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

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Abstract

This project provides evidence on the effectiveness of carbon labels and on the behavioral channels driving consumption responses. In a lab-in-the-field experiment conducted in a student restaurant setting, the effect of the carbon label is estimated as similar to a carbon tax of Euro 120/ Ton. This is four-fold the current German carbon tax on energy and transport. A second lab-in-the-field experiment provides first evidence on behavioral channels. Contrary to the common notion that effects are driven by a correction of misperceptions about carbon impact, this channel only provides a partial explanation to the consumption changes observed. Results instead point towards the direction of attention as the more influential channel. The labels do not seem to impose disproportionate psychological costs, as I elicit a positive or neutral effect of the labels on consumer surplus for almost all participants. I complement evidence from the one-shot lab-in-the-field setting with evidence from a large-scale field experiment, during which carbon labels were installed in one of the University of Bonn's student restaurants. Effects observed are in line with lab-in-the-field estimates and persist over a five-week label period and in the three-week post-intervention observation period. A post-intervention survey in the field experiment setting finds 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

It is estimated that the food system causes 26% - 34% of global greenhouse gas emissions (Poore and Nemecek (2018), Crippa et al. (2021)). The modeling results of 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)). 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. 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)).

To evaluate whether this attention is justified, we need to first better understand how effective carbon labels are compared to other policy instruments. However, current evidence assesses the effectiveness of carbon labels in terms of the percentage changes in consumption behavior achieved in a specific experimental setting. This percentage change will naturally strongly depend on the setting-specific consumption offer and pricing structure, making it difficult to compare effects across consumption contexts and policy instruments. A second challenge is that our understanding of the behavioral channels through which carbon labels affect consumers is very limited. Previous literature (e.g. 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. 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 project provides causal evidence on both the effectiveness of carbon labels and the behavioral channels through which they impact consumption behavior. Using a lab-in-the-field experiment, I provide a first experimental estimate of how the effect of carbon labels quantifies relative to a carbon tax. I do so by eliciting subjects' incentivized willingness to pay for different restaurant meals, and then repeating the elicitation, showing carbon labels to the treatment group but not the control group.[1] Comparing meal-specific changes in demand with the emissions caused by each of the meals gives an estimate of how high of a carbon tax would produce similar changes in demand as the carbon labels. I estimate this amount at €120 Ton, almost five-fold the German carbon tax on petrol and slightly lower than estimates of the social cost of carbon (e.g. Rennert et al. (2022)).

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

A second lab-in-the-field experiment provides evidence on the behavioral channels through which the carbon labels impact consumers. The design closely follows that of the experiment described above, but subjects are additionally asked between the elicitations to guess the emissions caused by each meal. This captures how much each consumer under- or overestimates the emissions caused by a certain meal. 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 emissions were underestimated, and upward if emissions were overestimated. 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.

An additional treatment condition reveals that the direction of attention is likely the more relevant behavioral channel. For a separate group of participants, attention is directed toward the issue of carbon emissions, but participants are not provided with any emissions information. In this group, willingness to pay is also elicited twice, with subjects asked to guess the emissions caused by each meal between the elicitations. Attention is also directed towards carbon labels through the belief elicitation, but participants are not shown carbon labels in the second willingness to pay elicitation. Remarkably, this treatment produces similar changes in demand as providing participants with carbon labels. Together, these observations point at the direction of attention being at least as important of a channel as the correction of biased beliefs. In terms of policy implications, this finding speaks towards implementing carbon labels even in relatively knowledgeable populations and retaining them over longer periods to retain attention.

The lab-in-the-field experiments included a total of over 700 participants, each of whom completed the experiment online and then directly traveled to the University of Bonn to pick up the experiment payment in cash and the restaurant meal corresponding to the participant's choice in the decision chosen for implementation. Meals were warm and perishable and meant for immediate consumption. The decisions made in the lab-in-the-field experiment thus reflected a typical lunch choice.

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 9.000 guests, I estimate that the labels increase consumption of the vegetarian (the lower carbon) meal by 3.2 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.

Both the lab-in-the-field experiments and the field experiment provide evidence that the effect of the carbon labels on consumer surplus is neutral or positive. 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. Similarly, 70% of guests affected by the labels in the field experiment would like the labels to be installed permanently. This is the first evidence on the psychological impact of environmental labels and is important information both for the design and implementation of behavioral interventions. It suggests that the intervention is correcting an internality and thereby raising consumer surplus rather than imposing additional psychological costs on the consumer.

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. My contribution is also methodological, since my experimental design (based on 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), Allcott and Kessler (2019)) and on calorie labels in hypothetical meal choices (Thunström (2019)). 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

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)). 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).[2] Participants were informed that the experiment would be conducted online, but that they would be required to travel 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)).

The lunch provided to participants and their pay-out directly depends on one of the meal purchasing decisions they make in the online experiment. It is not known to participants for which decision this is the case and it is thus in their best interest to make each meal purchasing decision as if it was to have real consequences. As meals are directly provided to participants, participants are making a choice for immediate consumption and not for some point in the future. The experiment is designed in such a

[2] Vegetarians are excluded from the main analysis since the experiment is adjusted for vegetarians to only vegetarian meals. The findings described below replicate with vegetarian participants.

way that participants will always be provided with a meal, regardless of the choices they make. This mimics the real-life fact that the alternative to not purchasing lunch is not "not eating", but choosing a different lunch. When participants pick up their meal, it is warm, ready-to-eat, and can 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 ingredient lists provided by the student restaurant.

