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

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Abstract

This project provides evidence that carbon labels effectively change consumption behavior by directing attention and correcting misperceptions about carbon impact. Using lab-in-the-field experiments conducted in a student restaurant setting at the University of Bonn, I estimate that carbon labels have a similar impact as a carbon tax of €120/ Ton. This amount is four-fold the current German carbon tax on energy and transport. While it's commonly believed that carbon labels mainly impact behavior by correcting misperceptions about carbon impact, I show that this channel only partially explains consumption changes. I provide evidence that the direction of attention is the more influential channel, suggesting that carbon labels can also be effective in more knowledgeable populations. I also examine participants' willingness to pay to see or avoid labels, finding no evidence of labels imposing disproportionate psychological costs. A large-scale field experiment supports the lab-in-the-field results, with effects persisting over a five-week label period and in the three-week post-intervention observation period. Finally, a post-intervention survey reveals that over 70% of participants favor a permanent installation of carbon labels.

Keywords: Behavioral Intervention, Field Experiment, Food Consumption

JEL codes: D12, C91, C93, Q18

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1 INTRODUCTION

The food system causes 26% - 34% of global greenhouse gas emissions ((Poore and Nemecek, 2018), (Crippa et al., 2021)). Clark et al. (2020) predict that even if we eliminated fossil fuels immediately, emissions from the global food system alone would make it impossible to limit warming to 1.5° and even difficult to realize the 2° target. Shifting towards diets with lower carbon footprints would greatly reduce these emissions ((Poore and Nemecek, 2018), (Kim et al., 2020)). An effective policy measure to achieve this goal would be the introduction of a global carbon tax on agricultural products ((Jansson et al., 2023)). However, the introduction of carbon taxes on agricultural goods has so far been limited and remains an unpopular policy measure ((Dechezleprêtre et al., 2022)). Therefore, it is of utmost importance to examine alternative policy instruments that may effectively reduce emissions in the sector while still enjoying sufficient support in the population. Among these are behavioral instruments, such as labeling food items in terms of the greenhouse gas emissions they cause. Carbon labels have increasingly gained attention from regulatory agencies. For example, the European Commission proposes mandatory climate labels in its Farm to Fork strategy, ((European Commission, 2023)). Labels have also been implemented by US companies such as Oatly, an oat milk producer, Just Salad, a restaurant chain, Panera Bread and Allbirds, a shoe brand ((Wolfram, Jessica, 2021)).

However, we currently still face three challenges in our understanding of carbon labels. A first challenge is that the current evidence for the effectiveness of carbon labels on food items ((Camilleri et al., 2019), (Lohmann et al., 2022) [1]) is of a reduced-form nature, providing evidence that carbon labels impact consumption behavior, but not allowing for out-of-sample predictions on effect magnitudes. A second challenge is that our understanding of the behavioral channels through which carbon labels affect consumers is very limited. Previous literature ((Shewmake et al., 2015), (Camilleri et al., 2019) and (Imai et al., 2022)) and much of public discussion have mainly perceived carbon labels as a potential vehicle to correct consumers' misperceptions about the emissions caused by products. There is no causal evidence that this is the main channel through which carbon labels influence consumer behavior. Yet, this understanding will likely influence label implementation. For example, it might guide whether it is perceived as worthwhile to introduce carbon labels to relatively knowledgeable populations or over longer periods of time. Further, it is important to understand behavioral channels and the labels' effect on consumers to ensure that the labels do not impose disproportionate psychological costs. This leads us to the third challenge: We currently lack evidence on whether and to which extent carbon labels impose psychological costs on consumers.

To address the first challenge, I take a structural approach to estimate the magnitude of the effect carbon labels have on consumption behavior ((DellaVigna, 2018)). In a lab-in-the-field experiment I directly experimentally elicit how the effect of the carbon labels quantifies relative to a carbon tax. At the beginning of the experiment, all 289 participants indicate their willingness to pay for different restaurant meals. They then repeat the elicitation, with carbon labels shown to the treatment group but not the control group.[2] The carbon labels I test include both an ordinal ranking (traffic light system) and a quantitative ranking (greenhouse gas emissions in kg). This has been identified as

[1] I prominently mention these studies because they are of a non-hypothetical nature and allow for a causal interpretation. In addition, there are various hypothetical and correlational studies, which also follow a reduced-form approach. See Rondoni and Grasso (2021) for a review.

[2] The behavior of the control group shows that simply repeating baseline elicitation has no significant effect on willingness to pay

an effective combination in previous literature (see (Taufique et al., 2022) and (Potter et al., 2021)). Willingness to pay elicitations are incentivized: Experiment participants complete the experiment online at a pre-specified time and then make their way to one of the University buildings. The cash and restaurant meal they are handed corresponds to one of the choices they make in the experiment, with the outcome depending on their indicated willingness to pay and a random price draw. Meals are warm and perishable, reflecting a typical student restaurant lunch choice.

I estimate how high of a carbon tax would produce similar changes in demand as the carbon labels by comparing meal-specific changes in willingness to pay with the emissions caused by each of the meals. This is because a decrease in willingness to pay for a meal has the same effect on the total quantity purchased as an equivalent increase in meal price. I estimate that a carbon tax of €120/Ton would produce a similar effect as carbon labels. Quantifying the effect of carbon labels in this manner has several advantages. First, it allows for a comparison with other policy tools and allows us to better understand the magnitude of the effect. €120/Ton is about four-fold the German carbon tax on petrol. At the same time, it is still slightly lower than estimates of the social cost of carbon (€160/Ton in (Rennert et al., 2022)) - This suggests that the labels are not inefficiently "over-correcting" behavior. Second, studies such as Jansson et al. (2023) calculate the mitigation in greenhouse gas emissions achievable with varying magnitudes of taxes on greenhouse gas emissions caused by agricultural production. Findings from such studies can also be transferred to carbon labels using this quantification. Third, for projects employing a similar experimental approach, treatment effect sizes will easily be comparable across different consumption contexts and behavioral interventions. In a field setting, in contrast, effect sizes are tracked in terms of percentage change in sales or likelihood of purchase. However, the extent to which the effect labels have on willingness to pay translates into changes in purchases is strongly influenced by the meal offer and the pricing structure of the particular field setting. This confounds any comparison of effect sizes across field experiments - It is hard to disentangle whether one study found larger effect sizes because the labeling intervention had a stronger effect on guests or whether the price structure of this restaurant was such that the effect which the labeling intervention had on guests became more visible in actual purchases made.[3] Note as well that recovering an effect size estimation in terms of change in willingness to pay in a field experiment setting requires, additionally to a labeling intervention, estimates of setting-specific own-price and cross-price elasticities (following a sufficient statistics approach as described in (Chetty, 2009)). Such elasticities are difficult to obtain in the student restaurant context - Each university restaurant is somewhat different (e.g. offering meal components vs. entire meals) and student restaurants usually do not vary prices much.

To address the second challenge, I compare treatment effects and consumer misperceptions among 444 participants in a second lab-in-the-field experiment. The design closely follows that of the experiment described above, but subjects additionally guess the emissions caused by each meal between elicitations. This captures how much each consumer under- or overestimates the emissions caused by a certain meal.

[3] As an illustrating example, imagine two restaurants, A and B, which both implement carbon labels. For sake of simplicity, let's assume they both offer only a beef meal (causing 4 kg of emissions) and a vegetarian meal (causing 1 kg of emissions). Let's assume that in both cases, carbon labels decrease guests' willingness to pay for the beef meal by 0.50 Euro. In restaurant A, the beef meal costs €5, while the vegetarian meal costs €2. In restaurant B, both meals cost €3. Under which conditions would we expect this change in willingness to pay to translate into the same change in the likelihood of purchasing the beef meal? For example, this would be the case under uniformly distributed additional willingness to pay for the beef meal, relative to the vegetarian meal. Further, the effect of the label should not be heterogenous in this willingness to pay. These are strong assumptions, which would for example not hold in the context of the Bonn student restaurants.

In the analysis, I compare these misperceptions with individual treatment effects to gain insights on how much of the treatment effect is attributable to the correction of misperceptions. A correction of carbon misperceptions as the main mechanism would imply that participants adjust their willingness to pay downward if they underestimated emissions, and upward if they overestimated emissions. However, I find that willingness to pay for high-emission meals significantly decreases in both cases. This is to a significantly lesser extent in the case of overestimation, providing some evidence for the correction of biased beliefs as one relevant channel. Nevertheless, the fact that willingness to pay decreases downward even in the case of overestimation speaks against it being the main mechanism.

I provide evidence that the direction of attention is likely the more relevant behavioral channel. A separate group of participants also indicates their willingness to pay for each of the meals twice, and guesses the emissions caused by each meal between the elicitations. Importantly, I direct participants' attention towards carbon emissions through the belief elicitation, but I do not show participants any carbon labels in the second elicitation. They thus do not receive any information on carbon emissions caused and simply repeat the baseline elicitation. Remarkably, participants adjust their willingness to pay in the second elcitiation similarly to those participants who in fact see the carbon labels. This points towards the direction of attention being at least as important of a channel as the correction of biased beliefs. This finding speaks towards implementing carbon labels even in relatively knowledgeable populations and retaining them over longer periods to retain attention.

Using data from a large-scale field experiment, I provide evidence that the above estimates are reconcilable with behavior observed outside of a one-shot consumption setting. This field experiment was conducted with a similar experiment population (students and other persons affiliated to the University of Bonn). While one of Bonn's student restaurants was equipped with carbon labels for five weeks, the two other student restaurants served as control restaurants. The restaurants coordinate their daily menus, and there is little spillover between the restaurants. For around two-thirds of restaurant guests, the consumption decisions of a given individual are trackable over time using the student's payment card ID. Using a sample of over 80.000 observations from over 2.500 guests, I estimate that the labels decrease consumption of the meat (the higher carbon) meal by 3.1 percentage points. The effect of the label persists in the three weeks following the intervention period, after which the university restaurant closed for summer break. This is the first evidence of the post-intervention effects of a carbon labeling intervention.

To address the third challenge, I elicit the effect the labels have on consumer surplus in the lab-in-the-field experiments and gather suggestive evidence in the field setting. Recent literature has highlighted the importance of considering psychological costs on consumers when evaluating behavioral interventions ((Butera et al., 2019),(Allcott and Kessler, 2019)). Following a similar design as Allcott and Kessler (2019), the lab-in-the-field experiments elicit participants' willingness to pay to see or avoid labels on their final three consumption decisions. For the vast majority (95%) of participants, there is no evidence of a net psychological cost imposed, with many participants seemingly incurring psychological benefits with the labels. This suggests that the intervention is correcting an internality - In the absence of labels, consumers choose meals that are more carbon-intensive than they would choose in their private optima (following the internality definition in (Allcott et al., 2014)). Correspondingly, the labels raise consumer surplus rather than imposing additional psychological costs on the consumer.

In a post-intervention survey conducted after the field experiment in the student restaurant (234 respondents), 73% of guests affected by the labels reported that they would like the labels to be installed permanently (18% did not know, 9% against). In contrast, only 60% were in favor of a carbon tax in the student restaurant, 14% did not know and 26% were against. This also speaks towards carbon labels enjoying greater political support than carbon taxes, making an implementation more politically feasible.

My contributions to literature are three-fold: First, I contribute to the literature on the effectiveness of carbon labels on food consumption. There is some experimental evidence that carbon labels affect consumption behavior. The most reliable estimate is provided by Lohmann et al. (2022) who conduct a field experiment in the student restaurant context, and observe how consumption behavior changes in a student restaurant equipped with carbon labels relative to a control restaurant not equipped with labels. They find that labels decrease the probability of selecting a high-carbon meal by approximately 2.7 percentage points. Brunner et al. (2018) study a similar context, but only observe changes over time in a single restaurant. They find a decrease in sales of red labeled meat dishes by 2.4 percentage points. Further correlational evidence ((Vlaeminck et al., 2014), (Spaargaren et al., 2013) and (Visschers and Siegrist, 2015)) and evidence from hypothetical decisions (e.g. (Banerjee et al., 2022) and (Osman and Thornton, 2019)) suggests carbon labels reduce carbon emissions. Other studies examine consumer behavior in the lab, asking consumers to make a decision for consumption at some point in the future. Camilleri et al. (2019) finds carbon labels effective, while Imai et al. (2022) does not find an effect.

These previous studies estimate effect sizes in terms of percentage changes in consumption behavior, which are difficult to compare across consumption contexts and policy instruments. In my lab-in-the-field experiment, I provide the first experimental estimate of how effective carbon labels are compared to a carbon tax. Within-subject designs as used here and in other structural behavioral studies (e.g. (Taubinsky and Rees-Jones, 2018)) can easily be adapted to other experiment populations, consumption environments, or other behavioral interventions, making intervention effects comparable across various domains. The experimental design is further validated by my large-scale field experiment producing effect estimates in line with the results of my lab-in-the-field experiment. Further, my field experiment provides the - to my knowledge first - estimate of the post-intervention effects of a carbon labeling intervention. In a broader sense, this paper also adds to environmental interventions in the restaurant context (e.g. (Jalil et al., 2020)) and carbon labels in the general food consumption context (e.g. (Panzone et al., 2021) study the grocery shopping context).

Second, I contribute to the literature on the effect of attentional biases on consumption decisions. The idea that it is not only informational, but also attentional biases leading individuals to make non-optimal decisions in environmentally relevant context has been pointed out in other consumption contexts ((Tiefenbeck et al., 2018), (Allcott and Taubinsky, 2015), (Taubinsky and Rees-Jones, 2018)). Taubinsky and Rees-Jones (2018) have provided evidence that the provision of real-time information can correct such biases in the energy consumption context. This project provides first evidence of attentional biases present in the food consumption context. This is a new finding in the context of carbon labels, which has so far mainly focussed on the role of labels in correcting biased beliefs (e.g. (Shewmake et al., 2015), (Camilleri et al., 2019)). Further, I provide evidence from a discrete choice context of how a behavioral intervention can correct attention biases and thereby reduce carbon

emissions.

Finally, I contribute to the relatively young literature on the psychological costs or benefits of behavioral interventions. Recent research has assessed the psychological effects of receiving public social comparison information. Butera et al. (2022) find that public recognition delivers significant utility gains to high-performing individuals, while low-performing individuals incur significant utility losses. Allcott and Kessler (2019) find that home energy social comparison reports increase social welfare, but that traditional evaluation approaches largely overstate these gains. Thunström (2019) finds in a hypothetical choice experiment that carbon labels impose psychological costs on participants with low self control. I provide first evidence on the consumer surplus impact of carbon labels, both by eliciting effects on consumer surplus directly using a similar method to that employed by Allcott and Kessler (2019) and by conducting an opinion survey at the end of the field experiment.

The rest of this paper is structured as follows. Section 2 will describe the lab-in-the-field experiment quantifying the effectiveness of carbon labels on consumption decisions (from now on referred to as lab-in-the-field experiment 1), and section 3 will describe the lab-in-the-field experiment examining mechanisms (from now on referred to as lab-in-the-field experiment 2). Section 4 will describe the design and results of the field experiment. Section 5 will present results on the effect on consumer surplus, drawing on data from all experiments. Section 6 discusses findings.

2 LAB-IN-THE-FIELD EXPERIMENT 1: QUANTIFYING THE EFFECTIVENESS OF LABELS

Experiment design

Overview

To cleanly measure the impact of carbon labels, willingness to pay for the same meal should be observed for the same individual, at the same time: once in the absence of carbon labels and once in the presence of carbon labels. I thus follow a within-subject approach, eliciting willingness to pay of each participant two times. To control for any possible effects of asking individuals twice, some participants do not see carbon labels in either of the two elicitations. I summarize the most important design choices below and add details in the following subsections.

