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## Analysis

# Tailored information and the public support for carbon pricing in Germany

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## ABSTRACT

Providing information about carbon pricing is widely considered to be key to increasing public support for it. A number of studies have analyzed such effects, but little attention has been paid to how changes to the design can enhance the effectiveness of information treatments. Typically, generic information with low affective appeal has been used. In contrast, this paper, drawing on recent research into the design of information provisions, employs a targeted and tailored approach to ensure high receptiveness. Utilizing novel German survey data, we examine whether video-based information tailored to individuals' carbon pricing concerns outperforms generic information. Our results confirm that targeted and tailored information significantly increases support, primarily for respondents concerned about costs. However, the effect of tailored information concerning fairness and effectiveness is statistically not different from that of the generic control video. Nevertheless, it reduces strong opposition, providing valuable insights for policy acceptance. These findings suggest that integrating targeted communication strategies into climate policy design may help build broader policy tolerance and stability, though further research is needed to confirm their effectiveness beyond the specific context of this study.

## 1. Introduction

Carbon pricing schemes are widely recommended by economists as the most effective policy instrument for mitigating climate change (Akerlof et al., 2019). However, despite their widespread adoption in many countries (IMF, 2021), public opposition remains a significant barrier to the wider implementation of the policy as well as the adoption of more ambitious price trajectories (Douenne and Fabre, 2022; Drews and van den Bergh, 2016; Sommer et al., 2022). Although economists recognize the benefits of carbon pricing, such as its high efficiency in reducing emissions and the potential to offset any regressive distributional impacts through revenue redistribution (Goulder et al., 2019; Klenert et al., 2018), concerns regarding the high cost (e.g., Jagers and Hammar, 2009; Baranzini and Carattini, 2017; Hagemann et al., 2019), unfair distribution of costs (e.g., Rivers and Schaufele, 2015; Clayton, 2018; Dreyer and Walker, 2013; Bergquist et al., 2022; Brannlund and Persson, 2012), and the ineffectiveness of the policy in reducing global

emissions (e.g., Sælen and Kallbekken, 2011; Steg et al., 2006) are important drivers of low public support.

To the extent that these concerns are based on misperceptions or lack of knowledge, providing the public with information about actual costs, effectiveness, and fairness of the policy may increase public support. However, it needs to be delivered effectively in the sense that it is not ignored, distrusted, or even discredited by the target audience. In a recent review of information interventions, Haaland et al. (2023) emphasize the importance of tailoring credible, clearly presented messages based on personal characteristics to the audience. This contrasts somewhat with the mostly text-heavy information treatments used to test the role of information for policy acceptance.

Building upon these insights, we analyze whether tailored communication – providing information targeted to distinct groups based on certain characteristics – is more effective than general information

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in increasing support for carbon pricing. To this end, we designed a large-scale survey experiment with approximately 4,000 respondents, addressing key concerns identified in the literature as the main characteristic used for segmentation: personal cost, fairness, and effectiveness.<sup>2</sup> Respondents were first asked to identify their primary concern and then shown an animated video tailored to that concern. A randomly selected control group was shown a generic video on the introduction of carbon pricing.

Comparing the effects of the treatment and control videos, this paper is the first to provide empirical evidence on the effectiveness of tailored communication in increasing public support for carbon pricing. Beyond changes in majority support, we further examine the potential of tailored communication to reduce strong opposition to carbon pricing and, hence, polarization in policy attitudes. Finally, we employ machine learning techniques to study treatment heterogeneity and identify audience segments that respond more strongly to the tailored information. Through this research, we contribute to the broader literature on communication strategies for climate policies, offering important insights for policymakers and the economic literature concerned with building public support for carbon pricing.

Previous economic research has focused primarily on the core information – assuming that individuals have information deficits that they want to address – and less on the most effective method of conveying this information (e.g., Douenne and Fabre, 2022; Carattini et al., 2017; Andre et al., 2021; Cantner and Rolvinger, 2024). However, as noted by Bager and Mundaca (2017) in the context of policies addressing smart meter technologies, the design and how the information is presented matter for its effectiveness. Recently, information experiments have increasingly included aspects related to effective messaging (Haaland et al., 2023). For example, Dechezleprêtre et al. (2025) and Fang and Innocenti (2023) exploit experimental variation between video treatments to test for differences in the impact of alternative information videos on the support for carbon pricing. Both studies find that combining explanations of policy functionality with discussions of the costs of climate change outperforms the effect of either type of information alone.

In addition, several laboratory and survey experiments show that once people are (better) informed about revenue recycling options in carbon pricing schemes, such as targeting vulnerable households or earmarking revenues for green investments, public acceptance of the policy increases significantly (e.g., Sælen and Kallbekken, 2011; Baranzini and Carattini, 2017; Kallbekken et al., 2011; Kötchen et al., 2017; Beiser-McGrath and Bernauer, 2019; Steg et al., 2006).

While these findings shed light on the importance of the information content, there is a wide range of insights and recommendations from psychological and communication research that have yet to be applied in economics. Most empirical research on effective climate policy communication focuses on the United States. It is primarily based on laboratory experiments and not directly related to carbon pricing (e.g., Kim et al., 2023; Martel-Morin and Lachapelle, 2022; Myers et al., 2012; Hine et al., 2016). In a study of political campaigns in the US, Goldberg et al. (2021) show that a tailored campaign in targeted congressional districts increased Republicans' understanding of the existence, causes, and harms of climate change by several percentage points. A recent study by Mildener et al. (2022) shows that individualized information about actual personal rebates from carbon pricing has only a limited effect on public support in Switzerland. At

<sup>2</sup> In a recent meta-analysis of more than 50 studies, Bergquist et al. (2022) find that while concern and belief in climate change are strong drivers of carbon pricing support, perceived fairness and environmental effectiveness are even more important. Other studies emphasize cost burden as a significant barrier to public support for carbon pricing (e.g., Sælen and Kallbekken, 2011; Sommer et al., 2022; Douenne and Fabre, 2022; Beiser-McGrath and Bernauer, 2024).

the same time, the study finds that support among conservatives in Canada actually declined in response to the campaign, as opponents of the policy used the information to campaign about the inadequacy of the rebates. Other studies have shown framing a Pigouvian tax as a carbon “offset” rather than a “tax” can have a positive effect on public support (Hardisty et al., 2010; Drews and van den Bergh, 2016).<sup>3</sup>

Germany is an interesting case study for this experiment for several reasons. As the fourth largest economy, it is responsible for nearly 2% of global greenhouse gas (GHG) emissions. To meet its emissions reduction targets, Germany introduced a carbon price of €25 per metric ton of CO<sub>2</sub> on fossil fuels in the heating and transport sectors in 2021.<sup>4</sup> In the run-up to implementation, there has been an intense public debate about the necessity, design, and distributional impacts of the proposed policy and how they will affect public support. Partly in response to this heated debate, the government launched an advertising campaign, including animated informational videos, to raise awareness and build public support. Given these circumstances, our study provides insights into the effectiveness of communication strategies in a real-world setting. While acknowledging cross-country differences in how ideology, partisanship, and socio-economic factors influence policy support (Beiser-McGrath and Bernauer, 2024), our comprehensive survey data and heterogeneity analyses offer insights into effective messaging strategies beyond the German context.<sup>5</sup>

Our results suggest that, overall, tailored information (videos addressing a specific concern) has a greater impact on the stated acceptance of carbon pricing than a generic informational video. However, this effect is primarily driven by the impact of cost information, while communication on the other concerns of fairness and effectiveness does not outperform the generic video. Nonetheless, we observe heterogeneous treatment effects across all three tailored videos. For instance, the fairness video is particularly effective in increasing acceptance among respondents with particularly strong fairness preferences. In addition, the effectiveness video increases acceptance among respondents who already have a good knowledge of carbon pricing. Finally, we observe a reduction in strong opposition to the policy across all three video treatments. These findings suggest that tailoring communication to specific concerns can enhance public acceptance of carbon pricing. However, the extent and consistency of these benefits depend on the nature of the concern and the characteristics of the target audience, which we discuss extensively at the end of this paper.

In Section 2, we introduce our research hypotheses. Section 3 describes the methodology and data, including the experimental design (Section 3.1) and the empirical methods (Section 3.4). We present the empirical results in Section 4, followed by an analysis of the effectiveness of the targeting strategy and the tailoring of the videos. The paper concludes with a discussion of key findings, their limitations, implications for policy, and directions for future research.

