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The Effectiveness of Carbon Labels

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Carbon labels have been shown to reduce the carbon footprint of consumption choices in several contexts. But are they also an effective policy tool? This depends on how the reductions produced by carbon labels relate to what can be achieved with the alternative policy tools we have available. This paper establishes a comparison to carbon taxes, using several field experiments in the student canteen. I estimate that carbon labels reduce carbon emissions by approx. 4%, and that a carbon tax of €120 per ton would be needed to achieve similar reductions with price changes alone. This comparison conveys that carbon labels are relatively effective: €120 per ton exceeds current EU ETS trading prices by more than 150% and is three times the current German carbon tax on gasoline. Furthermore, I provide evidence that the main reason carbon labels are effective is not that they are able to correct consumers' misperceptions about carbon footprints. Instead, they appear to primarily influence consumers by directing attention towards carbon emissions at the moment of choice.

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

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1 Introduction

The concept of labeling products with the carbon emissions[1] they cause has received attention from academia,[2] regulators,[3] and private companies.[4] This is especially the case for food products. The food sector is currently responsible for 26%–34% of global greenhouse gas emissions,[5] but this amount could be greatly reduced by shifting to diets with lower carbon footprints.[6] While command-and-control measures and carbon taxes face particular resistance in the food sector,[7] softer policy tools such as carbon labeling seem to be more acceptable to most consumers.[8]

But are carbon labels also an effective policy tool? Previous studies find that carbon labels reduce carbon emissions by between 1% and 5%, using field experiments in student canteens, super markets and online food delivery platforms (see Brunner et al., 2018; Bilén, 2022; Lohmann et al., 2022; Ho and Page, 2023). But are these reductions "large enough" to warrant implementation? This paper addresses this question by highlighting an important consideration: if carbon labels are not used, emissions must be reduced through alternative means.[9] This creates an opportunity cost of not adopting carbon labels. This paper quantifies this cost by assuming that reductions not achieved via labels are instead achieved through a carbon tax. Specifically, it examines how high a carbon tax would need to be to achieve the same emission reductions as produced by carbon labels. I focus on carbon taxes because they are widely regarded a straightforward and effective policy tool.[10]

My two field experiments are designed to yield independent estimates of how consumption decisions are influenced by carbon labels, and how this compares to consumers' reaction to price changes resembling a carbon tax. In Experiment 1 ($N = 289$), a framed field experiment,[11] I elicit how participants' willingness to pay for typical student canteen meals changes when they are shown carbon labels. Taking this approach allows me to directly and precisely quantify the effect of carbon labels in terms of the carbon tax needed to produce an outcome equivalent to that produced by the labels. Experiment 2 is a natural field experiment for which I equipped one of the canteens

1. I use the term "carbon emissions' to refer to all greenhouse gas emissions. In my calculations, I convert gases other than CO_2 to CO_2 equivalents.

2. See Reisch et al. (2021) for an overview, as well as Ho and Page (2023), Lohmann et al. (2022), Bilén (2022), and Imai et al. (2022)

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

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

5. See e.g. Poore and Nemecek (2018) and Crippa et al. (2021). The largest contribution to this amount comes from agriculture and land use, while supply chain activities make up a smaller proportion. Clark et al. (2020) predict that even if fossil fuels were banned immediately, emissions from the global food system alone would make it impossible to limit warming to 1.5°C.

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

7. Dechezleprêtre et al. (2022) show this in global survey data. Further, Douenne and Fabre (2020) show considerable resistance in France to meat taxes, and Fesenfeld (2023) outline the political obstacles meat taxes face in Germany.

8. e.g. Feucht and Zander (2018) suggest consumers are even willing to pay a price premium when products are labeled in terms of their carbon emissions. See Yoeli et al. (2017) and Nisa et al. (2019) for a review of behavioral interventions to promote sustainable behavior. Implementing carbon labels instead of carbon taxes can also avoid some potential problems inherent to carbon taxes on meat, such as a slight tax regressivity (García-Muros et al., 2017; Funke et al., 2022).

9. Policymakers have committed to reducing greenhouse gas emissions (see, for example, the Paris Agreement (United Nations Framework Convention on Climate Change (UNFCCC), 2024) or, more recently, the European Climate Law of 2021, which makes climate neutrality by 2050 a legally binding commitment (European Commission, 2024).). The policy question is thus not whether, but how to reduce emissions.

10. Economists generally agree that carbon taxes or a carbon trading system are cost-effective methods of reducing emissions. See, for example, the European Association of Environmental and Resource Economists' statement on carbon pricing, European Association of Environmental and Resource Economists (2019). List et al. (2023) also consider it straight-forward to compare the effects of nudges with those of taxes.

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

of the University of Bonn with carbon labels for seven weeks (more than 125,000 purchase decisions by almost 10,000 customers). To maximize policy relevance, I designed the carbon labels to incorporate what has been identified as an effective combination in previous studies (Potter et al., 2021; Taufique et al., 2022), including both an ordinal (traffic light system) and a quantitative ranking (greenhouse gas emissions in kg). I analyze the effect of the labels in a difference-in-difference framework, and compare their effect to that of price changes resembling a carbon tax. I assess the latter using an extended data set of more than one year of student canteen data covering substantial price variation.

Findings are similar across both experiments: I estimate the carbon labels to have an effect similar to that of a carbon tax of €120 per tonne, and decrease carbon emissions by 4% relative to baseline. The effectiveness of the labels does not appear to diminish over the seven-week period in which they were installed in Experiment 2, and the treatment effects seem to remain at similar levels in the three weeks after the labels were removed and the canteen had not yet closed for the summer break.[12]

Would a carbon tax of €120 per tonne be considered a high tax or a low tax? A comparison to other policies suggests it is rather on the high end: €120 per tonne is about three times the current German carbon tax on gasoline. Moreover, the current carbon price in the EU ETS trading system is around €70 per tonne.[13] My results suggest that installing carbon labels might be more effective than extending either of these policies (given current price and tax levels) to the food sector, while also being less politically costly.[14]

Would a carbon tax of €120 per tonne be a sufficient policy to prevent climate change? One way to assess this is to compare the estimated magnitude with the social cost of carbon. However, considerable disagreement exists on the height of the social cost of carbon (see Desmet and Rossi-Hansberg, 2024, for a recent discussion), with estimates ranging from 50 USD (€49) per tonne and lower (some scenarios in Barrage and Nordhaus, 2024), €160 per tonne (e.g. Rennert et al., 2022), to substantially higher estimates (Bilal and Känzig, 2024; Moore et al., 2024). Depending on the true social cost of carbon, a €120 per tonne tax might be a sufficient stand-alone policy and might even "overcorrect" behavior (if the social cost of carbon of less than €120 per tonne), but if the true cost of carbon is higher than €120 per tonne, the carbon labels are not sufficient as a stand-alone policy to reduce emissions.

The question of whether the labels "overcorrect" behavior and potentially creates disproportional psychological costs for the consumer also depends on the mechanisms through which the labels influence consumers. One popular explanation is that carbon labels correct consumers' misperceptions of the carbon footprint of different items and this in turn affects their choices (Shewmake et al., 2015; Camilleri et al., 2019; Imai et al., 2022). I investigate the importance of this channel in an additional framed field experiment, Experiment 3 ($N = 444$). I compare how consumers react to seeing carbon labels on a product with their previous under- or overestimation of the product's carbon footprint. Participants react with a stronger decrease in demand if they previously underestimated emissions, providing some evidence that a correction of misperceptions plays a relevant role. However, a large part of participants' reaction to high-emission options is independent of previous under- or overestimation. The effect of the labels can thus only be partially explained by their ability to correct consumers' misperceptions of carbon footprints. Evidence from an additional treatment condition that directs attention without providing information suggests that a larger part of the labels' effect on consumers can be explained by them directing consumers' attention towards carbon emissions.

12. Experiment 1 was a one-shot setting and thus not suited to assess treatment effects over time or post-intervention effects.

13. Neither of these policies currently regulate the emissions produced by food consumption, and the carbon emissions produced by food consumption are currently not taxed in Germany.

14. The food sector is currently exempt from the EU-ETS trading scheme. See section 5 for evidence from Experiment 1 and 2 on the acceptance of carbon labels versus taxes.

To better understand the impact of the labels on consumer welfare, I additionally obtain a measure of participants' preferences towards the labels in all experiments: In Experiments 1 and 3, I collect an incentivized measure by making the display of the labels conditional on the preferences participants indicate. In Experiment 2, I conduct an unincentivized survey among canteen guests. In all experiments, less than 10% state a preference against the labels. I use consumers' elicited preferences towards the labels, together with observed treatment effects, to estimate a structural model of the effect of the labels on consumer welfare.

I mainly contribute to three strands of literature: First, I contribute to the literature on the effectiveness of carbon labels on food consumption. Previous studies have employed causal designs in a student canteen (Lohmann et al., 2022), a super market (Bilén, 2022), and in online food shopping (Imai et al., 2022; Ho and Page, 2023; Lohmann et al., 2024). Further studies have used correlational or event studies (Spaargaren et al., 2013; Vlaeminck, Jiang, and Vranken, 2014; Visschers and Siegrist, 2015; Brunner et al., 2018), hypothetical decisions (Osman and Thornton, 2019; Banerjee et al., 2023)[15], and lab decisions (Camilleri et al., 2019; Panzone et al., 2021).

While previous papers quantify effect sizes in terms of changes in consumer choices and emissions avoided, I quantify effect sizes directly in terms of how high of a carbon tax would be needed in the same context to achieve similar effects. This enables us to judge the effectiveness of carbon labels relative to other policies and the social cost of carbon.[16] Importantly, my experimental approach produces more meaningful results than simply taking the effect sizes identified by previous studies and translating these into equivalent carbon taxes using general price elasticities. Price elasticities differ across consumption settings and consumer groups (Lusk and Tonsor, 2016), and the emission savings that can be achieved with carbon taxes will also depend on context-specific factors such as the products on offer and their emissions. Notably, the same applies to carbon labels. Eliciting both the effectiveness of carbon labels and of carbon taxes in the same setting thus kills two birds with one stone: First, it yields a relevant comparison of the two policies. Second, it offers a means of interpreting effect sizes that predominantly removes mechanical context-specific factors.[17] This paper also connects to the literature examining the effect of a carbon tax of a single, fixed amount (Garnett et al., 2021; Lohmann et al., 2024).

Second, I contribute to the literature on the role of attentional biases in consumption decisions. The effectiveness of interventions that potentially direct consumers' attention has been shown in environmentally relevant contexts such as resource consumption (e.g. Allcott and Taubinsky, 2015; Tiefenbeck et al., 2018) and the purchase of environmentally durable goods (e.g. Rodemeier and Löschel, 2022). The evidence for attentional biases playing a role in food consumption decisions is merely suggestive (Lohmann et al., 2022; Ho and Page, 2023). In other work, carbon labels are conceptualized as a tool for correcting consumer misperceptions (Shewmake et al., 2015; Camilleri et al., 2019; Imai et al., 2022). While Shewmake et al. (2015) and Camilleri et al. (2019) do not test this idea empirically,Imai et al. (2022) perform an empirical test and do not find evidence for this mechanism.

I contribute to this literature by providing clear evidence in Experiment 3 that a correction of misperceptions is not the main factor driving the effectiveness of carbon labels. In this experiment, I elicit participants' prior beliefs of the carbon footprint of the different meals on which they make con-

15. Also see Rondoni and Grasso (2021) for a review.

16. Assessing the effectiveness of a policy by comparing it to alternative policies instead of considering a policy's effect sizes in isolation connects to the economic concept of opportunity costs and plays an important role in policy evaluation, (see e.g. Drummond et al., 2015; Weimer and Vining, 2017).

17. As an illustrative example of mechanical context-specific factors influencing the treatment effects identified for a carbon label, imagine a hypothetical canteen A that serves a very high-emission meat meal and a low-emission vegetarian meal, while canteen B serves a mid-emission meat meal and a low-emission vegetarian meal. Mechanically, it will be impossible for canteen B to achieve as high emission savings as canteen B can achieve. The same mechanical context-specific factors, however, impact the effectiveness of a carbon tax implemented in the same setting. By examining carbon label effectiveness in terms of the effectiveness of a carbon tax, we obtain an estimate that is independent of these factors and purely conveys the relative effectiveness of carbon labels as a policy tool in this setting.

sumption choices later in the experiment. This allows me to examine participant and meal-specific treatment effects depending on whether and by how much a participant had under- or overestimated the emissions of a meal. Combining this data with evidence from further treatment conditions (a condition directing attention without providing information, a condition using carbon offsetting, and a baseline condition), I structurally estimate a model describing the role of attentional biases and misperceptions as possible impediments to optimal decision-making. My paper provides a first quantification of the relevance of carbon labels in addressing each of these frictions.

Finally, I contribute to the literature on the consumer welfare impact of behavioral interventions. One strand of this literature derives consumer welfare from structural models or sufficient statistics (Chetty, 2009; DellaVigna, List, and Malmendier, 2012; DellaVigna et al., 2016; Rodemeier, 2021; Allcott et al., 2022; Goldin and Reck, 2022; Rodemeier and Löschel, 2022; Barahona, Otero, and Otero, 2023; List et al., 2023), while a second strand experimentally elicits consumers' willingness to pay to receive a behavioral intervention. Such an elicitation takes into account possible psychological costs and benefits arising to the consumer as a result of a change in consumption behavior induced by the intervention, as well as possible psychological costs and benefits arising independent of an impact on behavior (Allcott and Kessler, 2019; Thunström, 2019; Butera et al., 2022; Andor et al., 2023).[18]

I contribute to this third strand of literature by providing a first experimental estimate of the impact carbon labels have on consumer welfare, based on consumers' preferences for the presence of carbon labels elicited across all three experiments. Based on my theoretical framework and experimental data, I provide an estimate of the effect carbon labels have on consumer welfare and compare it to alternative interventions.

The rest of this paper is structured as follows. Section 2 describes how Experiment 1 quantifies the effectiveness of carbon labels using direct elicitation in a framed field experiment. Section 3 describes the design and results of Experiment 2, which is the natural field experiment corroborating my Experiment 1 estimate. Section 4 provides evidence on the behavioral channels driving treatment effects, drawing on data from Experiment 3. Section 5 discusses evidence from all three experiments concerning the psychological costs and benefits produced by the label. Section 6 then outlines a simple theoretical model describing the impact of carbon labels on consumers, which I structurally estimate using data from Experiment 3. Finally, Section 7 concludes.

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

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

2.1 Experimental design

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

18. Within my study context, one could think of such costs and benefits arising independent of a change in behavior as, for example, a change in feelings towards an unaffected choice or a change in the decision-making experience.

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

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

(3) I allocate participants to the LABEL or the CONTROL condition: Participants in the LABEL condition first indicate willingness to pay for four meals in the absence of carbon labels and shortly after indicate willingness to pay for the same four meals in the presence of carbon labels. Participants in the CONTROL condition make the same decisions but do not see any carbon labels in the second elicitation. Experiment participants are incentivized to indicate their true willingness to pay with a BDM mechanism, as detailed in section D.3.

(4) Willingness to pay for meals is elicited relative to an alternative lunch: In each of the 15 meal purchase decisions, participants first decide whether they prefer a given meal or a cheese sandwich.[19] 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.

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

(6) 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 with a BDM mechanism.[20] I discuss these results in Section 5.

Experiment timeline. The timeline of the online survey 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.[21] Second, participants indicate their baseline willingness to pay for four meals (four questions). Experiment participants are incentivized to report truthfully, as detailed in section D.3. Third, participants answer several incentivized and timed[22] guessing questions on unrelated issues (e.g. on the length of a popular running route in the city of Bonn). This is to create buffer time between the baseline

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

20. See section D.3 for details.

21. Any participant taking more than five attempts in doing so is excluded from the analysis, as pre-registered.

22. 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.

and second willingness to pay elicitation, in which all participants are again asked to indicate their willingness to pay for the four meals, but the decisions differ depending on the treatment condition they were assigned to by computer randomization:

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

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[23], with partly changed treatment conditions:

- Participants previously in the Label condition receive the Offset condition: Participants are informed that the emissions caused by their lunch choice (be it the meal or the sandwich) will be offset. As pre-registered, the Offset condition serves as a robustness check of the results of the Attention+Offset condition in Experiment 3, which is used as input for the structural estimation described in Section 6.2. This condition is thus not further described here, but details and results of the Offset condition are shown in section D.4 and in Tables B.7 B.8.
- Half of the participants previously in the Control condition receive 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.[24].

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.

Details on the meal purchasing decisions. Participants make a total of 15 meal-purchasing decisions in the course of the experiment (4 baseline, 4 first-round, 4 second-round, and 3 final decisions). The 12 first decisions revolve around the same 4 meals, and the final 3 decisions around 3 other, not previously seen meals. Participants who indicate that they are vegetarian are shown only vegetarian meals.[25] 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.[26]

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

23. 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 B.23.

24. This data is used for the analysis shown in Figure 12. 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.

25. Meals are detailed in Section D.2. Participants with stricter dietary requirements (vegan, gluten-intolerant, lactose-intolerant, or halal) are not permitted to participate in the experiment.

26. To ensure that results are not driven by a left-right effect, the left-right positioning of the two options is reversed in half of the experiment sessions. The order in which meals are shown is randomized.

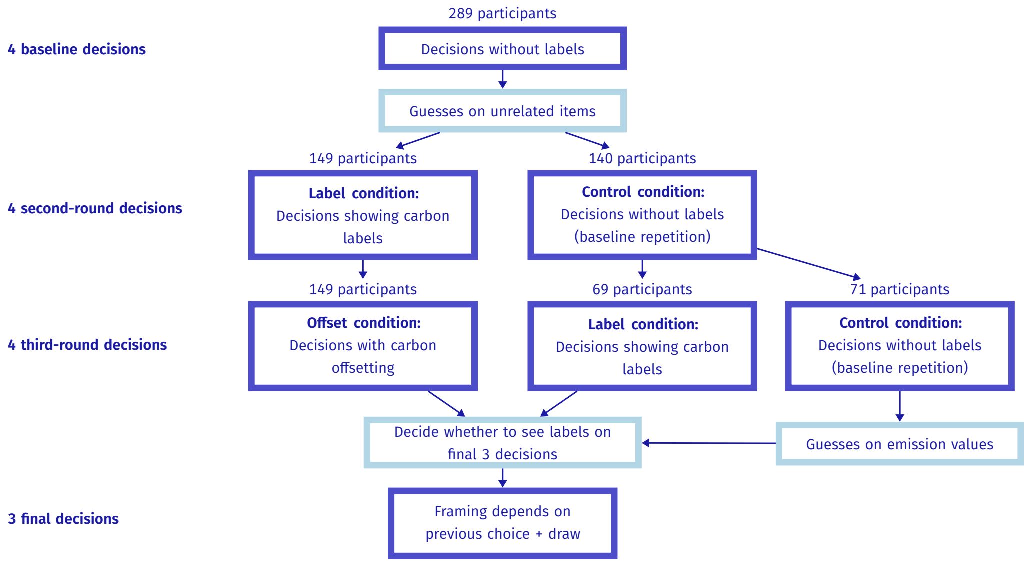


Figure 1. Experiment schedule and treatment groups

Note: Participants repeat the same four meal purchase decisions three times, with the decision framing differing across rounds. Treatments are described in more detail in the "Experiment timeline" paragraph above. The results of the OFFSET condition are not further discussed in this section, but details are described in section D.4, and results are shown in in Tables B.7 and B.8. As pre-registerd, the OFFSET condition serves as a robustness check of the results of the ATTENTION+OFFSET condition in Experiment 3, which is used as input for the structural estimation described in Section 6.2.

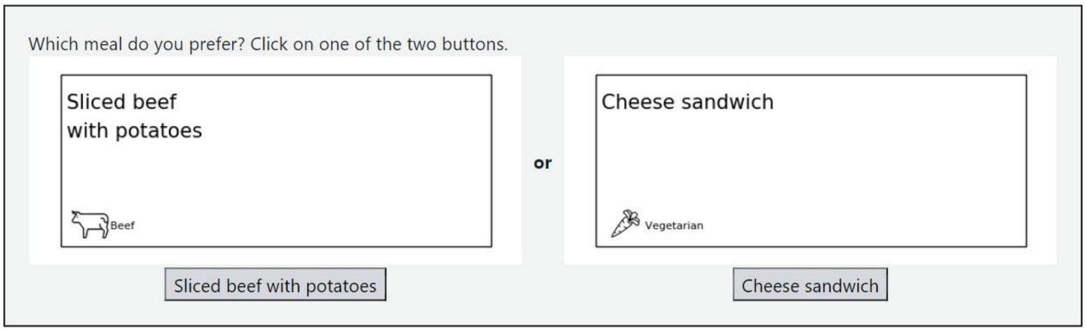


Figure 2. Meal purchase decision example step 1

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

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. [27]

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

27. 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 [28] was indicated in less than 3% of all observations. Figure B.1 shows the distribution of baseline willingness to pay values indicated.

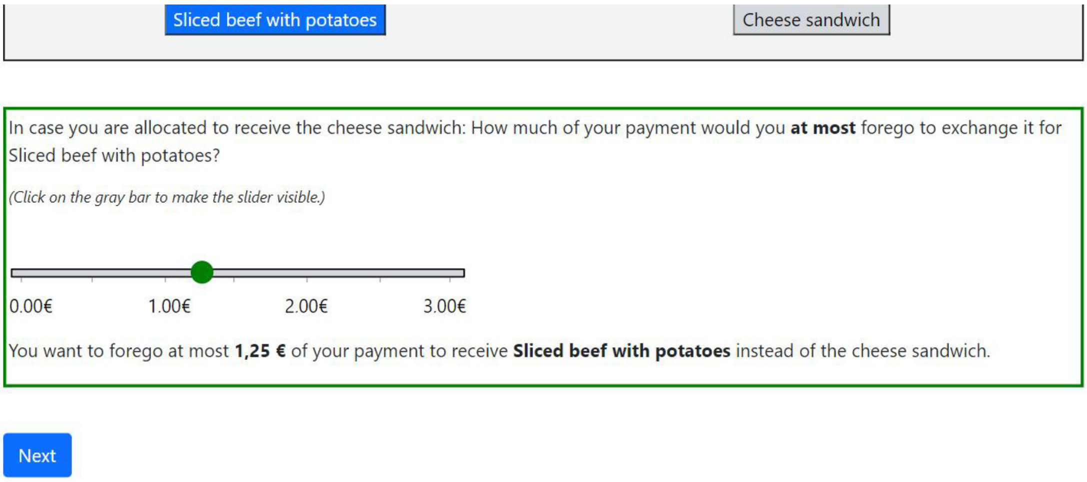


Figure 3. Meal purchase decision example step 2

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

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. Experiment participants are incentivized to report their true willingness to pay using a BDM mechanism, as detailed in section D.3.

Decision framing differs across treatment conditions. In the four baseline decisions, participants do not see any carbon labels but are merely shown the meal name and the meal's main ingredient (see Figure 2 for an example)[29]. 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.[30] For participants in the Control condition, there is no change in framing relative to the baseline decisions. For participants in the Offset condition, participants are told that the emissions caused by the meal will be offset with a donation to the non-profit carbon offseting service Atmosfair.[31]

Participants and set-up. 289 experiment participants are recruited from the participant pool of the BonnEconLab, the behavioral experimental lab of the University of Bonn, to participate in one of eight experimental sessions taking place between the 26th of October and the 5th of November 2021. I pre-registered the experiment design and the main outcomes shown in this section (Schulze Tilling, 2021b).[32] Participants are informed in the experiment invitation that vegetarian participants are permitted, but not participants with stricter dietary requirements (vegan, gluten-intolerant, lactose-intolerant, or halal). Participants are informed that the experiment will be conducted online, but that they are required to make their way to campus afterward to collect their payment in

29. I chose this display to reflect exactly how a meal would be displayed on the student canteen website, see Figure 8 for an example of implementation in the field.
30. 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.
31. The results of the Offset condition are not further discussed in this section, but details are described in section D.4, and results are shown in in Tables B.7 and B.8. As pre-registerd, the Offset condition serves as a robustness check of the results of the Attention+Offset condition in Experiment 3, which is used as input for the structural estimation described in Section 6.
32. See Tables B.7 and B.8 for all pre-registered main results.

