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ABSTRACT

How Malleable Are Pro-environmental Preferences? Evidence from a Randomized Survey Experiment*

With growing emphasis on sustainable practices, carbon taxes and congestion charges are emerging as key tools to reduce greenhouse gas emissions and improve air quality, yet they often face public resistance. Using longitudinal data from a randomized survey experiment in Luxembourg, this paper investigates whether providing relevant information about these two green mobility policies influences pro-environmental attitudes (stated support and willingness to pay for the carbon tax) and behaviors (carbon offsetting donations). The first treatment, which informs participants that public support for urban congestion charges tends to increase after implementation, has little to no effect. In contrast, information on the use of carbon tax revenues (redistribution and energy-efficient investments) has a large positive impact on both stated and revealed pro-environmental preferences. Our results indicate that support for the carbon tax is more elastic to information on its redistributive aspect, rather than on its use for funding green projects. Additionally, constraints to behavioral change and pre-treatment environmental attitudes play a role in treatment response heterogeneity, and show that confirmation bias can moderate responses to information, especially among those skeptical of climate science.

JEL Classification: D83, H23, H31, Q58

Keywords: survey experiment, climate policy, carbon tax, preferences, taxation, Luxembourg

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1 Introduction

In the face of mounting climate challenges and international calls to drastically reduce global GHG emissions, policies like carbon taxes and congestion charges have emerged as potentially transformative instruments (OECD, 2011; Dubash *et al.*, 2022; Stavins, 2011). Carbon taxes, praised for their effectiveness, efficiency, revenue potential, and transparency, achieve emission reductions by raising the costs of carbon-intensive goods and services (Aldy and Stavins, 2011; William, 2010), while congestion charges discourage car use in high-traffic areas by directly pricing the external costs of congestion.

However, these measures often encounter public and political resistance (Umit and Schaffer, 2020; Douenne and Fabre, 2022). Empirical evidence indicates that individuals' support hinges not only on perceived economic impacts (Metcalf and Stock, 2020, 2023), but also on perceived fairness, with beliefs about winners and losers across income levels influencing acceptance (Douenne and Fabre, 2020). While carbon taxes can appear regressive, studies highlight that revenue allocation can mitigate these concerns (West and Williams, 2004; Bento *et al.*, 2009; Douenne and Fabre, 2022), and fairness-enhancing strategies such as compensating low-income households (Bolderdijk *et al.*, 2017; Jagers *et al.*, 2019; Maestre-Andrés *et al.*, 2019) or investing in climate projects (Maestre-Andrés *et al.*, 2021) bolster public support. Additionally, perceived coerciveness reduces appeal (Attari *et al.*, 2010; Kallbekken *et al.*, 2011; Brannlund and Persson, 2012; de Groot and Schuitema, 2012), while familiarity and experience with the policy tend to increase acceptance over time (Ewald *et al.*, 2022). Similarly, congestion charges have been found to gain greater support post-implementation due to increased public trust, demonstrated effectiveness, and the realization of co-benefits (Schuitema *et al.*, 2010; , n.d.; Gu *et al.*, 2018). Building on these insights, Carattini *et al.* (2017) recommend (i) earmarking carbon tax revenues for mitigation projects or to address regressivity, and (ii) ensuring clear, well-timed communication before and after implementation as critical steps to enhance public acceptability.

In this paper, we investigate whether providing relevant information about tax revenue allocation and increased support after implementation influence pro-environmental attitudes and behaviors. To this end, we leverage longitudinal data from a randomized survey experiment in Luxembourg, on a sample of about 1000 participants observed three times from November 2022 to August 2023. The survey covered a variety of topics, including individuals' sociodemographics, beliefs about climate change, trust in institutions and politicians, personality traits, and a wide variety of sustainable behaviors and attitudes, including carbon tax support and actual donations for carbon offsets. In the last survey wave, one third of respondents were randomly assigned to a 'revenue allocation' treatment, informing them on the use of the carbon tax revenues in Luxembourg, and another third was assigned to a 'benefits underestimation' treatment, informing them of the larger ex-post acceptability of climate taxes (such as congestion charges).

Comparing these groups with the remaining third of the sample (control group) allows us to estimate the causal effects of information provision on preferences for the carbon tax. We measure preferences not only by eliciting attitudes towards carbon emission reductions (stated support and willingness to pay for the carbon tax), but we additionally observe actual behaviors (carbon offsetting donations). One major advantage of integrating behavioral outcomes among our outcome variables is that it mitigates suspicions that our findings on stated preferences might be induced by experimenter demand or desirability bias.

Regarding the effect of the information treatments on attitudes towards the carbon tax, we find that information on the use of carbon tax revenues has a strong positive impact on respondents' support and willingness to pay for the carbon tax. More specifically, this treatment increases willingness to pay for the carbon tax by 0.15 standard deviations – an increase of almost €8 per tonne of CO2, roughly equivalent to the effect of a two-thirds increase in household income. As for the second information treatment, we expected that a heightened awareness of the acceptance and benefits of congestion charges might have improved overall support for climate taxes, including the carbon tax. Although the effect of the 'benefits underestimation' treatment is positive in most cases, it is always smaller in size than the 'revenue allocation' treatment, and it is not statistically significant at conventional thresholds. This suggests that information provision is more effective at improving policy support when the information directly targets a specific policy.

As for revealed preferences, the incentives design of the study allows us to measure actual carbon offsetting behaviors.[1] At the end of each wave, participants were indeed offered the option to convert their monetary compensation into the purchase of carbon credits. Interestingly, revealed preferences results are consistent with those on stated preferences, with the 'revenue allocation' information treatment having a large positive effect on both the extensive and intensive margin of carbon credits donations, whereas the 'benefits underestimation' treatment had no effect on average.

Results are robust to a battery of sensitivity and placebo tests. Among other things, results hold in longitudinal Difference-in-Differences model controlling for individual fixed-effects, which address any residual confounding coming from unobservable, time-invariant individual heterogeneity.[2]

The rest of the paper explores the role of different sets of individual factors in driving our results.

[1] As explained in greater detail in subsection 2.1, general survey participation was incentivized through a fixed participation compensation of €10 and enrollment to a lottery to win an additional €250 prize. In addition, extra bonuses were distributed to incentivize norm-eliciting questions.

[2] While individual characteristics are in principle balanced across the treatment and control groups, thanks to the random treatment assignment, the finite sample size could lead to slight unbalances happening by chance. To address this, we first formally test the balance of observable covariates and then address any residual concerns from potential unbalancedness of unobservable characteristics by estimating a longitudinal model with individual fixed-effects.

When looking at individual characteristics, we find little treatment effect heterogeneity on the basis of pre-determined variables (i.e. gender, education, having children). Digging further into individual drivers of pro-environmental attitudes and behaviors, we find that the 'revenue allocation' treatment effect is mostly driven by respondents with limited mobility and financial constraints – that is, respondents living in urban areas, who drive little and with higher household income. In addition, to investigate whether the information treatments complement or substitute preexisting pro-environmental attitudes and behaviors, we examine treatment effect heterogeneity based on respondents' past attitudes and behaviors. Results indicate that the 'revenue allocation' treatment acts as a substitute to pre-existing attitudes for individuals initially opposed or neutral to the carbon tax, shifting their stated preferences toward greater carbon tax support. For revealed preferences, only the groups at the extremes of the distributions respond positively to the 'revenue allocation' treatment, with those showing high levels of support or donations reacting disproportionately more. This suggests that, while the treatment acted as a substitute for weaker preexisting attitudes and behaviors, yielding modest improvements, its larger effects came from complementing the strong initial attitudes of those already scoring high on the outcome distributions.

Last, we address the role of priors about the treatment content in moderating the effect of the information treatments (Haaland *et al.*, 2023). The average treatment responses could in fact hide substantial heterogeneity based on whether or not respondents expect the information we provide them with, or find it aligned with their beliefs. For three out of four outcomes, the 'revenue allocation' treatment effect appears to be driven by respondents who did not know about the redistributive purposes of the carbon tax. For both revealed and stated preferences outcomes, the null zero effect of the 'benefits underestimation' treatment reflects a mix of positive and negative reactions to the information treatment. Those who did not know about the larger ex-post public support for climate taxes, but thought that the information made sense, increase their support for the carbon tax and donations towards carbon credits. In contrast, the 'benefits underestimation' treatment backfires among those who were expecting the opposite (i.e. *smaller* public support after the implementation of climate taxes), leading to reduced support for the carbon tax. Our findings suggest that participants in this group exhibit, on average, lower levels of trust – especially in climate science – and are more likely to doubt the reality of climate change or its anthropogenic causes. We interpret this as evidence that confirmation bias may exert a stronger influence when respondents are presented with information on others' attitudes toward climate taxes, as opposed to information on the use of tax revenues.

In Luxembourg, a country with one of the EU's highest per capita GHG emissions, carbon pricing measures already play a central role in climate policy. Since 2021, 88.9% of CO2 emissions have been covered through an emissions trading system (ETS) and carbon taxes (OECD, 2022). Recognizing the high levels of emissions per capita — 20.6 tonnes of CO_2 equivalent in 2019, partly due to nonresident commuters and transit freight traffic — the government introduced a carbon tax in 2019, initially set at €20 per tonne of CO_2. This tax, which has increased by €5 annually since 2022, is expected to raise around €150 million in additional revenues to fund climate measures, support low-income households, and finance investments in sustainable infrastructure.[3] Luxembourg's climate targets are ambitious: to achieve a 55% reduction in emissions outside the ETS by 2030 and to reach climate neutrality by 2050. Given these goals, effective policies are essential to decarbonize key sectors such as transport, service, and buildings.

This study contributes to the policy debate by investigating mechanisms that could enhance public support for carbon taxation, providing evidence that increased transparency about revenue use and perceived fairness can foster positive public attitudes, while information on others' attitudes toward climate taxes are welcomed with mixed reactions.

In addition, this study makes three primary contributions to the literature on public support for carbon taxation and sustainable policy design, advancing the understanding of both practical implementation and public perception of environmental taxes. First, while much of the existing research on carbon taxation has focused on hypothetical policies, we provide real-world insights by analyzing attitudes toward Luxembourg's implemented carbon tax. This focus allows us to examine actual responses to a policy with concrete fiscal impacts and a defined revenue allocation plan, addressing a gap in the literature regarding public reaction to existing, rather than hypothetical, carbon pricing schemes (Baranzini and Carattini, 2017; Jagers *et al.*, 2019; Klenert *et al.*, 2018; Maestre-Andrés *et al.*, 2021).

Second, this study sheds light on the underlying mechanisms of public support by providing causal estimates of how specific information on tax revenue uses and tax support influences sustainable attitudes and behaviors. Past studies have shown that information on how tax revenues are allocated can significantly impact attitudes, especially if revenues are earmarked for redistributive purposes or public investments that align with citizens' preferences (Bergquist *et al.*, 2020; Green, 2021). Our approach differentiates itself by examining the effects of two types of existing revenue allocation of the carbon tax in Luxembourg — redistribution to low-income households and investments in green projects — and their impact on public support. This dual focus offers a more granular understanding of how transparency and clarity about revenue allocation can either mitigate or exacerbate perceived economic inequalities and address the broader concern of tax regressivity often associated with carbon taxes (West and Williams, 2004; Bento *et al.*, 2009; Douenne and Fabre, 2020). Our findings suggest that respondents' support for the carbon tax is more sensitive to information on the redistributive uses

[3] See: `https://delano.lu/article/delano_carbon-tax-heart-climate-plan`, last accessed on 28 October 2024.

of the tax revenues than their investment into green projects, confirming that fairness concerns are an important component of tax acceptability.