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

2. Willingness to pay elicitations are incentivized: Of the 15 meal purchase decisions made in the course of the online experiment, one decision was implemented. Participants made their way to university campus shortly after completing the experiment and received their payment in cash as well as a student restaurant meal. Both were a function of the willingness to pay participants had indicated and a random price draw.

3. Willingness to pay is elicited relative to an alternative lunch: In each of the 15 meal purchase decisions, participants first decide whether they prefer a given meal or a cheese sandwich. They then indicate how much they are willing to pay to receive the given meal rather than the cheese

sandwich, and vice versa if they prefer receiving the cheese sandwich. Willingness to pay for a given meal is thus always measured relative to the cheese sandwich (reflecting the real-world fact that the alternative to not eating something is eating something else). The dependent variable of interest is the **change** in relative willingness to pay between the first and second elicitation.

4. Carbon labels show a quantitative and ordinal ranking: The carbon labels I test include greenhouse gas emissions in kg, as calculated based on the quantity of each meal ingredient and its average greenhouse gas emissions. It also includes an ordinal ranking using a traffic light system, ranking the meal relative to other meals typical of Bonn's student restaurants. A combination of ordinal and quantitative ranking has been identified as an effective combination in previous literature (see (Taufique et al., 2022) and (Potter et al., 2021)). Further, I designed the labels in cooperation with Bonn's student restaurants to ensure that I am testing a label which they would in fact be willing to implement. Labels also state how long of a car drive (in km) would cause the same amount of CO_2 emissions.

5. A third round of decisions is added to increase power: There is one additional willingness to pay elicitation after the second elicitation. Half of the participants previously in the Control condition are shown carbon labels, while the other half remains in the Control condition. The main results hold also when focusing only on the first and second round of decisions.

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. I use these values to analyze effects on consumer surplus.

Experiment timeline

The experiment timeline is visualized in Figure 1. First, the elicitation of willingness to pay is explained to participants and they are shown how their payout and the meal they receive will depend on the choices they make throughout the experiment. They are then asked four comprehension questions, which they must answer correctly before proceeding. Any participant taking more than five attempts in doing so is excluded from the analysis, as pre-registered. Second, participants indicate their baseline willingness to pay for four meals (four questions). Third, participants answer several incentivized and timed[4] guessing questions on unrelated issues (e.g. how many bridges cross over the Rhine in the city of Bonn).

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

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

[4]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.

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[5], with partly changed treatment conditions:

- Participants previously in the Label condition are in the third round assigned to the Offset condition: Participants are informed that the emissions caused by their lunch choice (be it the meal or the sandwich) will be offset.[6]
- Half of the participants previously in the Control condition are in the third round assigned to the Label condition, and half of the participants previously in the Control condition repeat the Control condition. Afterward, before proceeding with the experiment, this group guesses emission values.[7].

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 decisions, participants are asked whether they would like to see carbon labels for their final three purchase 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. Participants' willingness to pay to see or avoid labels is interpreted as the labels' effect on consumer surplus in the analysis, taking a similar approach as e.g. Allcott and Kessler (2019) and Butera et al. (2022). As this element of the experiment is shared with lab-in-the-field experiment 2, results are discussed jointly in section 5 of this paper.

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.

[5]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.

[6]The results of the Offset condition are not the focus of this paper.

[7]This data is used for the analysis shown in Figure 10 As these guessing questions occur after the first, second, and third willingness to pay elicitation, they do not affect the results displayed in this section.

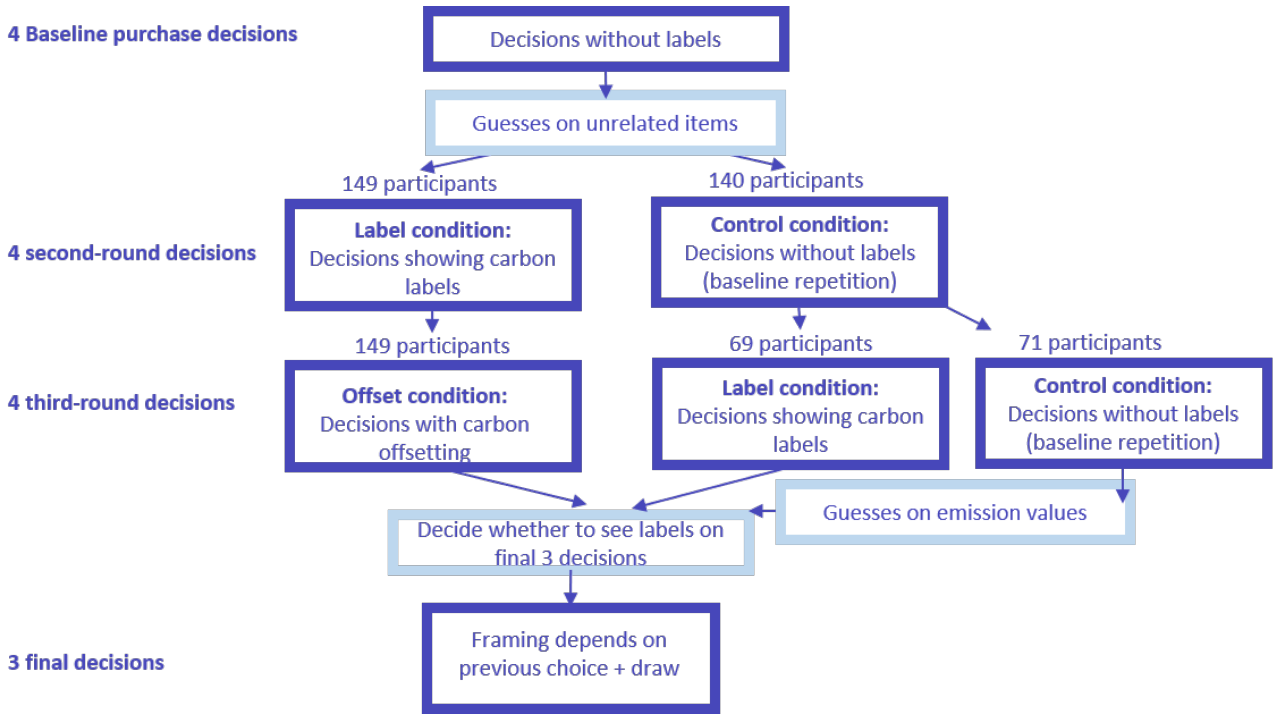

Figure 1: Experiment schedule and treatment groups

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). Participants who indicate that they are vegetarian are shown only vegetarian meals. In the four repeating decisions, these are: Filled courgettes with potato croquettes, Italian vegetable ragout with pasta, Cheese "Spätzle" with mushrooms and stir-fried vegetables with rice. Participants who indicate that they are not vegetarian are shown two vegetarian and two meat meals for the four repeating decisions: Filled courgettes with potato croquettes, Italian vegetable ragout with pasta, Chicken Schnitzel with rice and beef ragout with potatoes.

The decision set-up

In each decision, participants first choose whether they prefer consuming a certain meal or a cheese sandwich. An example for a baseline decision is shown in Figure 2. The left option in the example changes across decisions, indicating one of four specific meals, while the option on the right, the cheese sandwich, stays constant for all decisions.[8]

[8]To make sure that results are not driven by a left-right effect, half of the participants made their choices with the left-right positioning of the two options reversed.

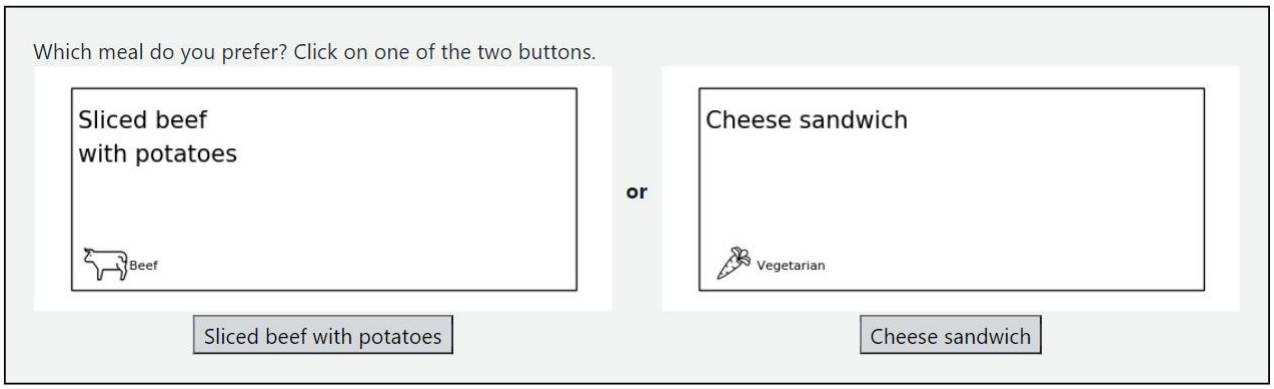

Figure 2: Meal purchase decision example: Step 1 of the purchasing decision

Once participants indicate their preference for one of the two options, a second window appears and they are in a second step asked how much of their experiment payment they would at most be willing to forego to ensure their preference (see example in Figure 3 in which the participant indicated a preference for Sliced beef in the first step). If participants prefer the specific meal, they indicate how much they are willing to forego to ensure they receive this meal instead of the cheese sandwich. If participants prefer the cheese sandwich, they indicate how much they are willing to forego to ensure they receive the cheese sandwich instead of the specific meal. Any amount between €0.00 Euro and €3.00 can be indicated on a slider in five-cent intervals. 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 restaurant. A willingness to pay of €3.00 or -€3.00 was indicated in less than 3% of all observations.

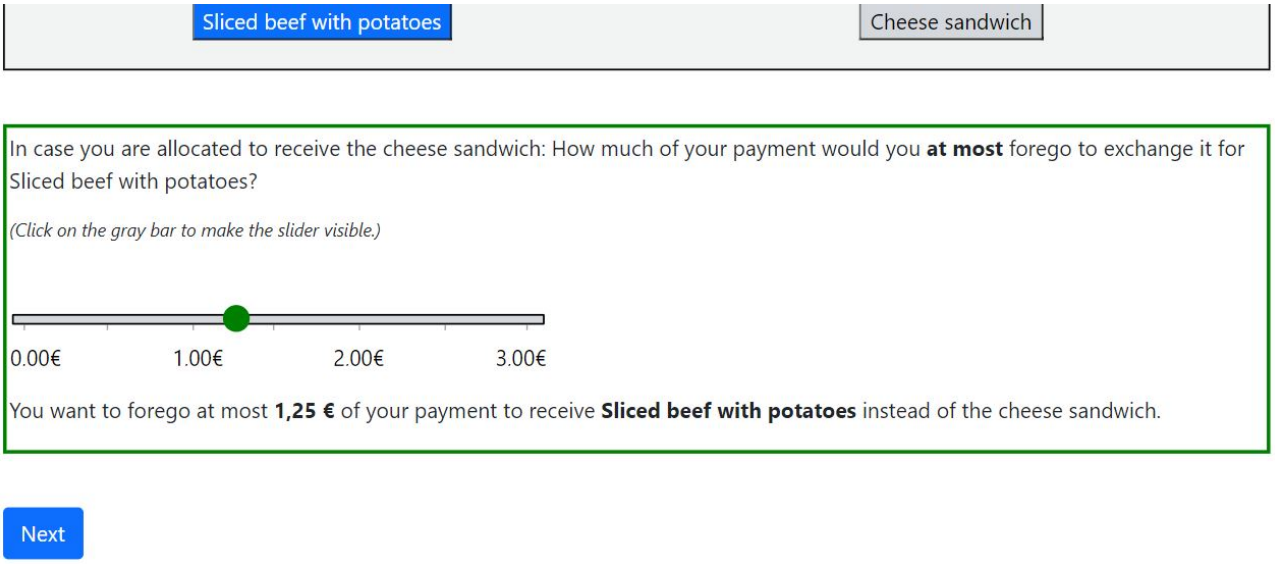

Figure 3: Meal purchase decision example: Step 2 of the purchasing decision

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

Decision framing differs across treatment conditions

In the four baseline decisions, participants do not see any carbon labels, but are merely shown the meal name and the meal's main ingredient (see Figure 2 for an example). 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. The four specific meals stay the same across rounds. For participants in the LABEL condition, emission values are added to the meal options. An example is shown in Figure 4. For participants in the CONTROL condition, there is no change in framing. For participants in the OFFSET condition, participants are told that the emissions caused by the meal will be offset. An example is shown in Figure 5.[9]

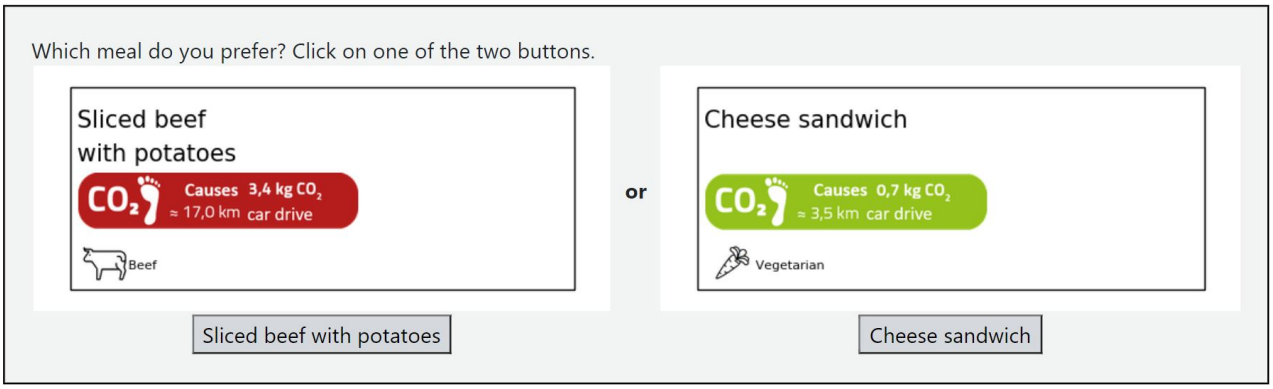

Figure 4: Meal purchase decision example: Decisions with labels

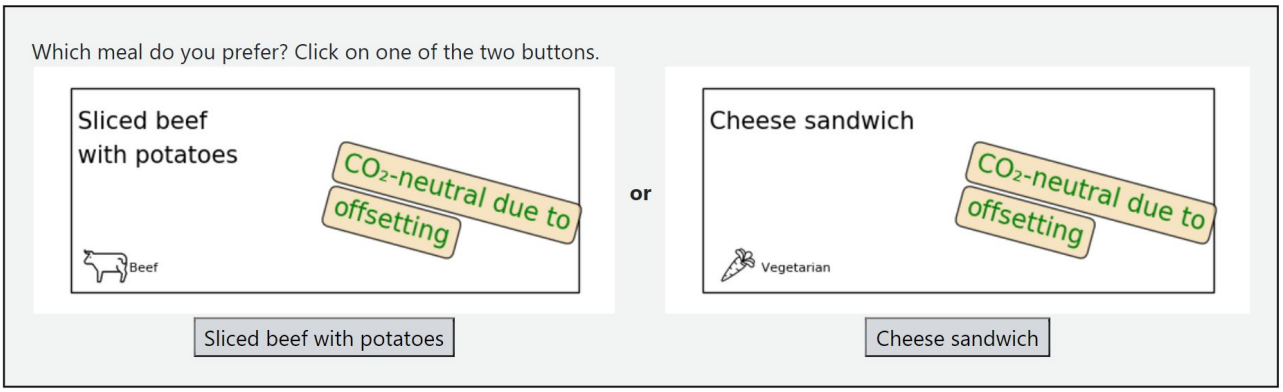

Figure 5: Meal purchase decision example: Decisions with carbon offsetting

The 3 final decisions

Before the three final decisions, a random draw and participants' preference for seeing or not seeing carbon labels determines whether decisions are framed just as at baseline (e.g. as in Figure 2) or with labels (e.g. as in Figure 4).