Experiment timeline

The experiment timeline is visualized in Figure 1. After responding to comprehension questions, participants indicate their baseline willingness to pay for four meals, as detailed below. During these baseline willingness to pay questions, participants are neither provided with emission labels nor is their attention directed towards the issue of greenhouse gas emissions in any way. Afterward, participants answer several incentivized and timed[3] 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 randomly assigned to. 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 are now shown greenhouse-gas-emission labels.

To increase power and elicit further information, participants' willingness to pay for the same four meals is elicited a third time[4], 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.[5]
- 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.[6].

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

[3]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.

[4]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.

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

[6]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.

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.

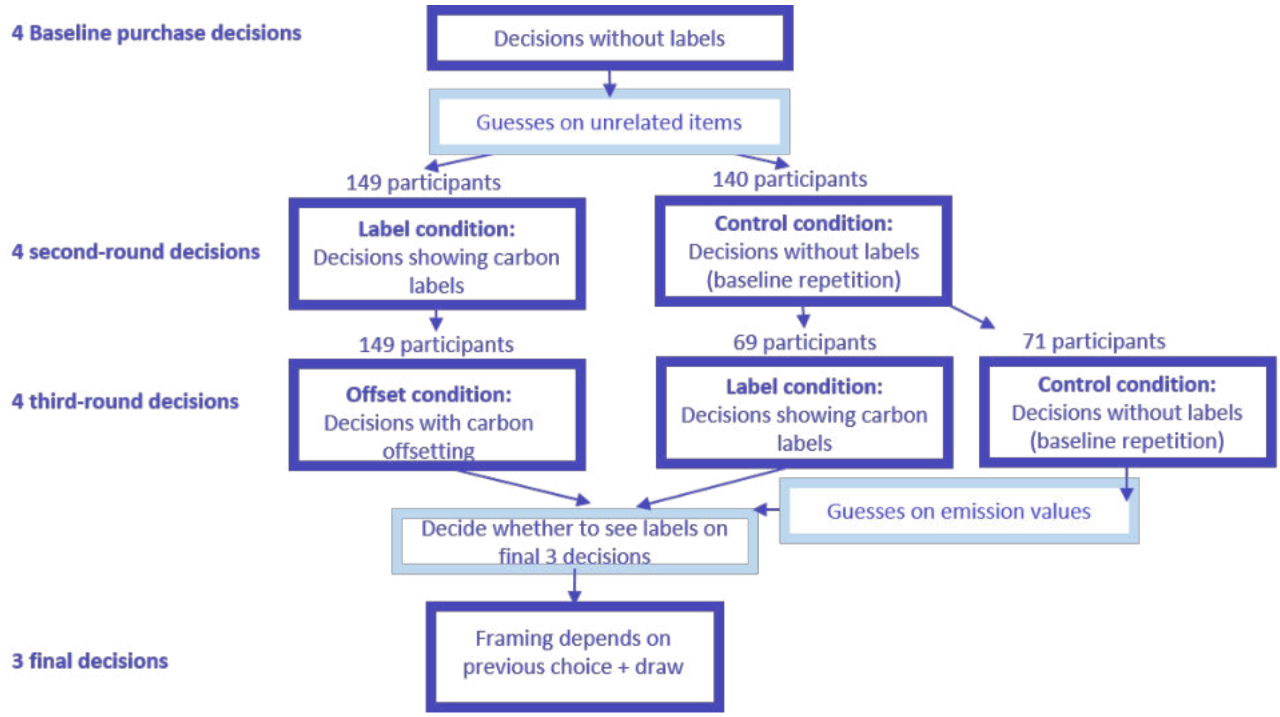

Figure 1: Experiment schedule and treatment groups

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).

The 4 baseline decisions

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.[7] 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). Any amount between €0.00 Euro and €3.00 can be indicated on a slider in five-cent intervals. If participants prefer the specific meal, they indicate how much they are willing to forego to ensure they receive this meal

[7] 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.

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.

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.

