(\frac{\alpha}{\alpha+\beta}\right)$	-0.119	0.429***	2.657	0.764***
	(0.571)	(0.137)	(9.787)	(0.259)
N	886	2,044	807	1,277

Note: This table presents parameter estimates that are analogous to the IV probit estimates presented in Table 4. We separately estimate the structural model for Biden voters and Trump voters with and without a college degree.

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

Table F.9: 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
Bias treatment	0.004	-0.047***	-0.129***	-0.035	-0.049**	-0.031
	(0.023)	(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 F.10: 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								
Bias 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
No bias treatment mean	0	0	0	0.806	0	0	0	0.870
p-value: Ex. 1 = Ex. 2	0.017	0.902	0.715	0.912	0.017	0.902	0.715	0.912
Panel B: Trump voters								
Bias 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
No bias treatment mean	0	0	0	0.664	0	0	0	0.780
p-value: Ex. 1 = Ex. 2	0.001	0.736	0.279	0.232	0.001	0.736	0.279	0.232
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. "Bias 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. The outcome variables in columns 1–3 and 5–7 are z-scored using the relevant no bias 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 F.11: Experiment 4: Treatment effects

	Left-wing bias						Right-wing bias					
	(1) Accuracy	(2) Left-wing bias	(3) Trust	(4) Quality	(5) Entertainment	(6) Complexity	(7) Accuracy	(8) Left-wing bias	(9) Trust	(10) Quality	(11) Entertainment	(12) Complexity
Panel A: Biden voters												
Bias treatment	-0.576***	0.384***	-0.495***	-0.313***	-0.157*	-0.300***	-0.980***	-0.822***	-0.980***	-0.825***	-0.419***	-0.271***
	(0.093)	(0.111)	(0.088)	(0.092)	(0.084)	(0.091)	(0.095)	(0.107)	(0.100)	(0.095)	(0.093)	(0.089)
N	486	486	486	486	486	486	477	477	477	477	477	477
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Panel B: Trump voters												
Bias treatment	-0.477***	0.408***	-0.393***	-0.446***	-0.123	-0.064	-0.231**	-0.308***	-0.145	-0.081	0.039	-0.037
	(0.094)	(0.100)	(0.093)	(0.102)	(0.099)	(0.093)	(0.096)	(0.104)	(0.094)	(0.099)	(0.100)	(0.095)
N	473	473	472	472	472	472	460	460	460	460	460	460
Controls	Yes	Yes	Yes	Yes	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 1 for an overview of experiments). Panel A shows results for Biden voters and Panel B shows results for Trump voters. "Bias treatment" is a binary indicator for whether respondents were informed that The Boston Herald reported the news in a left-wing biased way (columns 1–6) or in a right-wing biased way (columns 7–12). "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." "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 no bias group mean and standard deviation. All regressions include the standard set of control variables.

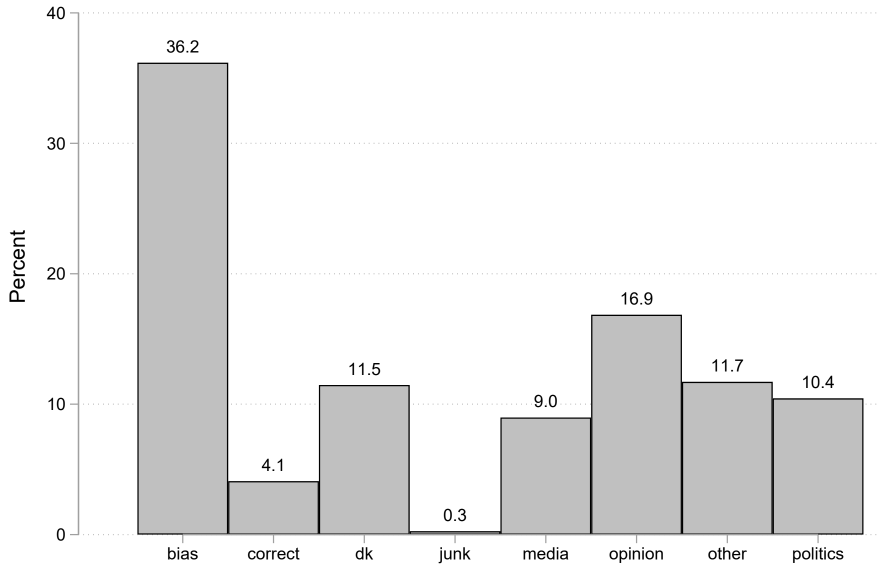
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Robust standard errors in parentheses.