Participants and set-up

The experiment was conducted with parts of the participant pool of the Bonn-EconLab, the behavioral experimental lab of the University of Bonn. It took place on four days between the 26th of October and the 5th of November 2021 with 289 participants and was pre-registered (Schulze Tilling (2021b)).

[9] This is in fact the case, if one of the decisions in the OFFSET condition is chosen for implementation.

Participants were informed in the experiment invitation that vegetarian participants were permitted, but not participants with stricter dietary requirements (vegan, gluten-intolerant, lactose-intolerant, or halal). Participants were informed that the experiment would be conducted online, but that they would be required to make their way to campus afterward to collect their payment in cash. They were also informed that they will be provided with a lunch in addition to their monetary payment and were not given any further information on the purpose of the experiment. The experiment was conducted using oTree software (Chen et al. (2016)).

When participants picked up their meal, it was warm, ready-to-eat, and could be consumed on the spot, as shown in Figure 6. Meal options were catered by the student restaurant and were meals typically offered in the student restaurant. The meal purchasing decisions participants made in the experiment thus closely mimicked real-life meal purchasing decisions at lunchtime. I calculated the emissions caused by each meal with the application Eaternity Institute (2020), using recipes provided by the student restaurant.

Figure 6: Gazebo set up on University campus to provide participants with their payment in cash and the meal or sandwich corresponding to their choice, while adhering to Covid regulations.

Incentivization

The **meal purchasing decisions** are incentivized as follows: At the beginning of the experiment, participants are informed that one of the 15 decisions will be implemented. They are not told for which of these this will be the case. It is thus in their best interest to treat each decision as the relevant one. For the relevant decision, the willingness to pay elicitation 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 Euro 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' payment. 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 Euro 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.

Data and results

Some observations were excluded as pre-registered [10]. Specifically, the 3% fastest participants were excluded, as well as the participants who needed more than five attempts for the comprehension check. Experiment participants were computer randomized into the groups "Label, then Offset", "Control, then Label" and "Control, then Control", with the group name describing the information shown to participants in the second and then the third elicitation. Summary statistics are shown in Table 1. There is a higher proportion of meat-eaters in the group "Control, then Control" (significant at the 5% level). I also perform the main analysis seperately for vegetarian and non-vegetarian participants - These analyses should not be influenced by the higher proportion of meat-eaters in the control group. I find very similar results and thus do not believe that the higher proportion of meat-eaters in the "Control, then Control" group poses reason for concern.

Table 1: Lab-in-the-field experiment 1: Socio-economic summary statistics

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

To identify the causal effect of carbon labels on participants' willingness to pay for a meal, I compare how willingness to pay for a specific meal changes between baseline and second-round or third-round decisions for participants in the Label condition versus for participants in the Control condition (see Figure 1).

In the analysis, I differentiate between changes in willingness to pay for meals with emissions lower than the cheese sandwich and meals with emissions higher than the cheese sandwich. This is because I expect participants to respond to carbon labels differently depending on how the emissions of the two options compare:

[10] Schulze Tilling (2021b)

- For meals with emissions lower than the cheese sandwich, emissions are reduced if consumers adjust their demand for these meals upward, effectively reducing demand for the more carbon-intensive cheese sandwich.
- For meals with emissions higher than the cheese sandwich, emissions are reduced if consumers adjust their demand for these meals downward.

For meals with lower emissions than the cheese sandwich, willingness to pay does not change significantly, although coefficients are moving in the expected direction. For meals with higher emissions than the cheese sandwich, willingness to pay in the CONTROL condition does not change, while that in the LABEL condition decreases by €0.27. This effect is significant at the 1% level. Effects are visualized in Figure 7 and detailed in Table 2, specification (1). Specification (2) does not group the four meals into low-emission and high-emission meals but instead regresses the change in willingness to pay on the difference in emissions between the warm meal and cheese sandwich. This specification estimates that on average, willingness to pay decreases by €0.12 for every additional kg of emissions that the warm meal causes on top of the cheese sandwich. This suggests that carbon labels induce a demand effect similar to that of a carbon tax of €120/ Ton. This is four-fold the current German CO_2 tax on petrol (€30/ Ton).

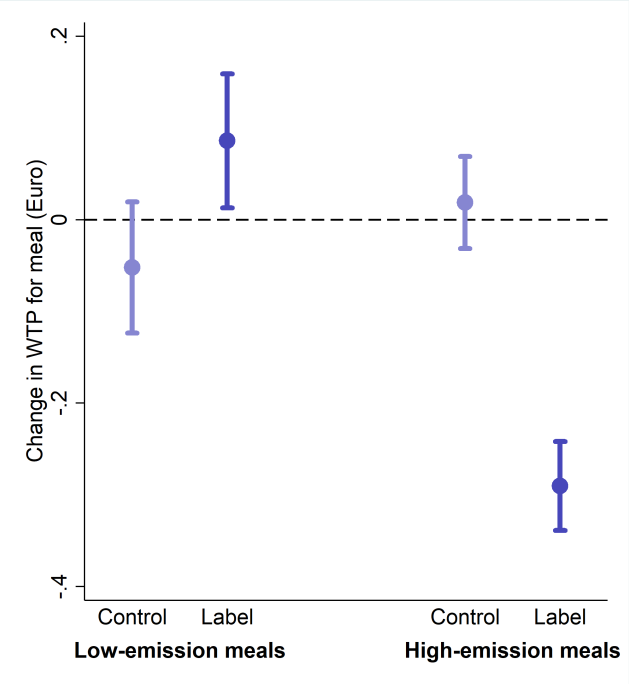

Figure 7: Within-subject change in willingness to pay for a specific meal, differentiated between participants in the "Control" and "Label" condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich)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

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

Standard errors in parentheses

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

Table 2: Dependent variable: within-subject change in willingness to pay for a specific meal, compared to baseline. Spec. (1) corresponds to Figure 7 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. In specification (2), emissions (kg) is 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.

3 LAB-IN-THE-FIELD EXPERIMENT 2: MECHANISMS

Experiment design

Overview

To identify the extent to which a correction of misperceptions drives the effect of carbon labels, the experiment should not only track how willingness to pay changes with carbon labels, but also participants' initial perceptions. I thus adapt lab-in-the-field experiment 1 by asking participants to guess the emissions caused by various meals (including the meals for which willingness to pay is elicited). These guessing questions occur between the first and second willingness to pay elicitations and replace the guessing on unrelated items. Apart from this design choice, lab-in-the-field experiment 2 closely resembles lab-in-the-field experiment 1, with the main design choices being the following:

1. Attent+Label and Attent condition: Participants were asked to guess emission values regardless of whether they were shown emission labels in the second round of decisions. Participants in the Attent condition did not see any emission labels in the second round of decisions and simply repeated the baseline elicitation. For these participants, the change from baseline to second-round willingness to pay measures the effect of drawing participants' attention to carbon emissions, but not providing them with any information on emissions.

2. Showing a reference meal: When participants guess the greenhouse gas emissions caused by different meals, they are always shown the meal "Red Thai Curry with pork and rice" as a reference meal. Including a reference meal ensures that participants' guesses are of a comparable magnitude. I am mostly interested in how participants perceive the emissions caused by different meals relative to each other and this should not be influenced by giving participants a reference category. Another advantage is that it helps participants without any idea for magnitude and prevents that participants enter random numbers due to frustration. Before the guess elicitation, I ask participants to guess the emissions caused by the reference meal (unincentivized). Over 46% of respondents responded 30 kg or more, and over 25% of respondents responded 100 kg or more. In the following incentivized guessing questions showing participants the reference meal, an amount of 30 kg or more was indicated in less than 7% of observations.

Experiment timeline

The experiment timeline is as in the first lab-in-the-field experiment, with one key difference. After participants completed all of the four baseline decisions, they guess the greenhouse gas emissions caused by different meals (see Figure 8). These include the four meals around which the meal purchasing decisions revolve, as well as six further meals (see Figure 10 for a list). Participants make each of the ten guessing decisions on separate screens, shown to participants in a random order. On each screen, they are always shown the emissions of a reference example meal (Red Thai Curry with pork and rice, causes 1.7 kg of CO_2). This reference meal is not included in any willingness to pay elicitations. An example is shown in Figure 9. The guessing questions are incentivized and timed as in lab-in-the-field experiment 1. Participants do not complete any guessing questions on unrelated items, as is the case in lab-in-the-field experiment 1.

After the guessing questions, participants' willingness to pay for the four meals is elicited a second time. Again, as in the first experiment, the framing of the decision and some characteristics of the decision depend on the treatment condition.

- In the ATTENT condition, the willingness to pay elicitation is exactly as in the first, baseline elicitation. However, since participants have completed the emission guessing task in the meantime, they have now spent time thinking about the issue of greenhouse gas emissions, and are thus "attent".
- In the ATTENT+LABEL conditions participants are now shown carbon labels when indicating their willingness to pay. An example is shown in Figure 4.
- In the ATTENT+OFFSET condition, participants are informed that the emissions caused by their lunch choice will be offset.[11]

In the next step, participants' willingness to pay for the four meals is elicited a third time, with partly changed treatment conditions:

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

The experiment then proceeds as in lab-in-the-field experiment 1.

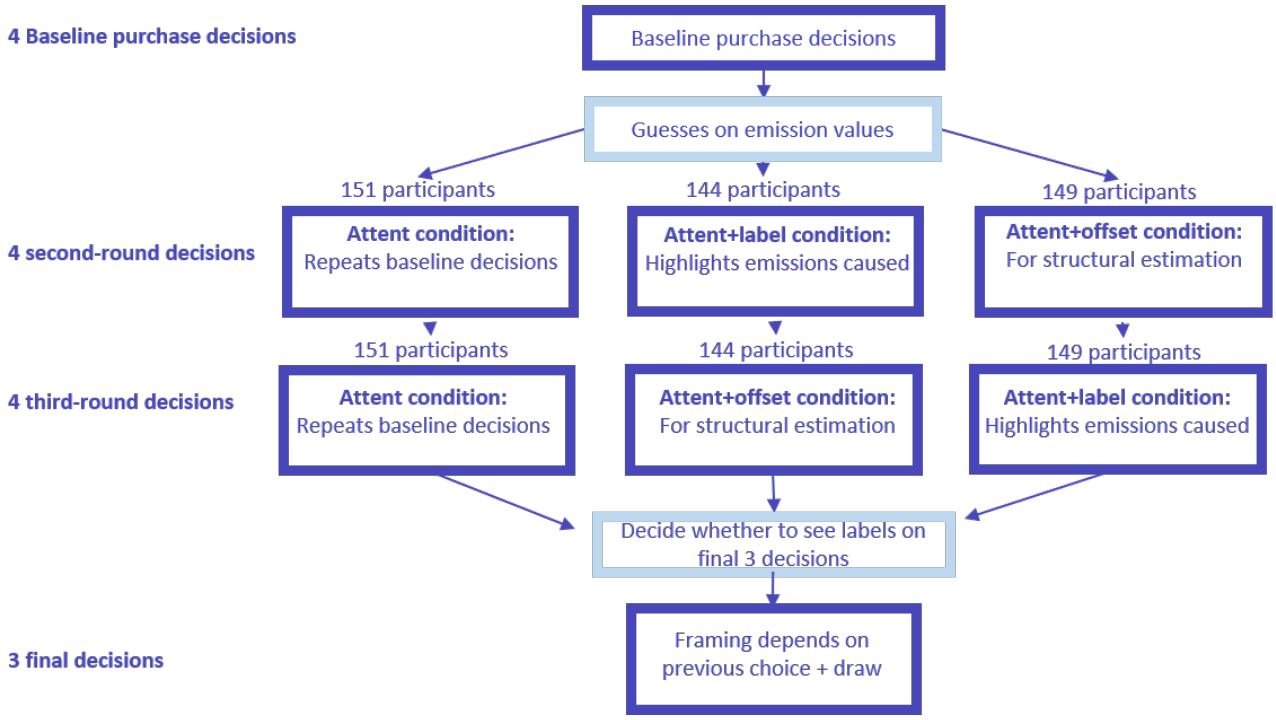

Figure 8: Experiment schedule and treatment groups

[11] Results of this condition are not the focus of this paper.

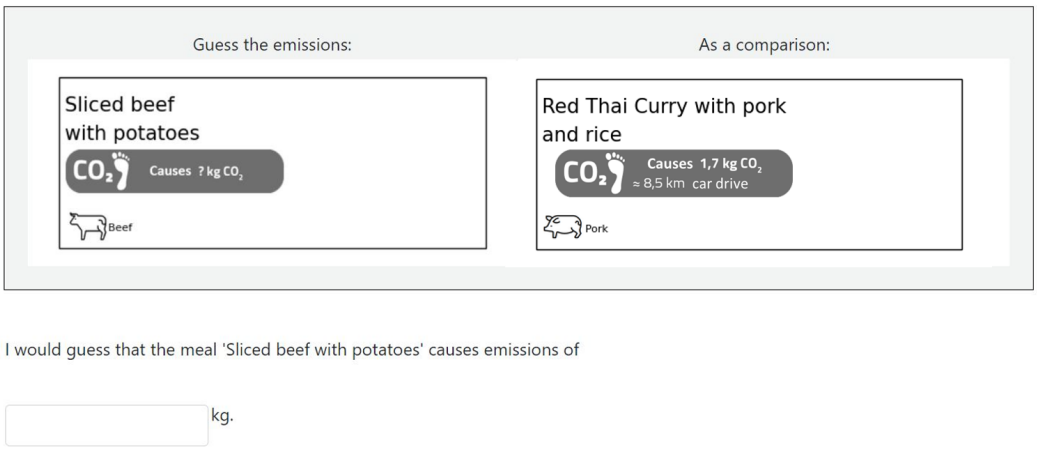

Figure 9: Example guessing questions

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 lab-in-the-field experiment 1.

Participants and set-up

The second lab-in-the-field experiment was also conducted with parts of the participant pool of the BonnEconLab, the behavioral experimental lab of the University of Bonn. It took place on six days between the 22nd of June and the 8th of July 2021 and was pre-registered ((Schulze Tilling (2021a))). 444 participants participated. Participant invitation and experiment set-up was as in the first lab-in-the-field experiment.

Data and results

Some observations were excluded as pre-registered [12]. Specifically, the 3% fastest participants were excluded, as well as the participants who took more than five attempts for the comprehension check. Experiment participants were randomized into the three treatment groups "Attent, then Attent", "Attent+Label, then Attent+Offset" and "Attent+Offset, then Attent+Label", with the group name describing the information shown to participants in the second and the third elicitation. Summary statistics are shown in Table 3. All characteristics are balanced across treatment assignments, i.e. I fail to reject significant differences between groups.

Table 3: Lab-in-the-field experiment 2 estimation sample: Socio-economic summary statistics

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

[12] Schulze Tilling (2021b)

The effect of carbon labels by previous estimation: All participants in experiment 2 were asked to guess the emissions caused by different meals. Further, the 71 participants in the "Control, then Control" group in experiment 1 also estimated greenhouse gas emissions towards the end of the experiment. Figure 10 draws on both these data sources and displays how average guesses deviated for each of the meals. On average, participants rather underestimate emissions (green-colored dots) and overestimate emissions for some low-emission meals (red-colored dots).