Third, our heterogeneity analysis adds nuance to the literature by examining how individual factors — such as sociodemographic traits, prior beliefs about climate change, trust in institutions, and behavioral constraints — influence responses to carbon tax policies. Prior research highlights that carbon taxes are often seen as coercive or regressive, which can limit support, particularly among individuals who feel disproportionately burdened by these taxes (Attari *et al.*, 2010; Kallbekken *et al.*, 2011; Brannlund and Persson, 2012; de Groot and Schuitema, 2012). Our findings contribute to this discourse by illustrating how perceptions of fairness can vary based on personal characteristics and context-specific factors, such as financial or mobility constraints, which affect the practical feasibility of adopting low-carbon alternatives (Douenne and Fabre, 2022). While novel to this paper, results on the heterogeneous 'benefits underestimation' treatment response by prior beliefs are consistent with evidence from Umit and Schaffer (2020), who show that political trust and external political efficacy correlate with lower uncertainty around the effectiveness of carbon taxes and improved support.

Last, our results capture not only stated support for carbon taxes but also actual pro-environmental behavior, providing a more comprehensive picture of the impact of information provision on preferences for climate taxes. To the best of our knowledge, this is the first experimental paper on this domain to measure revealed, on top of stated, preferences. Among other advantages, revealed preferences reflect actual choices, which are less prone to bias arising from experimenter demand effect or social desirability bias (de Quidt *et al.*, 2018) than stated preferences.

The remainder of the paper is organized as follows. Section 2 describes the data and the main estimation strategy. Section 3 presents the main results, accompanied by robustness checks and by a subsection addressing heterogeneity and mechanisms. Last, Section 4 offers concluding thoughts and directions for further research.

2 Data and methods

2.1 Data description

The data used in this study was collected by the Luxembourg Institute of Socio-Economic Research (LISER), in the form of a longitudinal online survey. The target population of the survey were around 3,700 individuals who had participated to LISER surveys in the past and who had consented to be contacted for future studies. Participants were informed about data usage and their rights, and their anonymity was guaranteed.[4]

The first wave of the study, conducted in November 2022, included 1,292 participants. In the second wave, which took place in April 2023, 1,026 respondents from the first wave participated. By the third wave in August 2023, 912 participants had completed all three rounds, forming the core sample for the analysis. The survey primarily explored behaviors related to sustainability, such as animal protein consumption, mobility patterns, home heating practices, and energy-efficient investments. Although these behaviors were self-reported, participants were also given the opportunity to donate part of their compensation to a carbon offsetting project, providing a concrete measure of sustainable action in addition to their survey responses. Attitudes toward sustainability were further assessed through questions about support for various green policies. Additionally, the survey gathered detailed information on the socio-economic and demographic characteristics of the respondents.

In each survey wave, to ensure accurate and engaged reporting, participants received a fixed reward of €10 conditional on completing the survey. In addition, they could receive a bonus of up to €30, based on the accuracy of answers to a series of guessing games regarding other respondents' prevalent actions and opinions. Last, respondents participated to a lottery in which ten participants per wave had a chance to win €250.

Importantly for this study, in wave 3 respondents were subject to a randomized information experiment. The information experiment was part of a module eliciting preferences for the carbon tax and, in order to ensure a minimum level of knowledge of the topic, each participant was exposed to an introductory text featuring a short description of the carbon tax. The module was presented to participants in the first third of the survey, thus minimizing survey-response fatigue, and right after the first of two attention checks.[5] The study design was such that, upon access to the third survey wave, participants had equal probabilities to be assigned to either one of the following three groups:

- *'revenue allocation' treatment*: participants in this group are first informed about the average yearly cost of the carbon tax for households in Luxembourg (€220 per year, €200 for the poorest 20%). Then, they receive information on the government allocation of carbon tax revenues, with half of the revenues being used for redistributive purposes (which fully compensate the cost of the tax for the least affluent 40%) and the remaining half being used to fund green projects (i.e. energy transition and public transport infrastructure).
- *'Benefits underestimation' treatment*: This second treatment is more focused on the benefits

[4] The choice to survey volunteers was guided by pragmatic aspects while remaining scientifically valid. Indeed, experimental literature has shown that the use of volunteer samples has a negligible impact on the measurement of preferences and other factors of interest (Anderson *et al.*, 2013; Abeler and Nosenzo, 2015).

[5] Failure to pass both attention checks resulted in the exclusion of participants from the survey.

coming from the implementation of the carbon tax, leaving aside the economic aspect linked to the revenues. Respondents are informed that people tend to underestimate the benefits of climate taxes before their implementation, but that support for this policies increases after the realization of benefits in terms of air quality, safety, congestion, savings.

- *Control*: Individuals in this last group do not receive any further information about the design of the carbon tax except for the general information frame that is presented to the entire sample at the beginning of this section.

More details on the exact wording of the introductory text and on the information provided to each group are available in Appendix B:. The randomness of the assignment to either group allows us to isolate the causal effect of a costless intervention (the provision of sustainability-related information) on the attitudes and behaviors of survey respondents, by simply comparing the outcomes of individuals in the treated groups to those in the control group.

The main outcome of interest is a categorical variable that measures participants' level of support for the carbon tax in Luxembourg. Respondents were asked to select one of five option: "strongly opposed", "somewhat opposed", "neutral/undecided", "somewhat favorable" and "strongly favorable". In addition, participants were asked to indicate the maximum level of the carbon tax (in euros per tonne of CO2) they would find acceptable, using a continuous scale of integers ranging from 0 to 300. This reflects the maximum tax burden, or maximum price, they are willing to pay to bear the benefits associated with the tax. To make the question as clear and relatable as possible, a descriptive table was provided alongside the question, where examples of carbon tax amounts were accompanied by their impacts on gasoline and diesel prices (expressed in euro cents per liter). The current carbon tax in Luxembourg (i.e. €30 per tonne of CO2) was highlighted for reference, together with its impact on gasoline price (10.9 cents per liter) and diesel price (9.1 cents per liter).

This first group of outcomes can be used to assess the effect of the information treatment on stated preferences for the carbon tax. However, stated preferences might suffer from experimenter demand effect and social desirability bias (de Quidt *et al.*, 2018), potentially causing respondents to overstate their support and willingness to pay for the carbon tax relative to their true underlying preferences. Our survey design addresses this limitation by supplementing stated preferences with observed behavior (revealed preferences), in the form of participants' voluntary donations of survey earnings to offset carbon emissions. Although revealed preferences may still carry some social desirability bias, the required monetary contribution imposes a higher personal cost than merely expressing support, helping to mitigate such biases. In particular, at the end of each wave participants were asked whether they would forgo their fixed earnings (€10 for participation, plus any bonus from the guessing games) and allocate them to the purchase of carbon credits. Respondents were additionally asked how much of the potential €250 lottery prize they would allocate to purchase carbon credits (with options ranging from 0 to 250), should they win the lottery. These two extra variables allow us to test whether the information treatment affected real behavior, by changing individuals' likelihood to donate part of their survey earnings to buy carbon credits. By combining stated and revealed preferences, we are thus able to gather a more comprehensive understanding of the consequences of our online experiment on behaviors and preferences.

2.2 Estimating equation and descriptive statistics

Our aim is to study the effect of two information treatments on preferences for the carbon tax in Luxembourg and donations to offset carbon emissions. We do so by estimating the following linear regression model:

$$Y_i = \alpha + \beta_1 R_i + \beta_2 BU_i + \gamma X_i + \epsilon_i \tag{1}$$

Where Y_i is one of our four main outcomes, namely: support for the carbon tax, measured on a 5-point Likert scale; willingness to pay (WTP) for the carbon tax; a dummy for donating base earnings to purchase carbon credits; and the share of potential lottery gains willing to donate to purchase carbon credits. For ease of interpretation, we standardize the first two outcomes to have mean zero and standard deviation (SD) one. R_i is a binary indicator for the 'revenue allocation' treatment arm, while BU_i indicates the 'Benefits Underestimation' treatment arm. X_i is a vector of individual controls, which includes standard individual characteristics – namely equivalized household income (in log),[6] household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Since the two revealed preference outcomes are measured at the end of the wave-3 survey, some additional controls are necessary in order to keep constant any confounding arising from subsequent, independent experimental modules that have been implemented in the survey. In particular, after the experiment on the carbon tax (the first to be asked in wave 3), five other independent survey experiments took place before we observed any carbon credit donations; we thus control for a set of treatment assignment dummies for each of these experiments.[7] Last, ϵ_i is the error term.

The coefficients of interest from Equation 1 are β_1 and β_2, which capture the average treatment effects of the 'revenue allocation' and the 'Benefits underestimation' treatments. Positive values of

[6] We use the square-root equivalence scale, as standard in the literature (Pendakur, 1999; Koulovatianos *et al.*, 2005).

[7] As the treatment assignment rules across experimental modules are independent of one another, controlling or not for these variables leaves our results unchanged.

the coefficients would imply that the information treatment improved support for the carbon tax and increased carbon-credit donations.

The final estimation sample consists of 907 participants with non-missing information for the variables of interest. Figure 1 and Figure 2 plot the distributions of the stated preferences outcomes (carbon tax support and WTP for the carbon tax) in the estimation sample. From Figure 1, the majority of participants is either neutral or favorable to the carbon tax. Among the opposed (25% of the sample), there is a relatively larger share of individuals reporting being "strongly opposed" rather than "somewhat opposed" to the tax. Figure 2 show that the WTP for the carbon tax is roughly normally distributed around the 2023 carbon tax level in Luxembourg (€30 per tonne of CO2), with a long right tail and some bunching around the value 100. The figure additionally shows the distribution of a similar variable, based on each respondent's guess of the maximum level of carbon tax supported by the majority of other respondents. Consistent with the literature on pluralistic ignorance about climate change (Leviston *et al.*, 2013; Lewandowsky *et al.*, 2015; Jiang-Wang *et al.*, 2024), whereby individuals tend to underestimate the degree to which others are concerned with the environment, respondents in our sample tend to report lower levels of carbon tax acceptance for others compared to their own.

This is confirmed by the descriptive statistics in Table 1, which show a €8.7 average gap between respondents' own WTP for the carbon tax and that attributed to others. Table 1 additionally describes revealed preferences outcomes: 25.9% of participants opted to donate part of their base earnings to purchase carbon credits, while the average donation from the lottery gains was 26.9%, with substantial variability (standard deviation of 0.357).

In terms of sample characteristics, the average household size is 2.75 members, with around 40% of the sample having at least one child. The sample features less women than men (33.8%) and relatively high-educated individuals (85.2% with post-secondary education). The most represented age group is 36-50 (42.9%), followed by 51-65 (31.9%), 26-35 (16.2%), and those over 65 (7.2%). The sample consists mostly of migrants, the vast majority of which come from the EU. The share of migrants in the sample is above the national average of roughly 50%, but lower than the one in the capital, Luxembourg city (72.7%). The majority of the sample resides in Luxembourg, while 32.2% are cross-border commuters living in one of the neighboring countries (Belgium, France, or Germany). In order to check that our results are not driven by self-selection into the sample, in robustness checks we re-weight observations to make them nationally representative of age and gender.

We last check whether observable characteristics are similarly distributed across treatments and control groups, as expected as a consequence of the random treatment assignment. Table A1 shows that individual characteristics are well-balanced across groups. The differences in mean characteristics across groups are close to zero and never statistically significant, suggesting that the random assignment was successful and that any differences in the outcomes across groups can be attributed to the treatment rather than pre-existing group heterogeneity. Nevertheless, because of the relatively small size of our main sample, we include the characteristics in Table A1 as controls in our preferred model specification.

3 Results

3.1 Main results

Results from the estimation of Equation 1 are reported in Table 2 and Table 3. Respectively, the first reports the effect of the information treatments on stated preferences concerning the carbon tax, while the second looks at whether the treatments had any effect on actual behaviors, in the form of monetary donations toward the offset of carbon emission. In particular, Table 2 displays results for two outcomes: carbon tax support in the first three columns and individual willingness to pay for the carbon tax in the last three. For each outcome, the first column features a simplified specification of Equation 1, where we pool individuals belonging to the two treatment arms into one single treated group, comparing them to the control group, and where we do not control for individual characteristics. Columns (2) and (5) then look at the two treatment arms separately and, last, columns (3) and (6) include individual controls to the regressions.

