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Pluralistic ignorance and climate policies: Information provision experiment

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ABSTRACT

This study examines how informational interventions that aim to correct systematic misperceptions of others' beliefs (i.e., pluralistic ignorance) affect individuals' willingness to engage in both private and publicly observable behaviors. We investigate this question in the context of carbon taxation using an online experiment with participants in the United States. Specifically, we test whether providing accurate information about public support for carbon taxation influences climate-related actions. While this informational intervention reduced misperceptions, it marginally lowered private climate action, measured by donations to an organization advocating for climate policies, although this effect was not statistically significant. The intervention did not affect the behavior of individuals who underestimated support for carbon taxation but negatively impacted the donation decisions of those who overestimated support, suggesting a boomerang effect. In contrast, the intervention increased public climate action, measured by the minimum reward participants required to attend climate policy discussions with peers. These findings suggest that correcting misperceived norms can generate heterogeneous behavioral responses, depending on individuals' prior beliefs and the type of action considered.

1. Introduction

People often misperceive others' beliefs and behaviors. Such systematic misperceptions, known as pluralistic ignorance (Miller and McFarland, 1991), have been documented in a variety of settings, including alcohol consumption (Prentice and Miller, 1993), school bullying (Dillon and Lochman, 2022), racial discrimination (Katz et al., 1931; O'Gorman, 1975), "canceling" behavior (Dias et al., 2025), women's reproductive rights (Fornaro, 2025), gender norms in the labor market (Bursztyn et al., 2020; De Souza and Schmader, 2022), tax compliance (Wenzel, 2005), public health behaviors (Urminsky and Bergman, 2021), and climate change action (Leviston et al., 2013; Sokoloski et al., 2018; Mindenberger and Tingley, 2019; Pompeo and Serdarevic, 2021; Sparkman et al., 2022; Drews et al., 2022; Fang and Innocenti, 2023; Andre et al., 2024a, 2024b).

Pluralistic ignorance can have important consequences for both individuals and society at large. If individuals believe that their opinions are unpopular, they may suppress the expression of their views (Noell-Neumann, 1974) or even publicly express views they do not hold because of fear of social, reputational, or material consequences (Kuran, 1989, 1998). This dynamic can lead to situations in which one viewpoint becomes dominant in public discourse (Noell-Neumann, 1974), thereby entrenching suboptimal or outdated norms. As a result, policymakers may implement policies that the public privately does not support or fail to address problems that

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silent majorities consider important.

A growing body of empirical research shows that providing accurate information about prevailing social norms can correct such misperceptions (Dillon and Lochman, 2019; Mindenberger and Tingley, 2019; Bursztyn et al., 2020; Burzstyn and Yang, 2022; Ecker et al., 2023; Andre et al., 2024b). However, evidence on the effectiveness of informational interventions in changing behavior is mixed (for a review, see Miller, 2023), and many studies report at most small effects (e.g., Dillon and Lochman, 2019; Pompeo and Serdarevic, 2021; De Souza and Schmader, 2022; Drews et al., 2022; Engler et al., 2025; Fang and Innocenti, 2023; Vlasceanu et al., 2024). One possible explanation is that informational interventions may operate asymmetrically depending on individuals' prior beliefs relative to the true norm. Individuals who initially underestimate the norm may update their beliefs upward and align their behavior with their revised perceptions. In contrast, individuals who overestimate the norm may revise their beliefs downward and consequently perceive weaker normative backing, potentially reducing their engagement. Moreover, norm corrections may affect publicly observable behaviors differently than those conducted in private, as the former are more influenced by concerns about social isolation, reputational exposure, and group identity signaling. Thus, the same informational treatment may generate heterogeneous behavioral responses depending on individuals' prior beliefs and the type of behavior considered.

These considerations call for a more disaggregated perspective when studying pluralistic ignorance. However, this perspective has received limited attention in the literature, including in high-stakes collective action domains such as climate change (e.g., Drews et al., 2023; Fang and Innocenti, 2023). We therefore ask: How do informational interventions aimed at correcting misperceptions affect individuals' willingness to engage in both *private* and *publicly* observable behaviors? And how do these effects depend on individuals' prior beliefs?

We study these questions in the context of public support for carbon taxation in the United States (US). This setting is well suited to examining the behavioral consequences of correcting misperceived norms for several reasons. First, surveys consistently document systematic gaps between actual and perceived support for climate policies, indicating the presence of pluralistic ignorance (Drews et al., 2022; Sparkman et al., 2022; Fang and Innocenti, 2023; Andre et al., 2024a). For example, Sparkman et al. (2022) find that Americans underestimate support for various climate policies by 20–40 %. Second, carbon taxation is a concrete and economically consequential policy instrument rather than an abstract attitude, allowing us to test whether norm corrections influence engagement in a potentially costly collective action domain. Third, political polarization surrounding climate change and climate policies in the US generates substantial heterogeneity in prior beliefs (Sparkman et al., 2022), which allows us to examine how informational corrections operate under differing initial perceptions. In this study, we focus on individuals affiliated with the two major political parties, the Democrats and the Republicans, which consistently exhibit the most pronounced divergence in climate-related beliefs and behaviors (Dunlap et al., 2016; Van Boven et al., 2018; Pew Research Center, 2020, 2025; Leiserowitz et al., 2023).

We conducted an online experiment in which participants were randomly assigned to one of two experimental conditions: an information treatment or a control group. In both conditions, we first elicited participants' prior beliefs about support for carbon taxes among people in the US. Participants in the information treatment then received information about the actual level of carbon tax policy support in the US (Leiserowitz et al., 2023), while those in the control group did not receive this information. We then elicited measures of both private and public climate actions. In this context, we defined private climate actions as behaviors performed individually and without public visibility, whereas public actions were outward-facing and observable by others. To measure private climate action, participants made an incentivized donation decision by allocating $10 between themselves and the Citizen's Climate Lobby, an organization advocating for climate policies, including carbon taxation. To measure public climate action, we elicited participants' minimum acceptable reward for attending a climate policy discussion meeting scheduled within two weeks after the experiment. During this video meeting, each participant would be paired with another participant from the same political party, encouraging discussion among like-minded individuals. Additionally, we collected data on participants' self-reported willingness to attend the meeting, their support for various climate policies, socio-demographic characteristics, and their perceptions of the credibility of the information provided in the treatment.

Our findings revealed that while the information about actual support for carbon taxation reduced misperceptions about public support for the climate policy, it lowered private climate action marginally, as reflected in slightly lower donation levels, though this effect was not statistically significant. Interestingly, this intervention did not affect the behavior of those who underestimated support for carbon taxation but negatively impacted the donation decisions of those who overestimated support, suggesting a boomerang effect. However, the information increased individuals' engagement in public climate action, as participants reduced the minimum reward they required to attend climate policy discussions with peers. We found no evidence that the treatment affected self-reported policy support.

There are two potential mechanisms that may explain the observed difference in private behavior. First, reducing donations is privately rewarding because it results in a monetary gain, whereas increasing donations requires accepting a monetary loss. Since losses loom larger than gains, individuals may resist upward adjustments more than downward ones (Kahneman and Tversky, 1979). Consequently, reducing costly action may be psychologically easier, whereas increasing costly action may be more difficult and could lead individuals to reinterpret the provided information in self-serving ways to maintain their current behavior (Bicchieri and Dimant, 2019).

Second, this difference may be reinforced by concerns about being the "sucker" in a public goods setting (Fehr and Gächter, 2000; Fischbacher et al., 2001). When support is lower than expected, scaling back contributions becomes reasonable and reduces the risk of paying more than others. Thus, for costly behaviors conducted in private, downward adjustment may be both less costly and safer than upward adjustment.

This study contributes to the literature in several important ways. First, we show that norm interventions can produce different responses for private behaviors depending on prior misperceptions, including boomerang effects among overestimators. This

complements studies showing that normative messages can generate boomerang effects in other contexts, such as littering (Reich and Robertson, 1979), alcohol consumption (Ringold, 2002; Wechsler et al., 2003), and electricity use (Schultz et al., 2007). For example, Schultz et al. (2007) found that informing homeowners about how much electricity they used compared with their neighbors led above-average consumers to reduce their usage but caused below-average consumers to increase theirs. Our results also complement studies showing that, in public goods settings, highlighting anti-social norms can be more contagious than highlighting pro-social ones (e.g., Lefebvre et al., 2015; Dimant, 2019). For instance, Lefebvre et al. (2015) found in an experiment conducted in three European countries that examples of low tax compliance increased tax evasion, whereas examples of high compliance had no significant effect. Our findings also contribute to environmental studies documenting heterogeneous responses to norm-based interventions depending on individuals' attitudes, such as concern about climate change (Geiger and Swim, 2016), climate skepticism (Andre et al., 2024b), or the degree of false consensus, that is, the extent to which individuals overestimate the prevalence of their own views in the population (Drews et al., 2023).