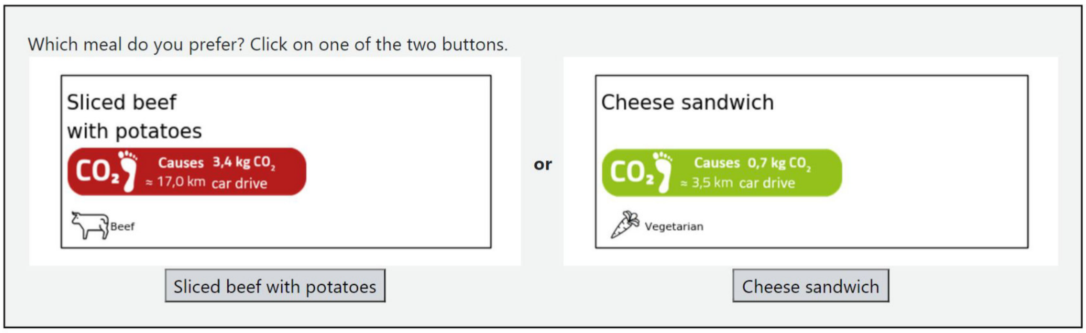


Figure 4. Meal purchase decision example: Decisions with labels

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

Figure 5. Gazebo set-up on University campus

Note: Set-up to provide participants with their payment in cash and a lunch corresponding to one of their choices. While completing the experiment, participants do not know which meal is payout-relevant.

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 in regular intervals offered by the student canteen, but they are not offered in the canteen on the particular experiment day, i.e. the student canteen prepared the meals only for my 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 5.

2.2 Estimation strategy

Participants' willingness to pay for a meal is likely influenced by a variety of factors (e.g. individual's tastes, hunger level or mood). To cleanly isolate the effect of seeing the labels, I examine the **change** in willingness to pay of a certain individual for a certain meal as the outcome variable in the causal analysis: Instead of directly examining an individual's willingness to pay for a meal in the Label or Control condition, I subtract the individual's baseline willingness to pay for the same meal from this amount, and then examine the remaining difference. This is the change in willingness to pay occurring due to being shown carbon labels (the Label condition) or due to merely being asked for willingness to pay a repeated time (the Control condition). One can also interpret the outcome variable as denoting individual- and meal-specific within-subject treatment effects, which I compare between treatment groups. An alternative approach would be to use willingness to pay as the dependent variable and include a fixed effect for every individual-specific meal choice. This approach yields similar results, as shown in Section B.9.

My basic specification is:

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

where Δ_{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, where willingness to pay is always expressed relative to the cheese sandwich.

$High_m$ is an indicator of whether the meal causes higher emissions than the sandwich.[33] 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.

$(Label_{ij} \times High_m)$ interacts the high-emission indicator with an indicator for whether individual i saw carbon labels in round j. This describes the average causal effect of carbon labels on willingness to pay for a meal that is high in carbon emissions. $(Label_{ij} \times Low_m)$ describes the average causal effect of carbon labels on willingness to pay for a meal that is low in carbon emissions. $ThirdRound_j$ is an indicator of whether it was the third round of decisions.[34]

2.3 Data and results

I exclude the 3% fastest participants as well as participants not passing the comprehension check after five attempts, as pre-registered.[35]. The remaining 289 experiment participants are computer-randomized into treatments. Section B.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 B.2, and results hold when restricting the sample to only students or only non-vegetarians, as shown in Section B.7. Section B.3 shows the baseline distribution of relative willingness to pay for meals. 22% of willingness to pay values are 0, indicating indifference between the warm meal and the cheese sandwich. 17% are negative, indicating a preference for the cheese sandwich. The remaining values are positive, with some bunching around the value of 1 €. Less than 4% of indicated willingness to pay values are at the boundaries of the -€3 to €3 interval.

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 €0.14 on average due to the labels. For meals with higher emissions than the cheese sandwich, willingness to pay decreases by €0.31 due to the labels. Changes in willingness to pay for participants in the Control condition are not significant, and, coefficient-wise, move in opposite directions. Thus, the mere act of asking participants for their willingness to pay multiple times does not seem to significantly impact their willingness to pay. Figure 6 illustrates effects by showing average changes in willingness to pay for the Control and Label groups, for low-emission and high-emission meals.

Specification (2) in Table 1 does not group the four meals into low-emission and high-emission meals but instead regresses the change in willingness to pay on the difference in emissions between the warm meal and cheese sandwich. I estimate the following equation, with Emi_m representing the difference in emissions: $\Delta WTP_{ijm} = \beta_1 Emi_m + \delta_1 (Label_{ij} \times Emi_m) + ThirdRound_j + \varepsilon_{ijm}$

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. Conversely,

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

34. An alternative approach to controlling for possible third-round effects is excluding third-round decisions entirely. This yields similar results (Table B.23).

35. See Schulze Tilling (2021a) Dohmen and Jagelka (2023) find that fast respondents are more likely to not pay attention and give random answers.

this implies that a change in pricing causing demand to shift in the same way — i.e. a €120 per tonne carbon tax[36] — produces similar effects as the carbon labels. Figure 7 visualizes the similarity in the effectiveness of these two policy interventions, i.e. carbon labels and a €120 per tonne tax. I construct demand curves using the valuations participants indicate in the experiment. The two graphs in the left column show how the demand curve for low-emission meals slightly shifts upwards and the demand curve for high-emission meals shifts downwards when experiment participants in the LABEL condition are shown carbon labels. The graphs in the right column show, in comparison, how the baseline demand curve shifts in a similar manner upon the introduction of a carbon tax of €120 per tonne.[37] The comparison between the two - label and tax - visualizes how the impact of carbon labels can be conceptualized as a shift in the demand curve, similar to how a carbon tax will impact demand.[38] Using the experiment data, I simulate how experiment participants would make choices in the student canteen with and without labels, and estimate that the labels would decrease emissions by 4.8%. Details are described in section B.4.

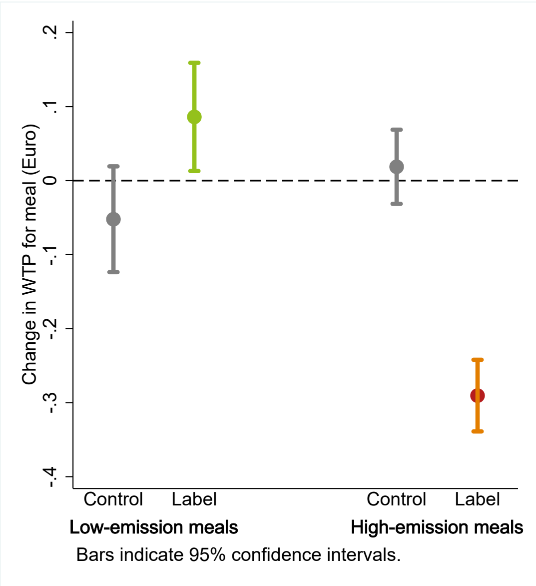


Figure 6. Within-subject change in willingness to pay for meals. Note: Effects are differentiated between participants in the CONTROL and LABEL condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese 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	0.02	0.02
	(0.03)	(0.03)
Constant		−0.02
		(0.02)
Participants control	139	139
Participants treated	217	217
Observations	1,704	1,704

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

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

36. Note that I am referring to a tax that is simply added to a product's price and included in the final posted price without being saliently displayed. Value added taxes are usually levied in European countries in this manner, the German carbon tax on petrol is levied in the same way, and the EU-ETS trading scheme affects prices in a similar manner. There is thus no additional behavioral salience factor to consider in such a taxation policy.
37. I construct these by deducting a carbon tax of €120 per tonne from the baseline willingness to pay participants indicate. Since WTP is measured relative to a cheese sandwich, the carbon tax is also computed relative to the cheese sandwich, i.e. it is negative for low-emission items.
38. See section B.10 for a more detailed explanation of the intuition behind this comparison.

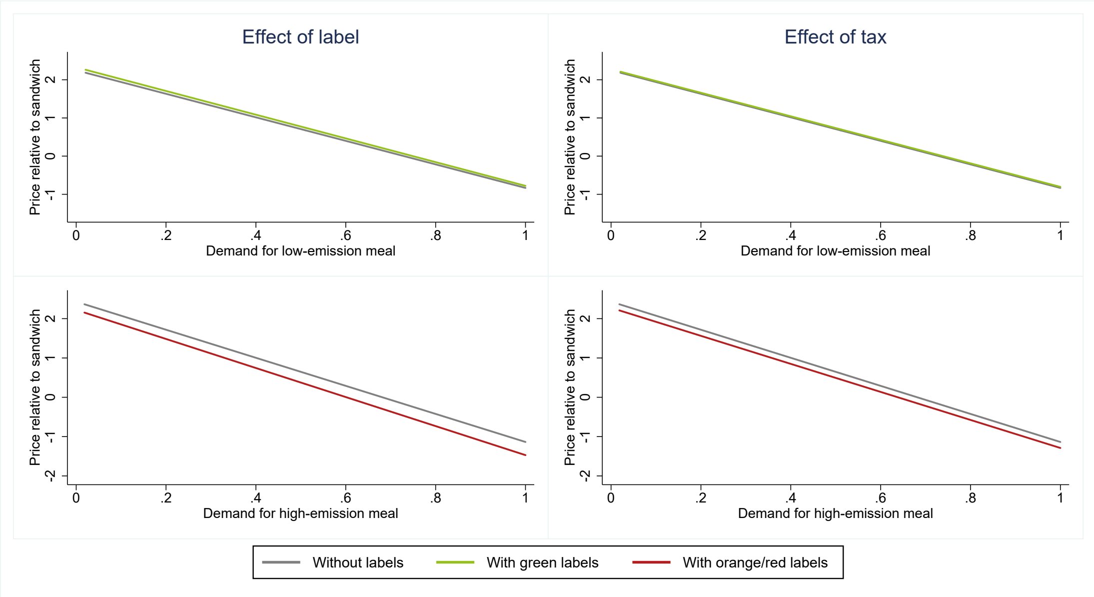


Figure 7. Demand curve shifts with labels vs. a carbon tax

Note: Demand curves for low-emission meals (top, N=265) and high-emission meals (bottom, N=603) estimated using data from participants in the Label condition. The gray lines plot a line based on the WTP participants indicated at baseline, and the green and red lines plot a curve based on WTP indicated with carbon labels (two graphs on the left) and net WTP after a carbon tax of €120 per ton is deducted (two graphs on the right).

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

While Experiment 1 directly quantifies the effect of carbon labels relative to a carbon tax in a one-shot consumption setting, Experiment 2 provides a second independent estimate based on consumption behavior observed over a longer time period in Bonn's student canteens. Subsection 3.1 describes how I asses the impact of carbon labels in this setting. Subsection 3.2 describes how I asses the impact of a price change in this setting, and compares effect sizes to those of carbon labels.

3.1 The effect of labels

3.1.1 Experimental design. To identify the causal effect of carbon labels in the field, I make use of the fact that there are multiple student canteens in Bonn that centralize their meal planning, i.e. on a given day roughly the same meals are offered in all canteens. I summarize the most important details below and describe the student canteen setting in Bonn more in detail in Section E. I preregistered the experiment design and main outcomes.[39]

(1) I use a difference-in-difference design: 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).

39. See AsPredicted#95108. I preregistered the experiment design and my main outcomes: meat/vegetarian consumption, consumption of green-labeled meals, greenhouse gas emissions and canteen visits during and after the intervention period, which I planned to examine in the full sample and including only purchases made with individual student canteen cards. The full set of preregistered main analyses is shown in section C.2.

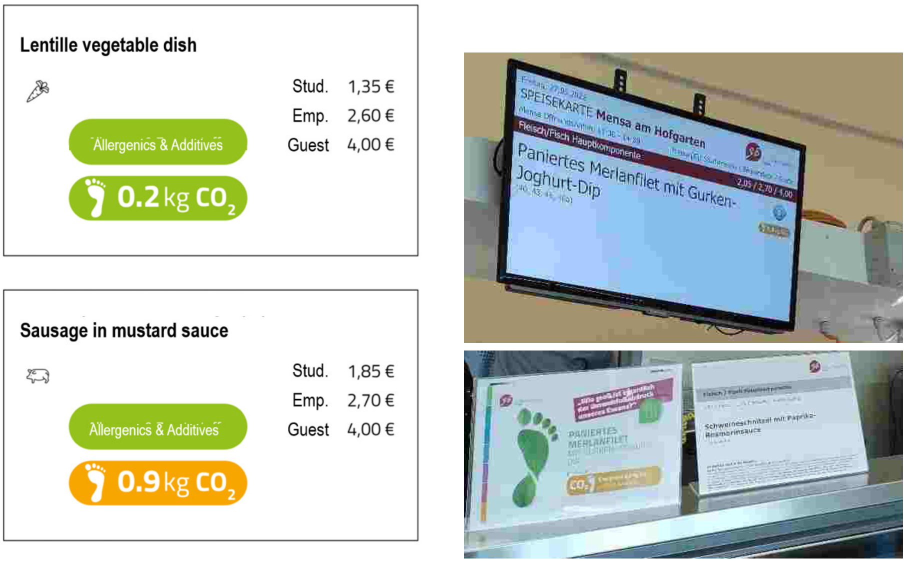


Figure 8. Labels in the canteen

Note: Labels online (left, menu translated from German) and in the student canteen (right)

(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 signs on top of the meal counters. Examples are shown in Figure 8. Emissions are 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 canteen, but not for sides and desserts, for ease of implementation and interpretability (see Appendix E 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 guest's total lunchtime consumption. The two main meal components on offer always consist of one vegetarian and one meat-based component, which is higher in carbon emissions than the vegetarian option.

(4) I accompany the natural field experiment with a pre-intervention (N>1,700) and post-intervention survey (N> 900) in the field. These capture students' demographic characteristics (connectable to canteen purchasing data) and opinions on the carbon labels. These surveys are described in more detail in Section E.8.

3.1.2 Estimation strategy. To estimate the causal effect of carbon labels in the student canteen, I use a difference-in-difference estimation using choice of emission-heavy main meal component as the outcome variable. This allows me to control for differences in canteen consumption behavior at baseline, as well as for time trends common between the two canteens.

My most basic difference-in-difference specification is:

$$\begin{aligned} Meat_{it} = \alpha + \beta_1 LabelPeriod_t + \beta_2 PostPeriod_t + \gamma Treat_{it} + \\ + \delta_1 (Treat_{it} \times LabelPeriod_t) + \delta_2 (Treat_{it} \times PostPeriod_t) + \epsilon_{it} \quad (2) \end{aligned}$$

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 higher-emission meat-based main meal component is purchased, and 0 if the lower-emission vegetarian main meal component is purchased. $LabelPeriod_t$ is an indicator of whether this purchase occurred during the seven-week intervention period, 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. $Treat_{it}$ is an indicator of whether the purchase occurred in the treatment canteen. ($Treat_{it} \times LabelPeriod_t$) is the variable of interest identifying the difference-in-difference estimate of any change in purchasing behavior occurring during the labeling period in the treated canteen relative to the control canteens. ($Treat_{it} \times PostPeriod_t$) identifies possible post-intervention effects.

Depending on the specification, I include more granular time controls, as well as controls for variations in canteen offer. My preferred specification assigns intention to treat on the guest level, and estimates ITT effects with guest fixed effects to take into account guests possibly changing the way in which they frequent canteens.[40]

3.1.3 Data and results. I include purchase data from April 4th 2022 (beginning of the semester) to July 8th 2022 (end of the semester) in my analysis. I drop data from seven days on which the treatment and the larger control canteen did not offer the same main meal components. I also drop all consumption of Ukrainian refugees, who received free meals in the student canteens from week 9 of the sample period. For my main analysis, I additionally drop data from the first week of the label period (week 5), since a "Healthy Campus" week occurred simultaneously and it is not clear whether the carbon labels or this event are driving the increased vegetarian consumption identified for week 5 in the event plot shown in Figure 9.[41] Section E.5 details these exclusions and shows that the main results are robust to changing exclusion criteria. The final full sample includes 121,371 observations, made by almost 10,000 guests. 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, 69% of sales are made with a personalized payment card, allowing me to track individuals across time.

Using those sales made with a personalized payment card, I construct an intent-to-treat sample, which I further restrict to canteen guests visiting the student canteen regularly pre-intervention (at least five times within four weeks), and pre-dominantly visiting the same canteen pre-intervention (at least 80% of pre-intervention visits to the same canteen). This allows me to classify guests as "intent to treat" based soley on their pre-intervention consumption behavior. Section E.5 further discusses these restrictions and shows that results are robust to using different threshold values.

Figure C.1 shows that the total sales of the treatment and control canteens develop fairly similarly throughout the entire 14-week period. Further, the ITT event plot in Figure 9 shows fairly similar pre-trends.

Table 2 shows regression results. Column (1) estimates the basic regression specification shown in equation 2. The following specifications gradually add more granular controls: Column (2) exchanges the "Label period" and "Post period" indicators for weekly controls for time trends, as well as controls for day-of-the-week. This allows for a more granular control of time trends (e.g. semester times, seasonal trends). Column (3) adds controls for whether a second vegetarian/ meat main meal

40. This is not very common, as canteens are located over 1.7 km (1.1 miles) apart. Section E discusses possible switching in detail, using pre-intervention individual-level purchase data to identify a guest's "home" canteen and then tracking "non-home" visits throughout the period. There is no clear time trend in switching attributable to the labels, and the proportion of meat purchases made by switchers does not increase throughout the period, which makes an intervention-motivated switching from treatment to control canteen seem unlikely. Nevertheless, spec. (5) in Table 2 controls for any such change in canteen frequenting behavior by assigning treatment as intent-to-treat (controlling for any changes in guests frequenting the treatment canteen specifically) and including individual fixed effects (controlling for any changes in guests frequenting the canteens in general).

41. The "Healthy Campus" week affected the treatment and the control canteens and should thus not have produced a differential effect for the treatment canteen, provided it affected all canteens in the same manner. To be conservative in my analysis, I exclude it from the main analysis nevertheless.

components was offered in the respective canteen. Columns (4) and (5) analyze the ITT sample. Estimating intent-to-treat effects and controlling for guest fixed effects ensures that effect sizes are not influenced by any potential changes in the frequency or way canteen guests frequent the canteens. [42] Col. (5) additionally includes date-specific time controls. This allows to not only control for time trends common between the canteens (as with the weekly controls), but also implicitly controls for the changing offer in the canteens, as the offer of main meal components is largely centralized across canteens.

The final column estimates that the labels caused a decrease in meat consumption of three percentage points during the labeling period. This is 6% of the ITT's treatment group baseline meat consumption. The effect on meat consumption in the three weeks following the intervention period - before the canteens went into summer break - is estimated at 4 percentage points, or 8% of baseline consumption. Figure 9 shows an event plot of estimated effect sizes.

To assess whether the strong post-intervention effects last, Figure C.2 includes data from the semester following the intervention (Oct. 22–Jan. 23) in the event plot. There is no evidence of lasting post-intervention effects, and the time trends suggest—if at all—an upwards-sloping pattern.[43] Post-intervention effects thus seem rather short-lived, in line with the attention-habit model described in Byrne et al. (2024): 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.

In the pre-intervention phase, the emissions of an average meat meal were 2.2 kg in the treated canteen, while those of an average vegetarian meal were 0.4 kg. Thus, a back-of-the envelope calculation (1.7 x 0.03) yields that, without any change in the meals on offer, the intervention would have led to reductions in greenhouse gas emissions of 51 gram or 4% of average baseline emissions (1.2 kg). However, the meals on offer in the canteens did change significantly between the pre-intervention and intervention period, leading to mechanical changes in the emissions of the average meal consumed. As section C.3 details, a simple difference-in-difference analysis using greenhouse gas emissions as the outcome variable would mistakenly pick up these changes caused by changes in offer and attribute them to the carbon labels. I thus perform the difference-in-difference analysis using greenhouse gas emissions as the outcome variable, but additionally controlling for the emissions of the meals on offer on a given day. As Table C.4 shows, I then estimate a treatment effect of 50 gram per meal on the ITT sample. As an additional check, I perform an alternative analysis that does not include control variables but instead restricts the sample in such a way that pre-intervention and intervention offer is identical (section C.3).

Section C.4 examines heterogeneity in treatment effects, and section C.5 draws on survey data to provide suggestive evidence on how the carbon labels influenced canteen guests in the field (e.g. visibility of the labels, effect of the labels on other attitudes).

42. Specifically, I fix a value of the "ITT guest" 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, "ITT guest" is set to 1, while it is set to 0 for individuals mainly going to the control restaurants during the pre-intervention period. Estimates are thus not affected by guests potentially changing their canteen frequenting behaviors during or after the intervention period. Control variables are also specific to the intent-to-treat canteen.

43. Unfortunately, I cannot track individuals in my main data set in the data set in the subsequent period, and thus cannot restrict the subsequent data set to purchases of guests who were already frequenting the student canteen in the prior semester. However, the upwards-sloping pattern makes it seem unlikely that treatment effects in fact persisted among the canteen guests who visited the student canteen in May 2022, and that the null effects are entirely attributable to incoming new and never-treated students.

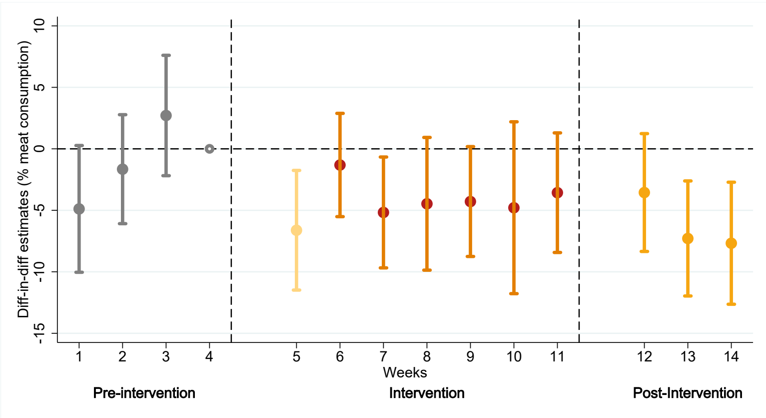


Figure 9. Event study: Difference in difference estimates

Note: Difference in difference estimates of the likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1–4 constitute the pre-intervention phase, while weeks 5–11 constitute the intervention phase, and weeks 12–14 the post-intervention phase. The regression specification follows specification (4) in Table 2, but estimates weekly effects and controls for weekly time trends, as detailed in regression table C.1. Week (5) is excluded from the main estimation in Table 2, because effects cannot be clearly attributed to the carbon labels, as described more in detail in Appendix E. Bars indicate 95% confidence intervals.

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

	Full sample			ITT sample	
	Base	Week FE	+Controls	+Guest FE	+Date FE
Treatment restaurant x Label period	-0.02***	-0.02***	-0.02***		
	(0.01)	(0.01)	(0.01)		
Treatment restaurant x Post period	-0.06***	-0.06***	-0.06***		
	(0.01)	(0.01)	(0.01)		
ITT guest × Label period				-0.03***	-0.03**
				(0.01)	(0.01)
ITT guest × Post period				-0.06***	-0.04***
				(0.01)	(0.02)
Treatment restaurant	-0.10***	-0.10***	-0.07***		
	(0.01)	(0.01)	(0.01)		
Label period	0.01**				
	(0.00)				
Post period	-0.00				
	(0.00)				
Canteen control for second veg. offered			-0.02***		
			(0.00)		
Canteen control for second meat offered			0.02***		
			(0.00)		
ITT control for second veg. offered				-0.01	-0.03***
				(0.01)	(0.01)
ITT control for second meat offered				0.02***	-0.00
				(0.01)	(0.01)
Constant	0.51***	0.49***	0.47***	0.49***	0.46***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Week fixed effects	No	Yes	Yes	Yes	Yes
Guest fixed effects	No	No	No	Yes	Yes
Guests control	6,909	6,909	6,909	1,021	1,021
Guests treated	2,840	2,840	2,840	342	342
Observations	121,371	121,371	121,371	27,640	27,640

Standard errors in parentheses

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

Notes: Note: Dependent variable: 0/1 indicator for consumption of the meat option. Col.(1) corresponds to Equation 2. The constant term describes the proportion of meat meals sold in the control canteens pre-intervention. Specifications (2)–(5) include weekly controls and day-of-the-week controls to control for time trends. The "Post period" and "Label period" indicators are thus dropped due to collinearity. Specifications (3)–(5) add controls for whether a second vegetarian/ meat main component is offered in the respective canteen, and the price difference between the two components. Spec. (4) and (5) restrict the sample to canteen guests paying with their individual payment card, visiting the student canteen regularly pre-intervention, and pre-dominantly visiting the same canteen pre-intervention (see main text and section E.5 for details). Both specifications are ITT, and the "Treatment restaurant" indicator is thus dropped. Specification (4) includes individual fixed effects, and specification (5) additionally includes daily time controls. The standard errors of Col.(1)-(3) are robust. The standard errors of Col.(4)-(5) are clustered at the individual level.