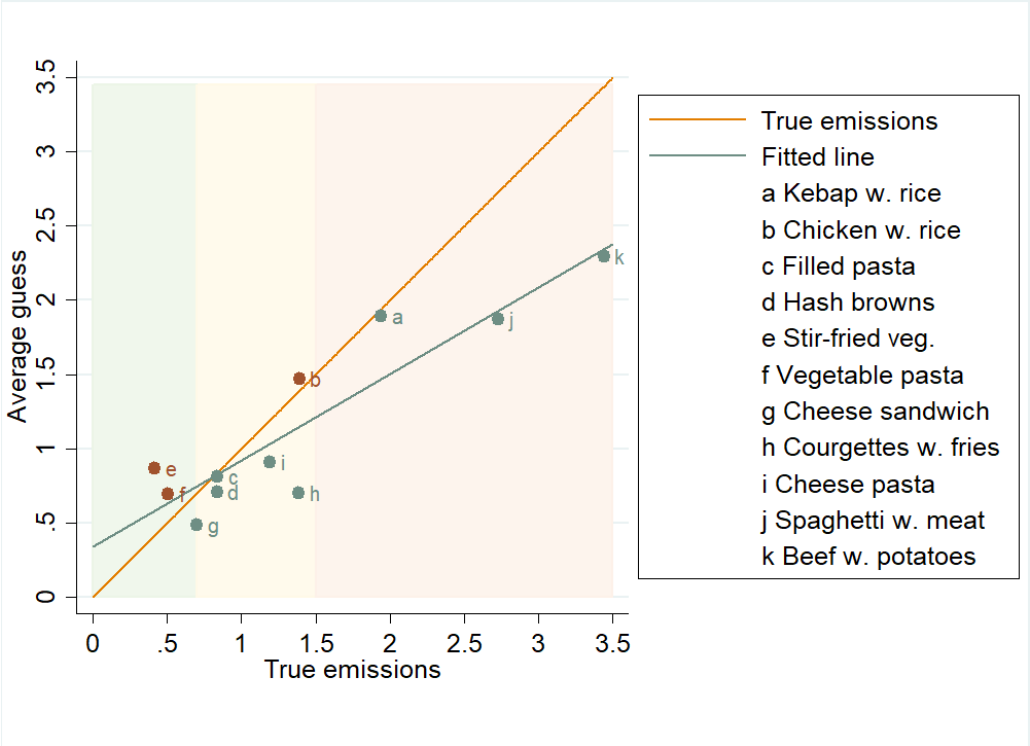

Figure 10: Average guess of the emissions caused by a given meal, plotted against true emissions. Values closer to the orange line were more precisely estimated. Meals corresponding to orange scatter points were on average overestimated in their emissions, while meals corresponding to green scatter plots were on average underestimated. The fitted line is described by $y = 0.39 + 0.57\ x$, with both the intercept and the coefficient significant at $p < 0.01$. Values based on guesses made by the participants of the second lab-in-the-field experiment and the participants in the "Control, then Control" group of the first lab-in-the-field experiment. An exception is the meal "Spaghetti with meat" which is only included in the guessing questions of the first lab-in-the-field experiment. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission value) are dropped. This leave a total of 4,261 observations made by 490 participants. The graph background is colored in green, yellow and red to show the label color assigned to the respective meals. Non-vegetarian participants make consumption decisions on the four meals "Chicken w. rice", "Vegetable pasta","Courgettes w. fries" and "Beef w. potatoes". Vegetarian participants made consumption decisions on the four meals "Stir-fried veg.", "Vegetable pasta","Courgettes w. fries" and "Cheese pasta"

In the next step of the analysis, I combine individual and meal-specific under- or overestimation of emissions with the corresponding treatment effects. If the correction of biased beliefs was driving the effect of the carbon label and a given individual underestimated the emissions of a certain meal, one would expect a downward correction of willingness to pay in reaction to the label. Correspondingly, if the individual overestimated the emissions of a certain meal, one would expect an upward correction. Figure 11 shows how the effect of labels on willingness to pay differs depending on estimation. If emissions were underestimated, willingness to pay on average decreases by €0.19, while if emissions were overestimated, willingness to pay on average decreases by €0.07. Table 4, Spec. (1) shows that this difference in the decrease in willingness to pay is significant at the 1% level. Spec. (2) does not

group observations by previous under- or overestimation but instead regresses the change in willingness to pay on the degree of underestimation (in kg). This specification suggests that seeing labels on average decreases willingness to pay by €0.16, with an additional decrease of €0.07 for each kg by which emissions were underestimated. This suggests that part of the effect of the labels can be explained through a correction in biased beliefs: Participants who previously underestimated the emissions of the warm meal relative to the cheese sandwich on average decreased their willingness to pay by a larger extent. However, a large part of the effect of the label - the large constant term in spec. (2) and the effect for overestimated meals in spec. (1) - cannot be explained by this mechanism. If participants previously overestimated emissions and the label's main effect was to correct beliefs, participants should be increasing their willingness-to-pay. There should not be such a significant decrease. Thus, this is evidence of a second relevant mechanism at play.

Contrary to this interpretation, one might argue that the pattern I observe is driven by individuals who do not place much confidence into their emission guesses, leading to the change in willingness to pay being largely independent of previous under- or overestimation. I do not directly observe participants' confidence in their emission guesses, but I do replicate the above analysis using overall guessing performance as a proxy for confidence. I replicate the analysis including (a) only individuals who did an above-average job at guessing the relative emissions of at least three of the four meals correctly (Figure 24 in the Appendix) and (b) only individuals who did an above-average job in guessing emission magnitudes (Figure 26 in the Appendix). Patterns look similar to Figure 11.

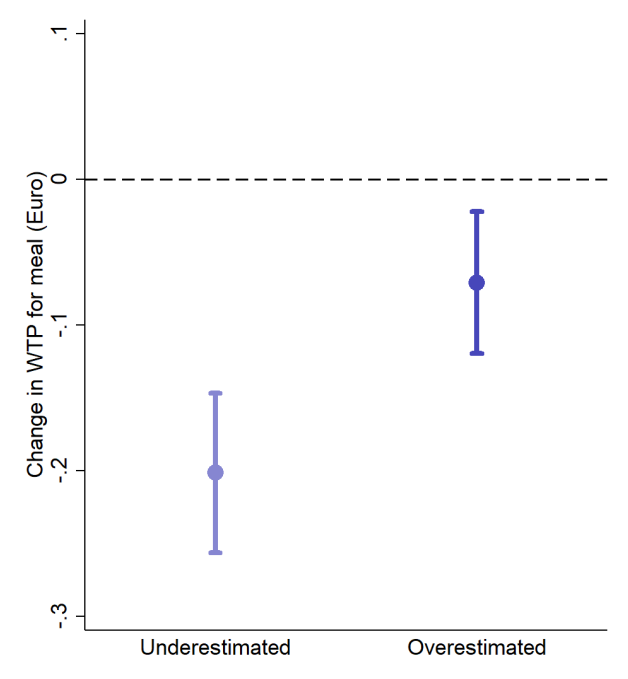

Figure 11: Within-subject change in willingness to pay for a specific meal when shown carbon labels, depending on whether the participant previously over- or underestimated the difference in emissions between the specific meal and the cheese sandwich. Participants are all in the "Attent+Label" condition. Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions	-0.13***	
	(0.04)	
Underestimation (in kg)		-0.07***
		(0.02)
Control for third round	0.05	0.07
	(0.05)	(0.05)
Constant	-0.10***	-0.16***
	(0.04)	(0.03)
Participants	293	270
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: Dependent variable: within-subject change in willingness to pay for a specific meal when shown carbon labels ("Attent+Label" condition). In spec. (1), treatment effects of the carbon label are split into a constant effect and the additional effect of previous underestimation. In specification (2), change in willingness to pay is regressed on underestimation in kg. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped.

The effect of directing attention: Participants might in general be knowledgeable about the carbon emissions caused by different meals, but not be attentive to these at the moment of choice. In the

ATTENT condition, participants' attention was drawn to the issue of carbon labels when participants guessed the emissions caused by each meal. However, they were not informed about emissions during the subsequent choices, as in the ATTENT+LABEL condition. The change in willingness to pay observed for low-emission meals and high-emission meals in the ATTENT and the ATTENT+LABEL condition is compared side-by-side in Figure 12. Simply increasing attention 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.

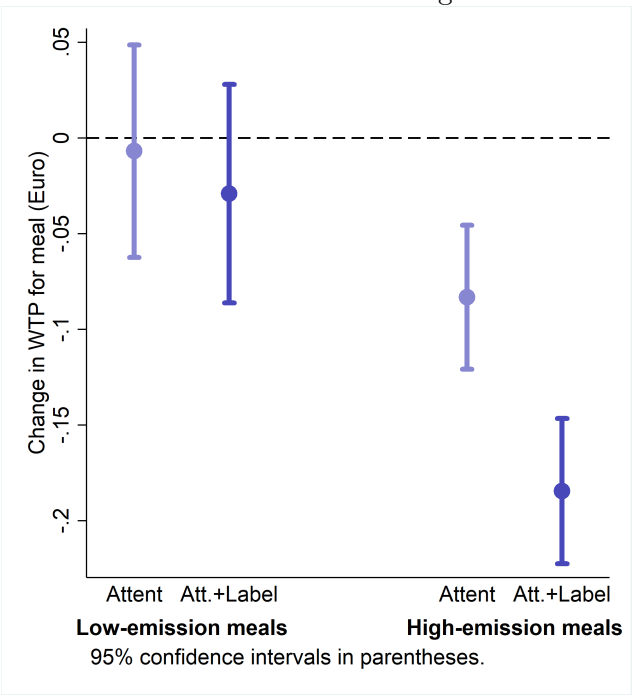

Figure 12: Within-subject change in willingness to pay for a specific meal, comparing participants in the "Attent" and "Attent+Label" condition. Effects are split into meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

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

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

Table 5: Dependent variable: within-subject change in willingness to pay for a specific meal when made attent. Spec. (1) corresponds to Figure 12 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. "High emission meal" describes the pure effect of being made attent, "High emission meal x Shown label" the additional effect of seeing information.

4 FIELD EXPERIMENT: LABEL EFFECTIVENESS OVER LONGER TIME PERIODS AND POST-INTERVENTION

Design and setting

The field experiment was conducted in the student restaurants 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 restaurant from the beginning of May to mid-June 2022 (seven weeks). From mid-June to the mid-July 2022 (three weeks, ended by the summer closing of the treated student restaurant), consumption decisions are observed to examine post-intervention behavior.

There are three student restaurants in Bonn: The treatment student restaurant, 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 restaurants, and there is thus a large overlap in the

daily offering. All three student restaurants offer two main meal components, which differ daily but are mostly the same across student restaurants. In addition, each of the student restaurants might offer additional options, which are student-restaurant-specific. The larger control restaurant sometimes offers pizza or pasta in addition, and all student restaurants might serve left-over 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 left-over main meal components were not labeled. [13] Correspondingly, the dependent variable in my main regression is whether the main meal component a restaurant guest chooses contains meat or is vegetarian.

An average student restaurant guest visited the student restaurant 8 times from April to mid-July. Around 31% visit 10 times or more, and around 11% visit 20 times or more. 90% of guests visited the same student restaurant at least 80% of the time. The student restaurants offer very cheap meals, with complete meals costing between 1 € and 3 €. In fast food restaurants located in the surrounding area, meals are priced at 4 € upward. In a survey of student restaurant guests with over 1,000 respondents (survey 2 described in the Appendix), over 40% of students report that they would have difficulty finding an affordable meal if the student restaurants would not exist. Switching between student restaurants and other gastronomic offers is thus also not frequent. Figure 29 in the Appendix includes an analysis based on the trackable personal card payments. I classify restaurant guests as "Treatment" or "Control" visitors based on their consumption behavior in the first two weeks. Around 3% of purchases made by "Control" visitors are made in the treated restaurant throughout the entire 14-week period. For "Treatment" vistors, the percentage fluctuates between 8% and 11%, with no clear trend attributable to treatment. Figure 30 further examines which percentage of these non-home visits involve consumption of a meat main component. There is no clear trend throughout the study period.

Further, an analysis of daily restaurant guests shows that the labeling intervention did not lead to a decrease in student restaurant guests, relative to the control restaurant (see Figures 32 and 33). The introduction of carbon labels in the treatment restaurant was displayed as a measure taken by the student restaurants 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 restaurant, as shown in Figure 14. 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 measure were advertised through different channels, and the survey was advertised as a chance to voice one's opinion on the offer of the student restaurant. It is thus unlikely that restaurant guests drew a connection between the initiative and the survey.

For the carbon labels, I calculated emission values with the application Eaternity Institute (2020), using ingredient lists provided by the student restaurant. The design of the carbon labels was proposed by the student restaurant, based on what is technically feasible and possibly implementable as a long-run measure. Examples are shown in Figure 13. They were coded in a traffic-light system, with thresholds

[13] The main reason for this was that I wanted to test carbon labeling in a manner that was feasible for the student restaurant to implement long-term. While main meal components are planned and known beforehand, sides and left-over dishes are decided spontaneously. Further, left-over 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.

determined such that approximately a third of the main components offered by the student restaurant 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. [14]

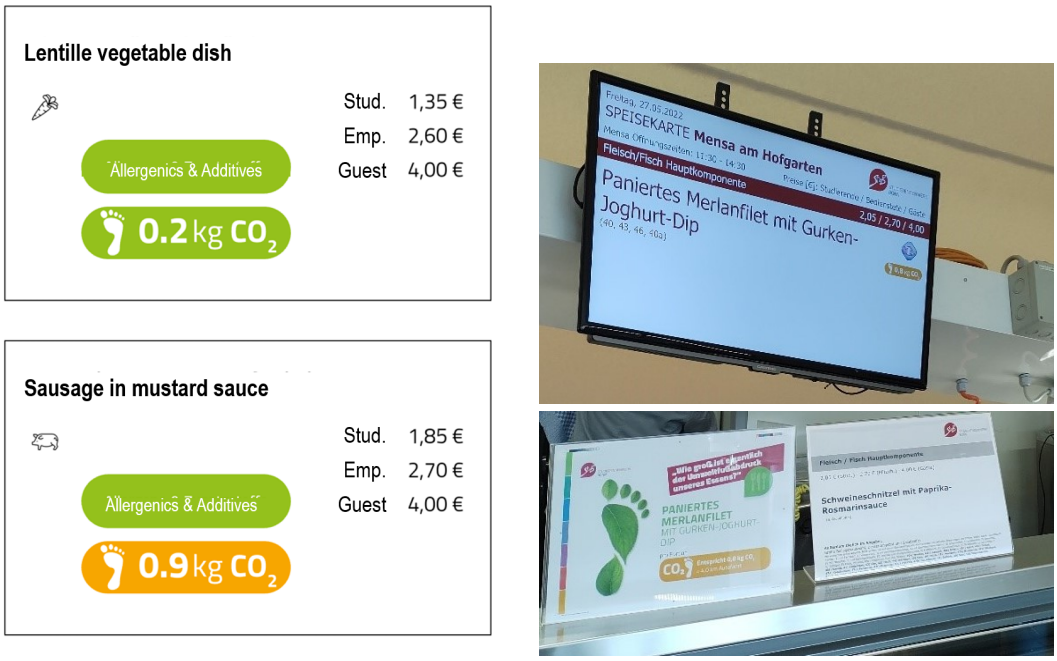

Figure 13: Labels online (left, menu translated from German) and in the student restaurant (right)

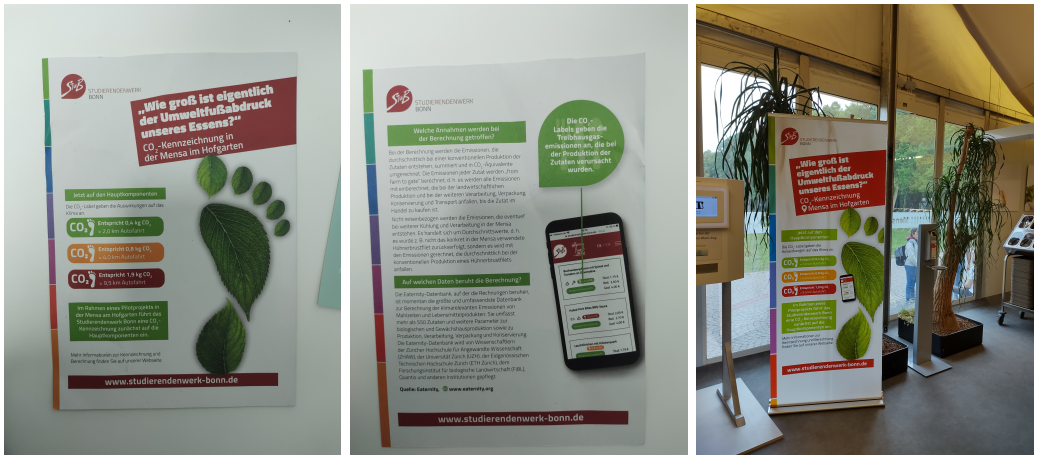

Figure 14: Explanation of the carbon labeling on flyers (left and center) and billboards in the entrance of the student restaurant (right).

