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 the channels by which they impact behavior. Through a series of lab-in-the-field ($N = 733$) and field experiments (more than 120,000 purchase decisions by over 10,000 customers) in the student canteen setting, I provide evidence that the labels causally impact consumption behavior and primarily influence consumers by directing their attention toward carbon emissions. While improving consumers' knowledge about carbon footprints also plays a role, it is secondary in comparison. In both experiment contexts, the overall effectiveness of the labels is similar to that of a carbon tax of €120 per tonne, and the labels on average create a psychological benefit for consumers.

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1 Introduction

Strong political action is necessary to limit global warming below 2°C (IPCC, 2023), but traditional policy tools for doing so do not enjoy broad support from all sectors. One especially striking case is the food sector, which causes 26%–34% of global greenhouse gas emissions (Poore and Nemecek, 2018; Crippa et al., 2021). Clark et al. (2020) predict that even if we eliminated fossil fuels 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 would greatly reduce these emissions (Poore and Nemecek, 2018; Kim et al., 2020). However, the introduction of carbon taxes on agricultural goods has so far been very limited and remains an unpopular policy measure (Dechezleprêtre et al., 2022).

Another way to influence consumption behavior—apart from a change in prices—might be to remove behavioral and informational frictions impeding consumers from making carbon-friendly consumption decisions. For example, consumers might lack knowledge about the carbon footprint of different options or pay insufficient attention to these factors at the moment of choice, and behavioral interventions may be able to correct these frictions. One intervention which has recently been receiving attention from academia[1], regulatory agencies[2], and private companies[3] is carbon labeling. Recent causal evidence (Bilén, 2022; Lohmann et al., 2022) shows that carbon labels impact consumer behavior.

This paper addresses the question of how much and why consumers react to carbon labels, quantifying their overall effectiveness as well as their effectiveness in removing information or attention frictions. Results are based on two lab-in-the-field ($N = 733$) and one field experiment (more than 120,000 purchase decisions by $N > 10{,}000$ customers). To allow for comparability across experiments, I conduct all three experiments in the student canteen context. While the student canteen context in itself offers potential for reducing emissions on a large scale[4], findings are also relevant for related food contexts, such as corporate canteens or grocery shopping, and also for other purchasing contexts in which the carbon footprint caused by different items could be calculated and labeled, e.g. shopping for toiletries or clothing.

Experiment 1 (a lab-in-the-field experiment with 289 participants) and Experiment 2 (a field experiment with over 10,000 guests making over 120,000 consumption choices) jointly establish that carbon labels affect consumption behavior in the student canteen setting and estimate the magnitude of the effect. In particular, I estimate which magnitude of a carbon tax would produce similar changes in purchase quantities as is produced by the carbon labels. In the lab-in-the-field setting, I establish an estimate based on how participants' willingness to pay for meals changes when shown carbon labels, while I rely on the combination of a carbon labeling intervention and pricing variation in the field setting. I estimate effectiveness in both settings to provide a precise and externally valid estimate: While the lab-in-the-field setting trumps the field setting in terms of precision and clear causal identification, the field setting provides evidence that the lab-in-the-field estimates are reconcilable with student canteen purchasing behavior observed over longer time periods. Experiment

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

2. E.g. 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).

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

4. 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).

3 ($N = 444$) then moves back to a lab-in-the-field context to examine the relative importance of the removal of information and attention frictions in driving consumers' response to carbon labels.

Experiment 1 ($N = 289$) is a lab-in-the-field experiment examining how willingness to pay for typical student canteen meals changes when participants are shown carbon labels. Comparing meal-specific changes in willingness to pay with the meals' carbon footprints yields that on average willingness to pay decreases by €0.12 for every kg of emissions caused by the 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 a carbon tax of €120 per tonne would produce a similar decrease in carbon emissions as carbon labels.

Experiment 2 is a field experiment ($N > 120{,}000$ choices from over 10,000 guests), showing that the consumption reactions I observe to carbon labels in the lab-in-the-field context 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-difference estimation of label effectiveness. The carbon labels I test are similar to those used in Experiment 1 and 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). I estimate that the labels decrease consumption of the higher carbon option by 2 percentage points. The effect of the labels persists in the three weeks following the intervention period, after which the university canteen closed for summer break. I compare this effect with an estimate of the effect of a carbon tax in the student canteen. I estimate that the effect produced by the carbon labeling intervention is comparable to a €80 per tonne to €120 per tonne carbon tax in the same setting—and thus similar to my lab-in-the-field estimate.

Estimates from the field setting thus corroborate the carbon tax equivalence estimate I establish in Experiment 1: In terms of emission savings, carbon labels produce a similar effect in the student canteen setting as a carbon tax of €120 per tonne. This quantification allows for a comparison with other policy tools and allows us to better understand the magnitude of the effect. €120 per tonne is about four-fold 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 deviates from the standard model in two ways. First, consumers may prefer lower carbon emissions, but may not be attentive to these at the moment of choice. Second, I allow for a lack of knowledge of the carbon emissions caused by different meals, i.e. misperceptions of carbon impact, as a second potential source of non-optimal behavior. Behavioral interventions such as carbon labels make the consumer both informed and attentive. [5]

Experiment 3 ($N = 444$) is a lab-in-the-field experiment seeking to quantify the relevance of each of these two possible channels: Are carbon labels mainly changing behavior by correcting mis-

5. These modeling choices are based on previous literature: I focus on attentional biases as an important factor impeding optimal decision making, as they have been identified as relevant in the tax salience and resource consumption salience 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 decision-making based on suggestions in recent papers on carbon labeling (Shewmake et al., 2015; Camilleri et al., 2019; Imai et al., 2022).

perceptions about carbon footprints or are they changing behavior mainly by increasing consumers' attention? The experiment elicits participants' meal valuations, prior beliefs of the carbon footprints of different meal options, and participants' willingness to pay to receive or avoid carbon labels in the student canteen context. 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 towards carbon emissions while 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, similar to patterns found by Attari et al. (2010) for energy-consuming appliances and Camilleri et al. (2019) for single food items. 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 merely increasing 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 and Experiment 3 data, 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, suggesting 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 disproportionate psychological costs on consumers.[6] This is further supported by a post-intervention survey conducted after Experiment 2, the field experiment in the student canteen ($N = 234$). 73% of guests affected by the labels report that they would like the labels to be installed permanently (18% do not know, 9% against). A carbon tax, 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.

The rest of this paper is structured as follows. Section 2 provides an overview of relevant previous research and describes the contribution of this project. Section 3 describes how Experiment 1 quantifies the effectiveness of carbon labels using direct elicitation in a lab-in-the-field setting. Section 4 describes the design and results of Experiment 2, which is the field experiment corroborating my estimate. 5 outlines a simple theoretical model describing possible behavioral biases influencing

6. 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 €0.0018 per consumption decision and by an additional €0.21 independent of their impact on consumption decisions.

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 6 describes Experiment 3, the lab-in-the-field experiment I conduct to examine the relevance of each of these channels, and discusses reduced-form evidence on the relevant behavioral channels. Section 7 structurally estimates the theoretical model using data from Experiment 3. Section 8 discusses the impact of behavioral interventions on consumer welfare, drawing on data from all experiments. Finally, section 9 concludes.

2 Related literature

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 provides the first evidence of attentional biases present in the food consumption context. 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 combining attentional biases and informational frictions as impediments to optimal decision-making can shed light on the mixed findings in previous literature (Imai et al. (2022), Camilleri et al. (2019), Lohmann et al. (2022), Bilén (2022)). More generally, I provide evidence from a discrete choice context 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 behavioral interventions can address (e.g. Benartzi et al., 2017; Reisch and Zhao, 2017).

Second, I contribute to the relatively young literature on the psychological costs or benefits of behavioral interventions. Literature in this direction so far has focused on the psychological effects of receiving social comparison information (Allcott and Kessler, 2019; Butera et al., 2022). Thunström (2019) is to my knowledge the only paper assessing the psychological costs of a label. In a hypothetical choice experiment, she finds that calorie labels impose psychological costs on participants with low self-control. I provide evidence of consumers' preferences for the presence of carbon labels, both by eliciting consumers' willingness to pay for the presence of carbon labels directly in lab-in-the-field experiments 1 and 2 and by conducting an opinion survey at the end of the field Experiment 3.

Finally, I contribute to the literature on the effectiveness of carbon labels on food consumption. Lohmann et al. (2022) estimate that labels in a Cambridge student canteen causally 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. Further 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) suggests carbon labels reduce carbon emissions. See Rondoni and Grasso (2021) for a review. 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) finds carbon labels effective, while Imai et al. (2022) does 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 my lab-in-the-field experiments 1 and 2, I provide the first experimental estimate of the effectiveness of carbon labels relative to a carbon tax. 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 my large-scale field Experiment 3 producing effect estimates in line with the results of my lab-in-the-field experiment.

Further, my field Experiment 3 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 (Jalil, Tasoff, and Vargas Bustamante, 2020) and carbon labels in the general food consumption context (e.g. Panzone et al. (2021) study the grocery shopping context).

3 Experiment 1: Quantifying the effectiveness of labels in a lab-in-the-field setting

Experiment 1 quantifies the effectiveness of carbon labels in a lab-in-the-field setting. Subsection 3.1 describes the experiment design, 3.2 describes the empirical strategy, and subsection 3.3 describes data and results.

3.1 Experiment design

Overview. To cleanly measure the impact of carbon labels, 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. I thus follow a within-subject approach, eliciting the willingness to pay of each participant multiple times. To control for any possible effects of asking individuals repeatedly for the same meal, some participants do not see carbon labels in the repeated elicitations. I summarize the most important design choices below and add details in the following subsections.

(1) 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 do not see any carbon labels in the second elicitation.

(2) Willingness to pay elicitations are incentivized: Of the 15 meal purchase decisions made in the course of the online experiment, one decision is implemented. Participants make their way to the university campus shortly after completing the experiment and receive their payment in cash as well as a student canteen meal. Both are a function of the willingness to pay indicated in the experiment and a random price draw.

(3) Willingness to pay 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. 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.

(4) Carbon labels show a quantitative and ordinal ranking: 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 a label that they would be willing to implement and thus comparability to Experiment 2. Labels also state how long a car drive (in km) would cause the same amount of CO_2 emissions.

(5) 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. I discuss these results in section 8.

Experiment timeline. The experiment 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[7] guessing questions on unrelated issues (e.g. on the length of a popular running route in Bonn).[8]

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.

7. 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.

8. One reason for adding these questions is to make the design as similar as possible to that in Experiment 3, in which participants instead guess which quantity of greenhouse gas emissions is caused by each meal.

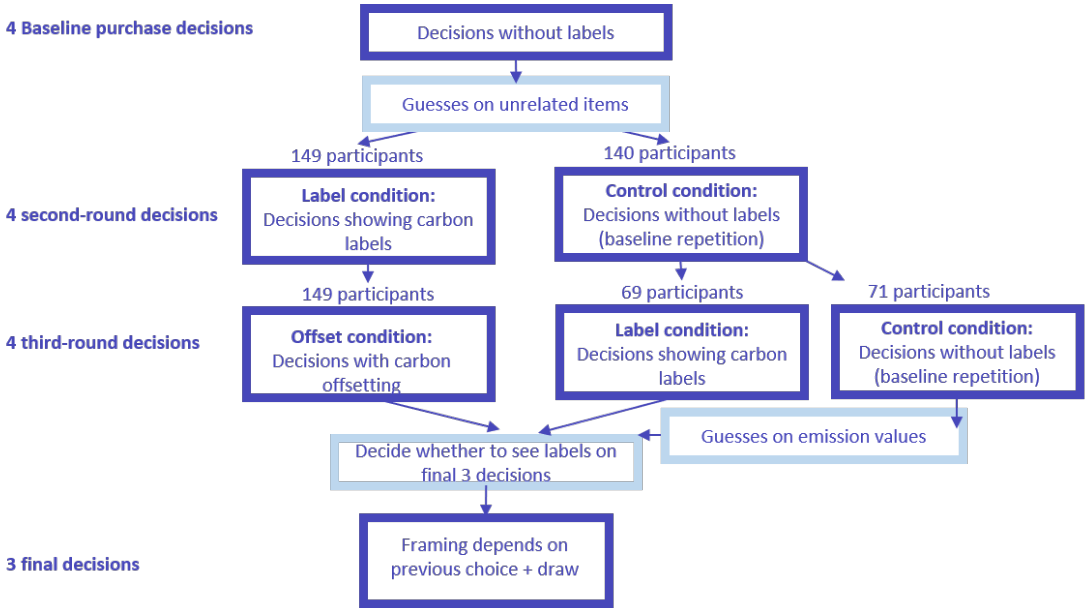

Figure 1. Experiment schedule and treatment groups

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[9], 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.[10]
- 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.[11].

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.

9. 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.7.
10. The results of the Offset condition are not further discussed in this section, but estimation results can be found in Table C.22. 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 7.
11. 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.