Second, we examine both private and publicly observable policy-relevant behaviors within a unified framework, demonstrating that norm information may differentially affect expressive behavior and privately costly actions. In the context of climate change, most previous studies using informational interventions have examined either the private (e.g., Mildenberger and Tingley, 2019; Drews et al., 2022; Fang and Innocenti, 2023) or the public dimension separately (e.g., Geiger and Swim, 2016). Our study integrates both elements and thereby complements work that attempts to study private and public behaviors within the same setting (e.g., Pompeo and Serdarevic, 2021; Andre et al., 2024b; Vlasceanu et al., 2024). For example, Andre et al. (2024b) found that norm-based informational interventions increased incentivized general willingness to fight climate change but had no significant effect on self-reported willingness to engage in political activism.

Third, this study employs incentivized measures rather than self-reports to elicit climate-related beliefs and behaviors. Incentivized decisions are generally more accurate, as monetary incentives can motivate participants to exert greater cognitive effort and disclose their true preferences (Forsythe et al., 1994; Charness et al., 2021), thereby reducing the influence of social desirability bias often associated with self-reports (Vesely and Klöckner, 2020). For instance, we measure public climate action by assessing participants' incentivized willingness to accept a monetary reward in exchange for participating in climate policy discussions. To our knowledge, this represents a novel methodological approach in the context of pluralistic ignorance and climate policy. In contrast, prior studies seeking to capture public climate action have typically relied on self-reported intentions to share information on social media (Pompeo and Serdarevic, 2021; Vlasceanu et al., 2024) or engage in political activism (Andre et al., 2024b).

While we study carbon taxation as a specific empirical context, the mechanisms we examine—belief updating about descriptive norms, heterogeneous responses to norm corrections, and differential effects on private versus publicly observable actions—are not unique to climate policy. Similar dynamics are likely to arise in other domains characterized by pluralistic ignorance and collective action, including public health behaviors, workplace norms, and politically contested social policies. More broadly, our findings highlight that informational interventions correcting misperceived norms may not produce uniform behavioral responses. Instead, their effects may depend on individuals' prior beliefs and on whether the targeted behaviors are privately undertaken or publicly visible. These considerations suggest that norm-based informational interventions should be designed and interpreted with caution, as they may generate heterogeneous responses and, in some cases, unintended behavioral effects.

The remainder of the paper is organized as follows. Section 2 outlines the experimental design, including the sample, detailed procedures, and experimental conditions. Section 3 presents the main and supplementary results of the study. Section 4 discusses the findings and concludes the paper.

2. Experimental design

2.1. Sample

We designed an online between-subjects experiment with two treatments.[1,2] The experiment was conducted in March 2024 on the Prolific platform. We recruited a total of 1587 participants from the US.[3,4] Participation in the experiment was restricted to individuals affiliated with either the Democratic (50.2 %) or Republican (49.8 %) party. Although Independents are an important and politically relevant group, we excluded them to focus on individuals affiliated with the two major parties, which consistently exhibit the most pronounced divergence in climate-related beliefs and behaviors (Dunlap et al., 2016; Pew Research Center, 2020; Sparkman et al., 2022; Leiserowitz et al., 2023). The sex distribution was 50.1 % male and 49.9 % female, and the average participant age was 43 years. More detailed descriptive statistics and balance checks are provided in Table 1. Some imbalances were observed in individual

[1] This experiment received ethical approval from the Committee on Research Ethics in Economics at the Faculty of Economics and Business Administration, application no. EC2024_01.

[2] The experiment was preregistered at AsPredicted (https://aspredicted.org/9TZ_G5J).

[3] Initially, we recruited 1600 participants. Based on preregistered exclusion criteria, 13 individuals who completed the experiment in under 2 minutes or took longer than 3 minutes on the priors' page were excluded from the analysis.

[4] To test the robustness of our findings, we repeated the main analysis using a reduced sample ($N = 1{,}505$) that excluded participants who took >84 seconds to complete the priors page (based on the mean plus two standard deviations of page submission time). This cutoff was not preregistered and should be interpreted as an exploratory sensitivity check. The results remained largely consistent with our main findings and are available upon request.

characteristics, such as education and income, between the experimental groups, indicating the need to control for these variables in the regression analysis.

2.2. Procedure

Participants in the experiment were paid $1.2 for their participation, with the potential for a bonus based on the accuracy of their responses and luck. On average, participants took approximately 7 min to complete the main experiment, and the average individual payoff was approximately $1.3. Both parts of the experiment were conducted using the Qualtrics survey software.

Fig. 1 outlines the main parts of the experiment and their sequence. The instructions for participants are described in detail in the Appendix A. First, socio-demographic information such as the country of residence, sex, political affiliation, age, education, income, and employment status was collected (block 1 in Fig. 1). Participants were then asked to express their agreement or disagreement with five general statements on climate change using a 7-point Likert scale. These questions, designed to measure participants' general views on climate change, were placed at the beginning of the experiment to ensure that responses were not influenced by subsequent treatments (block 2 in Fig. 1).

Next, an incentivized question was used to elicit participants' prior beliefs about the support for carbon taxation among people in the US (block 3 in Fig. 1). Participants were informed that many people in the US had recently been polled about their attitudes toward various climate policies. Participants were then asked to estimate the level of support among US registered voters for the following climate policy: "Requiring fossil fuel companies to pay a carbon tax and using the money to reduce other taxes (such as income tax) by an equal amount." This climate policy wording was taken from the Climate Change in the American Mind survey (Leiserowitz et al., 2023). The participants were given 3 min to answer the question and submit their response. This time limit was set based on our pre-testing, which showed that although it was possible to find the correct answer online within 3 min, doing so required navigating multiple sources and downloading an official report. Importantly, participants were not told where the information was published or whether it was publicly accessible. They were informed that the 5 % who guessed closest to the correct number would be paid $0.50. We offered a low incentive to encourage participants to report their beliefs truthfully while discouraging cheating by searching for answers online.

Participants in the information treatment were then exposed to information about actual support for the carbon tax policy, alongside their inferred support (block 4a in Fig. 1). Subsequently, an incentivized question was used to elicit participants' posterior beliefs about support for the same carbon taxation policy. To avoid replicating the exact scenario used to elicit prior beliefs, we introduced a new context for the posterior belief task. Specifically, we informed participants that, prior to the experiment, we had surveyed 100 individuals from the US on Prolific regarding their support for various climate policies (which we had indeed conducted).[5] Participants were then asked to estimate how many of those surveyed individuals supported the policy: "Requiring fossil fuel companies to pay a carbon tax and using the money to reduce other taxes (such as income tax) by an equal amount" (block 5 in Fig. 1). Again, participants were informed that the 5 % who guessed closest to the correct number would be paid $0.50. This step of eliciting posterior beliefs was included to test whether participants updated their beliefs. Participants in the control group (block 4b in Fig. 1) received no information and proceeded directly to the elicitation of posterior beliefs about others' support for carbon taxation (block 5 in Fig. 1).

Next, we elicited participants' *private* climate action, meaning action that is invisible to others (block 6 in Fig. 1). Here, participants made an incentivized donation decision, deciding how to allocate $10 between themselves and the Citizen's Climate Lobby, a nonprofit, nonpartisan grassroots advocacy organization focused on climate change. The instructions provided additional information about the Citizens' Climate Lobby, highlighting its focus on lobbying for national policies to address the climate crisis, including the introduction of a carbon tax (see the Appendix A for the full instructions).

The subsequent part of the experiment was designed to elicit participants' *public* climate action—an action that can be observed by others (block 7 in Fig. 1). Participants were asked a total of four questions about public climate actions, structured in two steps: first, whether they would share their donation decision with others, and second, their willingness to discuss it with others. All measures were collected after the main experiment.

In the first step, each participant was asked for their permission and willingness to share information about their donation decision with another randomly matched participant affiliated with the same political party. Specifically, participants were asked: "On a scale from 1 to 7, how willing are you to share information about your donation decision with another participant of this experiment?" and "Do you agree that we share information about your donation decision with another participant of this experiment?" It was clearly stated that only the individual donation decisions would be shared by the experimenter, without revealing any personal data to other participants.