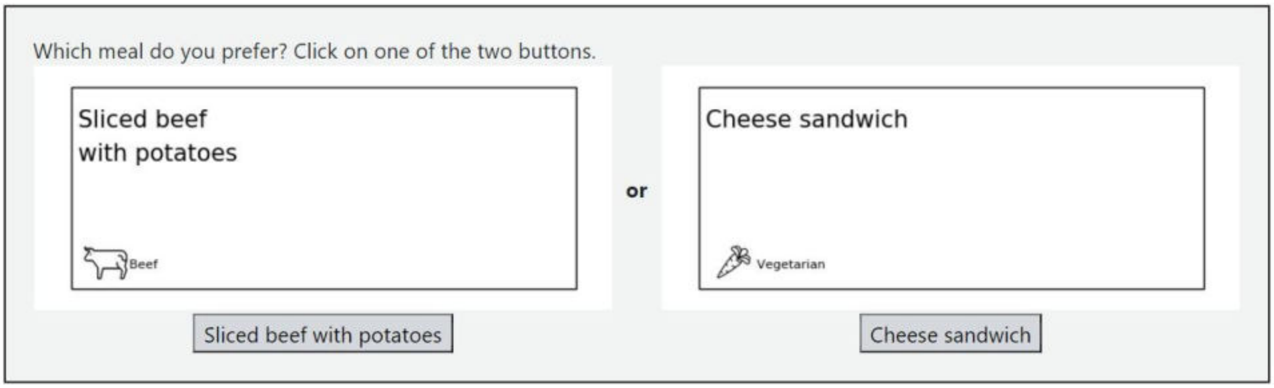

Figure 2: Meal purchase decision example: Step 1 of the purchasing decision

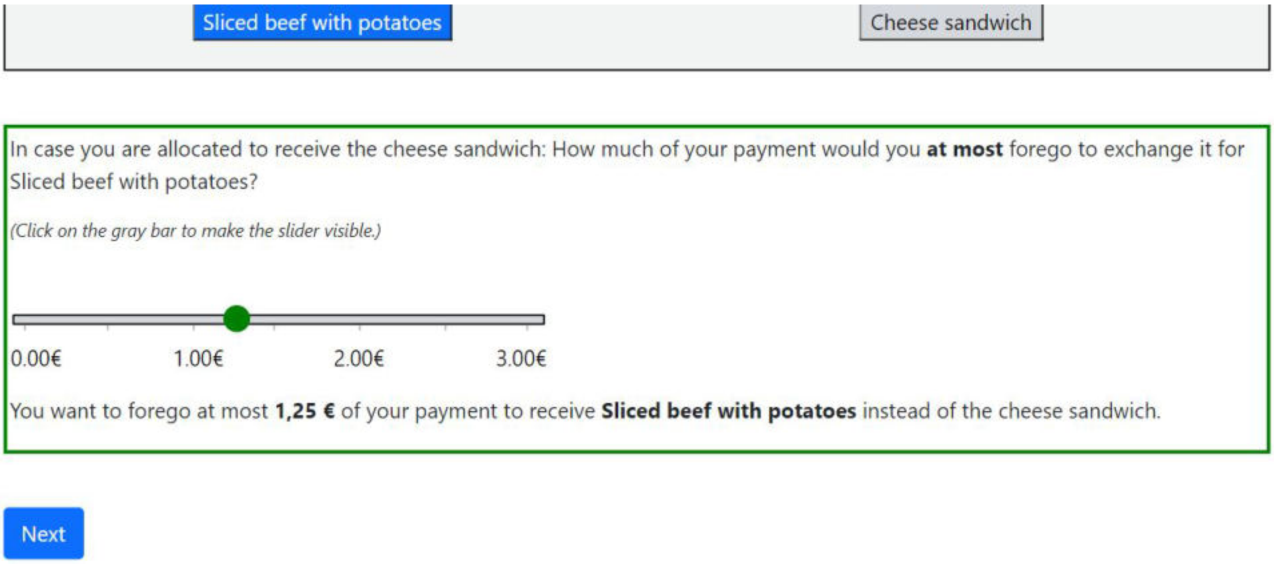

Figure 3: Meal purchase decision example: Step 2 of the purchasing decision

The 4 second-round and 4 third-round decisions

The 4 second-round and 4 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.[8]

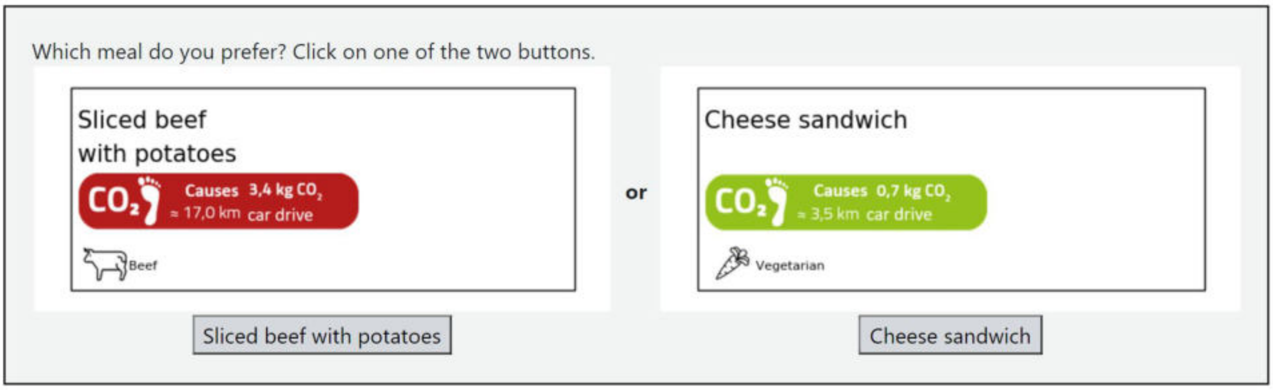

Figure 4: Meal purchase decision example: Decisions with labels

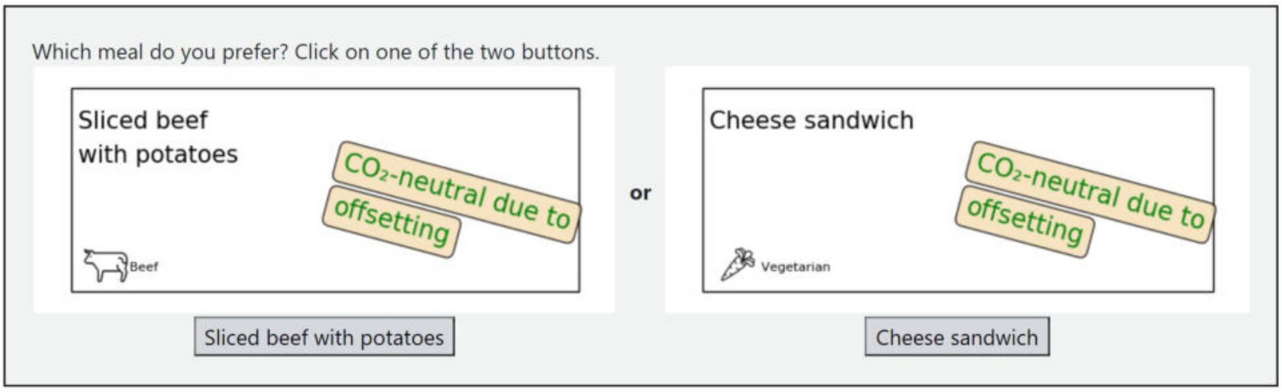

Figure 5: Meal purchase decision example: Decisions with carbon offsetting

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).