Column 1 of Table 2 suggests that individuals exposed to either information treatment support the carbon tax 0.13 SDs more than those in the control group. When disaggregating the treatment into its two arms, coefficients for the 'revenue allocation' treatment attract positive and statistically significant estimates both with and without the inclusion of individual controls. The small difference in magnitude from column (2) to (3) could be due to some individual controls resulting as slightly unbalanced, despite the random assignment to the treatments and control arms (see Table A1). Contrary to the 'revenue allocation' treatment, individuals exposed to information on the general tendency to underestimate benefits of green policies do not seem to change their support for the carbon tax significantly with respect to those in the control group – suggesting that the average effect of the information treatment found in column 1 is only driven by the participants assigned to the 'revenue allocation' treatment. A similar story holds when looking at the maximum level of the carbon tax that each individual would find acceptable, in columns (4) to (6). Here again the 'revenue allocation' treatment displays a positive, significant effect, while the coefficients attached to the 'benefits underestimation' treatment are smaller and insignificant. In particular, participants in the 'revenue allocation' treatment declare being willing to accept a 0.15 SDs higher carbon tax compared to those in the control group – an increase of almost €8 per tonne of CO_2. Overall, these results suggest that exposure to information describing the use of the resources collected thanks to the carbon tax positively influences support for such policies.

As shown in Figure 2, respondents exhibit pluralistic ignorance when asked to guess other respondents' maximum WTP for the carbon tax, underestimating the pro-environmental attitudes of others. To study whether the information treatment influenced not only stated preferences but also perceptions of others' preferences, we look at the effect of the treatments on the carbon tax WTP guessing game. Results are displayed in Table A3, following the same structure as the outcomes in Table 2. Individuals assigned to the 'revenue allocation' treatment think others are willing to accept a higher maximum carbon tax than those in the control group do. This increase is comparable in magnitude to that observed in column (6) of Table 2, suggesting that the information treatment did not affect the pluralistic ignorance gap.[8]

While stated preferences might be susceptible to experimenter demand effects, revealed preferences – based on actual behaviors – are considered to be more reliable. In Table 3, we ask whether the information treatment had any tangible impact on respondents' behaviors, by examining the likelihood of donating their actual and potential earnings from survey participation. The table follows again the same structure of Table 2, with the first three columns using the donation of base earnings to buy carbon credits as the outcome and the last three columns focusing on the share of potential lottery gains to donate. Table 3 confirms the findings coming from stated preferences: the treatment increased donations towards carbon credits, and the effect is mainly driven by individuals in the 'revenue allocation' treatment. Compared to the control group, individuals in the revenues group are 7.8 percentage points (pp) more likely to donate their base earnings. Additionally, if they win the €250 lottery, they agree to donate 4.7 pp more of their winnings to offset carbon emissions. Given that the average base earnings are approximately €25 (€10 for participation and an average gain of €15 from the guessing games), the coefficients in columns (3) and (6) translate into, respectively, a €1.95 per-capita increase in the donation of base earnings and a €11.75 increase in donations for each lottery winner. Donations for carbon offsets stemming from the assignment to the 'revenue allocation' treatment amount to €634.26, almost 10% of the total donations by all participants in wave 3 (€6,545.33).

Effect sizes are not only statistically, but also economically significant: when benchmarking the magnitudes of the 'revenue allocation' treatment against the marginal effect of the log of equivalized household income (included in the vector of individual controls, see Table A2 for the full list of

[8]The credibility of this result is reinforced by null results in a series of placebo tests, which are discussed in more detail in subsection 3.2.

coefficients), the point estimates in Table 2 and Table 3 are comparable to a 66% to 100% increase in average equivalized income.[9] In other words, the revenues information treatment is at least as effective in increasing pro-environmental attitudes and behaviors as a two-thirds increase in income would be.

3.2 Robustness checks

We perform a battery of robustness checks to test the reliability and consistency of our findings. We do so separately for outcomes capturing stated preferences in Table 4 and for revealed preferences in Table 5. Columns (1) and (5) of Table 4 and columns (1) and (4) of Table 5 show estimates for versions of the full model specification where observations are weighted with population-representative weights for age and gender. In all cases, the baseline results from Table 2 and Table 3 are confirmed, with the coefficients attached to the 'revenue allocation' treatment being slightly larger in the weighted sample. Next, in columns (2) and (6) of Table 4 and columns (2) and (5) of Table 5, we check whether our results hold when considering only the subsample of Luxembourgish residents. This is because we are eliciting support for a tax that is based in Luxembourg, so respondents from neighboring countries might not have the same stakes involved when asked to state their about their support for the tax. The effect of revenues on stated preferences in Table 4 is virtually unchanged in this smaller sample across all outcomes, while standard errors increase ubiquitously because of the loss of one-third of the total observations.

We then test whether results change when using the outcomes in different scales. Columns (3) and (4) of Table 4 show that the effect of the 'revenue allocation' treatment on support for the carbon tax is robust to, respectively, a dichotomization of the outcome (set to one for the "neutral/undecided", "somewhat favorable" or "strongly favorable", and zero otherwise) and the relaxation of the linearity assumption by using an ordered probit model. Similarly, in column (7), the effect of the revenues information treatment remains stable when using the natural logarithm of the WTP for the carbon tax, to account for its skewed distribution. For the outcomes in Table 5, estimating the 'revenue allocation' treatment effect on the donation of base earnings through a probit model yields similar marginal effects (column 3), as does dichotomizing lottery gains donations (column 6), with one indicating donations above the median (i.e., donating half of the potential gains or more) and zero otherwise.

Overall, results from Table 4 and Table 5 suggest that our baseline estimates are robust and likely reflect lower-bounds of the true causal effect of the revenues information treatment, since in most cases point estimates in the robustness tables are larger than those shown in the last columns of Table 2 and

[9] This is obtained from Table A2, by comparing the point estimates of the 'revenue allocation' treatment with the coefficient of the log of equivalized household income, rescaled to reflect a doubling in average income. The rescaling factor is equal to 0.693, i.e. the log of 2, due to the following equivalence $ln(\overline{Income} \times 2) - ln(\overline{Income}) = ln(2)$.

Table 3. We additionally perform placebo tests, to check that the treatment effects of the information provision are not spuriously driven by other factors. Figure A1 shows regression coefficients for the two treatments on a battery of pre-treatment outcomes. On the first two rows of the figure, we use the main four outcomes from Table 2 and Table 3, measured one wave prior to the administration of the information treatment in wave 3. Outcomes used in the last row, while measured in wave 3, are also measured prior to the information treatment: "Car ban" is a dummy for supporting the introduction of a ban to all cars in densely populated areas in Luxembourg, while "Car tax highway" is a dummy for supporting a €5 toll on Luxembourg's highways (i.e., €10 per day on a two-way commute). As expected, all coefficients in Figure A1 are small in size and not statistically different from zero.

While all regressions from our main model specification in Equation 1 control for a set of individual characteristics, there may still be some residual, unbalanced individual heterogeneity across groups that could partly confound our results, despite the randomized treatment assignment. The longitudinal nature of the dataset can help us address this concern. Figure A1 shows regression coefficients for the two treatments on a battery of pre-treatment outcomes that likely correlate with current carbon tax support and donations. On the first two rows of the figure, we use the main four outcomes from Table 2 and Table 3, measured one wave prior to the administration of the information treatment in wave 3. Outcomes used in the last row, while measured in wave 3, are also measured prior to the information treatment: "Car ban" is a dummy for supporting the introduction of a ban to all cars in densely populated areas in Luxembourg, while "Car tax highway" is a dummy for supporting a €5 toll on Luxembourg's highways (i.e., €10 per day on a two-way commute). As expected, all coefficients in Figure A1 are small in size and not statistically different from zero.[10]

In addition, we reshape the dataset longitudinally to perform a Difference-in-Differences (DiD) regression model with individual fixed effects. Table 6 and Table 7 compare the baseline results from Table 2 and Table 3 with longitudinal model specifications that use observations from the past two survey waves. In order to account for the serial correlation of errors over time within individual, standard errors are here clustered at the individual level. Stated preferences for the carbon tax were collected in two out of three survey waves (waves 2 and 3), while donations of the survey gains towards carbon credits are available for all three waves. In the DiD specification columns, the coefficients attached to the 'benefits underestimation' and the 'revenue allocation' treatments are no longer to be interpreted as estimates of the causal treatment effects, but rather reflect the contribution of unobservable cross-group time-invariant heterogeneity to the coefficients estimated in the wave 3 only (columns 1 and 4). The interaction terms between the treatment dummies and wave 3 now identify the treatment effects, net of individual time-invariant heterogeneity. The 'revenue allocation' treatment coefficient remains

[10]Note that Figure A1 could also be read as a falsification test, showing that the information treatments, as expected, did not impact any pre-treatment outcome.

statistically different from zero for three out of the four outcomes in Table 6 and Table 7, suggesting that unobserved time-invariant heterogeneity plays only a marginal role. In addition, the 'benefits underestimation' treatment effect becomes significant for lottery gains donations, suggesting that our baseline results underestimate respondents' behavioral response to the assignment to this treatment arm.

While the randomized experimental design provides internal validity to our estimates of the information treatments, we cannot exclude *a-priori* that the effects on stated preferences may reflect a general positive shift in subjective evaluations rather than a genuine increase in support for the carbon tax. To test this, we conduct placebo tests, analyzing responses to a set of subjective variables collected shortly after the information experiment, to ensure that our intervention did not broadly influence individuals' subjective evaluations. Figure A2 displays point estimates and confidence intervals for the 'revenue allocation' and 'benefits underestimation' treatment on the following variables: 'Altruistic', indicating participants' willingness to give to good causes without expecting anything in return; 'patient', reflecting their willingness to forego an immediate benefit for a larger future gain; 'revenge seeking', representing their likelihood of seeking revenge when treated unjustly; and 'chronic procrastinator', indicating the frequency with which they fail to complete tasks as planned.[11] None of the estimates is significantly different from zero, and all are small in magnitude, ranging from 0.8% to 6% of a SD. This confirms that the information treatments did not have unintended effects on other subjective variables.

3.3 Heterogeneity analysis and mechanisms

3.3.1 The role of individual characteristics

In order to test whether the effects of the information treatments on carbon tax support and donations differ across population subgroups, we conduct a heterogeneity analysis. In particular, we ask whether participants' predetermined characteristics, such as gender, education level, parity and place of residence, play a role in moderating their reaction to the the treatment. The results of this exercise are shown in Figure 3, which plots the estimated coefficients of the interaction terms between the two treatment statuses and the individual characteristics mentioned above.

Figure 3 shows that results in Table 2 and Table 3 are the same regardless of gender, education level (i.e. post-secondary education), and having at least one child. However, when looking at treatment effect heterogeneity by place of residence (urban vs rural), the positive impact of the 'revenue allocation' treatment on both carbon-tax support and the willingness to pay for the carbon tax is only found for

[11] The first three variables are collected on 0-10 Likert scales, while the latter on a 1-4 Likert scale. All four are here standardized to have mean zero and standard deviation one.

respondents living in an urban area, suggesting that mobility constraints and other characteristics linked to geographical sorting in rural areas might make respondents less reactive to the information treatments. Qualitatively similar results, albeit only marginally significant, are found also for the two donation outcomes.

To directly test for the role of mobility constraints, we explore the intensity of car usage, assuming, as previous studies suggest, that individuals who drive more are less supportive of the carbon tax or have a lower willingness to pay. To examine this, we rely on a 10-levels categorical variable capturing self-reported kilometers (km) driven per year to build a 'high-distance' dummy, equal to one for driving at least as much as the median driver and zero otherwise.[12] As expected, results in Table A4 show negative point estimates for the interactions between the information treatments and the high-distance dummy – that are larger for stated support than donations. In addition, the table shows that the 'benefits underestimation' treatment significantly increases support for the carbon tax among those who drive less than the median.