In the second step, participants were informed that a 5-minute live video meeting would be organized after the experiment. The meeting would consist of randomly matched pairs of participants affiliated with the same political party Its stated purpose was to discuss climate policies. At the beginning of the meeting, each participant would be informed about their counterpart's donation decision and political affiliation. Participants were asked to report the minimum compensation they would be willing to accept for attending such a meeting, with response options ranging from $0 to $50 in $0.10 increments. Finally, they were asked a binary-response question about their general willingness to attend the policy discussion meeting. The question read: "Are you willing to

[5] The pre-experimental survey was implemented in March 2024, before the experiment, and participants were paid $0.50 to complete the questionnaire.

Table 1
Summary statistics.

	Means (std. dev.)			Differences (*p*-values)
	(1) Full sample	(2) Control (C)	(3) Treatment (T)	(4) T - C
Prior belief	61.549	61.368	61.728	0.360
	(17.742)	(17.848)	(17.646)	(0.686)
Female	0.499	0.500	0.498	−0.002
	(0.500)	(0.500)	(0.500)	(0.940)
Age	43.439	43.052	43.820	0.768
	(13.724)	(13.946)	(13.500)	(0.265)
College degree	0.590	0.621	0.561	−0.060**
	(0.492)	(0.486)	(0.497)	(0.015)
High income	0.313	0.339	0.287	−0.052**
	(0.464)	(0.474)	(0.452)	(0.025)
Employed	0.773	0.775	0.771	−0.004
	(0.419)	(0.418)	(0.420)	(0.834)
Republican	0.498	0.496	0.499	0.003
	(0.500)	(0.500)	(0.500)	(0.900)
Parent	0.548	0.567	0.528	−0.039
	(0.498)	(0.496)	(0.499)	(0.118)
Observations	1587	788	799	1587

Note: Columns 1–3 show the means of individual characteristics in the full sample and in the control and treatment groups, with standard deviations in parentheses. Column 4 shows the differences in means between the treatment (T) and control (C) groups, with *p*-values in parentheses. Prior belief is the perceived proportion of registered voters in the US who support carbon taxation, ranging from 0 % to 100 %. Female, College degree, High income, Employed, Republican, and Parent are binary indicators. Employed includes those participants who reported being currently employed full- or part-time. College degree includes those who reported having a bachelor's degree or higher. High income includes those whose reported annual household income is $100,000 or more. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

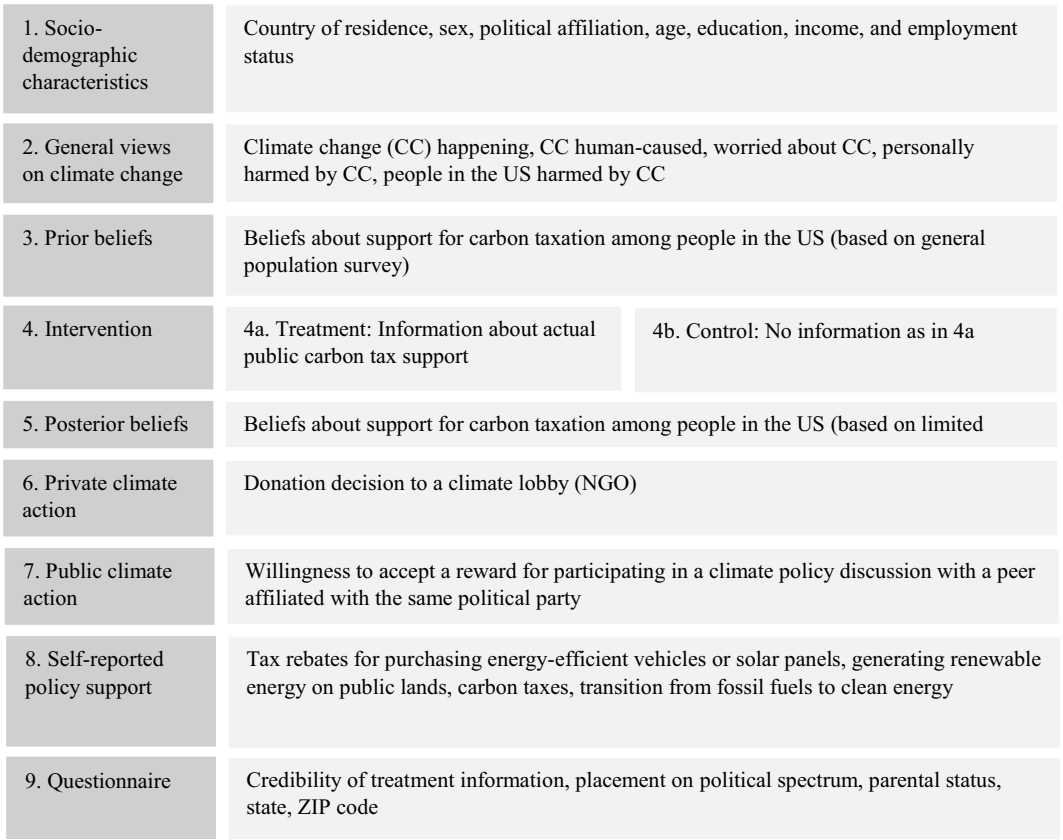

Fig. 1. Main parts of the experiment and their sequence.

participate in this 5-minute live video meeting with another participant of this experiment? If you agree and are selected, we will contact you on Prolific to arrange the meeting after the experiment has concluded."

Participants were informed in advance that only a subset of them would actually be matched and invited to participate in the meeting. Overall, 58 % of participants indicated that they were willing to participate in the video meeting. After the experiment, participants who had agreed to participate and indicated a willingness to do so for $5 or less were invited to the meeting. From this group, two participants affiliated with the same political party were randomly selected to attend. Although the selected pair confirmed their participation, neither participant ultimately joined the meeting. .[6]

Participants were also asked to report their support for various climate policies, including tax rebates for purchasing energy-efficient vehicles or solar panels, generating renewable energy on public lands, levying carbon taxes on fossil fuel companies, and transitioning the US economy from fossil fuels to clean energy (block 8 in Fig. 1). The wording of the climate policies was taken from the Climate Change in the American Mind survey (Leiserowitz et al., 2023).

The experiment concluded with a survey that included questions about participants' perception of the credibility of the provided information, which stated that 66 % of people in the US supported carbon taxes. Additional information was collected about the participants' placement on the political spectrum, parental status, and details about their state and ZIP code (block 9 in Figure 9).

We included two attention checks in the experiment. The first was administered at the beginning of the experiment, just before eliciting participants' views on climate change; the second was administered before the donation decision. Detailed experimental instructions can be found in the Appendix A.

2.3. Treatments

Treatment. Participants were informed that 66 % of registered US voters expressed support for the following climate policy: "Requiring fossil fuel companies to pay a carbon tax and using the money to reduce other taxes (such as income tax) by an equal amount" (block 4a in Fig. 1). Simultaneously, their guessed level of support for this tax, obtained earlier, was displayed. The presentation distinguished between the actual policy support, displayed in green text, and the participant's guess, shown in red (Fig. 2). To ensure that participants believed the actual level of support, they were told that they could request a copy of the survey results, which would be sent to them after the experiment. To prevent participants from rushing through the treatment page, the display of the "next" button in the information condition was delayed by 7 s.

Control. Participants were not provided with any information regarding the actual support for the climate policy in this condition, which served as the control in the experiment (block 4b in Fig. 1). We opted for a passive (pure) control, where participants receive no information, rather than an active control, where they would receive different information. A passive control was more suitable for this experimental design because it allowed us to investigate how providing information affects climate action compared to when no information is given. Moreover, as outlined by Haaland et al. (2023), having a pure control condition makes it easier to interpret the relationship between prior beliefs and outcome variables, as posterior beliefs in the control condition are not influenced by any new information. However, we acknowledge that receiving any information—regardless of its content—may influence participants' attention, engagement, or emotional responses (Haaland et al., 2023). Consequently, using a pure control group involves certain trade-offs, as it does not account for these non-informational effects that may arise simply from being exposed to a treatment.