<sup>3</sup> Similarly, Mundaca and Wamsler (2025) find that framing interventions targeting the willingness to adopt low-carbon and low-energy-intensive consumer and lifestyle behaviors as “contributions” rather than “taxes” is most likely to increase climate action among high-income earners.

<sup>4</sup> The policy provides for an explicit price trajectory that increases the carbon price to €55 by 2025, followed by the implementation of a national emissions trading scheme.

<sup>5</sup> Recent studies on carbon price support in Germany largely focus on the role of revenue recycling and often find that green spending is the most preferred revenue use type (e.g., Sommer et al., 2022; Kaestner et al., 2023). Insights from psychology point at the importance of pro-environmental orientation of citizens for policy support (Merten et al., 2022; Gerdes et al., 2023; Kaiser et al., 2023; Goerg et al., 2025). However, studies on the role of tailored information for carbon price acceptance in Germany are missing.

## 2. Research hypotheses

The main research question of this paper is whether targeted and tailored information can increase public support for carbon pricing more effectively than general information. As such, our focus is not on whether information *per se* is better than no information, as this has already been tested (Sælen and Kallbekken, 2011; Baranzini and Carattini, 2017; Kallbekken et al., 2011; Kotchen et al., 2017). Rather, our aim is to contribute to the sparse literature on what constitutes effective communication with respect to climate policies. We formulate two overarching hypotheses to guide our experimental design, grounded in the literature on information provision and policy support.

A large body of research in economics and psychology has discussed the role of information in shaping household behavior and attitudes. Some empirical studies have shown that tailored information can significantly affect households' attitudes and behavior (e.g. Allcott, 2011), especially when cognitive biases, such as present bias or limited attention, are at play. However, evidence on the effectiveness of such interventions remains mixed (e.g. Andor et al., 2020, 2022). Related research in psychology also highlights the ineffectiveness of mere information to overcome information deficits and change consumer behavior (Abrahamse et al., 2005; Bergquist et al., 2023).

Crucially, recent studies emphasize that the effectiveness of policy communication not only depends on the content, but also on how (Bager and Mundaca, 2017; Haaland et al., 2023) and to whom (Hine et al., 2014; Marshall et al., 2018) the information is presented. These insights are rooted in models of belief updating, where individuals revise their attitudes or behavior in response to credible and salient information — particularly when it addresses their priors or concerns. In this framework, communication effectiveness is enhanced by reducing informational frictions and aligning message framing with audience-specific preferences or values. Accordingly, targeting (delivering a message to specific audiences) and tailoring (adapting the message to audience characteristics) are seen as key mechanisms to increase the salience and credibility of the message (Moser, 2010; Haaland et al., 2023). We follow the recommendations by Haaland et al. (2023) and incorporate four empirically grounded principles in the design of our information treatments: (1) tailoring information to personal characteristics is preferable to general information, (2) the credibility of the information source needs to be considered, (3) the identification of the sender plays an important role in how information is perceived, and (4) the information should be presented in a way that maximizes comprehension. These principles jointly form the theoretical foundation for our hypotheses as well as empirical design and methods.<sup>6</sup>

In the context of carbon pricing, these considerations are particularly relevant because public support often hinges on specific concerns that at least partially may be grounded on false or incomplete knowledge. In particular, three concerns stand out: perceived fairness (Rivers and Schaufele, 2015; Maestre-Andrés et al., 2019; Bergquist et al., 2022; Benjamin et al., 2022), perceived cost burden (Jagers and Hammar, 2009; Rivers and Schaufele, 2015; Sommer et al., 2022), and perceived ineffectiveness of the policy in mitigating climate change (Sælen and Kallbekken, 2011). Providing respondents with factual, targeted information addressing these concerns may be more effective in correcting biases and increasing public support. Accordingly, we hypothesize that (H1) *tailoring information to a targeted audience increases carbon price support more effectively than generic information.*<sup>7</sup> Hence, we are interested in the conjoint effect of targeted and tailored information compared to more general information as our main analysis.

<sup>6</sup> While traditional information deficit models posit that providing factual information alone is sufficient to change attitudes, this view has been widely critiqued (e.g., Simis et al., 2016; Suldoovsky, 2017). Our approach explicitly moves beyond this, thereby increasing the likelihood of effective attitude change.

We further expect treatment responses to differ among the three targeted concern audiences, for several reasons. First, pre-treatment acceptance of carbon pricing is likely to be correlated with concerns, with some concerns being more strongly related to acceptance than others. This is likely to affect the marginal propensity of the treatment effect. Second, a moderating factor may be whether concerns are based on “incorrect” or “incomplete” knowledge (Li and Wagner, 2020), the extent of which may differ systematically across concerns. Third, the information content and consequently, participants' receptiveness to it may vary across the three concern dimensions, with cost information being more fact-based and fairness and effectiveness information potentially conflicting more with individuals' core moral beliefs (e.g., Aramovich et al., 2012). That said, recent evidence from Austria shows that value-congruent framing does not necessarily increase carbon pricing support (Bolte et al., 2024). Fourth, environmental attitudes, which are highly correlated with carbon price support (Merten et al., 2022; Kaiser et al., 2023), may affect individuals' responsiveness to the treatment. Last, besides best efforts to design three equivalent information treatments, differences in video quality may have influenced the outcome. Based on these considerations, we propose a second hypothesis: (H2) *The treatment response to the tailored videos varies across the three concern audiences (cost, fairness, and effectiveness).*<sup>8</sup>

## 3. Methodology and data

Our experiment was part of a large survey of about 7,000 German individuals conducted during the summer of 2021, eliciting citizens' attitudes toward the (then-current) national carbon price of €25 per tonne (Kaestner et al., 2024).<sup>9</sup> Participants were drawn from the forsa.omninet panel run by forsa,<sup>10</sup> a respected German survey institute. The panel comprises approximately 100,000 individuals and is representative of the German online population above the age of 14. For this study, survey participation was limited to individuals aged 18 and above to ensure representativeness of the German voting population. Data collection took place between August 18, 2021, and September 9, 2021, eight months after the introduction of the national carbon price (for further information on the survey, please refer to Holtz and Kaestner (2024)).<sup>11</sup>

The survey included three experiments (A, B, and C). Participants were randomly assigned to either experiment A (60%) or B (40%), followed in both instances by experiment C. This paper focuses on experiment A, which explores the impact of targeted information provision on individual acceptance of the German carbon price. Experiments B and C, which explore individual preferences regarding earmarking of revenues, are discussed in a separate study by Kaestner et al. (2023). The final sample for experiment A consists of 3383 respondents.

<sup>7</sup> Since tailoring communication requires targeting a specific audience, we explore the joint effect of both, comparing it to generic communication. Unlike prior work testing information versus no information (e.g., Sælen and Kallbekken, 2011; Baranzini and Carattini, 2017; Kallbekken et al., 2011; Kotchen et al., 2017), the focus of this study lies on what makes climate policy communication effective.

<sup>8</sup> Hypotheses 1 and 2 were formulated prior to data collection and discussed with both scientific and non-scientific audiences. While not pre-registered, documentation confirming their pre-data origin is provided among the supplementary materials. The wording of the hypotheses was slightly refined during the review process, without changing their substantive meaning. Analyses beyond these hypotheses are exploratory.

<sup>9</sup> Out of the 18,405 randomly invited panelists, 38% completed the survey, resulting in our targeted net sample of roughly 7000 respondents (N = 7058).

<sup>10</sup> The researchers had access to anonymized data only and participants gave their informed consent for data processing. All requirements of the German Data Protection Regulation were complied with.

<sup>11</sup> The survey data set is available as Scientific Use File (SUF) at the Research Data Center Ruhr at RWI – Leibniz Institute for Economic Research: <https://www.rwi-essen.de/en/research-advice/further/research-data-center-ruhr-fdz/data-sets/microdata/rwi-pik-capri-carbon-price-preferences-wave-1>.

**Table 1**  
Experimental group structure.

Audience #	(1) Costs		(2) Fairness		(3) Effectiveness		(4) Control	Total
	(1.T)	(1.C)	(2.T)	(2.C)	(3.T)	(3.C)		
info. video	cost	general	fairness	general	effect.	general	general	–
N	530	269	559	289	589	296	851	3383

Note: The table presents the sample sizes of the treatment and control groups for the three concern audiences, as well as the general control group.