3.2 The effect of a meat tax

3.2.1 Setting. To understand how the effect I observe for carbon labels in the student canteen compares to how demand would change with a carbon tax, I examine how canteen guests react to pricing variations in the canteen. In the experiment context, I consider the price difference between the meat and vegetarian option to be the relevant price figure driving the composition of meat and vegetarian purchases.[44] In a year of student canteen data, I observe ample variation in this price difference. This is the case for two reasons:

(1) There are price differences between the different meat (vegetarian) main meal components on offer, with prices ranging between 1.85 and 2.5 (1.35 and 2.4). Since specific vegetarian and meat main meal components are paired differently with each other across days, the price difference between the vegetarian and meat choice varies across days.
(2) The student canteen implemented a price increase in October 2022, which not only increased the general price level but also the price differential between the meat and the vegetarian main meal components. While this difference was on average around €0.33 from April to June 2022 (around 20% of the price of a veg. main meal component sold then), it increased to around €0.50 from October to December 2023(around 25% of the price of a veg. main meal component sold then) and remained at this higher level.

Using these price variations, I estimate the effect of a meat tax in the student canteens, using this as a proxy for a carbon tax. In the context of the student canteens in Bonn, this approximation is acceptable, since the student canteens usually offer only one meat and one vegetarian option, and the meat option is generally higher in emissions than the vegetarian option.[45] The average emissions of the vegetarian option sold during the period April to July 2023 were 0.41 kg per meal, versus 1.62 kg per meal for the meat option. A €0.10 tax on the meat option would thus, on average, tax for an additional 1.2 kg of emissions caused. This implies a tax of €80 per metric tonne.[46]

3.2.2 Estimation strategy. I regress guests' decision of whether to purchase the meat or vegetarian main meal component on the price difference between the two, while controlling for each option's attrativeness and time trends:

$$Meat_{ctp} = \alpha + \beta_1 \Delta Price_{ctp} + X_{ct} + \tau + \epsilon_{ptm} \quad (3)$$

The variable $Meat_{ctp}$ is a binary outcome describing whether in canteen c on day t, purchase p is a meat-based main meal component, i.e. $Meat_{ctp}$ equals 1 if the higher-emission meat-based main meal component is purchased, and 0 if the lower-emission vegetarian main meal component is purchased in purchase p. $\Delta Price_{ctp}$ describes the price difference in Euro between the main meal component m and the alternative meal component offered (i.e. if meal m is vegetarian, I take the price difference to the standard meat meal component offered in the canteen on that day, and if meal m contains meat, I take the price difference to the standard vegetarian meal component offered on that day.)[47] X_t controls for the attractiveness of the

Naturally, this analysis is correlational, since the variation in pricing is not purely exogenous. Specifically, the reasons why one meal is priced differently from another will likely correlate with

44. Section E describes the student canteen context more in detail.
45. In the time period for which I also have emissions data (April to July 2023), the meat option was always higher in emissions than the vegetarian option offered on a given day.
46. This is just a different way of expressing the €0.10 tax for 1.2 kg of emissions policy. The calculation is: €0.10 tax for 1.2 kg implies €0.08 tax for 1 kg. Multiplying both sides by 1000, we get €80 tax per metric tonne.
47. 94% of purchases are purchases of either the standard meat or the standard vegetarian main meal component. The remaining 6% are purchases of main meal components that are offered in addition, e.g. because there are still leftovers from the day before.

factors also determining a meal's popularity. I thus add a variety of control variables across specifications. The remaining variation could arguably be described as fairly exogenous - while a meal's price itself is not random, it is fairly random with which meal it is paired for the canteen's daily offer. Specifically, the same meat meal may sometimes be offered together with a cheap, and sometimes with a more expensive vegetarian option, changing the price difference a price-conscious consumer might consider. I use this variation to identify the effect of price fluctuations on demand.

3.2.3 Data and results. For the purpose of this analysis, I use student canteen consumption data ranging from April 2022 to March 2023[48], but only include purchases made by students in the analysis since employees and external guests pay higher prices.

Table 3 estimates specification 3 including different controls. Col. (1) estimates equation 3 without any controls, while Col. (2) includes over 100 binary meal-specific controls to control for the attractiveness of the two main meal components on offer (94% of purchases are of one of these two main options). The coefficient estimated in Col. (2) is over six-fold of that estimated in Col. (1), supporting the intuition that higher priced meals are typically also perceived as more attractive, confounding estimates in Col. (1). Apart from the two standard meals, there are sometimes additional meals on offer in the canteens, e.g. because there are still left-overs from the previous day. 6% of the purchases in my sample pertain to purchases of such additional meals. Col. (3) thus adds further meal-specific controls for these additional meals on offer. Col. (4) additionally controls for possible time effects by including week and day of the week controls, and Col. (5) controls for the effects of the labeling intervention in the treatment canteen. [49] I do not additionally include controls for "Label period" and "Post period" since these are picked up by the week effects. The coefficients estimated across Col. (2) to (5) are similar.

Using Col. (5) as my preferred specification, I find that a €0.1 increase in the price difference between vegetarian and meat main components (carbon tax of €80 per tonne) correlates with a 2 percentage point decrease in demand for the meat main component and a corresponding increase in demand for the vegetarian main component. Extrapolating from this estimate and assuming a linear effect, one can approximate that a carbon tax of €40 per tonne would correlate with a 1 percentage point decrease, and a carbon tax of €120 per tonne with a 3 percentage point decrease. In the previous section, I estimated the effect of carbon labels to also be a 3 percentage point decrease in meat consumption. This comparison thus corroborates the result from section 2 of carbon labels producing a similar effect in this setting as a carbon tax of €120 per tonne.

To provide further context to my results and compare with previous literature, I calculate price elasticities: €0.1 is around 4.3% of the meat meal price (€2.3 averaged over the 12 months time frame), and 2 percentage points is around 4.5% of average demand for the meat meal (44%). This would imply an own-price elasticity of around -1 for the meat meal. This estimate is similar to, but slightly higher than the price elasticity Roosen, Staudigel, and Rahbauer (2022) estimate for all German households (approx. -0.9). Further, Roosen, Staudigel, and Rahbauer (2022) predict that a 6.5% increase in the meat price in Germany would lead to an average household reducing their

48. I have access to this larger data set since the student canteen provided me with data twice: Once with data from April 2022 to July 2022 (my main data set for this paper), and again with data from August 2022 to July 2023 (my main data set for Klatt and Schulze-Tilling, 2024). I thus extend the pricing analysis to March 2023, but do not include data from April 2023 to July 2023 since the student canteens implemented another intervention in this time frame (analyzed in Klatt and Schulze-Tilling, 2024). Unfortunately, I am not able to link individual student canteen guests across the two data sets. The student canteen provided me with the recipes to the meals they served from April 2022 to July 2022 only, and I thus only have emissions data for the meals served during this period.

49. Note that I include these variables as controls and not as a means to identify the effect of the labels based on this regression. I interpret Table 2 as the main result, since this is the analysis I pre-registered. Interpreting and comparing the coefficients reported in Table 3 directly would hinge on the assumption that trends from August 2022 to July 2023 are parallel (This is not unreasonable, see Figure C.2). Comparing coefficients would then suggest that the labels have an effect similar to a carbon tax of €240 per tonne (as the effect of the labels is roughly double that of a tax of €120 per tonne.

emissions by 3.3 kg per month. A back-of-the-envelope calculation reveals that this is in a similar ballpark to my effect sizes.[50]

Importantly, Roosen, Staudigel, and Rahbauer (2022) emphasize that price elasticities differ across demographic groups, and estimate lower price elasticities for lower-income and younger individuals, i.e. characteristics which both apply to my sample. My estimates are thus on the higher end compared to the behavior one might expect from this population segment. One evident reason is that Roosen, Staudigel, and Rahbauer (2022) use supermarket data, while I observe individuals buying lunch in the student canteen. In the latter context, switching to the vegetarian option is, in a sense, effortless: It simply requires walking up to a different counter. In a supermarket context, the situation is more complex: Switching to a vegetarian meal in response to an increase in the meat price requires thinking of a different recipe than one originally had in mind, and perhaps exchanging some of the items already in the shopping basket, finding the new ingredients, etc. A more natural response to an increase in meat price is thus to stick to the originally planned recipe, but merely reduce the amount of meat used, or, as Roosen, Staudigel, and Rahbauer (2022) find, switching to meat mixtures. This highlights the benefit of observing the price elasticities specific to the consumption context, rather than only general price elasticities.

Table 3. Comparison of effects: labels vs. "carbon tax"

	Likelihood of consuming meat				
	(1)	(2)	(3)	(4)	(5)
Price difference (in Euro)	-0.03***	-0.19***	-0.18***	-0.20***	-0.20***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Treatment restaurant x Label period					-0.04***
					(0.01)
Treatment restaurant x Post period					-0.02***
					(0.00)
Treatment restaurant					-0.03***
					(0.00)
Constant	0.45***	0.62***	0.63***	0.59***	0.56***
	(0.00)	(0.03)	(0.04)	(0.05)	(0.05)
Meal-specific controls (stand.)	No	Yes	Yes	Yes	Yes
Meal-specific controls (add.)	No	No	Yes	Yes	Yes
Week and Day of the week controls	No	No	No	Yes	Yes
Observations	381,092	381,092	381,092	381,092	381,092

Standard errors in parentheses

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

Notes: Dependent variable: 0/1 indicator for consumption of the meat option. Linear probability regression drawing on student canteen data from April 2022–March 2023. The variable "Price difference" describes the price difference between the main meal component purchased and the standard alternative option, e.g. if a meat option is purchased, the price difference to the main vegetarian component on offer is calculated, and vice versa if a vegetarian meal option is purchased. Standard errors are robust.

50. Specifically, I approximate that a 4.3% increase in the meat price leads to a (1.7 x 0.02) 34 gram reduction in average meal emissions, with 1.7 being the average difference in emissions between meat and vegetarian option and 0.02 being the reaction in demand identified in my analysis. Multiplying by 31 days in a month and a 2 person household, I calculate emission savings of 34 x 31 x 2 = 2.1 kg emission savings per month.

4 Experiment 3: Behavioral channels

Why do consumers react to carbon labels? Experiment 3 provides reduced-form evidence, focusing on the ability of carbon labels in making consumers (1) informed about emissions, and (2) attentive towards emissions at the moment of choice. Subsection 4.1 describes the experimental design. Subsection 4.3 describes data and results.

4.1 Experimental design

Overview. I investigate two channels that might drive the effectiveness of carbon labels, based on previous literature:[51] (1) Labels inform consumers about the emissions caused by different items, and thus correct possible misperceptions. (2) Labels make consumers think about emissions at the moment of choice, and thus direct their attention. To investigate the relevance of each of the two channels in driving participants' consumption reactions, I conduct a framed field experiment similar to Experiment 1 apart from two key differences:

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

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

Experiment timeline. The experiment timeline is visualized in Figure 10. It proceeds very similarly to Experiment 1, with one key difference: Recall that in Experiment 1, experiment participants answer guessing questions on unrelated items after completing the four baseline purchase decisions (e.g. on the length of a popular running route in Bonn). In contrast, Experiment 3 participants do not answer these questions, but instead, guess the carbon footprints of different meals. These questions concern the four meals around which the meal purchasing decisions revolve, as well as six further meals (see Figure 12 for a list). To give participants a reference point for their guesses, they are informed about the emissions of a single example meal (Red Thai Curry with pork and rice, causes 1.7 kg of CO_2). An example of a guessing screen is shown in Figure 11. The guessing questions are incentivized and timed as in Experiment 1.[52]

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

- In the Attention condition, the willingness to pay elicitation is exactly as in the first, baseline elicitation. However, since participants completed the carbon footprint guessing task between the two elicitations, they have now spent time thinking about the issue of greenhouse gas emissions, and are thus arguably more attentive towards carbon emissions in their consumption choice.
- In the Attention+Label condition participants are additionally shown carbon labels when indicating their willingness to pay. An example is shown in Figure 4. They are thus attentive and informed.

51. See the introduction for a more thorough motivation.

52. Participants answer each of the ten guessing decisions on separate screens, shown to participants in a random order. On each screen, they are shown the emissions of the same example meal, Red Thai Curry with rice. This reference meal is not included in any willingness to pay elicitations. Section D.5 shows screenshots of the guessing instructions.

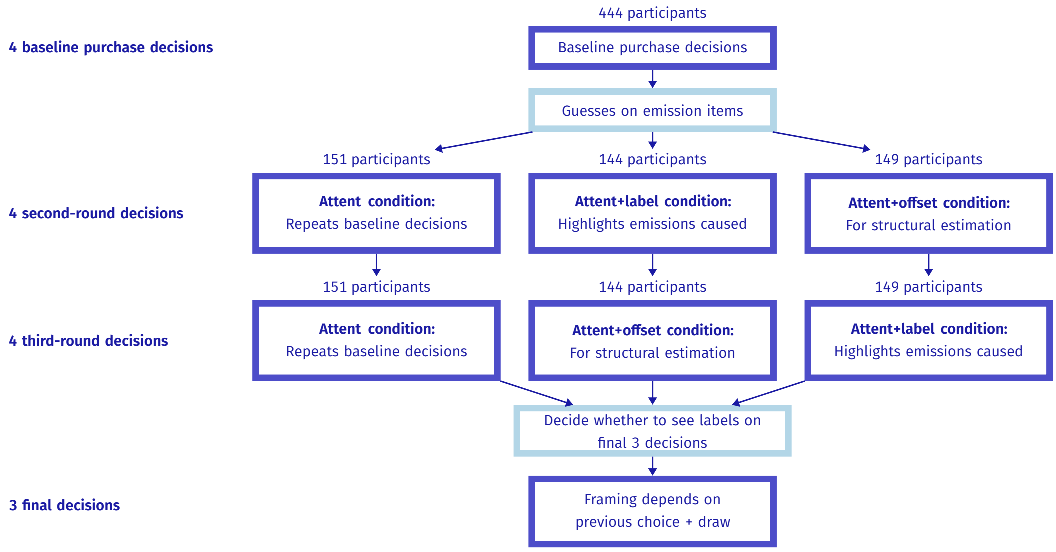


Figure 10. Experiment schedule and treatment groups

Note: Participants repeat the same four meal purchase decisions three times, with the decision framing differing across rounds. Treatments are described in more detail in the "Experiment timeline" paragraph above. The results of the ATTENTION+OFFSET condition are not further discussed in this section, details are described in section D.4, and results are shown in Tables B.7 and B.8. It is used as input for the structural estimation described in Section 6.

- 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.[53]

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[54], with partly changed treatment conditions:

- Participants previously in the ATTENTION+LABEL condition are now assigned to the ATTENTION+OFFSET condition and vice versa.
- Participants previously in the ATTENTION condition remain in the ATTENTION condition.

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

Participants and set-up. 444 experiment participants are recruited from the participant pool of the BonnEconLab, the behavioral experimental lab of the University of Bonn, to participate in one of 12 experimental sessions taking place between the 22nd of June and the 8th of July 2021. I pre-registered experiment design, sample restrictions, the analysis shown in Figure 14 and Table 5, and, roughly, the structural estimation.[55] Participant invitation and experiment set-up are as in Experiment 1.

53. They are not shown carbon labels. The results of the OFFSET condition are not further discussed in this section, details are described in section D.4 and results are shown in Tables B.7 and B.8. The OFFSET condition serves as input for the structural estimation described in Section 6, as detailed in Section 6.2.
54. 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.
55. See Schulze Tilling (2021b). Table B.9 B.10 show all preregistered main analyses. The analysis in Figure 12 was pre-registered as an additional analysis. The analysis shown in Figure 13 was not pre-registered.

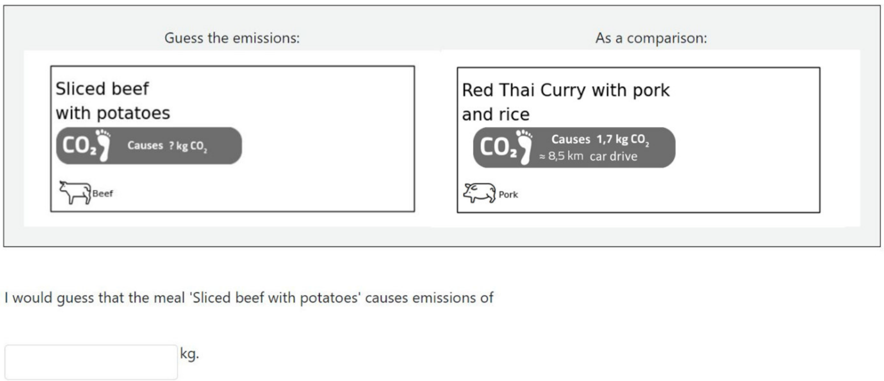


Figure 11. Example guessing questions

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

4.2 Data

I exclude the 3% fastest participants and participants not passing the comprehension check after five attempts, as pre-registered [56]. The remaining 444 participants are computer-randomized into treatments. Section B.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 B.2, and results hold when restricting the sample to only students or only non-vegetarians, as shown in Section B.7.

4.3 Estimation strategy and results

I use data from Experiment 3 to assess both the effect of the labels in correcting misperceptions, as well as their effect in directing participants' attention. Descriptive statistics, estimation strategy and results for each of these analyses are shown below.

The effect of correcting misperceptions. This subsection provides reduced-form evidence on whether treatment effects are reconcilable with a correction of misperceptions about carbon impact being the main channel driving treatment effects. This analysis draws on Experiment 3 participants' guesses of the carbon footprints of different meals. As first descriptive evidence, Figure 12 displays how average guesses deviated for each of the meals. On average, participants rather underestimate emissions (green-colored dots) for high-emission meals and overestimate emissions for low-emission meals (red-colored dots). Section B.13 shows further descriptive statistics on under- and overestimation of emissions, such as a comparison of the number of under- and overestimations by meal and participants, as well as the accuracy of the ranking of meals by carbon footprint which can be inferred from participants' guesses.

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

$$\Delta WTP_{ijm} = \alpha + \delta_1 Under_{im} + ThirdRound_j + \varepsilon_{ijm} \tag{4}$$

56. Schulze Tilling (2021b)

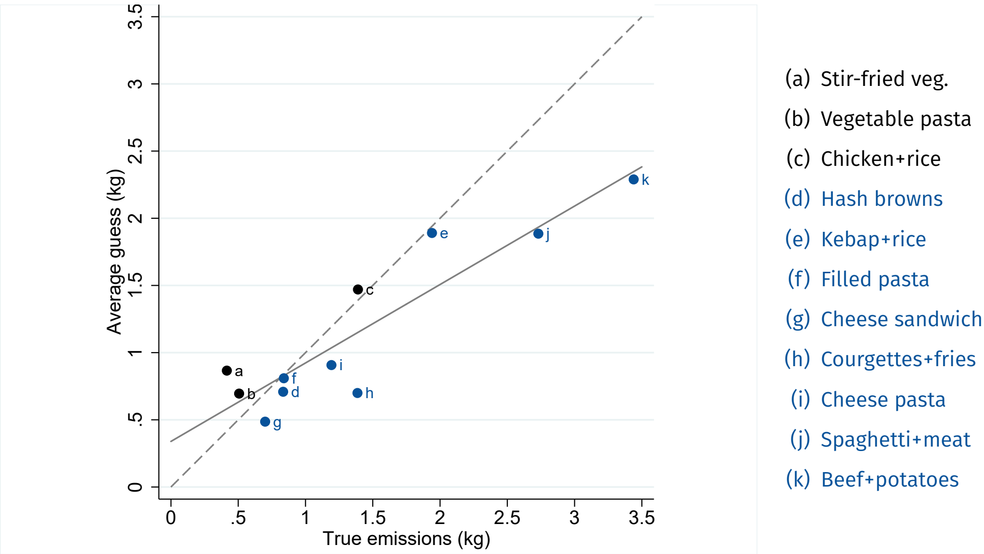


Figure 12. Average guess of the emissions caused by a given meal. Notes: Guesses are plotted against calculated emissions. Guesses closer to the dashed line are closer to calculated emission values. Meals corresponding to black scatter points are on average overestimated in their emissions, while meals corresponding to blue scatter plots are on average underestimated. The dashed fitted line is described by $y = 0.39 + 0.57x$, with both the intercept and the coefficient significant at $p < 0.01$. Values are based on guesses made by the participants of Experiment 3, and 71 participants in the "Control, then Control" group in Experiment 1. Data from these Experiment 1 participants is included in this graph, but not in any other analyses shown in this section. The meal "Spaghetti with meat" was only guessed by the 70 participants of Experiment 1 guessing emissions. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission value) are dropped. This leaves a total of 4,731 observations made by 490 participants.

where ΔWTP_{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.[57] 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 guess for the emissions of meal m and her guess for the cheese sandwich with the true difference in emissions.[58] $ThirdRound_j$ is an indicator of whether it was the third round of decisions. I use this specification to examine in reduced-form the role of a correction of misperceptions in driving treatment effects. If a correction of misperceptions was the main channel driving effects, one would expect treatment effects to be proportional to a subjects' underestimation of emissions.

Table 4, Spec. (1) shows the results of the OLS estimation of equation 4. 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 4 does not group observations by previous under- or overestimation but instead regresses the change in willingness

57. Please see Section 2.2 and B.9 for details on this specification.
58. This refers to the signed, not the absolute difference. For example, if a meal causes 0.2 kg of emissions more than the cheese sandwich, and the participants estimate that the meal causes 0.3 kg of emissions less than the cheese sandwich, this is an underestimation of the difference in emissions. Figure B.5 replicates results based only on participants' estimate of the meal emissions alone. Patterns are similar.

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. Spec. (2) estimates a decrease in willingness to pay independent of previous underestimation of €0.16. Figure 13 shows average effects split by previous under- or over-estimation of emissions and visualizes that participants on average also significantly adjust their willingness to pay downward for meals for which they previously *over*estimated emissions. In these cases, the labels inform participants that the meal has a lower relative carbon footprint than they previously expected. If a correction of misperceptions were the sole effect induced by the label, one would expect participants to adjust their willingness to pay upwards in such a situation, and not downwards. The pattern we see in Figure 13 is thus evidence against this being the case and in favor of a second mechanism driving treatment effects.

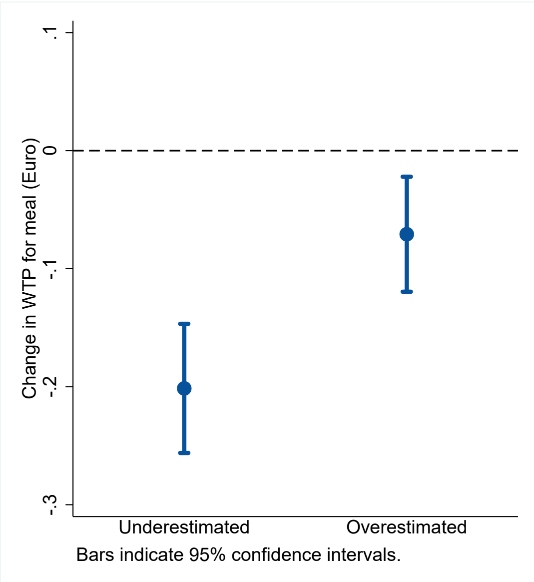


Figure 13. Within-subject change in willingness to pay for meals when shown carbon labels, depending on previous estimation. Notes: Effects are differentiated by whether the participant previously over- or under-estimated the difference in emissions between the specific meal and the cheese sandwich. Participants are all in the ATTENTION+LABEL condition. Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
underestimate		
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	262
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 4. Within-subject change in willingness to pay for meals when shown carbon labels, depending on participants' estimation of emissions. Notes: Dependent variable: within-subject change in willingness to pay for a meal when shown carbon labels, compared to baseline. Includes only participants in the ATTENTION+LABEL condition. Spec. (1) follows Equation 4.: 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 in spec. (2), the 10% most extreme guesses of the difference in emissions to the cheese sandwich (in terms of deviation from the true emission difference) are dropped. Standard errors are clustered at the individual level.