Table F.12: Robustness: Results among respondents who did not correctly guess the study purpose or guessed that it is about "bias"

	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: Biden voters						
Bias treatment (a)	-0.779***	-0.763***	-0.062**	-0.601***	0.241***	0.008
	(0.076)	(0.086)	(0.024)	(0.074)	(0.079)	(0.026)
N	761	761	761	817	817	817
Z-scored	Yes	Yes	No	Yes	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes
No bias treatment mean	0	0	0.188	0	0	0.169
p-value: Ex. 1 = Ex. 2	0.080	0.000	0.045	0.080	0.000	0.045
Panel B: Trump voters						
Bias treatment (b)	-0.078	-0.509***	0.007	-0.530***	0.199**	-0.056*
	(0.069)	(0.077)	(0.026)	(0.087)	(0.084)	(0.030)
N	815	815	815	580	580	580
Z-scored	Yes	Yes	No	Yes	Yes	No
Controls	Yes	Yes	Yes	Yes	Yes	Yes
No bias treatment mean	0	0	0.160	0	0	0.206
p-value: Ex. 1 = Ex. 2	0.000	0.000	0.106	0.000	0.000	0.106
p-value: a = b	0.000	0.316	0.046	0.679	0.969	0.104

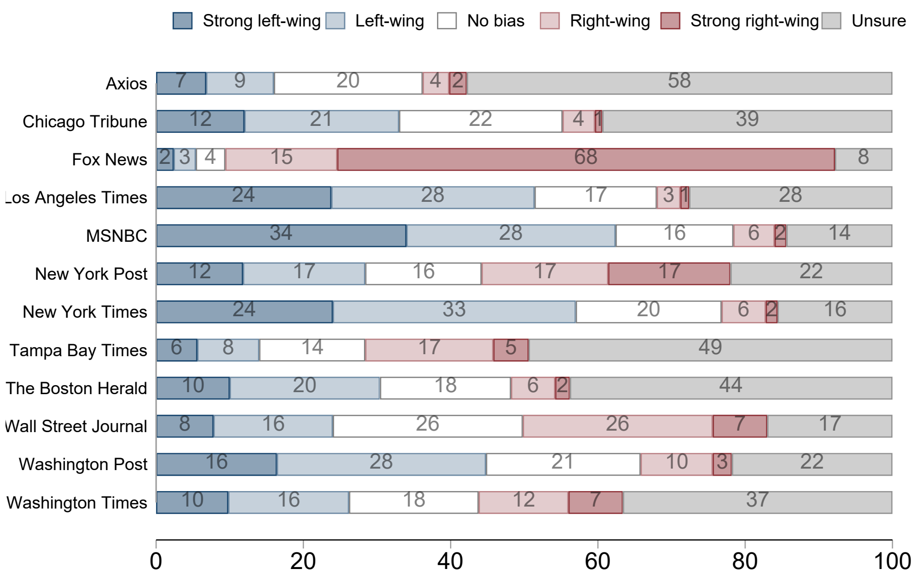
Note: This table presents OLS regression estimates using data from Experiment 1 (columns 1–3) and Experiment 2 (columns 4–6) where the dependent variables are post-treatment beliefs about accuracy (columns 1 and 4), the perceived left-wing bias of the newsletter (columns 2 and 5), and newsletter demand (columns 3 and 6). Panel A and Panel B present results for Biden and Trump voters, respectively. The regressions only include the subset of respondents who did not correctly guess the hypothesis of the study at the open-ended question about study purpose at the end of the survey. We also exclude respondents who guess that the study was about "bias". "Bias 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 no bias group mean and standard deviation. "p-value: Ex. 1 = Ex. 2" provides p-values for tests of the equality of coefficients between Experiment 1 and Experiment 2. "p-value: a = b" provides p-values for tests of the equality of coefficients between Trump and Biden voters. 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.

Figure F.1: Perceived study purpose



Note: This figure shows the distribution of the perceived study purpose among our respondents in Experiment 1 and 2. Specifically, at the end of the main experiments, respondents were asked the following open-ended question: "If you had to guess, what would you say was the purpose of this study?" A team of research assistants hand-coded the responses based on the following coding scheme: *bias*: Explicit mentions of bias in the media. *correct*: People correctly guessing the study's hypothesis (how perceptions of bias shape people's news consumption). *junk*: Nonsensical responses. *media*: Generic mentions that the study is about perceptions of media (without explicitly mentioning bias). *opinion*: Generic mentions that the study tries to assess opinions and attitudes. *dk* (don't know): People expressing uncertainty. *other*: Responses that do not fit into any of the other categories. *politics*: People generically about politics in a generic way.

Figure F.2: Beliefs about bias across outlets



Note: This figure uses data from Auxiliary Experiment 1 and presents the distribution of responses to the following question: "What kind of political bias do you expect the news outlets below to have?"