On top of mobility constraints, financial constraints could also play an important role in moderating responses to the information treatments. In order to test this, we interact the treatment status dummies with the natural logarithm of equivalized household disposable income. Results in Table A5 show that stated support for the carbon tax increases with income for both treatments, while donations are less reactive to it.

3.3.2 The role of priors

As shown in Appendix B:, participants exposed to the information treatments were provided with short texts to read and, in relation to these, some questions aimed at capturing whether or not they already knew or expected the information provided to them. We exploit these as a measure of treatment intensity, similar to the literature on the use of priors in information experiments (Haaland *et al.*, 2023). In Table 8, we disentangle each treatment into the different response categories given to the knowledge and expectation questions, both when the dependent is the support for the carbon tax and when it is the WTP for it. For the 'benefits underestimation' treatment, participants were asked whether they knew that support for climate taxes increases over time, with potential answers being "No, I would have thought that the support would have **decreased** after the tax implementation", "No, I would have thought that the support would have **remained the same** after the tax implementation", "No, I did not know but it makes sense to me", and "Yes, I knew that population support for climate taxes

[12]The median yearly driving distance in the sample falls in the 15,000 to 20,000 km-per-year interval, above the European average annual distance of 12,540 km in 2022 (source: `https://www.acea.auto/files/ACEA-Report-Vehicles-on-European-roads`, last accessed: 30 October 2024). This is not surprising, since Luxembourg has had the highest motorization rate among EU countries for 30 years until 2021, with 681 passenger cars per 1,000 inhabitants in 2019 (Eurostat, 2021).

increases after their implementation". For the 'revenue allocation' treatment, after being informed about the average yearly cost of the carbon tax to households, participants were first asked whether they expected the cost to be "Much smaller", "Slightly smaller", "About this amount", "Slightly larger" or "Much larger". Then, once informed about the use of the tax revenues from the carbon tax in Luxembourg, they were asked whether they knew about their allocation to redistributive purposes and to investments in sustainable projects.

While we cannot exclude that responses to these questions were at least in part affected by the treatment itself, they likely correlate with individuals' priors and can be informative on the intensity of the information treatments. The top half of Table 8 shows that the null average effect of the 'benefits underestimation' treatment hides substantial heterogeneity in responses by prior. Individuals who expected a decrease in support for carbon taxes (the opposite of the information provided in the 'benefits underestimation' treatment) react negatively to the information provision, indicating a decreasing level of support for the carbon tax if the information provided is not matching their expectations. On the contrary, individuals either anticipated or knew about the information provided display a positive and significant coefficient, that shows an increasing support towards the carbon tax after the information treatment. Results are qualitatively similar, albeit hardly significant, for the WTP for the carbon tax in the last three columns of Table 8 and for the donation outcomes in Table A7. These results seem to indicate that the null effect of the 'benefits underestimation' treatment found in Table 2 is actually an average of negative and positive effects driven by differences in the expectations about the public support for climate taxes and for the willingness to pay for it.

The table also suggests that the information treatment backfires for the group whose priors go in a direction opposite to the information received. When looking at the characteristics of this group in column (2) of Table A6, we can see that they display on average lower levels of trust, both compared to the control group and compared to the remaining participants assigned to their same treatment arm. In addition, they are significantly less likely to believe that climate change is real, or that it is due to human activities, and have lower levels of trust in the rigor and validity of climate sciences.

Moving onto the bottom half of Table 8, we first look at the prior on the cost of the carbon tax for households in a continuous fashion, while still controlling for being assigned to the 'revenue allocation' treatment (columns 1 and 4); we then disentangle the 'revenue allocation' treatment into four categories of knowledge about the allocation of carbon-tax revenues (columns 2 and 4), and last we control for both priors together (columns 3 and 6). Column 1 suggests that support for the carbon tax is increasing in the difference between the average cost of the carbon tax for households and participants' expectations about it.[13] Perhaps more interestingly, column 2 shows that only those who did not know about any

[13] In a different model specification, we included priors on the cost for households as a set of dummies for each category,

of the uses of the carbon-tax revenues and those who only knew about the financing of sustainable projects react to the information treatment, by increasing their support for the carbon tax. While we cannot reject the null hypothesis of equality of the coefficients of category 2 'Only knew about redistribution' and category 3 'Only knew about sustainable projects', this suggests that the 'revenue allocation' treatment effect is mostly driven by informing individuals about the redistributive purposes of the tax revenues, in line with the literature showing higher support for climate taxes that are linked to fairness (Sumner *et al.*, 2011; Jagers *et al.*, 2019; Maestre-Andrés *et al.*, 2021). When controlling for both the priors on the cost for households and priors on the use of revenues linked to the carbon tax, none of the coefficients is no longer different from zero at conventional thresholds, complicit the high cross-correlation between the two kinds of priors (see Figure A4 for the joint distribution of priors in the 'revenue allocation' treatment). Results in Table A7 show a similar pattern holding for priors on the use of carbon-tax revenues and revealed preferences, as measured by the donation outcomes. Here again, most of the increase in donations in the 'revenue allocation' treatment comes from participants who did not know about the redistributive purposes of the carbon tax revenues.

3.3.3 The role of past attitudes and behaviors

One may wonder whether the information treatments are complements or substitutes to individuals' preexisting environmental attitudes and behaviors. In other words, does the information reinforce support for the carbon tax among those who were already more supportive, or does it shift preferences and behaviors among those with initially low levels of support?

Leveraging the longitudinal nature of our data, we are able to address this question in Figure 4, which look at treatment effect heterogeneity by past levels of the outcome variables. For the two continuous outcomes (WTP for the carbon tax and the share of lottery gains donations), respectively in panels (b) and (d) in the Figure, we aggregate their past values in quintiles or quartiles.[14] Panel (a) of Figure 4 shows that support for the carbon tax increased the most in response to the 'revenue allocation' treatment among those who were either moderately opposed or neutral in wave 2. In this case, the information treatment acted as a substitute for preexisting carbon-tax support, shifting the stated preferences of individuals with moderate to low initial support. For the remaining outcomes, shown in panels (b) to (d), only the groups at the extremes of the distributions seem to respond positively to the 'revenue allocation' treatment, with those showing high levels of support or donations reacting disproportionately more. This suggests that, while the treatment substituted itself to poor preexisting

rather than treating it as a continuous variables. Results from this alternative specification (available upon request) show that the increased support for the carbon tax is driven by all participants in the revenues group, except those who expected the cost for households to be much smaller than the actual amount.

[14] We avoided using quintiles for the share of lottery gains donation because the bottom 40% of its distribution consists entirely of zeros, resulting in no variation across the bottom two quintiles. Similarly, using quartiles for WTP would dilute the key dynamics concentrated in the right tail.

attitudes and behaviors, leading to small improvements, its larger effects operated by complementing the initial attitudes of those already scoring high on the outcomes distributions. In contrast, no systematic pattern of complementarity or substitutability emerges for the ‘benefits underestimation’ treatment.

4 Conclusions

Carbon pricing remains a central and cost-effective policy tool in the global effort to reduce greenhouse gas (GHG) emissions. However, public acceptance, crucial for the successful implementation of such policies, often hinges on perceptions of fairness and the transparency of tax revenue use. Our study leverages data from a longitudinal survey conducted in Luxembourg from November 2022 to August 2023 to assess public support for Luxembourg’s carbon tax and to examine how informational interventions can shift public perceptions of the policy. Using an experimental design, we randomly assigned participants into a control group and two treatment groups, with only the latter receiving targeted information, respectively, about the carbon tax revenue allocation and about the larger ex-post public support for climate taxes.

By examining Luxembourg’s implemented carbon tax, this research bridges the gap between hypothetical and real-world studies of carbon pricing, providing practical insights on public response to an actual policy with defined revenue allocation. Our findings reveal that providing information on the use of carbon tax revenues not only enhances individuals’ support and willingness to pay for the carbon tax, but also increases actual monetary donations to purchase carbon credits. This result underscore the importance of transparency about revenue uses in increasing support for carbon taxes: informing the public about redistributive outcomes significantly raises both stated and revealed preferences for the tax, suggesting that fairness perceptions are pivotal not only to policy acceptability, but also to engage in individual contributions to mitigate carbon emissions. This suggests that by stimulating fairness and perceived coherence of climate action, the treatment stimulates both passive (policy support) and active (carbon offsetting donations) responses.

The observed heterogeneity in responses further emphasizes that individual factors—such as financial constraints, beliefs about climate change, and levels of institutional trust—play a substantial role in shaping tax acceptability. Individuals from higher-income, urban areas with limited reliance on cars were notably more responsive to information about revenue use, suggesting that contextual factors like the feasibility of low-carbon alternatives and sociodemographic traits influence support. Additionally, the study’s unique focus on actual behavior through revealed preferences, rather than solely stated preferences, contributes a new dimension to the literature by minimizing social desirability bias and illustrating the tangible behavioral impacts of transparency about tax revenues.

While the 'revenue allocation' information treatment led to a measurable increase in support, the benefits underestimation' treatment yielded mixed effects, with responses varying based on respondents' prior beliefs. This suggests that information addressing perceived economic benefits may backfire among individuals with skepticism toward climate policy or low trust in climate science. These findings highlight the complexity of addressing confirmation bias in public policy communication, as belief-aligned information is more likely to foster positive responses than general information about tax benefits.

These findings contribute to ongoing debates around climate policy acceptability by highlighting how policy design – including informational transparency and compensatory mechanisms – can influence public perception and willingness to contribute financially. Given the ambitious GHG reduction goals set by the European Environmental Agency, such insights are vital for policymakers seeking to design equitable and effective climate policies that maintain public trust and support. This study contributes to the policy debate by illustrating that well-communicated revenue allocation, coupled with an awareness of individual differences, is essential for the success of climate-related fiscal policies. A simple, untargeted information campaign on the revenues use of the carbon tax can increase willingness to pay for the carbon tax by €8 per tonne of CO2 on average – larger than 1.5 times the €5 yearly tax increases planned by the Luxembourgish government. Future research could further explore these dynamics across different policy contexts and scales, offering a refined understanding of how to balance effectiveness with public acceptance in the pursuit of sustainable, equitable climate action.