The effect of directing attention. This subsection provides reduced-form evidence on whether treatment effects are reconcilable with a direction of attention towards carbon emissions driving treatment effects. For the purpose of this analysis, I examine data from the ATTENTION and ATTENTION+LABEL conditions to estimate the magnitude of a possible attention effect. I estimate

$$\Delta WTP_{ijm} = \alpha + \beta_1 High_m + \beta_2 Low_m + \delta_1 (Label_{ij} \times High_m) + \delta_2 (Label \times Low_m) + ThirdRound_j + \varepsilon_{ijm} \tag{5}$$

where ΔWTP_{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 5, Figure 14 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 B.6 and B.7 in the Appendix. These results highlight that an increase in attention alone can explain a large proportion of the treatment effect. Section 6 provides an estimation of the quantitative relevance of each of the two channels in driving the label's treatment effect.

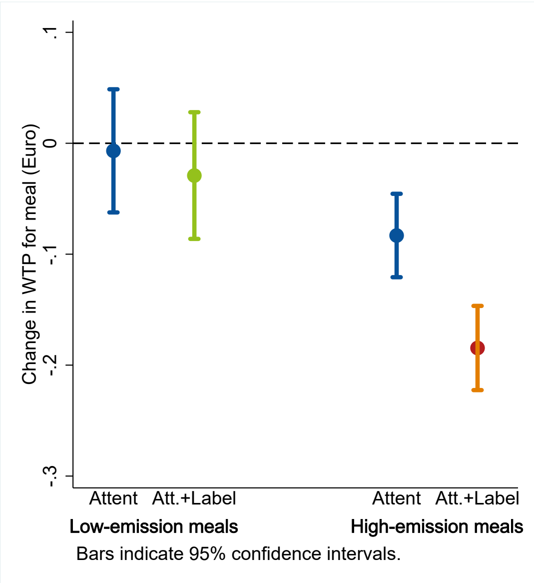


Figure 14. Within-subject change in willingness to pay for meals in the Attention vs. Attention+Label condition. Notes: Effects split to compare participants in the Attention and Attention+Label condition. Effects are split into meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

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

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

Table 5. Within-subject change in willingness to pay for meals in the Attention vs. Attention+Label condition. Notes: Dependent variable: within-subject change in willingness to pay for a specific meal when made attentive. Spec. (1) corresponds to Equation 5 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. "High emissions meal" describes the pure effect of being made attentive, "High emissions meal x Shown Label" the additional effect of seeing information. Includes data from participants in the Attention and Attention+Label condition. Standard errors are clustered at the individual level.

5 Consumer preferences for the presence of carbon labels

This section discusses experimental evidence of consumers' preferences for the presence of carbon labels in their consumption decisions. Section 5.1 discusses evidence from experiments 1 and 3, and Section 5.2 discusses evidence from Experiment 2.

5.1 Evidence from the framed field experiments

In both Experiments 1 and 3, participants indicate their willingness to pay for carbon labels being present or absent during their final set of consumption decisions. These elicitations are incentivized, as described in Section 2. The frequency distribution of willingness to pay values is visualized in Figure 15. About 50% of participants have a willingness to pay of 0, meaning they have no strong preference for the presence or absence of labels. Less than 5% have a negative willingness to pay, meaning they prefer the labels being absent. The remaining participants are willing to pay for the presence of labels, with 21% of the sample willing to pay €0.50 and above. Values barely differ between treatment groups, although willingness to pay seems to be slightly higher among those who have not yet seen labels in the course of the experiment, as shown in Table B.34.

Table B.35 shows a correlation analysis between willingness to pay for the presence of carbon labels and individual characteristics. Willingness to pay for seeing labels is strongly positively correlated with participants' approval of carbon labels being shown in the student canteen and participants' interest in using this information. It is also weakly positively correlated with participants' perceived strength of social norms for avoiding carbon emissions in food consumption, as measured based on Krupka and Weber (2013). Further, participants' reactions to carbon labels are strongly positively correlated with their willingness to pay for the presence of carbon labels (Table B.36). Participants who react strongly to the labels also have a stronger preference for seeing them in their decisions. Thunström (2019) finds that calorie labels create higher psychological costs for individuals with low self-control. I find no evidence of this also applying for carbon labels, as participants' self-control in eating behavior (as elicited using the questionnaire developed by Haws, Davis, and Dholakia (2016)) is not correlated with willingness to pay to see emission values.

5.2 Evidence from the natural field experiment

After Experiment 2 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 D. Importantly, the survey was advertised in a way such that survey respondents likely expected that results will be communicated to the student canteen.[59] Survey respondents thus had an incentive to report truthfully, as they could expect their response to affect student canteen policies and thus their future dining experience.

In the survey, 73% of the 234 participants are in favor of installing the labels permanently, 18% are not sure, and 9% against the measure. A revenue-neutral carbon tax of an unspecified amount,[60] in contrast, is 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.

59. I indeed involved the student canteen in the design of the survey and communicated results to them afterwards. The student canteen is currently planning to implement carbon labels permanently due to the emission savings produced and the positive canteen guest reaction.

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

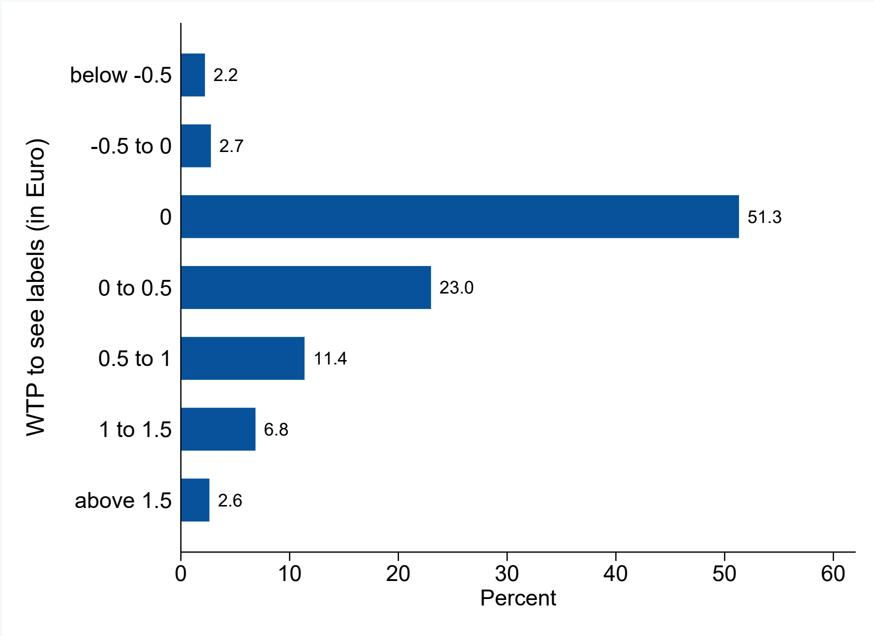


Figure 15. Distribution of willingness to pay indicated to see carbon labels on the final three consumption decisions

Note: In Euro. Based on Experiments 1 and Experiment 3. Includes data from all 731 participants.

6 Structural Model

To formalize how the two behavioral mechanisms shown in section 4 drive consumers' responses to carbon labels, and provide a quantitative estimate of the relative importance of each of the two channels, I introduce a simple discrete choice model of meal selection, which I structurally estimate using data from Experiment 3.

In the model, a consumer chooses from a set of meals and selects the meal that maximizes her perceived utility. In general, the perceived utility of a meal may depend on a multitude of meal attributes. The main attribute of interest in this model is the consumers' expectation of the carbon emissions caused by each meal. Ceteris paribus, the consumer has a higher valuation for a meal that causes fewer carbon emissions. How much the consumer cares about emissions depends on two parameters: the salience of carbon emissions at the moment of choice and the guilt the consumer perceives per kg of carbon emitted.[61]

6.1 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*

$$u(m) = v_m - p_m - \theta\gamma e_m. \tag{6}$$

Here, v_m is the *consumption utility* of meal m that is independent of emissions[62], 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.[63]

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

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

63. 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.

The *salience* of carbon emissions $\theta \in [0,1]$[64] and the consumer's *environmental guilt per perceived kg of emissions* γ jointly determine how much weight the consumer puts on carbon emissions when deciding.

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

$$e_m^{\text{info}} \;=\; (1-\kappa)e_m^{\text{true}} + \kappa e_m^{\text{prior}}. \tag{7}$$

Hence, the parameter $\kappa \in [0,1]$ is a measure of the stickiness of the consumers' prior estimate of emissions, e.g. due to a lack of trust in the carbon footprint information provided.[65] If the consumer is *attentive* to emissions, this sets $\theta = 1$.[66] Introducing *carbon labels* makes the consumer both informed and attentive.

6.2 Identification of parameters

The setting of experiments 1 and 3 corresponds to a special case of the model with a binary choice set $\mathcal{M} = \{m, o\}$ 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[67] as follows. First, in the absence of any treatment (elicitation at baseline), participants' willingness to pay is

$$WTP^B \;=\; v_m - v_o - \theta\gamma(e_m^{\text{prior}} - e_o^{\text{prior}}) \tag{8}$$

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

$$WTP^A \;=\; v_m - v_o - \gamma(e_m^{\text{prior}} - e_o^{\text{prior}}) \tag{9}$$

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 7. 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,[68] and willingness to pay indicated in the Attention+Label condition can thus be described as

$$WTP^{A+L} \;=\; v_m - v_o - \gamma\Big(\kappa e_m^{\text{true}} + (1-\kappa)e_m^{\text{prior}}\Big) \tag{10}$$

64. 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., 2024). In the framework of Bordalo, Gennaioli, and Shleifer (2022), a straight-forward reason why emissions might not be fully salient to consumers is a lack of prominence, as a meal's emissions are usually not (prominently) featured at the moment of choice.

65. 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.

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

67. 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.

68. 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 to larger treatment effects in the Label treatment in Experiment 1 than in the Attention+Label treatment in Experiment 3. These are shown side-by-side in Tables B.7 and B.8.

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 $e_m = 0$:

$$WTP^{A+O} = v_m - v_o \tag{11}$$

I rewrite the four equations in Section 6.2 for the structural estimation, as shown in Section A.1, and estimate parameters with GMM. I assume that the parameters γ, κ, and θ are homogeneous across participants.

Results are shown in Table A.2, Col.(1) . θ, the average attentiveness to greenhouse gas emissions in the absence of carbon labels, is estimated at 16%. This estimate implies that on average, individuals in my study react to the carbon footprint they perceive as if it was only 16% its size. The estimate is not significantly different from zero. Thus, the true level of attentiveness might also be zero, implying that individuals do not react at all to the perceived carbon footprint in the absence of any intervention. κ, the stickiness of the average consumers' prior estimate of a meal's carbon footprint, is estimated at 0.21 and also not statistically different from zero. This suggests that individuals on average place a relatively large weight $(1-\kappa)$ on the carbon footprint information shown on the carbon labels relative to their previous perception of the carbon footprint.γ describes how the emissions of one kg of greenhouse gas emissions affect an individual's utility. This is estimated as a decrease in monetized utility of Euro 0.12 per kg of emissions caused by the meal chosen, i.e. individuals on average experience guilt equivalent to a monetary cost of 0.12 per kg of perceived emissions, and statistically differs from zero. Based on the model, experiment data, and the estimation results, I provide an estimate of how much of the effect of the labels can be attributed to a direction of attention versus a correction of misperceptions (section A.4). I estimate that a direction of attention alone would have generated 79% of the emission savings produced by the labels, while a correction of misperceptions alone would have generated 11%. Columns (2)–(5) of Table A.2 show that estimates are similar in alternative specifications of the model.

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.2. Essentially, I assume that consumer welfare is a function of the true—and not the perceived—emissions caused by the meal consumed. Further, I allow for the carbon labels to have a psychological effect on consumers independent of their effect on consumption decisions, and interpret consumers' willingness to pay to see or avoid labels on their final three consumption decisions (shown in section 5) as a proxy for the effect of the labels on consumer welfare.[69] I estimate that the labels improve consumer welfare by the equivalent of 10¢for every choice affected by the labels, and on average create a fixed psychological benefit equivalent to 21¢.

7 Discussion

This paper provides evidence from the student canteen setting that carbon labels causally impact consumption behavior. I estimate emission savings of 3-4%, which one might intuitively dismiss as "too small" to consider carbon labels a worthwhile policy. However, my results also show that a carbon tax of €120 per tonne would have been needed to produce similar emission savings using a tax in the same setting. This comparison adds a new perspective to the effectiveness of carbon labels: €120 per tonne exceeds current EU ETS trading prices by more than 150%, and is three times the current German carbon tax on gasoline. Both of these policies - the EU ETS and the German carbon tax - have already in their current form experienced substantial political resistance, while resistance to behavioral instruments is typically smaller, both in the literature (see e.g. John, Martin, and Mikołajczak, 2023) and in the surveys I conducted in the student canteen. Thus, the comparison

69. Allcott and Kessler (2019) and Butera et al. (2022) take a similar interpretation.

might teach us to be a bit more modest in the emission savings we expect an intervention to produce to consider it worthwhile. While a reduction in emissions of 3-4% is certainly insufficient to solve the climate crisis we face,[70] it might still be understood as relatively large given what can be achieved within the realm of politically widely accepted policies, and given that it is generally difficult to shift food consumption behavior (see e.g. Guthrie, Mancino, and Lin, 2015).

My experiments also provide evidence on the channels driving the behavioral effects produced by the carbon labels. I show that a smaller part of the treatment effect can be explained by a correction of misperceptions, while a larger part of the effect is not explicable with this channel. My evidence suggests that this part of the effect can be explained by the labels directing attention towards carbon emissions. My results thus speak towards attention frictions playing an important role in impeding consumers from behaving in a carbon-friendly manner in the absence of labels. 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 food consumption.

The carbon labels were generally popular across all three experiments, with less than 10% of respondents expressing a preference against the labels. The results of Experiments 1 and 3 even suggest that consumers seem to be deriving a net psychological benefit from the labels. Why might this be the case? First, consumers might find the information itself intriguing, offering insights into the environmental impact of different food choices. Second, consumers might notice that they are more prone to take the environmentally friendly option in the presence of carbon labels and choose to see carbon labels as a type of commitment device. The carbon labels then remind them of self-set goals to decrease emission-heavy consumption. Third, consumers might appreciate that the labels help them make the environmentally-friendly choice, providing them to experience a feeling of "warm glow" or avoid a feeling of "cold prickle". Fourth, for those already inclined towards eco-friendly choices the presence of the labels might amplify the experience of warm glow. All of these four dynamics fit well into the framework outlined in section 6. The first two factors relate to costs or benefits created by the labels independent of their impact on consumption behavior. The third factor relates to increased utility from label-influenced choices, while the fourth factor relates to increased attentiveness towards emissions increasing the experienced intensity of warm glow for carbon-friendly (or cold prickle for emission-heavy) choices.

One might also argue that the labels make a social norm more salient, and force consumers to behave in a utility-decreasing manner due to social pressures. I find limited evidence for this being the main force driving results. In experiments 1 and 3, it seems unlikely that strong social norms drive the labels' treatment effects and approval for the labels. Participants make their choices anonymously, and suffer real consequences if their choice is not in line with their true interests. In the natural field experiment 2, it seems more plausible that social pressure drives part of the treatment effects: The carbon labels make the socially desirable choice (as designated by the student canteen) visible to all canteen guests and guests may fear being judged by other canteen guests if they choose differently. However, they have a chance to change their fortune in the survey I conduct in the field, by simply indicating a preference against the carbon labels. Only 9% of survey respondents do so, although it is arguably clear to respondents that their responses will be communicated to the student canteen and can have real consequences, as further described in section E. I thus do not focus on social pressure as a major force in my model in section 6, but one might still want to be mindful of such social pressures, and if a policymaker is strongly concerned about individuals opposed to carbon labels, it might be worthwhile to explore technological solutions that allow consumers to decide whether or not to see carbon labels.

70. One can understand my estimates as the carbon labels having a similar effect as pricing carbon at €120 per tonne. The social cost of carbon is often estimated at around €160 per tonne (e.g. Rennert et al., 2022), with recent papers estimating substantially higher amounts (Bilal and Känzig, 2024; Moore et al., 2024). Carbon labels are thus insufficient as a sole policy.

The experiment setting - student canteens - is an interesting application in its own right as it seems to be a promising environment for installing carbon labels[71]. Results can also provide suggestive guidance for related contexts, such as corporate canteens, restaurants and grocery shopping. Carbon labels are especially relevant as a potential policy tool for the food sector. Carbon taxes for this sector are still widely uncommon (e.g. the agricultural sector is excluded from the EU-ETS trading scheme) and Dechezleprêtre et al. (2022) identify agriculture-targeted policies as among the least popular policies to reduce carbon emissions. In such a setting, alternative policy tools are especially called for. Further, there are other discrete choice contexts in which the carbon footprint caused by different items could be calculated and labeled, e.g. shopping for toiletries or clothing. Future research could test the effectiveness and consumer welfare impact of carbon labels in these other consumption contexts, and also among other target populations. One way of doing so would be an adaptation of the design of Experiment 1—we would then be able to compare the relative effectiveness of labels across domains and populations.

Further research would also be beneficial to assess whether carbon labels affect consumers in other domains apart from the target behavior. Suggestive evidence from a field survey I conducted to accompany the natural field experiment (Experiment 2) provides no evidence of the carbon labels affecting consumers' attitudes towards other political measures to decrease carbon emissions (see section E.8 and Table C.9). However, spillovers might appear if labels are installed over longer time periods, or spillovers might affect other domains. Since the carbon labels mainly affect behavior by directing attention, attentional spillovers as described by Nafziger (2020) are also thinkable.

Appendix A Additional material on theoretical model and structural estimation

A.1 Equations for structural estimation and estimation results

To estimate the parameters of the structural model presented in section 6, I rewrite equations 8 to 11 as follows:

For equation A.1, I subtract equation 8 from equation 10:

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

For equation A.2, I subtract equation 8 from equation 9:

$$WTP^{A} - WTP^{B} = \gamma(e_{im}^{prior} - e_{io}^{prior})(1-\theta) \tag{A.2}$$

For equation A.3, I subtract equation 10 from equation 11:

$$WTP^{A+O} - WTP^{A+L} = -\gamma(e_{im}^{true} - e_{io}^{true})(1-\kappa) - \gamma(e_{im}^{prior} - e_{io}^{prior})\kappa \tag{A.3}$$

I then use data from Experiment 3 to estimate the parameters. To reduce the effect of outliers, I drop, for each meal, the 10% of observations pertaining to the 10% most extreme guesses. This leaves me with the following observations:

- 1.056 observations from 284 participants to estimate equation A.1
- 1.1104 observations from 146 participants to estimate equation A.2
- 1.056 observations from 284 participants to estimate equation A.3

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

For a better understanding of these observation and participant numbers, see Figure 10 that illustrates how experiment participants are allocated to the different treatment conditions in Experiment 3.

I estimate the three equations simultaneously using GMM in Stata, from the starting values Gamma=0.107, Theta=0.038, and Kappa=0.168. Column (1) shows my main specification, while col. (2)-(5) 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.

Table A.1. Structural estimates of model parameters

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

Standard errors in parentheses

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

Notes: 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.2, A.1, A.3. Columns (2)–Column (5) 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 $\theta = 1$.

A.2 Extension of the model to consumer welfare impact

To describe the effect of the labels on consumer welfare, I extend the model outlined in section 6 as follows:

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

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

Accordingly, carbon labels increase the likelihood of the consumer choosing the meal m that maximizes her true utility.[72] 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

$$u(P = 1) - u(P = 0) = u^{True}(m^L) - u^{True}(m^{prior}) + F \tag{A.5}$$

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.3 Quantification of welfare impact in the experiment setting

To apply equation A.5 to the experiment setting, I adapt it in two ways:

72. 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").

- I use participants' willingness to pay to see or avoid carbon labels as a proxy for $u(P=1)-u(P=0)$. However, the mere act of asking participants whether they would like to see carbon labels makes them attentive of emissions. Thus, the counterfactual they will compare their choice under carbon labels, m^L, to will be the choice they make when attentive of carbon emissions, m^A.
- Participants indicate their willingness to pay for meals relative to the cheese sandwich. I thus adapt equation A.4 to be expressed relative to the consumption utility obtained from the outside option (cheese sandwich), o_m, the emissions caused by the outside option, e_o^{true}, and price of the outside option, p_o.

Then, 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

$$u(P=1)-u(P=0) = u^{True}(m^{*L}) - u^{True}(m^{*A}) + F \tag{A.6}$$

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

$$u^{True}(m) = v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - p_m - p_o \tag{A.7}$$

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

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

In the experiment context, equation A.6 thus further transforms to:

$$\begin{aligned} u(P=1)-u(P=0) &= \mathbb{1}\Big(WTP^{A+L} \geq p_m - p_o\Big) \\ &\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - E[p_m - p_o | WTP^{A+L} \geq p_m - p_o]\Big) \\ -\mathbb{1}\Big(WTP^A \geq p_m - p_o\Big)&\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - E[p_m - p_o | WTP^A \geq p_m - p_o]\Big) + F \end{aligned} \tag{A.8}$$

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

$$\begin{aligned} WTP^P = {} & Prob\Big(WTP^{A+L} \geq p_m - p_o\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - E[p_m - p_o | \hat{V}_m^L \geq p_m - p_o]\Big) \\ & -Prob\Big(WTP^A \geq p_m - p_o\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - E[p_m - p_o | \hat{V}_m^A \geq p_m - p_o]\Big) \\ & +F \end{aligned} \tag{A.9}$$

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:

$$\begin{aligned} WTP^P = {} & \Big((WTP^{A+L}+3)/6\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - (WTP^{A+L}-3)/2\Big) \\ & -\Big((WTP^A+3)/6\Big)\Big(v_m - o_m - \gamma(e_m^{\text{true}} - e_o^{\text{true}}) - (WTP^A-3)/2\Big) + F \end{aligned} \tag{A.10}$$

For the estimation including welfare impact, I add equation A.10 to the estimation of equations A.1 to A.3, as well as participants' willingness to pay for the presence of labels, and estimate the four equations simultaneously. In Col. (1), I use only observations from those having experienced the ATTENT+LABEL condition to estimate equation A.10, since those participants who experienced only the ATTENT condition might not be able to form accurate expectations over the items in equation A.10. This leaves:

- 1.056 observations from 284 participants to estimate equation A.1
- 1.104 observations from 146 participants to estimate equation A.2
- 1.056 observations from 284 participants to estimate equation A.3
- 1.056 observations from 284 participants to estimate equation A.10 in Col. (1), and 2.160 observations from 430 participants in Col. (2).

I estimate equation A.10 for every meal I observe participants' choices on, using the same WTP^P for a single individual (as each participants only indicates his willingness to pay to see carbon labels once), but using participant and meal-specific baseline willingness to pay, emission values, and emission guesses. By using this estimation method, I essentially assume that participants form their valuation for the presence of carbon labels based on the emission labels to the meals they were shown beforehand. 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. It would thus be natural that participants extrapolate from the meal choices they made previously in the experiment.

Participants in the Attention condition have not seen emission labels before indicating their willingness to pay for the presence of labels, and would thus have to form a less informed expectation over the first two terms in A.10. I thus do not include them in the main estimation of F (Col.(1) in Table A.2. Col. (2) in Table A.2 includes these observations and estimates similar to the previous specification. Table B.34 shows that the average willingness to pay indicated for the presence of carbon labels does not differ across treatments.

Table A.2. Structural estimates of model parameters including data on willingness to pay for the presence of carbon labels

	(1)	(2)
Theta	0.18	0.18
	(0.17)	(0.17)
Gamma	0.12***	0.12***
	(0.02)	(0.02)
Kappa	0.23	0.23
	(0.20)	(0.20)
F	0.21***	0.20***
	(0.01)	(0.01)
Observations	3,216	3,216

Standard errors in parentheses

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

Notes: Analysis is based on data from Experiment 3. For each meal, the observations corresponding to the 10% most extreme guesses of the difference in emissions to the cheese sandwich (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.2, A.1, A.3. Column (2) includes values for willingness to pay for the presence of labels indicated by participants in the Attention treatment.