3. Results

3.1. Misperceptions and belief updating

First, we determined whether participants in the experiment misperceived public support for carbon taxation. Fig. 3 displays the distribution of prior beliefs, the mean prior belief, and the actual share of people in the US who support carbon taxation. There is notable heterogeneity in individual prior beliefs about support for carbon taxation. On average, participants believed that 61 % of people in the US support carbon taxation, a statistically significant difference from the actual share of 66 % (two-sided t-test, $N = 1587$, $p < 0.001$). Notably, 56 % of participants underestimated others' support for carbon taxation. There was no significant difference in mean prior beliefs between the experimental conditions (two-sided t-test, $N = 1587$, $p = 0.686$). Similarly, a Wilcoxon rank-sum test indicated that the distributions of prior beliefs did not significantly differ between the treatment and control groups ($N = 1587$, $p = 0.432$), confirming baseline balance across conditions.[7]

After confirming that participants, on average, misperceived support for carbon taxation, we examined whether providing information about actual support for carbon taxation influenced their beliefs about people's backing of carbon taxation in the anticipated direction. This step helps establish that any changes in the main outcome variables are driven by the beliefs altered through information provision, thereby strengthening the internal validity of the experiment.

On average, participants' posterior beliefs about support for carbon taxes were 66.7 % in the treatment condition and 65.3 % in the control condition. Although the difference is small, it is statistically significant (two-sided t-test, $N = 1587$, $p = 0.048$). Fig. 4, which

[6] This procedure for selecting participants for the video meeting, as well as the limited number of meetings ultimately initiated, was disclosed to the ethics committee during the approval process.

[7] The non-parametric tests reported in the paper were not part of the preregistered analysis plan and were conducted in response to a reviewer's request.

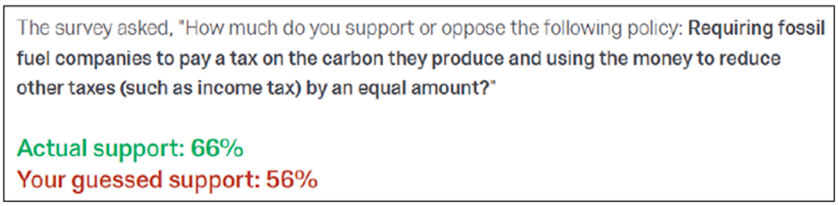

Fig. 2. Example of treatment screen. *Note:* Each participant in the treatment condition was shown the actual level of support for carbon taxation (green text) and their guessed support for the policy elicited in a previous question (red text). The percentage in red is for illustrative purposes only, as the participants' guesses varied. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.).

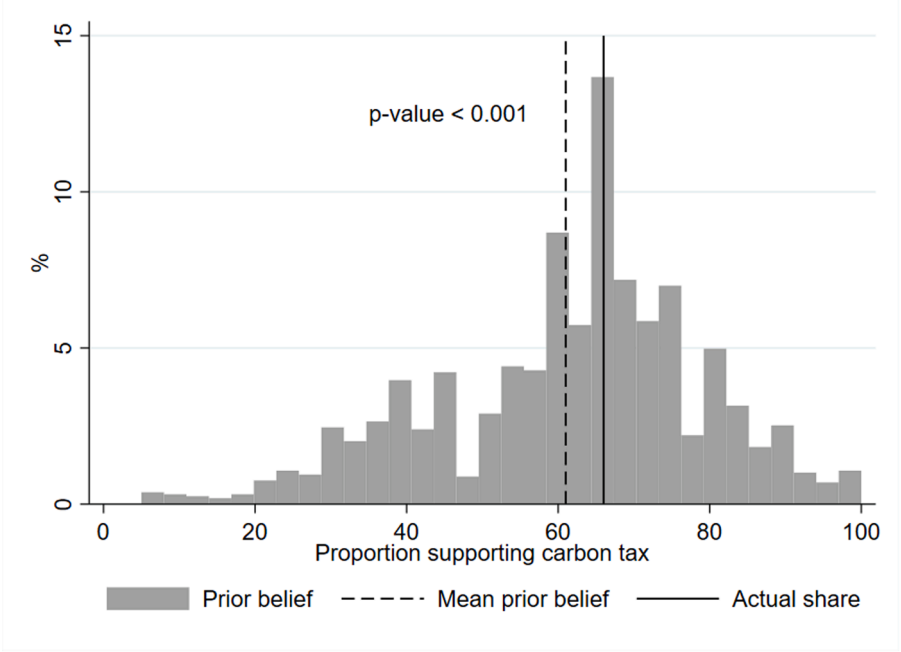

Fig. 3. Distribution of prior beliefs. *Note:* The gray columns display the distribution of participants' prior beliefs about others' support for carbon taxation. The vertical black dashed line represents the mean prior belief; the vertical solid black line represents the actual share of people in the US who support carbon taxation.

displays the distributions of prior and posterior beliefs by experimental condition, reveals a clear spike in posterior beliefs around the signal (i.e., actual public support for carbon taxation) among participants in the treatment group. However, the overall distribution of posterior beliefs does not significantly differ between participants who received the information and those who did not (Wilcoxon rank-sum test, $N = 1587$, $p = 0.845$).

The mixed effects of the treatment on posterior beliefs warrant a more disaggregated analysis, as the treatment may have affected participants differently depending on whether they initially underestimated or overestimated public support. We conducted a regression analysis with posterior beliefs as the dependent variable and the information treatment as the key independent variable. The results are presented in Table 2. Column 1 reports estimates from the full sample without controls. Column 2 adds controls for socio-demographic characteristics. Columns 3 and 4 show results for the subsets of participants who initially overestimated and underestimated public support for carbon taxation, respectively. The complete set of results can be found in Table B1 in the Appendix.

We found that the information treatment increased the average posterior belief by approximately 1.4–1.5 percentage points (columns 1–2 of Table 2). Importantly, the information treatment reduced the average posterior belief by 10.3 percentage points among participants who had overestimated policy support (column 3) and increased it by 9 percentage points among those who had underestimated the support (column 4). These results confirm the effectiveness of our informational intervention.

To provide a more structural measure of belief updating, we estimated a learning-rate specification following Haaland et al. (2023). In this framework, updating was modeled as a function of the initial perception gap, yielding a direct estimate of the learning rate and allowing us to assess whether the treatment increased responsiveness to the informational signal.

Specifically, we regressed updating, defined as the difference between participants' posterior and prior beliefs, on the treatment variable, the perception gap, and the interaction between the perception gap and the treatment indicator. The perception gap was

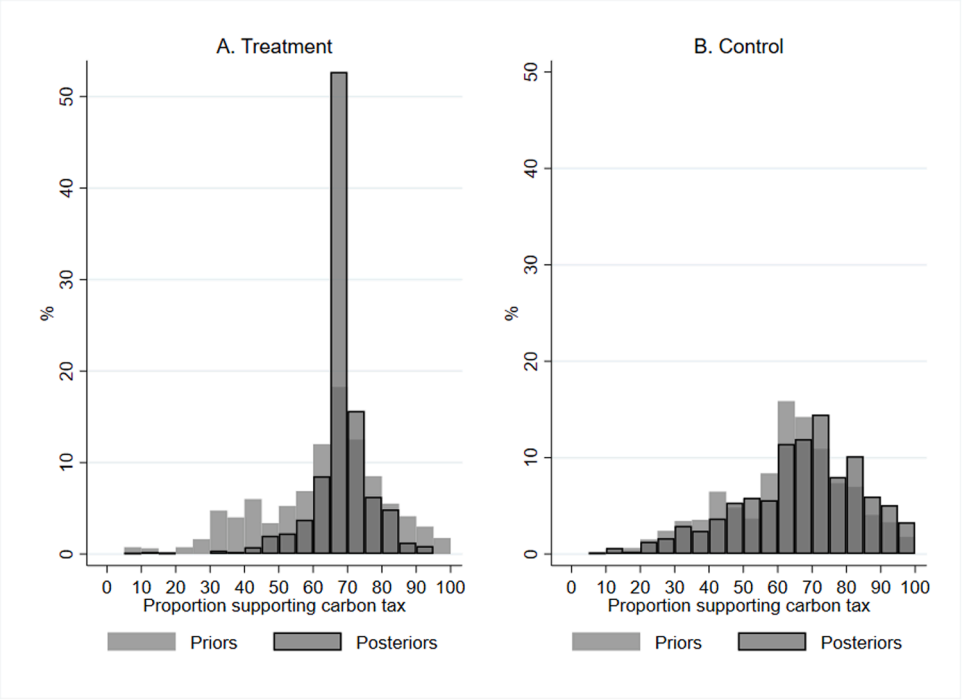

Fig. 4. Distribution of prior and posterior beliefs. *Note:* The light gray and dark gray bars represent the distributions of participants' prior and posterior beliefs, respectively, about others' support for carbon taxation, shown separately by experimental condition.

Table 2
Treatment effect on posterior beliefs.