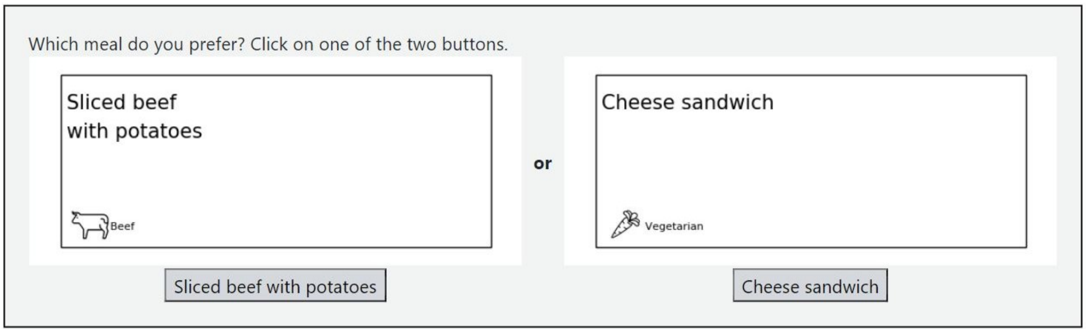

Figure 2. Meal purchase decision example: Step 1 of the purchasing decision

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), revolving around the same 4 meals. Participants who indicate that they are vegetarian are shown only vegetarian meals.

The decision set-up

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.[12]

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 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. [13]

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.

Decision framing differs across treatment conditions

In the four baseline decisions, participants do not see any carbon labels but are merely shown the

12. To ensure that results are not driven by a left-right effect, half of the participants made their choices with the left-right positioning of the two options reversed.

13. 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.

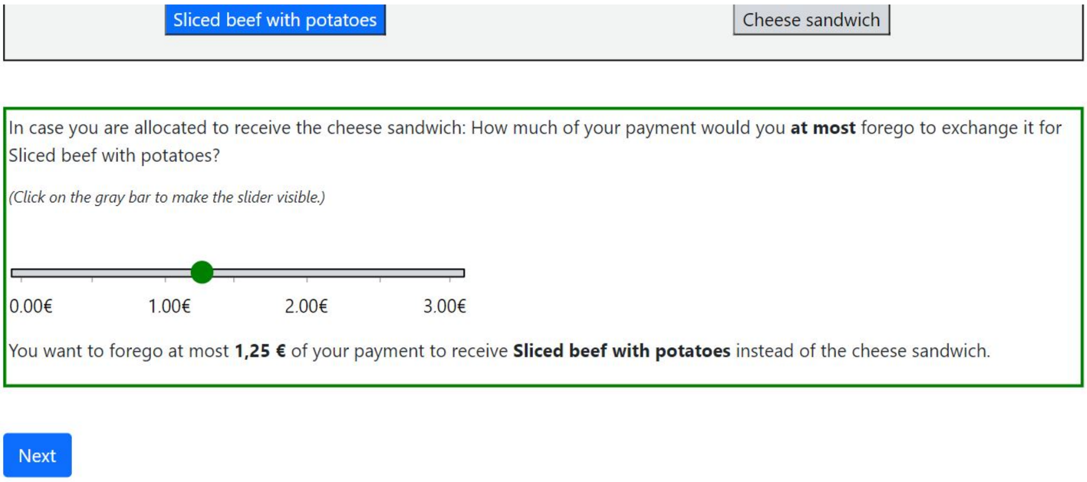

Figure 3. Meal purchase decision example: Step 2 of the purchasing decision

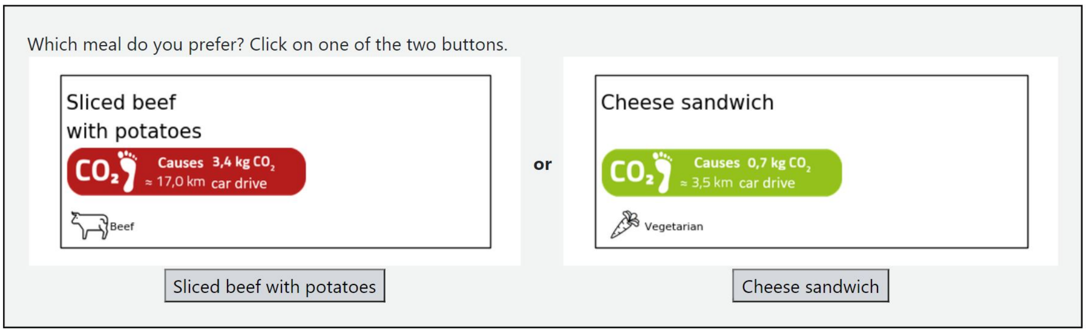

Figure 4. Meal purchase decision example: Decisions with labels

meal name and the meal's main ingredient (see Figure 2 for an example)[14]. 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.[15] For participants in the Control condition, there is no change in framing. For participants in the Offset condition, participants are told that the emissions caused by the meal will be offset. 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. Participants are informed in the experiment invitation that vegetarian participants are permitted, but not participants with stricter dietary requirements (vegan, gluten-intolerant,

14. I chose this display to reflect exactly how a meal would be displayed on the student canteen website.
15. 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.

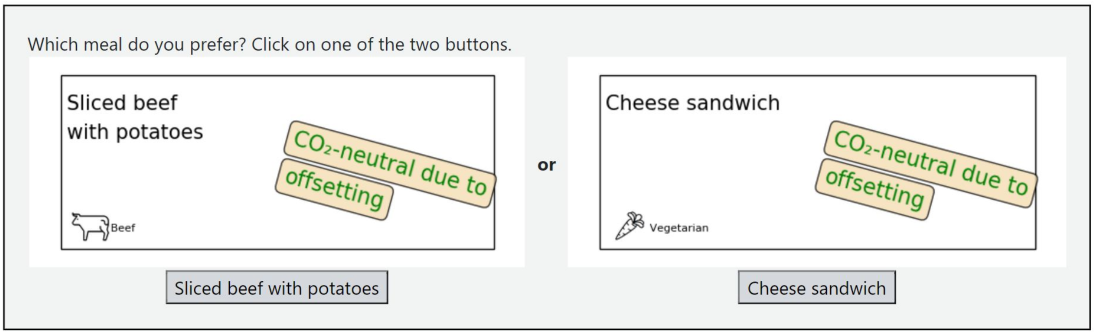

Figure 5. Meal purchase decision example: Decisions with carbon offsetting

Figure 6. Gazebo set up on University campus to provide participants with their payment in cash and a lunch.

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.

3.2 Estimation strategy

My basic specification is:

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

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. Thus, my dependent variable is how willingness to pay for a specific individual and a specific meal **changes** between decision rounds. In this manner, I control for individual-specific meal tastes at baseline. One can also understand $Diff_{ijm}$ 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.3.

$High_m$ is an indicator variable for whether the meal causes higher emissions than the sandwich. 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, emissions are reduced if consumers adjust their demand to pay for these meals upward, so willingness to pay should increase. For meals with emissions higher than the sandwich, emissions are reduced if consumers adjust their demand for these meals downward, so willingness to pay should increase.

$(Label_{ij} * High_m)$ 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. $(Label_{ij} * Low_m)$ 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.[16]

3.3 Data and results

I exclude the 3% fastest participants and participants not passing the comprehension check after five attempts, as pre-registered[17]. 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.

Table 1 Spec. (1) shows the results of the OLS estimation of equation 1, clustering standard errors at the individual level. For meals with lower emissions than the cheese sandwich, willingness to pay increases by €0.08 on average when participants are shown carbon labels. For meals with higher emissions than the cheese sandwich, willingness to pay in the Label condition decreases by €0.29. Changes in willingness to pay for participants in the Control 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.

16. An alternative approach to controlling for possible third-round effects is excluding third-round decisions entirely. This yields similar results (Table C.7).
17. Schulze Tilling (2021a)

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 shift in the demand curve results in the same effect on quantity purchased as a 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).

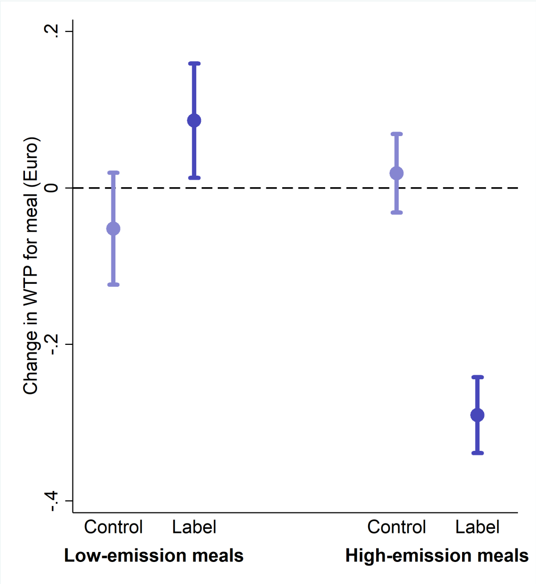

Figure 7. Within-subject change in willingness to pay for a specific meal, differentiated between participants in the Control and Label condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) 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 specific meal, compared to baseline. Spec. (1) corresponds to Figure 7 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.

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

The results in section 3 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 field setting (student canteens in Bonn) whether effects are similar if carbon labels are installed over longer time periods. Subsection 4 describes the experiment design, subsection 4.1 describes the estimation strategy, and subsection 4.2 describes data and results.

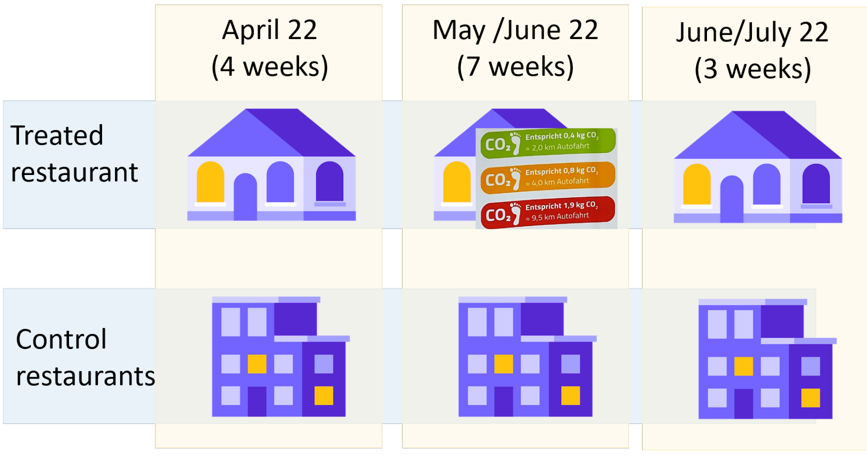

Figure 8. Timeline Experiment 2

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 who centralize their meal planning, i.e. roughly the same meals are offered in all restaurants. I summarize the most important details below and add describe the student canteen setting in Bonn more in detail in section D.1.

(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.

(4) I accompany the 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.

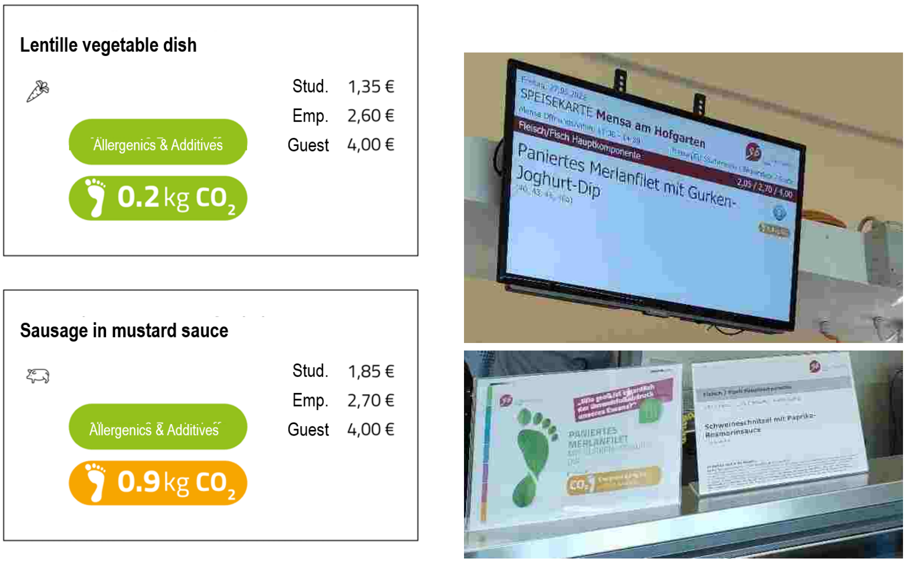

Figure 9. Labels online (left, menu translated from German) and in the student canteen (right)

4.1 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 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, with the most basic specification being:

$$Meat_{it} = \alpha + LabelPeriod_t + PostPeriod_t + Treat_{it} + \\ + \delta_1(Treat_{it} * LabelPeriod_t) + \delta_2(Treat_{it} * PostPeriod_t) + \epsilon_{it} \quad (2)$$

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.

