Changing consumption behavior with carbon labels: Causal evidence on behavioral channels and effectiveness*

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Carbon labels are an increasingly popular policy tool to decrease the carbon footprint of consumers' choices. However, not much is known about their effectiveness relative to other policy instruments and the channels via which they affect behavior. Through a series of experiments, including two framed field experiments ($N = 289$ and $N = 444$, respectively) and one natural field experiment (involving more than 120,000 purchase decisions by over 10,000 customers) conducted in a student canteen setting, I provide causal evidence that carbon labels impact consumption behavior. I evaluate the labels' effectiveness in comparison to a carbon tax, both through direct elicitation (framed field experiment) and by using pricing variations (natural field experiment). In both settings, I find that the overall effectiveness of the labels is similar to that of a carbon tax of €120 per tonne. Further, complementary evidence from both settings conveys that the labels on average create psychological benefits for consumers. In the second framed field experiment, I identify the behavioral channels driving label effectiveness by varying treatment conditions. I find that carbon labels mainly impact consumers by directing attention towards carbon emissions, and less by correcting consumers' perceptions about carbon footprints. Using a structural model and data from the second framed field experiment, I estimate that carbon labels on average increase consumer welfare.

Keywords: carbon footprint, food consumption, welfare, behavioral intervention, field experiment
JEL Classification: D12, C91, C93, Q18

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1 Introduction

The recent IPCC report makes it clear that strong political action is necessary to limit global warming to 1.5°C (IPCC, 2023). However, public support for traditional policy tools, such as carbon taxes and command-and-control measures, varies strongly between economic sectors. Support for such measures is especially low in the food sector (Dechezleprêtre et al., 2022), which is responsible for 26%–34% of global greenhouse gas emissions.[1] Clark et al. (2020) predict that even if fossil fuels were banned immediately, emissions from the global food system alone would make it impossible to limit warming to 1.5°C. Shifting towards diets with lower carbon footprints[2] would greatly reduce these emissions.[3] Yet, the global effort to mitigate greenhouse gas emissions in the food sector has been weak,[4] likely due to the political challenges faced by traditional policy measures.

A more politically feasible way to influence consumption behavior could be to remove behavioral and informational frictions that prevent consumers from making carbon-friendly consumption decisions. If consumers lack knowledge about the carbon footprint of different options[5] or pay insufficient attention to these factors at the moment of choice, behavioral interventions could correct these frictions. One intervention that has received attention from academia[6], regulatory agencies[7], and private companies[8] is carbon labeling.

This paper quantifies the effectiveness of carbon labels in reducing carbon emissions relative to a carbon tax. It examines the behavioral channels through which carbon labels impact behavior. Understanding these channels is relevant for assessing under which circumstances carbon labels are effective. Moreover, I make use of these insights to estimate the effect of carbon labels on consumer welfare.

Results are based on two framed field experiments ($N = 289$ and $N = 444$, respectively) and one natural field experiment (more than 120,000 purchase decisions by $N > 10{,}000$ customers).[9] To allow for comparability across experiments, I conduct all three experiments in a student canteen context. While the student canteen context in itself offers potential for reducing emissions on a large scale[10], the findings are also relevant for related settings, such as corporate canteens, grocery shopping, or other settings in which the carbon footprint caused by different items can be calculated and labeled, for example, shopping for toiletries or clothes.

Experiment 1, the first of the two framed field experiments, and Experiment 2, the natural field experiment, jointly establish that carbon labels affect consumption behavior and allow me to estimate the magnitude of the effect. In particular, I estimate the magnitude of a carbon tax that would produce similar changes in purchase quantities as are brought about by the carbon labels. In the framed field experiment, I directly elicit an estimate based on how participants' willingness to pay

1. See e.g. Poore and Nemecek (2018) and Crippa et al. (2021). The largest contribution to this amount comes from agriculture and land use/land-use change activities, while supply chain activities make up a smaller proportion.

2. I use the term "carbon footprint" to refer to all greenhouse gas emissions. In my calculations, I transfer gases other than CO_2 to CO_2 equivalents.

3. See e.g. Poore and Nemecek (2018), Kim et al. (2020), Grummon et al. (2023), and Scarborough et al. (2023). For example,Scarborough et al. (2023) study a UK sample and estimate the dietary impact of vegans as 25.1% of those of high meat-eaters. (Grummon et al., 2023) study a US sample and find that simple changes such as substituting chicken for beef can already reduce the dietary carbon footprint by more than 25% .

4. See e.g. OECD. (2019). For example, the agricultural sector is excluded from the EU-ETS trading scheme and the USA does not have a carbon tax on the agricultural sector.

5. Camilleri et al. (2019) and Imai et al. (2022)

6. See Reisch et al. (2021) for an overview.

7. For example, the Obama administration issued an executive order on Behavioral Science and the European Commission includes carbon labels in its Farm to Fork Strategy (Obama, 2015; European Commission, 2023).

8. For example, Oatly, an oat milk producer, Just Salad, a restaurant chain, Panera Bread and Allbirds, a shoe brand (Wolfram, 2021) all engage in carbon labeling.

9. My classification as framed or natural field experiment follows the Harrison and List (2004) taxonomy.

10. In Germany, 2.9 million individuals classified as students in 2021 (Federal Statistical Office (Germany), 2023), of which around 54% eat in the student canteen at least once a week (Federal Ministry of Education and Research (Germany), 2023).

for meals changes when shown carbon labels. In the natural field experiment, I rely on the combination of a carbon labeling intervention and variations in pricing. I estimate the effectiveness in both experimental settings to provide a precise and externally valid estimate: While the framed field experiment trumps the natural field experiment in terms of precision and clean causal identification, the natural field experiment provides evidence that the framed field estimates can be reconciled with student canteen purchasing behavior observed over longer time periods and across a large number of customers. Experiment 3 ($N = 444$) then returns to a framed field experimental set-up to assess the significance of both removing information frictions and addressing attention frictions in shaping consumers' responses to carbon labels.

Experiment 1 ($N = 289$) is a framed field experiment examining how willingness to pay for typical student canteen meals changes when participants are shown labels. The carbon labels I test include both an ordinal (traffic light system) and a quantitative ranking (greenhouse gas emissions in kg). This has been identified as an effective combination in the previous studies (Potter et al., 2021; Taufique et al., 2022). The willingness to pay values participants indicate in the experiment influence the meal they receive at payout. I examine how participants' willingness to pay for specific meals changes when they are shown carbon labels, and compare these with the respective carbon footprints of those meals. I find that, on average, there is a decrease of €0.12 for each kilogram of CO_2 emissions caused by a meal. A decrease in average willingness to pay for a meal should have the same effect on the total quantity purchased as an equivalent increase in meal price, and I thus conclude that carbon labels produce a similar decrease in carbon emissions as a carbon tax of €120 per tonne.[11]

Experiment 2 is a natural field experiment ($N > 120{,}000$ choices from over 10,000 guests), showing that the consumption reactions I observe to carbon labels in Experiment 1 are reconcilable with behavior observed outside of a one-shot consumption setting. One of Bonn's university canteens is equipped with carbon labels for five weeks, while the two other canteens serve as control restaurants, allowing for a difference-in-differences estimation of label effectiveness. The carbon labels I test are similar to those used in Experiment 1. I find that the labels decrease consumption of the higher carbon option by 2 percentage points or 5 percent of baseline consumption. The student canteen was still open for three weeks after the intervention period before closing for summer break. The effect of the labels persists in these three weeks. I compare the effect with an estimate of the effect of a carbon tax, which I estimate based on consumption choices and variations in canteen prices. I find that the effect of the carbon labeling intervention is comparable to that of a carbon tax of €80 to €120 per tonne in the same setting. This is similar to the effect magnitude I observe in Experiment 1.

This quantification enables me to compare carbon labels with other policy tools and allows us to better understand the magnitude of the effect. €120 per tonne is about four times the current German carbon tax on petrol. At the same time, it is still lower than many estimates of the social cost of carbon (e.g. €160 per tonne in Rennert et al., 2022). This suggests that the labels are not inefficiently "over-correcting" behavior.

To determine which behavioral channels drive consumers' reactions to carbon labels, I set up a simple theoretical model of consumption behavior in the presence of carbon emissions. My model features two main characteristics: First, consumers may prefer meals with lower carbon emissions, but may not be attentive to emissions at the moment of choice. Second, I allow for a lack of knowledge of the carbon emissions caused by different meals, that is, misperceptions of carbon footprints. Behavioral interventions such as carbon labels make the consumer both attentive and informed. [12]

11. This is €120 per metric tonne. One metric tonne ≈ 1.1 short Tons/US Tons.

12. These modeling choices are based on the existing literature: I focus on attentional biases as an important factor impeding optimal decision making, that have been identified as relevant in the tax salience and resource consumption contexts (Chetty, 2009; DellaVigna, 2009; Byrne et al., 2022), as well as in suggestive empirical evidence from the food consumption context (Lohmann et al., 2022). I focus on misperceptions of carbon impact as an important factor impeding optimal

Experiment 3 ($N = 444$) is a framed field experiment seeking to quantify the relevance of each of these two possible channels. Are carbon labels mainly changing behavior by correcting misperceptions about carbon footprints or are they primarily changing behavior by increasing consumers' attention? I elicit participants' meal valuations, prior beliefs of the carbon footprints of different meal options, and participants' willingness to pay for seeing or avoiding carbon labels in the student canteen context. The meal valuations participants indicate influence the meal they receive after completing the experiment, and willingness to pay to see or avoid labels is also incentivized. I observe purchasing behavior in different treatment conditions: first, in the absence of any behavioral intervention, second, with a behavioral intervention increasing attention (asking consumers to guess emissions), third, with a behavioral intervention increasing attention and correcting misperceptions (carbon labels) and, finally, when carbon emissions are removed (carbon offsetting).

Reduced-form results suggest that the labels primarily impact consumers by directing their attention toward carbon emissions. Improving consumers' knowledge about carbon impact plays a secondary role. Participants on average underestimate the emissions caused by high-emission meals and overestimate the emissions caused by low-emission meals. Correcting these misperceptions significantly impacts consumption choices: Consumers react to carbon labels with a stronger demand reduction if emissions were previously underestimated. However, a large part of the carbon labels' treatment effect is independent of previous under- or overestimation. The treatment effects observed for the intervention that merely increases attention without correcting misperceptions suggest that a large part of the remaining effect can be explained by an allocation of attention.

Using data from Experiment 3, I structurally estimate my model. Based on the estimated model parameters, I simulate how solely removing attentional biases or solely correcting consumers' misperceptions would impact carbon emissions and consumer welfare in the student canteen context. The former is more than seven times as effective as the latter, both in increasing consumer welfare and in decreasing carbon emissions. The combination of the two interventions (carbon labels) is most effective, and also more effective than the sum of the two single interventions, indicating important complementarities.

Data from all three experiments suggests that carbon labels are creating an overall psychological benefit to consumers. Experiments 1 and 3 elicit participants' willingness to pay to see or avoid carbon labels in a direct and incentive-compatible manner. The vast majority of participants report a zero (50%) or positive (45%) willingness to pay to see carbon labels. This evidence speaks against carbon labels imposing a net psychological cost on consumers. In the structural estimation of my model, I show that carbon labels on average create a psychological benefit for consumers independent of their impact on consumers' decisions. I estimate that carbon labels increase consumer welfare by on average 0.18¢per consumption decision, similar to the impact of a revenue-neutral carbon tax. Additionally, they create a fixed psychological benefit of 21¢independent of their impact on consumption decisions. These results are further supported by a post-intervention survey conducted after Experiment 2, the natural field experiment in the student canteen ($N = 234$). 73% of the guests exposed to the labels report that they would like the labels to be installed permanently (18% do not know, 9% against).

My contributions to the literature are three-fold: First, I contribute to the literature on the role of attentional biases in consumption decisions. The finding that it is not only informational but also attentional biases that lead to non-optimal decision-making has been pointed out in other environmentally relevant consumption contexts, mainly energy and resource consumption (Allcott and Taubinsky, 2015; Taubinsky and Rees-Jones, 2018; Tiefenbeck et al., 2018). This project adds to this by providing evidence of attentional biases present in the food consumption context. The literature on carbon labels has so far mainly portrayed the labels as a tool for correcting consumer misperceptions (Shewmake et al., 2015; Camilleri et al., 2019; Imai et al., 2022). My theoretical framework

decision-making based on suggestions in recent papers on carbon labeling (Shewmake et al., 2015; Camilleri et al., 2019; Imai et al., 2022).

combining attentional biases and informational frictions as impediments to optimal decision-making can shed light on the mixed findings on carbon label effectiveness in the existing literature (Camilleri et al., 2019; Bilén, 2022; Imai et al., 2022; Lohmann et al., 2022). More generally, I provide evidence of how a behavioral intervention can correct attentional biases and thereby reduce externalities and increase consumer welfare. I thus add to the literature on the frictions that behavioral interventions can address (e.g. Benartzi et al., 2017; Reisch and Zhao, 2017).

Second, I contribute to the relatively recent literature on the consumer welfare impact of behavioral interventions. One strand of this literature derives consumer welfare from structural models or sufficient statistics (Chetty, 2009; DellaVigna, List, and Malmendier, 2012; DellaVigna et al., 2016; Rodemeier, 2021; Goldin and Reck, 2022; List et al., 2022; Rodemeier and Löschel, 2022; Barahona, Otero, and Otero, 2023), while another directly measures consumer welfare by eliciting willingness to pay to receive a behavioral intervention, sometimes combined with a structural approach (Allcott and Kessler, 2019; Thunström, 2019; Allcott et al., 2022; Butera et al., 2022; Andor et al., 2023). I take an approach consistent with the second strand, providing first evidence of consumers' preferences for the presence of carbon labels. I elicit consumers' willingness to pay for the presence of carbon labels directly in framed field experiments 1 and 3 and, for robustness, conduct an opinion survey at the end of the natural field experiment 2. Based on my theoretical framework and experiment 3 data, I provide an estimate of the effect carbon labels have on consumer welfare and compare it to alternative interventions.

Finally, I contribute to the literature on the effectiveness of carbon labels on food consumption. Lohmann et al. (2022) find that labels in Cambridge student canteens decrease the probability of selecting a high-carbon meal by approximately 2.7 percentage points, using a difference-in-difference framework. Brunner et al. (2018) study a similar context but only observe changes over time in a single restaurant. They find a decrease in sales of red-labeled meat dishes by 2.4 percentage points Bilén (2022) study the introduction of carbon labels in the grocery shopping context and estimate a 2.5 percentage point reduction in carbon emissions caused by the carbon labels, employing a difference-in-difference estimation. Correlational evidence (Spaargaren et al., 2013; Vlaeminck, Jiang, and Vranken, 2014; Visschers and Siegrist, 2015) and evidence from hypothetical decisions (Osman and Thornton, 2019; Banerjee et al., 2022) further corroborate the finding that carbon labels can reduce carbon emissions. [13] Other studies examine consumer behavior in the lab, asking consumers to make a decision for consumption happening at some point in the future. Camilleri et al. (2019) and Panzone et al. (2021) find carbon labels effective, while Imai et al. (2022) do not find an effect.

Previous studies examining the effectiveness of carbon labels estimate effect sizes in terms of percentage changes in consumption behavior, which are difficult to compare across consumption contexts and policy instruments. In Experiment 1, I provide a first experimental estimate of the effectiveness of carbon labels relative to a carbon tax, yielding a useful context for interpreting effect sizes. Within-subject designs as used here and in other structural behavioral studies (Taubinsky and Rees-Jones, 2018) can easily be adapted to other experiment populations, consumption environments, or other behavioral interventions, making intervention effects comparable across various domains. The experimental design is further validated by the large-scale natural field experiment (Experiment 2) producing effect estimates in line with the results of Experiment 1.

Further, Experiment 2 provides the—to my knowledge first—estimate of the post-intervention effects of a carbon labeling intervention. In a broader sense, this paper also adds to environmental interventions in the restaurant context in general (e.g. Jalil, Tasoff, and Vargas Bustamante, 2020).

The rest of this paper is structured as follows. Section 2 describes how Experiment 1 quantifies the effectiveness of carbon labels using direct elicitation in a framed field experiment. Section 3 describes the design and results of Experiment 2, which is the natural field experiment corroborating

13. See Rondoni and Grasso (2021) for a review.

my Experiment 1 estimate. Section 4 outlines a simple theoretical model describing possible behavioral biases influencing consumption behavior in the food consumption context, and the channels through which I expect a behavioral intervention such as carbon labels to impact behavior. Section 5 describes Experiment 3, the framed field experiment I conducted to examine the relevance of each of these channels, and discusses reduced-form evidence. Section 6 structurally estimates the theoretical model using data from Experiment 3. Section 7 discusses the impact of behavioral interventions on consumer welfare, drawing on data from all experiments. Finally, Section 8 concludes.

2 Experiment 1: Quantifying the effectiveness of labels in a framed field experiment

Experiment 1 quantifies the effectiveness of carbon labels in a framed field experiment. Subsection 2.1 describes the experimental design, 2.2 describes the empirical strategy, and subsection 2.3 describes data and results.

2.1 Design of Experiment 1

Overview. To cleanly measure the impact of carbon labels and elicit how effectiveness quantifies relative to a carbon tax, willingness to pay for the same meal should best be observed for the same individual, at the same time, once in the absence of carbon labels and once in the presence of carbon labels. Experiment 1 is designed accordingly. I summarize the most important design choices below and add details in the following subsections.

(1) For this experiment, I move participants' lunch consumption decision to an online survey, which they fill out just before lunchtime on the experiment day. Participants make their way to the university campus shortly after completing the survey and receive the experiment payment and lunch option corresponding to the choices they made in the survey.

(2) In the survey, experiment participants indicate their willingness to pay for different meals multiple times, totaling to 15 meal purchase decisions. One of these is implemented at payout.

(3) I allocate participants to the Label or the Control condition: Participants in the Label condition first indicate willingness to pay for four meals in the absence of carbon labels and shortly after indicate willingness to pay for the same four meals in the presence of carbon labels. Participants in the Control condition make the same decisions but do not see any carbon labels in the second elicitation.

(4) In some of participants' later decisions, I Offset meals' carbon footprints with donations to Atmosfair. [14]

(5) Willingness to pay for meals is elicited relative to an alternative lunch: In each of the 15 meal purchase decisions, participants first decide whether they prefer a given meal or a cheese sandwich.[15] They then indicate how much they are willing to pay to receive the given meal rather than the cheese sandwich, and vice versa if they prefer receiving the cheese sandwich. Willingness to pay for a given meal is thus always measured relative to the cheese sandwich (reflecting the real-world fact that the alternative to not eating something is eating something else). The dependent variable of interest in the analysis is the **change** in relative willingness to pay between the first and second elicitation.

14. I use these estimates as a robustness check of the results of the Attention+Offset condition in Experiment 3. Details are further described in section B.0.3 and results are shown in Table C.6.

15. All the meals are typical student canteen meals and a cheese sandwich is also a typical lunch choice in Germany. Meals are further described in section B.

(6) Carbon labels show a quantitative and ordinal ranking (see Figure 4 for an example). The carbon labels I test include greenhouse gas emissions in kg, as calculated based on the quantity of each meal ingredient and its average greenhouse gas emissions. It also includes an ordinal ranking using a traffic light system, ranking the meal relative to other meals typical of Bonn's student canteens. A combination of ordinal and quantitative ranking has been identified as an effective combination in previous literature (see Taufique et al., 2022 and Potter et al., 2021). Further, I designed the labels in cooperation with Bonn's student canteens to ensure that I am testing labels that they would be willing to implement and thus to ensure comparability to Experiment 2. The labels also indicate the distance a car would need to be driven (in kilometers) to produce an equivalent level of CO_2 emissions.

(7) Willingness to pay to see or avoid carbon labels is also elicited: Before the final three meal purchase decisions (three new meals), participants indicate whether they would like to see carbon labels on these final decisions, and indicate their willingness to pay to enforce their choice. This elicitation is incentivized.[16] I discuss these results in Section 7.

Timeline. The survey timeline is visualized in Figure 1. First, the elicitation of willingness to pay is explained to participants and they are shown how their payout and the meal they receive will depend on the choices they make throughout the experiment. They then answer four comprehension questions, which they must answer correctly before proceeding. Any participant taking more than five attempts in doing so is excluded from the analysis, as pre-registered. Second, participants indicate their baseline willingness to pay for four meals (four questions). Third, participants answer several incentivized and timed[17] guessing questions on unrelated issues (e.g. on the length of a popular running route in Bonn).

The experiment then proceeds differently depending on the treatment group participants are assigned to by computer randomization. All participants are again asked to indicate their willingness to pay for the four meals, but the framing of the decision and some characteristics of the decision depend on the treatment condition:

- In the Control condition, decisions are exactly as in the first, baseline elicitation.
- In the Label condition, participants see carbon labels.

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[18], with partly changed treatment conditions:

- Participants previously in the Label condition are in the third round assigned to the Offset condition: Participants are informed that the emissions caused by their lunch choice (be it the meal or the sandwich) will be offset.[19]
- Half of the participants previously in the Control condition are in the third round assigned to the Label condition, and half of the participants previously in the Control condition repeat the Control condition. Afterward, before proceeding with the experiment, this group guesses emission values.[20].

16. See section B.0.2 for details.

17. For each question for which participants answer a number within 30% of the true value, €0.10 is added to participants' pay-out. Further, each question is restricted to 60 seconds of answering time to ensure that participants can not search for answers online.

18. In the analyses, I control for whether observations stem from a third-round elicitation. All the main results replicate including only data from the first two rounds, see Table C.20.

19. The results of the Offset condition are not further discussed in this section, but details are described in section B.0.3 and results are shown in Table C.6. The Offset condition serves as a robustness check of the results of the Attention+Offset condition in Experiment 3, which is used as input for the structural estimation described in Section 6.

20. This data is used for the analysis shown in Figure 14. As these guessing questions occur after the first, second, and third willingness to pay elicitation, they do not affect the results displayed in this section.

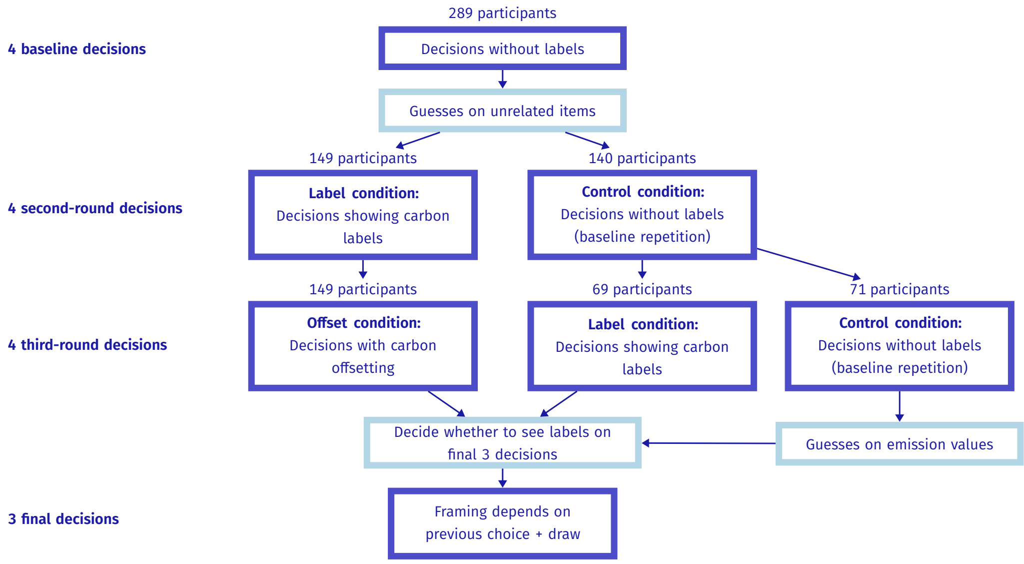

Figure 1. Experiment schedule and treatment groups. Participants repeat the same four meal purchase decisions three times, with the decision framing differing across rounds. Treatments are described in more detail in the "Experiment timeline" paragraph above. The results of the OFFSET condition are not further discussed in this section, but details are described in section B.0.3, and results are shown in Table C.6. The OFFSET condition serves as a robustness check of the results of the ATTENTION+OFFSET condition in Experiment 3, which is used as input for the structural estimation described in Section 6.

The three rounds include four meal purchasing decisions each, constituting a total of 12 decisions. Additionally, three final purchase decisions revolve around three not previously seen meals. Before seeing these final decisions, participants are asked whether they would like to see carbon labels for these decisions and indicate how much they are willing to pay such that their preferred display option is implemented. This elicitation is incentivized as detailed below.

In the final steps, participants answer questions concerning their environmental attitude and psychology, and participants' guesses of the calories contained in each meal are elicited for further robustness checks.[21]

Details on the meal purchasing decisions. Participants make a total of 15 meal-purchasing decisions in the course of the experiment (4 baseline, 4 first-round, 4 second-round, and 3 final decisions). The 12 first decisions revolve around the same 4 meals, and the final 3 decisions around 3 other, not previously seen meals. Participants who indicate that they are vegetarian are shown only vegetarian meals.[22] In each decision, participants first choose whether they prefer consuming a certain meal or a cheese sandwich. An example of a baseline decision is shown in Figure 2. The left option in the example changes across decisions to indicate one of the four meals, while the option on the right, the cheese sandwich, stays constant for all decisions.[23]

Once participants indicate their preference for one of the two options, a second window appears and they indicate how much of their experiment payment they would at most be willing to forego to ensure their preference (see example in Figure 3 in which the participant indicated a preference

21. See section C.10 and B.1 for results and experiment screenshots.
22. Meals are detailed in Section B.0.1.
23. To ensure that results are not driven by a left-right effect, the left-right positioning of the two options is reversed in half of the experiment sessions. The order in which meals are shown is randomized.

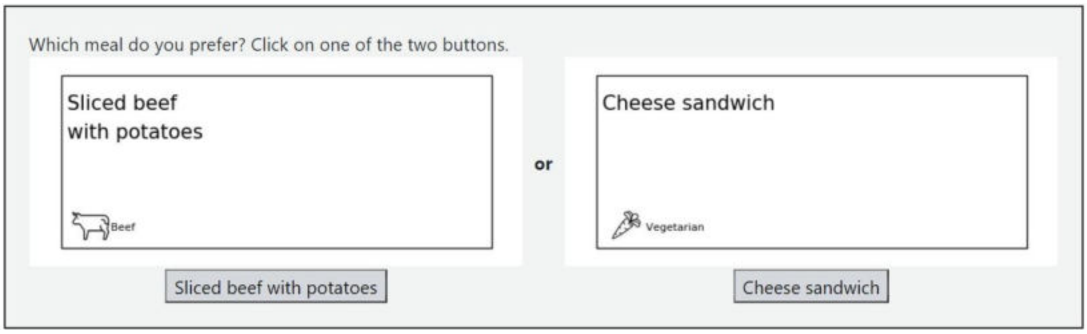

Figure 2. Meal purchase decision example: Step 1 of the purchasing decision. Depending on the participants' decision in Step 1 of the decision, Step 2 (Figure 3 asks participants for their willingness to pay to receive or avoid the warm meal.

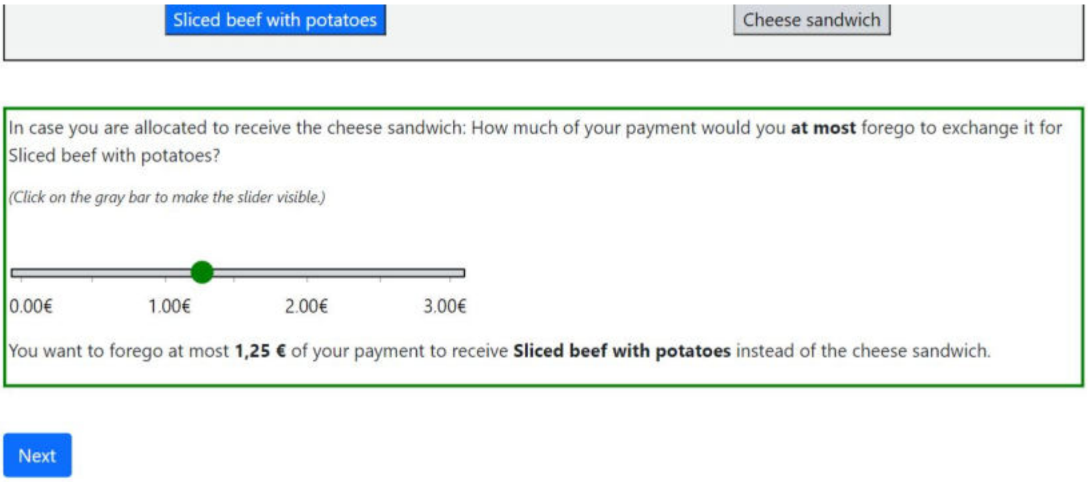

Figure 3. Meal purchase decision example: Step 2 of the purchasing decision. If participants indicate in Step 1 that they prefer the warm meal, Step 2 is as shown above. If participants indicate in Step 1 that they prefer the cheese sandwich, Step 2 asks participants how much they are at most willing to forego to receive the cheese sandwich instead of the warm meal.

for Sliced beef in the first step). If participants prefer the specific meal, they indicate how much they are willing to forego to ensure they receive this meal instead of the cheese sandwich. If participants prefer the cheese sandwich, they indicate how much they are willing to forego to ensure they receive the cheese sandwich instead of the specific meal. Any amount between €0.00 Euro and €3.00 can be indicated on a slider in five-cent intervals. [24]

This meal-purchasing procedure captures participants' willingness to pay for the specific meal, relative to the cheese sandwich. If participants indicate in the first step that they prefer the specific meal, the amount they indicate in the second step can be interpreted as willingness to pay to receive the meal. If participants indicate in the first step that they prefer the cheese sandwich, the amount they indicate in the second step can be interpreted as willingness to pay to avoid the meal, i.e. negative willingness to pay for the meal.

24. I chose €3.00 as the maximum amount since this is the maximum price a student would pay to purchase any of the meals in the student canteen. A willingness to pay of €3.00 or −€3.00 was indicated in less than 3% of all observations. Figure C.1 shows the distribution of baseline willingness to pay values indicated.

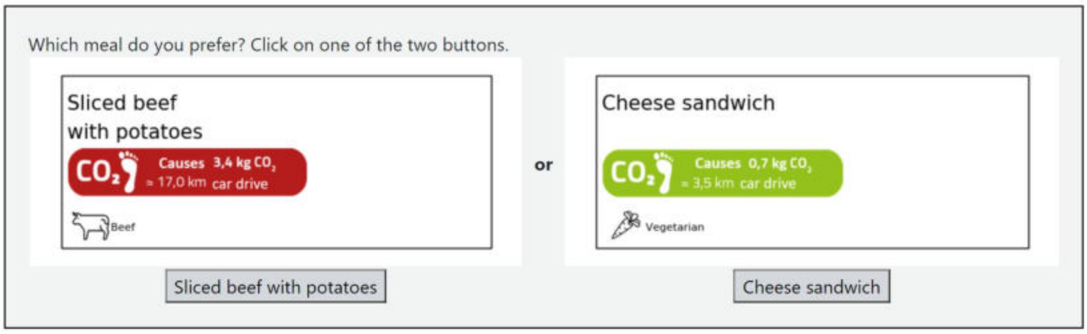

Figure 4. Meal purchase decision example: Decisions with labels. Carbon labels include both an ordinal (traffic light system) and a quantitative ranking (greenhouse gas emissions in kg). This has been identified as an effective combination in previous literature (Potter et al., 2021; Taufique et al., 2022).