A.4 Additional simulation results: comparison of different interventions

A.4.1 Overview and results. In the model described in Section 6, 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, 3) a LABEL intervention making them both attentive and informed, 4) a carbon tax of €120 per ton, and 5) a ban on meat. 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 homogeneous across participants, the model specification shown in Section 6, and some assumptions on what constitutes a typical student canteen offer and pricing structure.

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.

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. 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.

I then simulate in the following manner how each participant would choose between the three available options:

- For non-vegetarians: For each of the two warm meal options, I calculate the difference between the utility a participant perceives for this option relative to the cheese sandwich, and compare it to the true price difference between warm meal and sandwich. I assume the participant chooses the meal option for which this difference is the largest, i.e. consumer surplus is the highest. If the difference is negative, I assume they choose the cheese sandwich. For example, on Day 3, if I calculate a participant's perceived utility to be €2.00 both for the Chicken Schnitzel and the Italian vegetable ragout, I would compare the respective consumer surplus of €2.00 - €1.55 = €0.45 and €2.00 - €1.25 = €0.75, and assume that the participant chooses the Italian vegetable ragout on Day 3.
- For vegetarian participants, there is only one warm meal option offered in the canteen every day. Thus, I compare whether perceived utility relative to the cheese sandwich is higher than the relative price. For example, for Day 3, I would check whether relative willingness to pay for

the pasta is at least €1.25 and assume the participant then eats pasta, and assume they eat the cheese sandwich otherwise.

Participant's choices at baseline are straight-forward to calculate: I simply compare the willingness to pay participants indicated at baseline with the prices charged by the different options and assume the participant chooses the option generating the highest consumer surplus.

To calculate choices with an intervention solely increasing attention, I first calculate participant's perceived willingness to pay for a meal if only attention is raised, based on according to equations 8 and 9.

$$WTP^A = v_m - v_o - \gamma(e_{im}^{prior} - e_{io}^{prior}) \tag{A.11}$$

Based on this equation, I use participants' baseline willingness to pay and prior emission estimates as well as the estimated model parameters to calculate participants' perceived willingness to pay in the ATTENTION condition, and then simulate meal choices as in the previous calculation.

A KNOWLEDGE treatment is assumed to lead to the consumer updating her emissions estimate according to 7 without directing attention.

$$WTP^K = v_m - v_o - \theta\gamma(1-\kappa)(e_m^{true} - e_o^{true}) - \theta\gamma\kappa(e_m^{prior} - e_o^{prior}) \tag{A.12}$$

I calculate perceived willingness to pay and simulate meal choices as in the previous calculation.

A LABEL treatment combines both of the previous effects

$$WTP^L = v_m - v_o - \gamma(1-\kappa)(e_m^{true} - e_o^{true}) - \gamma\kappa(e_m^{prior} - e_o^{prior}) \tag{A.13}$$

I calculate perceived willingness to pay and simulate meal choices as in the previous calculation.

Finally, perceived willingness to pay with a CARBON TAX and MEAT BAN is as at baseline. However, I increase prices in the CARBON TAX treatment to incorporate a carbon tax of €120 per ton, and in MEAT BAN I modify the four exemplary days shown above to exclude the meat option.

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

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

In the extension of my model to consumer welfare specified in Section A.2, consumer welfare resulting from a meal choice is a function of the true—and not the perceived—emissions resulting from the meal choice. Carbon labels thus, by moving perceived emissions closer to true emissions,

Table A.3. Estimated effect of different policies in the student canteen

	# of choices			Δ GHGE	Δ consumer welfare			
Intervention	sandwich	veg.	meat	Average	Average	SD	Min	Max
None	73.1%	18.1%	8.8%					
Attention	74.4%	18.1%	7.4%	-.0267	.0010	.0160	-.0849	.2456
Knowledge	73.7%	18.2%	8.1%	-.0036	.0001	.0043	-.0657	.0583
Labels	74.1%	18.6%	7.3%	-.0338	.0018	.0164	-.0022	.2456
Carbon tax	72.4%	19.9%	7.7%	-.0310	.0013	.0676	-.3125	.2648
Meat ban	78.3%	21.7%		-.1473	-.0350	.1728	-1.3935	.2456

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

increase the likelihood of a consumer choosing the option maximizing his welfare. The final four columns of Table A.3 estimate how consumer welfare changes accordingly under each of the interventions. Importantly, these estimates account for the fact that a change in meal choice also leads to a change in consumption utility. For example, if a consumer switches from a meat to a vegetarian meal as a result of the label, but enjoys the taste of the meat meal more, the calculations account for this. They are thus considerably lower than a mere multiplication of the average reduction in greenhouse gas emissions with the average guilt perceived per kg of emissions.

I estimate that carbon labels improve consumer welfare by the monetary equivalent of 0.18¢ per choice (averaging over choices affected and not affected by the labels), or 10¢ on average for every choice affected by the labels. Synergies between the ATTENTION and KNOWLEDGE intervention are more sizable here, with the effects of the other two interventions merely summing to 0.11¢. Both the ATTENTION and the KNOWLEDGE intervention in some cases result in considerable decreases in consumer welfare. This can be the case if a consumer with large misperceptions of carbon impact is made attentive, or if a consumer who generally overestimates emissions and is very inattentive towards emissions is made knowledgeable of emissions. Welfare changes are thus in both cases more dispersed than for the LABELS intervention. Further, while I estimate that the average decrease in greenhouse gas emissions and increase in consumer welfare caused by the CARBON TAX is similar (albeit a bit smaller) than caused by the LABELS, the change in consumer welfare is substantially more dispersed with the CARBON TAX. The MEAT BAN produces the largest decrease in greenhouse gas emissions, but is also the only intervention for which I estimate a decrease in consumer welfare.

Appendix B Experiments 1 and 3: Additional tables and figures

B.1 Randomization checks

Table B.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 B.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 B.11 and Table B.12. 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 B.1. Randomization Experiment 1

	Average value					
	(1) Age	(2) Male	(3) Student	(4) Working	(5) 0 if does not eat meat	(6) Hungry
Control, then Control	-0.59	-0.00	0.08	0.05	-0.15**	0.02
	(1.09)	(0.07)	(0.06)	(0.07)	(0.06)	(0.38)
Control, then Label	-0.80	-0.01	0.01	0.10	-0.08	-0.05
	(1.08)	(0.07)	(0.06)	(0.07)	(0.06)	(0.38)
Constant	24.60***	0.33***	0.78***	0.58***	0.80***	5.16***
	(0.62)	(0.04)	(0.03)	(0.04)	(0.04)	(0.21)
Control, then Control	60	70	70	70	70	70
Control, then Label	62	69	69	69	69	69
Label, then Offset	126	148	148	148	148	148
Observations	248	287	287	287	287	287

Standard errors in parentheses

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

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

Table B.2. Randomization Experiment 3

	Average value					
	(1) Age	(2) Male	(3) Student	(4) Working	(5) 0 if does not eat meat	(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

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

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

B.2 Representativeness of the sample

Tables B.3 and B.4 report descriptive statistics for experiments 1 and 3. Table B.5 reports descriptive statistics elicited in a survey among student canteen guests, as described in Section E.8. 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 (41%). 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, 12% of guests paying with an individualized payment card are employees, 86% are students and 2% are non-student and non-employee.[73] Participants in Experiments 1 and 3 are less likely to be vegetarian than the average student canteen guest: While 75% and 76% of participants in Experiments 1 and 3, respectively, are non-vegetarian, only 66% of student canteen guests are non-vegetarian.

The largest differences between the experiment sample and survey and student canteen data are thus the proportion of non-students and the proportion of non-vegetarians. Section B.7 thus repeat the main analyses from experiments 1 and 3 splitting by whether participants are students or employees. Results seem broadly similar across students and non-students. However, compared to students, non-students seem to react less precisely to emission amounts, but react relatively uniformly to all high-emission meals (Table B.14), and labels seem to have no additional effect once participants have been made attentive of emissions (Table B.22), again suggesting a more rigid reaction by non-students. Comparing vegetarians and non-vegetarians, a similar picture emerges, with non-vegetarians reacting less precisely to emission amounts and previous understimation than vegetarians (Tables B.11 and B.15).

73. 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 B.3. Socio-economic summary statistics for Experiment 1

Variable	Explanation	Mean	Std. Dev.
Age	Age of participant	24.27	6.9
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	5.16	2.58
N	288		

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

Table B.4. Socio-economic summary statistics for Experiment 3

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		

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

Table B.5. Socio-economic summary statistics for student canteen guests - survey data

Variable	Explanation	Mean	Std. Dev.
Age	Age of participant	22.90	–
Male	Dummy: 1 if participant is a man	0.41	–
Student	Dummy: 1 if guest is a student	0.94	–
Non-vegetarian	Dummy: 1 if guest eats meat	0.66	–
N	1,451		

Notes: Statistics are based on the surveys I conducted among student canteen guests in April and June. I include only survey respondents who visited a student canteen at least once in the 14-week study period and paid with their individual payment cards. See E.8 for details on the survey design. To preserve anonymity (since I also asked these survey participants about their study field), I elicited age in intervals. To reach an estimation of the mean age, I set the age equal to the midpoint of each interval. For 13% of respondents, I have the information that they are below 20. For the calculation, I estimate their age at 18. For 54% of respondents, I have the information that they are between 20 and 23 (which I set to 21.5 for the estimation), 21% 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.

Table B.6. Socio-economic summary statistics for student canteen guests - consumption data

Variable	Explanation	Mean	Std. Dev.
Student	Dummy: 1 if guest is a student	0.85	–
Non-vegetarian	Dummy: 1 if guest eats meat	0.66	–
N	10,131		

Notes: Statistics are based on canteen purchases made with individual payment cards in the 14-week study period.

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

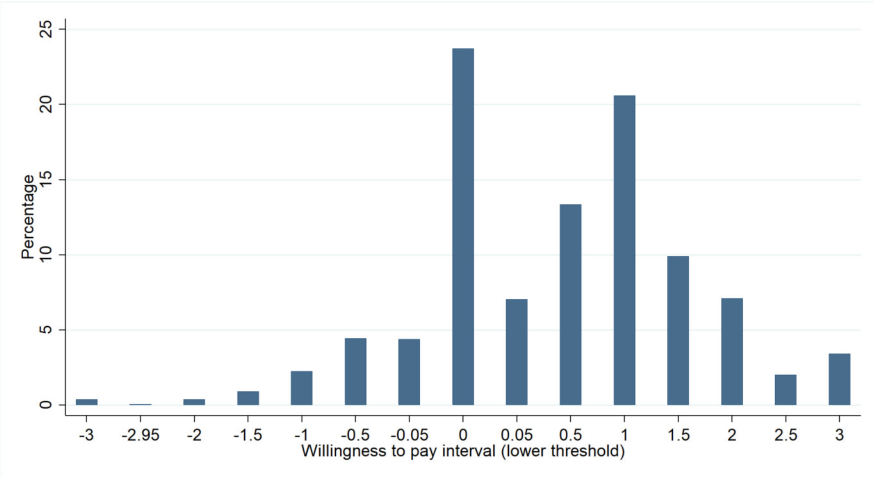


Figure B.1. Willingness to pay indicated for meals in the baseline purchase decisions in Experiment 1

Note: $N = 1,148$ (287 participants making 4 baseline decisions each).

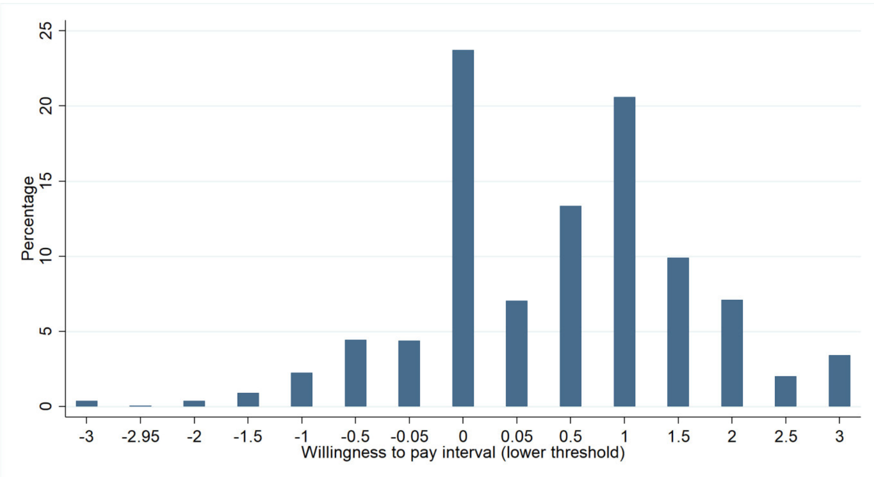


Figure B.2. Willingness to pay indicated for meals in the baseline purchase decisions in Experiment 3

Note: $N = 1,776$ (444 participants making 4 baseline decisions each).

B.4 Simulation to calculate emission savings in Exp. 1

To estimate the emission savings conveyed by the data collected in Experiment 1, I simulate how experiment participants would have chosen on four days with a typical canteen offer. The offer on each of these exemplary days is as follows:

- 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 1 and I know participants' taste preferences for these meals accordingly. 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.

I then simulate in the following manner how each participant would have chosen between the three available options:

- For non-vegetarians: For each of the two warm meal options, I calculate the difference between the willingness to pay participants indicated for this option relative to the cheese sandwich, and compare it to the true price difference between warm meal and sandwich. I assume the participant chooses the meal option for which this difference is the largest, i.e. consumer surplus is the highest. If the difference is negative, I assume they choose the cheese sandwich. For example, on Day 3, if a participant indicates a relative willingness to pay of €2.00 both for the Chicken Schnitzel and the Italian vegetable ragout, I would compare the respective consumer surplus of €2.00 - €1.55 = €0.45 and €2.00 - €1.25 = €0.75, and assume that the participant would have chosen the Italian vegetable ragout on Day 3.
- For vegetarian participants, there is only one warm meal option offered in the canteen every day. Thus, I compare whether reported willingness to pay relative to the cheese sandwich is higher than the relative price. For example, for Day 3, I would check whether relative willingness to pay for the pasta is at least €1.25 and assume the participant then eats pasta, and assume they eat the cheese sandwich otherwise.

I include only participants in this condition who experience the Label condition during Exp. 1, and simulate these participants' choices once based on the willingness to pay values they indicate at baseline, and then again based on the willingness to pay they indicate when they see carbon labels. For each condition, I calculate and compare aggregate emission savings. Average emissions per lunch are 0.904 kg at baseline, and 0.861 kg with labels. The difference in emissions is thus 43 gram, or 4.8% of baseline emissions.

B.5 Distribution of individual treatment effects in Exp. 1

Using only observations from the 217 participants who experienced carbon labels in Experiment 1 (868 observations), I can run spec. (2) in Table 1 at the individual level. 59% of individual-level coefficients estimated are negative, 12% are zero, and 29% are positive. Estimated coefficients range between -6.2 and 2.4. Coefficients are plotted in Figure B.3 below. I truncate the 10% most extreme coefficient estimations for better readability.

Individual-level coefficients are largely in line with the coefficient estimated in the main analysis in Table 1 (-0.12). This suggests that the main result is not driven by few particular individuals, but reflected in the behavior of a majority of the sample.

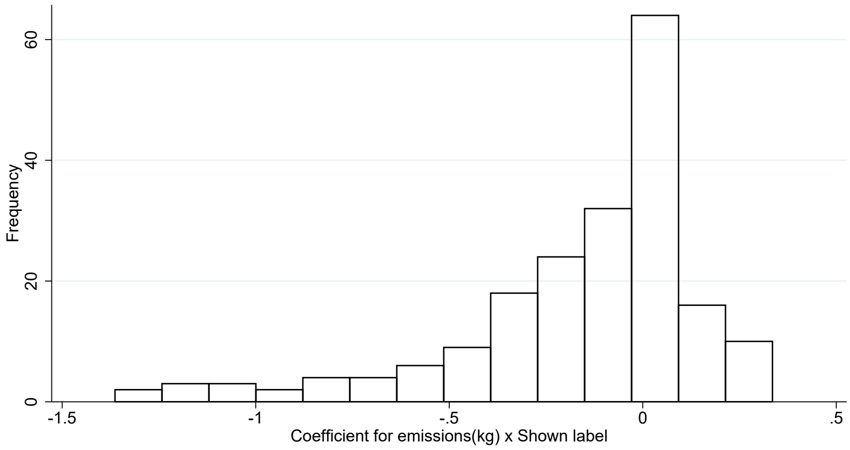


Figure B.3. Individual-level coefficients estimated in Experiment 1

Note: Individual-level coefficients for "Emissions(kg) x Shown label" in Spec. (2) of Table 1. $N = 197$ (10% most extreme coefficients truncated).

B.6 Pre-registered main effects Exp. 1 and Exp. 3

Experiments 1 and 3 were pre-registered on #AEARCTR-0007858 and #AEARCTR-0008435.

For Experiment 1, besides the analysis shown in the main text, I pre-registered an analysis pooling all data from Experiments 1 and 3. I include a description of results and respective results below. Results are in line with those described in the main text and included here for completeness.

These analyses are shown in Tables B.7 and B.8. Table B.7 includes interaction terms for all treatment conditions. The baseline condition is the Control condition from Experiment 1. Rows 3 and 4 show differential effects for the Label condition in Experiment 1, and rows 5 and 6 pick up differential effects if the emission labels indicate that emissions are offset. Rows 7 and 8 pick up differential effects for the Attent condition from Experiment 3, and rows 9 and 10 pick up differential effects for the Attent+Label condition, with rows 11 and 12 again picking up differential effects of the carbon labels indicating that emissions are offset.

Table B.8 shows, in Col. (1), a two by two analysis varying whether participants are shown carbon labels and whether they are made attentive of emissions, excluding data from the Offset condition. Col. (2) pools all offsetting and all labeling treatments to investigate the effect of carbon offsetting relative to providing labels.

Table B.7. Experiments 1 and 3: Comparison of treatment effects

	Change in WTP compared to baseline
	(1)
Low	-0.05^{*}
	(0.03)
High	0.02
	(0.02)
Low x Label	0.14^{***}
	(0.04)
High x Label	-0.31^{***}
	(0.05)
Low x Label x Offset	-0.03
	(0.04)
High x Label x Offset	0.23^{***}
	(0.05)
Low x Attent	0.04
	(0.04)
High x Attent	-0.10^{***}
	(0.03)
Low x Attent+Label	0.02
	(0.04)
High x Attent+Label	-0.20^{***}
	(0.04)
Low x Attent+Label x Offset	0.02
	(0.02)
High x Attent+Label x Offset	0.16^{***}
	(0.02)
Control for third round	0.00
	(0.01)
Participants control	139
Participants label	217
Participants offset	148
Participants attent	151
Participants attent+offset	293
Participants attent+label	293
Observations	5,848

Standard errors in parentheses

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

Notes: This analysis was preregistered in Schulze Tilling (2021b). Regression combines data from Experiments 1 and 3. Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline. 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). The baseline condition is Control. "Low" and "High" respectively turn 1 for low-emission and high-emission meals. "Low x Label" and "High x Label" respectively turn 1 for low-emission and high-emission meals in the Label and Offset conditions. "Low x Label x Offset" and "High x Label x Offset" respectively turn 1 for low-emission and high-emission meals in the Offset condition. "Low x Attent" and "High x Attent" respectively turn 1 for low-emission and high-emission meals in the Attention and Attention+Labels and Attention+Offset conditions. "Low x Attent+Label" and "High x Attent+Label" respectively turn 1 for low-emission and high-emission meals in the Attention+Labels condition. "Low x Attent+Offset" and "High x Attent+Offset" respectively turn 1 for low-emission and high-emission meals in the Attention+Offset condition. Standard errors are clustered at the individual level.

Table B.8. Experiments 1 and 3: Comparison of label and offset effects

	Change in WTP compared to baseline	
	(1)	(2)
Low	-0.06^{**}	-0.05^{*}
	(0.03)	(0.03)
High	0.01	0.02
	(0.02)	(0.02)
Low x All Label	0.14^{***}	0.07^{*}
	(0.04)	(0.04)
High x All Label	-0.31^{***}	-0.25^{***}
	(0.05)	(0.03)
Low x Attent	0.04	0.04
	(0.04)	(0.04)
High x Attent	-0.11^{***}	-0.10^{***}
	(0.03)	(0.03)
Low x Attent x Label	-0.12^{**}	
	(0.05)	
High x Attent x Label	0.10^{**}	
	(0.05)	
Low x Label x Offset		-0.01
		(0.02)
High x Label x Offset		0.20^{***}
		(0.02)
Control for third round	0.03	0.01
	(0.02)	(0.01)
Participants control	139	139
Participants label	217	217
Participants offset	0	148
Participants attent	151	151
Participants attent+offset	0	293
Participants attent+label	293	293
Observations	4,084	5,848

Standard errors in parentheses

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

Notes: This analysis was preregistered in Schulze Tilling (2021b). Regressions combine data from Experiments 1 and 3. Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline. The baseline condition is the CONTROL condition. "Low" and "High" respectively turn 1 for low-emission and high-emission meals. "Low x All Label" and "High x All Label" respectively turn 1 for low-emission and high-emission meals in the LABEL, ATTENTION+LABELS, OFFSET and ATTENTION+OFFSET conditions. "Low x Attent" and "High x Attent" respectively turn 1 for low-emission and high-emission meals in the ATTENTION and ATTENTION+LABELS and ATTENTION+OFFSET conditions. "Low x Attent x Label" and "High x Attent x Label" respectively turn 1 for low-emission and high-emission meals in the ATTENTION+LABELS condition. "Low x Label x Offset" and "High x Label x Offset" respectively turn 1 for low-emission and high-emission meals in the ATTENTION+OFFSET and OFFSET condition. Col. (1) is a 2 by 2 analysis comparing the effect of raising attention and labels, and excludes participants in the offset conditions. Col. (2) pools observations from the two offsetting conditions to estimate the effective of offsetting relative to labels. Standard errors are clustered at the individual level.

Tables B.9 and B.10 show the additional analyses I pre-registered for Experiment 3. The baseline category here is the ATTENTION CONDITION. I preregistered to examine WTP for meals as the dependent variable, while including participant × meal fixed effects. As shown in section B.9 this is econometrically equivalent to using the change in WTP as the outcome variable, as I do in the main text analyses. I chose to use the change in WTP as the outcome variable in the main text for exposition reasons. Col. (1) of Table B.9 directly examines the effect of providing labels additionally to directing attention to carbon emissions, and the effect of offsetting relative to directing attention. This is broadly similar to Table 5 in the main text. Similarly, Col. (2) performs a similar analysis interacting the emissions of each meal with treatments rather than using the Low and High indicators. Table B.10 further examines the effect of carbon offsetting, excluding data from the labeling condition. Col. (2) examines the effect of directing attention and offsetting as a function of emissions guessed by participants.

Table B.9. Experiment 3: Analysis label and offsetting effects

	WTP	
	(1)	(2)
Low x Post x Label	−0.02	
	(0.03)	
High x Post x Label	−0.10***	
	(0.04)	
Low x Post x Offset	−0.00	
	(0.03)	
High x Post x Offset	0.06*	
	(0.03)	
High x Post	−0.08***	
	(0.03)	
Low x Post	−0.03	
	(0.04)	
Emissions(kg) x Post x Label		−0.01
		(0.03)
Emissions(kg) x Post x Offset		0.06**
		(0.03)
Emissions(kg) x Post		−0.07***
		(0.02)
Label x Post		−0.07***
		(0.02)
Offset x Post		−0.01
		(0.02)
Control for third round	−0.00	−0.00
	(0.01)	(0.01)
Constant	0.70***	0.70***
	(0.01)	(0.01)
Participant x Meal FE	Yes	Yes
Participants attent	151	151
Participants attent+offset	293	293
Participants attent+label	293	293
Observations	5,328	5,328

Standard errors in parentheses

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

Notes: This analysis was preregistered in Schulze Tilling (2021a). Dependent variable: Willingness to pay for a meal. Regression specifications is similar to Equation 5, but additionally includes interactions for the Offset condition, and uses an approach with individual times meal fixed effects and willingness to pay as the dependent variable, instead of using difference in willingness to pay as in Table 5, similar to Table 2. 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). The baseline condition is the Attention condition, and the "Post" indicator refers to WTP registered in rounds 2 and 3 of the experiment. Standard errors are clustered at the individual level.