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

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

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.

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.

Data and results

Some observations were excluded as pre-registered (Schulze Tilling (2021b)). 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). Meat-eaters and vegetarians are analyzed separately, so this has no large consequences for the analysis.

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). I include only consumption decisions made by non-vegetarians, to keep meals constant across participants. 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.

- 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 €0.26. 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 €0.14 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).

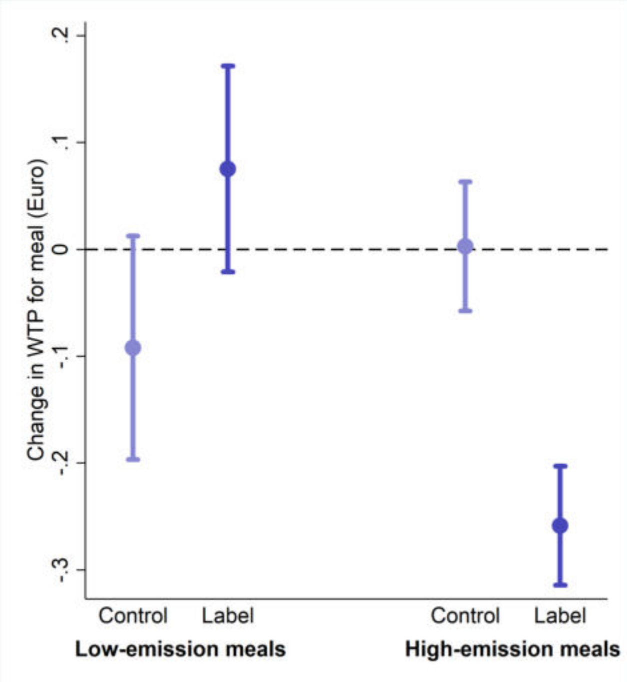

Figure 7: Within-subject change in willingness to pay for a specific meal, differentiated between participants in the "Control" and "Label" condition. Effects are split into effects for meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) 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	-0.26***	
	(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 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

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.

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).[9] 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.

[9]This reference meal is not included in any willingness to pay elicitations.

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.[10]

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.