Data and Results

The student restaurant provided me with purchase data from April 1st to July 29th, for the three student restaurants. I exclude the following purchases from the main analysis[15]

[14] 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 restaurant, but assume average conventional production. Emissions caused by the student restaurant cooling, freezing, and cooking ingredients on-site are not included. These calculation details are explained to students on the student restaurant website and on leaflets lying out on-site in the student restaurant.

[15] These exclusions were not pre-registered, since I did not anticipate these events.

- During the first week of the label period, display was irregular, as the student restaurant needed some "trial end error" to get the system running. On some days, the labels were only displayed in the student restaurant or only online. Further, the student restaurant had a special "Healthy Campus" week during the first week of May, during which it offered additional extraordinary meals which were also irregularly labeled[16]. It is thus not clear whether the decrease in meat consumption observed during this week was caused by the carbon labels (see Figure 34 in the Appendix). I thus exclude this week from the main analysis.
- There are seven days on which the treatment restaurant and the larger control restaurant did not offer the same main meal components: 7th of April, 19th of April, 20th of April, 17 of May, 15th of May, 24th of June, 27th of June. This is because, although menu planning is centralized, one of the student restaurants may not have been 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 restaurant on these days is likely mainly influenced by differences in offer rather than by differences in label treatment. I thus exclude these days.
- Starting from week 9 of the treatment period, Ukrainian refugees received meals in the treated student restaurant and the larger control restaurant, using specific student restaurant cards. I thus identify these sales and exclude them from all analyses. For the treated restaurant, they make up 12% of total sales in week 9,25% in week 10 and between 14% and 18% for the rest of the observation period. For the control restaurant, they make up between 2% and 7% of total sales.

For each purchase, I have data on the mode of purchase (student restaurant card or debit card), meal category (combined with daily menus, this provides the specific meal name), student restaurant card ID (if the purchase is made with the student restaurant card), cash register number, date of purchase, time of purchase (exact to the minute), and purchase value. The intervention had no relevant effect on the number of restaurant guests, as shown in the Appendix.

The final sample includes 121,071 observations, split between over 2,600 guests. The main analysis focuses on changes in the consumption of meat main meal components. Every day of the observation period, restaurant guests are offered the choice between a meat or a vegetarian main meal component. The emissions caused by the vegetarian main meal component are lower than or equal to the emissions of the meat meal offered on all observation days. The main specification is Col. (3) of Table 6. This specification includes controls for the number of vegetarian dishes, the price difference between vegetarian and meat meal, the total number of options, and the total daily sales, as included in Lohmann et al. (2022). Additionally, I include day-specific effects, which take up a large part of the variation in menu differences across days. I find that the labels decrease the likelihood of choosing the meat meal on offer by 2.1 percentage points, or 5.1% of the baseline likelihood. Figure 15 shows an event study of treatment effects, using the main specification. Col. (4) of Table 6 shows an intention to treat analysis at the individual level, including individual fixed effects. For this analysis, I restrict the sample to guests who payed with their individual student restaurant card (this is 2/3 of the sample). This makes it possible for me to track the consumption behavior of a single individual across time.

[16]The student restaurant only communicated this week to me shortly after pre-registration

Further, I restrict the sample to guests who visited the student restaurant at least ten times during the 13-week period (70% of the remaining sample), to enable a sufficiently powered estimation of individual fixed effects. Finally, for this analysis I classify individuals as belonging to the treated or control group based on their consumption behavior in the four-week pre-intervention period. Guests thus have to visit one of the student restaurants at least once during this period and a "home" student restaurant has to be clearly identifiable. I thus only include guests who visit the same student restaurant at least 80% of the time during the pre-intervention period (85% of the remaining sample). This analysis identifies an intention-to-treat effect of 1.5 percentage points, significant at the 10% level.

All specifications in Table 6 identify strong post-intervention effects. Time trends are visualized in an event study graph in Figure 15. The effectiveness of the labels seems to increase throughout the labeling period. One explanation for this might be that perhaps restaurant guests do not notice the carbon labels immediately, but only at their second or third visit to the student restaurant, e.g. when friends draw their attention to the labels. The magnitude of the identified post-intervention effects is comparable to the treatment effects estimated for the later weeks of the intervention period.

For an analysis of the impact on average greenhouse gas emissions per meal, I restrict the sample such that it only includes days in the intervention period for which there is a "gastronomic twin" in the pre-intervention period: a day in the pre-intervention period where the same two main meal components were served. Further, I drop any sales not related to the two main components shared between treatment and control restaurants. The reason for this restriction is that the average emissions per meal vary a lot between days due to a changing offer. As vegetarian consumption is, at baseline, higher in the treated than in the control restaurants, a less restricted analysis might falsely attribute changes in meal offer to the label. [17] The restricted sample contains 33,711 observations. As shown in Table 19 in the Appendix, I estimate that labels reduce average emissions per meal by 24 grams or around 3% of the emissions of a baseline meal.

Table 17 in the Appendix examines treatment effects in different subsamples, using Spec. (3) of Table 6. Treatment effects are similar when restricting the sample to only employees (col. 2), to off-peak visit hours (col. 3), to purchases made with an individual payment card (col. 4) and to restaurant guests paying by individual card and visiting the student restaurant rather frequently (at least ten times during the 13-week period, col. 5). Table 18 shows analyses restricting the sample to guests who pay by individual payment card and for whom I have demographic information (around 1,411 guests). This analysis is merely suggestive, as the number of observations is not sufficiently large to estimate treatment effects in such small subsamples.

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

Table 6: Field estimates of the effect of carbon labels on vegetarian consumption

	Likelihood of consuming meat (in per.)			
	(1) Meat meal	(2) Meat meal	(3) Meat meal	(4) Meat meal
Treatment restaurant x Label period	-2.09***	-2.07***	-2.07***	-1.53*
	(0.74)	(0.74)	(0.75)	(0.92)
Treatment restaurant x Post period	-6.89***	-6.82***	-5.27***	-3.15***
	(0.82)	(0.82)	(0.86)	(1.17)
Treatment restaurant	-10.09***	-9.98***	-13.44***	
	(0.59)	(0.59)	(0.76)	
Label period	0.55			
	(0.42)			
Post period	0.83*			
	(0.47)			
Second veg. main			-3.59***	-3.14***
			(0.51)	(0.64)
Price difference			-5.17***	-5.89**
			(1.90)	(2.44)
Number of meal options			-1.47***	0.99***
			(0.26)	(0.32)
Total daily sales			-0.91***	
			(0.06)	
Date effects	No	Yes	Yes	Yes
Fixed effects	No	No	No	Yes
Guests control	6,935	6,935	6,935	1,949
Guests treated	2,822	2,822	2,822	680
Observations	121,071	121,071	121,071	49,921

Standard errors in parentheses

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

Note: Dependent variable: Dependent variable: 0/1 indicator for consumption of the vegetarian option, multiplied by 100 to enable the interpretation of coefficients as percentage points. Specifications (2)-(4) include date effects, and the "Post period" and "Label period" indicators are thus dropped due to collinearity. Specification (4) includes individual fixed effects, and the "Treated" indicator is thus dropped due to collinearity.

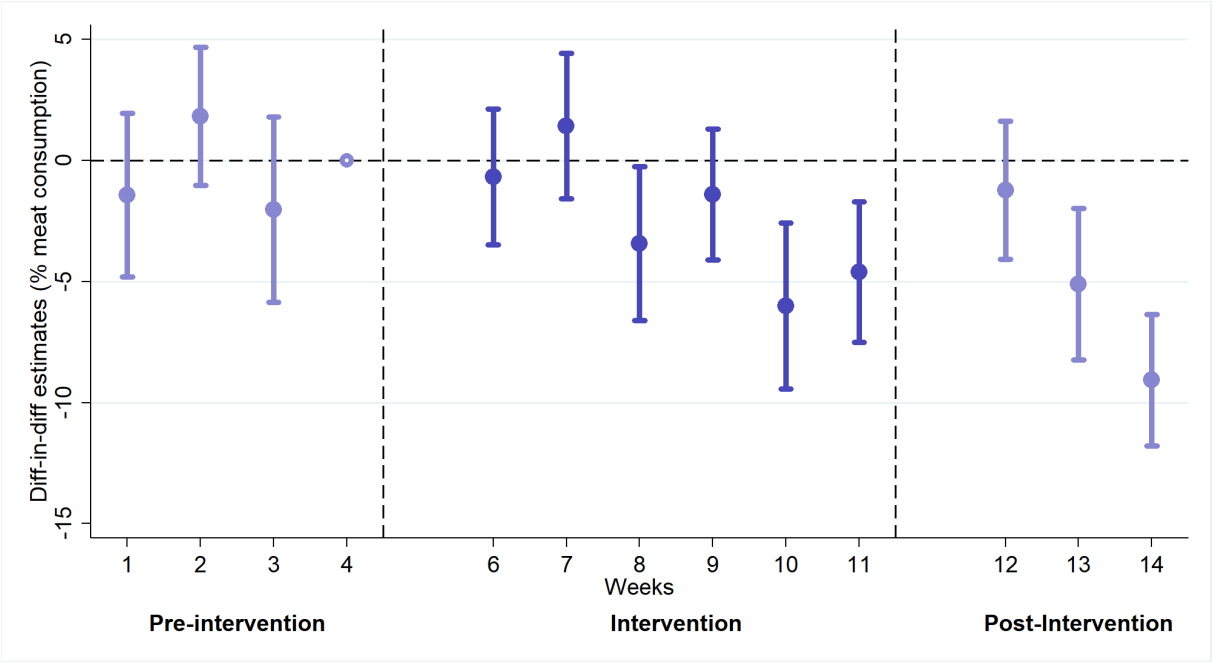

Figure 15: Event study: Difference in difference estimates of likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1-4 constitute the pre-intervention phase, while weeks 6-11 constitute the intervention phase, and weeks 12-14 the post-intervention phase. The regression specification closely follows specification (3) in Table 6, controlling for the number of vegetarian dishes, the price difference between vegetarian and meat meal, the total number of meal options and total daily sales. It also includes daily fixed effects. Bars indicate 95% confidence intervals.

5 EFFECTS ON CONSUMER SURPLUS

In both lab-in-the-field experiments, participants indicate their willingness to pay for being shown carbon labels. These elicitations were incentivized as described in section 2 and serve as an indication of the effect labels have on consumer surplus. The frequency distribution of willingness to pay values is visualized in Figure 16. About 50% of participants have a willingness to pay of 0, while less than 5% have a negative willingness to pay. The remaining participants are willing to pay a positive amount, with 21% of the sample willing to pay €0.50 and above. Values barely differ between treatment groups, although willingness to pay seems to be slightly higher among those who have not yet seen labels in the course of the experiment (Table 21 in the Appendix).

Table 22 in the Appendix shows a correlation analysis between willingness to pay to see carbon labels and individual characteristics. Willingness to pay for seeing labels is strongly positively correlated with participants' approval of carbon labels being shown in the student restaurant and participants' interest in using this information. It is also positively correlated with participants' perceived strength of social norms, as measured using the procedure developed by Krupka and Weber (2013). Willingness to pay to see carbon labels is weakly negatively correlated with participants' self-reported confidence in existing knowledge of emission values. Further, participants' self-control in eating behavior (as elicited using the questionnaire developed by Haws et al. (2016)) is very weakly correlated with willingness to pay to see emission values. Thunström (2019) find a similar, but much stronger relation between the experience of calorie labels and self control. Table 23 shows that the correlation between participants' willingness to pay to see carbon labels and the reaction observed to carbon labels previously in the experiment is very strong.

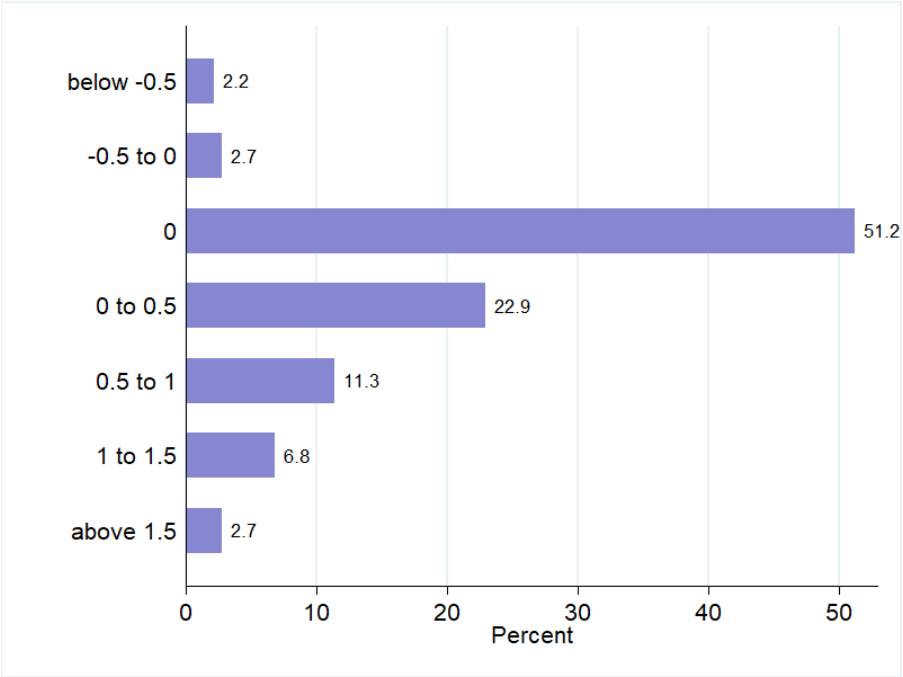

Figure 16: Average willingness to pay to see labels on the final three consumption decisions. Includes data from 733 participants from both lab-in-the-field experiments.

After the field experiment was completed, student restaurant guests were asked in a follow-up survey whether they would like the labels to be installed permanently. 73% of the 234 participants were in favor of installing the labels permanently, 18% were not sure and 9% against the measure.

6 DISCUSSION

This paper has provided evidence that carbon labels partly impact behavior by correcting biased beliefs, but that the direction of attention is likely the more influential channel. This finding speaks in favor of the introduction of carbon labels even in relatively knowledgeable populations and over longer periods of time. In the student restaurant setting, the effect of a carbon label is estimated as similar to a carbon tax of €120/ Ton, which is almost four-fold the current German carbon tax on petrol. Evidence from a field experiment shows that the effects of the carbon label persist over a five-week period and also in the three weeks after labels are removed. Further, psychological costs imposed on consumers through the labels appear to be minimal. Carbon labels thus seem to be a promising policy measure, at least as long as carbon taxes are not politically feasible.