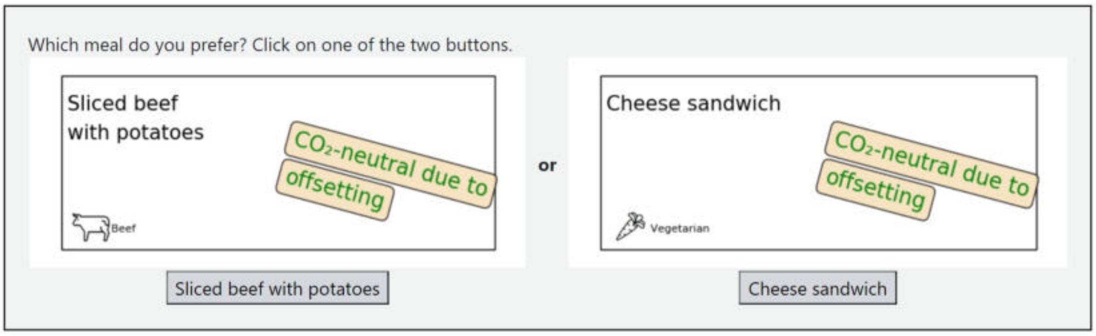

Figure 5. Meal purchase decision example: Decisions with carbon offsetting. If the meal is payout-relevant, I offset emissions of the selected meal with a corresponding donation to Atmosfair.

Decision framing differs across treatment conditions. In the four baseline decisions, participants do not see any carbon labels but are merely shown the meal name and the meal's main ingredient (see Figure 2 for an example)[25]. The four second-round and four third-round decisions are very similar to the baseline decisions, with the exception that the framing of the decision changes for some of the participants. For participants in the LABEL condition, emission values are added to the meal options. An example is shown in Figure 4.[26] For participants in the CONTROL condition, there is no change in framing relative to the baseline decisions. For participants in the OFFSET condition, participants are told that the emissions caused by the meal will be offset with a donation to Atmosfair. An example is shown in Figure 5.

Participants and set-up. 289 experiment participants are recruited from the participant pool of the BonnEconLab, the behavioral experimental lab of the University of Bonn, to participate in one of eight experimental sessions taking place between the 26th of October and the 5th of November 2021. I pre-registered the experiment design and the main outcomes shown in this section (Schulze Tilling, 2021b). Participants are informed in the experiment invitation that vegetarian participants

25. I chose this display to reflect exactly how a meal would be displayed on the student canteen website, see Figure 9 for an example of implementation in the field.
26. I calculated the emissions caused by each meal with the application Eaternity Institute (2020). The student canteen in Bonn catered the meals and provided me with recipes for the emissions calculation.

Figure 6. Gazebo set up on University campus to provide participants with their payment in cash and a lunch corresponding to one of their choices. While completing the experiment, participants do not know which meal is payout-relevant.

are permitted, but not participants with stricter dietary requirements (vegan, gluten-intolerant, lactose-intolerant, or halal). Participants are informed that the experiment will be conducted online, but that they are required to make their way to campus afterward to collect their payment in cash and a lunch. They are not given any further information on the purpose of the experiment. The experiment is conducted using oTree software (Chen, Schonger, and Wickens (2016)).

Meals are catered by the student canteen. All experiment meals are regularly offered by the student canteen, but they are not offered on the particular experiment day, i.e. the student canteen prepared the meals only for our experiment participants. When participants pick up their meal, it is warm, ready to eat, and can be consumed on the spot, as shown in Figure 6.

Incentivization. At the beginning of the experiment, participants are informed that one (to the participants unknown) meal purchase decision will be implemented. For the relevant decision, a random price draw and participants' willingness to pay determine whether the participant receives the meal or the cheese sandwich and which amount is deducted from his payment. Participants' decision to see or avoid carbon labels on their final three decisions is incentivized with a similar BDM mechanism, with a random price draw and participants' indicated willingness to pay to see or avoid the labels jointly determining whether carbon labels are shown on the final three decisions. The details of the incentivization mechanisms are explained in Appendix B.0.2.

2.2 Estimation strategy

In my estimation, I want to control for each individual's baseline tastes, hunger level, mood, and any other factors that might influence meal choices other than the carbon labels themselves. I thus examine **changes** in willingness to pay for a specific individual and a specific meal as the outcome variable: Instead of directly examining an individual's willingness to pay for a meal in the Label or Control condition, I subtract the individual's baseline willingness to pay for the same meal from this amount, and then examine the remaining amount, the change in willingness to pay.

In this manner, I control for individual-specific meal tastes at baseline. This includes any inclinations toward the warm meals or the cheese sandwich, and any other factors that might influence meal choice apart from the carbon labels. All of these factors should stay constant between the baseline elicitation and subsequent elicitations. One can also interpret the outcome variable as denoting individual- and meal-specific within-subject treatment effects, which I compare between treatment groups. An alternative approach would be to use willingness to pay as the dependent variable and include a fixed effect for every individual-specific meal choice. This approach yields similar results, as shown in Section C.7.

My basic specification is:

$$Diff_{ijm} = \beta_1 High_m + \beta_2 Low_m + \delta_1 (Label_{ij} \times High_m) + \delta_2 (Label_{ij} \times Low_m) + ThirdRound_j + \varepsilon_{ijm} \quad (1)$$

where $Diff_{im}$ describes the difference between willingness to pay of individual i in round j for meal m and individual i's baseline willingness to pay for meal m.

$High_m$ is an indicator variable for whether the meal causes higher emissions than the sandwich.[27] Low_m is an indicator of whether the meal causes lower emissions than the sandwich. Together, these variables capture any effect that the mere act of asking participants for their willingness to pay multiple times might have. I differentiate between meals with emissions lower than the sandwich and meals with emissions higher than the sandwich because I expect participants to respond to carbon labels differently depending on how the emissions of the two options compare: For meals with emissions lower than the cheese sandwich, the participant can reduce his expected emissions if he adjusts his willingness to pay for these meals upward. For meals with emissions higher than the sandwich, the participant can reduce his expected emissions if he adjusts his willingness to pay for these meals downward.

$(Label_{ij} \times High_m)$ interacts the high-emission indicator with an indicator for whether individual i saw carbon labels in round j. This describes the average causal effect of carbon labels on willingness to pay for a meal that is high in carbon emissions. $(Label_{ij} \times Low_m)$ describes the average causal effect of carbon labels on willingness to pay for a meal that is low in carbon emissions. $ThirdRound_j$ is an indicator of whether it was the third round of decisions.[28]

2.3 Data and results

I use data from Experiment 1 to estimate equation 1, but exclude the 3% fastest participants as well as participants not passing the comprehension check after five attempts, as pre-registered[29]. The remaining 289 experiment participants are computer-randomized into treatments. Section C.1 shows a randomization check. Participants are on average 24 years old, 67% are female, 80% are students and 25% are vegetarians. The sample is roughly representative of regular student canteen guests in terms of these characteristics, as discussed in Section C.2, and results hold when restricting the sample to only students or only non-vegetarians, as shown in Section C.5. Section C.3 shows the baseline distribution of relative willingness to pay for meals.

Table 1 Spec. (1) shows the results of the OLS estimation of equation 1, clustering standard errors at the individual level.[30] For meals with lower emissions than the cheese sandwich, willingness to pay increases by €0.14 on average due to the labels. For meals with higher emissions than the cheese sandwich, willingness to pay decreases by €0.31 due to the labels. Changes in willingness to pay for participants in the Control condition are not significant, and, coefficient-wise, move in opposite directions. Thus, the mere act of asking participants for their willingness to pay multiple times does not seem to significantly impact their willingness to pay. Figure 7 illustrates effects by showing average changes in willingness to pay for the Control and Label groups, for low-emission and high-emission meals.

Specification (2) in Table 1 does not group the four meals into low-emission and high-emission meals but instead regresses the change in willingness to pay on the difference in emissions between the warm meal and cheese sandwich. This specification estimates that on average, willingness to pay decreases by €0.12 for every additional kg of emissions that the warm meal causes on top of the cheese sandwich. This result can be interpreted as—assuming that a downward shift in the demand

27. For non-vegetarians, these were three of the four meals. For vegetarians, these were two of the four meals. See section B for details.

28. An alternative approach to controlling for possible third-round effects is excluding third-round decisions entirely. This yields similar results (Table C.20).

29. See Schulze Tilling (2021a). Dohmen and Jagelka (2023) find that fast respondents do not pay attention and give random answers.

30. The results of the Offset condition are not further discussed in this section, but details are described in section B.0.3 and results are shown in Table C.6. The Offset condition serves as a robustness check of the results of the Attention+Offset condition in Experiment 3, which is used as input for the structural estimation described in Section 6.

curve results in the same effect on quantity purchased as an upward shift in the supply curve—the carbon labels producing a similar impact in this setting as would result from a carbon tax of €0.12 per kg or €120 per tonne. This is four-fold the current German CO_2 tax on petrol (€30 per tonne).

Section C.8 shows the distribution of participants' reactions to carbon labels by baseline willingness to pay for meals. Section C.9 shows suggestive evidence of heterogeneity in treatment effects. Section C.10 tests whether the carbon labels influenced participants' perception of meals' calories (as a proxy for other meal characteristics). This is not the case.

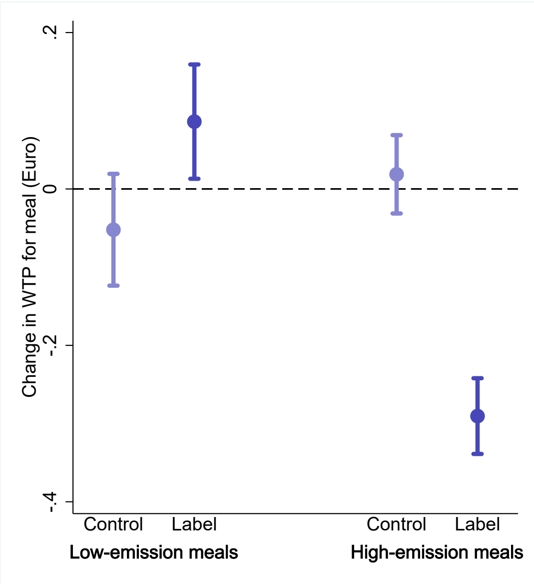

Figure 7. Within-subject change in willingness to pay for meals, differentiated between participants in the CONTROL and LABEL condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal × Shown label	-0.31^{***}	
	(0.05)	
Low emission meal × Shown label	0.14^{***}	
	(0.04)	
High emission meal	0.01	
	(0.02)	
Low emission meal	-0.06^{*}	
	(0.03)	
Emissions(kg) × Shown label		-0.12^{***}
		(0.03)
Emissions(kg)		0.02
		(0.01)
Shown label		-0.08^{**}
		(0.03)
Control for third round	0.01	0.02
	(0.03)	(0.03)
Constant		−0.02
		(0.02)
Participants control	140	140
Participants treated	218	218
Observations	1,716	1,716

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1. Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline. Spec. (1) corresponds to Equation 1 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. In spec. (2), emissions (kg) are defined as the emissions caused by the meal relative to the cheese sandwich. This is positive for "high-emission" and negative for "low-emission" meals. Standard errors are clustered at the individual level.

3 Experiment 2: Quantifying the effectiveness of labels in a field setting

The results in Section 2 show that carbon labels change consumer behavior and reduce emissions in a one-shot consumption setting. Experiment 2 tests the external validity of this result: It investigates in a natural field experiment in the student canteens in Bonn whether effects are similar if carbon labels are installed over longer time periods. Subsection 3 describes the experiment design, subsection 3.1 describes the estimation strategy, and subsection 3.2 describes data and results.

Design and setting

Overview. To identify the causal effect of carbon labels in the field, I make use of the fact that there are multiple student canteens in Bonn that centralize their meal planning, i.e. roughly the same meals are offered in all restaurants. I summarize the most important details below and describe

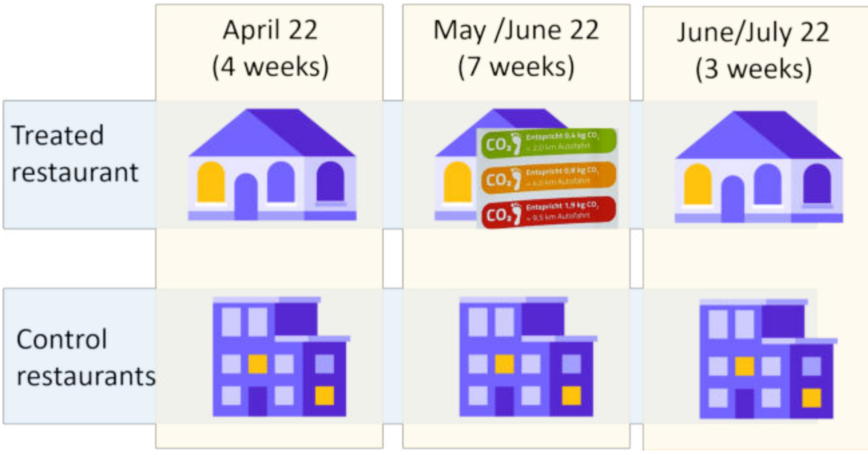

Figure 8. Timeline Experiment 2. The experiment timeline corresponds to the summer semester, ending with the student canteens going into the summer break. During the treatment phase, carbon labels were shown in the treated canteen, on the online menu, digital billboards in the canteen, and leaflets on meal counters.

the student canteen setting in Bonn more in detail in Section D.1. I pre-registered the experiment design and main outcomes.[31]

(1) I use a difference-in-difference design, as illustrated in Figure 8: Purchasing behavior in all three student canteens is first observed in the absence of labels (pre-intervention phase, 4 weeks), then labels are installed in the treatment student canteens (intervention phase, 7 weeks). After the removal of the labels, I observe consumption behavior until the end of the semester (post-intervention phase, 3 weeks).

(2) Carbon labels show a quantitative and ordinal ranking, and are similar to the carbon labels used in Experiment 1. In the treatment canteen, they are added to the online menu, to the digital billboards in the student canteen, and to the paper leaflets on top of the meal counters. Examples are shown in Figure 9. Emissions are again calculated based on student canteen recipes and Eaternity Institute (2020) emission values.

(3) Carbon labels are installed for the two main meal components sold by the treatment restaurant, but not for sides and desserts, for ease of implementation and interpretability (see D.1 for details). A typical student canteen meal consists of one meal component and one or two sides, with the main meal component on average causing 70% of the emissions caused by a typical meal. The two main meal components on offer always consist of one vegetarian and one meat-based component, which is higher in carbon emissions than the vegetarian option.

(4) I accompany the natural field experiment with a pre-intervention (N>1,700) and post-intervention survey (N> 900) in the field. These capture students' demographic characteristics (connectable to canteen purchasing data) and opinions on the carbon labels. These surveys are described in more detail in Section D.1.6.

3.1 Estimation strategy

The student canteens offer one vegetarian and one meat main meal component every day, with the vegetarian main meal component always causing lower emissions than the meat main meal

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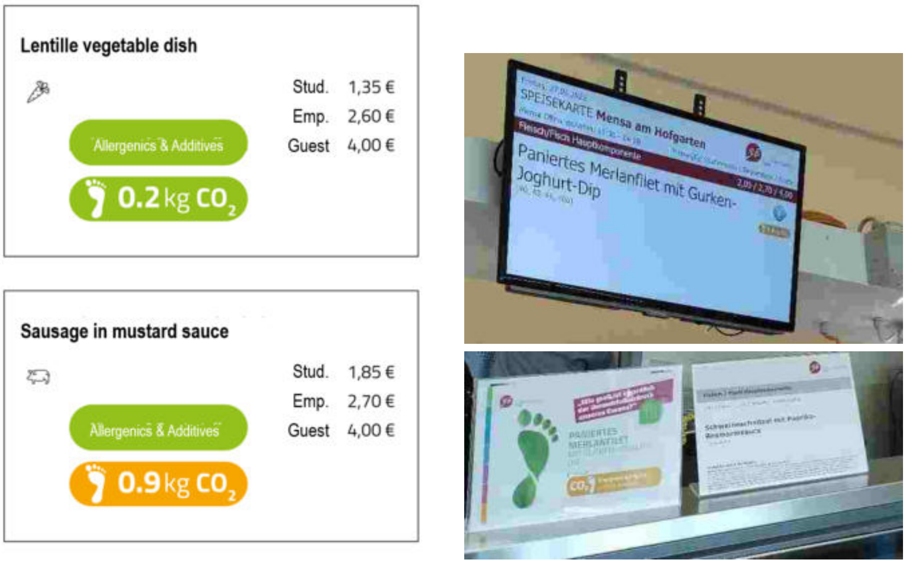


Figure 9. Labels online (left, menu translated from German) and in the student canteen (right)

component. The meal offer changes daily, and emissions caused thus largely fluctuate across days. The main analysis thus focuses on changes in the proportion of meat main meal components purchased.[32]

Section D.1.2 and section E.0.1 show that the carbon labeling intervention did not lead to increased switching between control and treatment canteens and that treatment spillover effects are very limited. However, the proportion of meat main components sold in the treatment and control canteen differ pre-intervention (41% and 51%). I thus use a difference-in-difference design to control for baseline differences in meat consumption. Identification hinges on the assumption of parallel trends between the canteens in the absence of treatment. While this assumption cannot be tested directly, I provide evidence suggesting that this is a reasonable assumption. Figure 10 shows that student canteen sales develop similarly throughout the entire 14-week period, Figure 11 shows reasonable pre-trends and Figure E.2 shows that trends are parallel in the semester following the intervention.

My most basic difference-in-difference specification is:

$$\begin{aligned} Meat_{it} = \alpha + LabelPeriod_t + PostPeriod_t + Treat_{it} + \\ + \delta_1 (Treat_{it} \times LabelPeriod_t) + \delta_2 (Treat_{it} \times PostPeriod_t) + \epsilon_{it} \end{aligned} \quad (2)$$

The variable $Meat_{it}$ is a binary outcome describing whether the main meal component purchased by individual i on day t is meat-based, i.e. $Meat_{it}$ equals 1 if the meat-based main meal component is purchased, and 0 if the vegetarian main meal component is purchased. $LabelPeriod_t$ is an indicator of whether this purchase occurred during the intervention period (May/June 22), and $PostPeriod_t$ is an indicator of whether this purchase occurred in the three weeks following the intervention period, before the canteens went into summer break (June/July 22). $Treat_{it}$ is an indicator of whether the purchase occurred in the treatment canteen.

32. Average carbon footprints at baseline differ between treatment and control canteens, and average emissions of the gastronomic offer also differ between the baseline and the intervention period. As further explained in Section E.0.4, an analysis of the full sample using carbon footprints as the outcome variable could mistakenly attribute changes in emissions due to changes in the gastronomic offer to the carbon label. Section E.0.4 shows an analysis with emissions as the main outcome variable on a sample restricted for this analysis. I find that the labels reduce emissions by 25 grams per meal, or 3% of baseline emissions.

($Treat_{it} \times LabelPeriod_t$) is the variable of interest identifying the difference-in-difference estimate of any change in purchasing behavior occurring during the labeling period in the treated canteen relative to the control canteens. ($Treat_{it} \times PostPeriod_t$) identifies possible post-intervention effects.

3.2 Data and Results

I include purchase data from April 1st (beginning of the semester) to July 8th (end of the semester) in my analysis. For each purchase, I observe the meal purchased, the price paid, and the location, day, and time of the purchase. I observe whether the purchase is made by a student (81% of purchases) or by an employee (17% of purchases). Further, around 2/3 of sales are made with a personalized payment card, allowing me to track individuals across time.

I drop data from seven days on which the treatment and the larger control canteen did not offer the same main meal components. I also drop all consumption of Ukrainian refugees, who received free meals in the student canteens from week 9 of the sample period. For my main analysis, I additionally drop data from the first week of the label period (week 5), since a "Healthy Campus" week occurred simultaneously and it is not clear whether the carbon labels or this event are driving increased vegetarian consumption.[33] The main results are robust to these exclusions, as discussed more in detail in Section D.1.

The final sample includes 120,093 observations, split between over 6,000 guests. Figure 10 shows how weekly student canteen sales developed throughout this period in the treatment and control student canteens. They follow a similar time trend and it thus seems unlikely that the carbon labels provoked a switch from treatment canteen guests to the control canteen. Section D.1 discusses possible switching in detail, using pre-intervention individual-level purchase data to identify a "home" restaurant and then tracking "non-home" visits throughout the period. There is no clear time trend in switching attributable to the labels, and the proportion of meat purchases made by switchers does not increase throughout the period, which also makes an intervention-motivated switching from treatment to control canteen seem unlikely.

Col. (1) in Table 2 estimates specification 2 in a linear probability model. It estimates that the carbon labels decrease the probability that a purchased main meal component is meat-based by 2 percentage points or 5% of the baseline likelihood. Post-intervention effects are estimated at 7 percentage points or 17% of the baseline likelihood. Figure 11 shows an estimation of weekly treatment effects, using week 4 as the baseline period. Effect sizes during the intervention period seem to increase over time. One explanation for this might be that perhaps canteen guests do not notice the carbon labels immediately, but only on their second or third visit to the student canteen. The large effect estimated for the post-intervention period is similar to that estimated for the final weeks of the intervention period.

Col. (2) of Table 2 drops the $LabelPeriod_t$ and $PostPeriod_t$ time controls and instead includes date-fixed effects. This allows for a more fine-grained control for time trends (e.g. semester times, seasonal trends) and changes in the gastronomic offer, since the offer changes daily but control and treatment canteens coordinate on meal offers). Estimated effect sizes are similar.

Col. (3)–Col. (5) examines whether treatment effects are caused by a change in canteen guests' behavior, as opposed to selection effects. I restrict the sample to canteen guests paying with their individual payment card, visiting the student canteen regularly pre-intervention (at least five times within four weeks) and at least once during the intervention phase, and pre-dominantly visiting the same canteen pre-intervention (at least 80% of pre-intervention visits to the same canteen). Col. (3) applies the same regression specification as in Col. (2) to the restricted sample for comparison

33. See Figure 11.

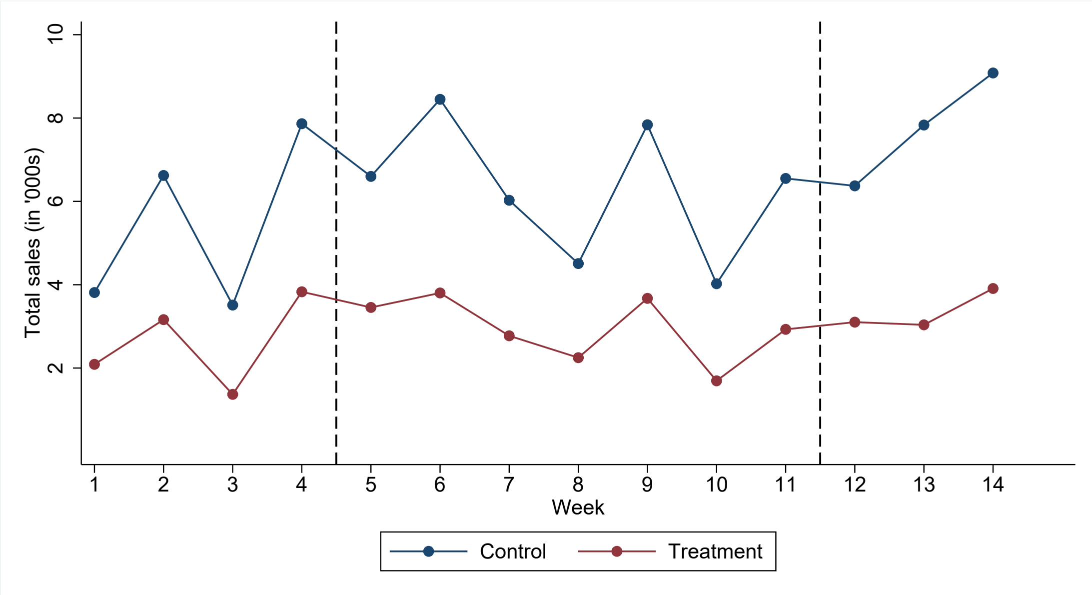


Figure 10. Number of weekly student canteen sales of main meal components, after data cleaning described in Section 3, but including week 5. $N = 130{,}132$. Weeks 1–4 are the pre-intervention period (April 2022), weeks 5–11 are the intervention period (May to Mid-June 2022), and weeks 12–14 are the post-intervention period (last week of June and two weeks of July 22). The drop in sales in week 10 is likely due to the one-week Pentecost holidays, during which no classes took place.

purposes. Col. (4) includes individual fixed effects in the regression, and Col. (5) shows an intent-to-treat analysis: Here, I fix a value of the "Treatment restaurant" indicator for each individual, depending on consumption behavior in the four-week pre-intervention period. For individuals mainly going to the treatment restaurant in the pre-intervention period, "Treatment restaurant" is set to 1, while it is set to 0 for individuals mainly going to the control restaurants during the pre-intervention period.

To assess whether the strong post-intervention effects last, Table E.3 includes data from the semester following the intervention (Oct. 22–Jan. 23) in the difference-in-difference estimation. There is no evidence of this being the case, and the time trends in Figure E.2 suggest—if at all—an upwards-sloping pattern. It, therefore, seems unlikely that treatment effects may have in fact persisted among the canteen guests who visited the student canteen in May 2022, and that my null effects are entirely attributable to incoming new and never-treated students.[34]

Post-intervention effects thus seem rather short-lived, in line with the attention-habit model described in Byrne et al. (2022): The pattern could be explained by the intervention drawing consumers' attention toward the issue of carbon emissions, and consumers making a short-lived habit out of paying attention to the issue. A similar pattern is observed for an attention-directing intervention in the resource conservation context described in Byrne et al. (2022).

Section E discusses additional results. Drawing on a larger data set of consumption data from April 22 to March 23, I identify a rough estimate of how demand for meat meals would react to a carbon tax in the student canteen. Using this estimate, I approximate that a carbon tax of €80 per tonne to €120 per tonne would result in a similar demand reaction as is produced by the carbon labels. This is reconcilable with the assessment that carbon labels are as effective as a carbon tax of €120 per tonne, as elicited in Experiment 1. Results are shown in Section E.0.3. Further, I estimate the decrease in average greenhouse gas emissions caused by the labels at 25g per meal on

34. Unfortunately, I can track individuals' payment card IDs from April to July 2022 or from August 2022 to January 2023, but not across the entire time frame.

Table 2. Field estimates of the effect of carbon labels on meat consumption

	Likelihood of consuming meat					
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment restaurant × Label period	-0.02^{***}	-0.02^{***}	-0.04^{***}	-0.02^{**}	-0.04^{***}	-0.02^{***}
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Treatment restaurant × Post period	-0.07^{***}	-0.07^{***}	-0.11^{***}	-0.04^{***}	-0.12^{***}	-0.07^{***}
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)	(0.01)
Treatment restaurant	-0.10^{***}	-0.10^{***}	-0.02	-0.08^{***}	-0.01	-0.10^{***}
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
Label period	0.01					0.01
	(0.00)					(0.00)
Post period	0.01^{*}					0.01^{*}
	(0.00)					(0.00)
Constant	0.51^{***}	0.48^{***}	0.42^{***}	0.45^{***}	0.42^{***}	
	(0.00)	(0.01)	(0.02)	(0.02)	(0.02)	
Date fixed effects	No	Yes	Yes	Yes	Yes	No
Guest fixed effects	No	No	No	Yes	No	No
Guests control	6,927	6,927	876	876	876	6,927
Guests treated	2,816	2,816	334	334	334	2,816
Observations	120,121	120,121	25,364	25,364	25,364	120,121

Standard errors in parentheses

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

Note: Dependent variable: 0/1 indicator for consumption of the meat option. Col. (1) corresponds to Equation 2. The Constant term describes the proportion of meat meals sold in the Control canteens pre-intervention. Specifications (2)–(5) include date fixed effects to control for the daily changing offer of main meal components, which is common across canteens. The "Post period" and "Label period" indicators are thus dropped due to collinearity. Specifications (3)–(5) restrict the sample to canteen guests paying with their individual payment card, visiting the student canteen regularly pre-intervention (at least five times within four weeks) and at least once during the intervention phase, and pre-dominantly visiting the same canteen pre-intervention (at least 80% of pre-intervention visits to the same canteen). Specification (4) includes individual fixed effects, and specification (5) estimates ITT effects. Specification (6) reports the marginal effects of a probit regression of spec. (1). The standard errors of Col. (1)-(2) are robust. The standard errors of Col. (3)-(5) are clustered at the individual level.

average. This is around 3% of the average emissions of a meal consumed at baseline (Section E.0.3). Examining heterogeneity in treatment effects, I find similar treatment effects when restricting the sample to only employees, only off-peak hours, only payments made by individual payment cards, or only frequent canteen guests. Combining the purchase data with demographic data I elicited in the field surveys described in Section D.1.6, I find suggestive evidence of treatment effects being larger for female guests, for younger guests, and for guests indicating that environmental aspects play an important role in their consumption choices (Section E.0.5). Section E.0.1 draws on survey data to provide suggestive evidence on how the carbon labels influenced canteen guests in the field (e.g. visibility of the labels, effect of the labels on other attitudes).

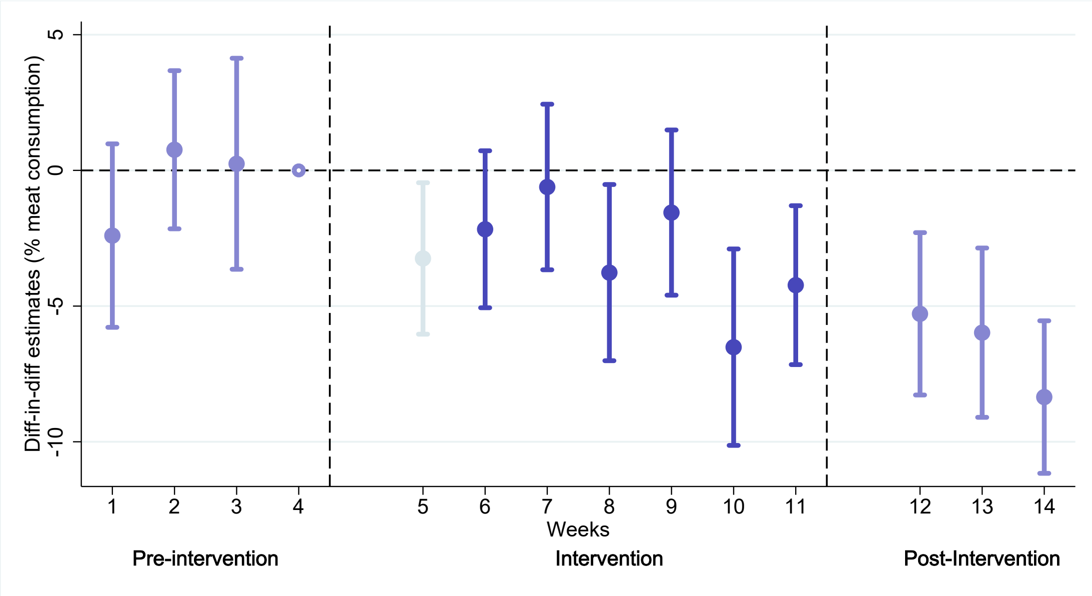


Figure 11. Event study: Difference in difference estimates of the likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1–4 constitute the pre-intervention phase, while weeks 5–11 constitute the intervention phase, and weeks 12–14 the post-intervention phase. The regression specification follows specification (1) in Table 2, but estimates weekly effects and controls for weekly time trends and student canteen offer, as detailed in regression table E.1. Figure E.2 shows an event plot without controls for student canteen offer. Week (5) is excluded from the main estimation in Table 2, because effects cannot be clearly attributed to the carbon labels, as described more in detail in Appendix D.1. Bars indicate 95% confidence intervals.

4 Structural Model

To provide insights into the behavioral mechanisms driving consumers' responses to carbon labels, I introduce a simple discrete choice model of meal selection in this section. In section 6 I will report model parameters using data from Experiment 3 which will be described in Section 5.