$(Treat_{it} * LabelPeriod_t)$ 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 restaurant relative to the control restaurants. $(Treat_{it} * PostPeriod_t)$ identifies possible post-intervention effects.

4.2 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 these individuals across time.

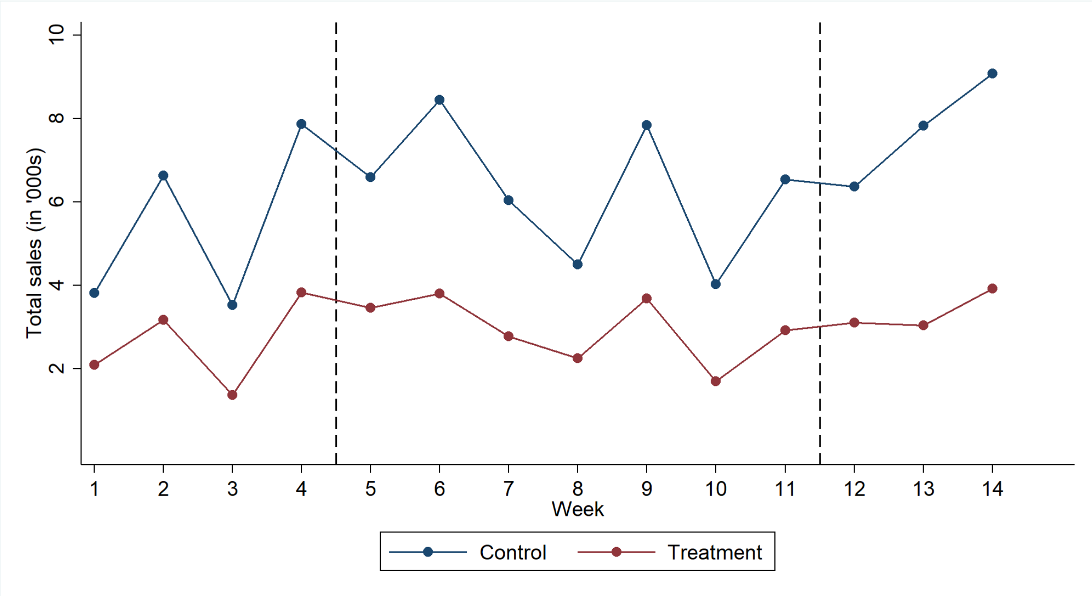

Figure 10. Number of weekly student canteen sales of main meal components, after data cleaning described in section 4, but including week 5. $N = 130, 132$. Weeks 1–4 are the pre-intervention period (April 2022), weeks 5–11 are the intervention period (May to Mid-June 2022), and weeks 12–14 are the post-intervention 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.

I drop data from seven days on which the treatment and 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, since a "Healthy Campus" week occurred simultaneously and it is not clear whether the carbon labels or this event are driving increased vegetarian consumption.[18] 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. The proportion of "non-home" purchases is generally higher for guests classified as treated (fluctuating between 5% and 9% of all purchases made by individuals classified as treated) than for guests classified as control (around 3% of purchases). There is no clear time trend in this 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 percent-

18. See Figure E.4 for an analysis including this week.

age 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. Examining hypothetical effects estimated for weeks 1-3 of the pre-intervention period gives evidence of the validity of the pre-trend assumption, which seems to be roughly fulfilled. 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 daily controls. 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 (rather than a pure selection of vegetarians and non-vegetarians out of the student canteen). 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 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.1 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.3 suggest—if at all—an upwards-sloping pattern. 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 effectiveness of €120 per tonne estimate shown in Experiment 1. Further, I estimate the decrease in average greenhouse gas emissions caused by the labels at 25g per meal on average. This is around 3% of the average emissions of a meal consumed at baseline. 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 card, or only frequent canteen guests. Combining the purchase data with demographic data I elicited in the field surveys described in section D.1, 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.

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 x 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 x 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 effects	No	Yes	Yes	Yes	Yes	No
Fixed effects	No	No	No	Yes	No	No
Guests control	6,924	6,924	879	879	879	6,924
Guests treated	2,815	2,815	336	336	336	2,815
Observations	120,093	120,093	25,390	25,390	25,390	120,093

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. Specifications (2)–(5) include date effects and the "Post period" and "Label period" indicators are thus dropped due to collinearity. 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).

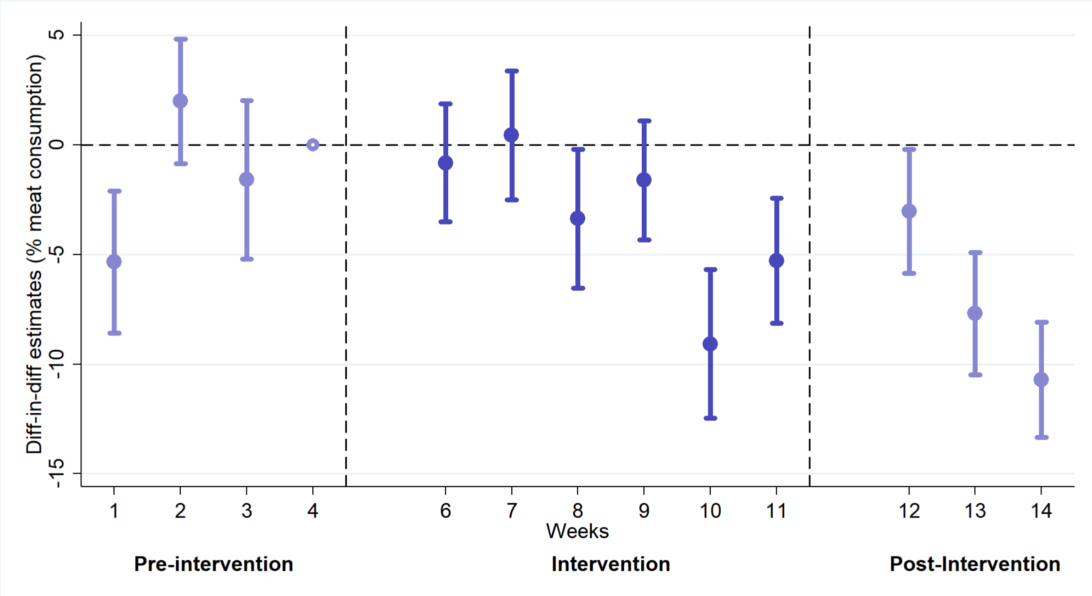

Figure 11. Event study: Difference in difference estimates of the likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1–4 constitute the pre-intervention phase, while weeks 6–11 constitute the intervention phase, and weeks 12–14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

5 Theoretical 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. Section 7 will estimate model parameters using data from Experiment 3 which will be described in section 6.

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 CO_2 emissions caused by each meal. Ceteris paribus, the consumer has a higher valuation for a meal that causes fewer CO_2 emissions. How much the consumer cares about emissions depends on two parameters: the consumers' environmental attitude, and the salience of CO_2 emissions at the moment of choice.

5.1 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*

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

Here, v_m is the *consumption utility* of meal m that is independent of emissions[19], 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.[20]

The *salience* of CO_2 emissions $\theta \in [0,1]$[21] and the consumer's *environmental guilt per perceived kg of emissions* γ jointly determine how much weight the consumer puts on CO_2 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 consumers' prior estimate of emissions, e.g. due to a lack of trust in the carbon footprint information provided.[22] If the consumer is *attentive* to emissions, this sets $\theta = 1$.[23] Introducing *carbon labels* makes the consumer both informed and attentive.

5.2 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[24] 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 $\theta = 1$,

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

19. 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 $v_m = \beta^T x_m + \varepsilon_m$.

20. 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.

21. 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)).

22. 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.

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

24. 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.

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 direction of attention induced by the carbon labels, [25] 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 $e_m = 0$:

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

6 Experiment 3: Behavioral channels

Experiment 3 provides lab-in-the-field evidence on the respective relevance of each of the two behavioral channels proposed in the theoretical model in section 5. Subsection 6.1 describes the experimental design. Subsection 6.2 describes data and reduced-form results. Experiment 3 data is also used to estimate the parameters of the theoretical model, as detailed in 5.2. Results of the structural estimation are discussed in section 7.

6.1 Experiment design

Overview. The theoretical framework in section 5 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 lab-in-the-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 treatment effects.

(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 to test the possible relevance of the attention channel. In the reduced-form analysis, I estimate treatment effects for this condition.

25. 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.22 and C.23.

Experiment timeline. The experiment timeline is visualized in Figure 12. It proceeds very similarly to Experiment 1. However, 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). Experiment 2 participants do not answer these questions, but instead guess the greenhouse gas emissions caused by different meals. These questions include the four meals around which the meal purchasing decisions revolve, as well as six further meals (see Figure 14 for a list). Participants make each of the ten guessing decisions on separate screens, shown to participants in a random order. On each screen, they are always 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 is shown in Figure 13. The guessing questions are incentivized and timed as in Experiment 1.

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 emission guessing task between the two elicitations, they have now spent time thinking about the issue of greenhouse gas emissions, and are thus 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.[26]

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[27], 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. Participant invitation and experiment set-up are as in Experiment 1.

26. The results of the Offset condition are not further discussed in this section, but estimation results can be found in Table C.23. The Offset condition serves as input for the structural estimation described in section 7, as detailed in section 5.2.
27. 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.

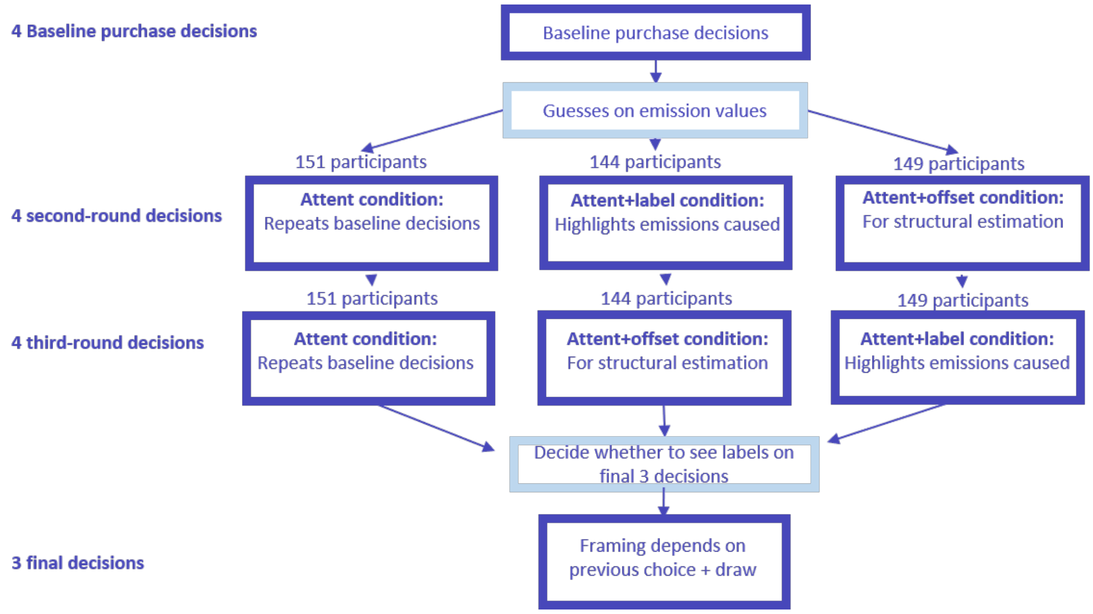

Figure 12. Experiment schedule and treatment groups

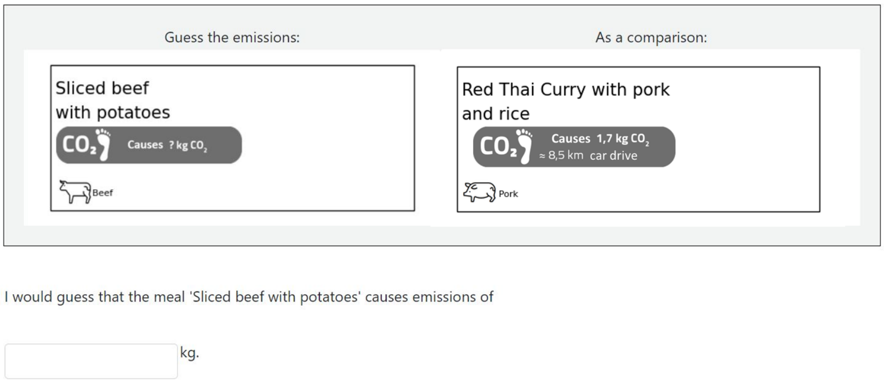

Figure 13. Example guessing questions

6.2 Data and results

I exclude the 3% fastest participants and participants not passing the comprehension check after five attempts, as pre-registered [28]. 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.