### 3.1. Experimental design

To test our two research hypotheses, we designed a randomized information experiment. Figure A.1 in Appendix A provides an illustration of the experimental setup, and Table 1 summarizes the treatment and control structure as well as the sample size. As a baseline, participants were asked to rate their acceptance of the carbon price on a Likert scale of 1 to 5, where 1 represented “strongly disagree”, 5 “strongly agree”, and a rating of 3 implied “neither agree nor disagree”. This question, included to allow comparisons with other studies, also allows exploring the mechanisms behind potential treatment response heterogeneity studied in Section 5.

The experiment itself began with three questions assessing respondents’ level of concern about (1) the cost burden, (2) the fairness, and (3) the effectiveness of the carbon price, respectively. Responses to these questions could vary from 1 (no concerns) to 5 (very strong concerns), with an additional option for “don’t know”. Next, we performed a two-step random sorting process to assign participants to treatment and control groups.<sup>12</sup> First, 25% of respondents were randomly assigned to a general control group (group 4 in Table 1 and Figure A.1). The remaining 75% of respondents were assigned to one of the three “concern audiences” (cost, fairness, and effectiveness) based on the highest concern expressed by each participant in response to the survey question.<sup>13,14</sup>

A key design decision in our experiment was the adoption of a unidimensional segmentation strategy based on respondents’ self-reported concern levels. A multidimensional segmentation approach, taking into account a broader spectrum of individual characteristics such as values, could likely yield more distinct audience groups, who are more similar in their treatment response. However, this would necessitate more detailed data gathering and processing. For large-scale information campaigns, this level of complexity would be impracticable and financially onerous. Therefore, we opted for a more pragmatic approach, segmenting respondents based on their primary concerns. This enables us to test whether even simple (unidimensional) and resource-efficient targeting can enhance the effectiveness of carbon pricing communication, while recognizing the inherent trade-off.

In the second randomization, concern audiences were randomly divided into treatment and control groups: Two-thirds within each concern audience (treatment groups 1.T, 2.T, and 3.T) were shown a video tailored to their prime concern, while the remaining third in each concern audience (1.C, 2.C, and 3.C), along with the general control, watched a generic video without tailored content (1705 participants

<sup>12</sup> Stratified randomization was implemented using pre-specified strata based on population averages for gender, age group, and household size in Germany. Within each stratum, participants were assigned to experimental conditions using computer-generated pseudo-random numbers in both stages of randomization. This procedure ensures replicable and unbiased allocation into the respective treatment arms.

<sup>13</sup> We will use the term “concern audience” to refer to individuals assigned to the cost, fairness, or effectiveness audience. Each of these comprises a treatment and control group.

<sup>14</sup> Respondents who indicated “no concern” or “don’t know” for all three concern questions were excluded from the experiment due to being ineligible for treatment. If multiple concerns ranked equally high, stratified randomization was used to assign respondents to smaller audience groups to ensure groups of equal size.

in total).<sup>15</sup> Treatment videos were tailored to each concern audience, using relatable characters, a trustworthy speaker, and accessible language, as recommended by the literature (Marshall et al., 2018; Hine et al., 2014). The control video intentionally resembled an informational video released by the German government in early 2021. Further details on the videos, including scripts, visuals, and access links, are available in Appendix I.

The double randomization process was employed to ensure that the treatment and control groups were sufficiently large and comparable, both at the general (H1) and the audience level (H2), so that any observed differences in the outcomes could be attributed to the treatment. In particular, the shares were chosen such that the allocation led to two rather equal groups of 50% in the control and treatment group, respectively, when comparing all three treated concern audiences combined against all combined control groups, while still maintaining a sufficiently large number of observations in the treatment and control groups at the audience level. Following the videos, respondents were asked again about their acceptance of the carbon price, the outcome variable in our empirical assessment.

As a manipulation check, respondents were again asked to rate their level of concern regarding the carbon price after watching the videos. While we queried concern levels both before and after the videos, the information was collected for distinct purposes. As explained earlier, pre-treatment concern measures were used to assign respondents to concern audiences as part of the targeting. The post-treatment concern measures were included solely as a manipulation check to verify whether the videos effectively influenced participants’ concern levels they were designed to target. As such, post-treatment concerns are not analyzed as a primary outcome variable. The significant differences in concern levels before and after the treatment indicate that the videos effectively influenced participants as intended, both across the entire sample and within each audience group (see Appendix G).

Lastly, our experiment was specifically designed to reduce the risk of various biases highlighted by the literature: (1) experimenter demand effect – respondents indicating higher acceptance because they think this is the goal of the survey (Rosenthal, 1966; Zizzo, 2010); (2) social desirability bias – respondents answering in a socially acceptable manner; and (3) hypothetical bias – responses diverging from true preferences because attitudes are elicited in relation to hypothetical scenarios. Since both groups viewed videos on the same subject matter, experimenter demand and social desirability biases are unlikely to differ systematically between the treatment and control groups. Therefore, any biases are canceled out by the difference specification between the treatment and control groups in the estimation.<sup>16</sup> While hypothetical bias remains a limitation in any stated attitudes study, the carbon price was already in effect at the time of the survey, and respondents were reminded of this. Any remaining bias is mitigated by comparing the acceptance rates of the treatment and control groups.

<sup>15</sup> Treatment compliance was ensured with an audio test and the inability of participants to fast forward or skip the video segment.

<sup>16</sup> Studies conducted by de Quidt et al. (2018) and Mummolo and Peterson (2019) found the impact of the experimenter demand effect on responses in an empirical setting to be minimal, further reducing any potential concerns.

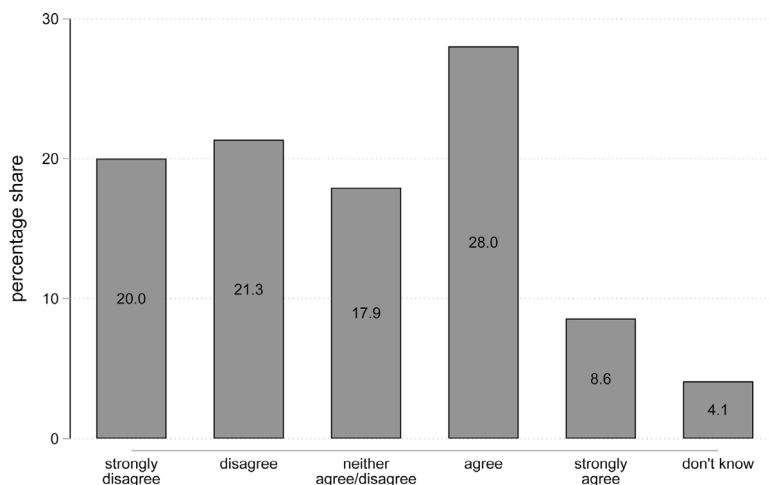


Fig. 1. Acceptance prior to the experiment.

Note: The figure presents the level of support to the carbon price by assessment of the €25 price level in force in Germany at the time of survey. N = 3383.

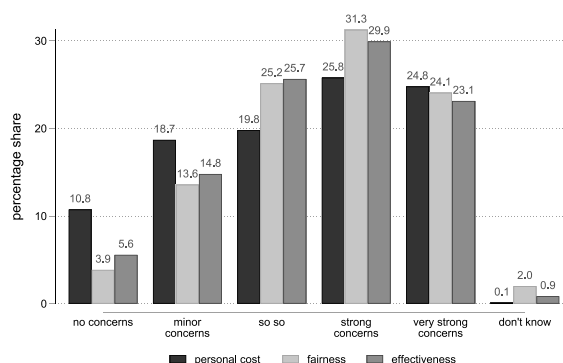


Fig. 2. Level of concern before treatment.

Note: The figure plots the indicated level of concern prior across the three dimensions effectiveness, personal cost, and fairness. Answers could range from 1 (no concerns) to 5 (very strong concerns) with the additional option of “don’t know”. N = 3383.

### 3.2. Baseline acceptance and concern levels

Prior to the experiment, we elicited individuals’ baseline acceptance levels. Fig. 1 plots the acceptance of the €25 carbon price — which was in force in Germany at the time of survey. Roughly 40% of respondents rejected it, while 37% accepted it. At the same time, 18% neither accepted nor rejected it, and another 4% were unsure. Although over 50% did not reject the policy outright, a priori the carbon price lacked majority support.

As depicted in Fig. 2, the responses from participants reveal a significant level of concern across all three elicited dimensions, with a strong correlation between the three concerns ( $\rho \in [0.35, 0.56]$  with  $p < 0.01$ ). More than half of the sample expressed some level of concern regarding the cost burden, fairness, and effectiveness of a carbon price. Interestingly, there are slightly higher shares of “strong concerns” and lower shares of “no concerns” regarding fairness and effectiveness, compared to cost burden. However, the share for “very strong concerns” varies only marginally across the three dimensions. Moreover, fewer respondents indicate uncertainty or no opinion (do not know) regarding the cost burden, which suggests that opinions on this dimension may be more firmly established.