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Figures and Tables

Figure 1: Distribution of support for the carbon tax

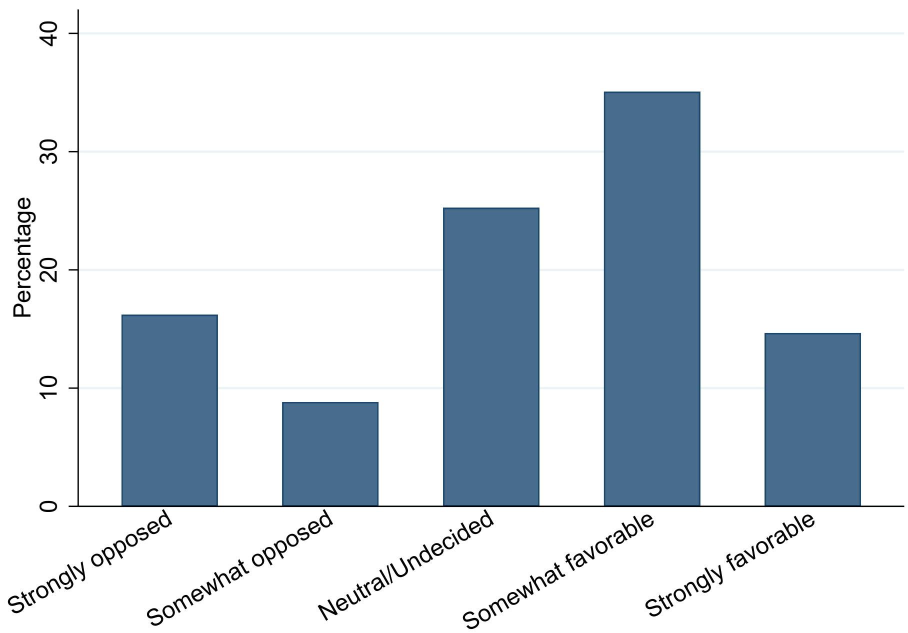
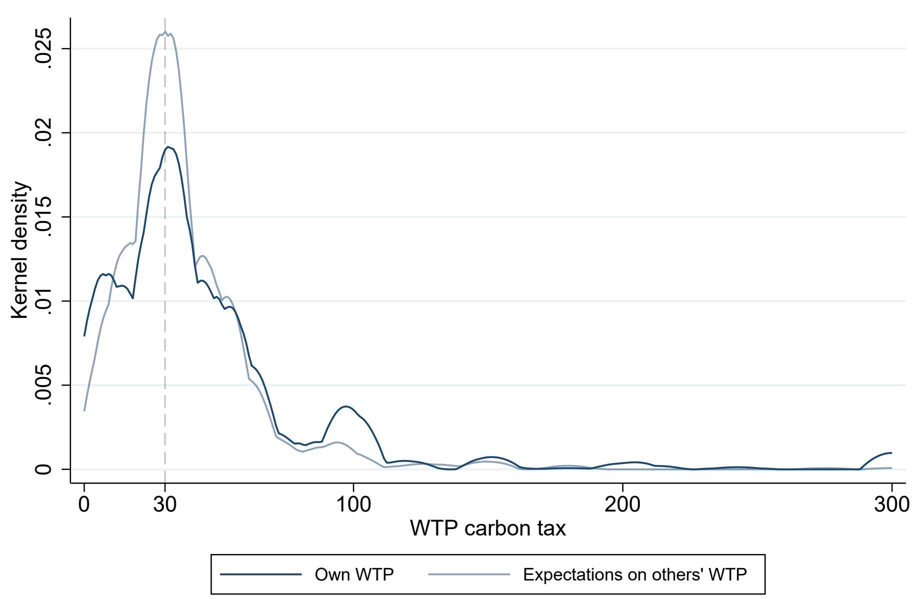
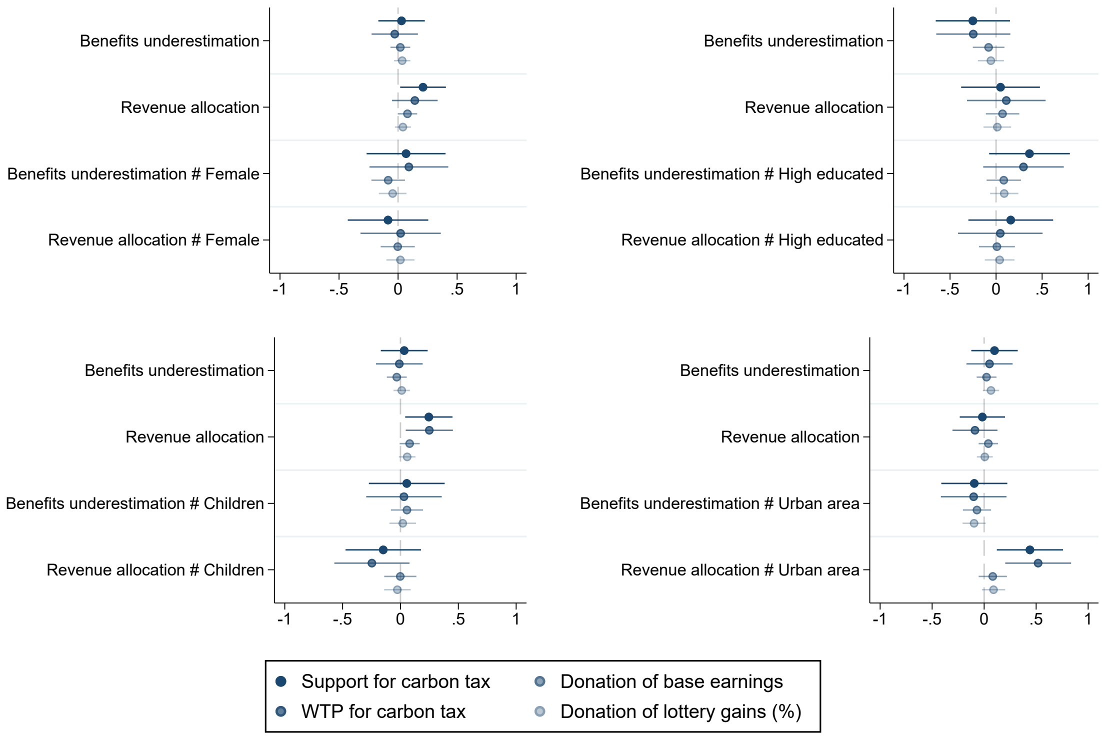


Figure 2: Distribution of own and expectations on others' WTP for the carbon tax



Note: The x-axis is expressed in euros per tonne of CO2. Kernel densities are computed based on an Epanechnikov kernel function for all individuals in the estimation sample. The dashed vertical line indicates the 2023 level of the carbon tax in Luxembourg, equivalent to €30 per tonne of CO2.

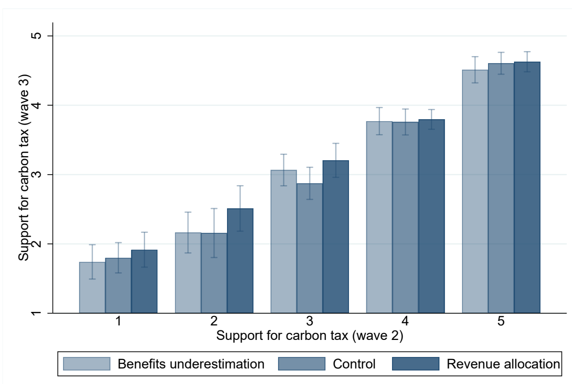
Figure 3: Heterogeneity by individual characteristics



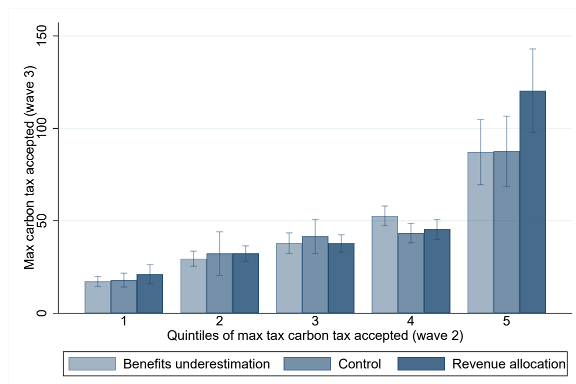
Note: Dots are regression coefficients from augmented versions of Equation 1, where the treatment status dummies are interacted with, respectively, dummies for gender, children, post-secondary education and living in an urban area. Different colors correspond to different outcomes, as indicated in the legend. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Horizontal lines are for 95% confidence intervals.

Figure 4: Average stated and revealed preferences, by treatment status and wave 2 levels

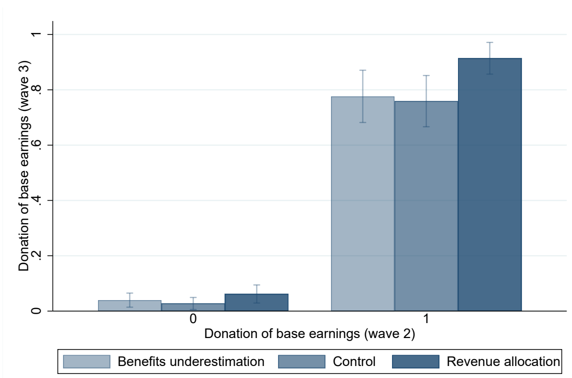
(a) Support for carbon tax



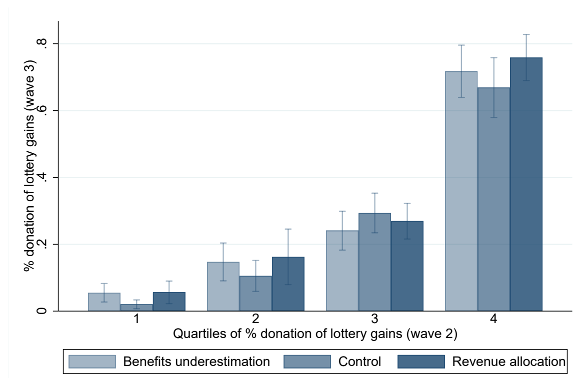
(b) WTP carbon tax



(c) Donation of base earnings



(d) Donation of lottery gains



Note: The four outcomes are indicated in panels (a) to (d). Bars in each subgraph plot the wave-3 average of one of the outcomes, by the level of the same outcome measured in wave 2. Different colors refer to different wave-3 treatment status. Vertical lines are for 95% confidence intervals.

Table 1: Summary statistics

	Mean	Std. Dev.	Min	Max
Outcomes				
Support for carbon tax	3.232	1.274	1	5
WTP for carbon tax	42.677	52.763	0	300
Others' WTP for carbon tax	33.922	32.278	0	210
Donation of base earnings	0.259	.	0	1
Donation of lottery gains (%)	0.269	0.357	0	1
Controls				
Household size	2.753	1.319	1	6
Equivalized income (log)	8.345	0.451	6	10
Female	0.338	.	0	1
Post-secondary education	0.852	.	0	1
Urban area	0.479	.	0	1
Household with children	0.394	.	0	1
Homeownership	0.808	.	0	1
Age				
18-25	0.019	.	0	1
26-35	0.162	.	0	1
36-50	0.429	.	0	1
51-65	0.319	.	0	1
>65	0.072	.	0	1
Employment status				
Employed	0.798	.	0	1
Unemployed	0.018	.	0	1
Retired/pensioner	0.029	.	0	1
Other	0.155	.	0	1
Nationality				
Luxembourgish	0.344	.	0	1
EU migrant	0.602	.	0	1
Non-EU migrant	0.054	.	0	1
Residence				
Resident in Luxembourg	0.668	.	0	1
Cross-border commuter	0.322	.	0	1
Other	0.010	.	0	1

Notes: descriptive statistics are based on the estimation sample of 907 observations.

Table 2: Main results: the effect of the information treatments on stated preferences

	Support for carbon tax			WTP for carbon tax		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.132*			0.087		
	(0.070)			(0.070)		
Benefits underestimation		0.059	0.055		0.005	0.004
		(0.081)	(0.081)		(0.081)	(0.081)
Revenue allocation		0.204**	0.184**		0.170**	0.149*
		(0.081)	(0.081)		(0.081)	(0.081)
Individual controls	.	.	Yes	.	.	Yes
Observations	907	907	907	907	907	907
Adjusted R-squared	0.003	0.005	0.023	0.001	0.004	0.029

Notes: Standard errors in parentheses * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area.

Table 3: Main results: the effect of the information treatments on revealed preferences

	Donation of base earnings			Donation of lottery gains (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	0.048			0.045*		
	(0.031)			(0.025)		
Benefits underestimation		0.002	-0.010		0.031	0.019
		(0.036)	(0.035)		(0.029)	(0.028)
Revenue allocation		0.093***	0.078**		0.059**	0.047*
		(0.036)	(0.035)		(0.029)	(0.028)
Individual controls	.	.	Yes	.	.	Yes
Observations	907	907	907	907	907	907
Adjusted R-squared	0.000	0.005	0.091	0.002	0.002	0.069

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. All regressions additionally control for treatment assignment dummies in subsequent, independent information experiments.