The effect of carbon labels by previous estimation. All participants in Experiment 3 were asked to guess the emissions caused by different meals. Further, the 71 participants in the "Control, then Control" group in Experiment 1 also estimated greenhouse gas emissions towards the end of the

28. Schulze Tilling (2021b)

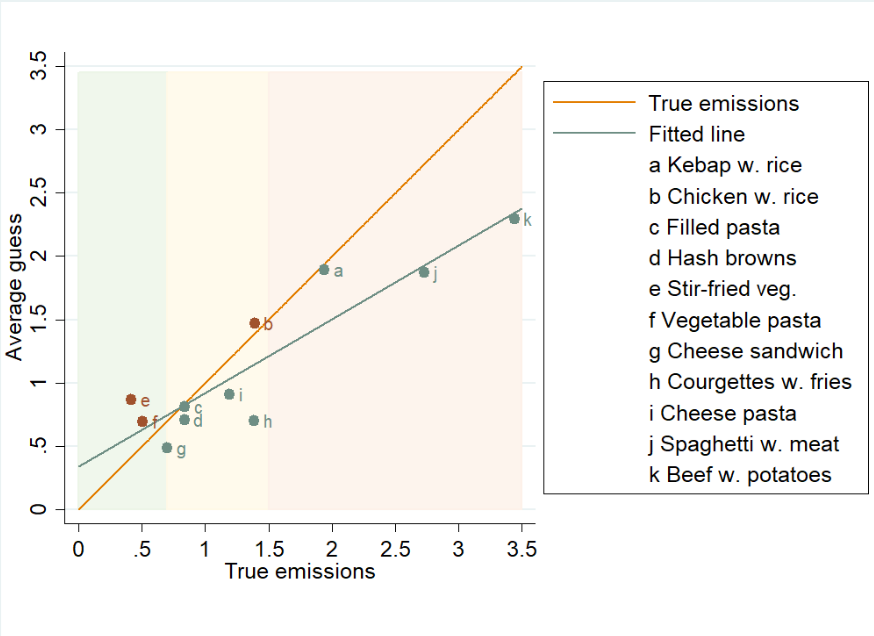

Figure 14. Average guess of the emissions caused by a given meal, plotted against true emissions. Values closer to the orange line are more precisely estimated. Meals corresponding to orange scatter points are on average overestimated in their emissions, while meals corresponding to green scatter plots are on average underestimated. The 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 and the participants in the "Control, then Control" group of Experiment 1. An exception is the meal "Spaghetti with meat" which is only included in the guessing questions of Experiment 1. 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. The graph background is colored green, yellow, and red to show the label color assigned to the respective meals.

experiment (see section 3 for details). Figure 14 draws on both these data sources and displays how average guesses deviated for each of the meals. On average, participants rather underestimate emissions (green-colored dots) and overestimate emissions for some low-emission meals (red-colored dots).

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

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

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.[29] 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. I calculate this indicator by comparing the difference between the individual's

29. Please see section 3.2 and C.3 for details on this specification.

guess for the emissions of meal m with her guess for the cheese sandwich and 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.

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. In spec. (2), it is €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 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.3 in the Appendix) and (b) only individuals who did an above-average job in guessing emission magnitudes (Figure C.5 in the Appendix). Patterns look similar to Figure 15.

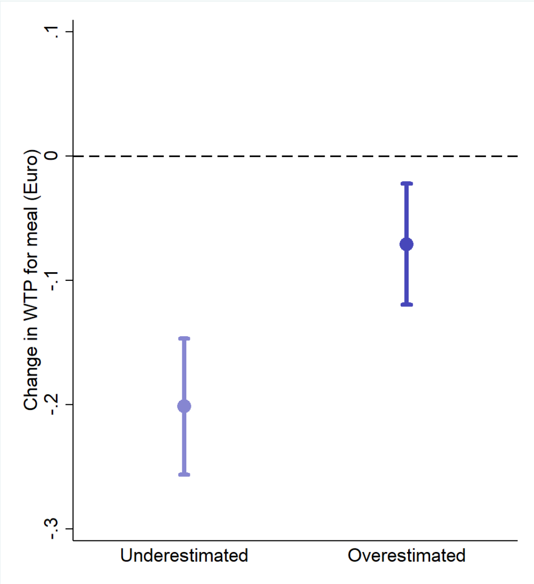

Figure 15. Within-subject change in willingness to pay for a specific meal when shown carbon labels, depending on whether the participant previously over- or underestimated the difference in emissions between the specific meal and the cheese sandwich. Participants are all in the ATTENT+LABEL condition. 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	267
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 specific meal when shown carbon labels (ATTENT+LABEL condition). In spec. (1), 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.

The effect of directing attention. In the next step of the analysis, I include data from the ATTENT condition in the analysis to estimate the magnitude of a possible attention effect. I estimate

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

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.8 and C.9 in the Appendix.

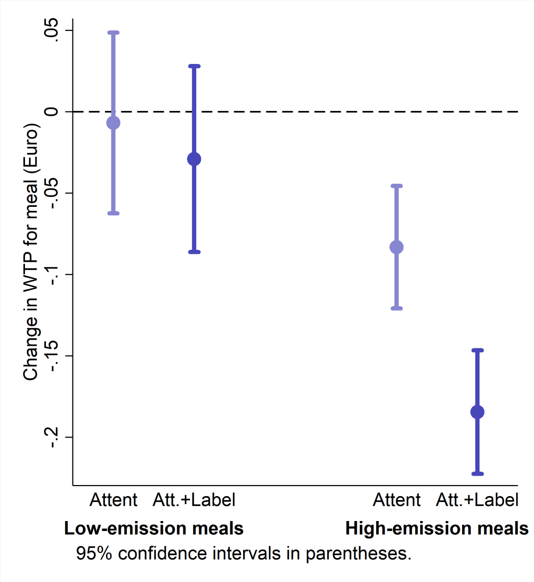

Figure 16. Within-subject change in willingness to pay for a specific meal, comparing participants in the ATTENT and ATTENT+LABEL condition. Effects are split into meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with 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 Figure 16 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.

7 Structural estimation

The structural estimation complements the reduced-form results from section 6.2. Section 7.1 estimates the parameters of the theoretical model described in section 5 using data from Experiment 3. Section 7.2 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 misperception-correcting and attention-directing effect of carbon labels identified in section 6.2.

7.1 Results

I rewrite the four equations in section 5.2 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% and insignificant. This suggests that individuals are relatively likely to be inattentive to carbon emissions when making consumption choices in the absence of labels. κ, the stickiness of the average consumers' prior estimate of emissions caused, is estimated at 0.21 and insignificant. This suggests that individuals on average trust the emissions information, increasing the potential for labels to correct behavior towards lower emissions. γ describes how the emissions of one kg of greenhouse gas emissions affect an individual's utility. This

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.

is estimated as a decrease in monetized utility of Euro 0.12 per kg of emissions caused by the meal chosen.

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 maximizing 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 €0.20 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.

7.2 Intervention comparison based on estimated parameters

In the model described in section 5, 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 5, and some assumptions on what constitutes a typical student canteen offer and pricing structure. These assumptions and the simulation are discussed more in detail in section A.4. Table 6 shows simulation results.

For all three interventions, the interventions do not impact consumption decisions in the vast majority of cases, with 98% to 99% of consumption decisions not affected by the interventions. In Experiment 2 described in section 4, I also estimate that a labeling intervention in the student canteen in the field only impacts 2% of consumption decisions, and this result is thus not overly surprising. However, the student canteen guests observed in Experiment 2 and the experiment participants observed in Experiment 3 seem to differ in their appreciation of student canteen meals. While the regular student canteen guests observed in Experiment 2 obviously value student canteen meals sufficiently to visit the student canteen regularly, participants' valuation for the student canteen meals in Experiment 3 is, in over 70% of cases, lower than the student canteen price.

The ATTENTION, KNOWLEDGE, and LABEL intervention all decrease the consumption of the meat option, with consumers reverting to the sandwich instead in the ATTENTION and KNOWLEDGE intervention, while consumers revert to a mix of sandwich and vegetarian meal in the LABEL intervention. 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 intervention 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.

The extension of my model to consumer welfare specified in section A.1 describes the consumer welfare resulting from a meal choice as 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 (e.g. 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 less, 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 €0.0016 per choice. Synergies between the ATTENTION and KNOWLEDGE intervention are more sizable here, with the effects of the other two interventions merely summing to 0.001. Section A.5 ex-

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

Intervention	# of choices sandwich	veg.	meat	Δ GHGE Average	Δ consumer welfare 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.

amines 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 in 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.[30] I assume that the proceeds from this tax are uniformly redistributed among individuals. 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 change 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.

8 Consumer preferences for the presence of carbon labels

This section discusses experimental evidence from all three experiments on consumers' preferences for seeing carbon labels in their consumption decisions. Section 8.1 discusses evidence from experiments 1 and 3, and section 8.2 discusses evidence from Experiment 2. Section 8.3 discusses possible determinants of consumers' willingness to see or avoid carbon labels.

8.1 Evidence from the lab-in-the-field experiments

In both lab-in-the-field experiments, participants indicate their willingness to pay for carbon labels being (non-)present during their final set of consumption decisions. These elicitations are incentivized as described in section 3. The frequency distribution of willingness to pay values is visualized in Figure 17. About 50% of participants have a willingness to pay of 0, while less than 5% have a negative willingness to pay. The remaining participants are willing to pay a positive amount, with 21% of the sample willing to pay €0.50 and above. Values barely differ between treatment groups,

30. I use a value of €120 per tonne for comparability with Experiment 1 results.

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.24.

Table 7 shows a correlation analysis between willingness to pay to see 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). Willingness to pay to see carbon labels is weakly negatively correlated with participants' self-reported confidence in existing knowledge of emission values. Further, participants' self-control in eating behavior (as elicited using the questionnaire developed by Haws, Davis, and Dholakia (2016)) is very weakly correlated with willingness to pay to see emission values. Thunström (2019) find a similar, but much stronger relation between the experience of calorie labels and self-control. For participants who saw carbon labels previously in the experiment, I estimate that stronger previous treatment effects strongly correlate with a higher willingness to pay for the presence of carbon labels in the final experiment decisions (Table C.25).

8.2 Evidence from the field experiment

After the 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 carbon tax, 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.

8.3 Discussion of possible preference drivers

The above data can be interpreted in different ways, depending on what one assumes to be driving consumers' preferences for carbon labels.

First, experiment participants' overwhelmingly positive willingness to pay for the presence of carbon labels and the support for carbon labels expressed in the field survey might be due to consumers receiving a psychological benefit from the carbon labels. For instance, seeing the carbon labels might provide consumers with a feeling of being informed and in control of their carbon emissions. This idea would be supported by the strong correlation of willingness to pay with participants' self-reported willingness to use the information and the strong correlation with participants' treatment effect.

Second, experiment participants' high willingness to pay for the presence of carbon labels in the lab-in-the-field experiments as well as the consumption reactions I observe to carbon labels in all three experiments might be due to participants' wish to behave in a socially desirable manner. While this could be understood as an "experimenter demand" effect in the lab-in-the-field setting, as the experiments' focus on carbon labels might signal to participants that the avoidance of carbon emissions is socially desirable, it could in turn be a "policy maker demand" effect in the field setting, with the student canteens' focus on carbon labels transmitting the same message. Lab-in-the-field experiment participants would then in the final step of the experiment also indicate a positive willingness

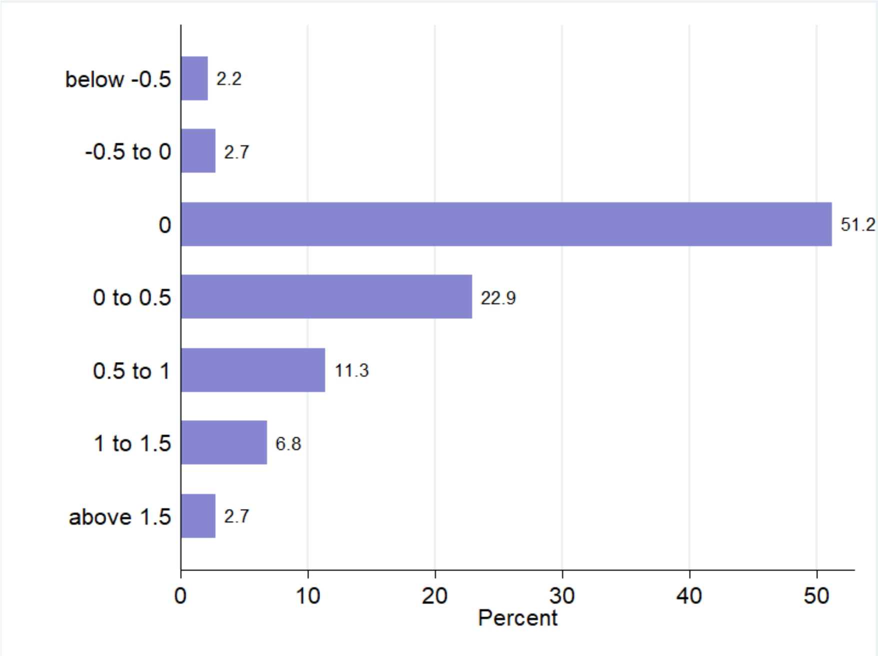

Figure 17. Average willingness to pay to see labels on the final three consumption decisions. Includes data from 733 participants from both lab-in-the-field experiments.

to pay for the presence of labels as a vehicle to help them adhere to the social norm. The weak positive correlation between participants' willingness to pay for the presence of carbon labels and the perceived strength of social norms towards avoiding a high carbon footprint in food consumption speaks in favor of this explanation.