### 3.3. Additional controls

Alongside the experiment, we elicited a variety of socio-economic, demographic, and attitudinal information from respondents. Summary

statistics of the sample are provided in Table 2. The average respondent is 54 years old and roughly 59% of the sample are male. The German annual micro census (Destatis, 2021b) indicates that 49.5% of the population is male, implying that female respondents are underrepresented in the sample. Compared to the German population, our sample is also somewhat older and better educated (see Appendix B).

Furthermore, we collected a large set of variables that are known to influence public support for carbon pricing. As a proxy of the cost burden, we inquired about the primary heating fuel (e.g., Jagers and Hammar, 2009; Baranzini and Carattini, 2017; Beuermann and Santarius, 2006). Over 70% of households heat with gas or oil, and over 90% of respondents are frequent car users, suggesting non-negligible additional costs due to the carbon price for most respondents. We also assessed political attitudes (e.g., Leiserowitz, 2006; Park and Vedlitz, 2013; Hammar and Jagers, 2007; McCright et al., 2014; Zhao et al., 2011; Baranzini and Carattini, 2017), finding high engagement with about 70% of respondents seeking political information daily. With an average score of 4.8 the sample has a fairly centrist political stance on a scale from 1 (left) to 10 (right). Trust in the government is low, with 57% distrusting the current government and 73% its climate policy. At the same time, about half of the sample say they “rather trust” or “completely trust” reports on climate change from public broadcasters or national daily newspapers.

Moreover, we gathered information on respondents’ prior knowledge of the carbon price. Only 22% correctly identified the carbon price

**Table 2**  
Summary statistics.

	Full sample		Costs		Effectiveness		Fairness	
	mean	sd	mean	sd	mean	sd	mean	sd
<i>Acceptance:</i>								
acceptance pre treatment	0.366		0.298		0.370		0.378	
acceptance post treatment	0.459		0.410		0.449		0.465	
<i>Socioeconomic factors:</i>								
respondent age	53.80	16.09	54.09	15.68	52.83	16.53	54.46	15.59
male	0.586		0.586		0.592		0.580	
high school grad. (Abitur)	0.428		0.340		0.471		0.418	
hh income (mean cat value)	3460.2	1467.8	3316.5	1411.5	3574.3	1487.7	3427.2	1471.1
rural	0.196		0.210		0.192		0.186	
formerly GDR	0.148		0.169		0.143		0.147	
<i>Cost factors:</i>								
fossil heating	0.710		0.727		0.702		0.702	
frequent driver	0.871		0.915		0.851		0.875	
<i>Political factors:</i>								
politically interested	0.706		0.681		0.702		0.731	
political affil. left-right (1–10)	4.775	1.770	5.116	1.698	4.670	1.762	4.686	1.813
trust government	0.429		0.412		0.421		0.406	
trust gov. climate	0.278		0.301		0.251		0.267	
trust media climate	0.518		0.440		0.536		0.503	
<i>General concerns:</i>								
worried climate	0.684		0.595		0.700		0.694	
worried health	0.426		0.457		0.402		0.434	
worried youth education	0.627		0.643		0.607		0.644	
<i>CO2 knowledge and environmental behavior:</i>								
adj. scale env. friendly behav. (0–1)	0.576	0.148	0.540	0.145	0.587	0.152	0.583	0.145
NEP scale continuous (0–1)	0.808	0.165	0.793	0.169	0.815	0.167	0.809	0.164
know corr. revenue use	0.515		0.512		0.499		0.499	
know CO2 price level	0.228		0.208		0.232		0.232	
<i>Fairness preferences:</i>								
equity fairness princ. (0–1)	0.506	0.198	0.517	0.201	0.505	0.200	0.504	0.194
equality fairness princ. (0–1)	0.638	0.184	0.656	0.185	0.626	0.183	0.634	0.187
merit fairness princ. (0–1)	0.697	0.173	0.684	0.175	0.697	0.176	0.705	0.174
<i>Local Covid cases:</i>								
COVID total cases (per 100k)	2763.7	4245.1	2902.6	4494.4	2684.9	4040.6	2748.2	4353.9
Observations	3383		1255		1388		1358	

level in effect at the time (Table 2). We also asked about the current use of carbon price revenues, which approximately half the sample answered correctly.

We also collected information regarding concerns on a range of other topics. Climate change was identified as the most pressing, followed closely by concerns about youth education; approximately 40% expressed concerns about their personal health, which is unsurprising, given that the survey took place during the SARS COV-19 pandemic. Finally, we measured environmental and fairness attitudes. A shortened version of the New Ecological Paradigm (Dunlap et al., 2000) – a measure of a pro-ecological worldviews – yielded an average score of 0.576 (on a 0-1 scale).<sup>17</sup> Fairness attitudes, elicited using the established scale from Schmitt et al. (1995), indicate that respondents tend to favor the merit principle over the equality and equity principles.<sup>18</sup>

### 3.4. Empirical approach

In line with the literature (e.g., Carattini et al., 2017; Baranzini and Carattini, 2017; Mildenerger et al., 2022; Fremstad et al., 2022),

<sup>17</sup> To assess the internal consistency of the New Ecological Paradigm (NEP) scale, we calculated Cronbach’s alpha. The reliability analysis for the 6-item scale yielded  $\alpha = 0.75$ , indicating acceptable internal consistency (Nunnally and Bernstein, 1994).

<sup>18</sup> Cronbach’s alpha for the three-item subscales ranged from 0.27 to 0.50. Although these values fall below the conventional threshold of 0.70, item-test correlations were moderate (0.63–0.72), and the scales have been previously validated in the literature. Given the short length of the scales, which tends to deflate alpha values, all items were retained.

we define policy acceptance  $Y_i$  as a binary variable equal to 1 for participants who “agree” or “strongly agree” (4 and 5) with the carbon price and 0 otherwise. To derive the average treatment effects (ATE) of our tailored videos, we employ two approaches: a simple difference-in-means (SDM) and a regression-adjusted (RA) estimator.

Under random assignment, the ATE can be derived using an SDM estimator. Let  $W_i$  be the treatment indicator for individual  $i$ . With  $N_1$  treated and  $N_0$  control individuals, the sample averages are defined as:

$$\bar{Y}_0 = N_0^{-1} \sum_{i=1}^N (1 - W_i) Y_i \tag{1}$$

$$\bar{Y}_1 = N_1^{-1} \sum_{i=1}^N (W_i) Y_i,$$

where  $Y_i$  is the observed outcome for individual  $i$ . Following from the sample averages, the SDM is then defined as:

$$\hat{\tau}_{SDM} = \hat{Y}_1 - \hat{Y}_0. \tag{2}$$

We can retrieve the SDM estimator from the simple regression (Imbens and Rubin, 2015a):

$$Y_i = \alpha + \tau W_i + \epsilon_i, \tag{3}$$

where  $\alpha$  is a constant,  $\tau$  is the parameter to be estimated, and  $\epsilon_i$  is a random error term.

Since the SDM is only valid under perfect randomization, we use a balancing test suggested by Imbens and Rubin (2015b) to validate the randomization strategy. This involves calculating the normalized difference in covariate values and their composite score as a natural measure of the distance between the locations of the distributions and

the overlap across the entire set of covariates. Unlike a t-statistic, the normalized differences provide a scale- and sample-size-free way of assessing overlap. Additionally, we analyze the difference in the logarithms of the two sample standard deviations to measure distribution dispersion. Detailed test results are provided in Appendix C. The results indicate a strong balance of covariates across treatment and control groups, in the full sample and within each respective concern audience. These results support the validity of our identification strategy.

However, given the complexity of our random sampling strategy, we assess the robustness of our ATE estimates by testing the sensitivity of the results to the inclusion of pre-treatment covariates. In this regard, we employ RA estimators that control for pre-treatment heterogeneity in the treatment and control groups by including relevant covariates in the regression model. The RA approach involves a two-step process. In the first step, two separate regression models of the outcome variable on a set of covariates are estimated for the treatment and control groups. In the second step, the predicted outcomes from the first step are averaged for each individual and experimental condition to obtain the potential-outcome means (POMs). The difference in these POMs provides an estimate of the ATE. By exploiting the differences in the averages of treatment-specific predicted outcomes, the RA approach effectively reduces the bias that can arise from pre-treatment differences in the covariates between the treatment and control groups.