Table 4: Robustness checks: Stated preferences

	Support for carbon tax				WTP for carbon tax		
	Weights	Residents	Dummy	Ordered Probit	Weights	Residents	Log
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Benefits underestimation	0.076	-0.010	0.031	0.053	-0.010	-0.080	0.076
	(0.080)	(0.101)	(0.035)	(0.087)	(0.080)	(0.108)	(0.097)
Revenue allocation	0.194**	0.200**	0.094***	0.180**	0.240***	0.128	0.132*
	(0.082)	(0.101)	(0.035)	(0.087)	(0.082)	(0.107)	(0.075)
Observations	907	606	907	907	907	606	848
Adjusted R-squared	0.029	0.022	0.018	.	0.063	0.032	0.036

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Columns 1 and 5 weight observations to be representative of the age and gender composition of the Luxembourgish population. Columns 2 and 6 restrict the sample to residents only. In column 3 the dependent variable is coded as a dummy for not opposing the carbon tax, while column 4 reports coefficients from an ordered probit version of the baseline model. Last, in column 7 the outcome is log-transformed (zeroes are dropped from the estimation sample).

Table 5: Robustness checks: Revealed preferences

	Donation of base earnings			Donation of lottery gains (%)		
	Weights	Residents	Probit	Weights	Residents	Dummy
	(1)	(2)	(3)	(4)	(5)	(6)
Benefits underestimation	-0.044	-0.042	-0.011	0.008	-0.011	0.029
	(0.034)	(0.044)	(0.033)	(0.028)	(0.037)	(0.035)
Revenue allocation	0.082**	0.067	0.078**	0.071**	0.051	0.067*
	(0.035)	(0.044)	(0.035)	(0.028)	(0.036)	(0.035)
Observations	907	606	907	907	606	907
Adjusted R-squared	0.160	0.105	.	0.155	0.087	0.060

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. All regressions additionally control for treatment assignment dummies in subsequent, independent information experiments. Columns 1 and 4 weight observations to be representative of the age and gender composition of the Luxembourgish population. Columns 2 and 5 restrict the sample to residents only. Column 3 shows marginal effects from estimating a non-linear version of the baseline model via a probit. Last, in column 6 the dependent is coded as a dummy for donating more than 50% of the potential lottery gain (50% is the median donation).

Table 6: DiD Estimation: Stated preferences

	Support for carbon tax			WTP for carbon tax		
	Baseline	DiD	DiD FE	Baseline	DiD	DiD FE
	(1)	(2)	(3)	(4)	(5)	(6)
Benefits underestimation	0.055	0.035		0.004	0.045	
	(0.084)	(0.074)		(0.072)	(0.092)	
Revenue allocation	0.184**	0.064		0.149*	0.029	
	(0.080)	(0.077)		(0.087)	(0.092)	
Benefits underestimation × Wave 3		0.021	0.021		-0.038	-0.038
		(0.066)	(0.066)		(0.080)	(0.079)
Revenue allocation × Wave 3		0.125*	0.125*		0.124	0.124
		(0.064)	(0.064)		(0.078)	(0.078)
Individual FE	.	.	Yes	.	.	Yes
Observations	907	1814	1814	907	1814	1814
Adjusted R-squared	0.023	0.031	0.003	0.029	0.028	0.005

Notes: Robust standard errors, clustered at the individual level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Outcomes are standardized using their mean and standard deviation in wave 3, for comparability with the baseline estimates in columns (1) and (4). All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. The DiD columns further control for survey wave fixed effects. Adjusted R-squared values reported in columns (3) and (6) reflect the adjusted within R-squared.

Table 7: DiD Estimation: Revealed preferences

	Donation of base earnings			Donation of lottery gains (%)		
	Baseline	DiD	DiD FE	Baseline	DiD	DiD FE
	(1)	(2)	(3)	(4)	(5)	(6)
Benefits underestimation	-0.010	-0.006		0.019	-0.015	
	(0.033)	(0.032)		(0.028)	(0.025)	
Revenue allocation	0.078**	0.039		0.047	0.018	
	(0.035)	(0.033)		(0.029)	(0.027)	
Benefits underestimation × Wave 3		-0.006	-0.006		0.035**	0.035**
		(0.022)	(0.022)		(0.017)	(0.017)
Revenue allocation × Wave 3		0.038*	0.038*		0.032*	0.032*
		(0.021)	(0.021)		(0.017)	(0.017)
Individual FE	.	.	Yes	.	.	Yes
Observations	907	2721	2721	907	2721	2721
Adjusted R-squared	0.091	0.101	0.006	0.069	0.079	0.008

Notes: Robust standard errors, clustered at the individual level, in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. All regressions additionally control for treatment assignment dummies in subsequent, independent information experiments. The DiD columns further control for survey wave fixed effects. Adjusted R-squared values reported in columns (3) and (6) reflect the adjusted within R-squared.

Table 8: The effect of the information treatments on stated preferences, by priors

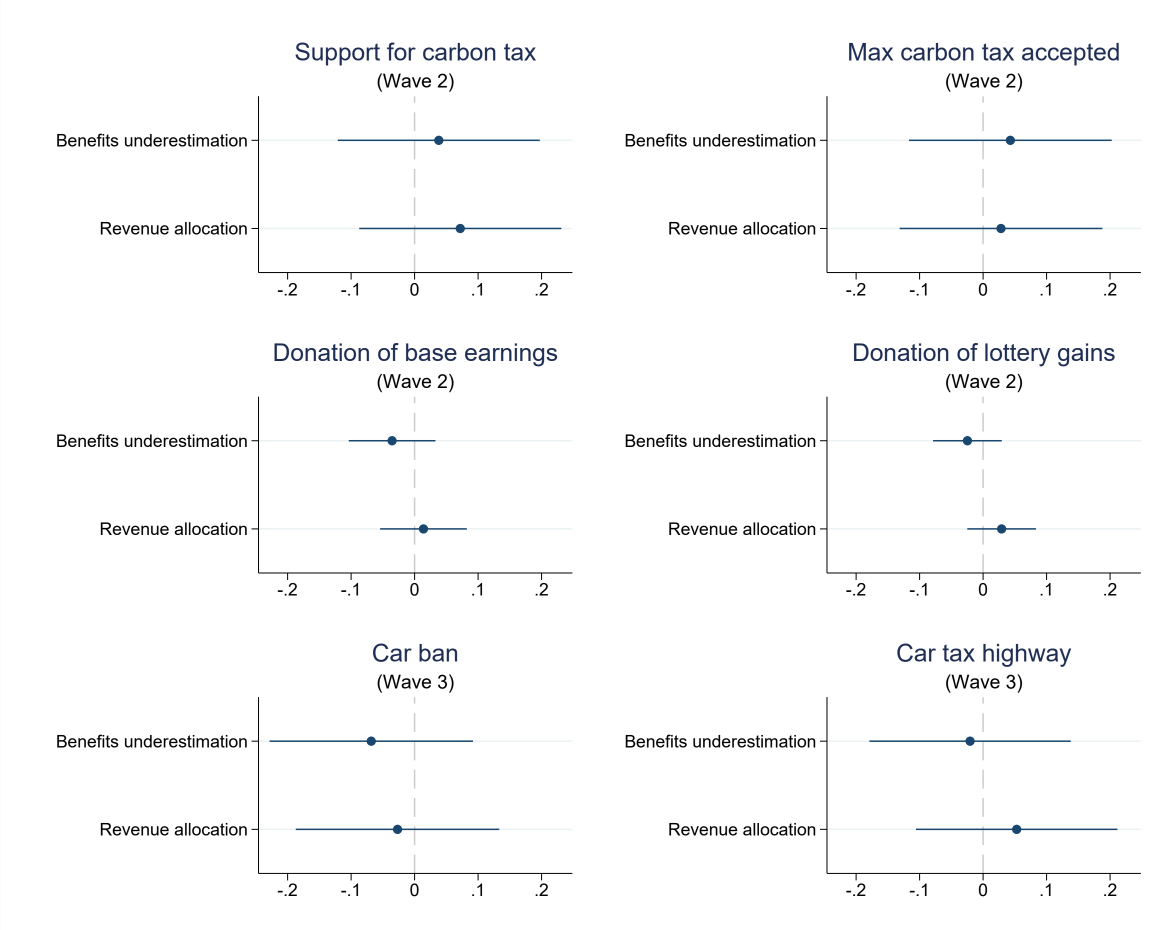
	Support for carbon tax			WTP for carbon tax		
	(1)	(2)	(3)	(4)	(5)	(6)
Benefits underestimation						
Priors:						
Expected a decrease in support	-0.351**	-0.351**	-0.350**	-0.217	-0.219	-0.219
	(0.139)	(0.139)	(0.139)	(0.140)	(0.140)	(0.140)
Expected same support	-0.107	-0.108	-0.107	-0.063	-0.064	-0.065
	(0.124)	(0.124)	(0.124)	(0.125)	(0.124)	(0.124)
Did not know, but it makes sense	0.298***	0.299***	0.299***	0.147	0.146	0.145
	(0.103)	(0.103)	(0.103)	(0.104)	(0.104)	(0.104)
I knew	0.262	0.261	0.263	0.000	-0.000	-0.001
	(0.195)	(0.195)	(0.195)	(0.196)	(0.196)	(0.196)
Revenue allocation						
Prior on carbon-tax cost for households:						
1-5 Likert scale	0.061		0.058	-0.001		-0.015
	(0.050)		(0.050)	(0.050)		(0.050)
Prior on use of carbon-tax revenues:						
Didn't know at all		0.177*	0.024		0.046	0.085
		(0.102)	(0.167)		(0.103)	(0.168)
Only knew about redistribution		0.174	0.002		0.348**	0.391*
		(0.140)	(0.204)		(0.141)	(0.206)
Only knew about sustainable projects		0.334**	0.160		0.231	0.275
		(0.140)	(0.206)		(0.141)	(0.207)
Knew about both uses		0.042	-0.122		0.094	0.135
		(0.156)	(0.210)		(0.157)	(0.211)
Observations	907	907	907	907	907	907
Adjusted R-squared	0.044	0.043	0.043	0.031	0.033	0.032

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Columns 1 and 4 additionally control for the 'revenue allocation' treatment dummy.

Online Appendix

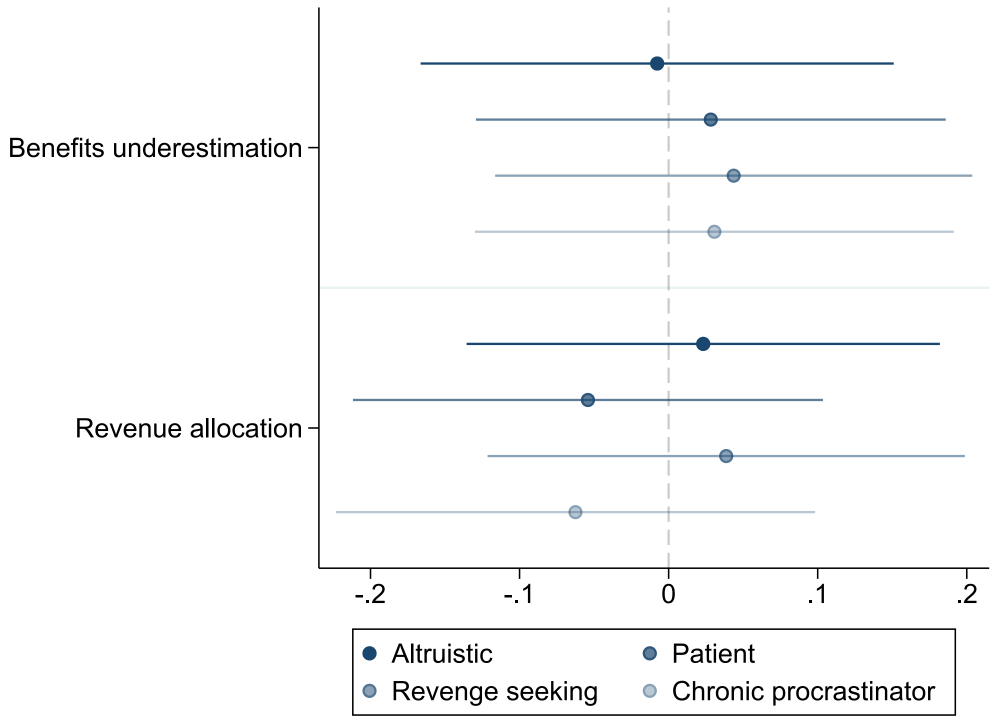
Appendix A: Additional figures and tables

Figure A1: Balance of pre-treatment outcomes



Note: Dots are regression coefficients of the treatment dummies, coming from versions of Equation 1 that use pre-treatment variables as outcomes. Outcomes used in the first two rows are defined as the same four main outcomes used in Table 2 and Table 3, but are measured one wave prior to the administration of the information treatment. Outcomes used in the last row, while measured in wave 3, are also measured prior to the information treatment. "Car ban" is a dummy for supporting the introduction of a ban to all cars in densely populated areas in Luxembourg; "Car tax highway" is a dummy for supporting a €5 toll every time one uses Luxembourg's highways (i.e., €10 per day on a two-way commute). All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Horizontal lines are for 95% confidence intervals.