One might then understand the carbon labels as in fact decreasing consumers' welfare, as choosing the socially desirable option to avoid a "cold prickle" effect of deviating from the social norm might prevent consumers from choosing the option that truly maximizes their utility. In this case, each induced carbon-friendly choice is costly for the consumer. However, such a negative experience with the labels is difficult to reconcile with the very positive attitude towards carbon labels captured in the field survey. I signaled to survey participants that survey results would be communicated to the student canteen, and survey participants thus could greatly alleviate social pressure by indicating that they are not in favor of a permanent installation of the labels. This would no longer make the presumably socially optimal choice evident for all student canteen guests, thus greatly weakening (although perhaps not completely removing) the induced social pressure to consume in an environmentally friendly manner.

I thus consider it likely that the first interpretation plays a greater role, and analyze effects on consumer welfare accordingly in 7.2. However, it is also unlikely that social desirability aspects play no role at all, and the treatment effects I observe in all three experiments are all reconcilable with the social desirability explanation outlined above. The effect of the carbon labels which I in my model describe as making consumers attentive to emissions could then instead be understood as making consumers attentive to the social desirability of avoiding emissions.

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

	(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				-0.03	
				(0.02)	
Eating self-control					0.01
					(0.03)
Constant	0.15***	-0.03	0.03	0.20***	0.20***
	(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).

9 Discussion

This paper provides causal 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 seem to primarily impact consumers by directing their attention towards carbon emissions. Correcting consumers' misperceptions about carbon footprints plays a secondary role in driving treatment effects. Consumers seem to, on average, incur a psychological benefit from the presence of carbon labels in their consumption decisions.

These results speak towards attention frictions playing an important role in impeding consumers from behaving in a carbon-friendly manner in the absence of an intervention. 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 to consumers, while reducing emissions in food consumption does not. On average, consumers seem to receive a psychological benefit from avoiding emissions and the carbon labels benefit consumers by removing frictions. Second, resource consumption is a continuous choice while food consumption is a discrete choice decision. Results are thus an example case that increasing attention can also be effective in a discrete choice context, and open the door to examining related discrete choice consumption contexts.

The student canteen context in itself offers potential for reducing emissions on a large scale. In a back-of-the-evelope calculation, I estimate that a national roll-out of carbon labels in all student canteens in Germany would avoid 2,000 tons of carbon annually.[31] In the student canteen in Bonn, I estimate the label implementation cost at €20 per tonne to €120 per tonne depending on whether the process is automatized.[32] Findings are likely also relevant for related food contexts, such as corporate canteens or grocery shopping. 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.

Results hold several possible policy implications for the student canteen and related contexts: First, the considerable effectiveness of carbon labels and their high acceptance speak in favor of implementing carbon labels as a second best option to a carbon tax. Second, the ability of carbon labels to correct attention frictions speaks in favor of implementing carbon labels even in relatively knowledgeable populations in which one would expect little

31. 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). I estimate that carbon labels decrease average emissions per meal by 25 gram, implying average annual savings of around 2,000 tons which equals the yearly average emissions of around 224 Germans. (**BU2**)

32. With an automatized process, the main cost would be the Eaternity application costing €0.60 per day. With a manual calculation everyday (e.g. by a student assistant paid €20 per hour, this would cost around €5.00 additionally per day. On average, 2,000 meals are bought in the canteen daily, so average daily savings would be 50 kg.

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.[33] 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 $WTP^{A+L} < p_m - p_o$

33. 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").

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

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

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 ATTENT 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.24 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

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

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

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

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 Attent treatment.

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

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 which 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.[34] 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.

34. 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.

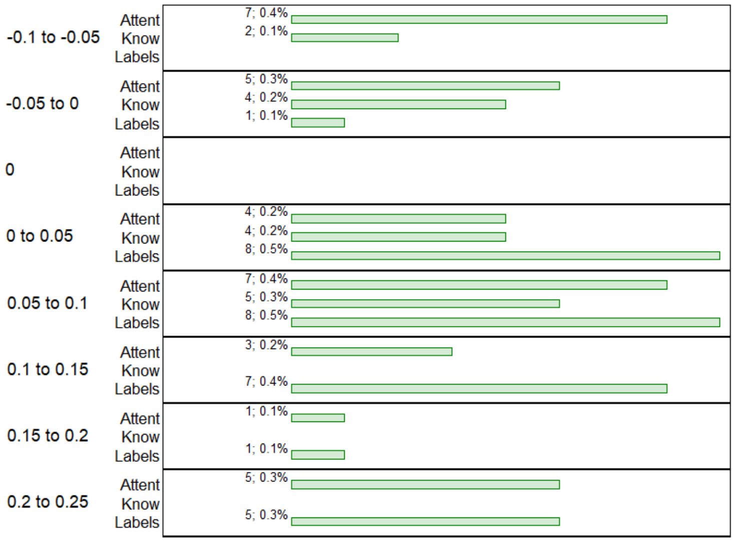

Figure A.1. Estimated change in consumer welfare per meal which would be caused by solely raising attention, solely correcting misperceptions or the combination of both (labels), in Euro. The figure shows utility changes for instances in which the interventions leads to behavioral change (otherwise change 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.

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.

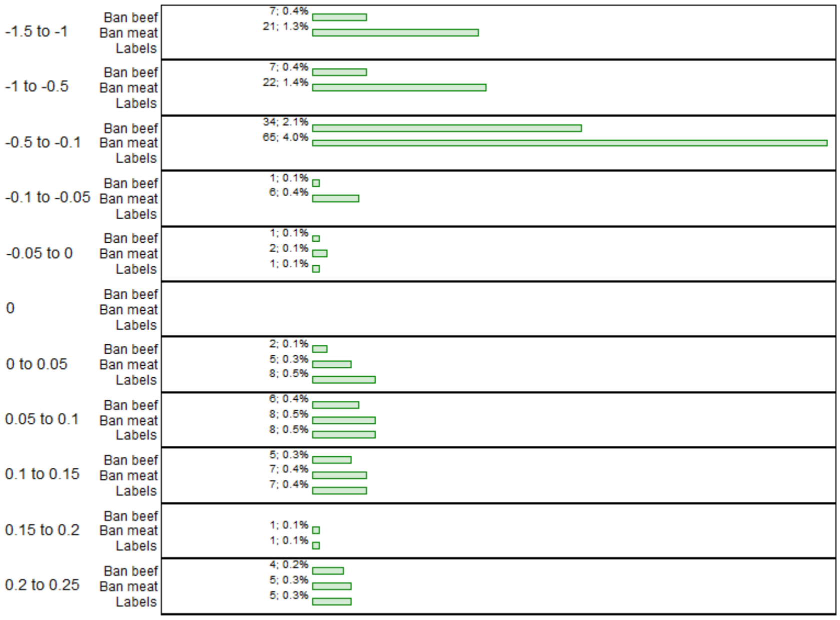

Figure A.2. Estimated change in consumer welfare which would be caused by a ban of beef or a ban of meat compared to carbon labels, in Euro. Estimation based on experiment data and student canteen prices and offer structure. The figure shows utility changes for instances in which the interventions lead 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.

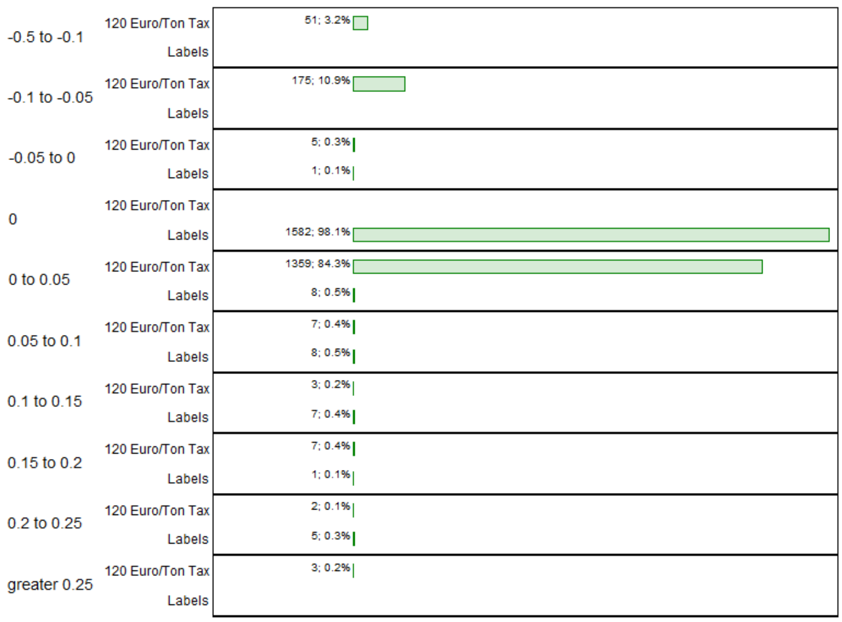

Figure A.3. Estimated change in consumer welfare which would be caused by a carbon tax of €120 per tonne compared to carbon labels, in Euro. Proceedings from the tax are re-distributed equally to all consumers. Estimation based on experiment data and student canteen prices and offer structure.

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

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, Italian vegetable ragout with pasta, Chicken Schnitzel with rice, and beef ragout with potatoes. Participants who indicate they are vegetarian decide on four vegetarian meals: Filled courgettes with potato croquettes, Italian vegetable ragout with pasta, Cheese "Spätzle" with mushrooms, and stir-fried vegetables with rice.

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.

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 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.

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.10 and Table C.11. 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.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 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. 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.[35]

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.12, C.20 and C.16 thus repeat the main analyses from experiments 1 and 3 including only students. Results are similar to those reported in sections 3 and 6, so results are not driven by a higher proportion of non-students.

35. 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		

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		

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

Variable	Explanation	Mean	Std. Dev.
Age	Age of participant	23.03	–
Male	Dummy: 1 if participant is a man	0.40	–
Student	Dummy: 1 if participant is a student	0.93	–
Non-vegetarian	Dummy: 1 if participant eats meat	0.7	–
N	1,795		

Note: Statistics are based on a survey I conducted among student canteen guests in April. See D.1 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 mean age, I set age equal to the midpoint of each interval (13% of respondents are below 20 (set to 18),53% of respondents are between 20 and 23 (set to 21.5), 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 Alternative econometric specifications

Alternatively to the estimation approach described in section 3.2, one could instead estimate the following specification:

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

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.

$(High_m * Post_j)$ 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. $(Low_m * Post_j)$ 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.

$(High_m * Post_j * Label_{ij})$ 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. $(Low_m * Post_j * Label_{ij})$ 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.6 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.6. 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
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

C.4 Replication excluding round 3 observations

Table C.7 replicates Experiment 1 results excluding observations from round 3 of the experiment.

Table C.7. 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.8. 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.9. 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.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.10. 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.11. 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.12. 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.13. 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	208
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.14. 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	62
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.15. 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.16. 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.17. 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.18. 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.19. 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.20. 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.21. Replication of Table 4 including only non-students.

C.6 Comparison of effects Experiment 1 and Experiment 3

Table C.22. Comparison of average within-subject effects in Experiment 1

	Change in WTP					
	(1) Con.	(2) Con.	(3) La.	(4) La.	(5) Of.	(6) 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

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

Table C.23. Comparison of effect sizes in Experiment 3

	Change in WTP					
	(1) At.+La.	(2) At.+La.	(3) At.	(4) At.	(5) At.+Of.	(6) 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

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

C.7 Participants' willingness to pay for the presence of carbon labels

Table C.24. 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.25. 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.