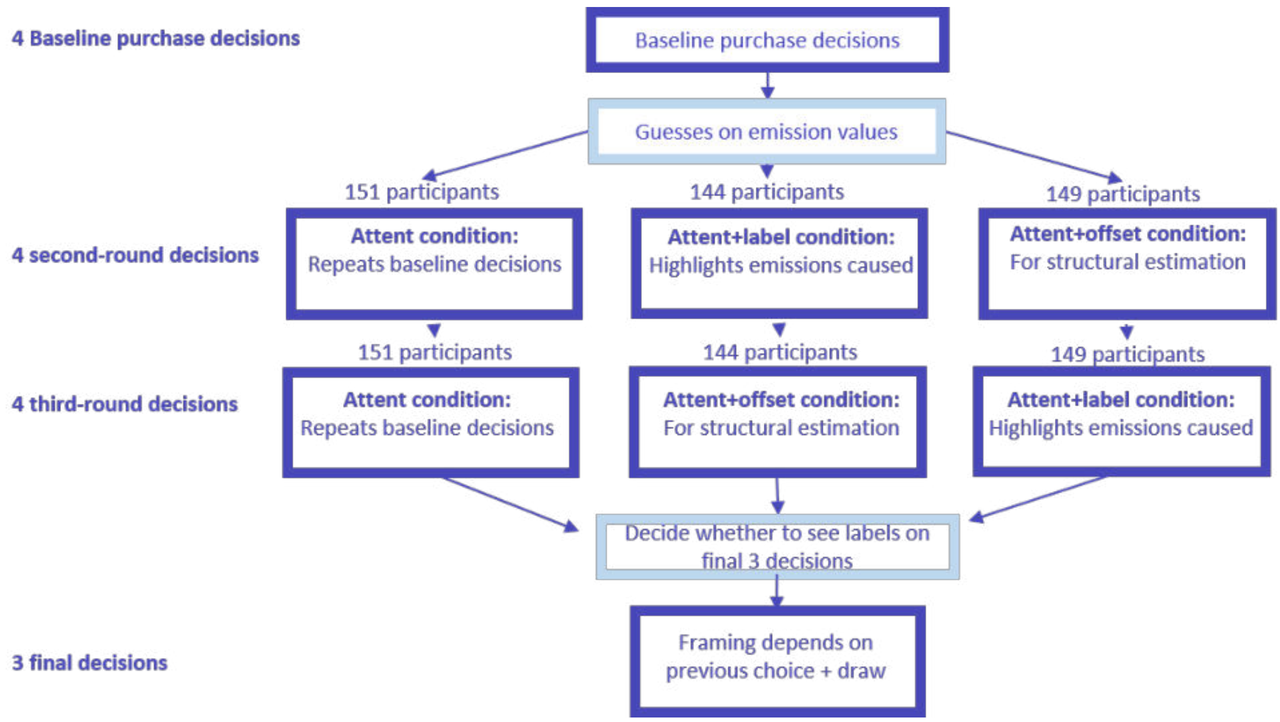

Figure 8: Experiment schedule and treatment groups

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

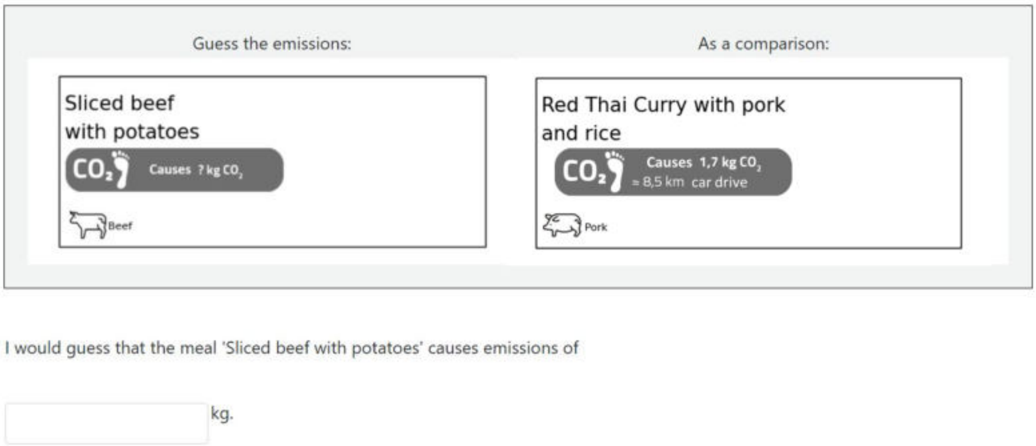

Figure 9: Example guessing questions

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.

Data and results

Some observations were excluded as pre-registered (Schulze Tilling (2021b)). 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		

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).

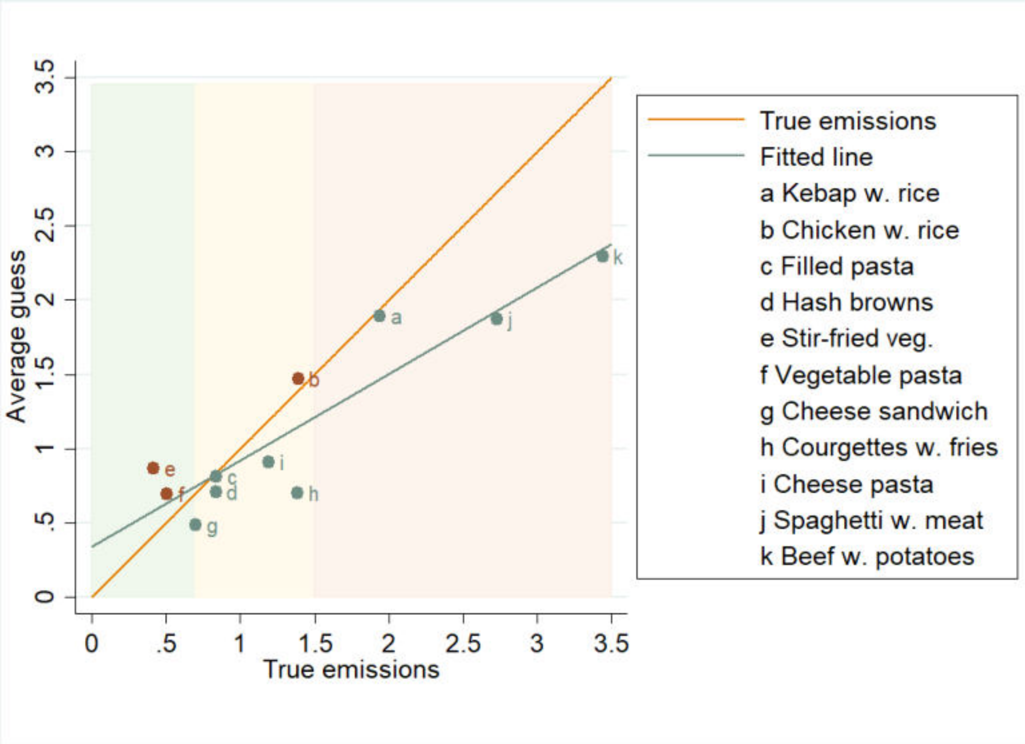

Figure 10: Average guess of the emissions caused by a given meal, plotted against true emissions. Values closer to the orange line were more precisely estimated. Meals corresponding to orange scatter points were on average overestimated in their emissions, while meals corresponding to green scatter plots 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".

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 €0.21, while if emissions were overestimated, willingness to pay on average decreases by €0.09. Table 4, Spec. (1) shows that this difference in the decrease in willingness to pay is significant at the 5% 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 €0.06 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.

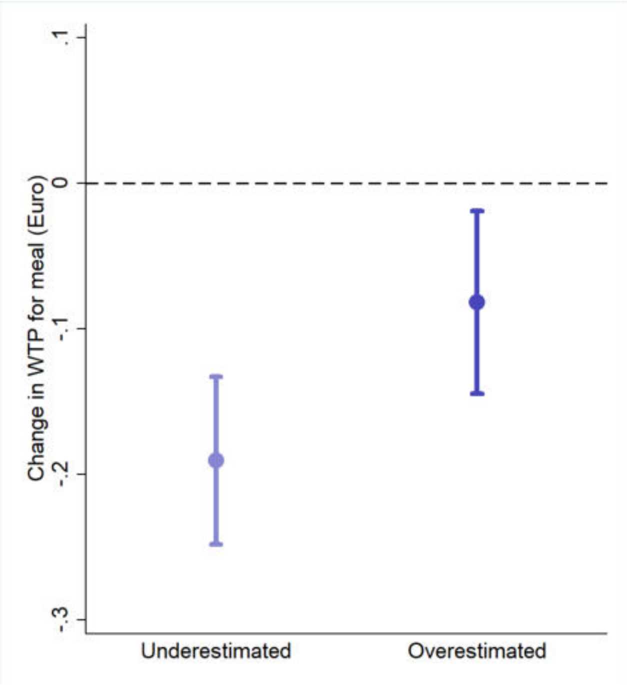

Figure 11: Within-subject change in willingness to pay for a specific meal when shown carbon labels, depending on whether the participant previously over- or underestimated the difference in emissions between the specific meal and the cheese sandwich. For each meal, the 10% most extreme guesses (in terms 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	-0.12***	
	(0.04)	
Underestimation (in kg)		-0.06**
		(0.03)
Control for third round	0.05	0.05
	(0.06)	(0.06)
Constant	-0.09*	-0.16***
	(0.05)	(0.04)
Participants	219	219
Obs. underestimate	528	528
Obs. overestimate	251	251
Observations	779	779

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). 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.