To derive the RA estimator, we define the linear projections ( $L$ ) of the potential outcomes on the vector of covariates  $X_i$  as

$$L[Y_i(0)|1, X_i] = \alpha_0 + X_i' \beta_0 \tag{4}$$

$$L[Y_i(1)|1, X_i] = \alpha_1 + X_i' \beta_1,$$

where the sample average is defined as  $\bar{X} = N^{-1} \sum_{i=1}^N X_i$ . Hence, the RA estimator is given by:

$$\hat{\tau}_{RA} = (\hat{\alpha}_1 - \hat{\alpha}_0) + \bar{X}'(\hat{\beta}_1 - \hat{\beta}_0). \tag{5}$$

We can obtain this estimator from the regression

$$Y_i = \alpha + \tau W_i + X_i' \beta + W_i \dot{X}_i' \delta + \epsilon_i, \tag{6}$$

where  $\dot{X}_i$  are the demeaned covariates, given the sample averages  $\bar{X}$ . With treatment heterogeneity, as demonstrated by [Negi and Wooldridge \(2021\)](#), the RA estimator with pre-treatment covariates asymptotically yields an ATE that is at least as precise as the SDM estimator.

#### 4. Results

Our results are organized into three main sections. First, to confirm the role of the targeted concerns, we examine the level of acceptance of the German carbon price before any treatment was administered. Next, we delve into the experimental results, with a particular focus on the differences observed between the three treatment videos. Finally, we explore the full Likert scale of the indicated acceptance level to assess any effects of the treatments on strong opposition to the carbon price.

##### 4.1. Understanding baseline acceptance of the carbon price: The role of concerns

To assess the treatment efficacy, we begin by examining the distribution of acceptance in our sample at baseline, focusing on its relationship to the concerns targeted by our intervention. To this end, we estimate a logistic regression model predicting pre-treatment acceptance as a function of the three concern variables, alongside a full set of controls including socioeconomic, demographic, attitudinal, and trust indicators. This analysis demonstrates how baseline concerns influence acceptance, providing insight into the potential of tailored information to impact outcomes. It also identifies other factors that may contribute to variations in treatment effects.

As with acceptance, the three concern measures are transformed into dummy variables that take the value of 1 for those with “strong

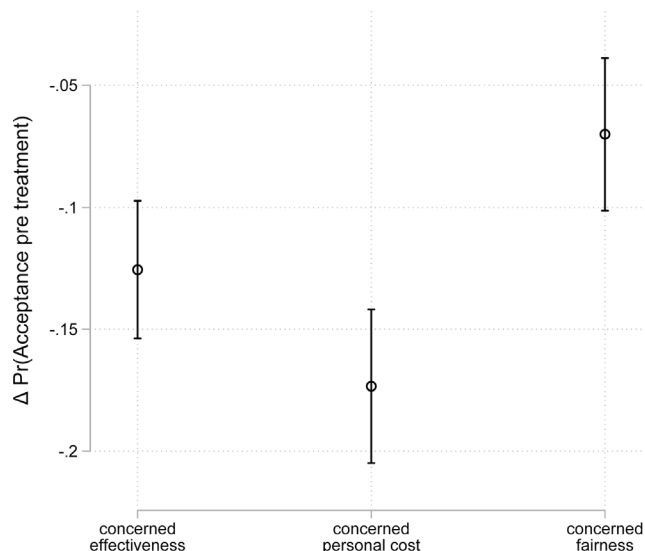


Fig. 3. Association between concerns and pre-treatment acceptance (*non causal*).

Note: The figure plots the marginal effects of the three concern dummies (personal cost, fairness, & effectiveness) derived from a logistic regression model. The binary outcome variable indicates acceptance of the carbon price prior to the treatment. Note that the results indicate correlation rather than causation. Additional covariates included in the model are: individual characteristics such as age and gender, cost burden indicators, political variables, other concerns, environmental knowledge and attitudes, fairness preferences, and COVID-19 cases per 100,000. Robust standard errors are applied. Whiskers represent the 95% CI.  $N = 3383$ .

concerns” or “very strong concerns” and zero otherwise.<sup>19</sup> See Appendix D for a detailed discussion of the estimation results, including alternative model specifications. It is noteworthy that the estimates should be interpreted as correlations rather than causal effects.

Our estimates largely corroborate findings from the existing literature concerning the association of our control variables with carbon price acceptance. A surprising result is the lack of a significant relationship between our carbon pricing cost proxies (fossil heating and frequent car usage) and acceptance. This lack of association suggests that proxies for objective cost exposure may be less predictive of carbon price attitudes than previously thought.

Instead, we find subjective concerns – personal cost, fairness, and effectiveness – are strongly correlated with baseline acceptance.<sup>20</sup> Fig. 3 plots the corresponding marginal effects for the three concern variables. Importantly, despite a comprehensive set of controls, the concern variables – particularly personal cost – exhibit the strongest association with acceptance. Since objective cost proxies appear relatively unimportant, the pronounced effect of cost concern suggests that such apprehensions may often stem from incomplete information and are thus more amenable to updating in response to tailored information.<sup>21</sup>

Overall, the strong negative association between the three concerns supports our decision to segment the respondents based on these factors in the experiment. In comparison, socio-economic variables are less predictive of policy support.

<sup>19</sup> Comparable transformations are applied to any control variables that are based on questions with a Likert scale as the response format.

<sup>20</sup> Differences between cost and both effectiveness and fairness concerns are statistically significant at  $p = 0.019$  and  $p = 0.000$ , respectively.

<sup>21</sup> Cost concerns are only weakly correlated with the cost proxies (fossil heating  $\rho = 0.0420, p = 0.015$ ; frequent car usage  $\rho = 0.1425, p < 0.001$ ).

**Table 3**  
Treatment effect on acceptance: Tailored/Targeted versus general information.

Model	POM	ATE		Obs.
	control	effect	p-value	
logistic regression model	0.440	0.037	0.031	3,383
regression adj. logit outcome model <sup>a</sup>	0.441	0.036	0.017	3,383

Note: For all models, robust standard errors are applied. For the simple logistic model, the ATE is retrieved calculating the marginal effect of the treatment indicator.

<sup>a</sup> Estimates are derived using regression adjustment methods that exploit differences in the means of treatment-specific predicted outcomes. Covariates included in the two outcome models are: individual characteristics, cost burden indicators, political variables, other concerns, environmental knowledge and attitudes, fairness preferences, and COVID-19 cases per 100,000. Model performance checks for the RA model indicate low multicollinearity (mean VIFs of 8.49, reduced to 1.27 [max 1.69] after dropping quadratic controls) and minimal outlier influence ( $\Delta D = 1.13$ ,  $\Delta \chi^2 = 0.995$ ,  $\Delta \beta = 0.017$ ). Model fit is good (Pearson  $\chi^2 = 3311.96$ ,  $p = 0.5797$ ) with no signs of over- or underdispersion.

#### 4.2. Joint treatment effect of tailored information

Relative to the baseline acceptance, we observe an average increase of 9.3 percentage points (ppt) in acceptance after the videos (significant at the 1% significance level; see Appendix E). This translates into a 22% increase in acceptance (4–5 on the Likert scale) among those who initially rejected the carbon price prior to the treatment (1–3). This effect size aligns with findings from other studies. For instance, [Dechezleprêtre et al. \(2025\)](#) observe a 9 to 15 ppt increase in the acceptance of a carbon price policy (with and without revenue redistribution) following exposure to a video explaining three environmental policies including a carbon price, among the German sub-sample.

However, our prime interest lies in understanding to what extent the 9 ppt overall increase in acceptance is driven by the tailored treatments. To this end, we estimate the ATE of the tailored videos on acceptance, independent of the targeted concern, as outlined in H1 and described in Section 3.4. Considering the binary definition of our outcome variable, we apply a logistic regression model for both the SDM and the RA approach. [Table 3](#) provides the corresponding estimates. The results based on SDM and the RA methods are highly similar in terms of effect size and precision, confirming that the randomization strategy worked well at the sample level.<sup>22</sup> We observe a small but significant ATE ranging from 0.034 to 0.037 at the 5% significance level, while the corresponding POM for the control group is 0.44.<sup>23</sup> Hence, the treatment videos increased acceptance by around 3.5 ppt over the control group. To contextualize this finding, while the control group's acceptance grew by 7 ppt, viewing a video tailored to their prime concern resulted in a more than 10 ppt increase in support among the treated individuals – a 50% increase in the effect size relative to the control.