C.8 Descriptive statistics on baseline willingness to pay for meals

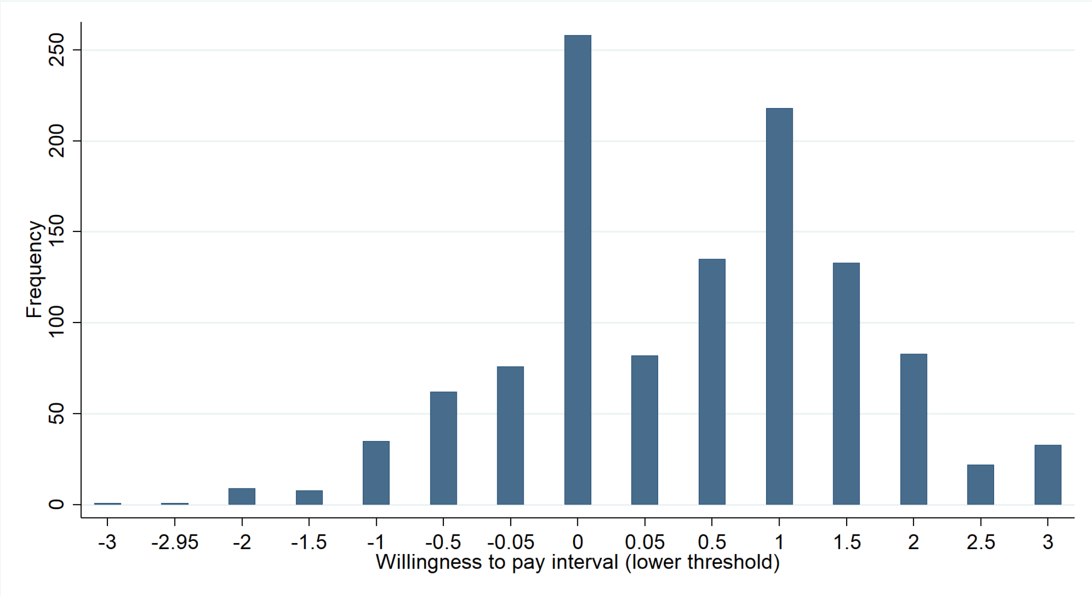

Figure C.1. Willingness to pay indicated for meals during the baseline purchase decisions in Experiment 1. $N = 1,156$ (289 participants making 4 baseline decisions each).

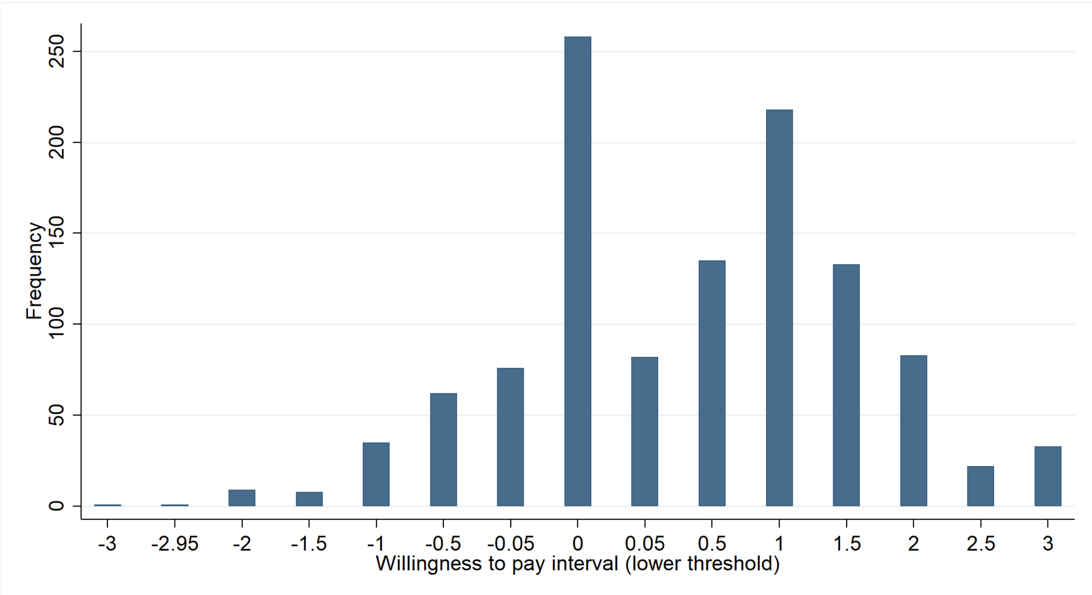

Figure C.2. Willingness to pay indicated for meals during the baseline purchase decisions in Experiment 3. $N = 1,776$ (444 participants making 4 baseline decisions each).

C.9 Heterogeneity in treatment effects in Experiment 1

Table C.26. Heterogeneity based on same items as heterogeneity analysis in the field (Table E.4)

	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 specific meal when shown carbon labels (Label condition). Linear regression following Spec. (1) in Table 1. Col. (1) includes all data, 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. Table E.5 reports evidence from experiment 2 for the same heterogeneity factors.

Table C.27. 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***	-0.40***	-0.31***	-0.33***
	(0.05)	(0.06)	(0.07)	(0.06)	(0.05)	(0.07)
Low emission meal x Shown label	0.14***	0.16***	0.13**	0.15***	0.14***	0.22***
	(0.04)	(0.06)	(0.07)	(0.05)	(0.04)	(0.06)
High emission meal	0.01	-0.01	0.01	-0.00	0.01	0.00
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)
Low emission meal	-0.06*	-0.04	-0.05	-0.05	-0.06*	-0.10**
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)
Control for third round	0.01	-0.00	0.03	0.03	0.01	0.01
	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.05)
Participants control	140	71	78	110	140	70
Participants treated	218	107	105	160	218	105
Observations	1,716	880	916	1,316	1,716	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 specific meal when shown carbon labels (LABEL condition). Linear regression following Spec. (1) in Table 1. 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).

Table C.28. Heterogeneity in experiment 1

	Change in WTP compared to baseline						
	(1) All	(2) Social circle	(3) Hungry	(4) Strong l.o.c.	(5) Low income	(6) Price sens.	(7) Env. important 2
High emission meal x Shown label	-0.31***	-0.36***	-0.40***	-0.40***	-0.33***	-0.27***	-0.39***
	(0.05)	(0.05)	(0.07)	(0.07)	(0.07)	(0.05)	(0.07)
Low emission meal x Shown label	0.14***	0.14**	0.08	0.18***	0.12*	0.19***	0.10*
	(0.04)	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)
High emission meal	0.01	0.04	0.05	0.04	0.05*	0.05*	-0.01
	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Low emission meal	-0.06*	-0.04	-0.02	-0.05	-0.01	-0.12**	-0.02
	(0.03)	(0.04)	(0.04)	(0.04)	(0.05)	(0.06)	(0.03)
Control for third round	0.01	0.03	0.02	0.03	0.00	0.01	0.01
	(0.03)	(0.04)	(0.05)	(0.04)	(0.05)	(0.04)	(0.04)
Participants control	140	79	66	84	54	60	78
Participants treated	218	123	104	118	82	109	115
Observations	1,716	968	816	988	656	800	924

Standard errors in parentheses

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

C.10 Effect on calorie guesses

C.11 Experiment 3: Additional descriptives and robustness tests for under- and over-estimation of emissions

Descriptives on under- and over-estimation.

Table C.29. Effects of the treatment on calories guessed

	Guess of calories in				
	(1) Chicken w. rice	(2) Courgettes w. fries	(3) Beef w. potatoes	(4) Cheese sandwich	(5) Veg. pasta
Saw carbon labels earlier	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

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

Table C.30. 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.31. 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.32. 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, 88 got two relative ranks right (i.e. two meals stood in the correct relationship to each other).

Results split by guess accuracy.

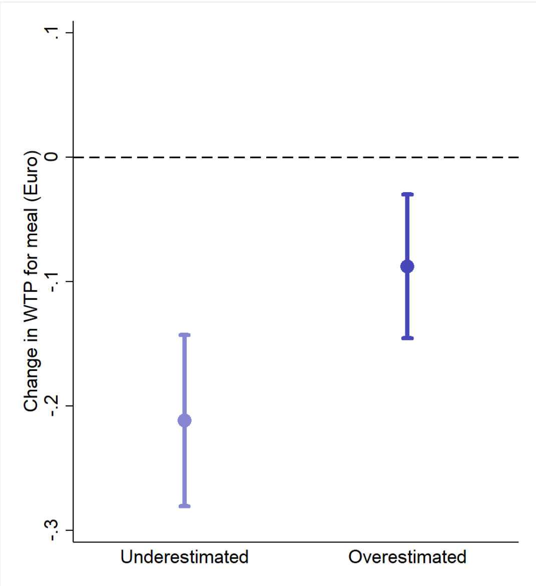

Figure C.3. Replication of Figure 15 including only individuals with at least three correct ranks (194 participants). Bars indicate 95% confidence intervals.

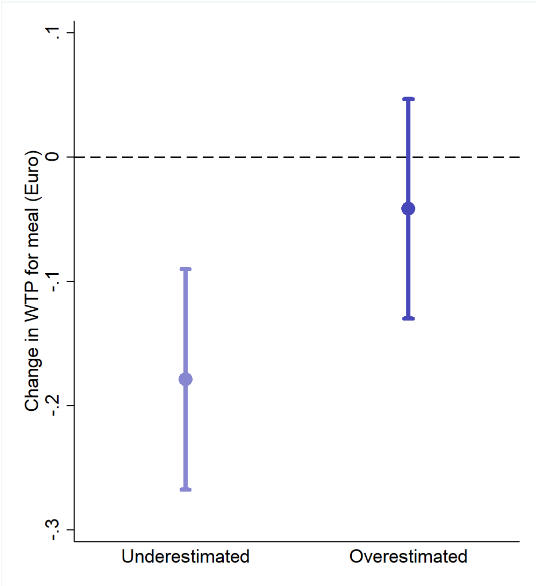

Figure C.4. Replication of Figure 15 including only individuals with at most two correct ranks (99 participants). Bars indicate 95% confidence intervals. Bars indicate 95% confidence intervals.

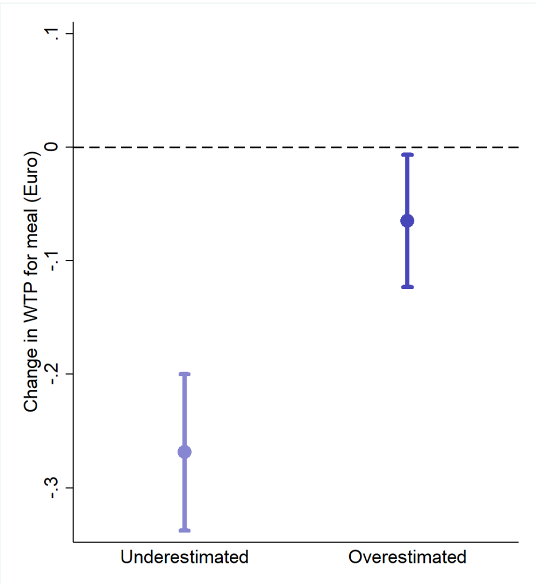

Figure C.5. Replication of Figure 15 including only individuals with at least three correctly guessed magnitudes (171 participants). Bars indicate 95% confidence intervals.

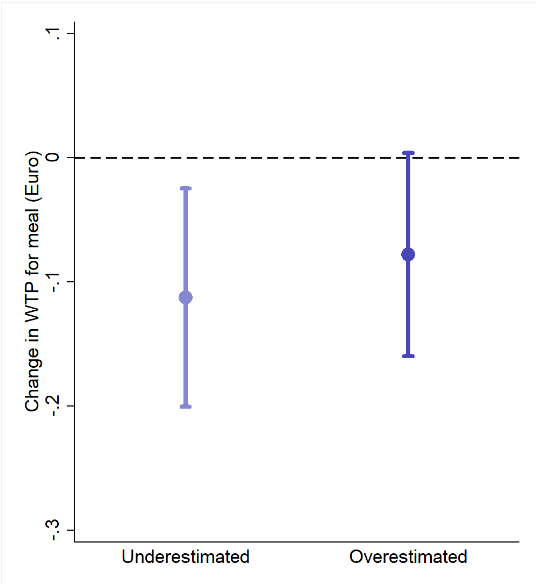

Figure C.6. Replication of Figure 15 including only individuals with at most two correctly guessed magnitudes (129 participants). Bars indicate 95% confidence intervals. Bars indicate 95% confidence intervals.

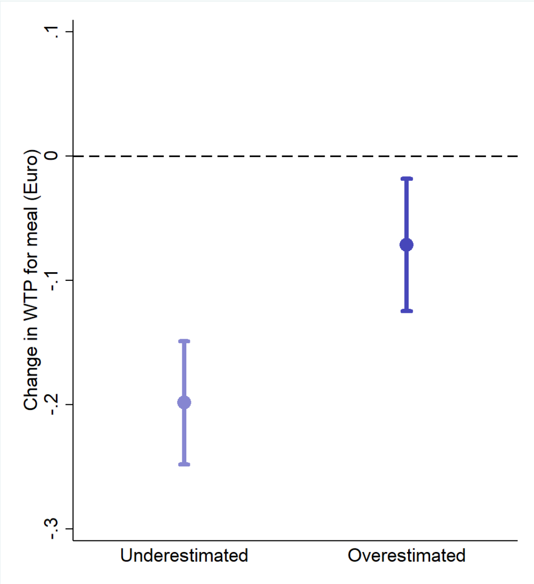

Figure C.7. Replication of Figure 15 based on under- or over-estimation of the specific meal, instead of under- or over-estimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals.

	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.33. 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.