Posterior belief	(1) Full sample	(2) Full sample	(3) Priors > Actual	(4) Priors < Actual
Treatment	1.403**	1.483**	−10.298***	9.019***
	(0.713)	(0.719)	(0.840)	(0.856)
Constant	65.256***	63.222***	86.683***	55.179***
	(0.643)	(4.704)	(7.534)	(3.301)
Observations	1587	1587	624	890
R-squared	0.002	0.040	0.230	0.151
Controls:				
Socio-demographics	No	Yes	Yes	Yes

Note: The table reports ordinary least squares (OLS) estimates with robust standard errors in parentheses. The dependent variable, posterior belief, represents the post-treatment belief about the share of individuals in the US expressing support for carbon taxation in a pre-experimental Prolific survey. Columns 1 and 2 show results where the dependent variable was regressed on the treatment variable using the full sample. Columns 3 and 4 display results for the subsamples of individuals who overestimated and underestimated the support for carbon taxation, respectively. The regressions presented in columns 2–4 include socio-demographic controls: sex (binary), age (continuous), income (categorical), education (categorical), employment (categorical), political party affiliation (binary), political spectrum (categorical), and parenthood (binary). ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

defined as the difference between the true signal and a participant's prior belief. In this specification, the coefficient on the perception gap captured the baseline learning rate in the control group and the interaction term indicated the additional responsiveness induced by the treatment. The results are presented in Table 3. Column 1 shows estimates without any controls and column 2 includes socio-demographic characteristics.

We found strong evidence of belief updating toward the informational signal. In the control group, respondents partially adjusted their beliefs: a one-percentage-point larger perception gap was associated with a 0.23-percentage-point revision in the direction of the true value, implying a baseline learning rate of 0.23. The adjustment was substantially stronger in the treatment group. The interaction coefficient equaled 0.64, meaning that treated respondents adjusted an additional 0.64 percentage points for each one-percentage-point larger perception gap. The total learning rate in the treatment group was therefore given by the sum of the baseline slope and the interaction term ($0.23 + 0.64 = 0.87$). These findings implied that treated respondents closed approximately 87 % of their initial perception gap in response to the informational signal. Taken together, these results showed that while some baseline updating occurred even in the absence of explicit information, the treatment induced substantially stronger convergence toward the true level of

Table 3
Belief updating.

Updating	(1)	(2)
Treatment	−1.611***	−1.598***
	(0.484)	(0.496)
Perception gap	0.225***	0.233***
	(0.0303)	(0.0302)
T x Perception gap	0.640***	0.641***
	(0.0387)	(0.0383)
Constant	2.848***	0.967
	(0.396)	(4.263)
Observations	1587	1587
R-squared	0.551	0.562
Controls:		
Socio-demographics	No	Yes

Note: The table reports ordinary least squares (OLS) estimates with robust standard errors in parentheses. The dependent variable, updating, represents the difference between participants' posterior and prior beliefs about the share of individuals in the US expressing support for carbon taxation. It was regressed on the treatment variable, the perception gap, and the interaction of the variable perception gap with the treatment variable. The perception gap was defined as the difference between the true signal and a participant's prior belief about carbon tax support. Column 1 shows the regression results without any controls. Columns 2 presents the regression outcomes with socio-demographic controls: sex (binary), age (continuous), income (categorical), education (categorical), employment (categorical), political party affiliation (binary), political spectrum (categorical), and parenthood (binary). ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

public support.

3.3. Private climate action

3.3.1. Main results

First, we investigated whether the information treatment had an effect on individuals' private climate action. We measured this through an incentivized donation decision, where participants allocated $10 between themselves and the Citizens' Climate Lobby. Fig. 5 presents the distributions of donation amounts across experimental conditions, along with the corresponding mean values for each group. The histograms show a notable concentration at zero: nearly 38 % of participants in the control group and 41 % in the treatment group chose not to donate anything to the climate lobby. The mean donation amount in the treatment group was $1.97, which was not significantly different from the $2.19 in the control group (two-sided *t*-test, $N = 1587$, $p = 0.107$). A non-parametric test confirmed this result, indicating no significant treatment effect on donation amounts (Wilcoxon rank-sum test, $N = 1587$, $p = 0.845$).

Further, we regressed the variable donation on the treatment indicator, adding controls for individual participant characteristics in subsequent model specifications. The results are presented in Table 4. Column 1 shows estimates without any controls, mirroring the results from the *t*-test discussed above. Column 2 includes socio-demographic characteristics. Column 3 extends the analysis by incorporating five variables on participants' climate views. These were based on participants' responses to five statements regarding climate change rated on a 7-point Likert scale: "Climate change is happening," "Climate change is mostly human-caused," "I am concerned about climate change," "Climate change will personally affect me," and "Climate change will affect many people in the US." The complete table with all controls is provided in Table B2 in the Appendix.

Contrary to our expectations, we found no significant effect of the information treatment on the size of donations, even after controlling for individual characteristics. Although the estimated coefficients for participants who received information about others' support for carbon taxation are negative, potentially indicating a slight reduction in donations to the climate organization, this effect is not statistically significant at conventional levels (e.g., $p < 0.05$).

Due to the censored nature of the donation data (participants' donations were restricted to $0–10), we also estimated Tobit regressions. The results, reported in Table B3 in the Appendix, are largely consistent with the OLS regression results. The coefficient of the treatment variable remained statistically insignificant at conventional levels when controlling for socio-demographic characteristics and climate change views.

Furthermore, given the substantial share of participants who donated nothing, we conducted a two-part (hurdle) model as a robustness check. The first part employed a logistic regression to estimate the likelihood of making a positive donation. As shown in Table B4 in the Appendix, the treatment had no statistically significant effect on the probability of donating. The second part estimated a linear regression of donation amounts on treatment and controls, restricted to participants who made a positive donation. These results also revealed no significant treatment effect on the amount donated among donors.

Additionally, we tested whether the treatment effects differed when using another commonly explored measure of climate action: self-reported support for climate policies, as opposed to an incentivized measure such as donation decisions. We found no evidence that the treatment affected self-reported policy support (see Appendix C for details).

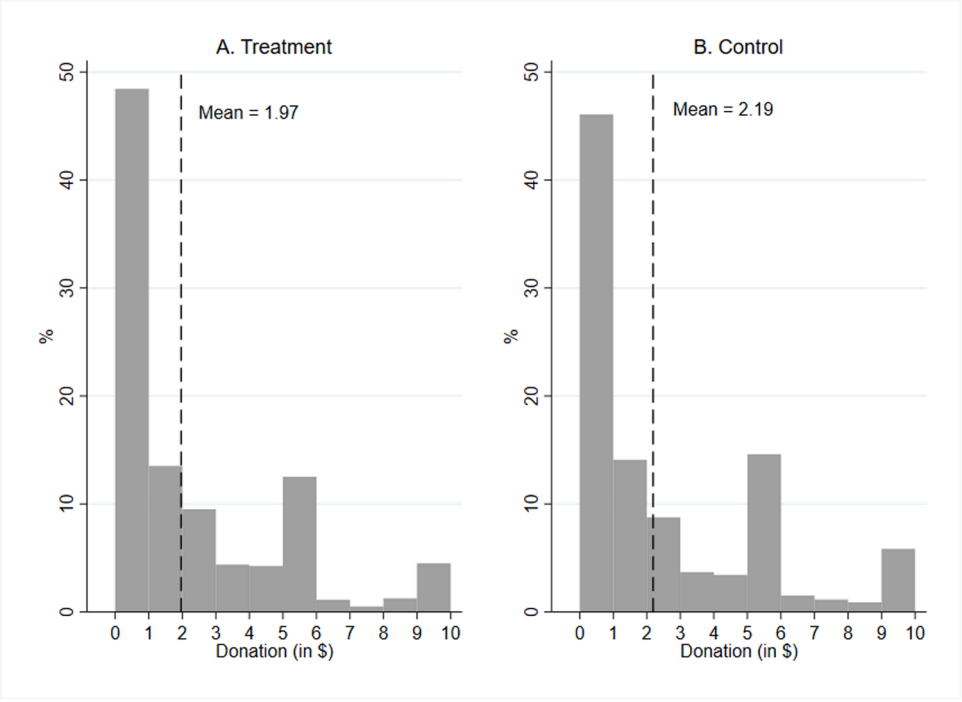

Fig. 5. Distribution of donations. *Note:* The figure displays the distribution of donations to the climate change organization by experimental condition. The vertical dashed lines indicate the mean donation for each group.

Table 4
Treatment effect on donation.