Table B.10. Experiment 3: Analysis offseting effects

	WTP	
	(1)	(2)
Low x Post x Offset	-0.07	
	(0.07)	
High x Post x Offset	0.07^{**}	
	(0.03)	
High x Post	-0.07^{***}	
	(0.02)	
Low x Post	0.04	
	(0.05)	
Guessed emissions(100 kg) x Post x Offset		0.19
		(0.14)
Guessed emissions(100 kg) x Post		-0.03^{***}
		(0.00)
Post x Offset		-0.00
		(0.02)
Control for third round	-0.01	-0.03^{**}
	(0.02)	(0.01)
Constant	0.71^{***}	0.69^{***}
	(0.01)	(0.01)
Participant x Meal FE	Yes	Yes
Participants attent	151	151
Participants attent+offset	293	293
Observations	4,156	4,156

Standard errors in parentheses

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

Notes: This analysis was preregistered in Schulze Tilling (2021a). Dependent variable: Willingness to pay for a meal. Here, the definition of low- and high-emission meals is based on participants' guesses for the meals and the cheese sandwich. Low-emission meals are meals for which the respective participant guessed lower emissions than for the cheese sandwich, and vice versa. Similarly, Col. (2) uses the guessed difference in emissions. The baseline condition is the Attention condition. Standard errors are clustered at the individual level.

B.7 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)
label		-0.04
		(0.04)
Control for third round	0.01	0.01
	(0.04)	(0.04)
Constant		-0.05*
		(0.03)
Participants control	96	96
Participants treated	169	169
Observations	1,244	1,244

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

Table B.11. 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)
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 B.12. 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)
label		-0.05
		(0.04)
Control for third round	0.01	0.01
	(0.03)	(0.03)
Constant		-0.04**
		(0.02)
Participants control	114	114
Participants treated	169	169
Observations	1,372	1,372

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

Table B.13. 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)
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 B.14. 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	220
Obs. underestimate	451	420
Obs. overestimate	418	367
Observations	869	787

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

Table B.15. Replication of Table 4 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	64
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 B.16. Replication of Table 4 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	198
Obs. underestimate	383	361
Obs. overestimate	391	340
Observations	774	701

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

Table B.17. Replication of Table 4 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	86
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 B.18. Replication of Table 4 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 B.19. Replication of Table 5 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 B.20. Replication of Table 5 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 B.21. Replication of Table 5 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 B.22. Replication of Table 5 including only non-students

B.8 Replication excluding round 3 observations

Table B.23. 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	139	139
Participants treated	148	148
Observations	1,148	1,148

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

Table B.24. Replication of Table 4 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	140
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 B.25. Replication of Table 5 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)
Control for third round	0.00
	(.)
Participants attent	151
Participants label	144
Observations	1,180

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

B.9 Exp. 1: Alternative econometric specifications

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

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

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

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

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

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

Spec. (1) in Table B.26 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 B.26. 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)
Participant x Meal FE	Yes	
Participants control	139	139
Participants treated	217	217
Observations	2,852	2,852

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

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

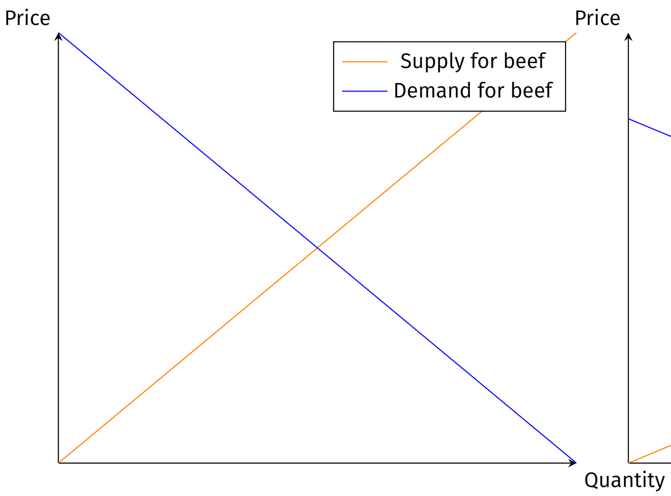
B.10 Exp. 1: Intuition behind expressing effect sizes in terms of a carbon tax

One of the main results shown in section 2.3 is that carbon labels in Experiment 1 produce a similar impact as would result from a carbon tax of €0.12 per kg or €120 per tonne. The underlying assumption for this comparison is that a shift in the demand curve due to the installation of carbon labels affects total quantity similarly as a would a shift in the demand curve due to the installation of a carbon tax.

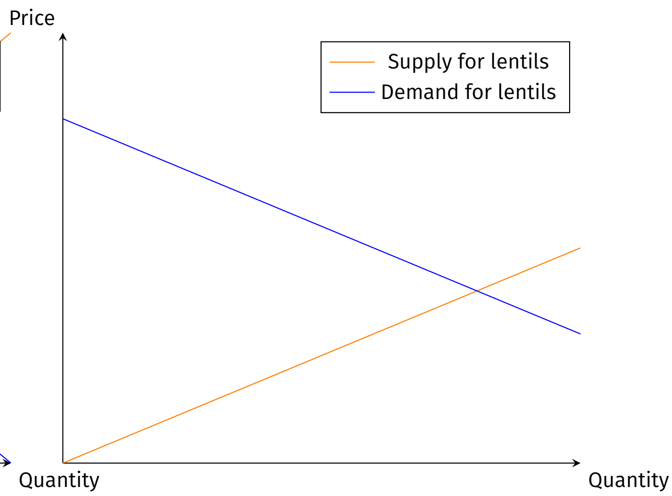
To illustrate this point, I first show in Figure B.4 how carbon labels and a carbon tax would affect price and quantity purchased in two specific product markets: beef and lentils. Images (a) and (b) show a stylized illustration of how the current market equilibrium in the beef market and the lentils market might look like. In each market, the equilibrium price and quantity is determined by the intersection of the supply and demand curves. Image (c) shows how the beef market would be affected by a downward shift in the demand curve. This shift in the demand curve could either result from consumers being willing to pay less for beef due to carbon labels, or consumers being willing to pay less because a carbon tax will be added to their purchase. The downward shift in the demand curve leads to the demand curve and supply curve now intersecting at a lower price and a lower quantity. Image (d) shows how the lentils market would be affected by an upward shift in the demand curve. This shift could again either result from consumers being willing to pay more for lentils as they recognize their good environmental performance on the carbon labels, or consumers being willing to pay more because there will be no carbon tax added to their purchase. The upward shift in the demand curve leads to the demand curve and supply curve now intersecting at a higher price and a higher quantity.

More generally, one could think of demand for emission-heavy goods in a more abstract sense, with there being some demand curve describing consumer demand for different items as a function of how much emissions result from their production. A carbon tax would shift this demand curve downward, just as would carbon labels. My analysis in section 2.3 quantifies the shift occurring through the labels in terms of which height of a carbon tax would be required to shift this demand curve downward by the same extent. Note that my estimate of €0.12 per kg averages over all participants, i.e. it already incorporates that some consumers might be reacting to the labels more strongly than other consumers.

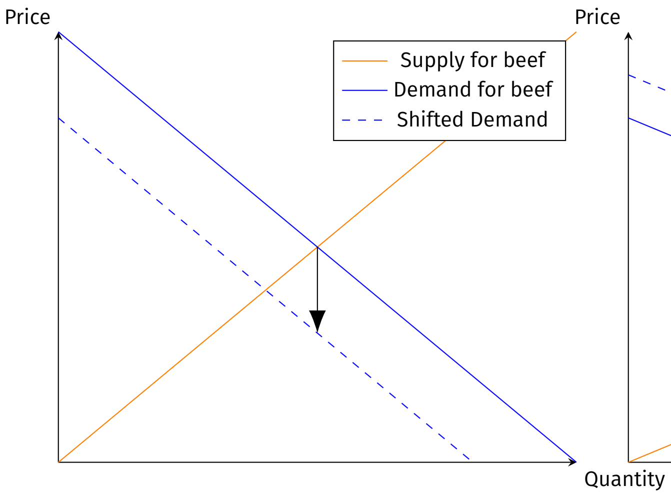
Importantly, my €120 per tonne equivalence result describes participant behavior in Experiment 1, i.e. it is specific to a certain population group and consumption context. To reach a carbon tax equivalence estimate for e.g. the entire German or European market, data from other population groups and consumption contexts is needed.



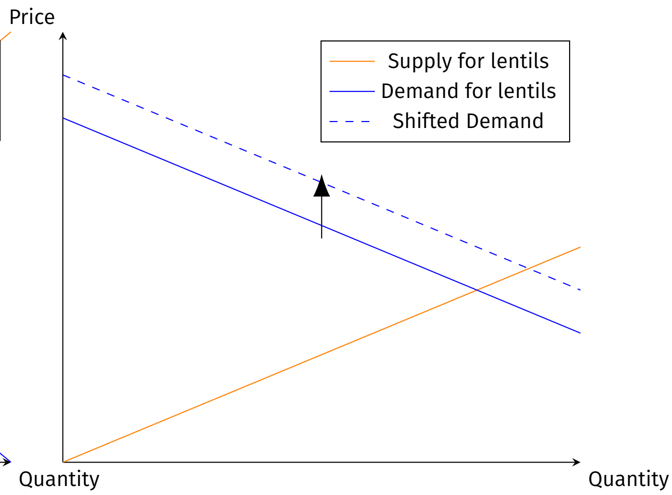
(a) Beef market equilibrium



(b) Lentils market equilibrium



(c) Shifted demand in beef market



(d) Shifted demand in lentils market

Figure B.4. Comparison of supply and demand in beef and lentils markets

B.11 Exp. 1: Heterogeneity in treatment effects

Table B.27. Pre-registered heterogeneity analysis (Table C.7)

	Change in WTP compared to baseline			
	(1)	(2)	(3)	(4)
	All	Low income	Env. important	High self-control
High emission meal x Shown label	-0.31^{***}	-0.33^{***}	-0.39^{***}	-0.33^{***}
	(0.05)	(0.07)	(0.07)	(0.07)
Low emission meal x Shown label	0.14^{***}	0.12^{*}	0.17^{***}	0.23^{***}
	(0.04)	(0.06)	(0.06)	(0.06)
High emission meal	0.01	0.05^{*}	-0.00	0.00
	(0.02)	(0.03)	(0.03)	(0.03)
Low emission meal	-0.06^{*}	-0.01	-0.08^{**}	-0.10^{**}
	(0.03)	(0.05)	(0.03)	(0.05)
Control for third round	0.02	0.00	0.04	0.01
	(0.03)	(0.05)	(0.04)	(0.05)
Participants control	139	54	89	69
Participants treated	217	81	122	104
Observations	1,704	652	1,028	832

Standard errors in parentheses

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

Notes: Table shows heterogeneity analyses pre-registered as further outcomes in Schulze Tilling (2021b). Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline. Specifications correspond to Equation 1 and do not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. Col.(1) includes all data, and Col.(2) includes only individuals with the lowest possible net income option that could be indicated (under €700 a month). Col.(3) includes only survey participants who report an above-average importance of environmental aspects in their food consumption decisions. Eating self-control in Col. (4) is measured using the questions developed by Haws, Davis, and Dholakia (2016). Standard errors are clustered at the individual level. I additionally pre-registered to investigate heterogeneity concerning education, but did not implement this due to lack of variation in my sample (almost all highly educated).

Table B.28. Further heterogeneity results in Experiment 1

	Change in WTP compared to baseline					
	(1) Female	(2) Below 24	(3) Hungry	(4) In favor	(5) Social circle	(6) Strong norms
High emission meal x Shown label	-0.36^{***}	-0.33^{***}	-0.36^{***}	-0.42^{***}	-0.36^{***}	-0.36^{***}
	(0.06)	(0.07)	(0.06)	(0.07)	(0.05)	(0.07)
Low emission meal x Shown label	0.10^{*}	0.12^{**}	0.10^{*}	0.13^{**}	0.14^{**}	0.16^{***}
	(0.06)	(0.06)	(0.05)	(0.07)	(0.06)	(0.06)
High emission meal	0.00	−0.01	0.04	0.01	0.04	−0.01
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.03)
Low emission meal	−0.04	−0.05	−0.03	−0.05	−0.04	−0.04
	(0.03)	(0.04)	(0.04)	(0.04)	(0.04)	(0.03)
Control for third round	0.03	0.03	0.02	0.03	0.03	−0.00
	(0.04)	(0.05)	(0.05)	(0.04)	(0.04)	(0.04)
Participants control	94	79	75	78	78	70
Participants treated	146	116	121	105	122	106
Observations	1,148	940	932	916	956	868

Standard errors in parentheses

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

Notes: Dependent variable: within-subject change in willingness to pay for a meal, compared to baseline. Specifications correspond to Equation 1 and do not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. Regressions include only individuals who report above-average values for the respective items. "Hungry" is measured on a 10-point scale using the question "How hungry are you feeling now, in this moment?". "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". The perceived strength of social norms is measured using the procedure developed by Krupka and Weber (2013). These analyses were not pre-registered. Standard errors are clustered at the individual level.

B.12 Exp. 1: Effect on calorie guesses

Table B.29. Effects of the treatment on calories guessed in Experiment 1

	Guess of calories in				
	(1) Meat low	(2) Veg high	(3) Meat high	(4) Sandwich	(5) Veg low
Sees carbon labels	89.49	127.61	25.29	21.27	83.28
	(85.02)	(79.35)	(48.74)	(20.48)	(71.59)
Constant	608.00***	510.07***	708.04***	275.23***	521.23***
	(37.10)	(47.64)	(42.79)	(16.40)	(36.57)
Participants control	70	70	70	70	70
Participants treated	217	217	217	217	217
Observations	287	287	287	287	287

Standard errors in parentheses

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

Notes: To test whether participants conclude other meal characteristics when seeing carbon labels, I ask participants to guess the calories of different meals towards the end of the experiment. Participants in the TREATMENT see carbon labels during the guess, while participants in the CONTROL group do not. As pre-registered (Schulze Tilling, 2021b), I use this analysis as a proxy for whether participants use carbon labels to infer nutritional characteristics of the meals. I ask for guesses for the meals for which the non-vegetarian participants make decisions in the main part of the experiment, including respectively a vegetarian and a non-vegetarian meal low and high in carbon emissions. Standard errors are robust.

B.13 Exp. 3: Descriptives on under- and over-estimation

Table B.30. Under- and over-estimation of meal emissions

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

Notes: Based on participants in the Attent+Label treatment. I show under- and overestimation of the emissions caused by those meals that are also used in the experiment decisions. 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 B.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

Notes: 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 B.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

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

B.14 Exp. 3: Results using alternative definitions

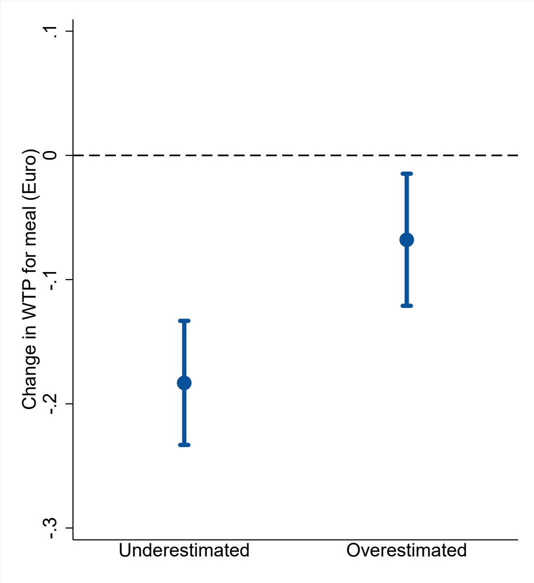


Figure B.5. Replication of Figure 13 based on under- or over-estimation of the meal. Notes: Figure based on under- or over-estimation of the meal instead of under- or over-estimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals.

	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 B.33. Replication of Table 4 based on under- or over-estimation of the meal. Notes: Regression based on under- or over-estimation of the emissions caused by the meal instead of under- or overestimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped.

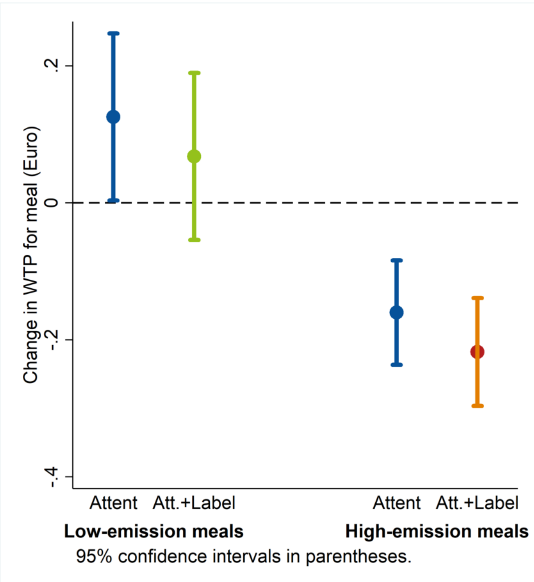


Figure B.6. Replication of Figure 14 with only accurate guesses, Notes: Includes only participant-meal combinations where emissions were guessed accurately enough to receive a bonus payment (guess within 20% of true value, 543 observations). Bars indicate 95% confidence intervals.

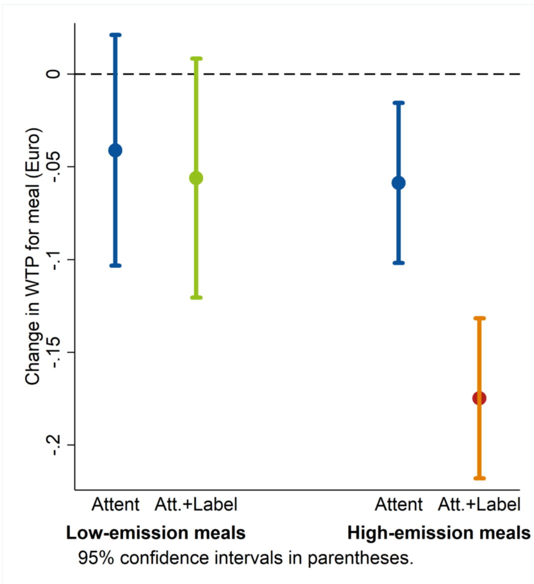


Figure B.7. Replication of Figure 14 with only inaccurate guesses. Notes: Includes only participant-meal combinations where emissions were not guessed accurately enough to receive a bonus payment (guess not within 20% of true value, 1,837 observations)

B.15 Participants' willingness to pay for the presence of carbon labels

Table B.34. Willingness to pay for seeing carbon labels by treatment group

	WTP for labels
	(1)
Control, then Labels	−0.13
	(0.08)
Labels, then Offset	−0.11
	(0.08)
Attent, then Attent	−0.08
	(0.07)
Attent+Label, then Offset	−0.07
	(0.07)
Attent+Offset, then Labels	−0.04
	(0.07)
Constant	0.28***
	(0.06)
Participants control, then Control	71
Participants Control, then Labels	69
Participants Labels, then Offset	148
Participants Attent, then Attent	151
Participants Attent+Offset, then Labels	149
Participants Attent+Label, then Offset	144
Observations	731

Standard errors in parentheses

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

Notes: 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 B.35. Correlation between willingness to pay for seeing carbon labels and individual characteristics

	WTP for the presence of carbon labels				
	(1)	(2)	(3)	(4)	(5)
Perceived strength of social norms	0.01**				
	(0.01)				
In favor of labels in student restaurant		0.03***			
		(0.01)			
Self-reported willingness to use info			0.03***		
			(0.01)		
Self-reported confidence in own knowledge				−0.01	
				(0.01)	
Eating self-control					0.00
					(0.01)
Constant	0.15***	−0.03	0.03	0.18***	0.21***
	(0.03)	(0.06)	(0.04)	(0.02)	(0.02)
Observations	731	731	731	731	731

Standard errors in parentheses

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

Notes: Note: Dependent variable: Willingness to pay for seeing labels (in Euro) 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).

Table B.36. Correlation between willingness to pay for seeing carbon labels and treatment effect

	WTP for labels	
	(1)	(2)
Estimate of individual's reaction to kg emissions	−0.06	
	(0.08)	
Estimate of individual's fixed reaction		−0.16
		(0.10)
Constant	0.19***	0.18***
	(0.02)	(0.02)
Participants Control, then Labels		69
Participants Labels, then Offset		148
Participants Attent+Offset, then Labels		149
Participants Attent+Label, then Offset		144
Observations	510	510

Standard errors in parentheses

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

Notes: Dependent variable: Willingness to pay for seeing labels for the final three consumption decisions. Independent variables: I perform the analysis shown in Col. (2) of Table 1 separately for each individual shown carbon labels during the experiment. Col. (1) regresses individual's willingness to pay for carbon labels on the coefficient I estimated for the individual for "Emissions(kg) x Shown label", i.e. the person's reaction dependent on emissions caused by the meal. Col. (2) regresses individual's willingness to pay for carbon labels on the coefficient I estimated for the fixed effect of "Shown label", i.e. the fixed reaction I estimate for this individual independent of meal emissions. The coefficients suggest that there is a correlation between showing a stronger reaction to carbon labels and being willing to pay a higher amount to be shown the labels.

Appendix C Experiment 2: Additional tables and figures

C.1 Time trends

Table C.1. Regression coefficients for the event plot in Figure 9

	(1) Choice of meat
Treated × Week 1	-4.88*
	(2.63)
Treated × Week 2	-1.66
	(2.26)
Treated × Week 3	2.71
	(2.49)
Treated × Week 5	-6.62***
	(2.48)
Treated × Week 6	-1.32
	(2.14)
Treated × Week 7	-5.17**
	(2.30)
Treated × Week 8	-4.47
	(2.75)
Treated × Week 9	-4.29*
	(2.28)
Treated × Week 10	-4.79
	(3.56)
Treated × Week 11	-3.57
	(2.48)
Treated × Week 12	-3.55
	(2.44)
Treated × Week 13	-7.29***
	(2.38)
Treated × Week 14	-7.68***
	(2.53)
Guests control	1,015
Guests treated	348
Observations	29,401

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

Notes: Dependent variable: 0/1 indicator for consumption of the meat option, multiplied by 100 to enable the interpretation of coefficients as percentage points. Regression additionally includes weekly controls, day-of-the-week controls,guest fixed effects, and canteen-level controls assigned according to ITT classification.

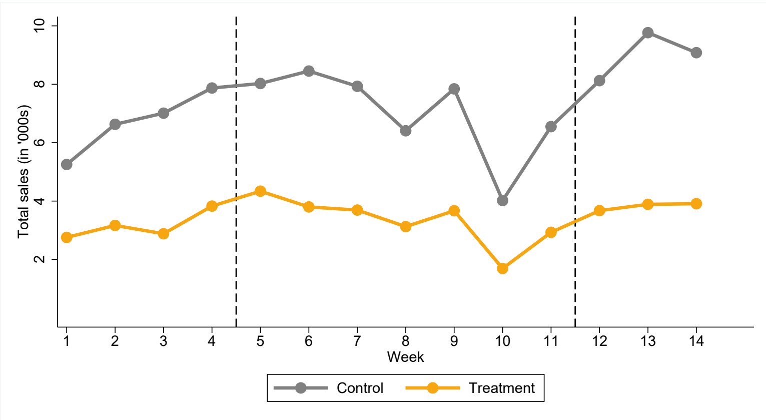


Figure C.1. Weekly student canteen sales of main meal components

Note: Raw aggregate sales of main meal components, excluding sales to Ukrainian refugees $N = 150{,}320$. 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.