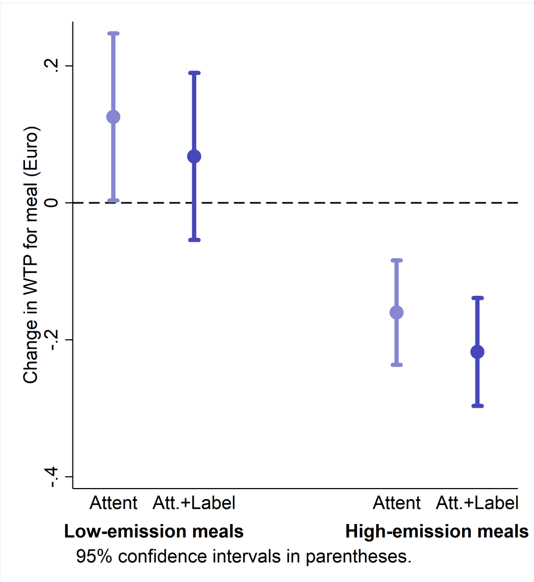

Figure C.8. Replication of Figure 16 including only participant-meal combinations where emissions were guessed accurately enough to receive a bonus payment (guess within 20% of true value, 543 observations). Bars indicate 95% confidence intervals.

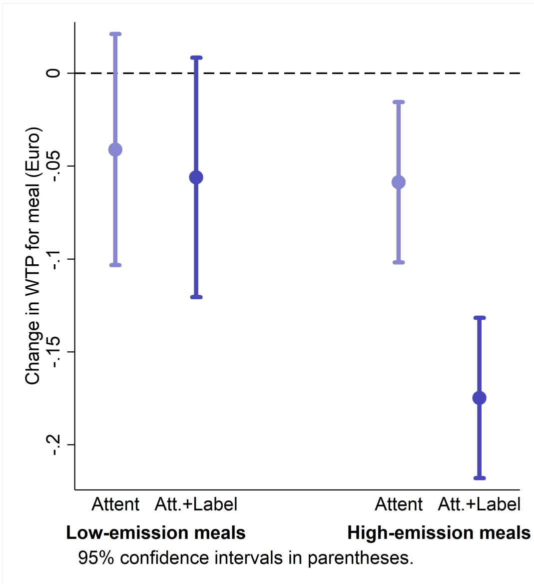

Figure C.9. Replication of Figure 16 including only participant-meal combinations where emissions were not guessed accurately enough to receive a bonus payment (guess not within 20% of true value, 1,837 observations)

Appendix D Experiment 2: Details on experimental set-up

D.1 Setting and additional descriptive statistics

Canteen set-up in Bonn. The 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. [36] Correspondingly, the dependent variable in my main regression is whether the main meal component a restaurant guest chooses contains meat or is vegetarian.

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.

36. 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.

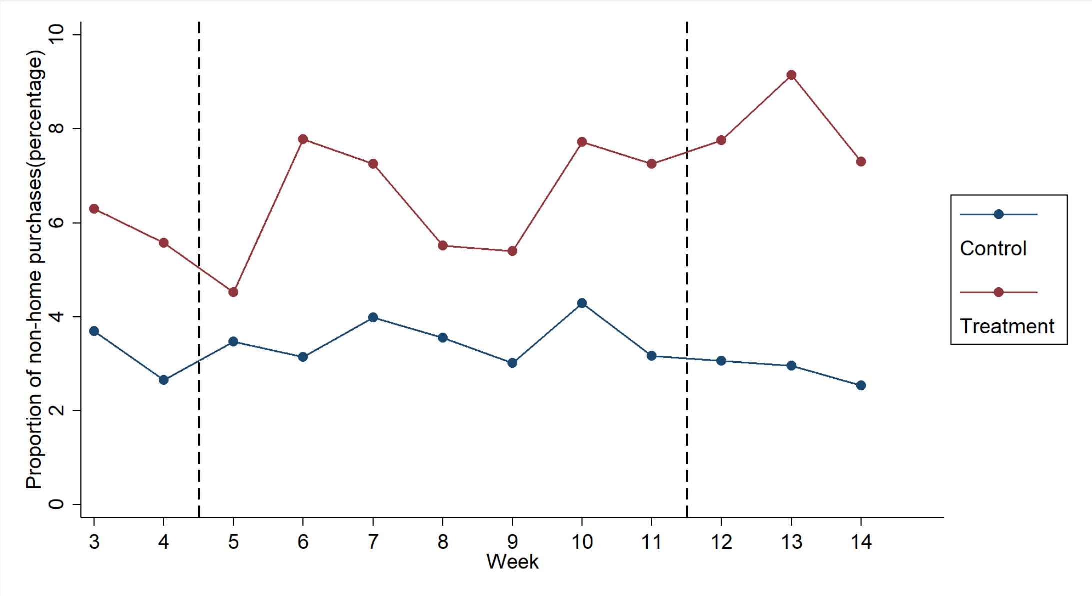

Figure D.1. Proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks. The sample is similar to that in spec. (4) in Table 2, but intention to treat is calculated based entirely on the first two weeks, based on a minimum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

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 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.

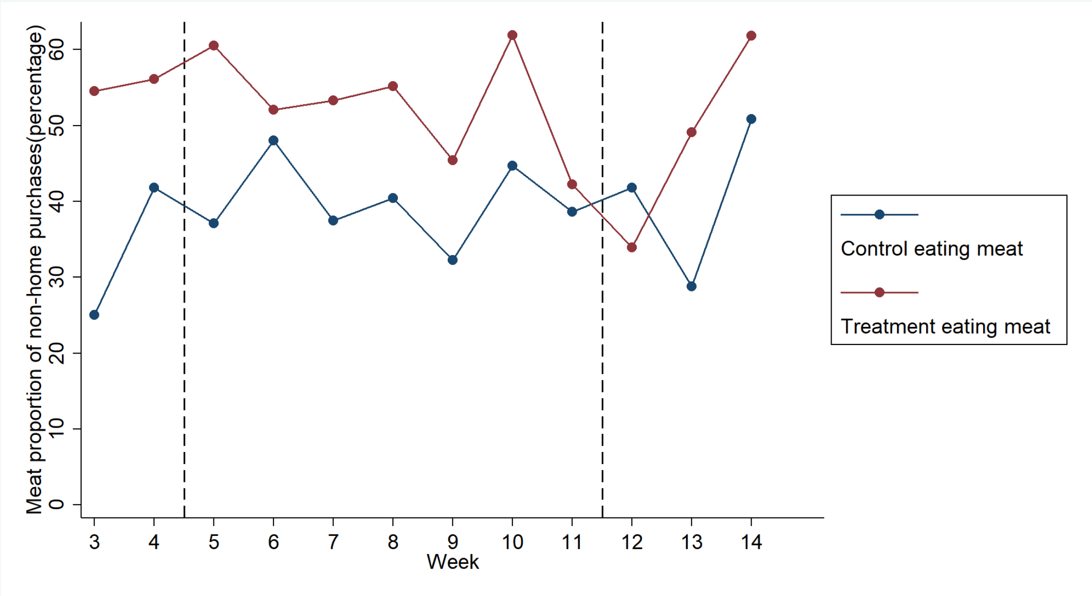

Figure D.2. Meat proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks. The sample is similar to that in spec. (4) in Table 2, but intention to treat is calculated based entirely on the first two weeks, based on a minimum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

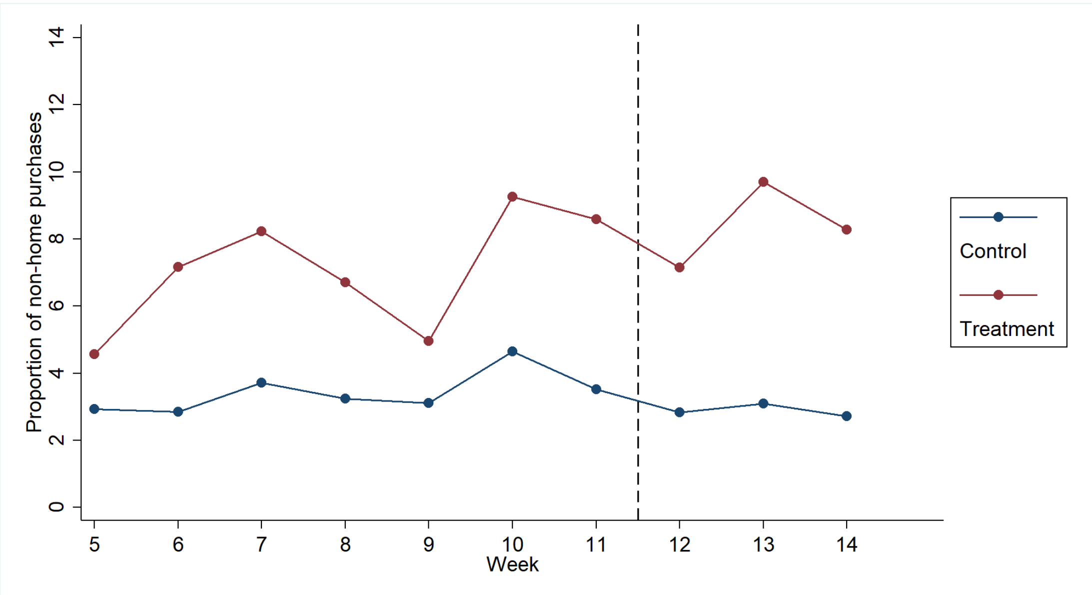

Figure D.3. Proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first four weeks (pre-intervention phase). The sample is similar to that in spec. (4) in Table 2, but also includes observations from week 5 of the sample period. $N =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

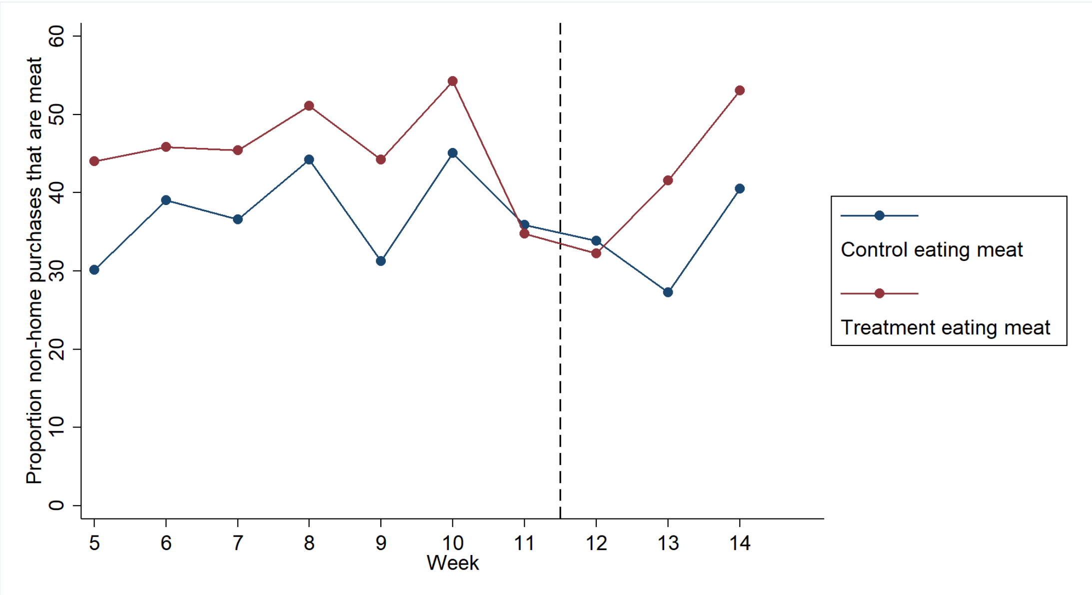

Figure D.4. Meat proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks (pre-intervention phase). The sample is similar to that in spec. (4) in Table 2, but also includes observations from week 5 of the sample period.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

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. [37]

37. 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.

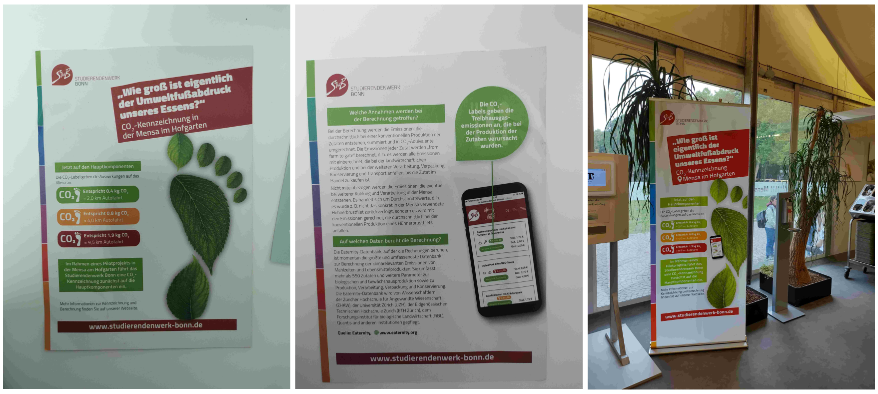

Figure D.5. Explanation of the carbon labeling on flyers (left and center) and billboards at the entrance of the student canteen (right).