Donation	(1)	(2)	(3)
Treatment	−0.221	−0.202	−0.225*
	(0.137)	(0.135)	(0.132)
Constant	2.187***	−1.091**	−3.410***
	(0.100)	(0.549)	(0.720)
Observations	1587	1587	1587
R-squared	0.002	0.072	0.134
Controls:			
Socio-demographics	No	Yes	Yes
Climate change views	No	No	Yes

Note: The table reports OLS estimates with robust standard errors in parentheses. The dependent variable, donation, indicates the decision to donate to the climate change organization. Column 1 shows results where the dependent variable is regressed on the treatment variable without any controls. Columns 2 and 3 present regression outcomes with controls. Column 2 includes socio-demographic controls: sex (binary), age (continuous), income (categorical), education (categorical), employment (categorical), political party affiliation (binary), political spectrum (categorical), and parenthood (binary). Column 3 incorporates socio-demographic and climate change views controls. The climate change views variables are based on participants' agreement with five statements on climate change measured on a 7-point Likert scale: "Climate change is happening," "Climate change is mostly human-caused," "I am worried about climate change," "Climate change will harm me personally," and "Climate change will harm many people in the US." ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

3.3.2. *Treatment effect heterogeneity*

As shown in Table 2, participants who underestimated and overestimated others' support for carbon taxation adjusted their beliefs differently after receiving information about the actual support for carbon taxation. Therefore, we examined whether the treatment effect on private climate action also varied based on prior beliefs. To investigate this, we regressed the donation decision on the treatment variable for subsets of individuals who either overestimated or underestimated support for carbon taxation. The results from the regression analysis are presented in Table 5. Columns 1–2 display the outcomes for individuals whose prior beliefs about policy support were below the actual support, and columns 3–4 focus on those whose prior beliefs were above or equal to the actual support. Columns 1 and 3 report the results from the regression without any controls, while columns 2 and 4 include controls for socio-demographic characteristics and climate views.

No statistically significant treatment effect on donation was found for participants who underestimated the support for carbon

Table 5
Treatment effect on donation by prior beliefs.

Donation	(1) Prior < Actual	(2) Prior < Actual	(3) Prior ≥ Actual	(4) Prior ≥ Actual
Treatment	0.037	0.031	−0.604***	−0.560**
	(0.172)	(0.166)	(0.221)	(0.217)
Constant	1.825***	−2.740***	2.693***	−3.896***
	(0.120)	(0.894)	(0.169)	(1.256)
Observations	890	890	697	697
R-squared	0.000	0.139	0.011	0.154
Controls:				
Socio-demographics	No	Yes	No	Yes
Climate change views	No	Yes	No	Yes

Note: The table reports OLS estimates with robust standard errors in parentheses. The dependent variable, donation, indicates the decision to donate to the climate change organization. Columns 1–2 display the results for the subsample of individuals who underestimated the support for carbon taxation. Columns 3–4 show the results for the subsample of individuals who overestimated or guessed the support for carbon taxation correctly. Columns 1 and 3 show the results where the dependent variable was regressed on the treatment variable without any controls. Columns 2 and 4 present the regression outcomes controlling for socio-demographic characteristics and climate change views. The socio-demographic controls include sex (binary), age (continuous), income (categorical), education (categorical), employment (categorical), political party affiliation (binary), political spectrum (categorical), and parenthood (binary). The climate change views variables are based on participants' agreement with five statements on climate change measured on a 7-point Likert scale: "Climate change is happening," "Climate change is mostly human-caused," "I am worried about climate change," "Climate change will harm me personally," and "Climate change will harm many people in the US." ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

taxation (columns 1–2 of Table 5). For individuals who overestimated or correctly guessed the support for carbon taxation, providing information about the actual support for the policy decreased the average donation by around $0.56–0.60 (columns 3–4 of Table 5). This finding of an unintended consequence is consistent with a boomerang effect.

We also analyzed whether the treatment effect varied based on the perception gap observed before the treatment between those who underestimated the support for carbon taxation and those who overestimated or correctly estimated it. The perception gap was defined as the absolute difference between participants' prior beliefs about public support for carbon taxation and the actual support.

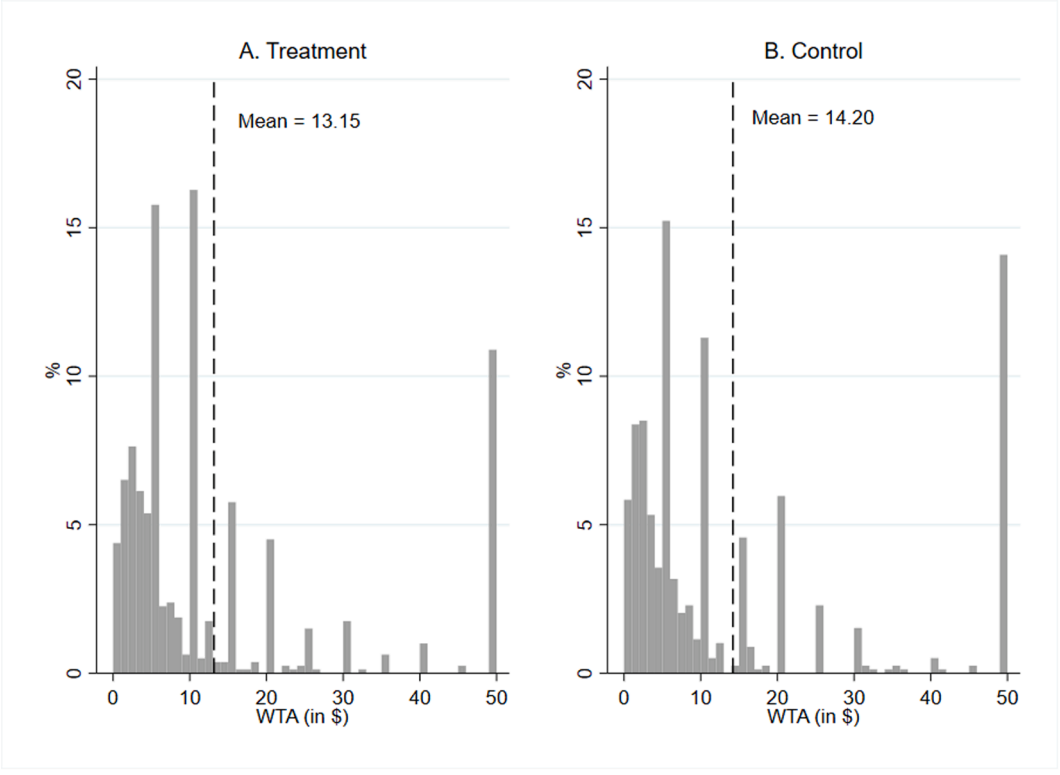

Fig. 6. Distribution of WTA. *Note:* The figure displays the distribution of participants' willingness to accept a reward for taking part in a policy discussion by experimental condition. The vertical dashed lines indicate the mean WTA for each group.

We regressed the donation decision on the treatment variable, the perception gap, and the interaction term of the perception gap and the treatment. We also controlled for individual socio-demographic characteristics and climate change views. The results, provided in Table B5 in the Appendix, show that in both subsamples, the coefficient for the interaction term is statistically insignificant. Thus, we found no evidence that the treatment effect varied according to the size of the perception gap.

Next, we investigated whether participants affiliated with the Democratic party responded differently to the informational treatment than participants affiliated with the Republican party, as climate change and climate policy have become increasingly polarized in the US in recent decades (Dunlap et al., 2016; Pew Research Center, 2020). To test this, we regressed the donation decision on the treatment variable, a dummy variable indicating that the participant is a Republican, and the interaction of the Republican variable with the treatment variable. The results presented in Table B6 in the Appendix show that there is no statistically significant difference in the treatment effect between Democrats and Republicans.

3.4. Public climate action

3.4.1. Main results

Next, we examined public climate action. We measured public climate action by asking participants about the lowest reward they would accept for joining a video meeting to discuss climate policies. Based on participant responses, we created the variable willingness to accept (WTA). Fig. 6 displays the distributions of the reported WTA by experimental condition, along with the mean value for each group.

The WTA measure captures the compensation required for participation among all respondents, including those who subsequently indicated that they would not participate in the video meeting. The WTA question preceded the binary question about willingness to participate, thereby capturing participants' stated compensation requirements independently of their subsequent participation decision. However, among participants who, in the follow-up question, expressed willingness to participate in the video meeting ($N = 921$), the average WTA was \$9.43; among those who indicated that they were not willing to attend ($N = 666$), the average WTA was \$19.54. Accordingly, the distribution shown in Fig. 6 reflects not only heterogeneity in required compensation but also broader reluctance to engage in a public discussion. The substantial mass at higher WTA values, particularly responses at the upper bound of \$50, thus indicates a high perceived cost of participation within the offered compensation range.