In the model, a consumer chooses from a set of meals and selects the meal that maximizes her perceived utility. In general, the perceived utility of a meal may depend on a multitude of meal attributes. The main attribute of interest in this model is the consumers' expectation of the carbon emissions caused by each meal. Ceteris paribus, the consumer has a higher valuation for a meal that causes fewer carbon emissions. How much the consumer cares about emissions depends on two parameters: the salience of carbon emissions at the moment of choice and the guilt the consumer perceives per kg of carbon emitted.[35]

4.1 Model

There is a finite set of meals $\mathscr{M}$ and a single consumer. The consumer chooses a meal $m \in \mathscr{M}$ which maximizes her *perceived utility*

$$u(m) = v_m - p_m - \theta\gamma e_m. \tag{3}$$

35. Instead of speaking of guilt, one can also re-formulate the model for the consumer to experience warm glow for every kg of emissions less caused by the chosen option relative to the option highest in emissions. Results would only differ in the interpretation of the parameter γ in the structural estimation.

Here, v_m is the *consumption utility* of meal m that is independent of emissions[36], p_m is the *price* of meal m, and e_m is the consumers *estimate of emissions* caused by meal m at the moment of choice.[37]

The *salience* of carbon emissions $\theta \in [0,1]$[38] and the consumer's *environmental guilt per perceived kg of emissions* γ jointly determine how much weight the consumer puts on carbon emissions when deciding.

The consumer's prior estimate of emissions caused by meal m is denoted by e_m^{prior}, which may differ from the true emissions, denoted by e_m^{true}. If the consumer is *informed*, her updated estimate of emissions is

$$e_m^{\text{info}} = (1-\kappa)e_m^{\text{true}} + \kappa e_m^{\text{prior}}. \tag{4}$$

Hence, the parameter $\kappa \in [0,1]$ is a measure of the stickiness of the consumers' prior estimate of emissions, e.g. due to a lack of trust in the carbon footprint information provided.[39] If the consumer is *attentive* to emissions, this sets $\theta = 1$.[40] Introducing *carbon labels* makes the consumer both informed and attentive.

4.2 Identification of Parameters

The setting of experiments 1 and 3 corresponds to a special case of the model with a binary choice set $\mathscr{M} = \{m, o\}$ with m being the meal option and o being the outside option of a cheese sandwich. The willingness to pay to exchange meals corresponds to

$$u(m) - u(o) = v_m - v_o - \theta\gamma(e_m - e_o),$$

where the values of θ, e_m and e_o depend on the treatment condition. The parameters θ, γ, and κ can be estimated from Experiment 3 data. I directly elicit e_m^{prior} and e_o^{prior}, as participants guess carbon footprints at the start of the experiment. Further, the treatment conditions yield four equations with four unknowns[41] as follows. First, in the absence of any treatment (elicitation at baseline), participants' willingness to pay is

$$WTP^B = v_m - v_o - \theta\gamma(e_m^{\text{prior}} - e_o^{\text{prior}}) \tag{5}$$

where I assume $\theta \in [0,1]$. The treatment condition, Attention directs participants' attention towards carbon emissions without providing information. Assuming this sets $\theta = 1$,

$$WTP^A = v_m - v_o - \gamma(e_m^{\text{prior}} - e_o^{\text{prior}}) \tag{6}$$

Presenting carbon labels directs participants' attention towards carbon emissions, but also provides information on true carbon emissions. I assume this sets $\theta = 1$ and the participant updates as described in equation 4. In Experiment 3, participants seeing carbon labels experience the Attention treatment on top of the Label treatment. This direction of attention has no effect on top of the

36. For the purposes of this paper, it is sufficient to consider v_m as being exogenously given for each meal. However, one can also think of v_m being derived from a vector of other observable attributes x_m and an unobservable taste shock ε_m, so that $v_m = \beta^T x_m + \varepsilon_m$.

37. Similar to Imai et al. (2022) I assume in this formulation that consumers' perceived utility is additively separable in v_m and perceived environmental guilt.

38. I hereby use a similar formulation as used in the literature on attentiveness to taxes and resource consumption (Chetty, 2009; DellaVigna, 2009; Byrne et al., 2022)).

39. The above formulation leans on the evidence-informed framework proposed by Epstein, Noor, and Sandroni (2008) to model non-Bayesian updating. Bouchaud et al. (2019) use the same updating rule to study under-reaction in financial markets.

40. This is just a normalization, for any other value $x > 0$ under attention, one could redefine $\theta = \theta/x$ and $\gamma = \gamma x$.

41. I treat $v_m - v_o$ as a single parameter in the estimation, i.e. I only identify the difference and not the individual values of v_m and v_o. e_m^{prior},e_o^{prior} are directly elicited, and e_m^{true} and e_o^{true} are known.

direction of attention induced by the carbon labels, [42] and willingness to pay indicated in the ATTENTION+LABEL condition can thus be described as

$$WTP^{A+L} = v_m - v_o - \gamma\left(\kappa e_m^{\text{true}} + (1-\kappa)e_m^{\text{prior}}\right) \tag{7}$$

where I assume $\kappa \in [0,1]$. The treatment condition ATTENTION+OFFSET removes the carbon emissions caused by both meal options. Assuming this sets $\theta = 1$, and $e_m = 0$:

$$WTP^{A+O} = v_m - v_o \tag{8}$$

5 Experiment 3: Behavioral channels

Experiment 3 provides framed field experiment evidence on the respective relevance of each of the two behavioral channels proposed in the theoretical model in Section 4. Subsection 5.1 describes the experimental design. Subsection 5.2 describes data and reduced-form results. Experiment 3 data is also used to estimate the parameters of the structural model, as detailed in 4.2. Results of the structural estimation are discussed in Section 6.

5.1 Experimental design

Overview. The theoretical framework in Section 4 proposes that carbon labels impact consumers by making consumers 1) informed, and 2) attentive. To investigate the relevance of each of the two channels, I conduct a framed field experiment similar to Experiment 1 apart from two key differences:

(1) To identify the extent to which an information effect drives consumers' reactions to carbon labels, I track participants' initial estimates of meals' carbon footprints. In the reduced-form analysis, I compare initial misperceptions with participants' reactions to carbon labels.

(2) To identify the extent to which an attention effect drives consumers' reactions to carbon labels, I include a separate experimental condition increasing attention towards carbon emissions without providing any information on carbon footprints. In the reduced-form analysis, I estimate treatment effects for this condition.

Experiment timeline. The experiment timeline is visualized in Figure 12. It proceeds very similarly to Experiment 1. Recall that in Experiment 1, experiment participants answer guessing questions on unrelated items after completing the four baseline purchase decisions (e.g. on the length of a popular running route in Bonn). In contrast, Experiment 3 participants do not answer these questions, but instead, guess the carbon footprints of different meals. These questions concern the four meals around which the meal purchasing decisions revolve, as well as six further meals (see Figure 14 for a list). Participants answer each of the ten guessing decisions on separate screens, shown to participants in a random order. On each screen, they are shown the emissions of the same reference example meal (Red Thai Curry with pork and rice, causes 1.7 kg of CO_2). This reference meal is not included in any willingness to pay elicitations. An example of a guessing screen is shown in Figure 13 and section B.1 shows screenshots of the guessing instructions. The guessing questions are incentivized and timed as in Experiment 1.

42. Specifically, I assume an ATTENTION+LABEL, LABEL and ATTENTION treatment would all set salience $\theta = 1$, without any additional attention-directing effect occurring from a combination of treatments. This assumption is in line with a comparison of effect sizes across experiments 1 and 3, where I see similar treatment effects across the LABEL treatment in Experiment 1 and the ATTENTION+LABEL treatment in Experiment 3. These are shown side-by-side in Tables C.6 and C.7.

The experiment then proceeds differently depending on the treatment group participants are assigned to by computer randomization. All participants are again asked to indicate their willingness to pay for the four meals, but the framing of the decision and some characteristics of the decision depend on the treatment condition:

- In the Attention condition, the willingness to pay elicitation is exactly as in the first, baseline elicitation. However, since participants completed the carbon footprint guessing task between the two elicitations, they have now spent time thinking about the issue of greenhouse gas emissions, and are thus arguably more attentive.
- In the Attention+Label condition participants are now shown carbon labels when indicating their willingness to pay. An example is shown in Figure 4. They are thus attentive and informed.
- In the Attention+Offset condition, participants are informed that the emissions caused by their lunch choice (be it the meal or the sandwich) will be offset.[43]

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[44], with partly changed treatment conditions:

- Participants previously in the Attention+Label condition are now assigned to the Attention+Offset condition and vice versa.
- Participants previously in the Attention condition remain in the Attention condition.

The experiment then proceeds as in Experiment 1. The design of the meal purchase decisions and their incentivization, as well as the incentivization of the elicitation of willingness to pay for seeing carbon labels, is as in Experiment 1.

Participants and set-up. 444 experiment participants are recruited from the participant pool of the BonnEconLab, the behavioral experimental lab of the University of Bonn, to participate in one of 12 experimental sessions taking place between the 22nd of June and the 8th of July 2021. I pre-registered experiment design, sample restrictions, the analysis shown in Figure 16 and Table 4, and, roughly, the structural estimation.[45] Participant invitation and experiment set-up are as in Experiment 1.

5.2 Data and results

I exclude the 3% fastest participants and participants not passing the comprehension check after five attempts, as pre-registered [46]. The remaining 444 participants are computer-randomized into treatments. Section C.1 shows a randomization check. Participants are on average 26 years old, 55% are female, 70% are students and 24% are vegetarians. The sample is roughly representative of regular student canteen guests in terms of these characteristics, as discussed in Section C.2, and results hold when restricting the sample to only students or only non-vegetarians, as shown in Section C.5.

43. The results of the Offset condition are not further discussed in this section, details are described in section B.0.3 and results are shown in Table C.7. The Offset condition serves as input for the structural estimation described in Section 6, as detailed in Section 4.2.

44. In the analyses, I control for whether observations stem from a third-round elicitation. All the main results replicate including only data from the first two rounds.

45. (Schulze Tilling, 2021b)

46. Schulze Tilling (2021b)

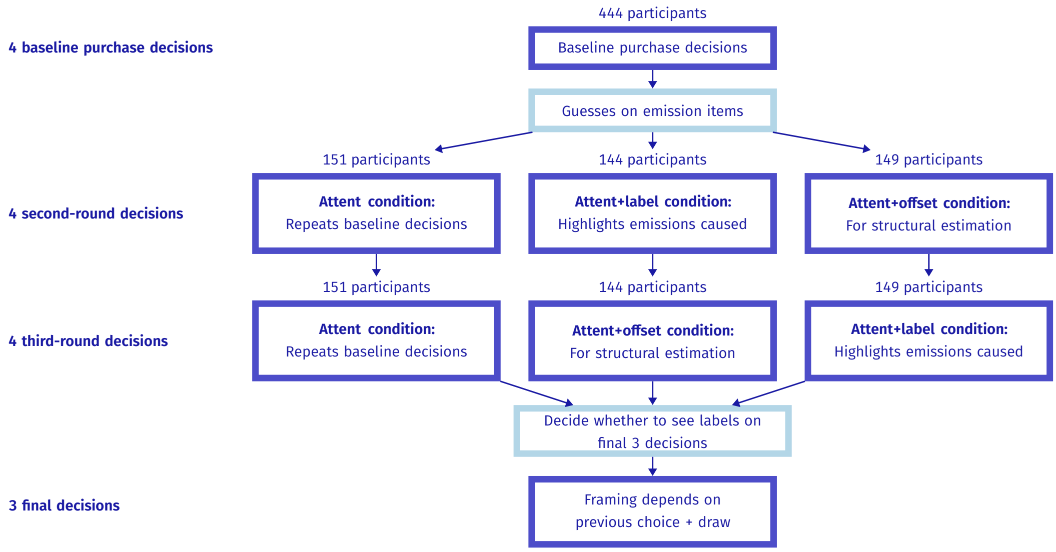


Figure 12. Experiment schedule and treatment groups. Participants repeat the same four meal purchase decisions three times, with the decision framing differing across rounds. Treatments are described in more detail in the "Experiment timeline" paragraph above. The results of the ATTENTION+OFFSET condition are not further discussed in this section, details are described in section B.0.3, and results are shown in Table C.7. It is used as input for the structural estimation described in Section 6.

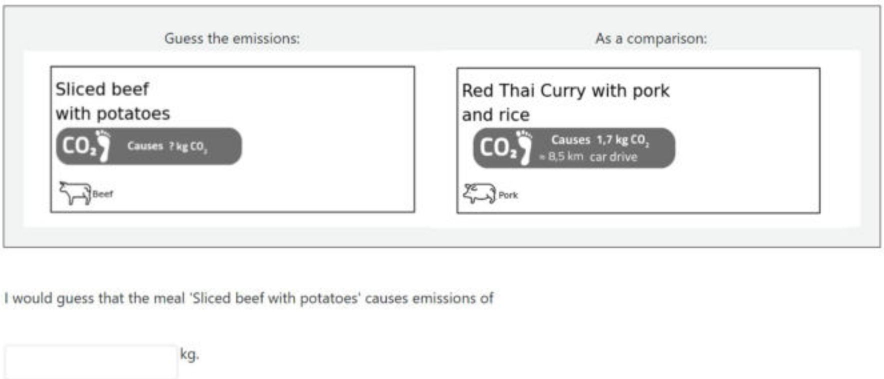


Figure 13. Example guessing questions. After completing the baseline purchase decisions and before the second round of decisions, all participants answer incentivized guessing questions in which they estimate the carbon footprint of ten different meals. The carbon footprint of the meal Red Thai Curry with pork and rice is always shown as a reference meal. Participants do not learn the carbon footprint of any other meal at this stage of the experiment.

The effect of carbon labels by previous estimation. All participants in Experiment 3 are asked to guess the carbon footprints of different meals. Figure 14 displays how average guesses deviated for each of the meals. On average, participants rather underestimate emissions (green-colored dots) for high-emission meals and overestimate emissions for low-emission meals (red-colored dots). Section C.11 shows further descriptive statistics on under- and overestimation of emissions, such as a comparison of the number of under- and overestimations by meal and participants, as well as the accuracy of the ranking of meals by carbon footprint which can be inferred from participants' guesses.

In the next step of the analysis, I combine individual and meal-specific treatment effects with participants' emission estimates for the respective meals. I estimate

$$Diff_{ijm} = \alpha + \delta_1 Under_{im} + ThirdRound_j + \varepsilon_{ijm} \tag{9}$$

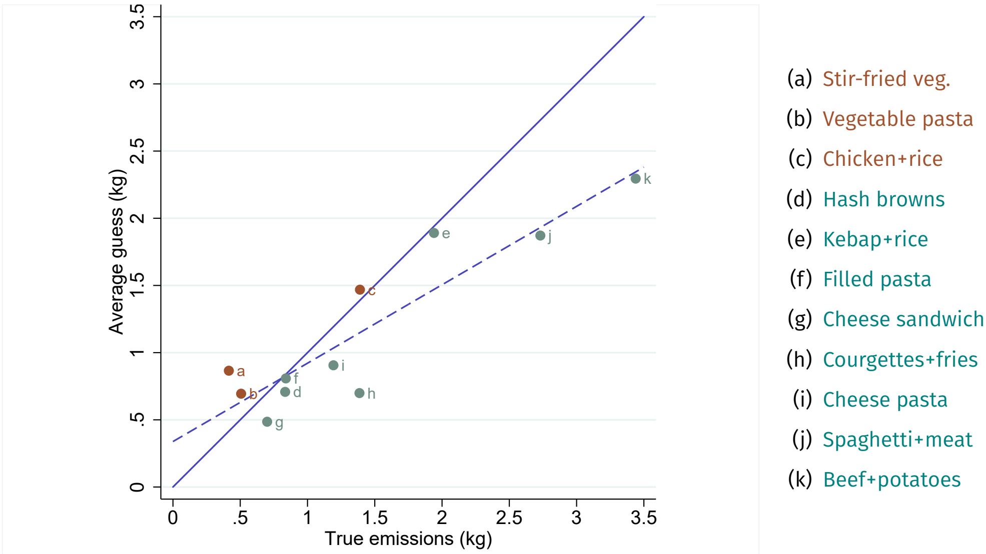


Figure 14. Average guess of the emissions caused by a given meal, plotted against true emissions. Values closer to the solid line are more precisely estimated. Meals corresponding to orange scatter points are on average overestimated in their emissions, while meals corresponding to green scatter plots are on average underestimated. The dashed fitted line is described by $y = 0.39 + 0.57x$, with both the intercept and the coefficient significant at $p < 0.01$. Values are based on guesses made by the participants of Experiment 3. Further, the 71 participants in the "Control, then Control" group in Experiment 1 also estimated greenhouse gas emissions towards the end of the experiment (see Section 2 for details). This data is included in this graph, but not in any other analyses shown in this section. The meal "Spaghetti with meat" was only guessed by the 71 participants of Experiment 1 guessing emissions. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission value) are dropped. This leaves a total of 4,261 observations made by 490 participants.

where $Diff_{ijm}$ describes the difference between willingness to pay of individual i in round j for meal m and individual i's baseline willingness to pay for meal m, as in Experiment 1.[47] I estimate this specification including only data from the Attention+Label condition. Thus, my dependent variable directly captures subject- and meal-specific treatment effects for carbon labels. $Under_{im}$ is an indicator of whether the individual underestimated the difference in emissions between meal m and the cheese sandwich.[48] I calculate this indicator by comparing the difference between the individual's guess for the emissions of meal m and her guess for the cheese sandwich with the true difference in emissions. $ThirdRound_j$ is an indicator of whether it was the third round of decisions.

Table 3, Spec. (1) shows the results of the OLS estimation of equation 9. If an individual underestimated the emissions of meal m relative to the cheese sandwich, presenting her with carbon labels on average leads to her decreasing her willingness to pay by an additional €0.13. This suggests that part of the effect of the labels can be explained through a correction in misperceptions on carbon impact: The labels inform participants that the meal has a higher relative carbon footprint than they previously expected, and they react accordingly. Spec. (2) in Table 3 does not group observations by previous under- or overestimation but instead regresses the change in willingness to pay on the degree of underestimation (in kg). This specification suggests that seeing labels on average decreases willingness to pay by €0.16, with an additional decrease of €0.07 for each kg by which emissions were underestimated.

47. Please see Section 2.2 and C.7 for details on this specification.
48. This refers to the signed, not the absolute difference. For example, if a meal causes 0.2 kg of emissions more than the cheese sandwich, and the participants estimate that the meal causes 0.3 kg of emissions less than the cheese sandwich, this is an underestimation of the difference in emissions.

The large negative constant term in both specifications is striking. In spec. (1), a decrease in willingness to pay of €0.10 is independent of a previous underestimation of emissions. Spec. (2) estimates a decrease in willingness to pay independent of previous underestimation of €0.16. Figure 15 shows average effects split by previous under- or over-estimation of emissions and visualizes that participants on average also significantly adjust their willingness to pay downward for meals for which they previously *over*estimated emissions. In these cases, the labels inform participants that the meal has a lower relative carbon footprint than they previously expected. If a correction of misperceptions were the sole effect induced by the label, one would expect participants to adjust their willingness to pay upwards in such a situation, and not downwards. The pattern we see in Figure 15 is thus evidence against this being the case and in favor of a second mechanism driving treatment effects.

I replicate the analysis in Figure 15 including (a) only individuals who did an above-average job at guessing the relative emissions of at least three of the four meals correctly (Figure C.4 in the Appendix) and (b) only individuals who did an above-average job in guessing emission magnitudes (Figure C.6 in the Appendix). Patterns look similar to Figure 15.

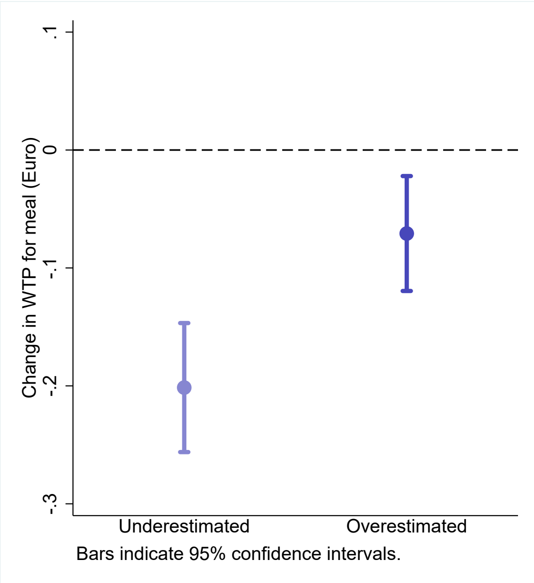


Figure 15. Within-subject change in willingness to pay for meals when shown carbon labels, depending on whether the participant previously over- or under-estimated the difference in emissions between the specific meal and the cheese sandwich. Participants are all in the ATTENTION+LABEL condition. Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	-0.13^{***}	
	(0.04)	
Underestimation (in kg)		-0.07^{***}
		(0.02)
Control for third round	0.05	0.07
	(0.05)	(0.05)
Constant	-0.10^{***}	-0.16^{***}
	(0.04)	(0.03)
Participants	293	272
Obs. underestimate	555	515
Obs. overestimate	562	494
Observations	1,117	1,009

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3. Dependent variable: within-subject change in willingness to pay for a meal when shown carbon labels, compared to baseline. Includes only participants in the ATTENTION+LABEL condition. Spec. (1) follows Equation 9.: treatment effects of the carbon label are split into a constant effect and the additional effect of previous underestimation. In spec. (2), change in willingness to pay is regressed on underestimation in kg. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped. Standard errors are clustered at the individual level.

The effect of directing attention. In the next step of the analysis, I include data from the ATTENTION and ATTENTION+LABEL conditions in the analysis to estimate the magnitude of a possible attention effect. I estimate

$$Diff_{ijm} = \alpha + \beta_1 High_m + \beta_2 Low_m + \delta_1 (Label_{ij} \times High_m) + \delta_2 (Label \times Low_m) + ThirdRound_j + \varepsilon_{ijm} \quad (10)$$

where $Diff_{ijm}$ is defined as above, and $High_m$ and Low_m are indicators for meal m's footprint relative to the cheese sandwich, while $Label_{ij}$ is an indicator for whether individual i sees carbon labels in round j, additionally to being made attentive.

Results are shown in Table 4, Figure 16 illustrates average changes in willingness to pay for the ATTENTION and the ATTENTION+LABEL treatment. Simply directing attention towards carbon emissions decreases willingness to pay for high-emission meals by €0.08, on average. Providing labels on top of increasing attention leads to an additional decrease of €0.10 for high-emission meals. The decrease in willingness to pay for high-emission meals in the ATTENTION condition is driven by decisions for which participants had a relatively good idea of the emissions caused by the meal in question. This is visualized in Figures C.9 and C.10 in the Appendix.

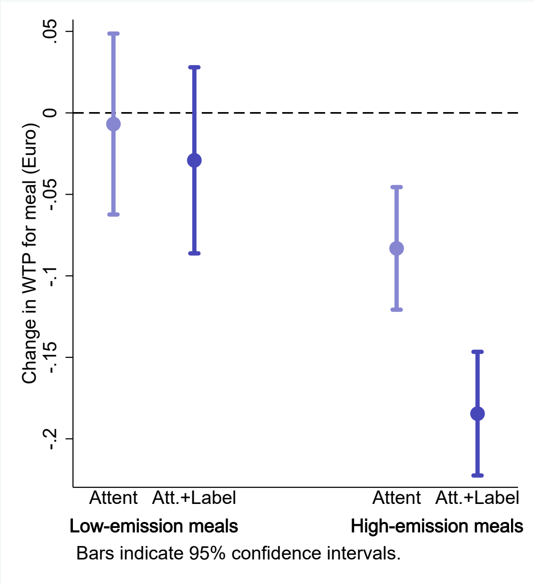


Figure 16. Within-subject change in willingness to pay for meals, comparing participants in the ATTENTION and ATTENTION+LABEL condition. Effects are split into meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline
	(1)
High emission meal x Shown label	-0.10^{***}
	(0.04)
Low emission meal x Shown label	-0.02
	(0.04)
High emission meal	-0.10^{***}
	(0.03)
Low emission meal	-0.02
	(0.03)
Control for third round	0.03
	(0.02)
Participants attent	151
Participants label	293
Observations	2,380

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4. Dependent variable: within-subject change in willingness to pay for a specific meal when made attentive. Spec. (1) corresponds to Equation 10 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. "High emissions meal" describes the pure effect of being made attentive, "High emissions meal x Shown Label" the additional effect of seeing information. Includes data from participants in the ATTENTION and ATTENTION+LABEL condition. Standard errors are clustered at the individual level.

6 Structural estimation

The structural estimation complements the reduced-form results from Section 5.2. Section 6.1 estimates the parameters of the theoretical model described in Section 4 using data from Experiment 3. Section 6.2 then simulates the effects of different types of interventions in the student canteen context and compares effects on carbon footprints and consumer welfare. This allows for a direct comparison of the importance of misperception-correcting and attention-directing effects of carbon labels identified in Section 5.2.

6.1 Results

I rewrite the four equations in Section 4.2 for the structural estimation, as shown in Section A.3, and estimate parameters with GMM. I assume that the parameters γ, κ, and θ are homogeneous across participants.

Results are shown in Table 5, Col. (1). θ, the average attentiveness to greenhouse gas emissions in the absence of carbon labels, is estimated at 16%. This estimate implies that on average, individuals in my study react to the carbon footprint they perceive as if it was only 16% its size. The estimate is not significantly different from zero, implying that the true level of attentiveness might also be zero. This would imply that individuals do not react at all to the perceived carbon footprint in the absence of any intervention.

κ, the stickiness of the average consumers' prior estimate of a meal's carbon footprint, is estimated at 0.21 and insignificant. This suggests that individuals on average place a relatively large weight $(1-\kappa)$ on the carbon footprint information shown on the carbon labels when revising their carbon footprint estimate upon seeing the labels.

γ describes how the emissions of one kg of greenhouse gas emissions affect an individual's utility. This is estimated as a decrease in monetized utility of Euro 0.12 per kg of emissions caused by the meal chosen, i.e. individuals on average experience guilt equivalent to a monetary cost of 0.12 per kg of perceived emissions.

Columns (2)–(6) show that estimates are similar in alternative specifications of the model. In column (2), I re-estimate the model imposing that $\kappa = 0$, i.e. that individuals completely trust the emissions information. In column (3), I re-estimate the model imposing that $\theta = 0$, i.e. that individuals are completely inattentive to carbon emissions in the absence of an intervention. In column (4), I impose $\theta = \kappa = 0$. In column (5), I impose $\theta = 1$, assuming that consumers are fully attentive to carbon emissions, even in the absence of labels.

To provide an estimate of the effect carbon labels have on consumer welfare, I expand the theoretical model to make predictions on the labels' effect on consumer welfare, as detailed in Section A.1. Essentially, I assume that consumer welfare is a function of the true—and not the perceived—emissions caused by the meal consumed. Thus, the carbon labels by construction increase consumer welfare by helping consumers make the choice that maximizes consumer welfare. I also assume that carbon labels have a psychological effect on consumers independent of their effect on consumption decisions. The sign of this fixed effect F is a priori undetermined and may reflect psychological costs or benefits accruing to consumers as a result of seeing the carbon labels. In the estimation shown in column (6) I add a fifth equation describing these effects to my GMM estimation and include participants' willingness to see or avoid labels on their final three consumption decisions in the estimation. Through the lens of the model, I interpret these values as an estimate of the labels' effect on consumer welfare, taking a similar interpretation as e.g. Allcott and Kessler (2019) and Butera et al. (2022). This allows me to estimate F. I estimate this figure at the monetary equivalent of €0.21 and significantly different from zero, suggesting that consumers on average experience a psychological benefit from seeing the carbon labels independent of their effect on consumption decisions.

Table 5. Structural estimates of model parameters

	(1)	(2)	(3)	(4)	(5)	(6)
Theta	0.16	0.03				0.18
	(0.18)	(0.17)				(0.17)
Gamma	−0.12***	−0.10***	−0.10***	−0.10***	−0.12***	−0.12***
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Kappa	0.21		0.12		0.12	0.23
	(0.20)		(0.19)		(0.21)	(0.20)
F						0.21***
						(0.01)
Observations	3,216	3,216	3,216	3,216	3,216	3,216

Standard errors in parentheses

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

Note: Analysis is based on data from Experiment 3. For each meal, the observations corresponding to the 10% most extreme guesses (in terms of deviation from the true emission value) are dropped. Regression does not include a constant, since the estimation follows the model outlined in Section 2. Column (1) shows the main estimation, based on equations A.9, A.8, A.10. Columns (2)–Column (7) each modify the model in Column (1) as follows: Column (2) imposes $\kappa = 0$. Column (3) imposes $\theta = 0$. Column (4) imposes $\theta = \kappa = 0$. Column (5) imposes $\theta = 1$. Column (6) includes equation A.7 in the estimation.

6.2 Intervention comparison based on estimated parameters

In the model described in Section 4, introducing carbon labels affects consumers by making them both informed and attentive. Using estimated parameters, I can compare the importance of each of these two effects in driving consumers' responses to carbon labels. I simulate how experiment participants would react to different interventions in the student canteen context: 1) a KNOWLEDGE intervention making them informed, but not attentive, 2) an ATTENTION intervention making them attentive, but not informed, and 3) a LABEL intervention making them both attentive and informed. This simulation is based on participants' tastes for different student canteen meals as elicited in Experiment 3, participants' prior estimates of emissions as elicited in Experiment 3, my estimates of θ, γ, and κ which I assume are homogenous across participants, the model specification shown in Section 4, and some assumptions on what constitutes a typical student canteen offer and pricing structure. These assumptions and the simulation are discussed in more detail in Section A.4. Table 6 shows simulation results.

For all three interventions, the interventions do not impact the vast majority of consumption decisions, with 98% to 99% of consumption decisions not affected by the interventions. This intuitively makes sense—Interventions will typically only affect decisions that were at the margin, to begin with. This is in line with my findings from the natural field experiment (Experiment 2) in which the labeling intervention also affects only 2% of consumption decisions, and correspondingly leaves 98% of consumption decisions unaffected. Participants' valuation for the student canteen meals in Experiment 3 is, in over 70% of cases, lower than the student canteen price. This is also in line with observations from the field experiment that an average student canteen guest does not visit the student canteen every day. An average student canteen guest visits the student canteen 20 times during the 14-week sample period, i.e. on 29% of possible occasions. On the remaining 71% of occasions, he will also opt towards an alternative lunch (e.g. taking a sandwich with him).

The ATTENTION, KNOWLEDGE, and LABEL intervention all decrease the consumption of the meat option. In the ATTENTION and KNOWLEDGE intervention, consumption of the cheese sandwich increases. In the LABEL intervention, consumption of the cheese sandwich and the vegetarian option increases. The ATTENTION intervention decreases the carbon footprint of an average meal by 27 grams, while the KNOWLEDGE intervention decreases carbon by 4 grams, and the LABEL interven-

Table 6. Estimated effect of different policies in the student canteen

	# of choices			Δ GHGE	Δ consumer welfare			
Intervention	sandwich	veg.	meat	Average	Average	SD	Min	Max
None	73.1%	18.1%	8.8%					
Attention	74.4%	18.1%	7.4%	-.0267	.0010	.0160	-.0849	.2456
Knowledge	73.8%	18.1%	8.1%	-.0036	.0001	.0043	-.0657	.0583
Labels	74.1%	18.6%	7.3%	-.0338	.0018	.0164	-.0022	.2456
Carbon tax	74.4%	18.7%	7%	-.0423	.0017	.0653	-.3130	.2626
Meat ban	78.3%	21.7%		-.1473	-.0350	.1728	-1.3935	.2456
Beef ban	78.3%	20.4%	1.4%	-.0800	-.0128	.1047	-1.3827	.2456

Note: Estimated change in consumption choices, consumption utility, and greenhouse gas emissions which would be caused by different types of interventions. Change in utility is in €per meal, and change in greenhouse gas emissions is in kg per meal. Simulations are based on estimated model parameters, experiment data, and canteen offer and price structure.

tion decreases carbon by 34 grams. The average effect of the ATTENTION intervention is thus around 7-fold that of the KNOWLEDGE intervention. Further, there are some synergies between the ATTENTION and KNOWLEDGE intervention, leading to the LABEL intervention producing a greater decrease in emissions than the sum of its parts.