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 E.4) 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, 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.

Survey accompanying 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.6). 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.

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.

Figure D.6. Leaflet advertising participation in the survey, as distributed in front of the student canteen.

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 (citation dropped to preserve anonymity), 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

Figures including week 5.

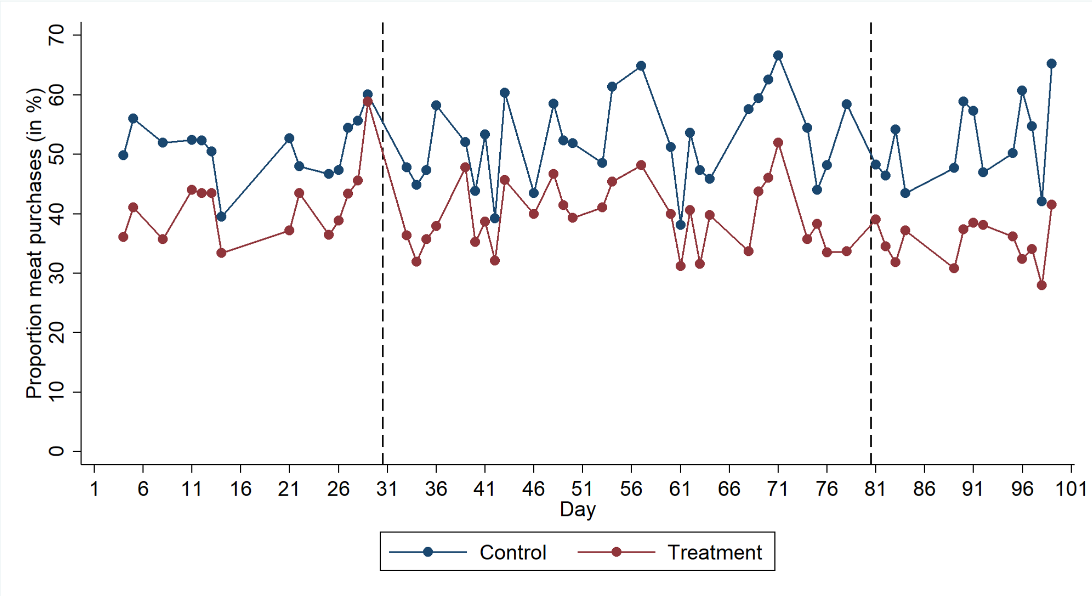

Figure E.1. Average proportion of meat meals sold in the sample period, using the final sample but including week 5. $N = 130, 132$

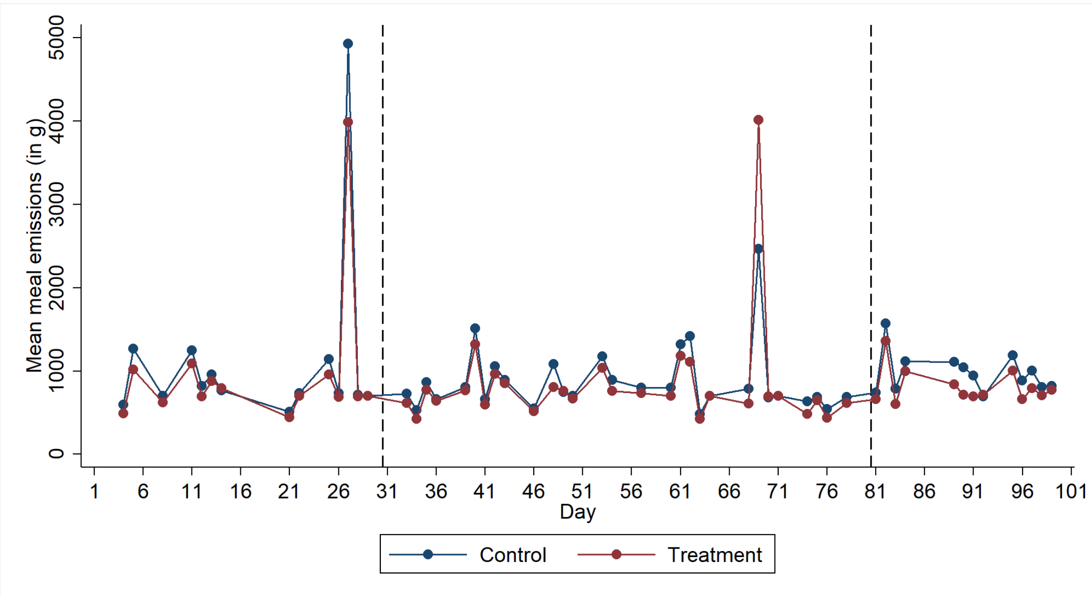

Figure E.2. Average emissions per meal in the sample period, using the final sample but including week 5. $N = 130, 132$

Long-term post-intervention effects.

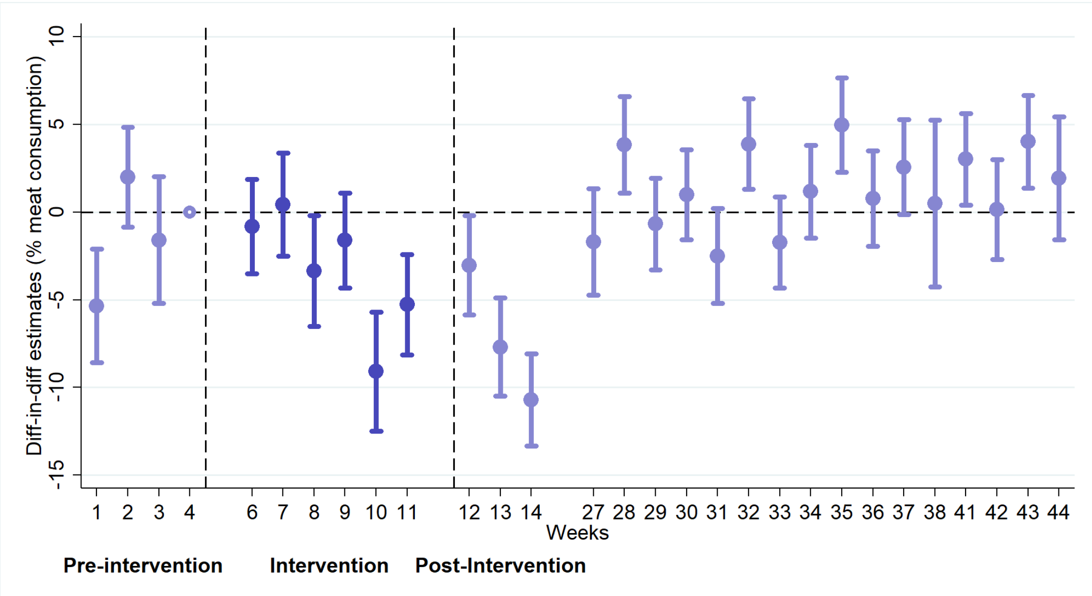

Figure E.3. Event study: Difference in difference estimates of the likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1–4 constitute the pre-intervention phase, while weeks 6–11 constitute the intervention phase, and weeks 12–14 the post-intervention phase. Weeks 27 onwards are the new semester. The regression specification closely follows specification (1) in Table 2, estimating treatment effects on a weekly basis and including weekly time controls. Bars indicate 95% confidence intervals.

Table E.1. 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.

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 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 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 expanded the time frame study 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 a veg. main meal component sold then), it increased to around €0.50 from October to December 2023(around 25% 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.2 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.

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.2 includes all student[38] 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 the 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 eight-fold 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 twelve-fold the effect of the carbon labels identified within the regression analysis shown in Table E.2 (implying rough equivalence of this effect to a carbon tax of €120 per tonne). In Experiment 1, the lab-in-the-field experiment discussed in section 3, 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

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

Table E.2. Comparison of effects: labels vs. "carbon tax"

	Likelihood of consuming meat		
	(1)	(2)	(3)
Price difference (in Euro)	-0.16***	-0.25***	-0.32***
	(0.05)	(0.05)	(0.08)
Treatment restaurant x Label period	-0.04***	-0.03***	-0.03***
	(0.00)	(0.01)	(0.01)
Treatment restaurant	-0.05***	-0.04***	-0.04***
	(0.00)	(0.00)	(0.00)
Constant	0.34***	0.41***	0.64***
	(0.07)	(0.08)	(0.04)
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	313,397

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.

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 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 high 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.3. 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.

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 E.2 and E.1 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. [39] The restricted sample contains 33,427 observations. As shown in Table E.3 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.

Heterogeneity in treatment effects. Table E.4 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.5 shows analyses restricting the

39. 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.

Table E.4. Effect of labels on meat consumption, different subsamples

	Likelihood of consuming meat				
	(1) All	(2) Employees	(3) Non-busy time	(4) Card payment	(5) Frequent
Treatment restaurant x Label period	-0.02***	-0.05*	-0.03***	-0.02**	-0.02*
	(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 effects	Yes	Yes	Yes	Yes	Yes
Fixed effects	No	No	No	No	No
Guests control	6,924	881	3,800	5,971	1,818
Guests treated	2,815	265	1,678	2,510	724
Observations	120,093	20,847	67,664	70,120	47,029

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 durin the 13-week sample period.

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.5. Effect of labels on meat consumption, different subsamples

	Likelihood of consuming meat				
	(1) All	(2) Survey	(3) Female	(4) Below 24	(5) Env. important
Treatment restaurant x Label period	-0.02**	-0.03	-0.05*	-0.05*	-0.04*
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)
Treatment restaurant x Post period	-0.09***	-0.09***	-0.08***	-0.09***	-0.08***
	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)
Treatment restaurant	-0.05***	0.00	0.06**	0.02	-0.02
	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)
Constant	0.39***	0.24***	0.16***	0.22***	0.11***
	(0.02)	(0.03)	(0.04)	(0.04)	(0.03)
Date effects	Yes	Yes	Yes	Yes	Yes
Fixed effects	No	No	No	No	No
Guests control	5,971	851	464	573	447
Guests treated	2,510	522	272	355	235
Observations	70,120	15,032	6,948	10,370	7,146

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 pay an important role in their food consumption decisions. Table C.26 reports evidence from experiment 1 for the same heterogeneity factors.

Additional figures.

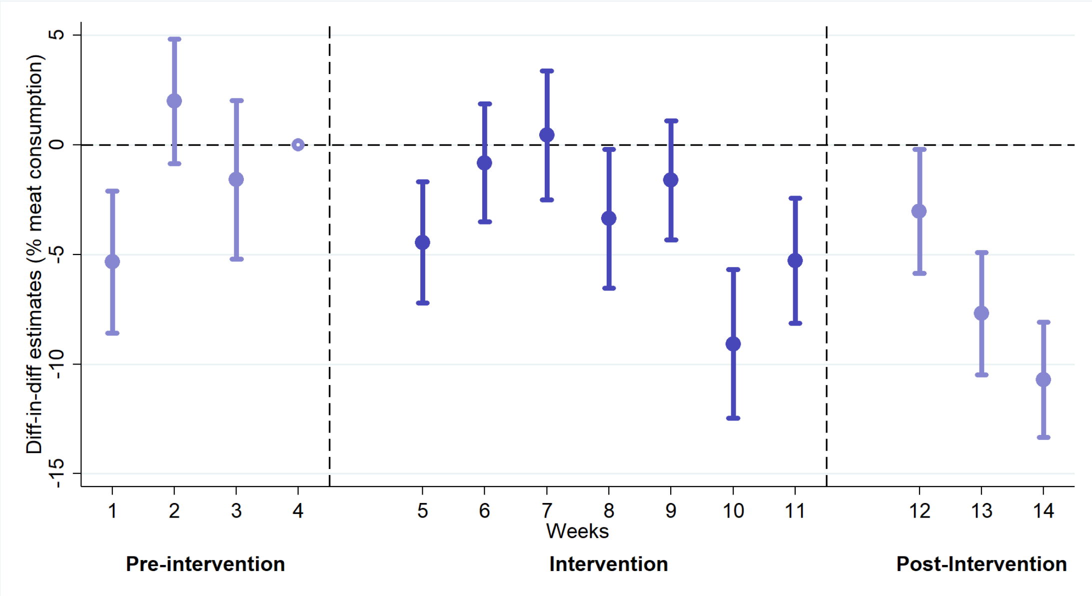

Figure E.4. Event study: Difference in difference estimates of the likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. In contrast to the main analysis, this analysis includes week 5. During week 5, the treated restaurant promoted healthy eating. The strong decrease in meat consumption in week 5 is thus not clearly attributable to the carbon labels. Weeks 1–4 constitute the pre-intervention phase, while weeks 6–11 constitute the intervention phase, and weeks 12–14 the post-intervention phase. The regression specification closely follows specification (1) in Table 2, estimating treatment effects on a weekly basis and including weekly time controls. Bars indicate 95% confidence intervals.

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