Further research would be beneficial to test the effectiveness of carbon labels in other consumption contexts and other target populations. The experiment design I propose can be easily adapted for this purpose, enabling comparison across domains and populations.

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APPENDIX A1: DETAILS ON EXPERIMENTAL SET-UP

Survey accompanying field experiment

Pre-intervention survey

During the second week of April, I conducted a survey among student restaurant guests at the treatment student restaurant and the first, larger, control restaurant. The survey was advertised as an opportunity to voice one's opinion on the offer of the student restaurant, took participants around five minutes, and motivated potential participants with the chance to win one of ten 50-Euro coupons for the student restaurant. 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 17). 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 17: Leaflet advertising participation in the survey, as distributed in front of the student restaurant.

In the survey, respondents indicated their student restaurant 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 restaurant offer. Responses to the questions on student restaurant offer and participant comments were analyzed, summarized, and presented to the gastronomic manager of the student restaurants. Over 1,700 restaurant guests participated in this first survey, 94% of these students.

Post-intervention survey

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

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

APPENDIX A2: ADDITIONAL TABLES AND FIGURES

Results of lab-in-the-field experiment 1 including only non-vegetarians

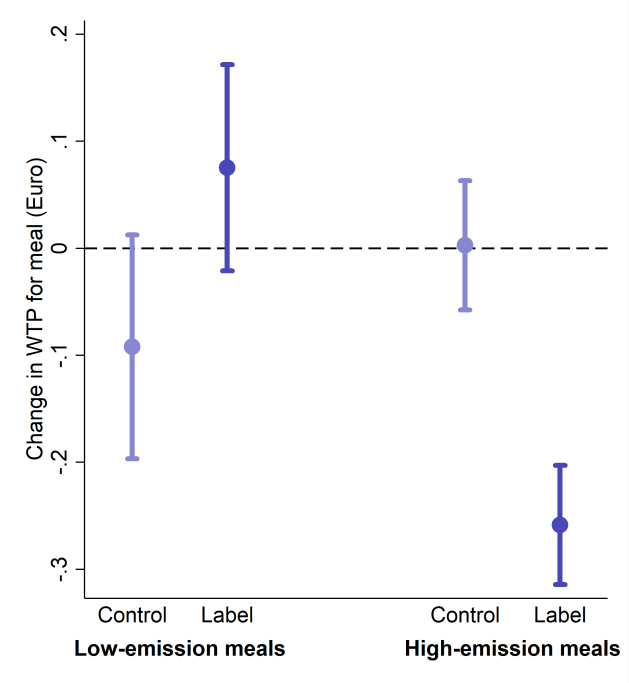

Figure 18: Within-subject change in willingness to pay for a specific meal, differentiated between participants in the "Control" and "Label" condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

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

Standard errors in parentheses

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

Table 7: Dependent variable: within-subject change in willingness to pay for a specific meal, compared to baseline. Spec. (1) corresponds to Figure 7 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. In specification (2), emissions (kg) is 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.

Results of lab-in-the-field experiment 1 including only vegetarians

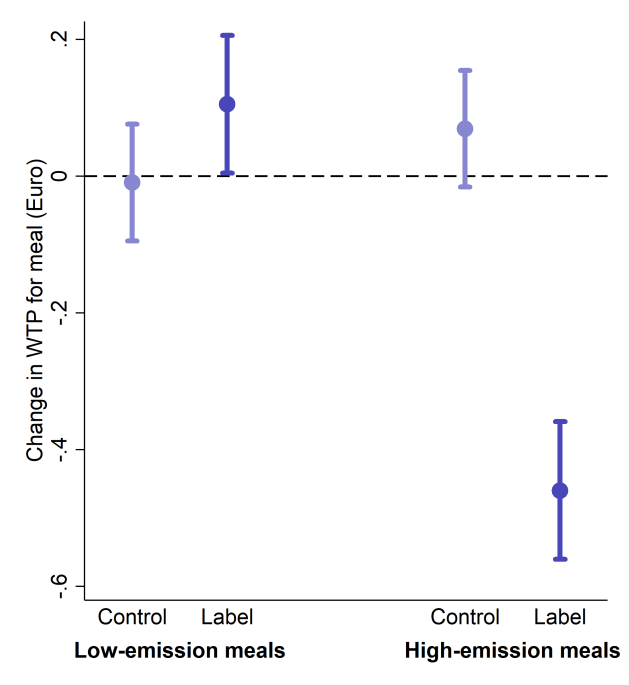

Figure 19: Within-subject change in willingness to pay for a specific meal, differentiated between participants in the "Control" and "Label" condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal x Shown label	-0.53***	
	(0.11)	
Low emission meal x Shown label	0.11	
	(0.07)	
High emission meal	0.06	
	(0.05)	
Low emission meal	-0.02	
	(0.04)	
Emissions(kg) x Shown label		-0.75***
		(0.18)
Emissions(kg)		0.08
		(0.08)
Shown label		-0.08
		(0.05)
Control for third round	0.04	0.04
	(0.04)	(0.04)
Constant		0.00
		(0.03)
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 8: Dependent variable: within-subject change in willingness to pay for a specific meal, compared to baseline. Spec. (1) corresponds to Figure 7 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. In specification (2), emissions (kg) is 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.

Results of lab-in-the-field experiment 2 including only non-vegetarians

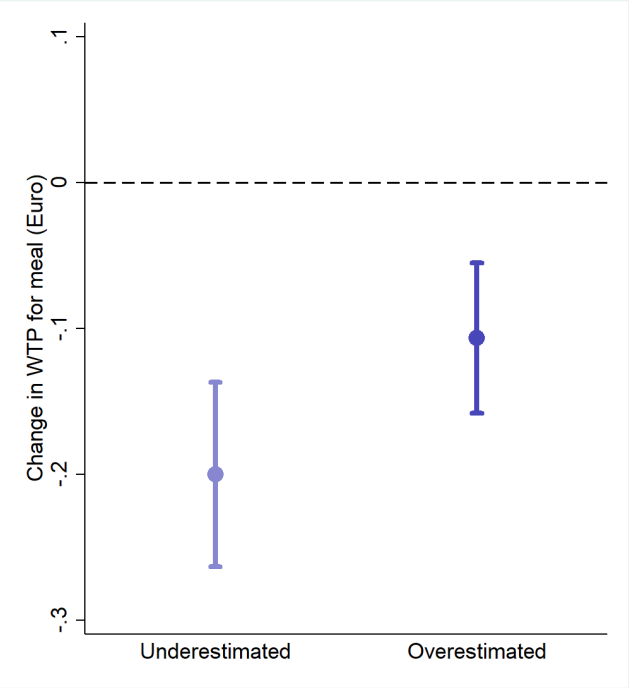

Figure 20: Within-subject change in willingness to pay for a specific meal when shown carbon labels, depending on whether the participant previously over- or underestimated the difference in emissions between the specific meal and the cheese sandwich. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped. Participants are all in the "Attent+Label" condition. Bars indicate 95% confidence intervals.

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

Standard errors in parentheses

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

Table 9: Dependent variable: within-subject change in willingness to pay for a specific meal when shown carbon labels ("Attent+Label" condition). For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped. In spec. (1), treatment effects of the carbon label are split into a constant effect and the additional effect of previous underestimation. In specification (2), change in willingness to pay is regressed on underestimation in kg.

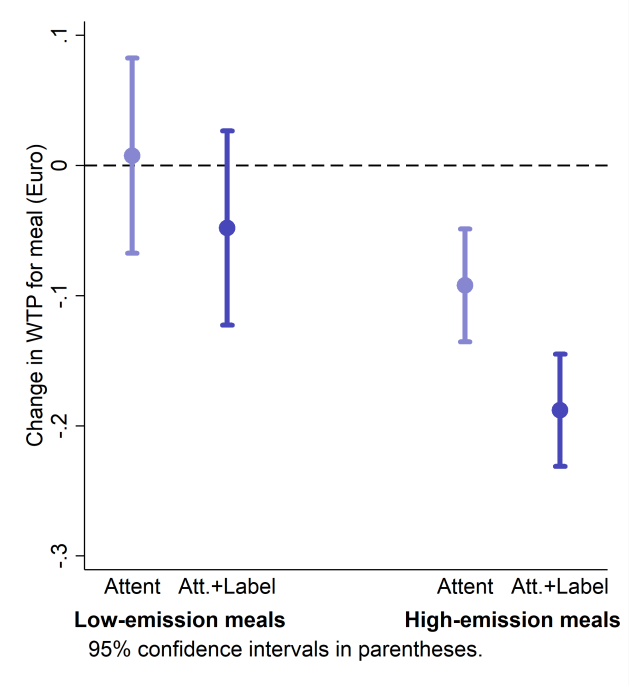

Figure 21: Within-subject change in willingness to pay for a specific meal, comparing participants in the "Attent" and "Attent+Label" condition. Effects are split into meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline
	(1)
High emission meal x Shown label	-0.10**
	(0.04)
Low emission meal x Shown label	-0.06
	(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 10: Dependent variable: within-subject change in willingness to pay for a specific meal when made attent. Spec. (1) corresponds to Figure 12 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. "High emission meal" describes the pure effect of being made attent, "High emission meal x Shown label" the additional effect of seeing information.

Results of lab-in-the-field experiment 2 including only vegetarians

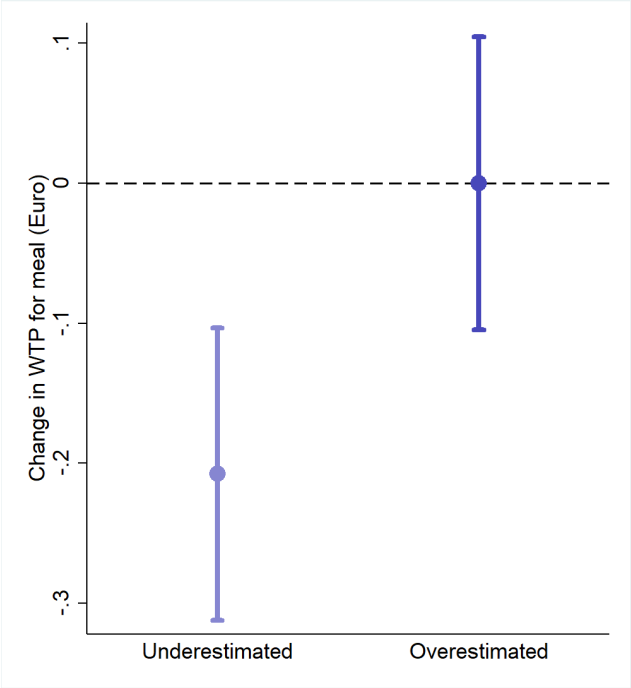

Figure 22: Within-subject change in willingness to pay for a specific meal when shown carbon labels, depending on whether the participant previously over- or underestimated the difference in emissions between the specific meal and the cheese sandwich. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped. Participants are all in the "Attent+Label" condition. Bars indicate 95% confidence intervals.

	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	58
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 11: Dependent variable: within-subject change in willingness to pay for a specific meal when shown carbon labels ("Attent+Label" condition). For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped. In spec. (1), treatment effects of the carbon label are split into a constant effect and the additional effect of previous underestimation. In specification (2), change in willingness to pay is regressed on underestimation in kg.

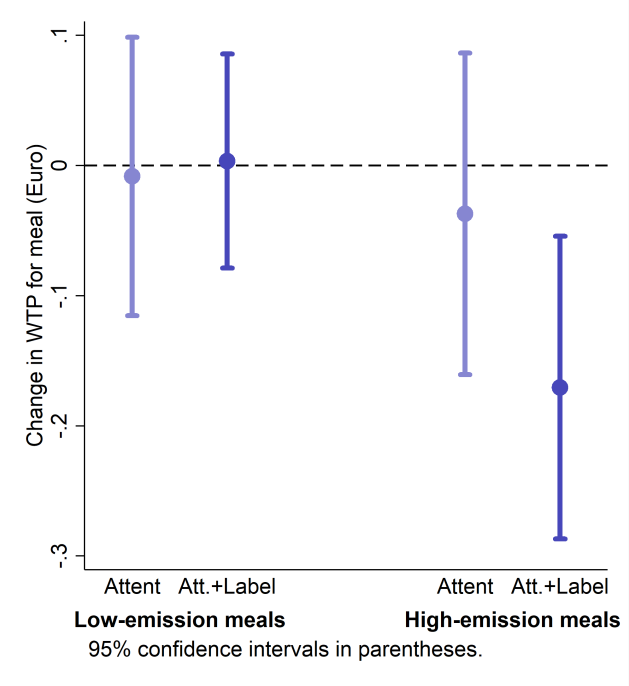

Figure 23: Within-subject change in willingness to pay for a specific meal, comparing participants in the "Attent" and "Attent+Label" condition. Effects are split into meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with high emissions (meals with emissions higher than the sandwich). Bars indicate 95% confidence intervals.

	Change in WTP compared to baseline
	(1)
High emission meal x Shown label	-0.12
	(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 12: Dependent variable: within-subject change in willingness to pay for a specific meal when made attent. Spec. (1) corresponds to Figure 12 and does not include a constant, because "Low emissions meal" and "High emissions meal" are mutually exclusive. "High emission meal" describes the pure effect of being made attent, "High emission meal x Shown label" the additional effect of seeing information.

Additional descriptives and robustness tests for under- and overestimation of emissions

Table 13: Lab-in-the-field experiment 2: Under- and overestimation of the emissions caused by the decision meals in the "Attention+Labels" group

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

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

Table 14: Lab-in-the-field experiment 2: Number of under- and overestimations per participant

No. overestimated	0	1	2	3	4	Total
No. underestimated						
0	0	0	0	8	40	48
1	0	4	84	216	0	304
2	4	96	512	0	0	612
3	16	124	0	0	0	140
4	68	0	0	0	0	68
Total	88	224	596	224	40	1.172

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

Table 15: Lab-in-the-field experiment 2: 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	289

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

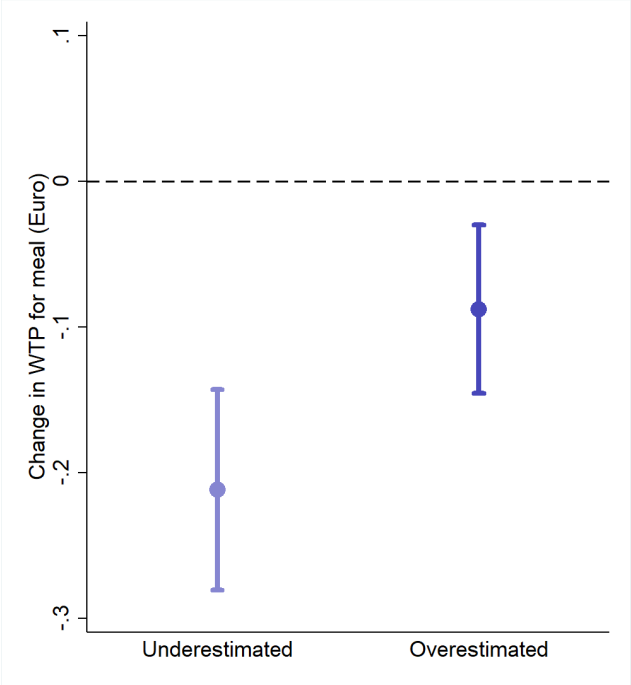

Figure 24: Replication of Figure 11 including only individuals with at least three correct ranks (194 participants). Bars indicate 95% confidence intervals.