#### 4.3. Differences in the treatment response across concern audiences

We next examine differences in the treatment effect across the three concern audiences (H2). To identify differences across the three treatments, we first restrict the control group for each treatment video to respondents selected for the respective audience, assigned to the audience control (1.C, 2.C and 3.C). We also include individuals drawn for the general control group (4) who would have been eligible for the treatment given their level of concern. For example, anyone in Group 4 whose primary concern is the fairness of the carbon price, qualifying them for fairness treatment, is reassigned to the fairness control group.<sup>24</sup>

[Table 4](#) reports the respective ATE for each video, applying both SDM and RA methods. The ATEs vary across the three videos, ranging from 0.088 for the cost video to 0.042 for the fairness video, though

<sup>22</sup> The findings are highly robust to estimation using non-parametric methods (see Appendix F) and to specifications using the full Likert scale rather than a binary outcome. Additional results are available upon request.

<sup>23</sup> The results are highly robust to estimation using different non-parametric specifications. See Appendix F for detailed results.

<sup>24</sup> Note that the results are robust to restricting the control to the narrower within-audience control (1.C, 2.C and 3.C).

the differences are statistically insignificant ( $\chi^2=0.369$  and 0.210). Notably, applying RA methods results in a decline in effect size for all ATEs compared to the SDM results.<sup>25</sup>

In contrast to the cost video, we cannot identify any individual statistically significant effects for the fairness or effectiveness videos using the RA method. We repeated our pre-survey power analysis based on the final sample sizes to calculate the minimum detectable effect (MDE) size for a binary outcome. The test yields MDEs of 0.0499 for the effectiveness video and 0.0506 for the fairness video, indicating that the experiment is underpowered given the small observed effects in [Table 4](#).<sup>26</sup> Even if statistical power were not an issue, a modest additional 3% increase in acceptance in response to viewing the effectiveness video may not justify the substantial additional costs of designing tailored messages. Political campaigns are unlikely to fund such efforts for relatively minor gains.

Consequently, the more segregated analysis reveals that the out-performance of our tailored videos is primarily driven by the cost video. For individuals concerned with increasing costs, watching our tailored video increased acceptance on average by 8 ppt compared to the control group. Putting this finding into perspective, viewing the cost video increased acceptance among the cost audience from 30% (acceptance prior to the treatment) to 48%, a shift by 18 ppt.

#### 4.4. Impact on strong opposition

Beyond changes in acceptance, we are also interested in the potential of our treatments to reduce the strength of the opposition to the policy, for example shifts from 1 “strongly disagree” to 2 “rather disagree”. While these shifts may not directly affect policy acceptance, they could signal an increase in policy tolerance. A reduction in vehement opposition could also prove valuable, potentially reducing the risk of social unrest in response to stricter carbon policies. While achieving majority acceptance remains the primary objective, promoting increased tolerance could be a secondary but important outcome of information treatments.

Therefore, we employ a generalized ordered logistic regression model ([Williams, 2006](#); [Fu, 1998](#)), using information on the Likert scale to estimate the treatment effect of each video on post-treatment acceptance levels. The dependent variable is defined as an ordinal

<sup>25</sup> The RA results remain highly robust when estimated using non-parametric methods, with the fairness and effectiveness treatments remaining statistically insignificant. See Appendix F for details. Likewise, the conclusions regarding treatment effects hold when analyzing acceptance levels along the full Likert scale rather than using a binary outcome (see [Fig. 4](#)).

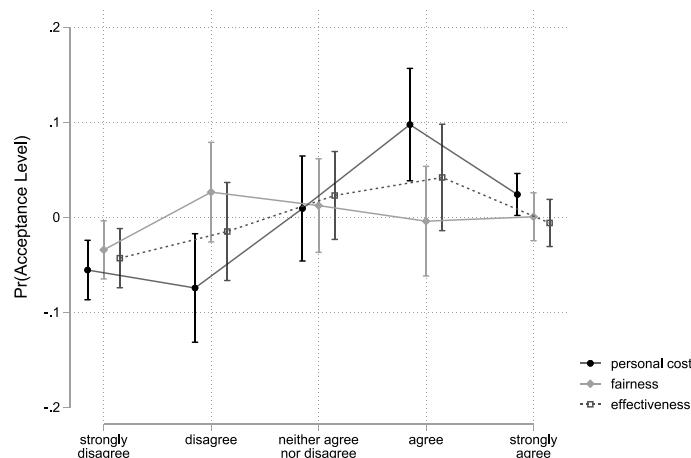
<sup>26</sup> A pre-survey power analysis indicated a MDE of 0.049 for within-audience comparisons, which we considered sufficient based on a conservative target MDE of 0.07 for binary outcomes ([Allcott and Taubinsky, 2015](#)). However, the general information video shown to the control group produced a larger-than-expected increase in policy acceptance, attenuating relative treatment effects and reducing them below the MDE for the fairness and effectiveness groups.

**Table 4**  
Treatment effects on acceptance by concern audience.

Concern	SDM methods			RA methods <sup>a</sup>			Obs.	
	POM	ATE		POM	ATE		T	C
	control	Effect	p-value	control	Effect	p-value		
personal cost	0.372	0.088	0.002	0.375	0.082	0.001	530	725
fairness	0.448	0.042	0.126	0.468	-0.000	0.987	559	799
effectiveness	0.426	0.055	0.042	0.434	0.034	0.151	589	799

Note: For all models, robust standard errors are applied.

<sup>a</sup> Estimates are predicted using regression adjustment methods that exploit the differences in the means of treatment-specific predicted outcomes. Covariates included in the two outcome models are: individual characteristics, cost burden indicators, political variables, other concerns, environmental knowledge and attitudes, fairness preferences, and COVID-19 cases per 100,000. Model diagnostics for the RA models indicate acceptable multicollinearity (mean VIF 9.3 with quadratic terms, reduced to 1.27 [max 1.68] after excluding them) and minimal outlier influence ( $\Delta D = 1.11$ ,  $\Delta \chi^2 = 1.04$ ,  $\Delta \beta = 0.046$ ). Model fit is good (Pearson  $\chi^2 = 3325.7$ ,  $p = 0.552$ ) with no evidence of over- or underdispersion.



**Fig. 4.** Average treatment effect on the level of acceptance.

Note: The control groups consist of respondents assigned to the control group within each respective audience (1.C|2.C|3.C + 4). Estimates are obtained from three separate generalized ordered logistic regressions with the post-treatment acceptance level (1–5) as the ordinal outcome variable, and the binary treatment variable as an explanatory variable. Covariates included in the three models are: individual characteristics, cost burden indicators, political variables, other concerns, environmental knowledge and attitudes, fairness preferences, and COVID-19 cases per 100,000. To facilitate model convergence, quadratic terms were excluded from the models. For all models, robust standard errors are applied. N: personal cost = 1237; fairness = 1350; effectiveness = 1375.

variable ranging from 1 (strongly disagree) to 5 (strongly agree).<sup>27,28</sup> The model also includes a set of controls to minimize any heterogeneity in the treatment effect resulting from pre-treatment differences between treated and controls.<sup>29</sup> Based on the estimates, we can infer whether and where along the scale we observe changes in the stated level of acceptance (e.g., among those strongly opposed versus those with less strong feelings).

Fig. 4 plots the ATEs of each video on the probability of respondents choosing the respective level of post-treatment acceptance. Our results show a statistically significant decline in the probability of respondents strongly disagreeing with the carbon price for all three treatment videos. For instance, the probability of strongly disagreeing with carbon pricing is reduced by around 5 ppt after watching the effectiveness video. This pattern suggests that the tailored videos were particularly

effective at reducing strong opposition toward the policy, regardless of the targeted concern.

### 5. (Un)successful targeting or tailoring

Our study suggests that providing tailored information can be an effective way to increase stated acceptance of a carbon price. However, we find that this effect is driven by the strong impact of tailored information among the cost audience. It remains unclear whether the lack of a relative impact – i.e., the failure of the fairness and effectiveness videos to outperform the control – is due to ineffective targeting (misidentifying the relevant audience) or tailoring (e.g., the information not being processed, understood, or accepted, possibly due to mistrust of the messenger).