Figure A2: Placebo tests on post-treatment outcomes



Note: Dots are regression coefficients of the treatment dummies, coming from versions of Equation 1 that use unrelated post-treatment variables as outcomes, each indicated in the legend. All outcomes are standardized. 'Altruistic' reflects participants' willingness to give to good causes without expecting anything in return; 'patient' reflects their willingness to give up something beneficial today to benefit more from it in the future; 'revenge seeking' reflects how likely they are to seek revenge when treated unjustly; and 'chronic procrastinator' indicates the frequency they are unable to complete tasked when planned. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Horizontal lines are for 95% confidence intervals.

Figure A3: Distribution of priors in the ‘benefits underestimation’ treatment

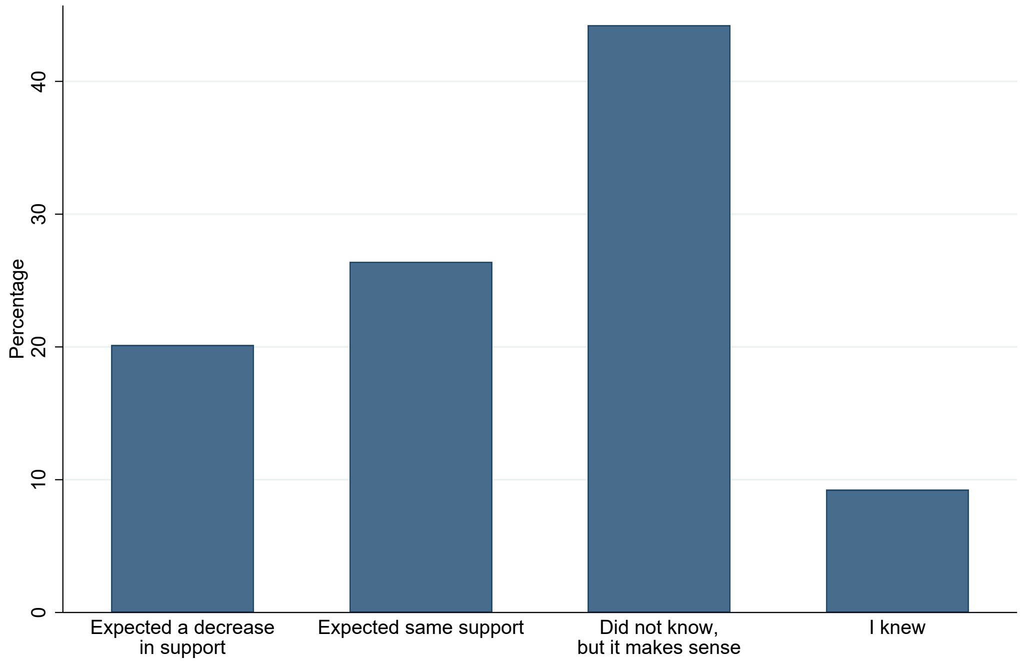
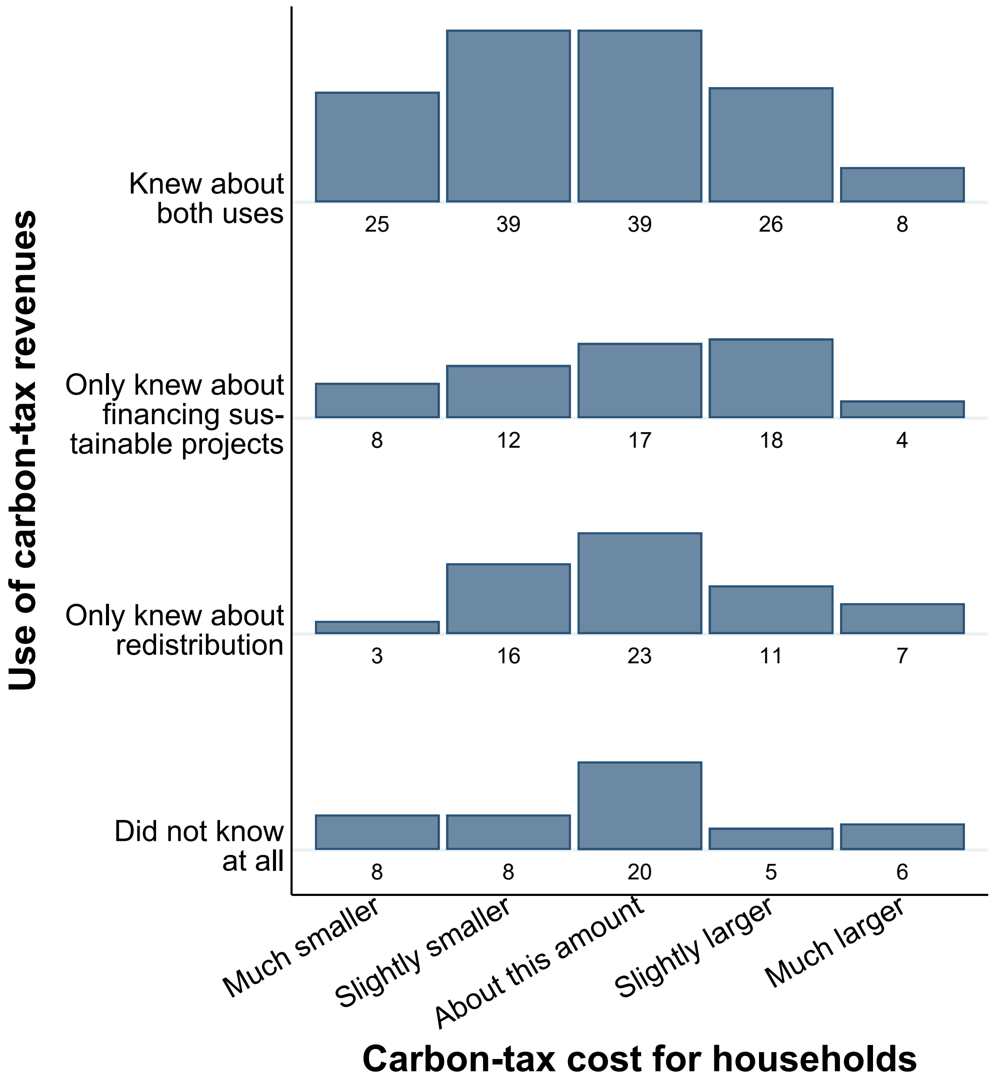


Figure A4: Joint distribution of priors in the ‘revenue allocation’ treatment



Note: Bars and numbers in the figure indicate the joint frequency of respondents in the ‘revenue allocation’ group, based on priors on the use of carbon tax revenues and priors on the average cost of the carbon tax for households.

Table A1: Balance of covariates between treatment and control groups

	Control (1)	Benefits Underest. (2)	Revenues (3)	(1)-(2)	(1)-(3)
Household Size	2.650	2.763	2.823	-0.113	-0.173
	[1.333]	[1.344]	[1.280]	(0.109)	(0.106)
Equivalized income (log)	8.322	8.330	8.373	-0.007	-0.051
	[0.449]	[0.442]	[0.476]	(0.036)	(0.038)
Female	0.329	0.366	0.320	-0.037	0.009
	[0.471]	[0.483]	[0.467]	(0.039)	(0.038)
Post-secondary education	0.855	0.836	0.869	0.019	-0.014
	[0.353]	[0.371]	[0.338]	(0.029)	(0.028)
Urban area	0.505	0.497	0.439	0.008	0.066
	[0.501]	[0.501]	[0.497]	(0.041)	(0.040)
Household with children	0.366	0.398	0.413	-0.032	-0.047
	[0.483]	[0.490]	[0.493]	(0.039)	(0.040)
Homeownership	0.782	0.813	0.823	-0.030	-0.041
	[0.413]	[0.391]	[0.382]	(0.033)	(0.032)
Age					
18-25	0.020	0.023	0.013	-0.003	0.007
	[0.140]	[0.150]	[0.114]	(0.012)	(0.010)
26-35	0.168	0.178	0.144	-0.009	0.024
	[0.375]	[0.383]	[0.352]	(0.031)	(0.029)
36-50	0.426	0.414	0.446	0.011	-0.020
	[0.495]	[0.493]	[0.498]	(0.040)	(0.040)
51-65	0.323	0.306	0.325	0.018	-0.001
	[0.469]	[0.462]	[0.469]	(0.038)	(0.038)
>65	0.063	0.079	0.072	-0.016	-0.009
	[0.243]	[0.270]	[0.259]	(0.021)	(0.020)
Employment status					
Employed	0.825	0.776	0.793	0.049	0.032
	[0.381]	[0.417]	[0.406]	(0.032)	(0.032)
Unemployed	0.026	0.010	0.016	0.017	0.010
	[0.161]	[0.099]	[0.127]	(0.011)	(0.012)
Retired/pensioner	0.017	0.033	0.036	-0.016	-0.020
	[0.128]	[0.179]	[0.187]	(0.013)	(0.013)
Other	0.132	0.181	0.154	-0.049	-0.022
	[0.339]	[0.386]	[0.362]	(0.029)	(0.028)
Nationality					
Luxembourgish	0.327	0.336	0.367	-0.009	-0.040
	[0.470]	[0.473]	[0.483]	(0.038)	(0.039)
EU migrant	0.627	0.599	0.584	0.028	0.043
	[0.484]	[0.491]	[0.494]	(0.040)	(0.040)
Non-EU migrant	0.046	0.066	0.049	-0.020	-0.003
	[0.210]	[0.248]	[0.217]	(0.019)	(0.017)
Residence					
Resident in Luxembourg	0.637	0.684	0.689	-0.047	-0.052
	[0.482]	[0.466]	[0.464]	(0.038)	(0.038)
Cross-border commuter	0.356	0.303	0.302	0.054	0.055
	[0.480]	[0.460]	[0.460]	(0.038)	(0.038)
Other	0.007	0.013	0.010	-0.007	-0.003
	[0.081]	[0.114]	[0.099]	(0.008)	(0.007)
Observations	303	304	305	607	608

Table A2: The effect of the information treatments on all outcomes, with all controls