The mean WTA was \$13.15 in the treatment group and \$14.20 in the control group, a difference that was not statistically significant (two-sided t-test, $N = 1587$, $p = 0.181$). A non-parametric test yielded a similar result, showing no significant treatment effect on WTA (Wilcoxon rank-sum test, $N = 1587$, $p = 0.894$).

Next, we regressed WTA on the treatment variable, adding controls for individual participant characteristics in subsequent model specifications. Column 1 of Table 6 presents the results from the regression without any controls, mirroring the t-test results described above. Column 2 shows the results from a regression that included socio-demographic characteristics; column 3 shows the results from a regression that, in addition, controlled for climate change views. Participants were informed that their donation decisions would be shared with matched participants during the discussion. Thus, there might be a negative relationship between donation decisions and WTA. For example, participants who donate more may be more willing to participate in a policy meeting to benefit from being seen as generous; thus, they may have a low WTA. Conversely, individuals who donate nothing may feel bad and request a higher reward for attending the meeting to compensate for the displeasure of being viewed as selfish by others. To address this potential endogeneity, columns 4–5 include participants' donations as a control variable. The complete table with all controls is provided in Table B7 in the

Table 6
Treatment effect on willingness to accept.

WTA	(1)	(2)	(3)	(4)	(5)
Treatment	−1.051	−1.465*	−1.450*	−1.573**	−1.556**
	(0.786)	(0.781)	(0.782)	(0.781)	(0.783)
Constant	14.199***	19.802***	23.744***	19.218**	22.126***
	(0.582)	(7.584)	(7.965)	(7.555)	(7.951)
Observations	1587	1587	1587	1587	1587
R-squared	0.001	0.041	0.047	0.049	0.053
Controls:					
Donation	No	No	No	Yes	Yes
Socio-demographics	No	Yes	Yes	Yes	Yes
Climate change views	No	No	Yes	No	Yes

Note: The table reports OLS estimates with robust standard errors in parentheses. The dependent variable, WTA, represents the reservation price for attending a video meeting with another participant from the experiment affiliated with the same political party. Column 1 shows the regression results without any controls. Columns 2–5 present the regression outcomes with controls. Column 2 includes socio-demographic controls: sex (binary), age (continuous), income (categorical), education (categorical), employment (categorical), political party affiliation (binary), political spectrum (categorical), and parenthood (binary). Column 3 incorporates socio-demographic and climate change views controls. The climate change views variables are based on participants' agreement with five statements on climate change measured on a 7-point Likert scale: "Climate change is happening," "Climate change is mostly human-caused," "I am worried about climate change," "Climate change will harm me personally," and "Climate change will harm many people in the US." Column 4 controls for the socio-demographic characteristics as well as the decision to donate to a climate change organization, elicited in a previous part of the experiment. Column 5 controls for socio-demographics, the donation decision, and climate change views. ***$p < 0.01$; **$p < 0.05$; *$p < 0.1$.

Appendix.

As expected, the regression results indicate that providing information about others' support for carbon taxation has a negative effect on participants' willingness to accept a reward for participating in a policy discussion, meaning it increases public climate action. However, the treatment effect is only marginally statistically significant at the 10 % significance level (columns 2–3 of Table 6). Controlling for the donation decision strengthens the evidence of a positive treatment effect on public climate action, as the effect becomes significant at the 5 % significance level (columns 4–5 of Table 6).

The variable WTA is based on censored data, as individuals could choose rewards from $0 to $50. Thus, we additionally estimated Tobit regressions on WTA using the same specifications as those presented in Table 6. The results from the Tobit regressions, reported in Table B8 in the Appendix, largely align with the results of the OLS regressions presented in Table 6.

3.4.2. Treatment effect heterogeneity

Next, we analyzed whether the treatment effect on the public climate action differed between participants who underestimated and those who overestimated the support for carbon taxation. We regressed WTA on the treatment variable, a dummy variable indicating that the participant underestimated public support for carbon taxation (underestimator), and the interaction term between the treatment and the underestimator variable. We also controlled for individual socio-demographic characteristics, climate change views, and donation decisions. We found no evidence that the average treatment effects differed between the two groups in any regression specifications (see Table B9 in the Appendix for the results).

Next, we examined whether the information treatment had a different effect on the reservation price for attending a policy discussion for Democrats and Republicans. We regressed WTA on the treatment variable, a dummy variable indicating that the participant was a Republican, and the interaction term between the Republican variable and the treatment variable. The results are presented in Table 7. Column 1 presents the results from the regression without any controls, while columns 2–5 report the results from additional specifications with different sets of controls, including donation decisions, socio-demographic characteristics, and views on climate change.

We found that in the control group, participants affiliated with the Republican Party, on average, required higher rewards for participating in a climate policy discussion than those affiliated with the Democratic Party. This indicates that Republicans were less willing to engage in public climate action than Democrats. Providing information on the actual share of people in the US supporting carbon taxation had a positive, albeit statistically insignificant, effect on Democrats' WTA, but it significantly reduced Republicans' WTA. Depending on the model specification, the difference in treatment effects between Democrats and Republicans was $5.8–6.2 (see the coefficients of the interaction terms in Table 7).

Did the treatment lead to increased willingness to participate in a pro- or anti-climate policy discussion among Republican participants? To answer this question, we tested whether the treatment led to different responses in Republicans' WTA between supporters and opposers of carbon taxation. We regressed the outcome variable WTA on the treatment variable, a dummy variable indicating that the participant supported carbon taxation, and their interaction term. The regression results (Table B10 in the Appendix) show that in the control condition, Republicans who supported carbon taxation had, on average, a lower WTA than those who did not support carbon taxation. However, the treatment did not have a different effect on the supporters and opponents of carbon taxation, as the interaction term was statistically insignificant. This suggests that the increased willingness to engage in public discussion among Republicans was broad-based. We treated this analysis as exploratory since it was not preregistered and the question associated with the policy support variable was asked at the end of the experiment, which may have been affected by the treatment.

4. Discussion and conclusion

In this study, we gathered experimental evidence from the US to examine how correcting misperceptions about others' support for carbon taxation affects individuals' climate actions, both private and public.

First, we showed that more than half of individuals (56 %) underestimated public support for carbon taxation. The degree of underestimation in our study was considerably lower than that observed in a representative survey by Sparkman et al. (2022). Several factors may explain this difference. Individuals who participated in our experiment were more educated than the general US population, with 60 % of participants over the age of 25 having at least a bachelor's degree, compared to <40 % of the US population (U.S. Census Bureau, 2022). More educated individuals may be better informed about the actual policy support among the public, which may result in lower misperception. Additionally, individuals in our sample were more supportive of carbon taxation compared to a nationally representative sample (Leiserowitz et al., 2023). The literature shows that when asked to estimate public support for a policy, policy supporters tend to give higher estimates than non-supporters, a phenomenon known as egocentric bias (Mildenberger and Tingley, 2019; Sparkman et al., 2022). Another potential reason for the smaller perception gap in our sample is the fact that, unlike in Sparkman et al. (2022), belief elicitation in our study was incentivized, which may have led to more accurate estimates of policy support (Charness et al., 2021).

Second, we found that providing information on actual policy support successfully reduced misperceptions. Individuals who underestimated support revised their beliefs upward, while those who overestimated support revised them downward after receiving the informational intervention. These findings on the effectiveness of informational interventions in updating beliefs align with those observed in the behavioral literature (e.g., Mildenberger and Tingley, 2019; Pompeo and Serdarevic, 2021; Dillon and Lochman, 2022; Andre et al., 2024b).

Third, we found that, on average, the informational intervention had no statistically significant effect on private climate action, as measured by donations to an organization supporting climate policy. However, the intervention had a negative effect on the private

Table 7
Treatment effect on willingness to accept by party affiliation.