#### 5.1. Failed tailoring

As described in Appendix I, we carefully developed the content, figures and formats of the videos, and pretested them with both a panel of experts and a pre-study sample from forsa. Nevertheless, it is possible that the video messages did not sufficiently address the concerns of their respective audience. To rule out systematic differences in the video quality, we conducted a short post-video evaluation covering both content and speaker perceptions. All respondents answered four questions pertaining to the video content. In addition, participants in the treatment groups were asked to respond to three questions

<sup>27</sup> If the assumptions of the ordered logit model, often referred to as parallel regressions assumptions (Williams, 2006), are violated, the generalized ordered logistic regression model is considered the superior model when dealing with ordinal data. The significant Brant test for the effectiveness and the fairness videos (Prob >  $\chi^2 = 0.0194$  and  $0.0842$ , respectively) indicates that the ordered logit regression model is inappropriate given the data at hand.

<sup>28</sup> Note that individuals who choose “don’t know” for post-treatment acceptance are dropped from the sample for this analysis.

<sup>29</sup> To facilitate model convergence, quadratic terms were excluded from the model.

about the speaker.<sup>30</sup> The post-video evaluation results are presented in Appendix H.

We observe only minor differences across the video evaluations. Regarding the content quality, the results reveal that a greater number of individuals indicated having learned something from the cost video, suggesting it introduced more novel information. This, in turn, may have facilitated greater belief updating and increased policy support. While the effectiveness video received the strongest support for its use of realistic examples, this did not translate into stronger treatment effects, suggesting that realistic examples are not sufficient to trigger belief updating. Moreover, respondents who watched the effectiveness video rated the speaker more competent and trustworthy — possibly reflecting a more favorable view of scientists. Yet, if these perceptions were decisive, they would instead undermine the superior performance of the cost video.<sup>31</sup> Consequently, the video evaluation suggests that belief updating, rather than perceived content quality, better explains treatment effects.

## 5.2. Failed targeting

Differences in treatment effects may also relate to the targeting strategy, which hinges on accurately identifying distinct concern audiences. Comparing the summary statistics for each concern audience (see Table 2) reveals some differences in the covariates across the three groups. For instance, pre-treatment acceptance was higher among the effectiveness and fairness groups, potentially reducing the marginal responsiveness to the treatments. Non-urban households and frequent drivers are more common in the cost audience, while individuals with left-wing political views, heightened environmental concerns, and a pro-ecological worldview are more prevalent among the fairness and effectiveness audiences. These latter groups also indicate greater trust in media reports on climate issues, but lower trust in government climate policies. Additional differences in age, education, and income were small but significant. In sum, separating respondents by their primary concern produced three somewhat distinct groups, however, the homogeneity of their response to the treatments remains uncertain.

To shed some light on this, we leverage machine learning techniques to study the heterogeneity of the treatment effects for each video. This allows us to discern whether and how different types of individuals within each concern audience respond to our videos. Employing a causal random forest methodology (for details, see Athey et al., 2019; Wager and Athey, 2018), we estimate conditional average treatment effects (CATEs) based on the covariates identified as potentially important in the literature. To assess the significance of these moderating effects, we plot the predicted CATEs on the  $y$ -axis and rank them according to their size on the  $x$ -axis.

The CATEs, presented in Fig. 5, reveal significant heterogeneity in the effect size for all three videos. Notably, roughly 10% of the predicted CATEs for the effectiveness and 25% for the fairness videos are negative, indicating a relative decrease in acceptance of up to 5 ppt. Conversely, all CATEs for the cost video are consistently positive. The presence of “relative” negative CATEs suggests that the targeting strategy for the effectiveness and fairness videos was not successful, as these two tailored videos did not consistently outperform the general information video.

The negative CATEs underscore the risk that mismatched targeting can be counterproductive, especially since perceptions of fairness and

<sup>30</sup> We omitted these questions for the control group, as the generic video was designed to resemble an official governmental video with an anonymous speaker.

<sup>31</sup> In a follow-up survey conducted in 2023 that included a second round of video evaluations, we found only minor differences in perceived didacticism across videos. Additionally, the effectiveness video was rated as the most informative.

effectiveness are shaped by values and emotions, making tailoring messages to address these concerns a challenging task (Kavvouris et al., 2020). Poor targeting or tailoring may increase the likelihood of resentment toward the information. Conversely, the positive CATEs across all three videos demonstrate the potential benefits of effective targeting: if the right message reaches the right person, acceptance can increase by as much as 20 ppt.

Finally, we analyze the relationship between the treatment effect size and each covariate. For this purpose, we estimate a simple OLS model, where the CATEs for each video are regressed on a set of covariates. As shown in Fig. 6, several patterns emerge. Rural residents and frequent drivers respond less positively to the treatment videos, possibly due to limited access to climate-friendly transportation alternatives, which may reduce the credibility of the message. The treatment effect of the fairness video declines with income, whereas it increases for the cost video. One possible explanation is that the portrayal of the polluter-pays principle may alienate wealthier respondents, who typically have larger carbon footprints.

All three videos underperform among respondents who have confidence in government climate policies. This group generally expresses less concern about carbon pricing, leading to a lower marginal propensity for treatment for this group. Similarly, the impact of the treatment decreases as respondents exhibit more environmentally friendly behaviors, perhaps because they already implement many of the behavioral changes depicted in the videos. By contrast, with growing pro-ecological worldviews respondents become more receptive to the effectiveness and fairness treatment, underscoring the role of core beliefs in shaping responsiveness to information.<sup>32</sup> Fairness preferences also strongly moderate treatment effects. As expected, the fairness video resonates well with individuals with strong fairness notions. In contrast, the absence of explicit fairness discussions in the non-fairness videos may have hindered efficacy for the other two groups.

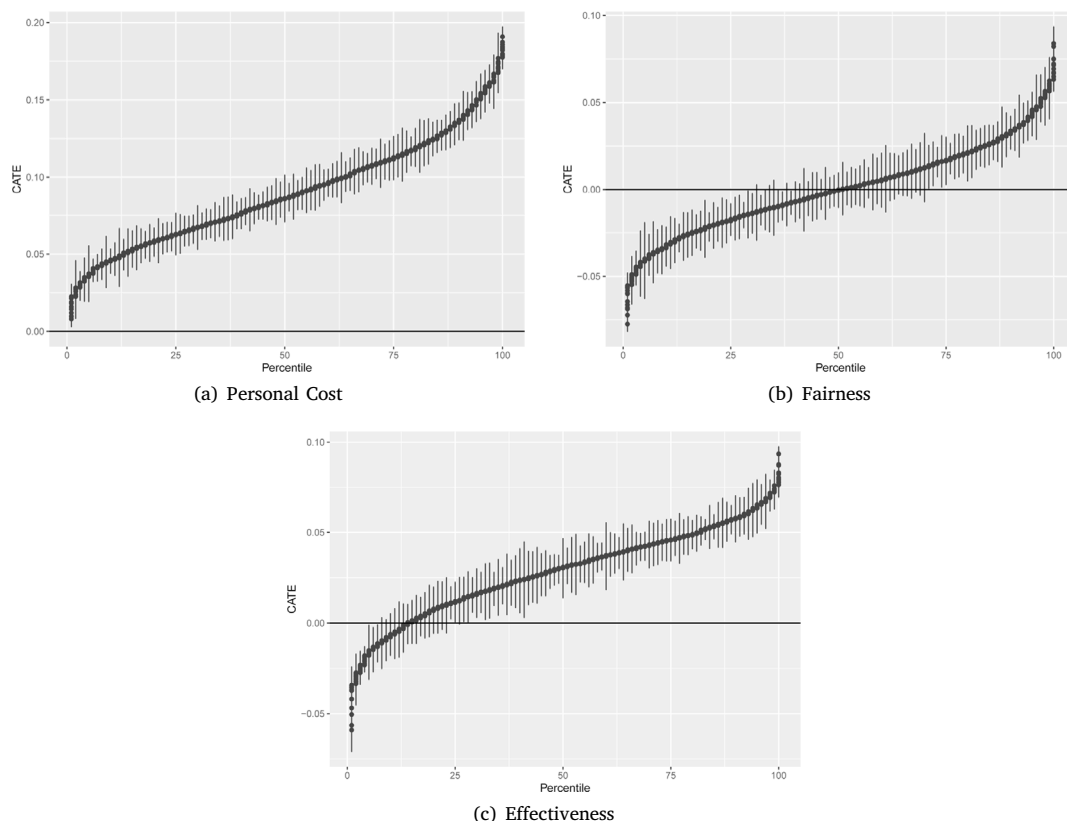
Participants with greater climate concerns are less responsive to the effectiveness treatment, possibly doubting that a carbon price alone is sufficient to address climate change. For them, a broader overview of the policy mix – as in the generic information video (the Federal Climate Change Act) – may be more compelling.