In the extension of my model to consumer welfare specified in Section A.1, consumer welfare resulting from a meal choice is a function of the true—and not the perceived—emissions resulting from the meal choice. Carbon labels thus, by moving perceived emissions closer to true emissions, increase the likelihood of a consumer choosing the option maximizing his welfare. The final four columns of Table 6 estimate how consumer welfare changes accordingly under each of the interventions. Importantly, these estimates account for the fact that a change in meal choice also leads to a change in consumption utility. For example, if a consumer switches from a meat to a vegetarian meal as a result of the label, but enjoys the taste of the meat meal more, the calculations account for this. They are thus considerably lower than a mere multiplication of the average reduction in greenhouse gas emissions with the average guilt perceived per kg of emissions.

I estimate that carbon labels on average improve consumer welfare by the monetary equivalent of 0.16¢per choice. Synergies between the ATTENTION and KNOWLEDGE intervention are more sizable here, with the effects of the other two interventions merely summing to 0.1¢. Section A.5 examines the distribution of welfare effects. Both the ATTENTION and the KNOWLEDGE intervention in some cases result in considerable decreases in consumer welfare. This can be the case if a consumer with large misperceptions of carbon impact is made attentive, or if a consumer who generally overestimates emissions and is very inattentive towards emissions is made knowledgeable of emissions. Welfare changes are thus in both cases more dispersed than for the LABEL intervention.

To provide comparability with other possible policy interventions, I also estimate the impact of a carbon tax of €120 per ton.[49] I assume that the proceeds from this tax are uniformly redistributed among individuals.[50] I estimate that such a measure would lead to a higher decrease in average carbon footprint than the carbon labels—42 g per meal vs. 34 g per meal—and a similar increase in consumer welfare. I also examine a meat ban, which would lead to much higher emissions reductions (147 g per meal) but also to a higher loss in consumer welfare. Importantly, these estimations assume that restaurant guests have no choice but to eat at the student canteen. The emission savings are thus rather an upper bound estimation and welfare effects a lower bound estimation.

49. I use a value of €120 per tonne for comparability with framed field experiment 1 results.
50. Whether and how the tax proceeds are redistributed among consumers does not affect the amount of greenhouse gas emissions avoided, but affects consumer welfare estimations.

7 Consumer preferences for the presence of carbon labels

This section discusses experimental evidence of consumers' preferences for the presence of carbon labels in their consumption decisions. Section 7.1 discusses evidence from experiments 1 and 3, and Section 7.2 discusses evidence from Experiment 2. Section 7.3 discusses possible determinants of consumers' willingness to see or avoid carbon labels.

7.1 Evidence from the framed field experiments

In both framed field experiments, participants indicate their willingness to pay for carbon labels being present or absent during their final set of consumption decisions. These elicitations are incentivized as described in Section 2. The frequency distribution of willingness to pay values is visualized in Figure 17. About 50% of participants have a willingness to pay of 0, meaning they have no strong preference for the presence or absence of labels. Less than 5% have a negative willingness to pay, meaning they prefer the labels being absent. The remaining participants are willing to pay for the presence of labels, with 21% of the sample willing to pay €0.50 and above. Values barely differ between treatment groups, although willingness to pay seems to be slightly higher among those who have not yet seen labels in the course of the experiment, as shown in Table C.32.

Table 7 shows a correlation analysis between willingness to pay for the presence of carbon labels and individual characteristics. Willingness to pay for seeing labels is strongly positively correlated with participants' approval of carbon labels being shown in the student canteen and participants' interest in using this information. It is also weakly positively correlated with participants' perceived strength of social norms for avoiding carbon emissions in food consumption, as measured by adapting the procedure developed by Krupka and Weber (2013). Further, participants' self-control in eating behavior (as elicited using the questionnaire developed by Haws, Davis, and Dholakia (2016)) is not correlated with willingness to pay to see emission values. This implies that the concerns Thunström (2019) identified for calorie labels—They might take a particularly high emotional toll on individuals with low self-control—do not seem to play a meaningful role in the context of carbon labels.

Remarkably, participants' reactions to carbon labels are strongly positively correlated with their willingness to pay for the presence of carbon labels (Table C.33). Participants who react strongly to the labels also have a stronger preference for seeing them in their decisions.

7.2 Evidence from the natural field experiment

After the natural field experiment is completed, student canteen guests are asked in a follow-up survey whether they would like the labels to be installed permanently. The details of this survey and the measures I took to limit non-response bias are described in Section B. 73% of the 234 participants are in favor of installing the labels permanently, 18% are not sure, and 9% against the measure. A revenue-neutral carbon tax of an unspecified amount[51] , in contrast, is only favored by 60% of students, while 14% do not know and 26% are against. Carbon labels thus seem to enjoy greater support than carbon taxes, making an implementation more feasible.

7.3 Discussion of possible preference drivers

Subsections 7.1 and 7.2 show evidence of a positive reception towards carbon labels. In the framed field experiment setting, 95% of participants would either like to see the carbon labels or are indifferent. In the natural field experiment, 91% of survey participants expressed a preference for or no

51. Specifically, I asked survey participants if they would be in favor of canteen prices being in line with the carbon labels (green-labeled meals being least expensive, red-labeled meals being most expensive).

clear opinion towards carbon labels. Importantly, these indications can not be described as "cheap talk" in either of the two settings. In the framed field setting, participants' responses directly influence the presentation of carbon labels on their final choices and affect their compensation in the experiment. In the natural field setting, survey participants expect the survey results to be communicated to the student canteen and to impact the future presence of the carbon labels.

These results raise the question of why consumers have a preference for the presence of carbon labels and how we should interpret these results. The evidence suggests that consumers benefit from the presence of carbon labels. Instead of incurring a net psychological cost from the labels, they rather seem to derive a net psychological benefit. Why might this be the case? First, consumers might find the information itself intriguing, offering insights into the environmental impact of different food choices. Second, consumers might notice that they are more prone to take the environmentally friendly option in the presence of carbon labels and choose to see carbon labels as a type of commitment device. The carbon labels then remind them of self-set goals to decrease emission-heavy consumption. Third, consumers might appreciate that the labels help them make the environmentally-friendly choice, providing them to experience a feeling of "warm glow" or avoid a feeling of "cold prickle". Fourth, for those already inclined towards eco-friendly choices the presence of the labels might amplify the experience of warm glow. All of these four dynamics fit well into the model's framework. The first two factors relate to costs or benefits created by the labels independent of their impact on consumption behavior. The third factor relates to increased utility from label-influenced choices, while the fourth factor relates to increased attentiveness towards emissions, θ, increasing the experienced intensity of warm glow for carbon-friendly (or cold prickle for emission-heavy) choices.

A fifth factor might be that consumers see benefits in other consumers seeing the carbon labels. I do not consider this factor in the structural model, since participants' decisions in the framed field setting are anonymous and their decision on the presence or absence of carbon labels only influences whether they see the labels, and does not impact anyone else. The setting is thus not apt to estimate such a parameter. In the natural field setting, however, a consumer might be in favor of carbon labels since he believes that this will lead to other consumers changing their behavior. Consumers might believe that the carbon labels make the social norm more salient to themselves and to other consumers. In many cases, consumers might derive utility from behaving according to the norm. Thus, all of these interpretations speak towards the carbon labels creating a benefit for consumers.

One might argue that behaving according to the norm can also, in some cases, decrease utility for consumers. Consumers might feel observed in their choices and thus—out of social pressure—make the socially desirable choice, although it is not in line with their true interests. In the framed field experiment, this is a bit difficult to imagine: Participants do not know the experimenter, make their choices anonymously, and suffer real consequences if their choice is not in line with their true interests. This interpretation would also imply that they are willing to pay money to see the carbon labels on their final choices to "look good" in front of an experimenter they do not know and who will only look at their choices in an anonymized manner. In the natural field experiment, the interpretation seems more plausible: The carbon labels make the socially desirable choice (as designated by the student canteen) visible to all canteen guests and guests may fear being judged by other canteen guests if they choose differently. They may be making consumption choices that do not maximize their true utility, and suffer a utility loss through the labels. In the survey I conduct in the field, they however have the chance to change their fortune: By indicating a preference against the carbon labels being continued, they can alleviate the social pressure and return to the consumption choices maximizing their true utility. Thus, if these forces were driving consumers' responses, I would expect a sizable proportion of canteen guests to voice their disapproval of the labels. It is unlikely that canteen guests are willing to incur great costs (since they expect the survey results to influence canteens' choices), only to "look good" in front of an experimenter they do not know and who will only look at their choices in an anonymized manner. There is a large discrepancy between this prediction and the survey responses I observe. Only 9% of survey respondents communicate that

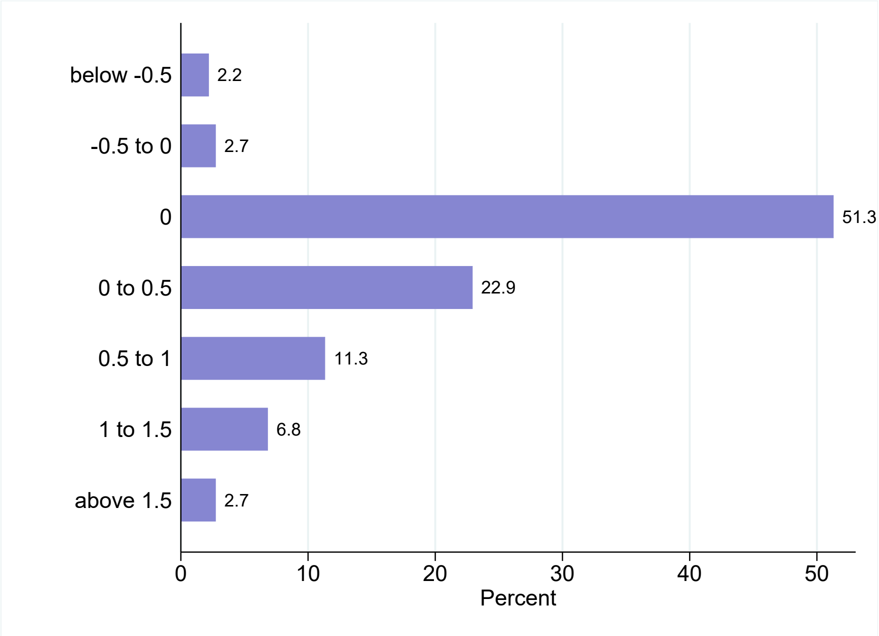


Figure 17. Distribution of willingness to pay indicated to see carbon labels on the final three consumption decisions in Experiments 1 and Experiment 3. Includes data from all 733 participants.

they would not like the carbon labels to be installed permanently. I thus do not consider it likely that carbon labels mainly affect the average consumer by moving him towards a socially desirable choice that is not in line with his true interests. However, the above interpretation might of course align with the experience of a select few consumers.

In general, my results point towards the carbon labels on average creating a net psychological benefit to consumers. However, it is important to acknowledge that, although a large majority of individuals seem to incur psychological benefits with carbon labels, there is a small proportion of individuals who prefer to avoid carbon labels. If a policymaker is strongly concerned about these individuals, it might be worthwhile to explore technological solutions that allow consumers to decide whether or not to see carbon labels.

Table 7. Correlations between willingness to pay for seeing carbon labels and individual characteristics

	WTP for the presence of carbon labels				
	(1)	(2)	(3)	(4)	(5)
Perceived strength of social norms	0.01*				
	(0.01)				
In favor of labels in student restaurant		0.03***			
		(0.01)			
Self-reported willingness to use info			0.03***		
			(0.01)		
Self-reported confidence in own knowledge				−0.01	
				(0.01)	
Eating self-control					0.00
					(0.01)
Constant	0.15***	−0.03	0.03	0.18***	0.21***
	(0.03)	(0.06)	(0.04)	(0.02)	(0.02)
Observations	732	732	732	732	732

Standard errors in parentheses

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

Note: Dependent variable: Willingness to pay for seeing labels for the final three consumption decisions. "In favor of labels in student canteen" is measuring using approval of the statement "I would appreciate if the student canteen would introduce such a measure". "Self-reported willingness to use info" is measured using approval of the statement "I would include this information in my decision.". "Self-reported confidence in own knowledge" is measured with two questions: (1) approval of the statement "I already know without labels which emissions are caused by different meals.", and (2) "I think this information will partially surprise me." The perceived strength of social norms is measured using the procedure developed by Krupka and Weber (2013). Eating self-control is measured using the questions developed by Haws, Davis, and Dholakia (2016).

8 Discussion

This paper provides evidence from the student canteen setting that carbon labels causally impact consumption behavior, estimating the effectiveness of carbon labels in reducing emissions as similar to that of a carbon tax of €120 per tonne. The labels primarily impact consumers by directing their attention toward carbon emissions, and their presence on average increases consumer welfare.

These results speak towards attention frictions playing an important role in impeding consumers from behaving in a carbon-friendly manner. While a lack of attention has been shown to play an important role in impeding sustainable behavior in the energy and resource consumption context (Allcott and Taubinsky, 2015; Taubinsky and Rees-Jones, 2018; Tiefenbeck et al., 2018), this is a new result in the food consumption context. The food consumption context differs from the resource consumption context in two ways. First, reducing energy and resource consumption usually also creates financial benefits for consumers, while reducing emissions in food consumption does not. Second, resource consumption is a continuous choice while food consumption is a discrete choice decision. Results showcase that increasing attention can also be effective in a discrete choice context, and open the door to examining related discrete choice consumption contexts.

While results corroborate previous findings on the emission-saving potential of attention-directing behavioral interventions in general, they also support the potential of carbon labels in particular. Findings are likely also relevant for related food contexts, such as corporate canteens or grocery shopping. Identifying carbon labels as a promising policy tool is especially relevant in the food sector. Carbon taxes for this sector are still widely uncommon (e.g. the agricultural sector is excluded from the EU-ETS trading scheme) and Dechezleprêtre et al. (2022) identify agriculturetargeted policies as among the least popular policies to reduce carbon emissions. In such a setting, alternative policy tools are especially called for.

Further, there are other discrete choice contexts in which the carbon footprint caused by different items could be calculated and labeled, e.g. shopping for toiletries or clothing. Future research could test the effectiveness and consumer welfare impact of carbon labels in these other consumption contexts, and also among other target populations. One way of doing so would be an adaptation of the design of Experiment 1—we would then be able to compare effects across domains and populations.

Further research would also be beneficial to assess whether carbon labels affect consumers in other domains apart from the target behavior. Suggestive evidence from a field survey I conducted to accompany the natural field experiment (Experiment 2) provides no evidence of the carbon labels affecting consumers' attitudes towards political measures to decrease carbon emissions (see section D.1.6 and Table E.1). However, spillovers may appear if labels are installed over longer time periods, or spillovers might affect other domains. Since the carbon labels mainly affect behavior by directing attention, attentional spillovers as described by Nafziger (2020) are also thinkable.

Appendix A Additional material on theoretical model and structural estimation

A.1 Extension of theoretical model to consumer welfare impact

Introducing *carbon labels* makes the consumer both informed and attentive. Her perceived utility then becomes more similar to her *true utility* for meal m,

$$u^T(m) = v_m - p_m - \gamma e_m^{\text{true}} \tag{A.1}$$

Accordingly, carbon labels increase the likelihood of the consumer choosing the meal m that maximizes her true utility.[52] If the consumer can make a choice $P \in 0,1$ on the presence of carbon labels in her decisions, the *utility change she experiences from the presence of labels* is

$$u(P) = u^{True}(m^L) - u^{True}(m^{prior}) + F \tag{A.2}$$

Here, $u^{True}(m^L)$ is the true utility the consumer would realize from the meal she chooses in the presence of the labels, while $u^{True}(m^{prior})$ is the true utility she would realize from the meal she chooses in the absence of labels. F denotes a *fixed psychological cost or benefit* the consumer experiences as a result of seeing the labels, independent of any behavioral change provoked by the carbon labels.

A.2 Identification of welfare impact in the experiment setting

In the experiment setting, the mere act of showing example carbon labels to participants and asking consumers for their willingness to pay to see carbon labels in a decision will make participants attentive to emissions (provided they have not already been made attentive of emissions earlier in the experiment). Thus, the difference in utility consumers' experience in the presence of carbon labels, $u(P=1)$ relative to utility in the absence of labels, $u(P=0)$, is

$$u(P = 1) - u(P = 0) = u^{True}(m^{*L}) - u^{True}(m^{*A}) + F \tag{A.3}$$

and the true utility the consumer reaps from meal m in the experiment context is

$$u^{True}(m) = v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - p_m - p_o \tag{A.4}$$

In the experiment setting, there are only two possible cases in which $u^T(m^{*L}) - u^T(m^{*A}) \neq 0$:

(1) The WTP which the participant indicates when seeing labels, WTP^{A+L} is higher than the price $p_m - p_o$ to receive meal m rather than the outside option o, but $WTP^A < p_m - p_o$

(2) The WTP which the participant indicates merely attentive, WTP^A is higher than the price $p_m - p_o$ to receive meal m rather than the outside option o, but $WTP^{A+L} < p_m - p_o$

In the experiment context, equation A.3 thus transforms to:

$$\begin{aligned} u(P = 1) - u(P = 0) = \mathbb{1}\Big(WTP^{A+L} \geq p_m - p_o\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - E[p_m - p_o | WTP^{A+L} \geq p_m - p_o]\Big) \\ -\mathbb{1}\Big(WTP^{A} \geq p_m - p_o\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - E[p_m - p_o | WTP^{A} \geq p_m - p_o]\Big) + F \end{aligned} \tag{A.5}$$

52. The consumers' true valuation of the emissions caused by the meal is not influenced by a lack of salience or misperceptions of the carbon impact. By modeling utility in this manner, I assume that consumers will at some point in their lives find out about the true emissions caused by their consumption decisions, and will experience ex-post regret accordingly (e.g. such as consumers might have experienced ex-post regret about previous decisions to take a plane as the general public became more aware of environmental impact, coining the term "flight shame").

When the participant indicates her willingness to pay for the presence of labels, she weights each event with the probability of it occurring:

$$
\begin{aligned}
WTP^{P} = & \; Prob\Big(WTP^{A+L} \geq p_m - p_o\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - E[p_m - p_o | \hat{V}_m^L \geq p_m - p_o]\Big) \\
& - Prob\Big(WTP^{A} \geq p_m - p_o\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - E[p_m - p_o | \hat{V}_m^A \geq p_m - p_o]\Big) \\
& + F
\end{aligned}
\tag{A.6}
$$

In the experiment, relative meal prices $p_m - p_o$ are drawn from a uniform distribution, with each value between -3 and 3 being equally likely, in five-step intervals. Thus, $Prob(p \leq x) = (x+3)/6$. Similarly, $E[p|p \leq x] = (x-3)/2$. Inserting this above:

$$
\begin{aligned}
WTP^{P} = & \Big((WTP^{A+L} + 3)/6\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - (WTP^{A+L} - 3)/2\Big) \\
& - \Big((WTP^{A} + 3)/6\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - (WTP^{A} - 3)/2\Big) + F
\end{aligned}
\tag{A.7}
$$

When I ask experiment participants for their willingness to pay for the presence of labels on their three final meals, I do not tell them in advance which meals these will be, and only tell them that these will be three new meals which they have not seen in the experiment previously. Thus, participants are not able to compute the first two terms of the above equation for the new meal. Participants in the Attent+Label condition have, however, seen carbon labels on the meals shown to them in the second or third round of their main choices. I would assume that participants indicate their willingness to pay for the presence of labels somewhat along the lines of "Based on the value I previously derived from the carbon labels, my willingness to pay to see carbon labels on a choice is XYZ." The willingness to pay for the presence of labels thus enters my estimation as a type of ex-post willingness to pay to see carbon labels on the four main meal decisions, for participants in the Attent+Label condition.

Participants in the Attention condition have not seen emission labels before indicating their willingness to pay for the presence of labels, and would thus have to form a less informed expectation over the first two terms in A.7. I thus do not include them in the main estimation of F (Col. (6) in Table 5 in the main text and Table A.1). Col. 7 in Table A.1 includes these observations and finds estimates similar to the previous specification. Table C.32 shows that the average willingness to pay indicated for the presence of carbon labels does not differ across treatments.

A.3 Equations for structural estimation

$$WTP^{A+L} - WTP^{B} = \gamma(e^{prior}_{im} - e^{prior}_{io})(\kappa - \theta) + \gamma(e_{im} - e_{io})(1-\kappa) \tag{A.8}$$

$$WTP^{A} - WTP^{B} = \gamma(e^{prior}_{im} - e^{prior}_{io})(1-\theta) \tag{A.9}$$

$$WTP^{A+O} - WTP^{A+L} = -\gamma(e^{true}_{im} - e^{true}_{io})(1-\kappa) - \gamma(e^{prior}_{im} - e^{prior}_{io})\kappa \tag{A.10}$$

$$\begin{aligned} WTP^{P} = &\Big((WTP^{A+L}+3)/6\Big)\Big(v_m - o_m - \gamma(e^{\text{true}}_{m} - e^{\text{true}}_{o}) - (WTP^{A+L}-3)/2\Big) \\ &-\Big((WTP^{A}+3)/6\Big)\Big(v_m - o_m - \gamma(e^{\text{true}}_{m} - e^{\text{true}}_{o}) - (WTP^{A}-3)/2\Big) + \hat{F} \end{aligned} \tag{A.11}$$

Table A.1. Structural estimates of model parameters

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Theta	0.16	0.03				0.18	0.12
	(0.18)	(0.17)				(0.17)	(0.20)
Gamma	-0.12^{***}	-0.10^{***}	-0.10^{***}	-0.10^{***}	-0.12^{***}	-0.12^{***}	-0.11^{***}
	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Kappa	0.21		0.12		0.12	0.23	0.17
	(0.20)		(0.19)		(0.21)	(0.20)	(0.22)
F						0.21^{***}	0.20^{***}
						(0.01)	(0.01)
Observations	3,216	3,216	3,216	3,216	3,216	3,216	3,216

Standard errors in parentheses

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

Note: Analysis is based on data from Experiment 3. For each meal, the observations corresponding to the 10% most extreme guesses (in terms of deviation from the true emission value) are dropped. Regression does not include a constant, since the estimation follows the model outlined in Section 2. Column (1) shows the main estimation, based on equations A.9, A.8, A.10. Columns (2)–Column (7) each modify the model in Column (1) as follows: Column (2) imposes $\kappa = 0$. Column (3) imposes $\theta = 0$. Column (4) imposes $\theta = \kappa = 0$. Column (5) imposes $\theta = 1$. Column (6) includes equation A.7 in the estimation. Column (7) includes values for willingness to pay for the presence of labels indicated by participants in the Attention treatment.

A.4 Details on the intervention comparisons

I use Experiment 3 data to deduce how experiment participants would make typical student canteen choices in the absence of any intervention, as well as under different interventions. Based on the willingness to pay which participants indicated for each of the four meals at baseline, I can deduce how experiment participants would make their consumption choice in a typical canteen setting, i.e. with a meal offer and pricing structure typical at the university of Bonn. In the next step, I additionally make use of the results from Col. (6) of the structural estimation shown in Table 5, participants' emission guesses, and true emissions to estimate the consumption choices that participants would make if they were experiencing an intervention.

I assume the following meal offer and pricing structure for the simulations. Specifically, I simulate how participants would choose on the following four exemplary days:

- Day 1: Canteen offers Filled courgettes with potato croquettes or Chicken Schnitzel with rice at a price of €3.05 each, as well as a cheese sandwich at a price of €1.50

- Day 2: Canteen offers Filled courgettes with potato croquettes or Beef ragout with potatoes at a price of €3.05 each, as well as a cheese sandwich at a price of €1.50
- Day 3: Canteen offers Italian vegetable ragout with pasta (€2.75) or Chicken Schnitzel with rice (€3.05), as well as a cheese sandwich at a price of €1.50
- Day 4: Canteen offers Italian vegetable ragout with pasta (€2.75) or Beef ragout with potatoes (€3.05), as well as a cheese sandwich at a price of €1.50

I chose the meals because these are the four meals I use in the baseline purchase decisions in Experiment 3 and I know participants' taste preferences for these meals accordingly.[53] The student canteen in Bonn always offers one meat meal and one vegetarian meal, so I designed the four days to cover all possible combinations of the four meals. The four meals are regularly offered in the student canteen, and I use the student canteen's prices for these meals in the simulations. Further, the student canteen always offers cheese sandwiches and prices these at €1.50, so this is included on all days as a third option.

To calculate choices in the absence of any intervention—line 1 in Table 6—I calculate for each participant and day the difference in the participant's baseline willingness to pay for the option and the canteen price. Since willingness to pay for each meal is in the experiment elicited relative to a cheese sandwich, I add €1.50 to all relative willingness to pay values. €1.50 is the price of a cheese sandwich in the canteen and also the average value of what experiment participants indicated in a hypothetical question as the amount they were willing to pay to receive a cheese sandwich. Accordingly, I set the willingness to pay for a cheese sandwich of all participants to €1.50. I assume the participant would decide to take the option with the largest difference between the two, allowing her to realize the highest consumer surplus.

To calculate choices with an intervention solely increasing attention—line 2 in Table 6—I use participants' baseline willingness to pay and prior emission estimates as well as the estimated model parameters to calculate an ATTENTION willingness to pay for each participant and meal, according to equations 5 and 6. I then simulate meal choices as in the previous calculation.

To calculate choices with an intervention solely increasing knowledge—line 3 in Table 6—I use participants' baseline willingness to pay, prior emission estimates, and estimated model parameters to calculate a KNOWLEDGE willingness to pay for each participant and meal. This is based on 5 and equation A.12 below. A KNOWLEDGE treatment is assumed to lead to the consumer updating her emissions estimate according to 4 without directing attention.

$$WTP^K = v_m - v_o - \theta\gamma(e_m^{\text{info}} - e_o^{\text{info}}) \tag{A.12}$$

I then simulate meal choices as in the previous calculation.

To calculate choices with a carbon label—line 4 in Table 6—I use participants' baseline willingness to pay, prior emission estimates, and estimated model parameters to calculate a LABEL willingness to pay for each participant and meal based on equations 5 and 6. I then simulate meal choices as in the previous calculation.

To calculate choices with a carbon tax—line 5 in Table 6—I repeat the analysis in line 1, but use adjusted canteen prices, increasing prices by €0.12 for every kg of emissions caused by an option (i.e. using a carbon tax of €120 per tonne). To calculate choices with a meat ban—line 6 in Table 6—I similarly remove the meat option from each of the daily choices. For the beef ban—line 7 in Table 6—I only removed the beef option on days 2 and 4.

53. This is the case for non-vegetarian participants. For vegetarians, the two meat meals are exchanged in the experiment for two vegetarian meals. They are dropped from the simulation.

A.5 Additional simulation results: Distribution of welfare changes

Figure A.1 shows that the change in utility achieved through making consumers solely attentive is more dispersed than with the combined intervention. In some instances, utility change is even slightly negative. This is mainly attributable to meals for which participants on average overestimated emissions. In this case, increasing attention without providing information can make consumers avoid meals that are in fact low in emissions. Solely increasing knowledge can also decrease consumption utility. These negative effects are also attributable to meals for which participants overestimated emissions, but explicable with a different channel: In the absence of any behavioral intervention, the overestimation can partly compensate for participants' lack of attention towards carbon emissions, and move participants more toward the optimal choice. When the misperception is removed, participants move further away from the optimal choice. There is also one case in which the carbon labels decreased consumption utility: This can be caused by special cases due to slow updating described by the κ parameter.

Providing both interventions prevents either of the two interventions from backfiring: Solely raising attention might lead to a decrease in consumer utility if meals are on average overestimated by consumers. Further, one can think of parameter combinations with very low consumer attention but high overestimation in which solely correcting misperceptions would also lead to a decrease in consumer utility. Providing both interventions simultaneously can prevent both of these situations.

Figures A.2 and A.3 additionally compare the distribution of welfare effects resulting from carbon labels to that of a ban on beef, meat, and a carbon tax.

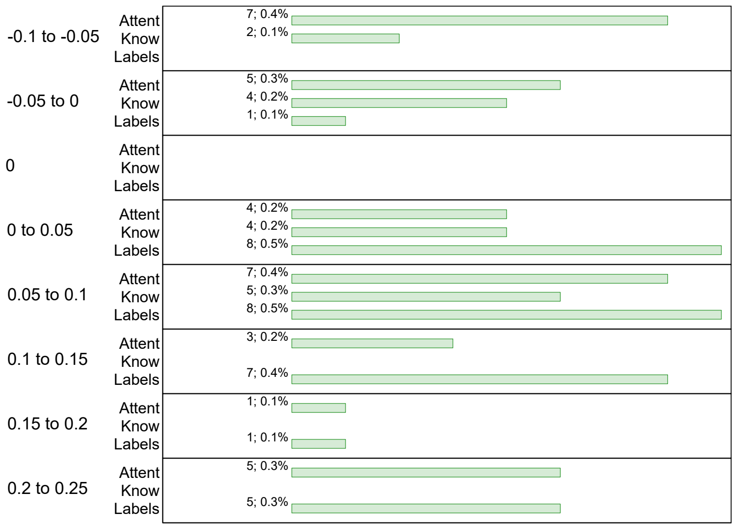


Figure A.1. Estimated change in consumer welfare per meal which would be caused by solely raising attention, solely correcting misperceptions or the combination of both (labels), in Euro. The Figure shows utility changes for instances in which the interventions lead to behavioral change (otherwise change in utility is 0). For the intervention raising attention, this is 2% of instances, for the intervention increasing knowledge it is 0.9% of instances, and for the carbon labels, it is 1.9% of instances. Read numbers e.g. as: With the Attention intervention, there are 7 instances in which a participant would on one of the four simulated canteen days experience a welfare loss equal to a monetary equivalent of €−0.1 and €−0.05 due to the Attention intervention. That is, he would choose a meal due to the Attention intervention that decreases his true utility by this amount. These are 0.4% of all consumption cases. There are 2 instances (0.1% of all consumption cases) in which a similar welfare loss would be incurred due to a Knowledge intervention, etc.

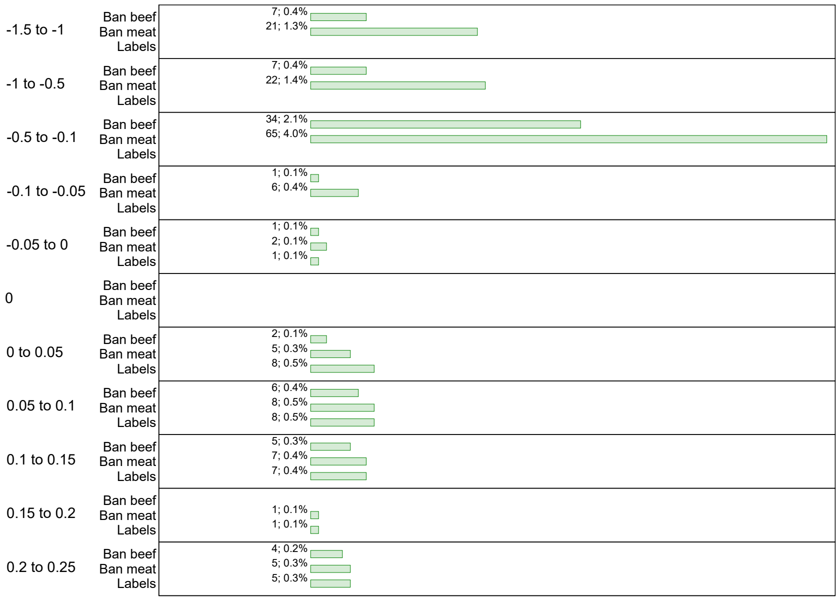


Figure A.2. Estimated change in consumer welfare which would be caused by a ban of beef or a ban of meat compared to carbon labels, in Euro. Estimation based on experiment data and student canteen prices and offer structure. The Figure shows utility changes for instances in which the interventions lead to behavioral change (otherwise change in utility is 0). For the beef ban, this is 4.2% of instances, for the meat ban it is 8.8% of instances, and for the carbon labels, it is 1.9% of instances. Example of how to read this table: With the Beef ban intervention, there are 7 instances in which a participant would on one of the four simulated canteen days experience a welfare loss equal to a monetary equivalent between €−1.5 and €−1 due to the Beef ban intervention. That is, he would choose a meal due to the Beef ban intervention that decreases his true utility by this amount. These are 0.4% of all consumption cases. There are 21 instances (1.3% of all consumption cases) in which a similar welfare loss would be incurred due to a Meat ban intervention, etc.