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 €0.11, on average. As shown in Table 5, providing labels on top of increasing attention leads to an additional decrease of €0.10 for high-emission meals.

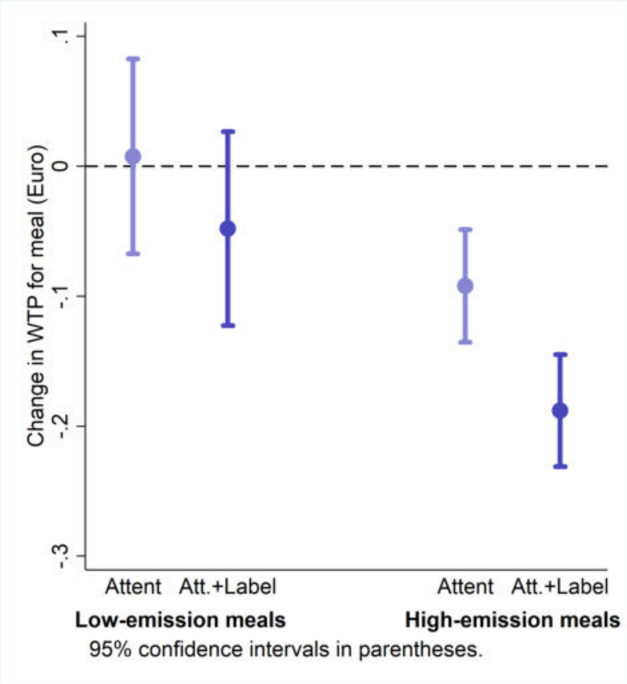

Figure 12: Within-subject change in willingness to pay for a specific meal, comparing participants in the "Attent" and "Attent+Label" condition. Effects are split into meals with low emissions (defined as meals with emissions lower than that of the alternative option, the cheese sandwich) and meals with 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	-0.06
	(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 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 (five 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. [11]

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.[12] 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 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. [13]

The labels were scheduled to be displayed from the 2nd of May until the 17th of June 2022. Unfortunately, display during the first week was irregular, with the labels on some days only displayed in the student restaurant or only online. Further, the student restaurant offered additional extraordinary meals during this period, which were also irregularly labeled. The first week of May is thus completely excluded from the analysis. Further, the 27th of April and the 6th of June are excluded (4% of all observations). On these days one or multiple student restaurants offered a meal with emissions of 8.8 kg, while the emissions of all other meals offered throughout the entire time frame were between 0.2 kg and 2.6 kg.

[11] 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.

[12] 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.

[13] 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.

Seven further days (7th of April, 19th of April, 20th of April, 17 of May, 15th of May, 24th of June, 27th of June) were excluded due to differences in the offer of main components between treated and control student restaurant.

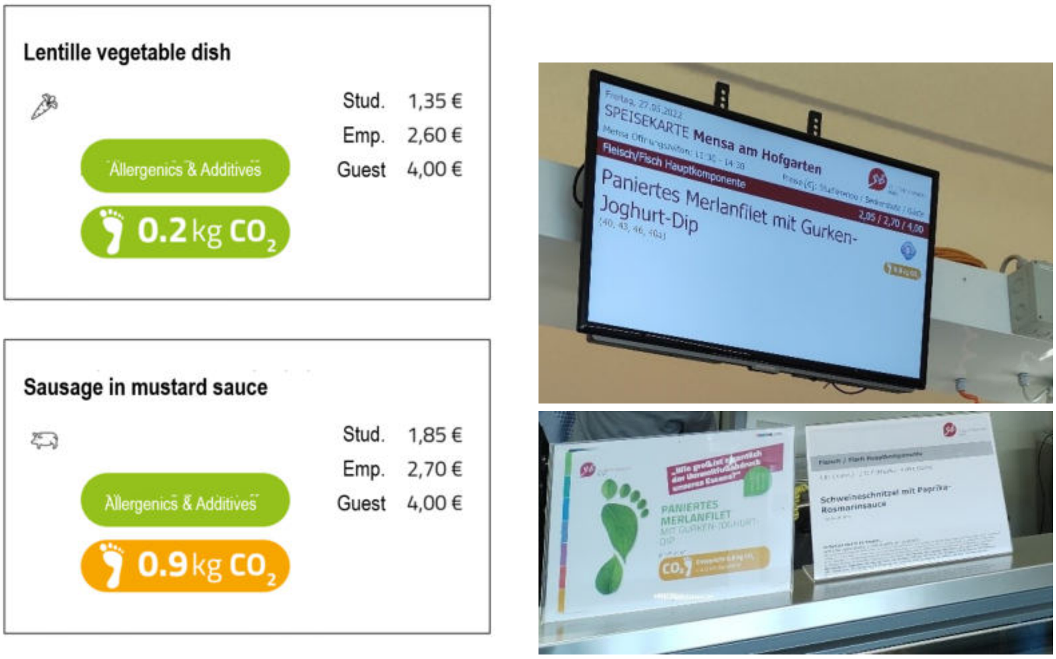

Figure 13: Labels online (left, menu translated from German) and in the student restaurant (right)