	Support for carbon tax (1)	WTP carbon tax (2)	Donation of base earnings (3)	Donation of lottery gains (%) (4)
Benefits underestimation	0.055	0.004	-0.010	0.019
	(0.081)	(0.081)	(0.035)	(0.028)
Revenue allocation	0.184**	0.149*	0.078**	0.047*
	(0.081)	(0.081)	(0.035)	(0.028)
Household size	0.039	0.089**	-0.006	0.005
	(0.036)	(0.036)	(0.015)	(0.013)
Equivalized income (log)	0.257***	0.327***	0.163***	0.081***
	(0.081)	(0.081)	(0.035)	(0.028)
Female	0.010	-0.105	0.041	0.057**
	(0.073)	(0.072)	(0.031)	(0.026)
Post-secondary education	0.090	0.062	0.024	0.009
	(0.100)	(0.099)	(0.042)	(0.035)
Urban area	0.114	0.129*	0.030	0.029
	(0.071)	(0.071)	(0.030)	(0.025)
Household with children	0.104	-0.086	0.021	-0.008
	(0.105)	(0.104)	(0.044)	(0.037)
Homeownership	-0.141	-0.091	-0.050	0.000
	(0.095)	(0.095)	(0.040)	(0.033)
Age				
26-35	-0.091	0.046	0.027	0.107
	(0.279)	(0.278)	(0.118)	(0.098)
36-50	0.043	0.027	0.060	0.144
	(0.274)	(0.273)	(0.116)	(0.096)
51-65	0.116	-0.110	0.165	0.228**
	(0.275)	(0.274)	(0.116)	(0.096)
>65	0.010	-0.063	0.273**	0.321***
	(0.323)	(0.322)	(0.137)	(0.113)
Employment status				
Unemployed	0.166	0.190	0.027	-0.084
	(0.265)	(0.264)	(0.113)	(0.093)
Retired/pensioner	0.399*	0.267	0.188**	0.172**
	(0.223)	(0.222)	(0.095)	(0.078)
Other	0.046	0.060	0.143**	0.080*
	(0.134)	(0.134)	(0.057)	(0.047)
Nationality				
EU migrant	0.098	0.141	0.003	0.017
	(0.088)	(0.087)	(0.037)	(0.031)
Non-EU migrant	-0.050	-0.156	-0.118*	-0.069
	(0.162)	(0.161)	(0.069)	(0.057)
Residence				
Cross-border commuter	-0.012	-0.049	-0.012	-0.002
	(0.087)	(0.087)	(0.037)	(0.030)
Other	0.517	-0.040	-0.003	-0.099
	(0.344)	(0.343)	(0.147)	(0.121)
Constant	-2.504***	-3.052***	-1.200***	-0.622**
	(0.721)	(0.719)	(0.309)	(0.255)
Observations	907	907	907	907
Adjusted R-squared	0.023	0.029	0.091	0.069

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Columns (3) and (4) additionally control for treatment assignment dummies in subsequent, independent information experiments (not shown in the Table).

Table A3: The effect of the information treatments on incentivized guesses on others' WTP for the carbon tax

	(1)	(2)	(3)
Treated	0.106		
	(0.070)		
Benefits underestimation		0.069	0.065
		(0.081)	(0.082)
Revenue allocation		0.143*	0.138*
		(0.081)	(0.082)
Individual controls	.	.	Yes
Observations	907	907	907
Adjusted R-squared	0.001	0.001	0.005

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Individual controls are equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area.

Table A4: Treatment effect heterogeneity by driving intensity

	Support for carbon tax (1)	WTP carbon tax (2)	Donation of base earnings (3)	Donation of lottery gains (%) (4)
Benefits underestimation	0.252**	0.037	-0.006	-0.027
	(0.116)	(0.116)	(0.050)	(0.041)
Revenue allocation	0.319***	0.272**	0.110**	0.039
	(0.118)	(0.118)	(0.050)	(0.041)
Benefits underestimation × High distance	-0.401**	-0.061	-0.006	0.090
	(0.163)	(0.163)	(0.070)	(0.058)
Revenue allocation × High distance	-0.255	-0.235	-0.062	0.014
	(0.163)	(0.163)	(0.069)	(0.057)
Observations	907	907	907	907
Adjusted R-squared	0.031	0.029	0.089	0.070

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. 'High distance' is a dummy equal to one if respondents drive more than the EU countries average and zero otherwise. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Columns (3) and (4) additionally control for treatment assignment dummies in subsequent, independent information experiments.

Table A5: Treatment effect heterogeneity by household income

	Support for carbon tax (1)	WTP carbon tax (2)	Donation of base earnings (3)	Donation of lottery gains (%) (4)
Benefits underestimation	-0.685	-2.687*	0.404	0.395
	(1.514)	(1.509)	(0.644)	(0.531)
Revenue allocation	-2.621*	-3.198**	-0.709	-0.834
	(1.489)	(1.483)	(0.633)	(0.521)
Benefits underestimation $\times$ Log Eq. Income	0.089	0.323*	-0.050	-0.045
	(0.182)	(0.181)	(0.077)	(0.064)
Revenue allocation $\times$ Log Eq. Income	0.336*	0.401**	0.094	0.105*
	(0.178)	(0.177)	(0.076)	(0.062)
Observations	907	907	907	907
Adjusted R-squared	0.025	0.033	0.093	0.073

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. Columns (3) and (4) additionally control for treatment assignment dummies in subsequent, independent information experiments.

Table A6: The distribution of trust and climate change beliefs

		Benefits underestimation			
	Control (1)	Backfire (2)	Others (3)	(2)-(1)	(2)-(3)
Trust variables					
Trust in national institutions	6.310	6.246	6.682	-0.064	-0.436
	[1.953]	[2.263]	[1.849]	(0.282)	(0.278)
Trust in politicians and political parties	4.020	3.803	4.318	-0.217	-0.515
	[2.028]	[2.112]	[2.137]	(0.287)	(0.305)
Trust in international institutions	5.472	5.459	5.734	-0.013	-0.275
	[2.296]	[2.618]	[2.287]	(0.330)	(0.338)
Trust in traditional media	4.953	4.951	5.187	-0.003	-0.236
	[2.220]	[2.341]	[2.140]	(0.315)	(0.313)
Trust rigor and validity of hard sciences	8.127	8.136	8.402	0.009	-0.266
	[1.702]	[1.870]	[1.576]	(0.247)	(0.238)
Trust rigor and validity of social sciences	6.107	6.131	6.417	0.024	-0.286
	[2.024]	[2.232]	[1.964]	(0.289)	(0.290)
Trust rigor and validity of climate sciences	6.983	6.672	7.332	-0.311	-0.660*
	[2.132]	[2.413]	[1.921]	(0.307)	(0.291)
Climate change is real	0.947	0.869	0.967	-0.078*	-0.098**
	[0.225]	[0.340]	[0.179]	(0.035)	(0.032)
Climate change due to human activities	3.782	3.679	3.910	-0.103	-0.231*
	[0.693]	[0.613]	[0.632]	(0.102)	(0.096)
Prioritize long-term policies	0.445	0.361	0.479	-0.085	-0.119
	[0.498]	[0.484]	[0.501]	(0.070)	(0.071)
Socio-economic variables					
Log equivalized income	8.321	8.319	8.333	-0.002	-0.014
	[0.450]	[0.558]	[0.409]	(0.066)	(0.063)
Post-secondary education	0.714	0.639	0.702	-0.075	-0.063
	[0.453]	[0.484]	[0.458]	(0.064)	(0.066)
Homeowner	0.784	0.803	0.818	0.019	-0.015
	[0.412]	[0.401]	[0.386]	(0.058)	(0.056)
Woman	0.329	0.410	0.355	0.081	0.054
	[0.471]	[0.496]	[0.480]	(0.067)	(0.069)
Urban area	0.502	0.475	0.500	-0.026	-0.025
	[0.501]	[0.504]	[0.501]	(0.070)	(0.072)
Household with children	0.369	0.328	0.417	-0.041	-0.089
	[0.483]	[0.473]	[0.494]	(0.068)	(0.070)
Observations	301	61	242	362	303

Notes: Standard deviations in brackets and standard errors in parentheses. The table's columns display the average values for each group (columns 1, 2, and 3), as well as the differences between the 'Backfire' and 'Control' groups (column 4) and between the 'Backfire' and the rest of those assigned to the 'benefits underestimation' treatment (column 5). All trust variables are measured on 0 to 10 Likert scales. The variables "*climate change is real*" and "*prioritize long-term policies*" are dummy variables where 1 means yes, while "*climate change due to human activities*" has five categorical response options going from 1 "*Only natural processes*" to 5 "*Only human activity*". * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A7: The effect of the information treatments on revealed preferences, by priors

	Donation of base earnings			Donation of lottery gains (%)		
	(1)	(2)	(3)	(4)	(5)	(6)
Benefits underestimation						
Priors:						
Expected a decrease in support	-0.078	-0.076	-0.076	-0.047	-0.046	-0.046
	(0.060)	(0.060)	(0.060)	(0.049)	(0.049)	(0.049)
Expected same support	-0.029	-0.029	-0.029	0.012	0.012	0.012
	(0.053)	(0.053)	(0.053)	(0.044)	(0.044)	(0.044)
Did not know, but it makes sense	0.035	0.037	0.036	0.048	0.049	0.049
	(0.044)	(0.044)	(0.044)	(0.037)	(0.036)	(0.036)
I knew	-0.026	-0.027	-0.027	0.041	0.040	0.040
	(0.084)	(0.084)	(0.084)	(0.069)	(0.069)	(0.069)
Revenue allocation						
Prior on carbon-tax cost for households:						
1-5 Likert scale	-0.018		-0.014	-0.005		-0.004
	(0.021)		(0.022)	(0.018)		(0.018)
Prior on use of carbon-tax revenues:						
Didn't know at all		0.133***	0.170**		0.082**	0.093
		(0.044)	(0.072)		(0.036)	(0.059)
Only knew about redistribution		0.034	0.076		0.050	0.063
		(0.060)	(0.088)		(0.049)	(0.072)
Only knew about sustainable projects		0.069	0.112		0.088*	0.101
		(0.060)	(0.088)		(0.049)	(0.073)
Knew about both uses		-0.012	0.028		-0.110**	-0.098
		(0.067)	(0.090)		(0.055)	(0.074)
Observations	907	907	907	907	907	907
Adjusted R-squared	0.091	0.094	0.093	0.068	0.079	0.078

Notes: Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. All regressions include the following individual controls: equivalized household income (in log), household size, and dummies for gender, age class, post-secondary education, employment, children at home, nationality, place of residence, homeownership, and living in an urban area. All regressions additionally control for treatment assignment dummies in subsequent, independent information experiments. Columns 1 and 4 additionally control for the 'revenue allocation' treatment dummy.

Appendix B: Information treatment

B1 Introductory block on carbon tax

A reminder about the carbon tax in Luxembourg

The carbon tax covers all fossil fuels (diesel, gasoline, heating oil, gas) It is a tax measure applicable to both individuals and businesses.

In 2023, the carbon tax is set at €30 per tonne of CO2 emitted.

For fuel, this corresponds to an additional cost for individuals and businesses of around 10 centimes per liter of gasoline or diesel.

B2 Revenue allocation treatment

What is the yearly cost to households?

The yearly cost is of €220 for an average household and of €200 for the poorest 20%

Are you surprised by this?

"I thought the yearly cost of the carbon tax was...

- Much smaller
- Slightly smaller
- About this amount
- Slightly larger
- Much larger

How are revenues used by the government?

- Half of the tax revenue is used to support the poorest resident households (via a "cost-of-living allowance" and a "climate tax credit"). These financial aids **fully compensate** the cost of the tax for the least affluent 40% in Luxembourg.
- The other half of the tax revenue, between €100 and €140 million in 2023, is earmarked to finance the energy transition and public transport infrastructure projects.

Did you know this?

- No, I did not know at all

- I only knew that a portion is redistributed to offset the impact of the tax on low-income households
- I only knew that part of it is used to finance various sustainable projects
- I knew both uses of the tax revenue

B3 Benefits underestimation treatment

Scientific studies show that people's reluctance towards climate taxes **decreases** over time: they become **more supportive** of these taxes ***after*** they have been put in place.

Did you know this?

- No, I would have thought that the support would have **decreased** after the tax implementation
- No, I would have thought that the support would have **remained the same** after the tax implementation
- No, I did not know but it makes sense to me
- Yes, I knew that population support for climate taxes increases after their implementation

As an illustration, let's consider **congestion charges**. This system imposes fees on vehicles entering high-traffic areas such as city centers. They are used in Singapore, London, Oslo, Aarhus, Stockholm and Gothenburg among others.

Studies have shown that, before the implementation of the **congestion charge**, people **underestimated** the numerous **beneficial effects** it would have in terms of: **air quality**, **parking** availability, **speed** of journeys, **accessibility** and **punctuality** of **public transport**, **safety** of **soft mobility**, **savings** for citizens who opted for public transport or soft mobility.