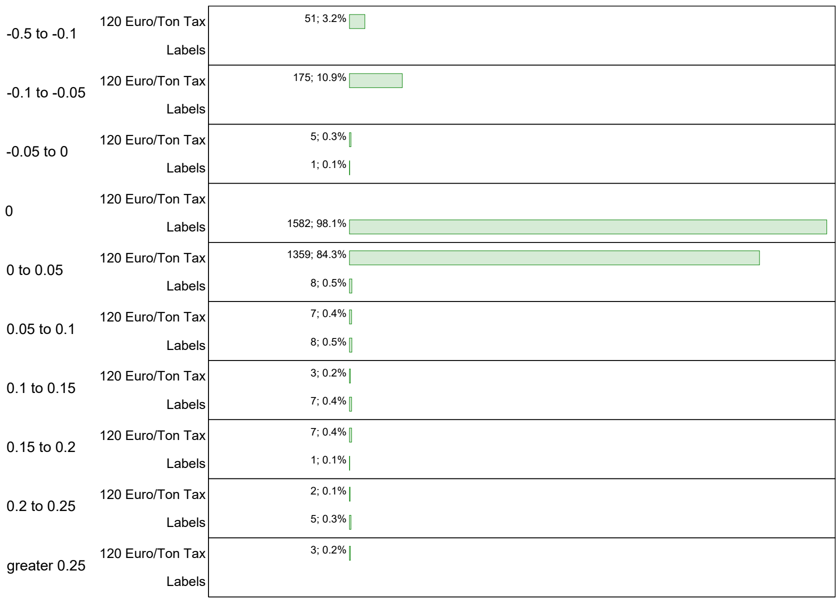


Figure A.3. Estimated change in consumer welfare which would be caused by a carbon tax of €120 per tonne compared to carbon labels, in Euro. Proceedings from the tax are re-distributed equally to all consumers. Estimation based on experiment data and student canteen prices and offer structure. Read numbers e.g. as: With the CARBON TAX intervention, there are 51 instances in which a participant would on one of the four simulated canteen days experience a welfare loss equal to a monetary equivalent between €−0.5 and €−0.1 due to the CARBON TAX intervention. That is, he would choose a meal due to the CARBON TAX intervention that decreases his true utility by this amount, relative to choice and prices in the absence of a tax. These are 3.2% of all consumption cases.

Appendix B Experiments 1 and 3: Details on experimental set-up

B.0.1 Meals used for elicitation. In the purchasing decisions in experiments 1 and 3, participants make decisions on the same four student canteen meals. These are all meals which are regularly offered in the student canteen. Participants who indicate that they are not vegetarian decide on two vegetarian and two meat meals: Filled courgettes with potato croquettes (1.4 kg of emissions, colored yellow in the labels), Italian vegetable ragout with pasta (0.5 kg of emissions, colored green in the labels), Chicken Schnitzel with rice (1.4 kg of emissions, colored yellow in the labels), and beef ragout with potatoes (3.4 kg of emissions, colored red in the labels). Participants who indicate they are vegetarian decide on four vegetarian meals: Filled courgettes with potato croquettes (1.4 kg of emissions, colored yellow in the labels), Italian vegetable ragout with pasta (0.5 kg of emissions, colored green in the labels), Cheese "Spätzle" with mushrooms (1.2 kg of emissions, colored yellow in the labels), and stir-fried vegetables with rice (0.4 kg of emissions, colored green in the labels). The cheese sandwich is the outside option to every choice and causes 0.7 kg of emissions and is colored green on the labels.

I randomized the order in which meals appear (both in the decision and the emission estimating screens) to avoid order effects. Further, I changed the left-right positioning of the warm meal vs. the cheese roll to right-left for half of the experiment sessions to avoid positioning effects.

B.0.2 Incentivization of elicitations. The elicitation of participants' **willingness to pay for consuming the meals** is incentivized with an adapted BDM mechanism: There is a 50% probability that the specific meal and a 50% probability that the cheese sandwich is randomly drawn as the default meal. If the default meal and the preferred meal indicated in the first part of the decision (e.g. Figure 2) coincide, the participant is given the preferred meal at zero price. If the two do not coincide, a price is randomly drawn at which the two options can be exchanged. Each value between €0.00 and €3.00 can be drawn with equal probability, in five-cent steps. If the willingness to pay indicated by the participant in the second part of the decision (e.g. Figure 3) is equal to or above the price drawn, the price is deducted from the participants' payment and participants are provided with the preferred option. If willingness to pay is below the price drawn, participants are provided with the less preferred option, and no amount is deducted from participants' payments. The outcome lunch is provided to participants directly after the experiment, together with participants' payment in cash. For this purpose, experiment participants are required to travel to the university campus immediately after completing the experiment. Less than 4% did not pick up their cash payment and meal. The incentivization structure was explained to participants and they were required to pass an extensive comprehension check, which less than 4% of participants did not pass.

This **willingness to pay for seeing labels elicitation** is incentivized with a similar BDM mechanism. There is a 50% probability that the default option is that choices are displayed with, and a 50% probability that the default option is that choices are displayed without labels. If the default display option and the preferred display option coincide, the preferred display option is implemented at zero price. If the two do not coincide, a price is randomly drawn at which the display option can be changed. Each value between €0.00 and €3.00 can be drawn with equal probability, in five-cent steps. If the willingness to pay indicated by the participant in the second part of the decision (similar to Figure 3, with display options instead of meals) is equal to or higher than the price drawn, the preferred display option is implemented. The price drawn is only deducted from participants' payment if one of the final three meals is relevant for pay-out. If the willingness to pay is lower than the price drawn, the less-preferred display option is implemented.

B.0.3 Decisions under carbon offsetting. In the ATTENTION+OFFSET condition in Experiment 3 and the OFFSET condition in Experiment 1, participants are informed that, if one of the decisions made in this treatment is implemented, the emissions of the meal provided to them (regardless of whether it is the warm meal or the cheese sandwich) are offset by the experimenter with a donation to Atmosfair. The example screens in Subsection B.1 show how this is communicated to experiment participants.

Towards the end of the experiment, after participants have completed all meal decisions, I elicit participants' attitudes towards the effectiveness of carbon offsetting and ask for participants' prior experiences with carbon offsetting. Tables B.1 and B.2 show descriptives pooled across Experiments 1 and 3. Table B.1 shows that 75% of participants had heard of carbon offsetting previously, while 34% have used carbon offsetting themselves.

Table B.2 shows that participants broadly agree with carbon offsetting being effective (Measured as agreement to the statement "Voluntary carbon offsetting is an effective climate protection measure"). They disagree with them replacing other climate protection measures (Measured as agreement to the statement "If I offset emissions for a carbon-intensive activity such as a flight, it is okay to book another flight."). They agree with carbon offsetting not replacing other climate protection activities (Measured as agreement to the statement "Carbon offsetting cannot replace personal efforts to protect the climate."). Interestingly, having experienced the Attention+Offset or the Offset condition earlier in the experiment increases support for the second and decreases support for the third statement.

These descriptive statistics convey that carbon offsetting likely removes a part of environmental guilt, but may not be removing it entirely.

Table B.1. Beliefs of carbon offsetting effectiveness

	Familiarity with offsetting	
	(1)	(2)
	Heard of	Have used
In offset condition earlier in exp.	-0.04	-0.01
	(0.03)	(0.04)
Constant	0.75***	0.34***
	(0.03)	(0.03)
Observations	732	732

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table B.2. Beliefs of carbon offsetting effectiveness

	Familiarity with offsetting		
	(1)	(2)	(3)
	Effective	Can replace	Cannot replace
In offset condition earlier in exp.	0.15	0.45***	-0.50***
	(0.18)	(0.16)	(0.17)
Constant	5.55***	2.86***	8.14***
	(0.14)	(0.12)	(0.13)
Observations	732	732	732

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

B.1 Experiment screens (English translation)

Survey start screen
Welcome to the BonnEconLab online study. Please note that you may only take part in this study once. Furthermore, you may only take part if you have registered for the study in our participation database. Please complete this survey on your computer. Participation with mobile devices such as smartphones or tablets is not possible. The payout for this experiment will be done using your personal participant code: 12pI2q5vh Please write down your code! You will need approximately 45 minutes to process this survey. After fully completing the survey, you can collect your payout at our location at the Hofgartenwiese (see map below) until 2 p.m. today. You will not be able to receive your payout at any other time! In this experiment, your payout consists of several components:

- You receive exactly one dish (your lunch).
- You receive an expense compensation of €9.00 in cash.
- You may receive an additional payout of up to €1.60 in addition to the expense compensation. This depends on your answers in the marked part of the study.
- In addition, chance determines whether, depending on your answers in another (also clearly marked) part of the study, you will receive another additional payout of up to €1.10.

Payment will be made in the BonnEconLab pavilion on the Hofgartenwiese (Regina-Pacis-Weg). You will find us at the place marked with a blue arrow under a pavilion.

Decision description screen - Screenshot 1/4

Description of upcoming decisions | Comprehension questions

The second part of the study is about to begin. Your decisions in this part of the study will affect your expense compensation and the dish you receive.

On this page you will find explanations and examples. On the following page we will check your understanding of these explanations. By clicking on the tab above you can switch between the two pages.

Once the comprehension questions have been answered correctly, you can proceed with further work on the survey.

How do your decisions affect your payout?

- In this experiment, your payout consists of three components:
  - You receive exactly one dish (your lunch).
  - You receive an expense compensation. At the moment, the expense compensation is €9.00. You will make a total of 15 decisions over the course of this study. For each of these decisions, you have the option of waiving part of the expense compensation (maximum €3.00). For that, you will receive a court you prefer.
  - In two other parts of the study, you may receive an additional amount of up to €1.60 in addition to the expense compensation, depending on your answers. In addition, depending on your answers in a third part of the study, chance will determine whether you will receive an additional amount of up to €1.10. The relevant parts of the study are clearly marked.
- For each of the 15 decisions, indicate which of the two courts you prefer. Then specify the maximum amount of your expense compensation you would like to forgo in order to receive the preferred court.

The decision that is implemented shall be subject to the following:

- Chance decides whether you will receive your favourite dish for free:
  - Case 1 (50% probability): You will receive your favourite dish for free.
  - Case 2 (50% probability): You will be assigned the non-preferred dish first. In this case, specify the maximum amount of your expense compensation you would like to forgo in order to receive your favourite dish instead.
- If case 2 occurs, it is again a matter of chance:
  - A **surcharge** is determined at random. Any value between €0 and €3 (in 5 cent increments) is equally probable.
  - If the amount you have declared is more than the surcharge, you will receive your preferred dish. For this, the surcharge will be deducted from your expense compensation.
  - If the amount you specify is less than the surcharge, you will receive the non-preferred dish free of charge.

For the other 14 decisions which are not being implemented, the following rules apply:

- These decisions have no effect on the dish you receive.
- These decisions have no effect on your compensation.

You will not know which of the 15 decisions will be implemented until the end of the study. It is therefore in your best interest to make every decision carefully.

Example decision

You can receive either a cheese roll or the 'Baked Feta Cheese with Rice' dish.

Which dish do you prefer? Click on one of the two buttons. Try it!

Baked Feta Cheese with Rice vegetarian	oder	Cheese Roll Details: vegetarian
[Baked Feta Cheese with Rice]		[Cheese Roll]

Decision description screen - Screenshot 2/4

Example scenario 1

Assuming you made the following decision:

Which dish do you prefer? Click on one of the two buttons.

Baked Feta Cheese with Rice

vegetarian

oder

Cheese Roll

Details: vegetarian

Baked Feta Cheese with Rice

Cheese Roll

If you are given the cheese roll: What is the maximum amount of your expense compensation you would be willing to give up in exchange for stir-fry sweet and sour with rice?

(Click on the grey bar to make the slider visible).

You would like to give up a maximum of **€1.20** of your allowance to receive the dish **Baked Feta Cheese with Rice** instead of the cheese roll.

Here's what happens in this example (which you have no control over):

- You are first assigned your **less preferred dish**, the cheese roll.
- A **surcharge of €0.60** is randomly determined.

This means for you:

The surcharge with the amount of 0,60 € is lower than the maximum amount of 1,20 € you specified. You will receive the dish 'Baked feta cheese with rice'. For this, € 0.60 will be deducted from your expense compensation.

Decision description screen - Screenshot 3/4

Example scenario 2

Assuming you made the following decision:

Which dish do you prefer? Click on one of the two buttons.

Baked Feta Cheese with Rice

vegetarian

oder

Cheese Roll

Details: vegetarian

Baked Feta Cheese with Rice

Cheese Roll

If you are given the cheese roll: What is the maximum amount of your expense compensation you would be willing to give up in exchange for stir-fry sweet and sour with rice?

(Click on the grey bar to make the slider visible).

You would like to give up a maximum of **€1.20** of your allowance to receive the dish **Baked Feta Cheese with Rice** instead of the cheese roll.

Here's what happens in this example (which you have no control over):

- You are first assigned your **less preferred dish**, the cheese roll.
- A **surcharge of 2.00 €** is randomly determined.

This means for you:

The surcharge with the amount of 2.00 € is higher than the maximum amount of 1,20 € you specified. You will receive the cheese roll. Therefore, nothing will be deducted from your expense compensation.

Decision description screen - Screenshot 4/4

Example scenario 3

Assuming you made the following decision:

Which dish do you prefer? Click on one of the two buttons.

Baked Feta Cheese with Rice

vegetarian

oder

Cheese Roll

Details: vegetarian

Baked Feta Cheese with Rice

Cheese Roll

If you are given the cheese roll: What is the maximum amount of your expense compensation you would be willing to give up in exchange for stir-fry sweet and sour with rice?

(Click on the grey bar to make the slider visible).

0.00€ 1.00€ 2.00€ 3.00€

You would like to give up a maximum of **€1.20** of your allowance to receive the dish **Baked Feta Cheese with Rice** instead of the cheese roll.

Here's what happens in this example (which you have no control over):

- You are assigned your **preferred dish**, 'Baked feta cheese with rice', for free.

This means for you:

You receive the dish 'Baked feta cheese with rice'. Nothing will be deducted from your expense compensation.

Continue to the questions

You can always return to this page while answering the questions.

Comprehension questions - Screenshot 1/3

Description of upcoming decisions | Comprehension questions

Comprehension questions

Please answer the following comprehension questions. If you want to look at the description of the survey again, you can switch back and forth between this page and the previous page by clicking on the tab at the top.

After correctly answering the comprehension questions, you can continue with the further processing of the survey.

Question 1

Assuming you made the following decision:

Which dish do you prefer? Click on one of the two buttons.

Baked Feta Cheese with Rice

vegetarian

oder

Cheese Roll

Details: vegetarian

Baked Feta Cheese with Rice | Cheese Roll

If you are given the cheese roll: What is the maximum amount of your expense compensation you would be willing to give up in exchange for stir-fry sweet and sour with rice?

(Click on the grey bar to make the slider visible).

0.00€ 1.00€ 2.00€ 3.00€

You would like to give up a maximum of **€1.30** of your allowance to receive the dish **Cheese Roll** instead of the Baked Feta Cheese with Rice.

Here's what happens in this example (which you have no control over):

- The decision was carried out.
- You are first assigned your **less preferred dish**, the Baked Feta Cheese with Rice.
- A **surcharge of 0.70 €** is randomly determined.

What do you receive?

- The baked feta cheese with rice and your full expense compensation.
- The baked feta cheese with rice and 0.70 euros will be deducted from your expense compensation.
- The cheese roll and 0.70 euros will be deducted from your expense compensation.
- The cheese roll and your full expense compensation.

Question 2

Assuming you made the following decision:

Which dish do you prefer? Click on one of the two buttons.

Baked Feta Cheese with Rice	oder	Cheese Roll
vegetarian		Details: vegetarian
Baked Feta Cheese with Rice		Cheese Roll

If you are given the cheese roll: What is the maximum amount of your expense compensation you would be willing to give up in exchange for stir-fry sweet and sour with rice?

(Click on the grey bar to make the slider visible).

0.00€ 1.00€ 2.00€ 3.00€

You would like to give up a maximum of **€1.30** of your allowance to receive the dish **Cheese Roll** instead of the Baked Feta Cheese with Rice.

Here's what happens in this example (which you have no control over):

- The decision was carried out.
- You are assigned your **preferred dish**, the cheese roll.

What do you receive?

- The baked feta cheese with rice and your full expense compensation.
- The baked feta cheese with rice and 0.70 euros will be deducted from your expense compensation.
- The cheese roll and 0.70 euros will be deducted from your expense compensation.
- The cheese roll and your full expense compensation.

Question 3

Assuming you made the following decision:

Which dish do you prefer? Click on one of the two buttons.

Baked Feta Cheese with Rice	or	Cheese Roll
vegetarian		Details: vegetarian
Baked Feta Cheese with Rice		Cheese Roll

If you are given the cheese roll: What is the maximum amount of your expense compensation you would be willing to give up in exchange for stir-fry sweet and sour with rice?

(Click on the grey bar to make the slider visible).

0.00€ 1.00€ 2.00€ 3.00€

You would like to give up a maximum of **€1.30** of your allowance to receive the dish **Cheese Roll** instead of the Baked Feta Cheese with Rice.

Here's what happens in this example (which you have no control over):

- The decision was carried out.
- You are first assigned your **less preferred dish**, the Baked Feta Cheese with Rice.
- A **surcharge of 2.70 €** is randomly determined.

What do you receive?

- The baked feta cheese with rice and your full expense compensation.
- The baked feta cheese with rice and 2.70 euros will be deducted from your expense compensation.
- The cheese roll and 2.70 euros will be deducted from your expense compensation.
- The cheese roll and your full expense compensation.

Question 4

How many of the 15 decisions actually have an impact on the dish you are handed and your expense compensation?

- All the 15 decisions have an impact.
- Five of the 15 decisions have an impact.
- One of the 15 decisions has an impact.
- One of the 15 decisions has an influence.

Back to the explanation | Continue with the rest of the survey

Example baseline decision

You can receive either a cheese roll or the dish ‘Stir-fry sweet and sour with rice’ with your payout.

Which dish do you prefer? Click on one of the two buttons.

Stir-fry sweet and sour with rice

vegetarian

or

Cheese Roll

vegetarian

Stir-fry sweet and sour with rice

Cheese Roll

If you are given the Stir-fry sweet and sour with rice: What is the **maximum** amount of your expense compensation you would be willing to give up in exchange for the cheese roll?

(Click on the grey bar to make the slider visible).

0.00€ 1.00€ 2.00€ 3.00€

You would like to give up a maximum of **€0.75** of your allowance to receive the dish **cheese roll** instead of the Stir-fry sweet and sour with rice.

Continue

You will now guess for a total of eleven meals how high the CO2 emissions are which are caused by the respective meal.

- You have **60 seconds** to answer each question.
- For each question in which your guess does not deviate from the correct value by more than 30%, **0.10 Euro is added to your payout**.

During each guessing question you will be shown the emissions caused by the meal “Red Thai Curry with Pork and Rice” **as a reference value**.

Your reference value:

Red Thai-Curry with Pork and Rice

Pork

Which assumptions should be taken for the guessing questions?

For the following questions you will not be shown any ingredient lists or a description of the origin of the ingredients. This is because we only want to give you the information which you would normally find in a restaurant. We would like to know how you, based only on the name of the meal on the menu, guess the magnitude of the emissions caused by a meal.

Of course, the emissions of a seemingly identical meal can differ, e.g., depending on the exact ingredients and depending on whether the ingredients were produced in an ecologically sustainable or in a conventional manner. Please assume a conventional production and a conventional meal preparation – just like you would expect it, if you are offered such a meal without any further information in a restaurant.

Please take into account all emissions caused in the agricultural production and in food processing, packaging, conservation and transport of ingredients, up until an ingredient can be purchased in the store. You do not need to take into account emissions which are caused by the transport of ingredients from store to restaurant

Continue

Example carbon footprint estimation

Remaining time on this page. **0:54**

What do you estimate: How high are the greenhouse gas emissions (in CO2-equivalents), which are caused by the meal "Stuffed Zucchini with croquettes"?

Guess the emissions:	As a reference:
Stuffed Zucchini with croquettes	**Red Thai-Curry with Pork and Rice**
CO_2 Causes ? kg CO_2	CO_2 Causes 1,7 kg CO_2 ≈ 8,5 km Car drive
Vegetarian	Pork

I estimate that the meal "Stuffed Zucchini with croquettes" causes emissions of

kg.

Continue

Introduction decisions with labels

You will now make four more of the 15 decisions. One of the 15 decisions will be implemented.

You will be shown the greenhouse gas emissions (in CO2 equivalents) of both dishes for the upcoming decisions.

For those interested: More information on the calculation of greenhouse gas emissions:

What assumptions are made in the calculation?

In the calculation, the emissions attributable to a dish are calculated as the sum of the emissions generated in the production of the ingredients. The emissions of each ingredient are calculated "from farm to gate", i.e. all emissions are included that occur during agricultural production and during further processing, packaging, preservation and transport until the ingredient is available for purchase in shops. Not included are the transport from the shop to the restaurant or end consumer and the emissions that arise from any further refrigeration in the restaurant or at the end consumer, as well as the emissions that arise from cooking the dish.

When calculating the values, conventional (i.e. not specifically organically certified) agriculture is assumed. Otherwise, assumptions are made about production that reflect the production of the average product found on our supermarket shelves.

What data is the calculation based on?

The Eaternity database on which the calculations are based is currently the largest and most comprehensive database for calculating the climate-relevant emissions of meals and food products. It includes more than 550 ingredients and other parameters on organic and greenhouse production as well as production, processing, packaging and preservation. The eaternity database is maintained by scientists from the Zurich University of Applied Sciences (ZHAW), the University of Zurich (UZH), the Swiss Federal Institute of Technology Zurich (ETH Zurich), the Research Institute of Organic Agriculture (FiBL), Quantis and other institutions.

Source: eaternity.

Continue

Example decision with labels

You can either get a cheese roll or the dish 'stir-fry sweet and sour with rice' with your payout.

Which dish do you prefer? Click on one of the two buttons.

Stir-fry sweet and sour with rice	or	Cheese Roll
CO_2 Causes 0,4 kg CO_2 ≈ 2,0 km Car drive		CO_2 Causes 0,7 kg CO_2 ≈ 3,5 km Car drive
vegetarian		vegetarian
[Stir-fry sweet and sour with rice]		[Cheese Roll]

If you are given the cheese roll: What is the **maximum** amount of your expense compensation you would be willing to give up in exchange for stir-fry sweet and sour with rice?

(Click on the grey bar to make the slider visible).

0.00€ 1.00€ 2.00€ 3.00€

You would like to give up a maximum of **€1.10** of your allowance to receive the dish **stir-fry sweet and sour with rice** instead of the cheese roll.

Continue

For those interested: More information on the calculation of greenhouse gas emissions:

What assumptions are made in the calculation?

In the calculation, the emissions attributable to a dish are calculated as the sum of the emissions generated in the production of the ingredients. The emissions of each ingredient are calculated "from farm to gate", i.e. all emissions are included that occur during agricultural production and during further processing, packaging, preservation and transport until the ingredient is available for purchase in shops. Not included are the transport from the shop to the restaurant or end consumer and the emissions that arise from any further refrigeration in the restaurant or at the end consumer, as well as the emissions that arise from cooking the dish.

When calculating the values, conventional (i.e. not specifically organically certified) agriculture is assumed. Otherwise, assumptions are made about production that reflect the production of the average product found on our supermarket shelves.

What data is the calculation based on?

The Eaternity database on which the calculations are based is currently the largest and most comprehensive database for calculating the climate-relevant emissions of meals and food products. It includes more than 550 ingredients and other parameters on organic and greenhouse production as well as production, processing, packaging and preservation. The eaternity database is maintained by scientists from the Zurich University of Applied Sciences (ZHAW), the University of Zurich (UZH), the Swiss Federal Institute of Technology Zurich (ETH Zurich), the Research Institute of Organic Agriculture (FiBL), Quantis and other institutions.

Source: eaternity.

Introduction decisions with offsetting

You will now make four more of the 15 decisions. One of the 15 decisions will actually be implemented.

If it is one of the now following four choices that is implemented, the greenhouse gas emissions of the dish you have been handed will be offset by a donation to the NGO atmosfair. This happens regardless of whether the dish was originally assigned to you or whether you exchanged it for the other dish by paying a surcharge. Atmosfair uses the donation to support sustainable energy projects so that the emissions are saved elsewhere. In this way, the dish handed out to you becomes emission-neutral / CO2-neutral.

For those interested: Further information on CO2 offsetting:

How does the CO2 offset work?

The donation to atmosfair is used to develop renewable energies in countries where they hardly exist yet, i.e. mainly in developing countries. In this way, atmosfair saves CO2 that would otherwise have been produced by fossil energies in these countries.

Example projects

- Atmosfair uses donations to reduce the selling price of energy-efficient stoves in Nigeria. In Nigeria, 75% of families cook on open fires, and a family of 7 consumes 5 tonnes of wood per year. This enormous consumption of firewood has already led to almost total deforestation and the progressive spread of deserts, especially in the poor north of the country. Energy-efficient stoves use about 80% less wood.
- Atmosfair uses donations to make small-scale biogas plants more affordable in Nepal. This project targets families living in rural areas who previously used wood as an energy source for cooking. In this way, the increasing deforestation of Nepal's forests can be counteracted.
- Atmosfair uses donations to support a small hydropower plant in Honduras. In this way, four villages that previously used wood and diesel generators for energy supply could be connected to the electricity grid for the first time. In addition, electricity can be fed into the national grid, replacing electricity from gas-fired power plants.

Source: atmosfair

Continue

Example decision with offsetting

You can either receive a cheese roll or the dish ‘Italian Vegetable ragout with pasta’ with your payout.

The emissions attributable to each dish are offset by a donation to the NGO atmosfair. Atmosfair supports sustainable energy projects with the donation, so that the emissions are saved elsewhere.

Which dish do you prefer? Click on one of the two buttons.

Italian Vegetable ragout with pasta

CO2 neutral

Emission offset

vegetarian

Italian Vegetable ragout with pasta

or

Cheese Roll

CO2 neutral

Emission offset

vegetarian

Cheese Roll

If you are assigned the cheese roll: What is the **maximum** amount of your expense compensation that you would be willing to give up in exchange for Italian Vegetable ragout with pasta? (Click on the grey bar to make the slider visible).

0.00€ 1.00€ 2.00€ 3.00€

You would like to give up a maximum of **0.75 €** of your expense compensation to receive the **Italian Vegetable ragout with pasta** instead of the cheese roll.

Continue

For those interested: Further information on CO2 offsetting:

How does the CO2 offset work?

The donation to atmosfair is used to develop renewable energies in countries where they hardly exist yet, i.e. mainly in developing countries. In this way, atmosfair saves CO2 that would otherwise have been produced by fossil energies in these countries.

Example projects

- Atmosfair uses donations to reduce the selling price of energy-efficient stoves in Nigeria. In Nigeria, 75% of families cook on open fires, and a family of 7 consumes 5 tonnes of wood per year. This enormous consumption of firewood has already led to almost total deforestation and the progressive spread of deserts, especially in the poor north of the country. Energy-efficient stoves use about 80% less wood.
- Atmosfair uses donations to make small-scale biogas plants more affordable in Nepal. This project targets families living in rural areas who previously used wood as an energy source for cooking. In this way, the increasing deforestation of Nepal's forests can be counteracted.
- Atmosfair uses donations to support a small hydropower plant in Honduras. In this way, four villages that previously used wood and diesel generators for energy supply could be connected to the electricity grid for the first time. In addition, electricity can be fed into the national grid, replacing electricity from gas-fired power plants.

Source: atmosfair

Introduction calorie estimation

You will now estimate the energy value of each dish in kilocalories (kcal) for a total of five dishes. For each estimation question, the completion time is **limited to 60 seconds**. For each estimation question where your estimate does not deviate from the correct value by more than 30%, **your payout increases by 0.10 euros**.

What assumptions should be made for the estimation?

You will not be presented with ingredient lists for the following estimation questions. This is because we want to give you, as much as possible, only the information that you would find in the restaurant. We want to know how you estimate the energy value of a dish, based solely on the name of the dish in the menu.

Continue

Example calorie estimation

Remaining time on this page. **0:54**

What do you estimate: What is the energy value in kilocalories (kcal) of the dish ‘Beef ragout with potatoes’?

Beef ragout with potatoes

CO_2 **Causes** 3,4 kg CO_2

≈ 17,0 kmCar drive

Beef

I estimate that the dish ‘Beef ragout with potatoes’ has

[ ] kcal.

Continue

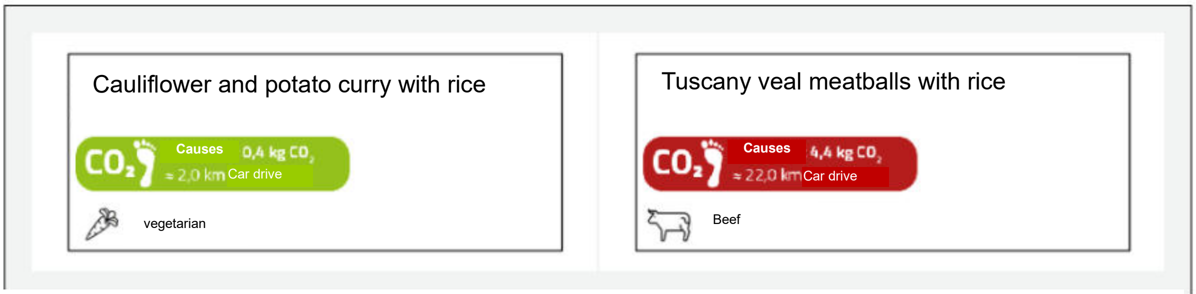
WTP for label presence

You are about to make the last three of the 15 decisions. One of the 15 decisions will actually be implemented.

But now there are two differences:

5. There are now three **new dishes** that you have not seen in your previous decisions.
6. You can see **emission labels** for these three dishes. These labels show the greenhouse gas emissions of the dishes in CO2 equivalents.

For example, two of the labels might look like this:



The display of the labels can either be preset so that:

- The labels are also displayed to you, or that
- The labels are not displayed to you.

Chance decides whether the display setting of the labels corresponds to your wishes without charge.

- Case 1 (probability 50%): We (do not) display the labels according to your wishes.
- Case 2 (probability 50%): The labels are initially preset so that it does not correspond to your wishes. For this case, you specify the **maximum** amount of your expense compensation you would like to give up in order to get your preferred display setting instead.

If **case 2** occurs, chance decides again:

- A **price** is determined randomly. Every value between 0€ and 3€ (in 5 cent steps) is equally probable.
- If the given amount is **higher** than the price, you will still get your **preferred display setting**. For this**, the charge will be deducted from your expense compensation**. However, this will only happen if one of the three dishes shown equally actually determines your payout.
- If the specified amount is **less** than the price, you will receive your **non-preferred display setting for free**.