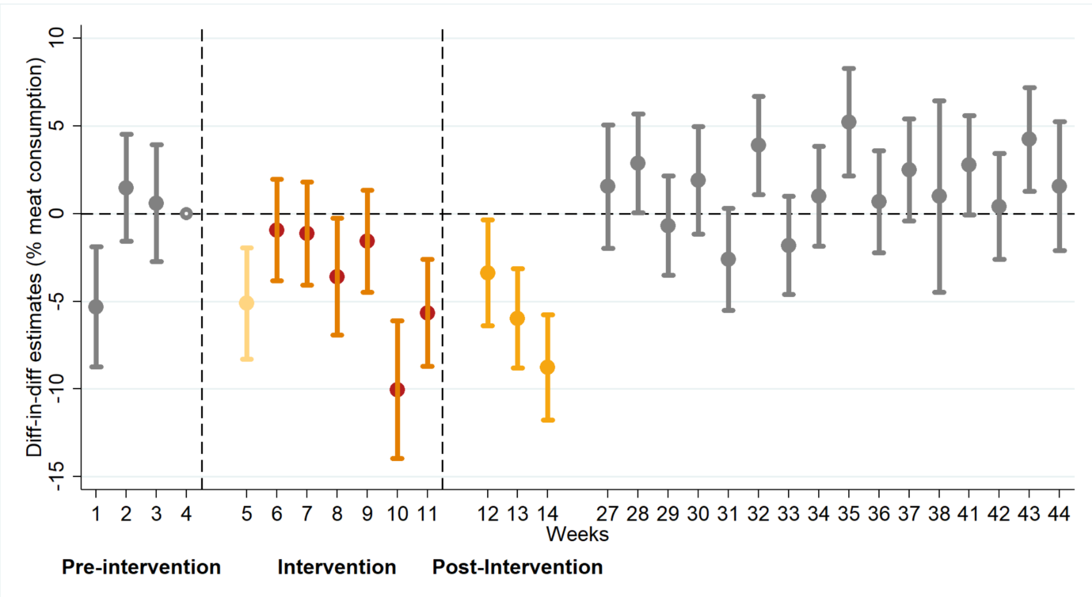


Figure C.2. Event study including data from the following semester

Note: 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 (2) in Table 2. An ITT analysis and inclusion of guest fixed effects is not possible in this data set, since individuals' anonymized ID numbers differ between the study period and the following semester. Weeks 25 to 26 are excluded due to the semester break. Weekly time controls and day-of-the-week controls are included. Bars indicate 95% confidence intervals.

C.2 Additional pre-registered main effects Experiment 2

The preregistration to Experiment 2 can be found under https://aspredicted.org/sc53-s3c9.pdf. I pre-registered to examine:

(1) The effect of the labels on meat consumption, during and after the intervention. This analysis is shown in the main text.

(2) The effect of the labeling intervention on canteen guests' likelihood of choosing a green-labeled, yellow-labeled, or red-labeled meal. However, the canteen usually only offers two meals (usually one green meal and one yellow or red-labeled meal), and the type of meal offered might also influence the groups of students deciding whether or not to go to the canteen. This makes a standard difference-in-difference analysis questionable, as it might also pick up changes in the guest composition. Below tables show results nevertheless. Table B.7 uses all data, but restricts observations in the respective columns to days on which e.g. green-labeled and yellow-labeled meals were on offer, vs. green vs red. labeled meals, etc. Table C.3 uses the ITT sample and includes guest fixed effects. This specification controls for the composition of canteen guests differing between the different offer days. It suggests that canteen guests move sways from red-labeled meals towards green-labeled meals (Yellow meals and red labeled meals are never offered together during the study period). Table C.2 using all data additionally suggests that guests might consume less green meals in favor of more yellow-labeled meals, but this pattern does not repeat in the ITT sample analysis.

(3) The effect of the labeling intervention on greenhouse gas emissions. As detailed in section C.3 below, this is also not straight-forward to examine due to differences in meat consumption between treatment and control group pre-intervention, paired with a change in the greenhouse gas emissions of the meals on offer between pre-intervention and intervention period. Table C.4 performs Spec. (1) of the main results table 2 on the full and on the ITT sample. Col. (1) and (3) do not use any additional controls and find no evidence of a decrease in greenhouse gas emissions caused by the labels. Col. (2) and (4) additionally controls for the emissions caused by the respective meat meal and vegetarian meal on offer on a given day. These meals influence the total greenhouse gas emissions of the control and treatment meals differently, since the meat emissions matter less, and the vegetarian emissions matter more for the treatment canteen, since the proportion of veg. meals consumed at baseline is higher. I thus additionally include an interaction between meat and vegetarian option and treatment canteen as controls. Col. (4) includes the same controls, but assigns the interaction on an ITT basis. Col. (2) suggests that emissions decreased by 70 gram per meal, while Col. (4) suggests a decrease of 50 gram. An alternative way to analyze the effect of the treatment on greenhouse gas emissions is shown in section C.3.

(4) The effect of the labels on guest numbers. Figure C.1 shows that sales developed similarly in the two canteens throughout the sample period. As an additional analysis, Table C.5 expands the ITT sample such that it becomes a panel data set, filling in zeros for days on which an individual guest did not visit the canteen. In Table C.5 I then repeat the main ITT analysis from Column (4) and (5) Table 2 using a canteen guest's decision to visit or not visit the canteen as the outcome variable. The coefficient during the labeling intervention period is positive and insignificant, suggesting no effect of the intervention on guests' likelihood of frequenting the canteens. The coefficient for the post-intervention period is significant and negative. However, it seems unlikely that the labeling intervention caused a decrease in canteen visits during the post-intervention period. Instead, this coefficient might be picking up differences in canteen guests' likelihood of frequenting the canteens as the semester fades out. Specifically, treatment canteen guests might have been less present on campus during the last weeks of the semester. Note that the main ITT analysis including individual fixed effects should not be influenced by changes in canteen frequenting behaviors.

Table C.2. Pre-registerd binary outcomes, using all data

	Full sample					
	Green vs. Yellow	Green vs. Red	Yellow	Red	Fish/Meat	Veg.
Treatment restaurant x Label period	-0.05**	0.02*	0.05***	-0.02*	-0.02***	0.02***
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Treatment restaurant x Post period	-0.19***	0.05***	0.19***	-0.05***	-0.06***	0.06***
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Treatment restaurant	0.12***	0.09***	-0.12***	-0.09***	-0.10***	0.10***
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Label period	0.04***	-0.01	0.08***	0.01	0.01**	-0.01**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Post period	0.09***	0.02***	0.07***	-0.02***	-0.00	0.00
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Constant	0.46***	0.51***	0.54***	0.49***	0.51***	0.49***
	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)
Guests control	1,192	4,382	1,495	4,382	6,995	6,995
Guests treated	484	1,819	592	1,819	2,754	2,754
Observations	22,220	76,134	28,159	76,134	121,371	121,371

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

Table C.3. Preregistered binary outcomes, using individual guest data

	ITT sample					
	Green vs. Yellow	Green vs. Red	Yellow	Red	Fish/Meat	Veg.
Treatment restaurant x Label period	0.05	0.03*	-0.03	-0.03*	-0.03***	0.03***
	(0.03)	(0.01)	(0.04)	(0.01)	(0.01)	(0.01)
Treatment restaurant x Post period	-0.19***	0.04**	0.15***	-0.04**	-0.05***	0.05***
	(0.06)	(0.02)	(0.05)	(0.02)	(0.01)	(0.01)
Label period	0.04**	-0.00	0.08***	0.00	-0.01*	0.01*
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Post period	0.14***	0.02**	0.03	-0.02**	-0.02**	0.02**
	(0.03)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Guest fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Guests control	147	623	187	623	965	965
Guests treated	49	233	55	233	334	334
Observations	5,134	17,189	6,413	17,189	27,640	27,640

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

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

Table C.4. Average greenhouse gas emissions (in kg) as outcome variable

	Full sample		ITT sample	
	Basic spec.	With controls	Basic spec.	With controls
Treatment restaurant x Label period	0.02	-0.07***		
	(0.02)	(0.02)		
Treatment restaurant x Post period	0.03	-0.07***		
	(0.02)	(0.01)		
ITT guest × Label period			-0.03	-0.05*
			(0.04)	(0.03)
ITT guest × Post period			-0.01	-0.03
			(0.04)	(0.03)
Label period	-0.27***	0.01	-0.31***	-0.04***
	(0.01)	(0.01)	(0.02)	(0.01)
Post period	-0.31***	-0.02***	-0.32***	-0.05***
	(0.01)	(0.01)	(0.02)	(0.01)
Treatment restaurant	-0.15***	0.12***		
	(0.02)	(0.03)		
Emissions meat meal		0.55***		0.52***
		(0.01)		(0.02)
Emissions veg. meal		0.50***		0.44***
		(0.01)		(0.03)
Treatment restaurant × Emissions veg. meal		0.09***		
		(0.02)		
Treatment restaurant × Emissions meat meal		-0.16***		
		(0.02)		
ITT guest × Emissions veg. meal				0.15***
				(0.06)
ITT guest × Emissions meat meal				-0.06*
				(0.03)
Constant	1.25***	-0.07***	1.25***	0.02
	(0.01)	(0.02)	(0.01)	(0.03)
Week fixed effects	No	No	Yes	Yes
Guest fixed effects	No	No	Yes	Yes
Guests control	6,937	6,937	967	967
Guests treated	2,812	2,812	328	328
Observations	121,371	121,371	27,640	27,640

Standard errors in parentheses

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

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

Table C.5. Decision to visit one of the student canteens as outcome variable

	ITT sample	
	Guest FE	Date+Guest FE
Treat x Inter period	0.56	0.71
	(1.38)	(1.38)
Treat x Post period	-4.55**	-4.32**
	(1.80)	(1.80)
ITT control for second veg. offered	-0.70	0.79
	(0.59)	(0.83)
ITT control for second meat offered	2.87***	0.95
	(0.95)	(1.20)
Constant	41.90***	36.70***
	(1.00)	(1.36)
Week fixed effects	Yes	Yes
Guest fixed effects	Yes	Yes
Guests control	1,022	1,022
Guests treated	341	341
Observations	42,253	42,253

Standard errors in parentheses

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

Notes: Dependent variable differs by column. Column 1: 0/1 indicator for whether guest visited canteen. Column 2: 0/1 indicator for whether guest visited canteen and consumed meat. Column 3: 0/1 indicator for whether guest visited the canteen and consumed the vegetarian option. Dependent variables are multiplied by 100 to enable the interpretation of coefficients as percentage points.

C.3 Effect on carbon footprint

The average emissions of the meals on offer indeed varied substantially between the pre-intervention and intervention period, due to a changing offer (see Figures E.4 and E.3 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, an unrestricted difference-in-difference would pick up changes in emissions due to changes in offer, and falsely attribute these to the label.

To illustrate this problem: 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 difference-in-difference 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. The opposite is the case in a scenario in which the emissions of the meat meal on offer decrease, i.e. the meat meal with 1.2 kg of emissions is offered on the first and the meat meal with 1 kg of emissions is offered on the second day. The analysis then identifies an increase in emissions caused by the carbon labels, although again consumer behavior did not change.

The situation in the student canteens in the study context is similar to the second case: In the pre-intervention period, emissions of the average meat meal are 2.1 kg, while they are 1.5 kg in the intervention period. Emissions of the average vegetarian meal are similar. At the same time, there are large differences in meat consumption between canteens, with on average 41% of meals consumed in the treatment canteen pre-intervention containing meat and 50% of meals consumed in the control canteen pre-intervention containing meat.

I approach this problem in different ways: The main text (section 3.1.3) includes a back-of-the-envelope calculation approximating emission savings in the absence of any changes in meal offer. Table C.4 includes controls for student canteen offer. To provide an additional check to the back-of-the-envelope calculation above, I additionally perform an analysis on a subset of the data set. 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, for any day I assign the emissions caused by the main meal components sold in the treated canteen to any additional sales outside of the the main meal components. The restricted sample contains 36,198 observations. As shown in Table C.6, I estimate that labels reduce average emissions per meal by 90 grams or around 8% of the emissions of a baseline meal.

Table C.6. Effect of labels on average emissions per meal

	Full sample		
	Base	Week FE	Date FE
Treatment restaurant × Label period	-0.07*	-0.05	-0.09**
	(0.04)	(0.04)	(0.03)
Treatment restaurant	-0.17***	-0.22***	-0.21***
	(0.03)	(0.02)	(0.02)
Label period	-0.12***		
	(0.02)		
Constant	1.36***	0.42***	1.23***
	(0.02)	(0.03)	(0.02)
Date fixed effects	No	Yes	Yes
Guest fixed effects	No	No	No
Guests control	5,148	5,148	5,148
Guests treated	2,067	2,067	2,067
Observations	36,198	36,198	36,198

Standard errors in parentheses

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

Notes: Dependent variable: Emissions caused by main meal component, in gram. The sample is restricted to days in the intervention period for which there is a "gastronomic twin" in the pre-intervention period. Regression follows Spec. (1) and (2) in Table 2, using greenhouse gas emissions instead of the choice of the meat meal as the outcome variable. Spec. (2) exchanges the "Label period" indicator for week and day-of-the-week controls. Spec. (3) includes date-specific controls.

C.4 Heterogeneity in treatment effects

Table C.7 examines treatment effects in different subsamples, using Spec. (4) of Table 2. Treatment effects seem weaker when restricting the sample to only employees (col. 2), and slightly stronger when restricting to off-peak visit hours (col. 3). This might be due to the labels being more salient in a less busy environment. Table C.8 shows analyses restricting the sample to guests who pay by individual payment card (Col. 1) and for whom I have demographic information using the survey data (Col. 2-6). This suggestive analysis indicates that treatment effects are stronger for females, and slightly stronger for canteen guests below 24 of age. The heterogeneity analyses for Experiment 1 (Table B.28) show similar results with respect to gender and age.

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

	Likelihood of consuming meat		
	All	Employees	Off peak
ITT guest × Label period	-0.03^{***}	-0.05	-0.04^{***}
	(0.01)	(0.04)	(0.02)
ITT guest × Post period	-0.06^{***}	-0.12^{**}	-0.09^{***}
	(0.01)	(0.06)	(0.02)
ITT control for second veg. offered	-0.01	-0.02	0.00
	(0.01)	(0.01)	(0.01)
ITT control for second meat offered	0.02^{***}	0.03^{*}	0.02^{**}
	(0.01)	(0.02)	(0.01)
Constant	0.49^{***}	0.66^{***}	0.49^{***}
	(0.01)	(0.02)	(0.01)
Week fixed effects	Yes	Yes	Yes
Guest fixed effects	Yes	Yes	Yes
Guests control	1,021	150	556
Guests treated	342	28	217
Observations	27,640	3,851	15,393

Standard errors in parentheses

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

Notes: Dependent variable: 0/1 indicator for consumption of the meat option. Linear probability model regression following spec. (4) in Table 2. Col.(1) includes all ITT data, Col.(2) only university employees, and Col.(3) excludes peak hours (midday until 1 PM). Standard errors are clustered at the individual level.

Table C.8. Effect of labels on meat consumption, different subsamples

	Likelihood of consuming meat				
	All	Survey	Female	Below 24	Env. important
ITT guest × Label period	-0.03^{***}	-0.06^{***}	-0.11^{***}	-0.06^{***}	-0.05^{**}
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)
ITT guest × Post period	-0.06^{***}	-0.07^{***}	-0.13^{***}	-0.07^{***}	-0.08^{**}
	(0.01)	(0.02)	(0.03)	(0.03)	(0.03)
ITT control for second veg. offered	-0.01	0.01	0.00	0.00	0.02
	(0.01)	(0.01)	(0.02)	(0.01)	(0.02)
ITT control for second meat offered	0.02^{***}	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Constant	0.49^{***}	0.35^{***}	0.23^{***}	0.35^{***}	0.16^{***}
	(0.01)	(0.02)	(0.03)	(0.02)	(0.02)
Week fixed effects	Yes	Yes	Yes	Yes	Yes
Guest fixed effects	Yes	Yes	Yes	Yes	Yes
Guests control	1,021	209	92	149	110
Guests treated	342	122	50	84	51
Observations	27,640	6,743	2,773	4,777	3,144

Standard errors in parentheses

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

Notes: Dependent variable: 0/1 indicator for consumption of the meat option. Linear probability model regression following spec. (4) in Table 2. Col.(1) includes all ITT data, Col.(2) only student canteen guests who participated in the pre-intervention field survey. Col.(3) includes, of these, only females. Col.(4) includes only under 24-year olds. Col.(5) includes only survey participants who report that environmental aspects play an important role in their food consumption decisions. Standard errors are clustered at the individual level.

C.5 Field survey results

Below I describe the results of surveys conducted in the control and treatment canteens pre- and post- intervention, as described in section E.8.

Did canteen guests see the labels? Of the post-survey respondents, 373 went to the treated student canteen at least once during the intervention period. 70% of these report having seen the labels. 425 respondents did not go to the treated canteen during the intervention period, according to their individual student canteen cards. However, they might have in fact still gone, but not paid with their individual cards. Of these respondents, 8% report having seen the labels. 214 respondents went to the treated restaurant at least four times during the intervention period. 80% of these guests report having seen the labels.

Do canteen guests feel they reacted to the labels? Of the post-survey respondents who noticed the labels and visited the treated student canteen at least once during the intervention period, 18% report having incorporated the labels in their decisions (agreement of 4 or 5 on a 5-point scale asking how strongly participants incorporated the labels in their choices). Of those who visited the canteen more frequently and saw the labels (172 participants), 16% report having incorporated the labels in their decisions.

How do canteen guests make their consumption choices? 34% of guests report making their choice mainly using the information given on the canteen website. 30% mainly use the digital billboards. 36% report mainly deciding by looking at the food counters. Figure 8 shows how the carbon labels were shown in each of these decision contexts. Of the three decision contexts, the carbon labels were most salient on the canteen website. Table C.8 shows how treatment effects differ for guests making their decisions online. Results suggest that effects are stronger for this group.

Do the carbon labels affect other attitudes? I do not find any clear evidence of the carbon labels affecting my measure of support for a carbon tax or for command-and-control measures. Table C.9 shows the results of a difference-in-difference analysis of the treatment on political attitudes elicited in the pre- and post-intervention survey. Columns (1) and (2) include all survey data, using participants' self-report of their most frequented canteen to classify guests as treatment or control, and including individual fixed effects to control for prior individual attitudes. Columns (3) and (4) include only relatively frequent canteen gusts and classify guests as treated or control based on behavior pre-intervention. While Col. (1) and (2) suggest that the treatment might have slightly decreased support for command and control instruments but increased support for a carbon tax, Col. (3) and (4) do not show any such pattern. Similarly, Table C.10 investigates possible effects of the treatment on the experience of eating in the canteen, and content with the canteen and the university in general. Also here, results are mixed and there is no clear evidence of the labels affecting any of these measures.

Table C.9. Effect of the labels on attitudes towards other political measures

	Full sample		ITT restricted sample	
	(1)	(2)	(3)	(4)
Treatment × Post period	−0.12	0.14	0.01	0.02
	(0.08)	(0.09)	(0.11)	(0.14)
Post period	0.03	0.07	−0.02	0.16
	(0.06)	(0.07)	(0.08)	(0.10)
Constant	4.28***	4.38***	4.24***	4.33***
	(0.03)	(0.03)	(0.04)	(0.05)
Guest fixed effects	Yes	Yes	Yes	Yes
Guests control	335	335	177	177
Guests treated	605	605	209	209
Observations	1,880	1,880	772	772

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

Notes: Dependent variable: Spec. (1) and (3): "It should be prohibited to build new houses not adhering to cur- rent environmental standards." Spec. (2) and (4): Agreement with the statement "Flying should be more expensive since it is bad for the environment."All are measured on a 7-point scale. I only include individuals who participated in the pre-and the post- survey. In Spec. (1)-(2), I classify an individual as treated if they self-report to mainly visit the treatment canteen. In Spec. (3)-(4), I classify individuals based on their matched consumption data: I only include individuals who ate at one of the canteens at least five times during the pre-intervention period, and only include individuals who eat at either the treatment or one of the control canteens in 80% of these visits. I classify intention to treat accordingly. In all specifications, I include guest fixed effects to control for initial attitudes. Standard errors are clustered at the individual level.

Table C.10. Effect of the labels on happiness with the canteen and university

	Full sample				ITT restricted sample			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treatment × Post period	0.15*	0.13	−0.10	0.06	−0.18	−0.28**	0.13	−0.01
	(0.09)	(0.09)	(0.10)	(0.08)	(0.14)	(0.13)	(0.15)	(0.12)
Post period	−0.10	−0.08	0.09	−0.01	0.16	0.18*	−0.01	0.16*
	(0.07)	(0.07)	(0.08)	(0.06)	(0.10)	(0.10)	(0.11)	(0.09)
Constant	4.39***	4.40***	3.45***	3.44***	4.34***	4.35***	3.44***	3.40***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)	(0.04)
Guest fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Guests control	335	335	335	335	177	177	177	177
Guests treated	605	605	605	605	209	209	209	209
Observations	1,880	1,880	1,880	1,880	772	772	772	772

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

Notes: Dependent variable: Spec. (1) and (5): ""Eating in the student canteen is a nice experience for me." Spec. (2) and (6): ""I feel comfortable in the student canteen." Spec. (3) and (7): ""I feel that my wishes are taken into account in the canteen's offer." Spec. (4) and (8): content with the University of Bonn in general. All are measured on a 7-point scale. I only include individuals who participated in the pre-and the post- survey. In Spec. (1)-(4), I classify an individual as treated if they self-report to mainly visit the treatment canteen. In Spec. (5)-(8), I classify individuals based on their matched consumption data: I only include individuals who ate at one of the canteens at least five times during the pre-intervention period, and only include individuals who eat at either the treatment or one of the control canteens in 80% of these visits. I classify intention to treat accordingly. In all specifications, I include guest fixed effects to control for initial attitudes. Standard errors are clustered at the individual level.

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

D.1 Pre-registration

I pre-registered Experiment 3 on June 21st 2021 under #AEARCTR-0007858 and Experiment 1 on October 24th 2021 under #AEARCTR-0008435.

D.2 Meals used for elicitation

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

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

D.3 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.

D.4 Decisions under carbon offsetting

In the Attention+Offset condition in Experiment 3 and the Offset condition in Experiment 1, participants are informed that, if one of the decisions made in this treatment is implemented, the emissions of the meal provided to them (regardless of whether it is the warm meal or the cheese sandwich) are offset by the experimenter with a donation to Atmosfair. The example screens in Subsection D.5 show how this is communicated to experiment participants.

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

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

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

Table D.1. Familiarity with carbon offsetting

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

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

Table D.2. Beliefs on carbon offsetting effectiveness

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

Standard errors in parentheses

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

D.5 Experiment screens (English translation)

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

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

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

Decision description screen - Screenshot 1/4

Description of upcoming decisions | Comprehension questions

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

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

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

How do your decisions affect your payout?

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

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

- Chance decides whether you will receive your favourite dish for free:
  - Case 1 (50% probability): You will receive your favourite dish for free.
  - Case 2 (50% probability): You will be assigned the non-preferred dish first. In this case, specify the maximum amount of your expense compensation you would like to forgo in order to receive your favourite dish instead.

- If case 2 occurs, it is again a matter of chance:
  - A **surcharge** is determined at random. Any value between €0 and €3 (in 5 cent increments) is equally probable.
  - If the amount you have declared is more than the surcharge, you will receive your preferred dish. For this, the surcharge will be deducted from your expense compensation.
  - If the amount you specify is less than the surcharge, you will receive the non-preferred dish free of charge.

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

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

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

Example decision

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

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

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

Example scenario 1

Assuming you made the following decision:

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

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

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

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

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

This means for you:

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

Decision description screen - Screenshot 3/4

Example scenario 2

Assuming you made the following decision:

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

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

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

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

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

This means for you:

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

Decision description screen - Screenshot 4/4

Example scenario 3

Assuming you made the following decision:

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

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

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

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

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

This means for you:

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

Continue to the questions

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

Comprehension questions - Screenshot 1/3

Description of upcoming decisions | Comprehension questions

Comprehension questions

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

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

Question 1

Assuming you made the following decision:

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

Baked Feta Cheese with Rice

vegetarian

Baked Feta Cheese with Rice

oder

Cheese Roll

Details: vegetarian

Cheese Roll

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

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

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

What do you receive?

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

Question 2

Assuming you made the following decision:

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

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

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

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

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

What do you receive?

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

Question 3

Assuming you made the following decision:

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

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

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

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

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

What do you receive?