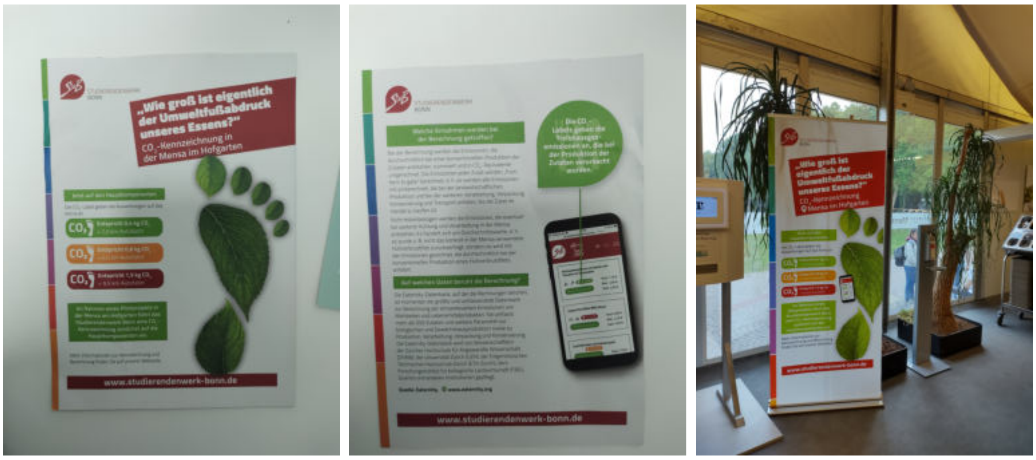

Figure 14: Explanation of the carbon labeling on flyers (left and center) and billboards in the entrance of the student restaurant (right).

Data and Results

The student restaurant provided me with purchase data from April 1st to July 29th, for the three student restaurants. 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.

For my main analysis, I restrict the sample to purchases made with a student restaurant card (2/3rds of observations). The final sample includes 80,061 observations, split between 6,881 guests in the control restaurants and 2,795 guests in the treated restaurant. The main analysis focuses on changes in the consumption of vegetarian meals, with the vegetarian meal being lower or equal in emissions to 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, since the gastronomic daily offer in the control and treatment restaurant is largely similar. I find that the labels increase the likelihood of choosing the vegetarian meal on offer by 3.2 percentage points, or 5.5% of the baseline likelihood. The vegetarian meal is lower in emissions or lower in emissions than the meat meal during the entire study period. Effects persist in the three weeks after removal of the labels. Figure 17 in the Appendix shows an event study of treatment effects, using the main specification. Col. (4) of Table 6 shows that results are also robust to the inclusion of individual fixed effects. For this analysis, I restrict the sample to guests who visited the student restaurants at least ten times during the 13-week period and visited the same student restaurant at least 80% of the time. Visits to the non-home student restaurant are dropped. This is to allow for a clean and well-powered estimation of fixed effects.

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 and 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. [14] The restricted sample contains 20,237 observations. As shown in Table 10 in the Appendix, I estimate that labels reduce average emissions per meal by 30 grams or around 4% of the emissions of a baseline meal.

Figure 18 in the Appendix examines treatment effect heterogeneity. This analysis is merely suggestive, as the number of observations is not sufficiently large to precisely estimate triple interaction effects. However, as we currently lack even suggestive evidence on treatment effect heterogeneity, such an analysis may still be of interest. To examine the effect of individual characteristics, I analyze a subsample of restaurant guests who provided demographic information (1,411 guests providing data in the pre-intervention survey described in Appendix A1). The analyses suggest that treatment effects are higher for female guests, for younger guests (23 and younger) and guests who report that environmental aspects play an important role in their consumption choice. To examine heterogeneity in treatment effects depending on consumption patterns, I estimate a triple interaction effect using the full data set (9,676 guests). The analysis suggests that treatment effects are higher for visitors who visited the student restaurant more often during the label period.[15] Further, I analyze the effect of higher

[14] 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.

[15] This is defined as at least three times - this is the case for half of the student restaurant guests

vegetarian consumption at baseline. [16] Strikingly, previously higher vegetarian consumption seems to have zero impact on treatment effects. There might be two counter-balancing effects at play here: First, guests who previously consume more vegetarian options might be more concerned about the environment and thus more likely to show higher treatment effects. Second, there is a mechanical effect of guests previously consuming more meat having larger leeway in adjusting their consumption towards more vegetarian meals.

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

	(1) Veg. meal	(2) Veg. meal	(3) Veg. meal	(4) Veg. meal
Treatment restaurant x Label period	2.26**	2.19**	3.17***	2.26**
	(1.02)	(1.02)	(1.04)	(1.00)
Treatment restaurant x Post period	9.25***	9.02***	7.22***	3.44***
	(1.22)	(1.22)	(1.29)	(1.21)
Treatment restaurant	7.22***	7.19***	7.41***	
	(1.23)	(1.23)	(1.77)	
Label period	-0.12			
	(0.53)			
Post period	-1.46**			
	(0.62)			
Second veg. main			5.21***	3.42***
			(0.62)	(0.64)
Price difference			4.67**	2.49
			(2.32)	(2.46)
Number of meal options			0.79**	-0.41
			(0.39)	(0.41)
Total daily sales			0.10	-0.28
			(0.17)	(0.25)
Date effects	No	Yes	Yes	Yes
Fixed effects	No	No	No	Yes
Guests control	6,882	6,882	6,882	1,926
Guests treated	2,794	2,794	2,794	645
Observations	80,061	80,061	80,061	47,752

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.