Which display settings do you prefer? Click on one of the two buttons.

Labels should be **shown** | Labels should **not be shown**

If the display of labels is **not** preset and one of the three choices, you make now actually determines your payout: What is the **maximum** amount of your expense compensation you would like to give up in order to have the labels displayed?

(Click on the gray bar to make the slider visible).

0.00€ 1.00€ 2.00€ 3.00€

You want to give up a maximum of **1.70 €** of your expense compensation to **unlock the display of labels**.

Continue

Appendix C Experiments 1 and 3: Additional tables and figures

C.1 Randomization checks

Table C.1 shows a randomization check for participants of Experiment 1. Participants are computer assigned into one of the following three groups: 1) Label condition in the second round and Offset condition in the third round, 2) Control condition in the second round and Label condition in the third round, 3) Control condition in the second round and Control condition in the third round. Table C.1 tests whether there are significant differences between these three groups in age, gender, student status, employment, vegetarianism, and hunger at the time of the experiment. There is a higher proportion of non-vegetarians in the group “Control, then Control” (significant at the 5% level), but the groups do not significantly vary otherwise.

To test whether the higher proportion of non-vegetarians impacts results, I perform the main analysis separately for vegetarian and non-vegetarian participants. These analyses should not be influenced by the higher proportion of non-vegetarians in the control group. Results are shown in Table C.8 and Table C.9. Results only including non-vegetarians are similar in coefficient size to the main results. I thus do not believe that the higher proportion of non-vegetarians in the “Control, then Control” group poses a reason for concern.

Table C.1. Randomization Experiment 1

	Average value					
	(1) Age	(2) Male	(3) Student	(4) Working	(5) Non-vegetarian	(6) Hungry
Control, then Control	-0.53	-0.00	0.08	0.05	-0.14**	0.06
	(1.08)	(0.07)	(0.06)	(0.07)	(0.06)	(0.37)
Control, then Label	-0.75	-0.01	0.00	0.10	-0.08	-0.03
	(1.08)	(0.07)	(0.06)	(0.07)	(0.06)	(0.38)
Constant	24.56***	0.33***	0.78***	0.58***	0.81***	5.15***
	(0.62)	(0.04)	(0.03)	(0.04)	(0.04)	(0.21)
Control, then Control	61	71	71	71	71	71
Control, then Label	62	69	69	69	69	69
Label, then Offset	127	149	149	149	149	149
Observations	250	289	289	289	289	289

Standard errors in parentheses

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

Note: The analysis checks whether there are significant differences in any of the six variables between treatment groups. The group “Label, then Offset” is the baseline category. I do not have full observations for the variable “age”, since some participants reported unrealistic numbers Summary statistics for each variable are shown in Table C.3.

Table C.2. Randomization Experiment 3

	Average value					
	(1) Age	(2) Male	(3) Student	(4) Working	(5) Non-vegetarian	(6) Hungry
Attention+Offset, then Attention+Labels	0.04	-0.01	-0.00	0.00	0.03	0.27
	(0.88)	(0.06)	(0.05)	(0.05)	(0.05)	(0.29)
Attention+Labels, then Attention+Offset	-0.53	0.02	0.01	-0.04	0.04	0.10
	(0.89)	(0.06)	(0.05)	(0.05)	(0.05)	(0.30)
Constant	25.93***	0.45***	0.69***	0.75***	0.74***	4.73***
	(0.63)	(0.04)	(0.04)	(0.04)	(0.03)	(0.21)
Attention, then Attention	124	151	151	151	151	151
Attention+Label, then Attention+Offset	126	144	144	144	144	144
Attention+Offset, then Attention+Label	131	149	149	149	149	149
Observations	381	444	444	444	444	444

Standard errors in parentheses

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

Note: The analysis checks whether there are significant differences in any of the six variables between treatment groups. The group "Attention, then Attention" is the baseline category. I do not have full observations for the variable "age", since some participants reported unrealistic numbers Summary statistics for each variable are shown in Table C.4.

C.2 Representativeness of the sample

Tables C.3 and C.4 report descriptive statistics for experiments 1 and 3. Table C.5 reports descriptive statistics elicited in a survey among student canteen guests, as described in Section D.1.6. In terms of age, participants of experiments 1 and 3 are slightly older than the student canteen guests (average age of 24 and 26 vs. an average age of 23 in the survey). The proportion of males is slightly lower in Experiment 1 (33%) and slightly higher in Experiment 3 (45%) than in the survey (40%), while the proportion of non-vegetarians is similar across all three data sources (70%–76%). In the student canteen purchase data analyzed in Experiment 2, 66% of guests paying with an individual payment card make at least one non-vegetarian purchase during the sample period.

The proportion of students is higher in the survey (93%)than in experiments 1 and 3 (80% and 69%). However, it is likely that my survey over-proportionally surveyed student canteen guests who are students. In the student canteen purchase data analyzed in Experiment 2, 17% of guests paying with an individualized payment card are employees, 81% are students and 2% are non-student and non-employee.[54]

Overall, these statistics suggest that the participants of experiments 1 and 3 are fairly representative of student canteen guests. The largest difference between the experiment sample and survey and student canteen data is the proportion of non-students present in both. Tables C.10, C.18 and C.14 thus repeat the main analyses from experiments 1 and 3 including only students. Results are similar to those reported in sections 2 and 5, so results are not driven by a higher proportion of non-students.

54. This is the only demographic characteristic reported in the student canteen purchase data. I thus rely on the survey data for the other characteristics.

Table C.3. Experiment 1: Socio-economic summary statistics

Variable	Explanation	Mean	Std. Dev.
Age	Age of participant	24.16	7.05
Male	Dummy: 1 if participant is a man	0.33	–
Student	Dummy: 1 if participant is a student	0.80	–
Working	Dummy: 1 if participant is working in some form	0.62	–
Non-vegetarian	Dummy: 1 if participant eats meat	0.75	–
Hungry	Hunger on scale of 1 to 10 beginning experiment	4.16	2.58
N	289		

Note: Table shows average socio-economic summary statistics for participants of Experiment 1.

Table C.4. Experiment 3: Socio-economic summary statistics

Variable	Explanation	Mean	Std. Dev.
Age	Age of participant	25.77	7.02
Male	Dummy: 1 if participant is a man	0.45	–
Student	Dummy: 1 if participant is a student	0.69	–
Working	Dummy: 1 if participant is working in some form	0.74	–
Non-vegetarian	Dummy: 1 if participant eats meat	0.76	–
Hungry	Hunger on scale of 1 to 10 beginning experiment	4.85	2.54
N	444		

Note: Table shows average socio-economic summary statistics for participants of Experiment 3.

Table C.5. Survey among student canteen guests: Socio-economic summary statistics

Variable	Explanation	Mean	Std. Dev.
Age	Age of participant	22.90	–
Male	Dummy: 1 if participant is a man	0.41	–
Student	Dummy: 1 if participant is a student	0.94	–
Non-vegetarian	Dummy: 1 if participant eats meat	0.68	–
N	1,451		

Note: Statistics are based on a survey I conducted among student canteen guests in April. I include only survey respondents who visited a student canteen at least once in the 14-week study period and paid with their individual payment cards. See D.1.6 for details on the survey design. To preserve anonymity (since I also asked these survey participants about their study field), I elicited age in intervals. To reach an estimation of the mean age, I set the age equal to the midpoint of each interval. For 13% of respondents, I have the information that they are below 20. For the calculation, I estimate their age at 18. For 53% of respondents, I have the information that they are between 20 and 23 (which I set to 21.5 for the estimation), 23% of respondents are between 24 and 27 (set to 25.5), 6% of respondents are between 28 and 31 (set to 30), and 4% of respondents are 32 or older (set to 35). I did not directly elicit vegetarianism, but I elicited how much of a role animal rights play in participants' consumption decisions. I code participants reporting the highest degree of importance as vegetarians.

C.3 Descriptive statistics on baseline willingness to pay for meals

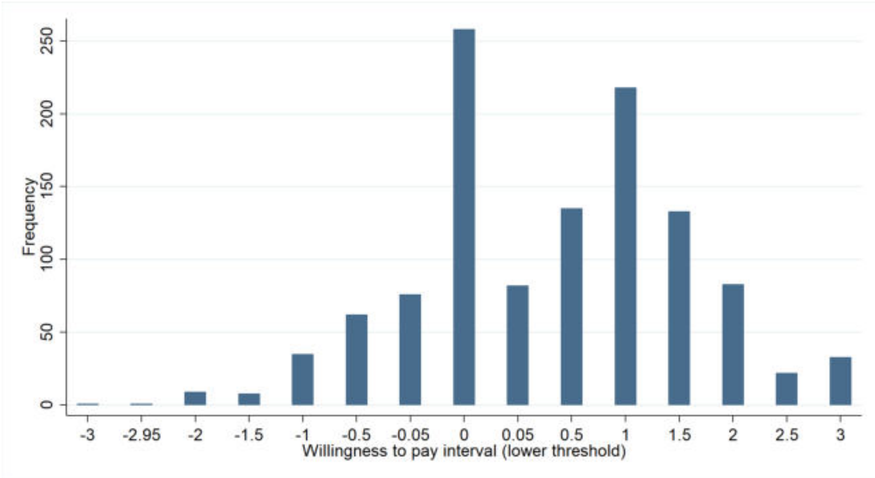


Figure C.1. Willingness to pay indicated for meals during the baseline purchase decisions in Experiment 1. $N = 1,156$ (289 participants making 4 baseline decisions each).

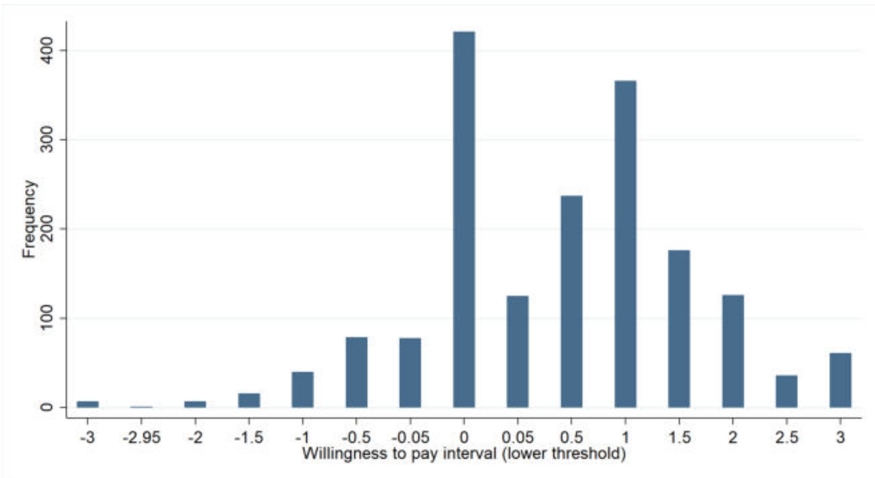


Figure C.2. Willingness to pay indicated for meals during the baseline purchase decisions in Experiment 3. $N = 1,776$ (444 participants making 4 baseline decisions each).

C.4 Comparison of effects Exp. 1 and Exp. 3

Table C.6. Comparison of average effects in Experiment 1

	Change in WTP					
	(1)	(2)	(3)	(4)	(5)	(6)
	Con.	Con.	La.	La.	Of.	Of.
High emission meal	0.01		-0.15***		0.13***	
	(0.01)		(0.03)		(0.03)	
Low emission meal	-0.05*		0.08*		-0.04	
	(0.03)		(0.04)		(0.03)	
Emissions(kg)		0.01		-0.10***		0.09***
		(0.01)		(0.02)		(0.02)
Control for third round	-0.01	-0.01	0.03	0.02		
	(0.02)	(0.02)	(0.04)	(0.04)		
Constant		-0.01		-0.05**		0.04***
		(0.01)		(0.02)		(0.02)
Participants	140	140	218	218	149	149
Observations	1,452	1,452	1,485	1,485	1,009	1,009

Standard errors in parentheses

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

Note: Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline (Col. 1-4) or WTP with carbon labels (Col. 5-6). Regression specifications follow 1. Col. (1) and (2) include only participants in the Control condition, Col. (3) and (4) only participants in the Label condition, and Col. (5) and (6) only participants in the Offset. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). To analyze the Offset condition, I use as the dependent variable participants' WTP for meals with offsetting minus WTP with carbon labels. This is to isolate the effect of the offsetting, keeping the attention (Informing participants about carbon offsetting draws attention to emissions) and information effect (With offsetting, participants are informed that emissions are zero) of carbon offsetting constant. Spec. (5) and (6) do not control for the third round of decisions, since Experiment 1 has an Offset condition only in the third round and not in the second round of decisions. Standard errors are clustered at the individual level. Bars indicate 95% confidence intervals.

Table C.7. Comparison of effect sizes in Experiment 3

	Change in WTP					
	(1)	(2)	(3)	(4)	(5)	(6)
	At.+La.	At.+La.	At.	At.	At.+Of.	At.+Of.
High emission meal	-0.10***		-0.04***		0.08***	
	(0.02)		(0.01)		(0.02)	
Low emission meal	-0.04		-0.01		0.02	
	(0.03)		(0.03)		(0.03)	
Emissions(kg)		-0.08***		-0.06***		0.07***
		(0.01)		(0.02)		(0.01)
Control for third round	0.03	0.03	0.01	0.01	-0.01	-0.01
	(0.03)	(0.03)	(0.01)	(0.01)	(0.02)	(0.02)
Constant		-0.04**		-0.00		0.03**
		(0.02)		(0.01)		(0.02)
Participants	293	293	151	151	293	293
Observations	2,051	2,051	2,114	2,114	2,051	2,051

Standard errors in parentheses

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

Note: Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline (Col. 1-4) or WTP with carbon labels (Col. 5-6). Regression specifications follow 1. Col. (1) and (2) include only participants in the Attention+Label condition, Col. (3) and (4) only participants in the Attention condition, and Col. (5) and (6) only participants in the Attention+Offset. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). To analyze the Attention+Offset condition, I use as the dependent variable participants' WTP for meals with offsetting minus WTP with carbon labels. This is to isolate the effect of the offsetting, keeping the attention (Informing participants about carbon offsetting draws attention to emissions) and information effect (With offsetting, participants are informed that emissions are zero) of carbon offsetting constant. Standard errors are clustered at the individual level. Bars indicate 95% confidence intervals.

C.5 Results split by (non-) vegetarians and (non-) students

Experiment 1.

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal x Shown label	-0.26^{***}	
	(0.05)	
Low emission meal x Shown label	0.17^{***}	
	(0.06)	
High emission meal	-0.00	
	(0.02)	
Low emission meal	-0.10^{**}	
	(0.05)	
Emissions(kg) x Shown label		-0.12^{***}
		(0.03)
Emissions(kg)		0.03^{**}
		(0.01)
Shown label		-0.04
		(0.04)
Control for third round	0.01	0.01
	(0.04)	(0.04)
Constant		-0.05^{*}
		(0.03)
Participants control	97	97
Participants treated	170	170
Observations	1,256	1,256

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.8. Replication of Table 1 including only non-vegetarians.

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal x Shown label	-0.53^{***}	
	(0.11)	
Low emission meal x Shown label	0.11	
	(0.07)	
High emission meal	0.06	
	(0.05)	
Low emission meal	-0.02	
	(0.04)	
Emissions(kg) x Shown label		-0.75^{***}
		(0.18)
Emissions(kg)		0.08
		(0.08)
Shown label		-0.08
		(0.05)
Control for third round	0.04	0.04
	(0.04)	(0.04)
Constant		0.00
		(0.02)
Participants control	43	43
Participants treated	48	48
Observations	460	460

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.9. Replication of Table 1 including only vegetarians.

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal x Shown label	-0.29^{***}	
	(0.05)	
Low emission meal x Shown label	0.15^{***}	
	(0.05)	
High emission meal	-0.01	
	(0.02)	
Low emission meal	-0.08^{**}	
	(0.03)	
Emissions(kg) x Shown label		-0.13^{***}
		(0.03)
Emissions(kg)		0.01
		(0.01)
Shown label		-0.05
		(0.04)
Control for third round	0.01	0.01
	(0.03)	(0.03)
Constant		-0.04^{**}
		(0.02)
Participants control	115	115
Participants treated	170	170
Observations	1,384	1,384

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.10. Replication of Table 1 including only students.

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal x Shown label	-0.41^{***}	
	(0.09)	
Low emission meal x Shown label	0.03	
	(0.07)	
High emission meal	0.12^{**}	
	(0.06)	
Low emission meal	0.08	
	(0.07)	
Emissions(kg) x Shown label		-0.08
		(0.08)
Emissions(kg)		0.02
		(0.03)
Shown label		-0.22^{***}
		(0.07)
Control for third round	0.05	0.05
	(0.09)	(0.09)
Constant		0.10^{*}
		(0.06)
Participants control	25	25
Participants treated	48	48
Observations	332	332

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.11. Replication of Table 1 including only non-students.

Experiment 3.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	−0.11**	
	(0.04)	
Underestimation (in kg)		−0.06**
		(0.03)
Control for third round	0.05	0.05
	(0.05)	(0.05)
Constant	−0.12***	−0.16***
	(0.04)	(0.04)
Participants	227	206
Obs. underestimate	451	420
Obs. overestimate	418	364
Observations	869	784

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.12. Replication of Table 3 including only non-vegetarians.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	−0.21***	
	(0.07)	
Underestimation (in kg)		−0.14**
		(0.06)
Control for third round	0.05	0.13
	(0.10)	(0.09)
Constant	−0.02	−0.18**
	(0.09)	(0.07)
Participants	66	63
Obs. underestimate	104	96
Obs. overestimate	144	130
Observations	248	226

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.13. Replication of Table 3 including only vegetarians.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	-0.18***	
	(0.04)	
Underestimation (in kg)		-0.10***
		(0.03)
Control for third round	0.10*	0.11**
	(0.05)	(0.06)
Constant	-0.12**	-0.21***
	(0.05)	(0.04)
Participants	203	184
Obs. underestimate	383	360
Obs. overestimate	391	344
Observations	774	704

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.14. Replication of Table 3 including only students.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	-0.00	
	(0.05)	
Underestimation (in kg)		-0.02
		(0.04)
Control for third round	-0.06	-0.06
	(0.08)	(0.09)
Constant	-0.05	-0.05
	(0.06)	(0.05)
Participants	90	81
Obs. underestimate	172	158
Obs. overestimate	171	153
Observations	343	311

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.15. Replication of Table 3 including only non-students.

	Change in WTP compared to baseline
	(1)
High emission meal x Shown label	−0.10**
	(0.04)
Low emission meal x Shown label	−0.06
	(0.05)
High emission meal	−0.11***
	(0.03)
Low emission meal	−0.01
	(0.04)
Control for third round	0.04
	(0.03)
Participants attent	112
Participants label	227
Observations	1,804

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.16. Replication of Table 4 including only non-vegetarians.

	Change in WTP compared to baseline
	(1)
High emission meal x Shown label	−0.12
	(0.08)
Low emission meal x Shown label	0.03
	(0.06)
High emission meal	−0.05
	(0.04)
Low emission meal	−0.04
	(0.04)
Control for third round	0.02
	(0.04)
Participants attent	39
Participants label	66
Observations	576

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.17. Replication of Table 4 including only vegetarians.

	Change in WTP compared to baseline
	(1)
High emission meal x Shown label	-0.17***
	(0.04)
Low emission meal x Shown label	-0.02
	(0.05)
High emission meal	-0.08***
	(0.03)
Low emission meal	-0.03
	(0.03)
Control for third round	0.05*
	(0.03)
Participants attent	104
Participants label	203
Observations	1,644

Standard errors in parentheses

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

Table C.18. Replication of Table 4 including only students.

	Change in WTP compared to baseline
	(1)
High emission meal x Shown label	0.04
	(0.08)
Low emission meal x Shown label	-0.03
	(0.08)
High emission meal	-0.14**
	(0.06)
Low emission meal	-0.00
	(0.06)
Control for third round	-0.01
	(0.04)
Participants attent	47
Participants label	90
Observations	736

Standard errors in parentheses

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

Table C.19. Replication of Table 4 including only non-students.

C.6 Replication excluding round 3 observations

Table C.20. Replication of Table 1 excluding round 3 observations

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal x Shown label	-0.34***	
	(0.06)	
Low emission meal x Shown label	0.15**	
	(0.06)	
High emission meal	0.02	
	(0.02)	
Low emission meal	-0.05*	
	(0.03)	
Emissions(kg) x Shown label		-0.15***
		(0.04)
Emissions(kg)		0.03**
		(0.01)
Shown label		-0.07*
		(0.04)
Control for third round		
Constant		-0.02
		(0.02)
Participants control	140	140
Participants treated	149	149
Observations	1,156	1,156

Standard errors in parentheses

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

Table C.21. Replication of Table 3 excluding round 3 observations

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	-0.12**	
	(0.05)	
Underestimation (in kg)		-0.06*
		(0.03)
Constant	-0.10**	-0.17***
	(0.04)	(0.03)
Participants	144	133
Obs. underestimate	269	248
Obs. overestimate	281	248
Observations	550	496

Standard errors in parentheses

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

Table C.22. Replication of Table 4 excluding round 3 observations

	Change in WTP compared to baseline
	(1)
High emission meal x Shown label	-0.11**
	(0.05)
Low emission meal x Shown label	-0.06
	(0.05)
High emission meal	-0.09***
	(0.03)
Low emission meal	-0.01
	(0.03)
Participants attent	151
Participants label	144
Observations	1,180

Standard errors in parentheses

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

C.7 Exp. 1: Alternative econometric specifications

Alternatively to the estimation approach described in Section 2.2, one could instead estimate the following specification:

$$WTP_{ijm} = \alpha_{im} + \beta_1(High_m \times Post_j) + \beta_2(Low_m \times Post_j) + \delta_1(High_m \times Post_j \times Label_{ij}) + \delta_2(Low_m \times Post_j * Label_{ij}) + ThirdRound_j + \varepsilon_{ijm} \quad \text{(C.1)}$$

This specification is more similar to a classic diff-in-diff approach. Instead of directly using the difference between indicated willingness to pay for a meal and baseline willingness to pay as the dependent variable (as in 1), I use raw willingness to pay of individual i in round j for meal m as the dependent variable. Accordingly, I also include observations from the baseline elicitation round in the regression.

α_{im} are individual and meal-specific fixed effects. These are 1156 fixed effects in total: 289 participants × 4 meals. These fixed effects control for individual-specific baseline tastes. Note that it would not make much sense to include merely a single fixed effect for each individual. A single fixed effect would capture the average willingness to pay of each individual across the four meals. However, I expect the effect of the carbon labels to differ across meals. Willingness to pay for low-emission meals should increase as a result of the label, while willingness to pay for high-emission meals should decrease. It is thus insufficient to control for individuals' willingness to pay averaged across meals. To illustrate with an example, imagine I only had two meals, one low-emission and one high-emission meal. An individual has a willingness to pay of €1.00 for the low-emission meal and a willingness to pay of €3.00 for the high-emission meal. When the individual sees the carbon labels, he adjusts his willingness to pay for the low-emission meal upward to €2.00 euros, and his willingness to pay for the high-emission meal downward to €2.00 euros. Treatment effects are thus sizable. However, his average willingness to pay for the two meals did not change, and a regression including a single individual fixed effect term would falsely not identify a treatment effect.

$(High_m \times Post_j)$ is an indicator variable for whether the meal causes higher emissions than the sandwich, and interacted with the elicitation round $j > 1$, i.e. it being the second or third round of elicitations and not the baseline round. $(Low_m \times Post_j)$ is the equivalent indicator for low-emission meals. Note that all meals classified are classified either as Low_m or $High_m$. The two variables thus together capture the $Post_j$ effect, and a separate $Post_j$ indicator would be dropped due to collinearity. I also do not include separate controls for Low_m and $High_m$ since meal characteristics are captured by the α_{im} fixed effects.

$(High_m \times Post_j \times Label_{ij})$ interacts the high-emission and $Post_j$ indicator with an indicator for whether individual i saw carbon labels in round j. This describes the average causal effect of carbon labels on willingness to pay for a meal that is high in carbon emissions. $(Low_m \times Post_j \times Label_{ij})$ describes the average causal effect of carbon labels on willingness to pay for a meal that is low in carbon emissions. $ThirdRound_j$ is an indicator of whether it was the third round of decisions. Standard errors are clustered at the individual level.

Spec. (1) in Table C.23 shows regression results. They are very similar to those reported in the main text. Spec. (2) replicates Spec. (2) of Table 1 with a fixed effect approach and also finds similar results as reported in the main text.

Table C.23. Replication of Experiment 1 results with fixed effects approach

	WTP	
	(1)	(2)
High x Post x Label	-0.30^{***}	
	(0.04)	
Low x Post x Label	0.09^{**}	
	(0.04)	
High x Post	0.01	
	(0.02)	
Low x Post	-0.03	
	(0.04)	
Emissions(kg) x Post x Label		-0.12^{***}
		(0.03)
Emissions(kg) x Post		0.01
		(0.01)
Post x Label		-0.08^{***}
		(0.03)
Post		-0.02
		(0.02)
Control for third round	0.01	0.01
	(0.03)	(0.03)
Constant	0.65^{***}	0.65^{***}
	(0.01)	(0.01)
Participants control	140	140
Participants treated	218	218
Observations	2,872	2,872

Standard errors in parentheses

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

Note: Table replicates the estimation in Table 1 using willingness to pay for meals directly as the outcome variable, instead of taking the difference. Spec. (1) corresponds to Equation C.1 and includes individual× meal fixed effects. It does not include a "Post" or a "Post× Label" variable, because "Low emissions meal" and "High emissions meal" are mutually exclusive. In spec. (2), emissions (kg) are defined as the emissions caused by the meal relative to the cheese sandwich. This is positive for "high-emission" and negative for "low-emission" meals. Standard errors are clustered at the individual level.

C.8 Exp. 1: Reaction to carbon labels by baseline WTP

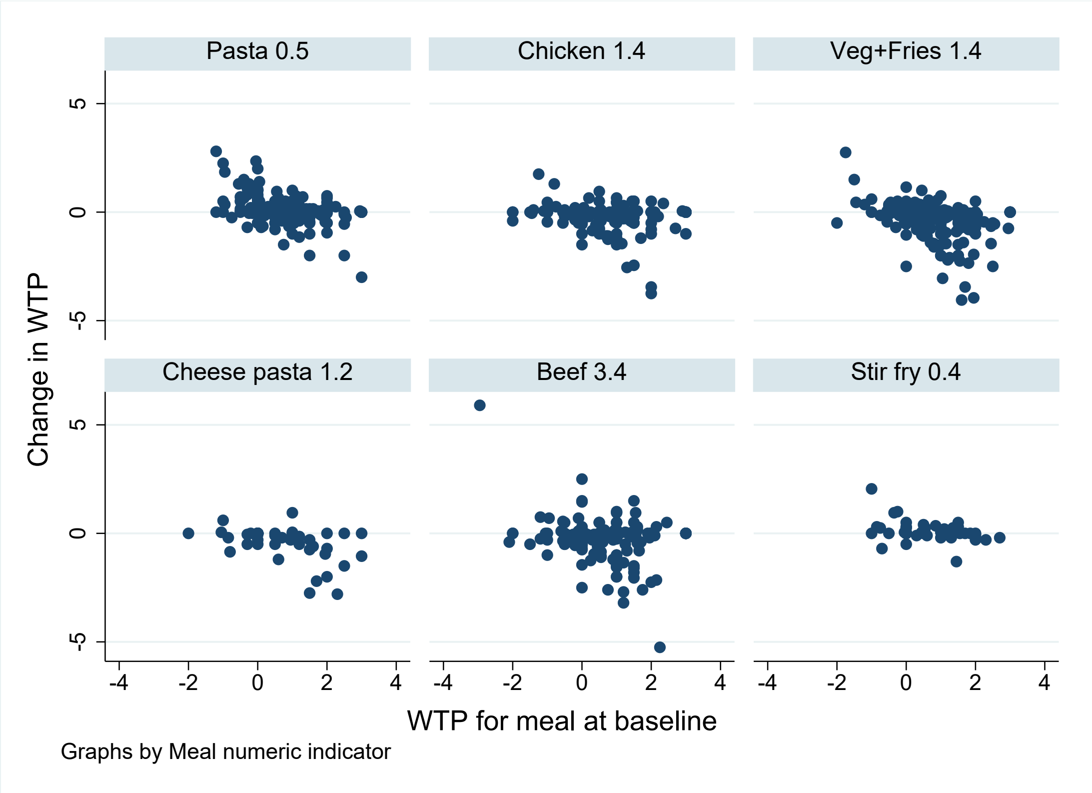


Figure C.3. Scatterplot of participants' change in willingness to pay for meals when shown carbon labels, by baseline willingness to pay. $N = 218$ for meals shown to vegetarians and non-vegetarians: Vegetable pasta (emissions 0.5 kg), Courgettes with fries (emissions 1.4 kg). $N = 170$ for meals shown only to non-vegetarians: Chicken with rice (emissions 1.4 kg) and beef with potatoes (3.4 kg). $N = 48$ for meals only shown to vegetarians: Cheese pasta "Spätzle" (1.2 kg) and Stir-fried vegetables (0.4 kg). Plots show that individuals across a variety of baseline WTP categories are reacting to the carbon labels—It is not a couple of individuals alone driving effects.

C.9 Exp. 1: Heterogeneity in treatment effects

Table C.24. Heterogeneity based on same items as heterogeneity analysis in the field (Table E.6)

	Change in WTP compared to baseline			
	(1)	(2)	(3)	(4)
	All	Female	Below 24	Env. important
High emission meal x Shown label	-0.31^{***}	-0.36^{***}	-0.32^{***}	-0.39^{***}
	(0.05)	(0.06)	(0.07)	(0.07)
Low emission meal x Shown label	0.14^{***}	0.10^{*}	0.12^{**}	0.17^{***}
	(0.04)	(0.05)	(0.06)	(0.06)
High emission meal	0.01	0.00	−0.02	−0.00
	(0.02)	(0.02)	(0.03)	(0.03)
Low emission meal	-0.06^{*}	−0.04	−0.05	-0.08^{**}
	(0.03)	(0.03)	(0.04)	(0.03)
Control for third round	0.01	0.03	0.03	0.04
	(0.03)	(0.04)	(0.05)	(0.04)
Participants control	140	95	80	90
Participants treated	218	147	118	123
Observations	1,716	1,160	952	1,040

Standard errors in parentheses

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

Note: Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline. Specifications correspond to Equation 1 and do not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. Col. (1) includes all data, and Col. (2) includes only females. Col. (3) includes only under 24-year olds. Col. (5) includes only survey participants who report an above-average importance of environmental aspects in their food consumption decisions. Standard errors are clustered at the individual level. Table E.7 reports evidence from Experiment 2 for the same heterogeneity factors.

Table C.25. Heterogeneity based on same items as correlation analysis on WTP determinants (Table 7)

	Change in WTP compared to baseline					
	(1) All	(2) Strong norms	(3) In favor	(4) Use info	(5) Own knowledge	(6) High self-control
High emission meal x Shown label	-0.31^{***}	-0.36^{***}	-0.42^{***}	-0.47^{***}	-0.29^{***}	-0.33^{***}
	(0.05)	(0.06)	(0.07)	(0.08)	(0.05)	(0.07)
Low emission meal x Shown label	0.14^{***}	0.16^{***}	0.13^{**}	0.21^{***}	0.11^{**}	0.22^{***}
	(0.04)	(0.06)	(0.07)	(0.07)	(0.05)	(0.06)
High emission meal	0.01	−0.01	0.01	−0.01	0.02	0.00
	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Low emission meal	-0.06^{*}	−0.04	−0.05	-0.07^{*}	-0.08^{*}	-0.10^{**}
	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)
Control for third round	0.01	−0.00	0.03	0.02	0.04	0.01
	(0.03)	(0.04)	(0.04)	(0.05)	(0.04)	(0.05)
Participants control	140	71	78	76	69	70
Participants treated	218	107	105	106	135	105
Observations	1,716	880	916	912	932	844

Standard errors in parentheses

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

Note: Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline. Specifications correspond to Equation 1 and do not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. Regressions include only individuals who report above-average values for the respective items. "In favor of labels in student canteen" is measuring using approval of the statement "I would appreciate if the student canteen would introduce such a measure". "Self-reported willingness to use info" is measured using approval of the statement "I would include this information in my decision.". "Self-reported confidence in own knowledge" is measured with two questions: (1) approval of the statement "I already know without labels which emissions are caused by different meals.", and (2) "I think this information will partially surprise me." The perceived strength of social norms is measured using the procedure developed by Krupka and Weber (2013). Eating self-control is measured using the questions developed by Haws, Davis, and Dholakia (2016). Standard errors are clustered at the individual level.