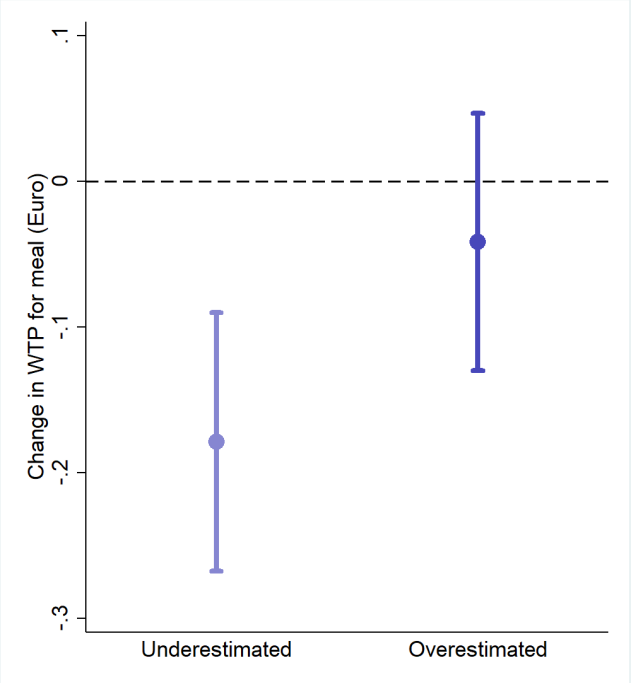

Figure 25: Replication of Figure 11 including only individuals with at most two correct ranks (99 participants). Bars indicate 95% confidence intervals. Bars indicate 95% confidence intervals.

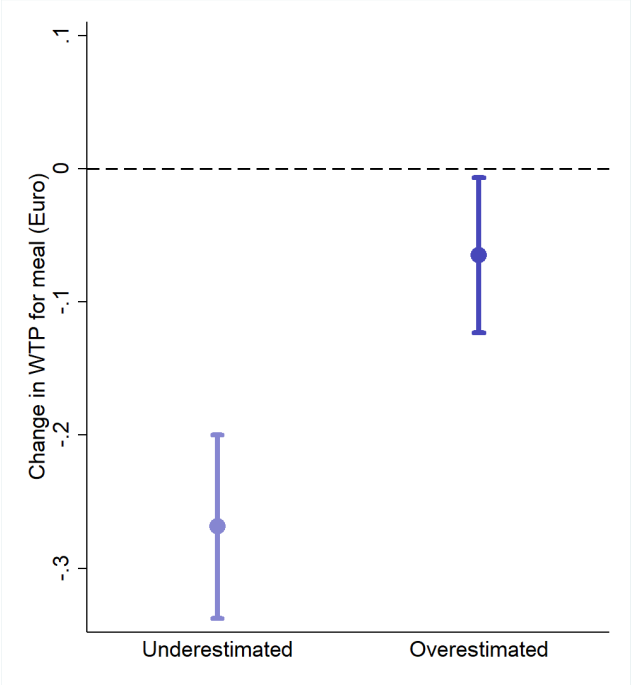

Figure 26: Replication of Figure 11 including only individuals with at least three correctly guessed magnitudes (171 participants). Bars indicate 95% confidence intervals.

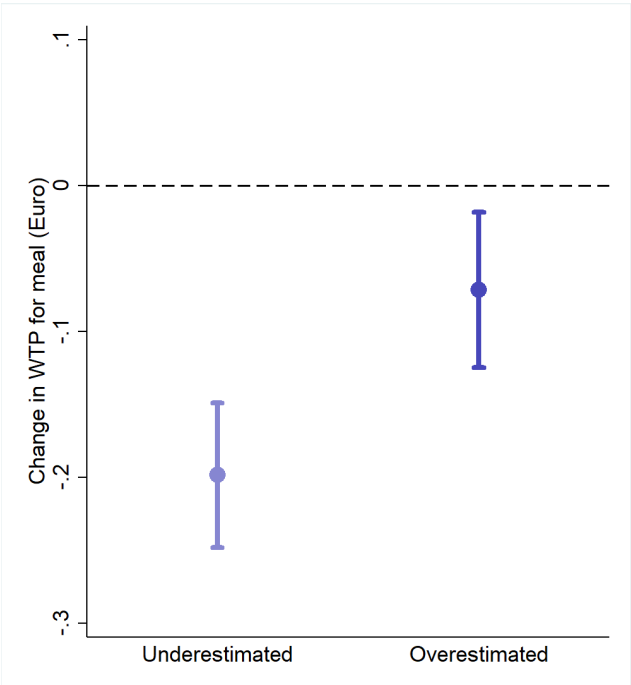

Figure 28: Replication of Figure 11 based on under- or overestimation of the specific meal, instead of under- or overestimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals.

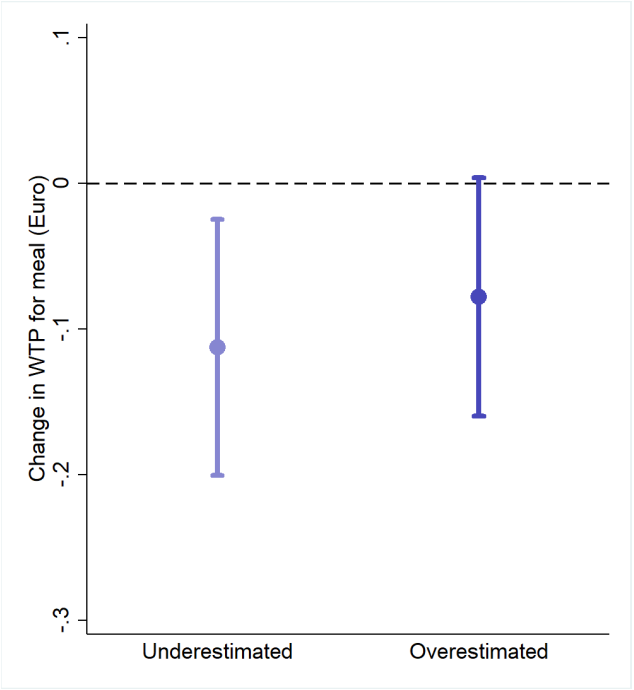

Figure 27: Replication of Figure 11 including only individuals with at most two correctly guessed magnitudes (129 participants). Bars indicate 95% confidence intervals. Bars indicate 95% confidence intervals.

	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 16: Replication of Table 4 based on under- or overestimation of the specific meal, instead of under- or overestimation of the difference in emissions between the meal and the cheese sandwich. Bars indicate 95% confidence intervals. In specification (2), change in willingness to pay is regressed on underestimation in kg. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission difference) are dropped.

Additional descriptives and results for the field experiment

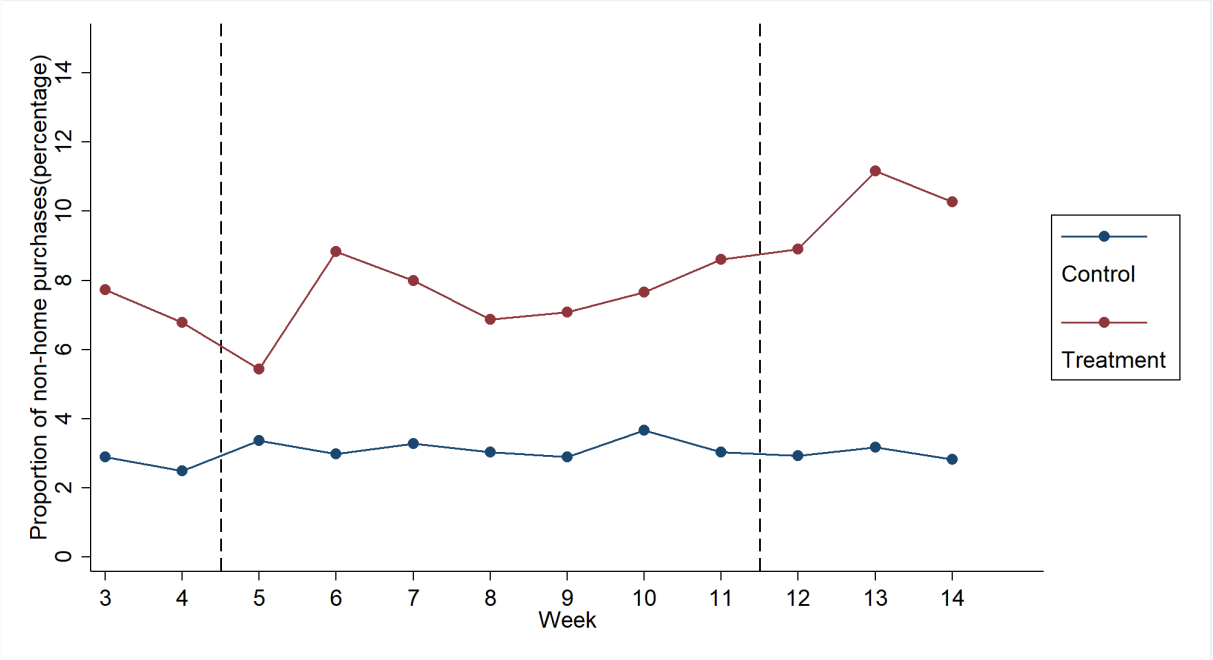

Figure 29: Proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks. In accordance with the individual fixed effects sample, the sample includes only payments made by individual card, and only includes guests who visited at least 10 times during the 12 week period, and at least once in the first two weeks. I further restrict the sample to those 93% of the remaining sample including only individuals who ate at the same restaurant in at least 80% of cases during those first two weeks.

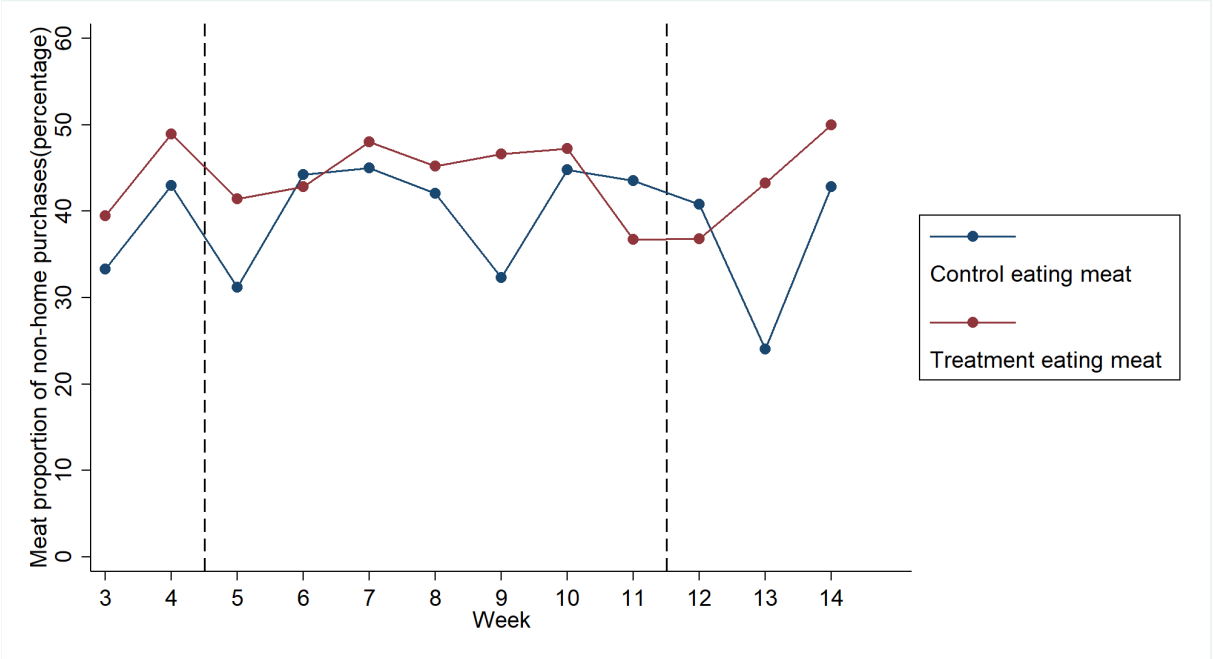

Figure 30: Proportion of choices for the meat component among non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first two weeks. In accordance with the individual fixed effects sample, the sample includes only payments made by individual card, and only includes guests who visited at least 10 times during the 12 week period, and at least once in the first two weeks. I further restrict the sample to those 93% of the remaining sample including only individuals who ate at the same restaurant in at least 80% of cases during those first two weeks.

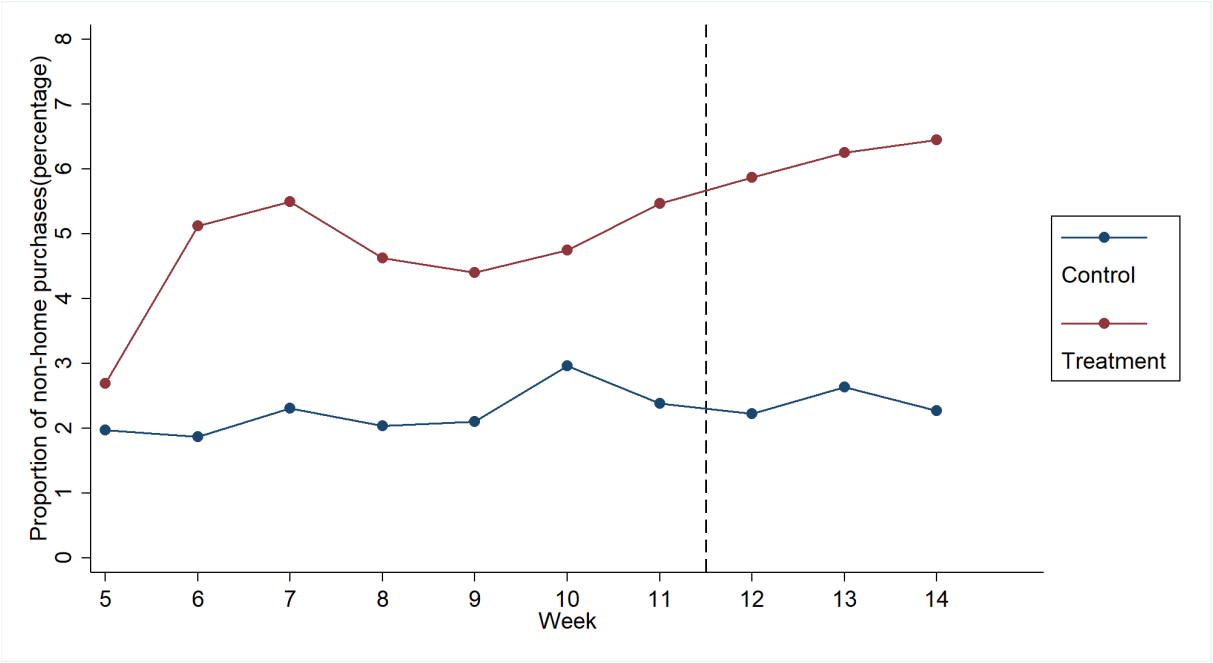

Figure 31: Proportion of non-home visits in percentage points, with classification as the "home" restaurant based on behavior in the first four weeks. In accordance with the individual fixed effects sample, the sample includes only payments made by individual card, and only includes guests who visited at least 10 times during the 12 week period, and at least once in the first four weeks. I further restrict the sample to those 93% of the remaining sample including only individuals who ate at the same restaurant in at least 80% of cases during those first two weeks.