WTA	(1)	(2)	(3)	(4)	(5)
Treatment	1.818*	1.597	1.612	1.488	1.510
	(1.065)	(1.065)	(1.067)	(1.063)	(1.064)
Republican	3.438***	4.225**	3.354*	3.867**	3.229
	(1.159)	(1.939)	(1.972)	(1.951)	(1.979)
T x Republican	−5.767***	−6.151***	−6.149***	−6.150***	−6.158***
	(1.567)	(1.561)	(1.559)	(1.555)	(1.555)
Constant	12.49***	19.07***	22.98***	18.49**	21.36***
	(0.730)	(7.356)	(7.747)	(7.326)	(7.732)
Observations	1587	1587	1587	1587	1587
R-squared	0.010	0.050	0.057	0.058	0.063
Controls:					
Donation	No	No	No	Yes	Yes
Socio-demographics	No	Yes	Yes	Yes	Yes
Climate change views	No	No	Yes	No	Yes

Note: The table reports OLS estimates with robust standard errors in parentheses. The dependent variable, WTA, represents the reservation price for attending a video meeting with another participant from the experiment affiliated with the same political party. It was regressed on the treatment variable, the binary variable Republican, and the interaction of the variable Republican with the treatment variable. Column 1 shows the regression results without any controls. Columns 2–5 present the regression outcomes with controls. Column 2 includes socio-demographic controls: sex (binary), age (continuous), income (categorical), education (categorical), employment (categorical), political party affiliation (binary), political spectrum (categorical), and parenthood (binary). Column 3 incorporates socio-demographic and climate change views controls. The climate change views variables are based on participants' agreement with five statements on climate change measured on a 7-point Likert scale: "Climate change is happening," "Climate change is mostly human-caused," "I am worried about climate change," "Climate change will harm me personally," and "Climate change will harm many people in the US." Column 4 controls for the socio-demographic characteristics as well as the decision to donate to a climate change organization, elicited in a previous part of the experiment. Column 5 controls for socio-demographics, the donation decision, and climate change views. $^{***}p < 0.01$; $^{**}p < 0.05$; $^{*}p < 0.1$.

climate action of individuals who initially overestimated public support for carbon taxation; on average, these participants reduced their donation amounts after receiving the information. This suggests that correcting misperceptions can have a boomerang effect. Boomerang effects—situations where exposure to norm-based interventions leads to outcomes opposite to those intended—have been documented in previous studies on environment-friendly behaviors (e.g., Schultz et al., 2007). There may be several reasons why some overestimators reduced their private climate action. First, it may be psychologically easier to conform to a lower norm as it results in immediate financial gains. Second, they may wish to avoid being "suckers" by contributing more to the public good of fighting climate change than others do.

Our findings differ from those of Andre et al. (2024b), who found a positive average effect of belief-correcting information about Americans fighting climate change on donations, driven almost entirely by underestimators, and no evidence of a boomerang effect. A key distinction likely lies in the context of the informational intervention. Whereas Andre et al. (2024b) examine beliefs about climate engagement in a relatively abstract framing (i.e., fighting climate change in general), we focus on a specific climate policy instrument—carbon taxation. This policy, framed explicitly as a tax on the carbon emissions of fossil fuel companies, is indirectly associated with personal financial costs. In such a context, norm updates may activate cost salience in addition to general informational learning, which may alter how individuals respond to the intervention in a donation task. Consistent with this interpretation, evidence from the tax compliance literature shows that information about low compliance norms can increase tax evasion, while information about high compliance norms may have little effect on actual compliance (Lefebvre et al., 2015).

Importantly, our results on private climate action align closely with those of Drews et al. (2022), who also studied carbon taxation and reported limited average effects of information alongside substantial heterogeneity. Specifically, they found little evidence that providing information about public acceptance of carbon taxes increased individuals' self-reported support for the policy but identified heterogeneous treatment effects depending on the extent and direction of individuals' initial misperceptions relative to their own policy stance, operationalized through the false consensus effect. Downward policy support revisions were particularly common, especially among initial policy supporters. While our study differs in several respects—we examined incentivized donation behavior rather than self-reported policy acceptance, drew on a US sample rather than a Spanish one, and classified heterogeneity based on the overall direction of misperception (overestimation versus underestimation of public support) rather than projection from one's own attitude—the central pattern is similar. In both studies, null average effects mask meaningful heterogeneity tied to the structure of prior beliefs, and corrective information can in some cases produce unintended, even counterproductive, responses. Taken together, the evidence from Spain and the US indicates that such heterogeneous responses are not confined to a single institutional or cultural setting.

Finally, we found that providing information on actual policy support had a marginal positive effect on public climate action, specifically, it reduced the minimum reward required for participating in climate policy discussions with peers. Informing participants that most people support carbon taxation may have alleviated fears of being disliked or losing respect when discussing climate policies. Interestingly, the informational intervention significantly increased the willingness to discuss climate policies among Republicans. This increase was broad-based, observed among both Republicans who supported carbon taxation and those who opposed it. However, it is unclear whether the same mechanisms were at play for supporters and opponents of climate policies.

The overall marginally positive effect on willingness to engage in climate discussions is consistent with the findings of Geiger and Swim (2016), who, in a small-scale experiment ($N = 194$), showed that correcting pluralistic ignorance about others' concern for climate change increased concerned individuals' willingness to talk about it. In contrast, our results differ from those of Andre et al. (2024b), who found no significant effect of belief-correcting information on non-incentivized willingness to participate in political climate activism. One possible reason for this difference is that the two studies measure different kinds of climate engagement, and our study relies on an incentivized behavioral measure, whereas theirs is based on self-reported willingness to act in a hypothetical scenario.[8]

Our measure should not be interpreted as a comprehensive indicator of public climate action. Instead, it captures a narrower but still meaningful aspect: participants' openness to engage in climate policy discussions, which does not necessarily reflect support for climate policies. For instance, some participants may have wanted to join the conversation to express opposition or persuade others against climate action. Because our experimental design did not capture the direction or content of the intended discussions (e.g., pro- or anti-policy), we cannot determine how this willingness translates into actual advocacy. This opens important avenues for future research, including field studies that examine how interest in climate policy discussion relates to concrete forms of climate activism.

Future research could also explore strategies to limit the emergence of boomerang effects when correcting misperceptions. For example, it could investigate whether, in addition to misperception-correcting information, providing information that encourages collaboration toward a common goal or that indicates that norms are shifting in favor of pro-climate behavior (dynamic norms) can prevent boomerang effects (Sparkman et al., 2021). Another avenue for future research is to explore how individuals' climate behaviors are influenced when they receive misperception-correcting information about narrower groups with which they identify, or when such information is provided by in-group leaders. For instance, what would be the effect of information on Republicans' climate action if they were informed of actual policy support among Republicans, or if the message was delivered by an authoritative Republican leader?

Representative samples are not essential for deriving generalizable estimates of effect sizes within countries (Weinberg et al., 2014; Mullinix et al., 2015; Coppock et al., 2018). Nevertheless, larger representative samples of the US population could provide additional insights into groups that are underrepresented in our sample, such as individuals with lower levels of education or different political views. While we focused on Republicans and Democrats due to their sharply polarized views on climate change and policy (Dunlap et al., 2016; Sparkman et al., 2022; Leiserowitz et al., 2023), including Independents in future research could offer valuable insights into more moderate or undecided segments of the population. We leave these aspects for future research.

The mixed evidence on the impact of information interventions in influencing climate behavior makes it challenging to formulate clear policy recommendations for increasing climate awareness and action. On one hand, the negative effect on private climate behavior among individuals who overestimated public support highlights the need for caution when implementing misperception-correcting interventions. Our findings suggest that such efforts may backfire and reduce private pro-climate behavior. On the other hand, norm-based information can promote willingness to engage in discussions about climate change—particularly among groups often perceived as skeptical of climate policies, such as Republicans. Encouraging open policy dialogue may represent a meaningful step toward reducing political polarization and building broader support for climate action. Therefore, in contexts where misperceptions are not particularly large and widespread (as in our case), policymakers might consider more targeted interventions that foster discussion while minimizing potential disengagement among overestimators. In situations where misperceptions are more widespread, norm-based interventions may be applied more broadly, as the communication of accurate social norms is likely to resonate with a wider audience and carry lower risks of unintended consequences.

Declaration of competing interest

We declare no conflict of interest.

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Supplementary materials

Supplementary material associated with this article can be found, in the online version, at doi:10.1016/j.jebo.2026.107601.

[8] In Andre et al. (2024b), willingness to engage in political activism is assessed using 11 items. Most of these items refer to publicly visible actions—such as volunteering for a pro-climate organization, attending a pro-climate protest, or contacting government officials. However, some items relate to actions that could be considered more private, such as voting for a pro-climate candidate or donating money to a pro-climate organization. All 11 responses were combined into a single political activism index used as the outcome variable in their analysis. As a result, the index cannot be interpreted as a clear-cut measure of public climate action.

Data availability

Data will be made available on request.

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