Table C.26. Heterogeneity in Experiment 1

	Change in WTP compared to baseline					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Social circle	Hungry	Strong l.o.c.	Low income	Price sens.
High emission meal x Shown label	-0.31^{***}	-0.36^{***}	-0.40^{***}	-0.40^{***}	-0.33^{***}	-0.27^{***}
	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)	(0.05)
Low emission meal x Shown label	0.14^{***}	0.14^{**}	0.08	0.18^{***}	0.12^{*}	0.19^{***}
	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)
High emission meal	0.01	0.04	0.05	0.04	0.05^{*}	0.05^{*}
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
Low emission meal	-0.06^{*}	-0.04	-0.02	-0.05	-0.01	-0.12^{**}
	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)
Control for third round	0.01	0.03	0.02	0.03	0.00	0.01
	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)
Participants control	140	79	66	84	54	60
Participants treated	218	123	104	118	82	109
Observations	1,716	968	816	988	656	800

Standard errors in parentheses

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

Note: Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline. Specifications correspond to Equation 1 and do not include a constant, because “Low emissions meal” and “High emissions meal” are mutually exclusive. Regressions include only individuals who report above-average values for the respective items. “Social circle” is measured using approval of the statement “My friends and acquaintances would approve if I try to reduce my carbon footprint.”. “Hungry” is measured on a 10-point scale using the question “How hungry are you feeling now, in this moment?”. “L.o.c. (Locus of control)” is measured with approval of the statement “I believe I can contribute to the solution of the climate crisis by reducing my carbon footprint.”. “Low income” includes only individuals with the lowest possible net income option (under €700 a month). “Price sensitive” is measured using participants’ reports of the importance of meal price in a typical meal decision. Standard errors are clustered at the individual level.

C.10 Exp. 1: Effect on calorie guesses

Table C.27. Experiment 1: Effects of the treatment on calories guessed

	Guess of calories in				
	(1)	(2)	(3)	(4)	(5)
	Chicken-rice	Courgettes-fries	Beef-potatoes	Cheese sandwich	Veg. pasta
Sees carbon labels	81.31	131.47	3.15	24.13	85.67
	(185.95)	(113.90)	(58.00)	(23.36)	(109.30)
Constant	639.36***	506.27***	732.98***	272.62***	518.82***
	(164.59)	(98.92)	(51.33)	(20.29)	(94.93)
Participants control	71	71	71	71	71
Participants treated	218	218	218	218	218
Observations	217	289	217	289	289

Standard errors in parentheses

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

Note: To test whether participants conclude other meal characteristics when seeing carbon labels, I ask participants to guess the calories of different meals towards the end of the experiment. Participants in the TREATMENT see carbon labels during the guess, while participants in the CONTROL group do not. There is no significant effect of seeing the labels on calorie guesses.

C.11 Exp. 3: Descriptives on under- and over-estimation

Table C.28. Under- and over-estimation of the emissions caused by the decision meals in the ATTENT+LABEL treatment

Meal	Relative emissions	No. underestimated	No. overestimated	No. correct	Total
Vegetable pasta	-0.2 kg	31	249	13	293
Chicken w. rice	0.7 kg	47	163	17	227
Courgettes w. fries	0.7 kg	249	33	11	293
Cheese pasta	0.5 kg	31	24	11	66
Beef w. potatoes	2.7 kg	193	32	2	227
Stir-fried veg.	-0.3 kg	4	61	1	66
Total	654	459	59	55	1.172

Note: Relative emissions are emissions relative to the cheese sandwich (0.7 kg). I classify a participant as underestimating this amount if their guess for the meal's emissions minus their guess for the cheese sandwich is lower than the actual relative emissions. I classify a participant as overestimating this amount if their guess for the meal's emissions minus their guess for the cheese sandwich is higher than the actual relative emissions.

Table C.29. Number of under- and over-estimations per participant

No. overestimated	0	1	2	3	4	Total
No. underestimated						
0	0	0	0	2	10	12
1	0	1	21	54	0	76
2	1	24	128	0	0	153
3	4	31	0	0	0	35
4	17	0	0	0	0	17
Total	22	56	149	56	10	293

Note: Relative emissions are emissions relative to the cheese sandwich (0.7 kg). I classify a participant as underestimating this amount if their guess for the meal's emissions minus their guess for the cheese sandwich is lower than the actual relative emissions. I classify a participant as overestimating this amount if their guess for the meal's emissions minus their guess for the cheese sandwich is higher than the actual relative emissions. Each cell shows the number of participants with the respective number of under- or over-estimations.

Table C.30. Number of participants who correctly guessed how the four decision meals rank relative to each other

No. of correctly ranked meals	No. participants
0	11
2	88
3	188
4	6
Total	293

Note: If a participant indicated emission values for the four decision meals such that the value he indicates for the lowest-ranking meal is the lowest in his ranking, the second-lowest-ranking meal is the second-lowest in his ranking, the third-lowest-ranking meal is the third-lowest, etc. I count him as getting all four relative ranks right. This is true for six participants. 188 participants got three relative ranks right, and 88 got two relative ranks right (i.e. two meals stood in the correct relationship to each other).

C.12 Exp. 3: Results split by guess accuracy

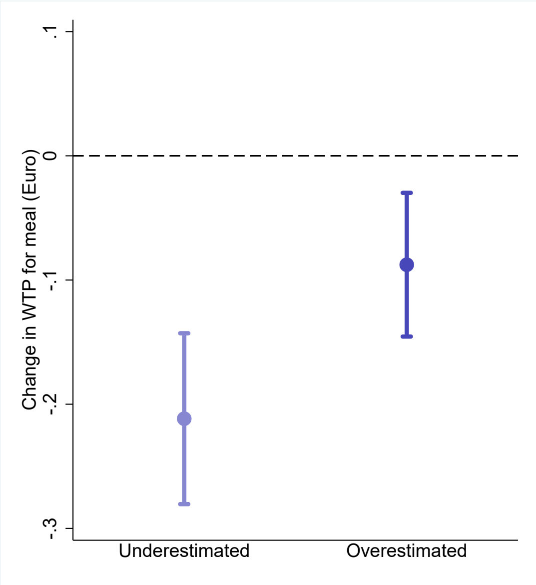


Figure C.4. Replication of Figure 15 including only individuals with at least three correct ranks (194 participants). Bars indicate 95% confidence intervals.

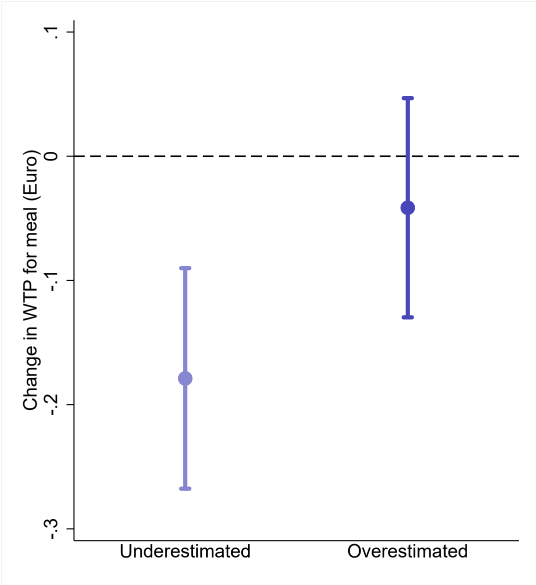


Figure C.5. Replication of Figure 15 including only individuals with at most two correct ranks (99 participants). Bars indicate 95% confidence intervals. Bars indicate 95% confidence intervals.

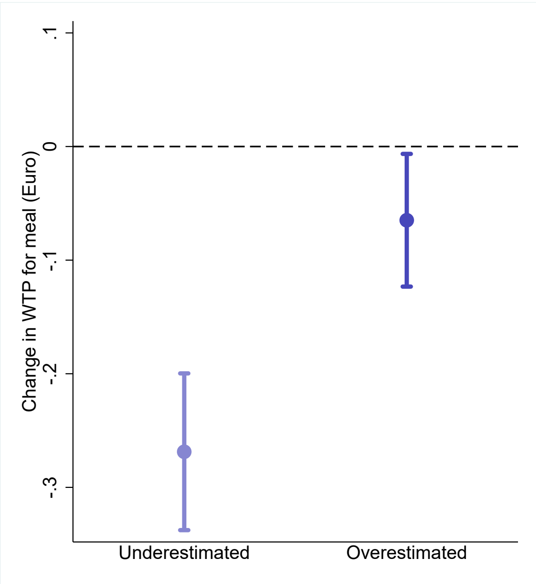


Figure C.6. Replication of Figure 15 including only individuals with at least three correctly guessed magnitudes (171 participants). Bars indicate 95% confidence intervals.

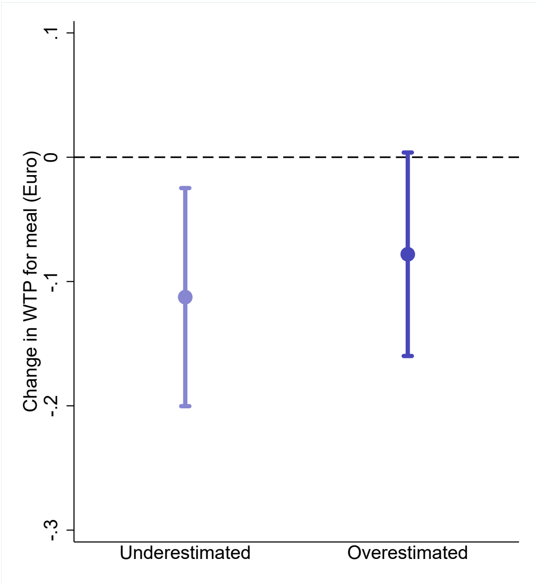


Figure C.7. Replication of Figure 15 including only individuals with at most two correctly guessed magnitudes (129 participants). Bars indicate 95% confidence intervals. Bars indicate 95% confidence intervals.

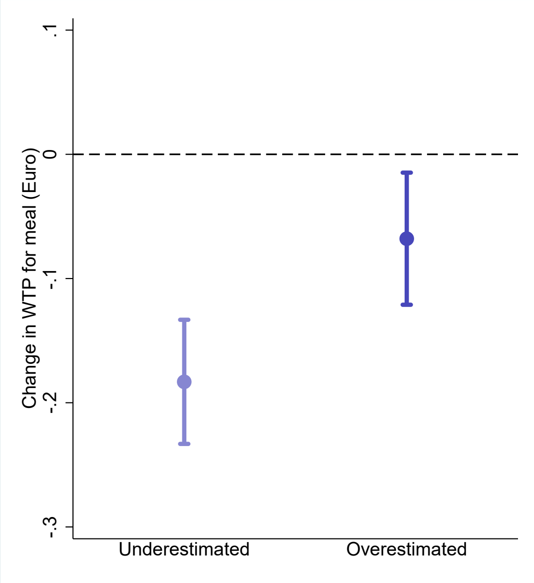


Figure C.8. Replication of Figure 15 based on under- or over-estimation of the specific meal, instead of under- or over-estimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	-0.13***	
	(0.04)	
Underestimation (in kg)		-0.04
		(0.03)
Control for third round	0.05	0.06
	(0.05)	(0.05)
Constant	-0.09***	-0.18***
	(0.03)	(0.03)
Participants	293	267
Obs. underestimate	651	640
Obs. overestimate	471	376
Observations	1,122	1,016

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table C.31. Replication of Table 3 based on under- or over-estimation of the specific meal, instead of under- or overestimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals. In specification (2), change in willingness to pay is regressed on underestimation in kg. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped.

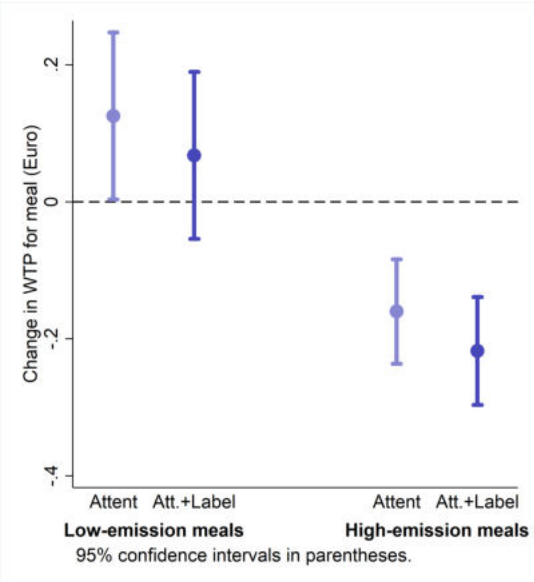


Figure C.9. Replication of Figure 16 including only participant-meal combinations where emissions were guessed accurately enough to receive a bonus payment (guess within 20% of true value, 543 observations). Bars indicate 95% confidence intervals.

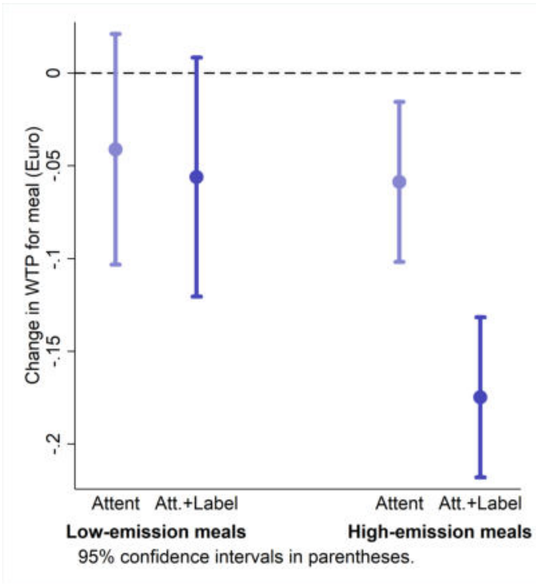


Figure C.10. Replication of Figure 16 including only participant-meal combinations where emissions were not guessed accurately enough to receive a bonus payment (guess not within 20% of true value, 1,837 observations)

C.13 Participants' willingness to pay for the presence of carbon labels

Table C.32. Willingness to pay for seeing carbon labels by treatment group

	(1) wtp
Control, then Label	-0.13 (0.08)
Label, then Offset	-0.11* (0.07)
Attent, then Attent	-0.08 (0.07)
Attent+Label, then Offset	-0.07 (0.07)
Attent+Offset, then Labels	-0.04 (0.07)
Control, then Control	0.00 (.)
Constant	0.28*** (0.05)
N	731

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Average deviation from the average willingness to pay to see emission labels for the final three consumption decisions, by treatment group. "Control, then Control" is the baseline condition.

Table C.33. Correlations between willingness to pay for seeing carbon labels and treatment effect

	(1)
Decrease in WTP for highest-emission meal	-0.21*** (0.02)
Constant	0.15*** (0.02)
Observations	397

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Dependent variable: Willingness to pay for seeing labels for the final three consumption decisions. Independent variable: The decrease in the participant's willingness to pay for the highest-emission meal when shown emission labels. Regression is restricted to participants who were shown emission values in the experiment. The coefficient signals that participants showing a stronger reaction to carbon labels are also willing to pay a higher amount to be shown the labels.

Appendix D Experiment 2: Details on experimental set-up

D.1 Setting and additional descriptive statistics

D.1.1 Canteen set-up in Bonn. The natural field experiment was conducted in the student canteens of the University of Bonn from April 2022 to July 2022. The whole of April (four weeks) served as a pre-intervention phase in which baseline consumption decisions were observed. Emission labels were introduced in the treatment student canteen from the beginning of May to mid-June 2022 (seven weeks). From mid-June to mid-July 2022 (three weeks, which ended with the summer closing of the treated student canteen), I continue to observe consumption decisions to examine post-intervention behavior.

There are three student canteens in Bonn: The treatment student canteen, the first control restaurant (located 1.7 km from the treatment restaurant), and the second control restaurant (located 4.7 km from the treatment restaurant and frequented much less than the other two restaurants). Menu planning is centralized among the three student canteens, and there is thus a large overlap in the daily offering. All three student canteens offer two main meal components, which differ daily but are mostly the same across student canteens. In addition, each of the student canteens might offer additional options, which are student-restaurant-specific. The larger control restaurant sometimes offers pizza or pasta in addition, and all student canteens might serve leftover main meal components from the previous day, soup, and side dishes. In the treatment restaurant, only the main meal components were equipped with carbon labels, and sides and leftover main meal components were not labeled. [55] Correspondingly, the dependent variable in my main regression is whether the main meal component a restaurant guest chooses contains meat or is vegetarian.

D.1.2 Canteen visiting patterns. An average student canteen guest visited the student canteen 20 times from April to mid-July. Around 74% visit 10 times or more, and around 45% visit 20 times or more. 90% of guests visited the same student canteen at least 80% of the time. The student canteens offer very cheap meals, with complete meals costing between €1.00 and €3.00. In fast food restaurants located in the surrounding area, meals are priced at €4.00 upward. In a survey I conducted among over 1,000 student canteen guests (survey 2 described in the Appendix), over 40% of students report that they would have difficulty finding an affordable meal if the student canteens did not exist. Switching between student canteens and other gastronomic offers is thus also not frequent. Figure D.1 in the Appendix includes an analysis based on the trackable personal card payments. I classify restaurant guests as "Treatment" or "Control" visitors based on their consumption behavior in the first two weeks. Around 3% of purchases made by "Control" visitors are made in the treated restaurant throughout the entire 14-week period. For "Treatment" visitors, the percentage fluctuates between 5% and 9%. Figure D.2 further examines which percentage of these non-home visits involve the consumption of a meat main component. There is no clear trend throughout the study period.

Further, an analysis of daily restaurant guests shows that the labeling intervention did not lead to a decrease in student canteen guests, relative to the control restaurant (see Figure 10). The introduction of carbon labels in the treatment restaurant was displayed as a measure taken by the student canteens themselves, with no connection presented to the University of Bonn or me specifically as the researcher. The introduction of the emission labels was explained on billboards and leaflets available inside the student canteen, as shown in Figure D.5. I conducted two surveys accompanying the

55. The main reason for this was that I wanted to test carbon labeling in a manner that was feasible for the student canteen to implement long-term. While main meal components are planned and known beforehand, sides and leftover dishes are decided spontaneously. Further, leftover main meal components only make up a smaller part of daily sales and the emissions caused by side dishes are almost negligible compared to those of the main meal components. Sales of all products are tracked, and label effects in the main sample are conservatively calculated over all main meal components offered, i.e. including main meal components spontaneously added to the menu but not labeled.

measure, one before the intervention period and one after the intervention period, further described in the Appendix. The surveys and the labeling measures were advertised through different channels, and the survey was advertised as a chance to voice one's opinion on the offer of the student canteen. It is thus unlikely that restaurant guests drew a connection between the initiative and the survey.

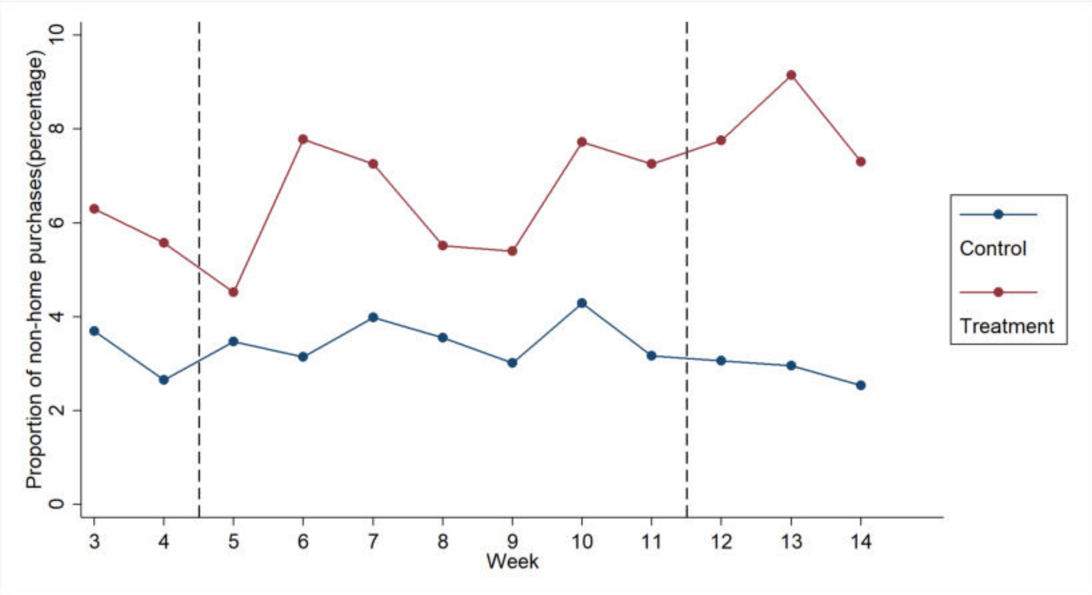


Figure D.1. Proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks. The sample is similar to that in spec. (4) in Table 2, but the intention to treat is calculated based entirely on the first two weeks, based on a minimum of two visits during this period. $N = 37,030$

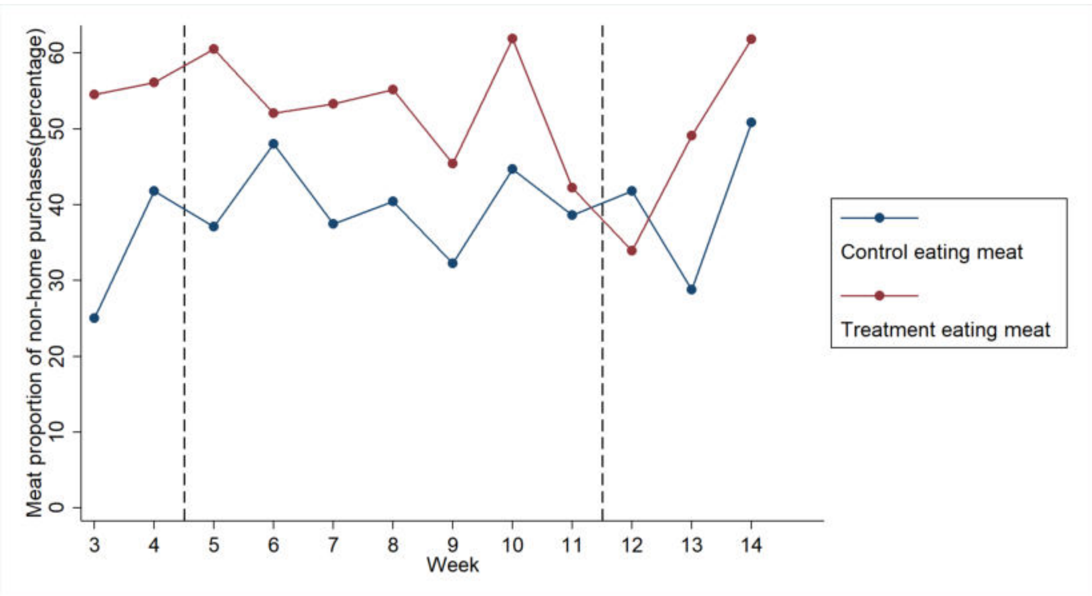


Figure D.2. Meat proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks. The sample is similar to that in spec. (4) in Table 2, but the intention to treat is calculated based entirely on the first two weeks, based on a minimum of two visits during this period. $N = 37,030$

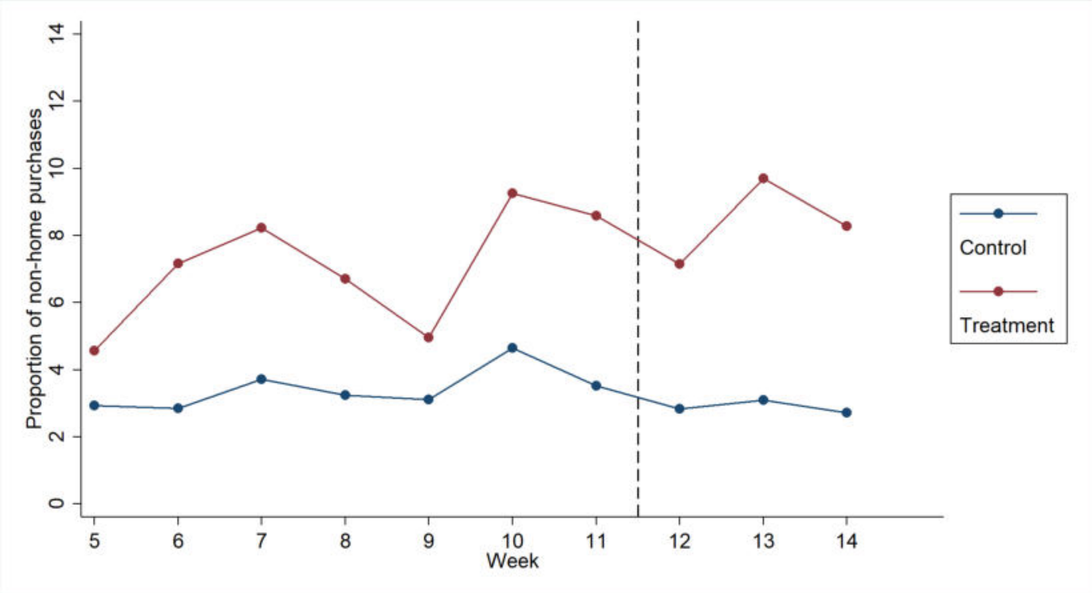


Figure D.3. Proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first four weeks (pre-intervention phase). The sample is similar to that in spec. (4) in Table 2, but also includes observations from week 5 of the sample period. $N = 45,628$

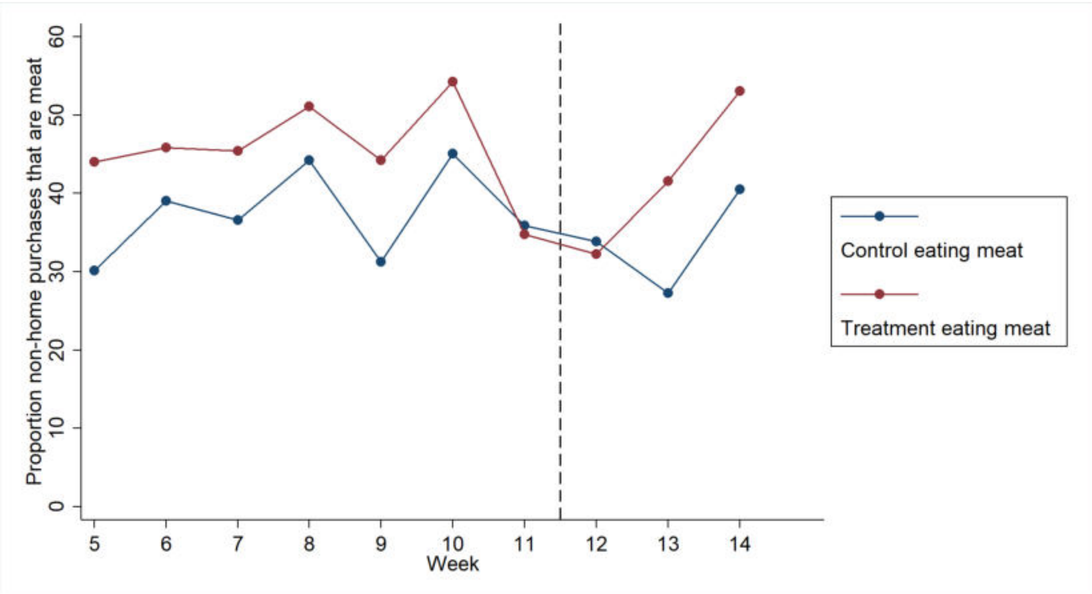


Figure D.4. Meat proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks (pre-intervention phase). The sample is similar to that in spec. (4) in Table 2, but also includes observations from week 5 of the sample period. $N = 45,628$

D.1.3 Carbon label calculation. For the carbon labels, I calculated emission values with the application Eaternity Institute (2020), using ingredient lists provided by the student canteen. The design of the carbon labels was proposed by the student canteen, based on what is technically feasible and possibly implementable as a long-run measure. Examples are shown in Figure 9. They were coded in a traffic-light system, with thresholds determined such that approximately a third of the main components offered by the student canteen during the study period would be classified as green, one-third as yellow, and one-third as red. This corresponded to thresholds of 0.7 kg and 1 kg. [56]

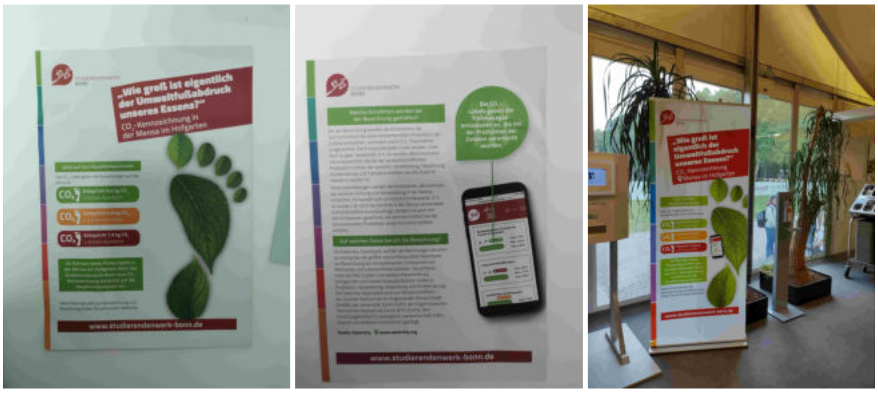


Figure D.5. Explanation of the carbon labeling on flyers (left and center) and billboards at the entrance of the student canteen (right).

D.1.4 Data set. The main data set covers purchase data from April 1st, 2022 to July 8th, 2022. Spec. (1) in Table D.1 performs the basic analysis shown in the main text in Table 2 in Col. (1) on all data before any exclusions.

- Starting from week 9 of the treatment period (May 30th to June 3rd), Ukrainian refugees received free meals in the treated student canteen and the larger control restaurant, using specific student canteen cards. I thus identify these sales and exclude them from all analyses. For the treated restaurant, they make up 12% of total sales in week 9,25% in week 10, and between 14% and 18% for the rest of the observation period. For the control restaurant, they make up between 2% and 7% of total sales. Spec. (2) in Table D.1 shows how this exclusion affects results.
- During the first week of the label period (May 2nd to May 6th), the display was irregular, as the student canteen needed some "trial and error" to get the system running. On some days, the labels were only displayed in the student canteen or online. Further, the student canteen had a special "Healthy Campus" week during the first week of May, during which it offered additional extraordinary meals which were also irregularly labeled. It is thus not clear whether the decrease in meat consumption observed during this week (see Figure 11) can be attributed to the carbon labels. To be conservative, I exclude this week from the main analysis. Spec. (3) in Table D.1 additionally excludes week 5 from the sample.
- There are seven days on which the treatment restaurant and the larger control restaurant did not offer the same main meal components: 7th of April, 19th of April, 20th of April, 17 of May,

56. Carbon emission labels for a given meal are calculated as the sum of the emissions caused by each of the ingredients. For each ingredient, emission values are calculated "from farm to gate". Hereby, it is assumed that the production process mirrors the average conventional production, e.g. I do not track the specific chicken breast bought by the student canteen but assume average conventional production. Emissions caused by the student canteen cooling, freezing, and cooking ingredients on-site are not included. These calculation details are explained to students on the student canteen website and on leaflets lying out on-site in the student canteen.