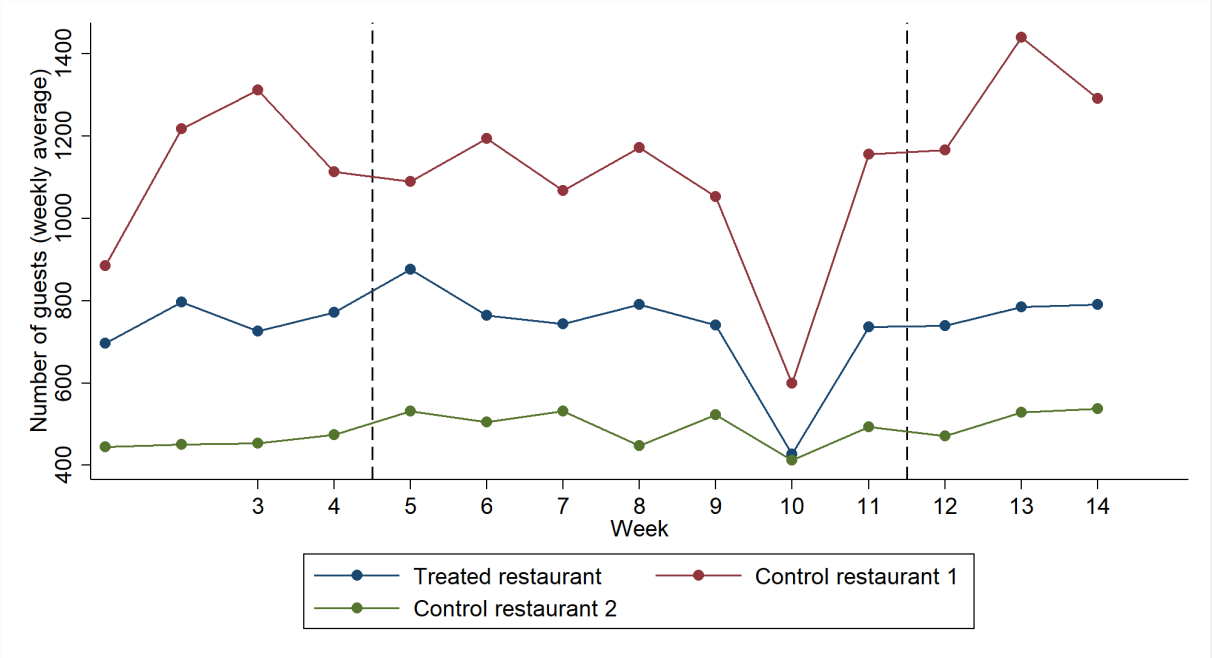

Figure 32: Number of daily student restaurant visitors, weekly average

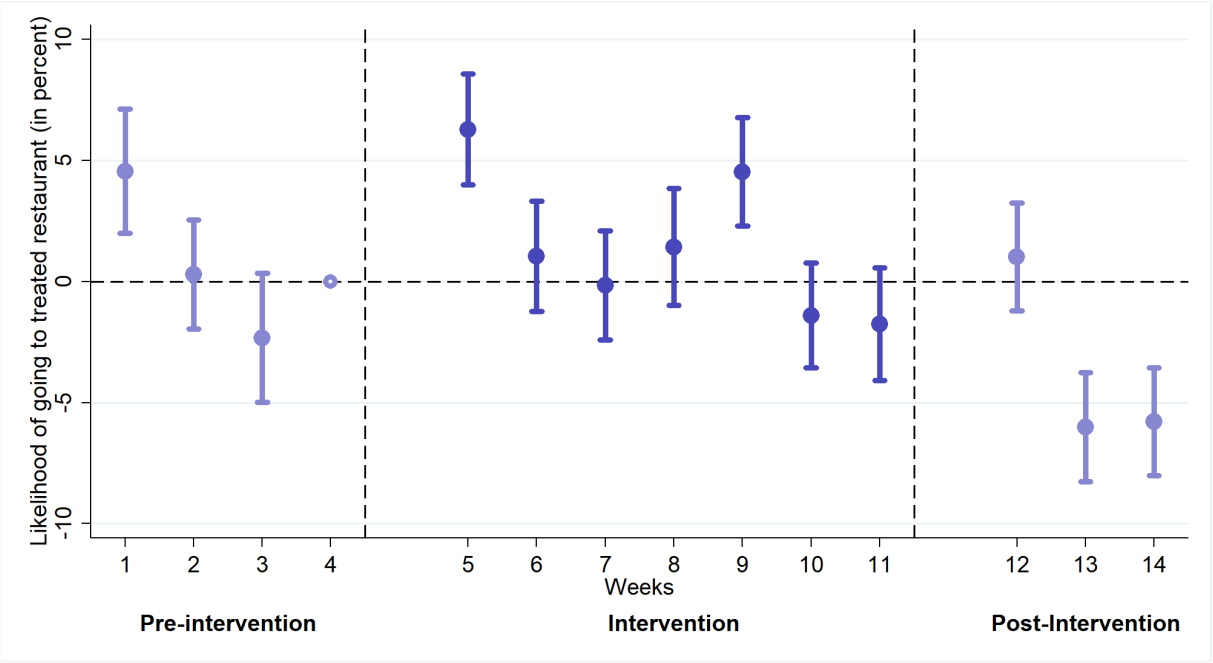

Figure 33: Event study: Difference in difference estimates of likelihood of visiting the treated student restaurants relative to the control restaurant (in percentage points) , using week 4 of the pre-intervention phase as a baseline. Weeks 1-4 constitute the pre-intervention phase, while weeks 6-11 constitute the intervention phase, and weeks 12-14 the post-intervention phase. The regression specification includes daily effects. Bars indicate 95% confidence intervals.

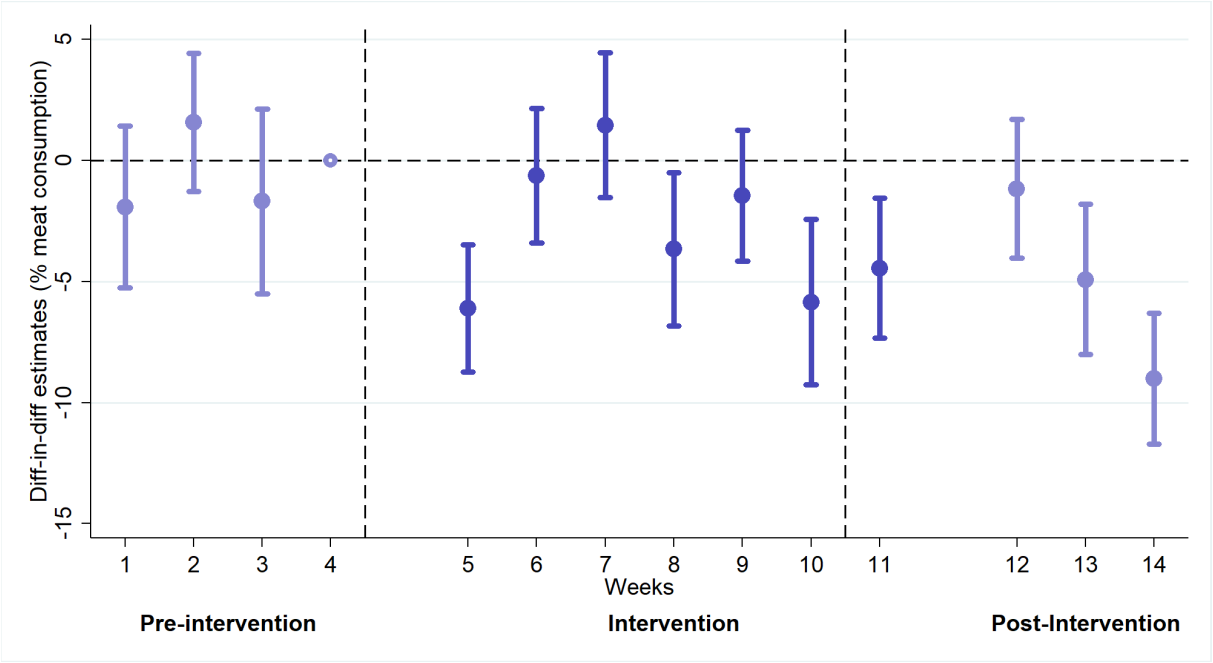

Figure 34: Event study: Difference in difference estimates of likelihood of consuming the meat option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1-4 constitute the pre-intervention phase, while weeks 6-11 constitute the intervention phase, and weeks 12-14 the post-intervention phase. The regression specification closely follows specification (3) in Table 6, controlling for the number of vegetarian dishes, the price difference between vegetarian and meat meal, the total number of meal options and total daily sales. It also includes daily fixed effects. Bars indicate 95% confidence intervals.

Table 17: Effect of labels on vegetarian consumption, different subsamples

	Likelihood of consuming meat (in per.)				
	(1) All	(2) Employees	(3) Non-busy time	(4) Card payment	(5) Frequent
Treatment restaurant x Label period	-2.07***	-5.43**	-2.37**	-3.09***	-2.88**
	(0.75)	(2.76)	(0.98)	(0.94)	(1.15)
Treatment restaurant x Post period	-5.27***	-10.84***	-3.42***	-7.40***	-6.50***
	(0.86)	(3.18)	(1.14)	(1.12)	(1.36)
Treatment restaurant	-13.44***	-2.43	-15.09***	-7.71***	-7.69***
	(0.76)	(2.40)	(1.02)	(1.03)	(1.24)
Second veg. main	-3.59***	-1.87	-3.12***	-4.86***	-4.52***
	(0.51)	(1.39)	(0.69)	(0.68)	(0.81)
Price difference	-5.17***	-4.86	-6.68***	-5.59**	-5.78**
	(1.90)	(5.54)	(2.52)	(2.43)	(2.93)
Number of meal options	-1.47***	-0.38	-2.26***	-0.77**	-0.62
	(0.26)	(0.63)	(0.35)	(0.34)	(0.41)
Total daily sales	-0.91***	-0.25**	-1.04***	-0.10	-0.25***
	(0.06)	(0.12)	(0.08)	(0.08)	(0.09)
Date effects	Yes	Yes	Yes	Yes	Yes
Fixed effects	No	No	No	No	No
Guests control	6,935	883	3,808	6,927	2,246
Guests treated	2,822	266	1,684	2,817	864
Observations	121,071	21,052	68,215	82,745	58,264

Standard errors in parentheses

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

Table 18: Effect of labels on vegetarian consumption, different subsamples

	Likelihood of consuming meat (in per.)				
	(1) All	(2) Survey	(3) Male	(4) Above 23	(5) Env. important
Treatment restaurant x Label period	-3.09***	-4.54**	-3.94	0.98	-3.79
	(0.94)	(1.86)	(2.78)	(3.31)	(2.32)
Treatment restaurant x Post period	-7.40***	-8.72***	-9.60***	-7.36*	-7.03**
	(1.12)	(2.27)	(3.50)	(4.11)	(2.82)
Treatment restaurant	-7.71***	8.96***	3.40	5.40	2.92
	(1.03)	(2.38)	(4.24)	(4.18)	(2.96)
Second veg. main	-4.86***	-5.41***	-8.96***	-6.66**	-2.84
	(0.68)	(1.49)	(2.45)	(2.74)	(1.76)
Price difference	-5.59**	-0.39	0.31	-3.65	-6.32
	(2.43)	(4.99)	(7.65)	(9.09)	(6.31)
Number of meal options	-0.77**	1.09	1.34	0.37	0.40
	(0.34)	(0.80)	(1.30)	(1.42)	(0.99)
Total daily sales	-0.10	1.77***	1.32***	1.89***	1.20***
	(0.08)	(0.22)	(0.47)	(0.40)	(0.25)
Date effects	Yes	Yes	Yes	Yes	Yes
Fixed effects	No	No	No	No	No
Guests control	6,927	907	362	301	472
Guests treated	2,817	560	247	191	249
Observations	82,745	16,439	8,091	5,326	7,704

Standard errors in parentheses

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

Table 19: Effect of labels on average emissions per meal

	(1) GHGE (g)	(2) GHGE (g)	(3) GHGE (g)	(4) GHGE (g)
Treatment restaurant x Label period	-16.18	-24.78**	-23.52**	-51.40
	(11.21)	(10.22)	(10.19)	(37.28)
Treatment restaurant	-50.41***	-45.32***	-49.39***	
	(7.40)	(6.70)	(8.23)	
Label period	4.72			
	(6.25)			
Number of meal options			2.93	27.53**
			(3.37)	(13.25)
Total daily sales			-6.83***	
			(1.14)	
Date effects	No	Yes	Yes	Yes
Fixed effects	No	No	No	Yes
Guests control	5,087	5,087	5,087	168
Guests treated	1,987	1,987	1,987	37
Observations	33,711	33,711	33,711	2,365

Standard errors in parentheses

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

Dependent variable: Meals caused by main meal component, in gram. Specifications (2)-(4) include date effects, and the "Label period" indicator is thus dropped due to collinearity. Specification (4) includes individual fixed effects, and the "Treated" indicator is thus dropped due to collinearity. Sample is restricted to days in the intervention period for which there is a "gastronomic twin" in the pre-intervention period and to sales of the two main meal components, as described in section 4 "Data and Results" in the main text.

Table 20: Effect of labels on vegetarian consumption with restricted sample

	(1) Veg. meal	(2) Veg. meal	(3) Veg. meal	(4) Veg. meal
Treatment restaurant x Label period	6.25***	6.79***	6.66***	12.85***
	(1.43)	(1.43)	(1.46)	(4.70)
Treatment restaurant	4.57***	4.03***	5.50***	
	(1.38)	(1.37)	(2.01)	
Label period	-0.58			
	(0.73)			
Second veg. main			-2.31	-1.97
			(1.50)	(3.84)
Price difference			-20.05***	-0.52
			(6.16)	(15.73)
Number of meal options			-0.16	-1.75
			(0.57)	(1.56)
Total daily sales			0.14	-1.72*
			(0.21)	(0.89)
Date effects	No	Yes	Yes	Yes
Fixed effects	No	No	No	Yes
Guests control	5,076	5,076	5,076	159
Guests treated	1,994	1,994	1,994	32
Observations	23,091	23,091	23,091	2,223

Standard errors in parentheses

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

Dependent variable: 0/1 indicator for consumption of the vegetarian option, multiplied by 100 to enable the interpretation of coefficients as percentage points. Specifications (2)-(4) include date effects, and the "Label period" indicator is thus dropped due to collinearity. Specification (4) includes individual fixed effects, and the "Treated" indicator is thus dropped due to collinearity. Sample is restricted to days in the intervention period for which there is a "gastronomic twin" in the pre-intervention period and to sales of the two main meal components, as described in section 4 "Data and Results" in the main text.

Table 21: Willingness to pay for seeing carbon labels by treatment group

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

Standard errors in parentheses

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

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

Table 22: Correlations between willingness to pay for seeing carbon labels and individual characteristics

	(1)	(2)	(3)	(4)	(5)
Perceived strength of social norms	0.01*				
	(0.01)				
In favor of labels in student restaurant		0.03***			
		(0.01)			
Self-reported willingness to use info			0.03***		
			(0.01)		
Self-reported confidence in own knowledge				-0.03	
				(0.02)	
Eating self-control					0.01
					(0.03)
Constant	0.15***	-0.03	0.03	0.20***	0.20***
	(0.03)	(0.06)	(0.04)	(0.02)	(0.02)
Observations	732	732	732	732	732

Standard errors in parentheses

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

Note: Dependent variable: Willingness to pay for seeing labels for the final three consumption decisions. "In favor of labels in student restaurant" is measuring using approval of the statement "I would appreciate if the student restaurant 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." 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 et al. (2016).

Table 23: Correlations between willingness to pay for seeing carbon labels and treatment effect

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

Standard errors in parentheses

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

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