The heterogeneity in the CATEs highlights both the potential of refined targeting and the risks of misdirected tailoring. A more nuanced segmentation – considering demographics, values, prior knowledge, or attitudes alongside concerns – may enhance the success of information campaigns. However, it should be noted that 54% of the participants indicated high cost concerns and could, in theory, be assigned to the cost-related audience. Certain participants may also have been better served by the cost video than by the fairness or effectiveness videos, despite expressing high concerns in those dimensions. In general, our findings emphasize the need for further research to develop optimal segmentation approaches to effectively communicate climate policies.

## 6. Discussion and conclusion

Targeted communication delivered by trusted sources is widely regarded as a potentially effective strategy to increase public support for carbon pricing and climate policy more broadly (Hine et al., 2014; Marshall et al., 2018). However, empirical evidence on its actual effectiveness and optimal strategies for targeting and tailoring communication remains sparse. We contribute to this literature by conducting a survey experiment to assess the impact of tailored informational videos – designed to address common concerns about carbon pricing – on a target audience’s acceptance of the policy. Our results indicate that tailored videos on the cost, fairness, and effectiveness of the carbon

<sup>32</sup> A vast literature examines the determinants of pro-environmental behavior, emphasizing the crucial role of core beliefs and values. See, for example, Steg (2016).



**Fig. 5.** Ranked predicted conditional treatment effects.

*Note:* CATEs are estimated using random forest methodology (for details, see [Athey et al., 2019](#); [Wager and Athey, 2018](#)). Additional covariates included in the model are: individual characteristics such as age and gender, cost burden indicators, political variables, other concerns, environmental knowledge and attitudes, fairness preferences, and COVID-19 cases per 100,000. The control groups consist of respondents assigned to the control group within each respective audience and those eligible to the respective treatment in the general control group (1.C|2.C|3.C + 4). N: personal cost = 1255; fairness = 1358; effectiveness = 1388.

price are collectively more effective than an untailed control video at increasing overall acceptance. This effect is primarily driven by the strong impact of the cost video. While we did not find any conclusive evidence on the overperformance of the fairness and effectiveness videos, all three treatments individually reduced the proportion of respondents who are strongly opposed to the policy — suggesting a tolerance effect. Furthermore, conditional average treatment effects reveal considerable heterogeneity in treatment responses: while we observe relatively strong increases in acceptance in response to tailored messages of up to 20 ppt, a non-negligible share of respondents in the fairness and effectiveness groups respond less positively than those in their respective control groups. This underscores that even well-intentioned tailoring may not always outperform untargeted communication, and in some cases, may even reduce message effectiveness when values or trust in the messenger are misaligned.

While the study offers valuable insights, a few important limitations remain. First, the unexpectedly large positive impact of the untailed control video limited our statistical power to detect differences in the effect sizes across the three tailored concerns. As a result, we cannot draw definitive conclusions regarding the superiority of tailoring beyond the cost video.

Second, the external validity of the study is limited in the sense that the detailed results are not necessarily generalizable. The study was conducted in Germany in 2021 in a unique and highly dynamic policy context with a strong focus on the high perceived cost burden of the new carbon price. While we believe that the overarching findings are applicable more widely, particularly in similar Western European contexts, further research is required to test the generalizability of the results for other countries and policy settings.

Third, despite the careful development of the treatment, we were only able to test one tailored message for each concern. Ideally, we could evaluate different versions of tailored messages to identify the ‘best’ information treatment. This limitation was primarily due to budgetary and time constraints, as designing and producing multiple tailored videos for each concern is costly and time-consuming and would have required a considerably larger sample to allow identification of differences across the multiple concern videos.

Fourth, and linked to the third limitation, the respective target group must first be reached in real-world contexts. Even with a perfectly tailored message, the information may have no impact if it does not reach its audience. How to identify the appropriate communication channels and successfully reach the audience has yet to be analyzed.

Finally, for policy implications, it is important to distinguish between changes in short-term stated policy acceptance and actual behavioral change. While our results show that tailored communication can shift attitudes, it remains unclear whether these effects persist in the long term (e.g. [Schultz, 2014](#)). Moreover, attitude changes do not automatically translate into actions, such as voting for political parties advocating carbon pricing. The disconnect between attitudes and behavior is well documented (e.g., [Abrahamse et al., 2005](#); [Bergquist et al., 2023](#)). This raises the broader question of whether tailored communication can ultimately help translate increased acceptance into meaningful behavioral change — and, in turn, support the implementation of effective climate policy.

Despite these limitations, our analysis offers several policy recommendations. First, targeting and tailoring information that addresses carbon pricing concerns can indeed increase policy acceptance. This is particularly important for countries like Germany, where tailored

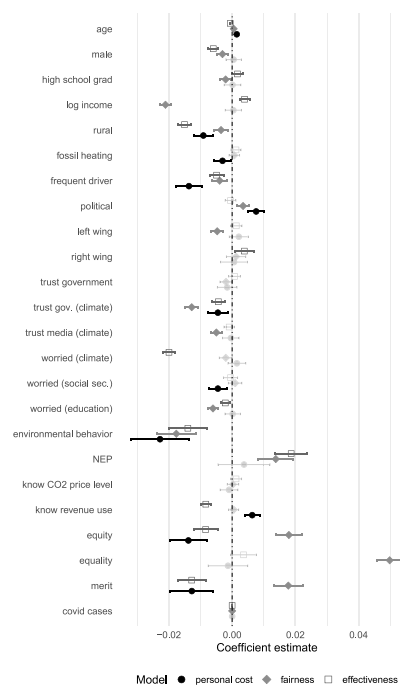


Fig. 6. Treatment heterogeneity by video treatment.

Note: The figure plots the coefficient estimates from a simple OLS regression, in which the estimated CATEs are regressed against a set of covariates. For ease of reading, insignificant estimates in light gray. The control groups consist of respondents assigned to the control group within each respective audience, and those eligible to the respective treatment in the general control group (1.C|2.C|3.C + 4). N: personal cost = 1255; fairness = 1358; effectiveness = 1388.

communication strategies have not yet been employed. Second, tailored communication incurs higher costs than generic communication. Our findings suggest that identifying audiences and tailoring respective messages may not always be successful and that expected benefits have to be weighed against their costs. Third, given the generally high concerns regarding costs and the success of the cost video, providing tailored cost information has shown promise to increase policy acceptance. Fourth, policymakers should recognize that tailored communication can yield important secondary benefits by reducing strong opposition. Tailored messages appear to perform consistently better at “defusing” strong opposition, even when they fail to raise support.

Our findings, along with the limitations identified above, point to several key areas for future research. A better understanding is needed of when and to what extent tailored communication is most effective, in particular with regard to cost-effectiveness. For example, it remains an open question whether it would be sufficient to focus on tailored cost messages, or whether more sophisticated strategies – e.g., targeting values and sentiments – are more effective. Moreover, the tolerance effect deserves further scholarly attention. It may have important political implications, such as enhancing stability of contentious policies by reducing resistance in the medium term and expanding the range of options available for policymakers.

In addition, the actual effectiveness of different media channels for communicating tailored messages warrants further investigation. This includes traditional media, where audience characteristics are typically well known and persistent (e.g., newspapers), as well as social media platforms. While social media is increasingly being used by politicians for climate communication, it presents new challenges (Winter, 2023), such as designing effective dynamic targeting algorithms and addressing the privacy concerns associated with large-scale data use. Last, tailored messages, especially on social media, may be increasingly

exploited to disseminate misinformation, leading to greater polarization or confusion among targeted audiences. As such, future research should not only focus on the impact and cost-effectiveness of tailored messages, but also explore the potential of tailored communication to instill polarization.

#### CRedit authorship contribution statement

**Antonia Schwarz:** Writing – review & editing, Writing – original draft, Project administration, Methodology, Formal analysis, Data curation, Conceptualization. **Anna Stünzi:** Writing – review & editing, Writing – original draft, Methodology, Conceptualization. **Kathrin Kaestner:** Writing – review & editing, Methodology, Data curation, Conceptualization. **Michael Pahle:** Writing – review & editing, Funding acquisition, Conceptualization. **Stephan Sommer:** Writing – review & editing, Funding acquisition, Formal analysis, Conceptualization.

#### Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

#### Appendix A. Supplementary data

Supplementary material related to this article can be found online at <https://doi.org/10.1016/j.ecolecon.2025.108849>.

#### Data availability

The data is publicly available. We have provided the link in the manuscript. Upon publication the code will be made available.

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