15th of May, 24th of June, and 27th of June. This is because, although menu planning is centralized, one of the student canteens may not have delivered an ingredient on time or may realize another ingredient is about to expire and independently adjust its meal offer. Any differences in the choice of the main meal component between treatment and control restaurants on these days are likely mainly influenced by differences in offer rather than by differences in label treatment. I thus exclude these days. Spec. (4) in Table D.1 additionally excludes these seven days from the sample (the final sample used in the main text).

For each purchase, I have data on the mode of purchase (student canteen card or debit card), meal category (combined with daily menus, this provides the specific meal name), student canteen card ID (if the purchase is made with the student canteen card), cash register number, date of purchase, time of purchase (exact to the minute), and purchase value.

Table D.1. Field estimates of the effect of carbon labels on meat consumption

	Likelihood of consuming meat			
	(1) Full data	(2) Excl. Ukr.	(3) +Excl. W5	(4) +Excl. diff. offer
Treatment restaurant x Label period	-0.02**	-0.03***	-0.02***	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)
Treatment restaurant x Post period	-0.01	-0.07***	-0.07***	-0.07***
	(0.01)	(0.01)	(0.01)	(0.01)
Treatment restaurant	-0.10***	-0.10***	-0.10***	-0.10***
	(0.01)	(0.01)	(0.01)	(0.01)
Label period	0.01***	0.01**	0.01**	0.01
	(0.00)	(0.00)	(0.00)	(0.00)
Post period	0.02***	0.01	0.01	0.01*
	(0.00)	(0.00)	(0.00)	(0.00)
Constant	0.51***	0.51***	0.51***	0.51***
	(0.00)	(0.00)	(0.00)	(0.00)
Date effects	No	No	No	No
Fixed effects	No	No	No	No
Guests control	7,298	7,217	6,798	5,589
Guests treated	3,278	2,939	2,716	2,329
Observations	155,411	150,345	137,962	120,121

Standard errors in parentheses

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

Note: Spec. (1) includes all data from weeks 1 to week 14. Spec. (2) excludes consumption by Ukrainian refugees. Spec. (3) additionally excludes the first week of the label period (week 5). Spec. (4) additionally excludes seven days on which the offer of the treatment and control canteens strongly differed, resulting in the final sample analyzed in Table 2. Specification follows 2.

D.1.5 Descriptive statistics on meat consumption and average emissions.

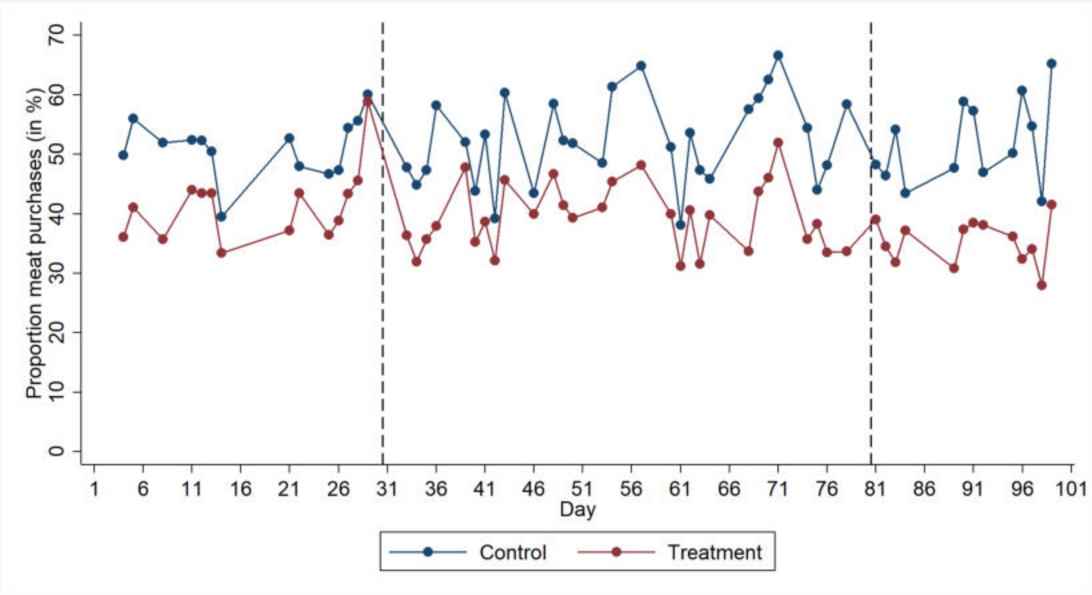


Figure D.6. Average proportion of meat meals sold in the sample period, using the final sample but including week 5. $N = 130, 132$

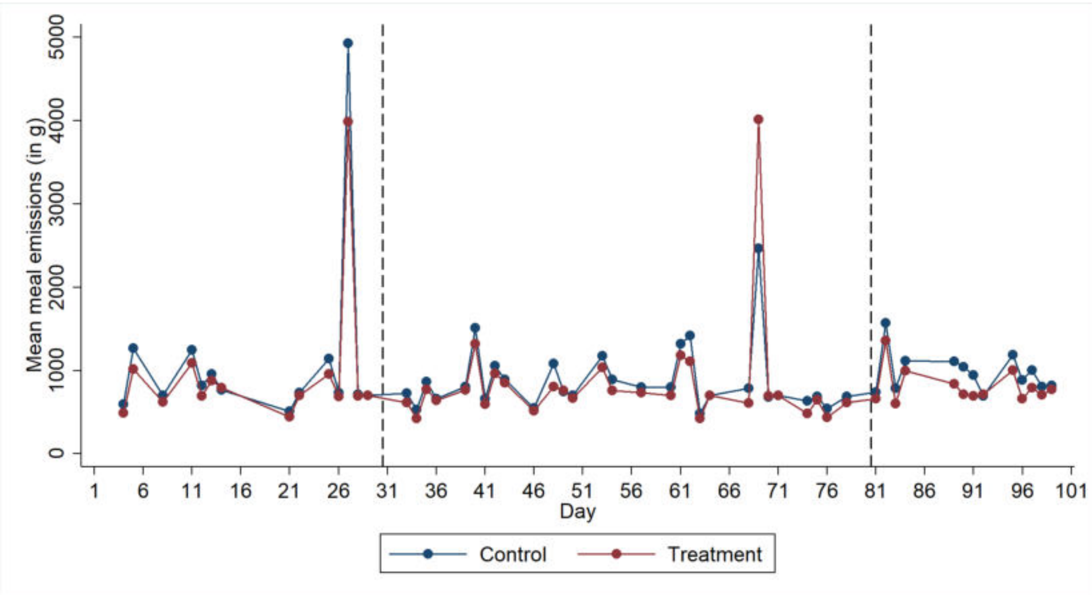


Figure D.7. Average emissions per meal in the sample period, using the final sample but including week 5. $N = 130, 132$

D.1.6 Survey accompanying natural field experiment. **Pre-intervention survey:** During the second week of April, I conducted a survey among student canteen guests at the treatment student canteen and the first, larger, control restaurant. The survey was advertised as an opportunity to voice one's opinion on the offer of the student canteen, took participants around five minutes, and motivated potential participants with the chance to win one of ten €50 coupons for the student canteen. The survey was advertised through multiple channels. First, I put up posters advertising the survey in many faculties throughout the University of Bonn. Second, I distributed leaflets in front of the treatment restaurant and the larger control restaurant, together with research assistants (see Figure D.8). It is common for students and student groups to advertise surveys, projects, and events in this manner. Finally, the experimental lab at the University of Bonn sent out an e-mail to its entire participant pool advertising participation.

Figure D.8. Leaflet advertising participation in the survey, as distributed in front of the student canteen.

In the survey, respondents indicated their student canteen card number and consented to their survey responses being connected to their consumption decisions from April to July. They filled out questions on demographics, environmental attitudes, political preferences, and preferences towards the student canteen offer. Responses to the questions on student canteen offer and participant comments were analyzed, summarized, and presented to the gastronomic manager of the student canteens. Over 1,700 restaurant guests participated in this first survey, 94% of these students.

Post-intervention survey: From the 22nd of June, I started sending out invitations to participate in a second survey. These were sent out by e-mail to those participants of the first survey who indicated their e-mail addresses and consented to be contacted for a second survey. This was the case for 94% of participants in survey 1. Of the 1,558 I invited to the survey, 918 filled out survey 2. I invited participants in a staggered fashion over two weeks and sent a reminder on the 7th of July. Again, survey respondents had the opportunity to win one of ten 50 €coupons for the student canteen.

In survey 2, I repeated some of the questions from survey 1, to assess whether attitudes changed differentially in the treatment student canteen. As pre-registered, the main attitudes of interest were (1) agreement with the statement "Flying should be more expensive, since it is bad for the environment", as a proxy for support for carbon taxes, and (2) agreement to the statement "It should be prohibited to build new houses not adhering to current environmental standards" as a proxy for support for command-and-control policy instruments to cut carbon. The final (3) outcome of interest is the participants' subjective experience of eating in the student canteen, assessed by agreement to the statement "Eating in the student canteen is a nice experience for me". The survey further included some questions of interest to the student canteen following the outcome of the first survey. At the end of the survey, participants could indicate whether and how they had perceived the emission labels, as well as voice their opinion on the initiative.

Appendix E Experiment 2: Additional tables and figures

E.0.1 Field survey results. Did canteen guests see the labels? Of the post-survey respondents, 340 went to the treated student canteen at least once during the intervention period. 72% of these report having seen the labels. 516 respondents did not go to the treated canteen during the intervention period, according to their individual student canteen cards. However, they might have in fact still gone, but not paid with their individual cards. Of these respondents, 13% report having seen the labels. 177 respondents went to the treated restaurant at least four times during the intervention period. 80% of these guests report having seen the labels.

Do canteen guests feel they reacted to the labels? Of the post-survey respondents who noticed the labels and visited the treated student canteen at least once during the intervention period, 18% report having incorporated the labels in their decisions (agreement of 4 or 5 on a 5-point scale asking how strongly participants incorporated the labels in their choices). Of those who visited the canteen more frequently (146 participants). 16% report having incorporated the labels in their decisions.

How do canteen guests make their consumption choices? 34% of guests report making their choice mainly using the information given on the canteen website. 29% mainly use the digital billboards. 36% report mainly deciding by looking at the food counters. Figure 9 shows how the carbon labels were shown in each of these decision contexts. Of the three decision contexts, the carbon labels were most salient on the canteen website. Table E.7 shows how treatment effects differ for guests making their decisions online. Results suggest that effects are stronger for this group.

Do the carbon labels affect other attitudes? I do not find any evidence of the carbon labels affecting my measure of support for a carbon tax or for command-and-control measures (see Table E.1). I also do not find any evidence that they significantly affect students' experience of eating in the canteen.

Table E.1. Effect of the labels on other attitudes

	Approval of...					
	(1)	(2)	(3)	(4)	(5)	(6)
	tax	command-control	experience	tax	command-control	experience
Treatment restaurant × Post period	−0.03	−0.15	−0.14	0.01	−0.18	−0.15
	(0.08)	(0.10)	(0.09)	(0.10)	(0.12)	(0.11)
Post period	−0.04	0.23***	0.08	−0.04	0.23***	0.08
	(0.05)	(0.06)	(0.06)	(0.05)	(0.06)	(0.06)
Constant	4.31***	4.39***	4.43***	4.33***	4.37***	4.43***
	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
Guest fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Guests control	472	472	472	472	472	472
Guests treated	359	359	359	208	208	208
Observations	1,662	1,662	1,662	1,360	1,360	1,360

Standard errors in parentheses

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

Dependent variable: Spec. (1) and (4): Agreement with the statement "Flying should be more expensive since it is bad for the environment." Spec. (2) and (5): "It should be prohibited to build new houses not adhering to cur- rent environmental standards." Spec. (3) and (6): ""Eating in the student canteen is a nice experience for me." All are measured on a 7-point scale. I only include individuals who participated in the pre-and the post- survey and who made at least one student canteen purchase during the 14-week study period. In Spec. (1)-(3), I classify an individual as treated if he visits the treated canteen at least once during the intervention period. In Spec. (4)-(6) I only include those treated individuals who visit the treated canteen at least three times during the intervention period. I only include individuals in the analysis who responded to both surveys and include guest fixed effects to control for initial attitudes. Standard errors are clustered at the individual level.

Table E.2. Survey among student canteen guests: Socio-economic summary statistics

Variable	Explanation	Mean	Std. Dev.
Age	Age of participant	22.90	–
Male	Dummy: 1 if participant is a man	0.41	–
Student	Dummy: 1 if participant is a student	0.94	–
Non-vegetarian	Dummy: 1 if participant eats meat	0.68	–
N	1,451		

Note: Statistics are based on a survey I conducted among student canteen guests in April. I include only survey respondents who visited a student canteen at least once in the 14-week study period and paid with their individual payment cards. See D.1.6 for details on the survey design. To preserve anonymity (since I also asked these survey participants about their study field), I elicited age in intervals. To reach an estimation of the mean age, I set the age equal to the midpoint of each interval. For 13% of respondents, I have the information that they are below 20. For the calculation, I estimate their age at 18. For 53% of respondents, I have the information that they are between 20 and 23 (which I set to 21.5 for the estimation), 23% of respondents are between 24 and 27 (set to 25.5), 6% of respondents are between 28 and 31 (set to 30), and 4% of respondents are 32 or older (set to 35). I did not directly elicit vegetarianism, but I elicited how much of a role animal rights play in participants' consumption decisions. I code participants reporting the highest degree of importance as vegetarians.

	(1) Meat meal
Treated × Week 1	-2.40
	(1.72)
Treated × Week 2	0.76
	(1.49)
Treated × Week 3	0.24
	(1.98)
Treated × Week 5	-3.25**
	(1.42)
Treated × Week 6	-2.17
	(1.48)
Treated × Week 7	-0.61
	(1.56)
Treated × Week 8	-3.77**
	(1.66)
Treated × Week 9	-1.55
	(1.55)
Treated × Week 10	-6.51***
	(1.85)
Treated × Week 11	-4.23***
	(1.49)
Treated × Week 12	-5.29***
	(1.53)
Treated × Week 13	-5.98***
	(1.59)
Treated × Week 14	-8.35***
	(1.43)
Treatment restaurant	-11.12***
	(1.18)
Other offer: special dish	0.16
	(0.48)
Other offer: special veg. dish	-1.29
	(0.79)
Other offer: pasta	-4.49***
	(0.63)
Other offer: pizza	-4.68***
	(0.80)
Other offer: add. vegan dish	2.07***
	(0.49)
Other offer: grill	-0.46
	(0.77)
Other offer: stew/soup	-2.40**
	(0.99)
Other offer: pan	2.36***
	(0.59)
Second veg. main	-1.93***
	(0.38)
Second meat main	1.54***
	(0.41)
Main components sold in '000s	-0.73***
	(0.08)
Guests control	7,018
Guests treated	2,857
Observations	130,139

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Regression table split into two columns for readability. Dependent variable: 0/1 indicator for consumption of the meat option, multiplied by 100 to enable the interpretation of coefficients as percentage points. Regression additionally includes weekly controls and day-of-the-week controls.

Figure E.1. Regression table for Figure 11

E.0.2 Time trends.

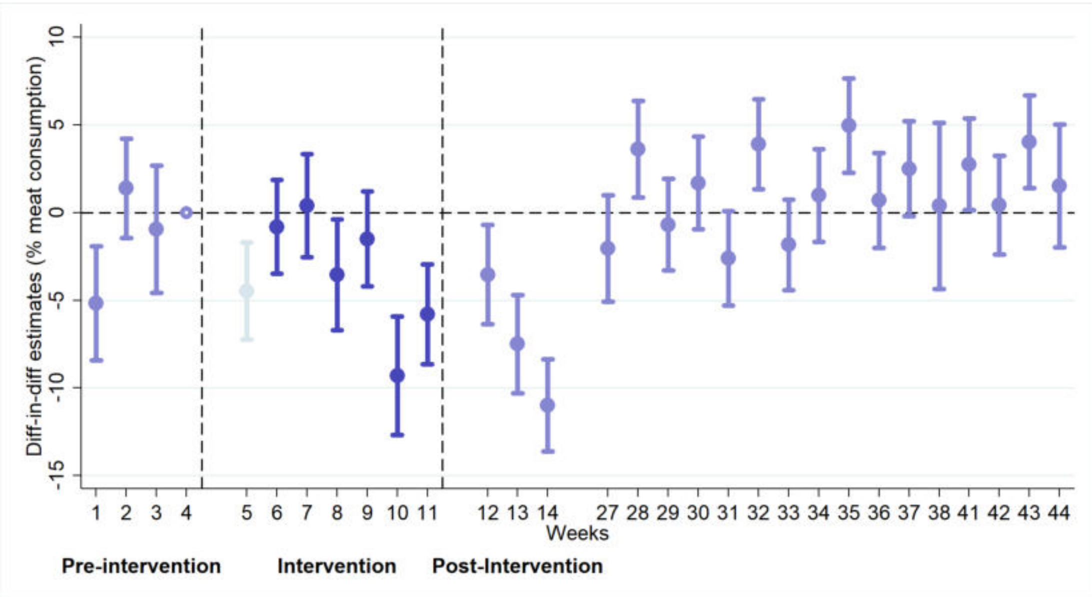


Figure E.2. Event study: Difference in difference estimates of the likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1–4 constitute the pre-intervention phase, while weeks 6–11 constitute the intervention phase, and weeks 12–14 the post-intervention phase. Weeks 27 onwards are the new semester. The regression specification closely follows specification (1) in Table 2, estimating weekly treatment effects and including weekly time controls and day-of-the-week controls. Bars indicate 95% confidence intervals.

Table E.3. Effect of labels over longer time period

	Likelihood of consuming meat			
	(1)	(2)	(3)	(4)
Treatment restaurant x Label period	-0.04***	-0.04***	-0.02***	-0.02***
	(0.01)	(0.01)	(0.01)	(0.01)
Treatment restaurant x Post period	-0.02***	-0.02***		
	(0.00)	(0.00)		
Treatment restaurant x June			-0.06***	-0.06***
			(0.01)	(0.01)
Treatment restaurant x July			-0.08***	-0.09***
			(0.01)	(0.01)
Treatment restaurant x October			0.01*	0.01*
			(0.01)	(0.01)
Treatment restaurant x November			0.02**	0.02***
			(0.01)	(0.01)
Treatment restaurant x December			0.02***	0.02***
			(0.01)	(0.01)
Treatment restaurant x January			0.03***	0.03***
			(0.01)	(0.01)
June			0.00	
			(0.01)	
July			0.02***	
			(0.01)	
October			0.01	
			(0.00)	
November			-0.02***	
			(0.00)	
December			0.01***	
			(0.00)	
January			-0.01**	
			(0.00)	
Treatment restaurant	-0.08***	-0.08***	-0.10***	-0.10***
	(0.00)	(0.00)	(0.01)	(0.01)
Label period	0.01***		0.01	
	(0.00)		(0.00)	
Post period	0.01**			
	(0.00)			
Constant	0.51***	0.48***	0.51***	0.48***
	(0.00)	(0.01)	(0.00)	(0.01)
Date effects	No	Yes	No	Yes
Fixed effects	No	No	No	No
Guests control	12,387	12,387	12,387	12,387
Guests treated	5,401	5,401	5,401	5,401
Observations	300,241	300,241	300,241	300,241

Standard errors in parentheses

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

Dependent variable: 0/1 indicator for consumption of the meat option, multiplied by 100 to enable the interpretation of coefficients as percentage points.

E.0.3 Effect magnitude relative to carbon tax and comparison with Experiment 1 effects. It is interesting to see how the effect magnitude I observe in Experiment 1—€120 per tonne—relates to the effect sizes I observe in the natural field experiment. I thus draw on additional student canteen data to estimate the effect a carbon tax would have in the student canteen context to put my natural field experiment result into perspective. I estimate that the carbon labels in the field produce a similar effect as would be expected from a carbon tax of €80 per tonne to €120 per tonne in the same field setting.

For this analysis, I use a larger set of student canteen purchase data to include the time frame from April 2022 to March 2023. Student canteen prices for the same main meal components vary throughout this period due to a price increase in October 2023, which not only increased the general price level but also the price differential between the meat and the vegetarian main meal components. While this difference was on average around €0.33 from April to June 2022 (around 20% of the price of a veg. main meal component sold then), it increased to around €0.50 from October to December 2023(around 25% of the price of a veg. main meal component sold then) and remained at this higher level.

Since the price increase affected all student canteens, I cannot identify the causal effect of the price increase in a difference in difference framework. Instead, Table E.4 takes a descriptive approach, running a simple linear regression controlling for factors other than prices that might affect purchasing behavior, and identifying how the residual variation in purchasing behavior correlates with the price difference between the meat and vegetarian main meal component.

I include the following controls: To control for time trends, I include week and day-of-the-week effects. To control for the main meal components on offer differing in their attractiveness, I include over 100 binary meal-specific control variables controlling for the two most sold main meal components offered in a given canteen on a given day.

Spec. (1) in Table E.4 includes all student[57] purchases of main meal components in this period. Spec. (2) restricts the analysis to purchases of the two main meal components that are most sold in a given canteen on a given day. This is my preferred specification since I can here control for the attractiveness of every meal in the sample. I find that a €0.01 increase in the price difference between vegetarian and meat main components correlates with a 0.25 percentage point decrease in demand for the meat main component and a corresponding increase in demand for the vegetarian main component. An increase in the price difference between the meat and the vegetarian main component of €0.01 can roughly be understood as a carbon tax of €0.01 per kg (or €10 per tonne). This is because the average emissions difference between the meat and the vegetarian main meal component offered in the student canteen is around 1 kg.

The effect I identify for such a carbon tax of €10per tonne—a 0.25 percentage point decrease in demand for the meat main component—is one eighth of the effect of carbon labels identified in the causal analysis shown in the main text (implying rough equivalence of this effect to a carbon tax of €80 per tonne), and one twelfth of the effect of the carbon labels identified within the regression analysis shown in Table E.4 (implying rough equivalence of this effect to a carbon tax of €120 per tonne). In Experiment 1, the framed field experiment discussed in Section 2, I estimate that carbon labels produce a similar effect as a carbon tax of €120 per tonne. My field results can second this estimate: In the student canteen setting, I estimate the effect of the carbon labels to be similar to that of a carbon tax of €80 to €120 per tonne introduced in the same setting.

To provide further context to my results and compare with previous literature, I calculate price elasticities: €0.1 is around 5.0% of the meat meal price at baseline, and 2.5 percentage points is around 5.6% of the baseline demand for the meat meal. This would imply an own-price elasticity of around $-1,1$ for the meat meal. Since a 2.5 percentage point decrease in demand for the meat main component corresponds to a corresponding increase in demand for the vegetarian main component, figures can also be used to calculate a cross-price elasticity for the vegetarian meal: A 2.5 percentage

57. I only include purchases made by students in the analysis since employees and guests face a different price structure.

point increase in demand for the vegetarian meal is around 4.5% of the baseline demand for the vegetarian meal, implying a cross-price elasticity of 0.9.

Compared to estimates of Wirsenius, Hedenus, and Mohlin (2011), these estimates are rather on the higher end. To calculate the impact of an EU-wide carbon tax on animal products, they assume an own price elasticity of -0.5 for eggs, -0.5 for dairy products, -1 for poultry, -0.8 for pork and -1.3 for ruminant meat for food demand in the EU. Further, they estimate a slightly negative cross-price elasticity for cereals (-0.01) following a price increase for meat products, and a zero cross-price elasticity for other vegetarian products.

One reason for the differences in estimates is most likely the vastly different context: Increasing the price of the meat option in the student canteen is different from imposing an EU-wide tax on meat. Students might be especially price-sensitive and might also substitute their meat consumption intertemporally. Since the price of the meat and the vegetarian component offered in the student canteen differs across specific meals offered, the price difference between meat and vegetarian components fluctuates across days: After the price increase, the price difference is €0.4 in around 40% of cases, and €0.1, €0.6, and €0.9 in around 20% of the remaining cases each. Students might thus respond to a particularly high price difference by moving meat consumption to a day with a lower price difference.

Table E.4. Comparison of effects: labels vs. "carbon tax"

	Likelihood of consuming meat	
	(1)	(2)
Price difference (in Euro)	-0.16***	-0.25***
	(0.05)	(0.05)
Treatment restaurant x Label period	-0.04***	-0.03***
	(0.00)	(0.01)
Treatment restaurant	-0.05***	-0.04***
	(0.00)	(0.00)
Constant	0.34***	0.41***
	(0.07)	(0.08)
Week and Day of the week controls	Yes	Yes
Meal-specific controls	Yes	Yes
Guests control	12,053	11,239
Guests treated	5,496	4,878
Observations	384,767	343,891

Standard errors in parentheses

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

Note: Dependent variable: 0/1 indicator for consumption of the meat option. Linear probability regression drawing on student canteen data from April 2022–March 2023. Both specifications include week and day-of-the-week effects, as well as over 100 binary controls for the day and student-canteen specific most-sold vegetarian and meat meal. The variable "Price difference" describes the price difference between these two most-sold options. Spec. (2) keeps only sales of the two most popular options.

E.0.4 Effect on carbon footprint. For an analysis of the impact on average greenhouse gas emissions per meal, I restrict the main sample such that it only includes days in the intervention period for which there is a "gastronomic twin" in the pre-intervention period: a day in the pre-intervention period where the same two main meal components were served. Further, I drop any sales not related to the two main components shared between treatment and control restaurants. The reason for this restriction is that the average emissions per meal vary a lot between days due to a changing offer (see Figures D.7 and D.6 for a comparison of daily variations in meat consumption vs. daily variation in average emissions). As vegetarian consumption is, at baseline, higher in the treated than in the control restaurants, a less restricted analysis might falsely attribute changes in the carbon footprint of the meals offered in the pre-intervention vs. in the intervention period to the label. [58] The restricted sample contains 33,427 observations. As shown in Table E.5 in the Appendix, I estimate that labels reduce average emissions per meal by 25 grams or around 3% of the emissions of a baseline meal.

Table E.5. Effect of labels on average emissions per meal

	(1) GHGE (g)	(2) GHGE (g)
Treatment restaurant x Label period	-17.31	-25.39**
	(11.26)	(10.25)
Treatment restaurant	-49.14***	-44.34***
	(7.44)	(6.74)
Label period	5.12	
	(6.26)	
Date effects	No	Yes
Fixed effects	No	No
Guests control	5,075	5,075
Guests treated	1,977	1,977
Observations	33,427	33,427

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Dependent variable: Emissions caused by main meal component, in gram. The sample is restricted to days in the intervention period for which there is a "gastronomic twin" in the pre-intervention period. Regression follows Spec. (1) and (2) in Table 2, using greenhouse gas emissions instead of the choice of the meat meal as the outcome variable.

58. As a simple illustration of why this is necessary: Imagine there is only one pre-intervention and one intervention day. On the pre-intervention day, the offer is a vegetarian meal with emissions of 0.3 kg and a meat meal with 1 kg of emissions per meal. In the treated restaurant, 59% of visitors consume vegetarian at baseline, so average emissions are 0.59 kg. In the control restaurant, 50% consume vegetarian at baseline, so average emissions are 0,65 kg. On the intervention day, the vegetarian offer still has 0.3 kg, but the meat meal now has 1.2 kg. Assuming no change in behavior, average emissions in the treated restaurant are 0.67 kg and 0.75 kg in the control restaurant. A naive analysis would then identify a differential 0,02 decrease in emissions in the treated restaurant compared to the control restaurant, although consumer behavior did not change. Thus, for the emissions analysis, I restrict the sample to establish an identical offer between the pre-intervention and intervention periods.

E.0.5 Heterogeneity in treatment effects. Table E.6 examines treatment effects in different subsamples, using Spec. (2) of Table 2. Treatment effects are similar when restricting the sample to only employees (col. 2), to off-peak visit hours (col. 3), to purchases made with an individual payment card (col. 4) and to restaurant guests paying by individual card and visiting the student canteen rather frequently (at least ten times during the 13 weeks, col. 5). Table E.7 shows analyses restricting the sample to guests who pay by individual payment card and for whom I have demographic information (around 1,400 guests). This suggestive analysis indicates that treatment effects are stronger for females, canteen guests below 24 of age, and those who report environmental aspects playing an important role in consumption choice.

Table E.6. Effect of labels on meat consumption, different subsamples

	Likelihood of consuming meat				
	(1)	(2)	(3)	(4)	(5)
	All	Employees	Non-busy time	Card payment	Frequent
Treatment restaurant x Label period	-0.02^{***}	-0.05	-0.03^{***}	-0.02^{**}	-0.03^{*}
	(0.01)	(0.03)	(0.01)	(0.01)	(0.01)
Treatment restaurant x Post period	-0.07^{***}	-0.11^{***}	-0.06^{***}	-0.09^{***}	-0.08^{***}
	(0.01)	(0.03)	(0.01)	(0.01)	(0.02)
Treatment restaurant	-0.10^{***}	-0.03	-0.10^{***}	-0.05^{***}	-0.05^{***}
	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
Constant	0.48^{***}	0.63^{***}	0.49^{***}	0.39^{***}	0.41^{***}
	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)
Date fixed effects	Yes	Yes	Yes	Yes	Yes
Guest fixed effects	No	No	No	No	No
Guests control	6,927	882	3,797	5,977	1,823
Guests treated	2,816	266	1,680	2,504	719
Observations	120,121	20,850	67,654	70,100	47,020

Standard errors in parentheses

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

Note: Dependent variable: 0/1 indicator for consumption of the meat option. Linear probability model regression following spec. (2) in Table 2. Col. (1) includes all data, Col. (2) only university employees, Col. (3) excludes peak hours, Col. (4) to payments made by individual payment card, and Col. (5) includes only guests who visited the canteen at least ten times during the 13-week sample period. Spec. (1)-(3) estimate robust standard errors, and Spec. (4)-(5) cluster standard errors at the individual level.

Table E.7. Effect of labels on meat consumption, different subsamples

	Likelihood of consuming meat					
	(1)	(2)	(3)	(4)	(5)	(6)
	All	Survey	Female	Below 24	Env. important	Decide online
Treatment restaurant x Label period	-0.02^{**}	-0.03	-0.05^{*}	-0.04^{*}	-0.04^{*}	-0.08^{*}
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)	(0.05)
Treatment restaurant x Post period	-0.09^{***}	-0.09^{***}	-0.08^{***}	-0.09^{***}	-0.08^{***}	-0.08
	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)	(0.05)
Treatment restaurant	-0.05^{***}	0.00	0.06^{**}	0.01	-0.02	-0.03
	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)	(0.06)
Constant	0.39^{***}	0.24^{***}	0.16^{***}	0.23^{***}	0.11^{***}	0.25^{***}
	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)	(0.08)
Date fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Guest fixed effects	No	No	No	No	No	No
Guests control	5,977	849	463	572	446	151
Guests treated	2,504	522	272	354	234	119
Observations	70,100	15,031	6,958	10,364	7,147	2,687

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Note: Dependent variable: 0/1 indicator for consumption of the meat option. Linear probability model regression following spec. (2) in Table 2. Col. (1) includes all data, Col. (2) only student canteen guests who participated in the pre-intervention field survey. Col. (3) includes, of these, only females. Col. (3) includes only under 24-year olds. Col. (5) includes only survey participants who report that environmental aspects play an important role in their food consumption decisions. Table C.24 reports evidence from Experiment 1 for the same heterogeneity factors. Spec. (1) estimates robust standard errors, and Spec. (2)-(5) cluster standard errors at the individual level.

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