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

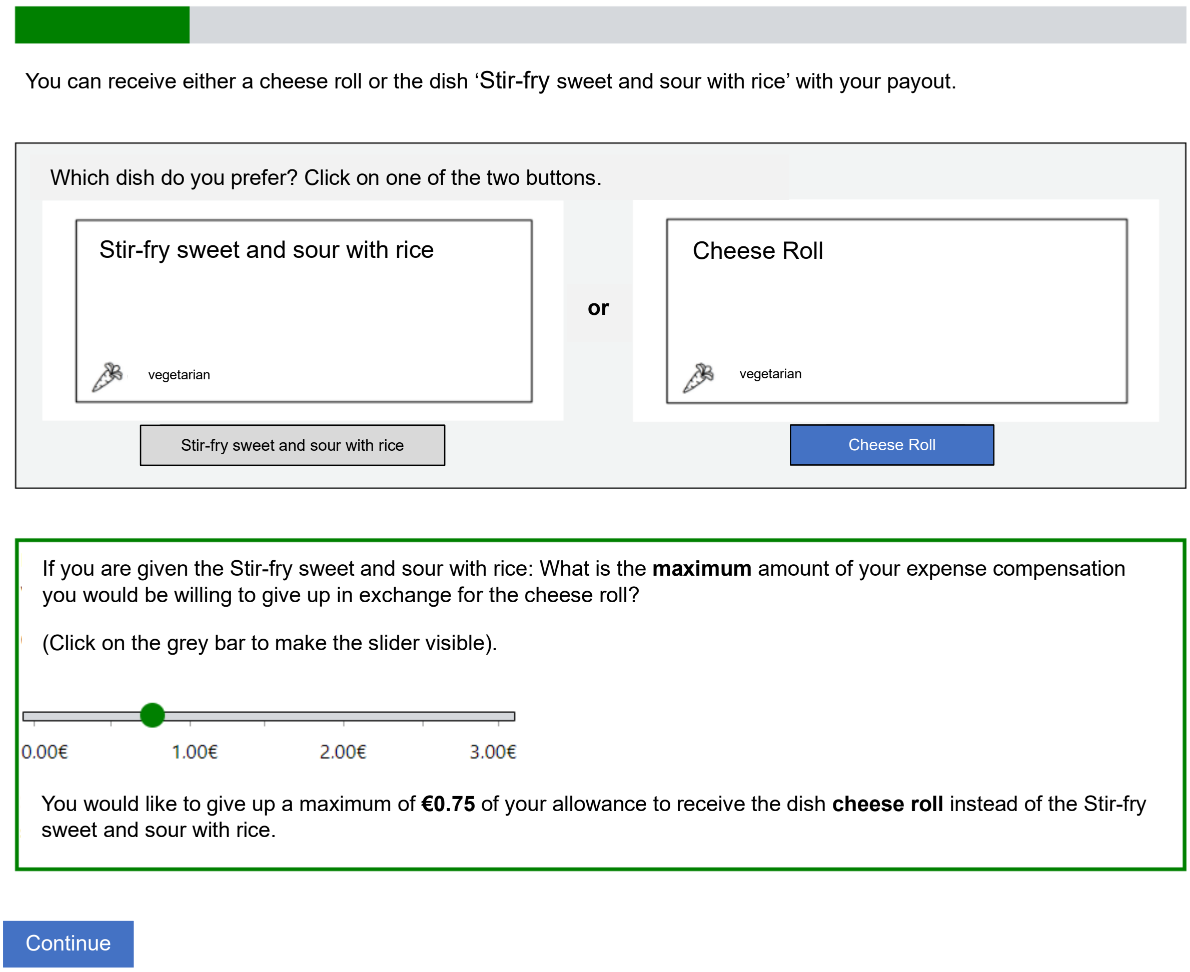
Question 4

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

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

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

Example baseline decision



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

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

Stir-fry sweet and sour with rice

vegetarian

or

Cheese Roll

vegetarian

Stir-fry sweet and sour with rice

Cheese Roll

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

Continue

Introduction carbon footprint estimation

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

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

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

Your reference value:

Red Thai-Curry with Pork and Rice

CO_2 Causes ≈ 1,7 kg CO_2
≈ 8,5 km /Car drive

Pork

Which assumptions should be taken for the guessing questions?

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

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

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

Continue

Example carbon footprint estimation

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

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

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

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

kg.

Continue

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

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

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

What assumptions are made in the calculation?

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

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

What data is the calculation based on?

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

Source: eaternity.

Continue

Example decision with labels

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

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

Stir-fry sweet and sour with rice

CO_2 Causes 0,4 kg CO_2 ≈ 2,0 km Car drive

vegetarian

or

Cheese Roll

CO_2 Causes 0,7 kg CO_2 ≈ 3,5 km Car drive

vegetarian

Stir-fry sweet and sour with rice

Cheese Roll

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

Continue

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

What assumptions are made in the calculation?

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

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

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Source: eaternity.

Introduction decisions with offsetting

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

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

For those interested: Further information on CO2 offsetting:

How does the CO2 offset work?

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

Example projects

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

Source: atmosfair

Continue

Example decision with offsetting

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

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

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

Italian Vegetable ragout with pasta	or	Cheese Roll
CO_2 neutral Emission offset vegetarian		CO_2 neutral Emission offset vegetarian
Italian Vegetable ragout with pasta		Cheese Roll

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

0.00€ 1.00€ 2.00€ 3.00€

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

Continue

For those interested: Further information on CO_2 offsetting:

<u>How does the CO_2 offset work?</u>

The donation to atmosfair is used to develop renewable energies in countries where they hardly exist yet, i.e. mainly in developing countries. In this way, atmosfair saves CO_2 that would otherwise have been produced by fossil energies in these countries.

<u>Example projects</u>

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

Source: atmosfair

Introduction calorie estimation

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

What assumptions should be made for the estimation?

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

Continue

Example calorie estimation

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

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

Beef ragout with potatoes

CO_2 Causes 3,4 kg CO_2

≈ 17,0 kmCar drive

Beef

I estimate that the dish 'Beef ragout with potatoes' has

kcal.

Continue

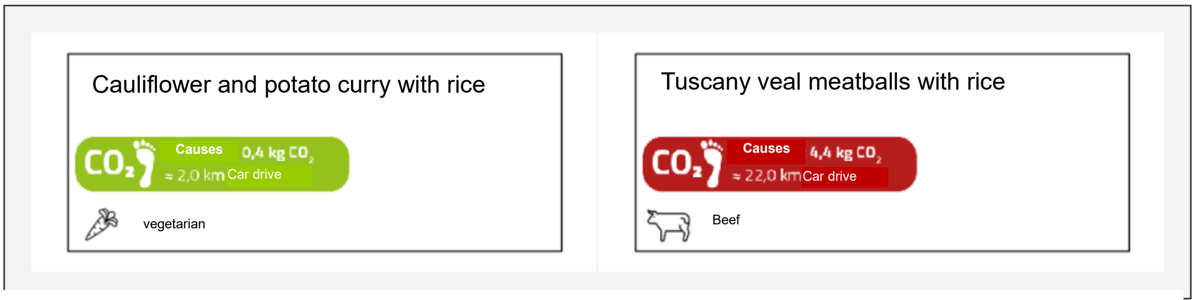
WTP for label presence

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

But now there are two differences:

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

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



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

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

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

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

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

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

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

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

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

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

0.00€ 1.00€ 2.00€ 3.00€

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

Continue

Appendix E Experiment 2: Details on the experimental set-up

E.1 Pre-registration

I pre-registered Experiment 2 on the 25th of April 2022 on aspredicted #95108.

E.2 Canteen set-up in Bonn

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

There are three student canteens in Bonn: The treatment student canteen, the first control restaurant (located 1.7 km from the treatment restaurant), and the second control restaurant (located 4.7 km from the treatment restaurant and frequented much less than the other two restaurants). Menu planning is centralized among the three student canteens, and there is thus a large overlap in the daily offering. All three student canteens offer two main meal components, which differ daily but are mostly the same across student canteens. In addition, each of the student canteens might offer additional options, which are student-restaurant-specific. The larger control restaurant sometimes offers pizza or pasta in addition, and all student canteens might serve leftover main meal components from the previous day, soup, and side dishes. In the treatment restaurant, only the main meal components were equipped with carbon labels, and sides and leftover main meal components were not labeled. [74] Correspondingly, the dependent variable in my main regression is whether the main meal component a restaurant guest chooses contains meat or is vegetarian.

E.3 Canteen visiting patterns

An average student canteen guest visited the student canteen 10 times from April to mid-July. Around 34% visit 10 times or more, and around 15% 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. This suggests that switching between student canteens and other gastronomic offers is not frequent.

Do canteen guests regularly frequent multiple canteens? Figure E.1 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. 91% of those regularly frequenting canteens during these two weeks (i.e. at least twice) visit the same canteen at least 80% of the time. I classify guests as "Control" or "Treatment" guests based on these two weeks. Around 2% of purchases made by "Control" visitors are made in the treated restaurant in the remaining 12-week period, while around 5% of the canteen visits of those classified as "Treatment" guests are to one of the Control canteens. Figure E.1 calculates weekly statistics on switching and shows time

74. 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.

trends. It does not seem as if switching between canteens differed during the intervention period from post-intervention patterns, except for a small drop in treatment guests switching to the control canteen in week 5. Note, however, that week 5 is anyways excluded from the main analysis in Table 2 as explained in more detail in section E.5. Further, an analysis of daily restaurant guests shows that the labeling intervention does not seem to have led to a decrease in student canteen guests, relative to the control restaurant (see Figure C.1).

Note that the ITT specification shown in Table 2 by design controls for any change in canteen frequenting behavior induced by the intervention. Since I use an intent-to-treat specification, effect sizes are not impacted by possible increased switching between canteens. Further, since I include guest fixed effects, changes in average consumption behavior due to a mere change in the composition of canteen guests are controlled for.

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 E.2. 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.

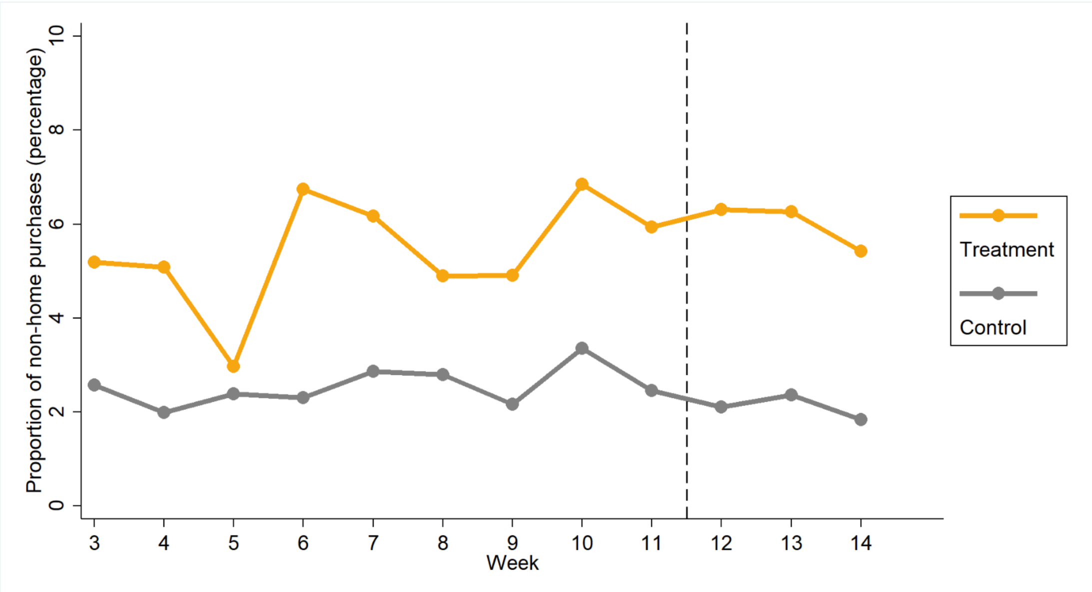


Figure E.1. Visits to the "non-home" canteen

Note: In percentage points relative to total canteen visits. Classification as the "home" canteen based on behavior in the first two weeks. The sample is similar to that in spec. (4) in Table 2, but the intention to treat is calculated based entirely on the first two weeks, based on a minimum of two visits during this period. $N = 39,318$

E.4 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 8. 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. [75]

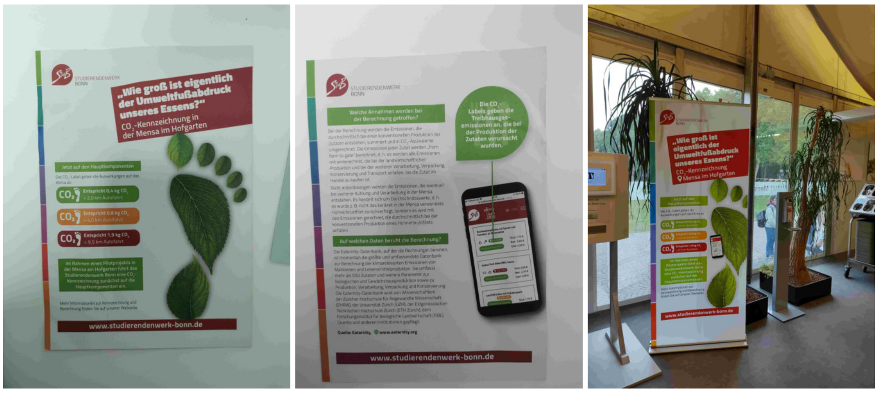


Figure E.2. Explanation of the carbon labeling initiative in the canteen

Note: Leaflets (left and center) and billboards at the entrance of the student canteen (right).

E.5 Data set construction: Full sample

The main data set covers purchase data from April 1st, 2022 to July 8th, 2022. Spec. (1) in Table E.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 9,26% in week 10, and between 13% and 17% for the rest of the observation period. For the control restaurant, they make up between 2% and 5% of total sales. Spec. (2) in Table E.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 9) can be attributed to the carbon labels. To be conservative, I exclude this week from the main analysis. Spec. (3) in Table E.1 additionally excludes week 5 from the sample.

75. 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.

- There are seven days on which the treatment restaurant and the larger control restaurant differed in the main meal components they offered. [76] 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 E.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.

76. Specifically, these seven days include: (1) one day on which both the meat and vegetarian main meal component offered in the treatment canteen were not the most-offered meal components in the control canteens, and (2) six days only one type of main meal component offered in the treatment canteen was also the most-sold respective meal component in control, and the other type of main meal component offered in treatment substantially differed to what was offered in control. I code this main meal component as substantially differing if both of the following conditions are met: First, the most-sold meal component sold in control differs in its characteristics (i.e. meat type, vegan or non-vegan, carb-heavy or not) to the most-sold meal component in treatment. Second, the most-sold meal component in treatment is not among the two most-sold meal components of its type in control.

Table E.1. Field estimates of the effect of carbon labels on meat consumption, testing robustness to different data exclusion criteria

	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***	-0.06***
	(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	-0.00
	(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	7,327	7,217	6,711	5,838
Guests treated	3,237	2,927	2,648	2,328
Observations	155,398	150,320	137,955	121,371

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

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

E.6 Data set construction: ITT sample

From the full sample data set detailed above, I construct the ITT sample data set:

- I restrict the sample to purchases made with a personal payment card (69% of purchases).
- Using the individual payment data, I can identify guests who purchased several meal components on a single day. These are 7% of the remaining sample. While the analyses on the full sample are at the level of the individual purchase (does the purchase contain meat?), the analyses on the restricted sample are at the level of the individual guest (does the guest eat meat on a given day?). If a guest purchases multiple main meal components, it is not clear whether they consume these themselves or whether they are paying for a friend. I thus drop all purchases made by a specific guest if they make multiple purchases on a given day.
- Further, I restrict the analysis to regular canteen guests, which I define as individuals who visited one of the student canteens at least four times during the pre-intervention period (41% of the remaining sample). Results are robust to different cut-off values, as Table E.2 shows.
- Finally, I restrict the sample to canteen guests visiting the same canteen in 80% of their visits (87% of the remaining sample). Results are robust to different percentage cutoff values, as Table E.3 shows.

Table E.2. Field estimates of the effect of carbon labels on meat consumption, testing robustness to different data exclusion criteria

	Likelihood of consuming meat				
	(1) > 4 visits	(2) > 2 visits	(3) > 3 visits	(4) > 5 visits	(5) > 6 visits
ITT guest x Label period	-0.03***	-0.02***	-0.03***	-0.03**	-0.03*
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
ITT guest x Post period	-0.05***	-0.04***	-0.05***	-0.04***	-0.04**
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
Constant	0.51***	0.49***	0.50***	0.53***	0.53***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Date effects	No	No	No	No	No
Fixed effects	Yes	Yes	Yes	Yes	Yes
Guests control	803	1,541	1,123	556	373
Guests treated	261	510	351	170	114
Observations	27,640	41,643	34,509	21,313	15,618

Standard errors in parentheses

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

Note: Spec. (1) conducts the ITT analysis following the above described data preparation procedure, i.e. guests are classified as regular student canteen guests if they visit the treatment canteen at least five times during the pre-intervention period. Col. (2) instead requires at least 2 visits, Col. (3) requires at least three visits, Col. (4) at least 5, and Col. (5) at least 6 visits. All specifications include individual fixed effects.

Table E.3. Field estimates of the effect of carbon labels on meat consumption, testing robustness to different data exclusion criteria

	Likelihood of consuming meat			
	(1) 80	(2) 60	(3) 70	(4) 90
ITT guest x Label period	-0.03***	-0.02**	-0.03***	-0.03***
	(0.01)	(0.01)	(0.01)	(0.01)
ITT guest x Post period	-0.05***	-0.05***	-0.05***	-0.06***
	(0.01)	(0.01)	(0.01)	(0.02)
Constant	0.51***	0.50***	0.50***	0.51***
	(0.00)	(0.00)	(0.00)	(0.00)
Date effects	No	No	No	No
Fixed effects	Yes	Yes	Yes	Yes
Guests control	803	875	848	744
Guests treated	261	292	275	231
Observations	27,640	30,259	28,841	25,453

Standard errors in parentheses

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

Note: Spec. (1) conducts the ITT analysis following the above described data preparation procedure, i.e. assigning guests as ITT if they visit the treatment canteen in at least 80% of their canteen visits pre-intervention. . Col. (2) instead uses a 60% assignment rule, Col. (3) uses a 70% assignment rule, and Col. (4) uses a 90% assignment rule. All specifications include individual fixed effects.

E.7 Descriptive statistics on meat consumption and average emissions

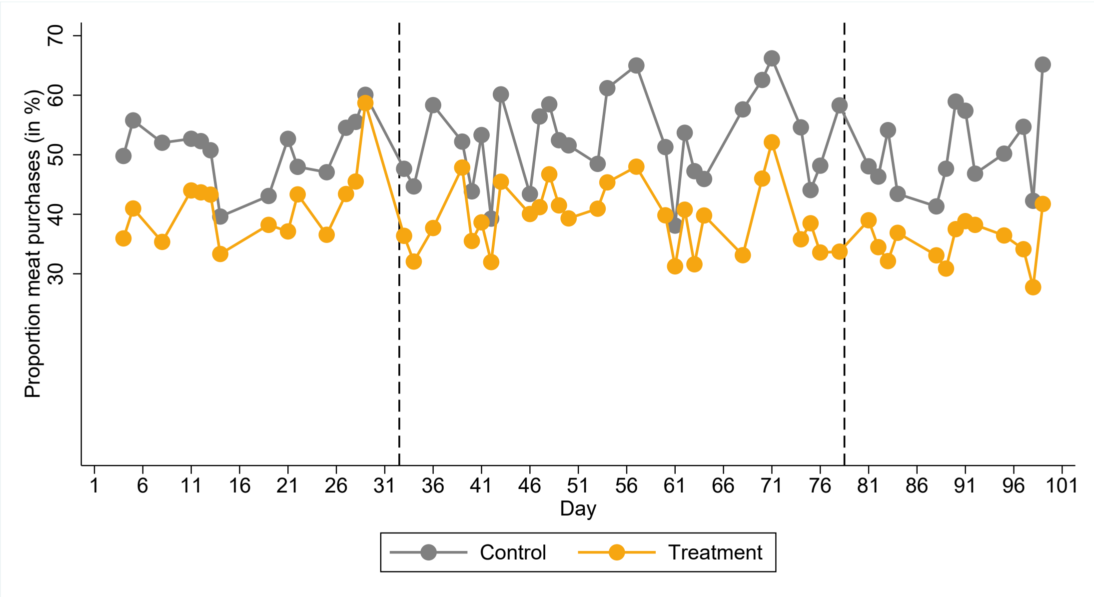


Figure E.3. Proportion of meat meals sold in the canteen

Note: using the final sample but including week 5. $N = 129, 166$

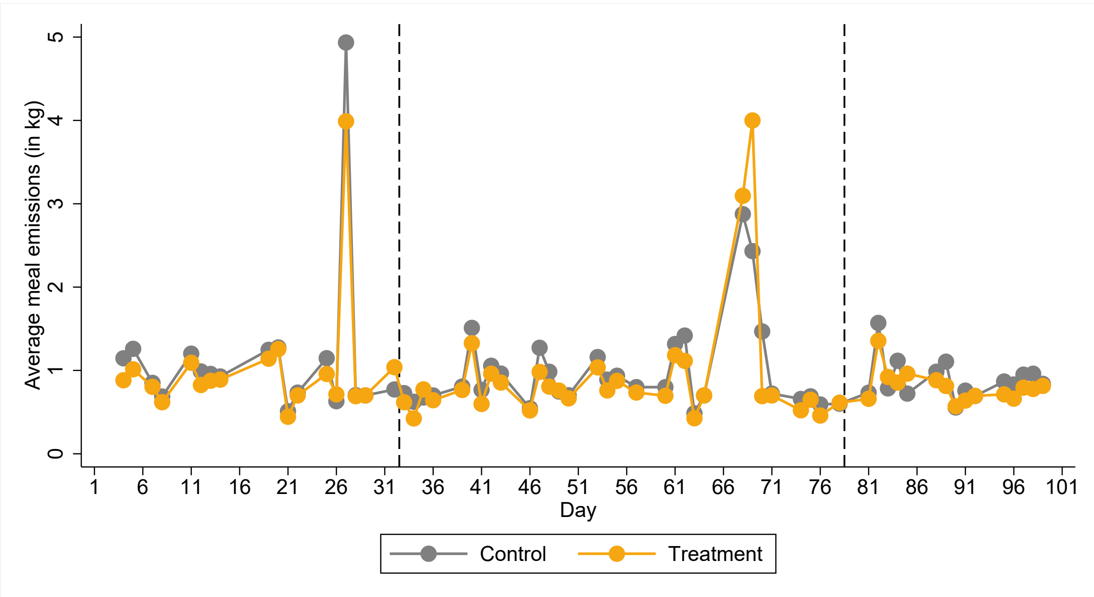


Figure E.4. Average emissions per meal sold in the canteen

Note: Using the final sample but including week 5. $N = 129, 166$

E.8 Survey accompanying natural field experiment

Pre-intervention survey: During the second week of April, I conducted a survey among student canteen guests at the treatment student canteen and the first, larger, control restaurant. The survey was advertised as an opportunity to voice one's opinion on the offer of the student canteen, took participants around five minutes, and motivated potential participants with the chance to win one of ten €50 coupons for the student canteen. The survey was advertised through multiple channels. First, I put up posters advertising the survey in many faculties throughout the University of Bonn. Second, I distributed leaflets in front of the treatment restaurant and the larger control restaurant, together with research assistants (see Figure E.5). It is common for students and student groups to advertise surveys, projects, and events in this manner. Finally, the experimental lab at the University of Bonn sent out an e-mail to its entire participant pool advertising participation.

Figure E.5. Leaflet advertising participation in the survey

Note: Leaflet was distributed in front of the student canteen.

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

Post-intervention survey: From the 22nd of June, I started sending out invitations to participate in a second survey. These were sent out by e-mail to those participants of the first survey who indicated their e-mail addresses and consented to be contacted for a second survey. This was the case for 93% of participants in survey 1. Of the 1,558 I invited to the survey, 940 filled out survey 2. I invited participants in a staggered fashion over two weeks and sent a reminder on the 7th of July. Again, survey respondents had the opportunity to win one of ten 50 €coupons for the student canteen.

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

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