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

[16] I restrict this analysis to guests who visited the student restaurant at least three times during the pre-intervention period (to give an adequate estimate of baseline vegetarian consumption) and guests who did not consume vegetarian on 100% of visits. I then split guests about evenly into two groups, 1) lower vegetarian consumption, consuming vegetarian in a third or less of cases and 2) higher vegetarian consumption.

of the effect labels have on consumer surplus. The frequency distribution of willingness to pay values is visualized in Figure 15. 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 7 in the Appendix).

Table 8 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 9 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.

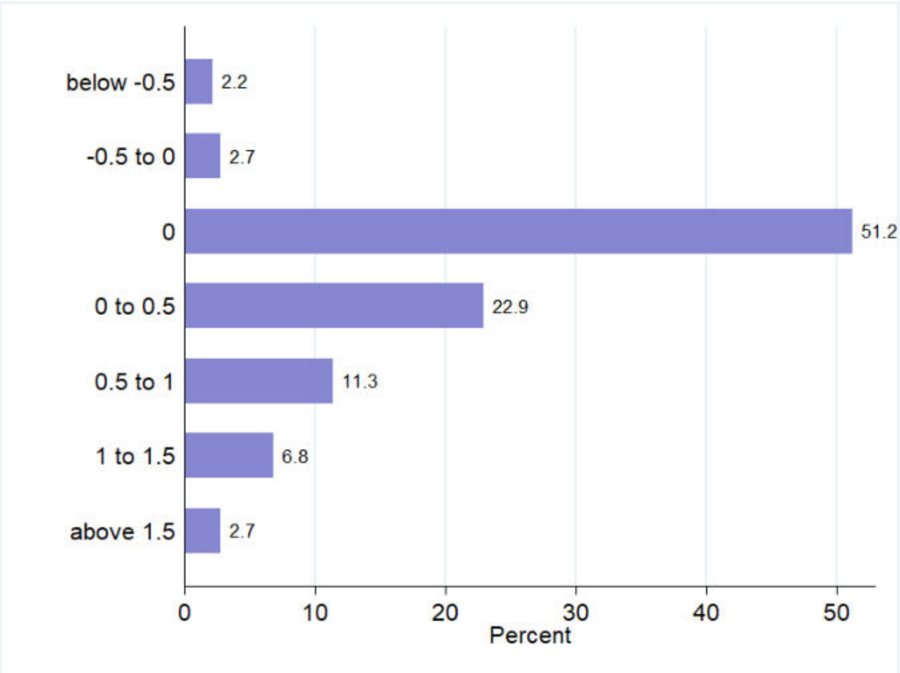

Figure 15: Average willingness to pay to see labels on the final three consumption decisions. Includes data from 733 participants from both lab-in-the-field experiments.

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 16). 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 16: 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

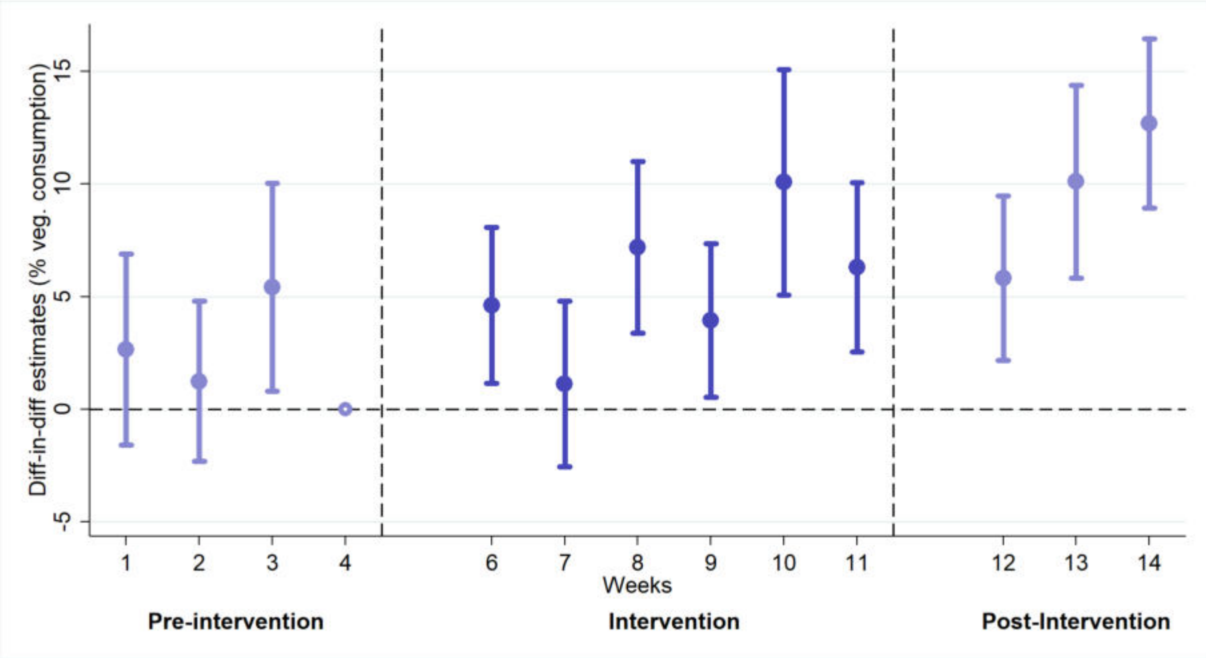

Figure 17: Event study: Difference in difference estimates of likelihood of consuming the vegetarian option (in percentage points), using week 4 of the pre-intervention phase as a baseline. Weeks 1-4 constitute the pre-intervention phase, while weeks 6-11 constitute the intervention phase, and weeks 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.

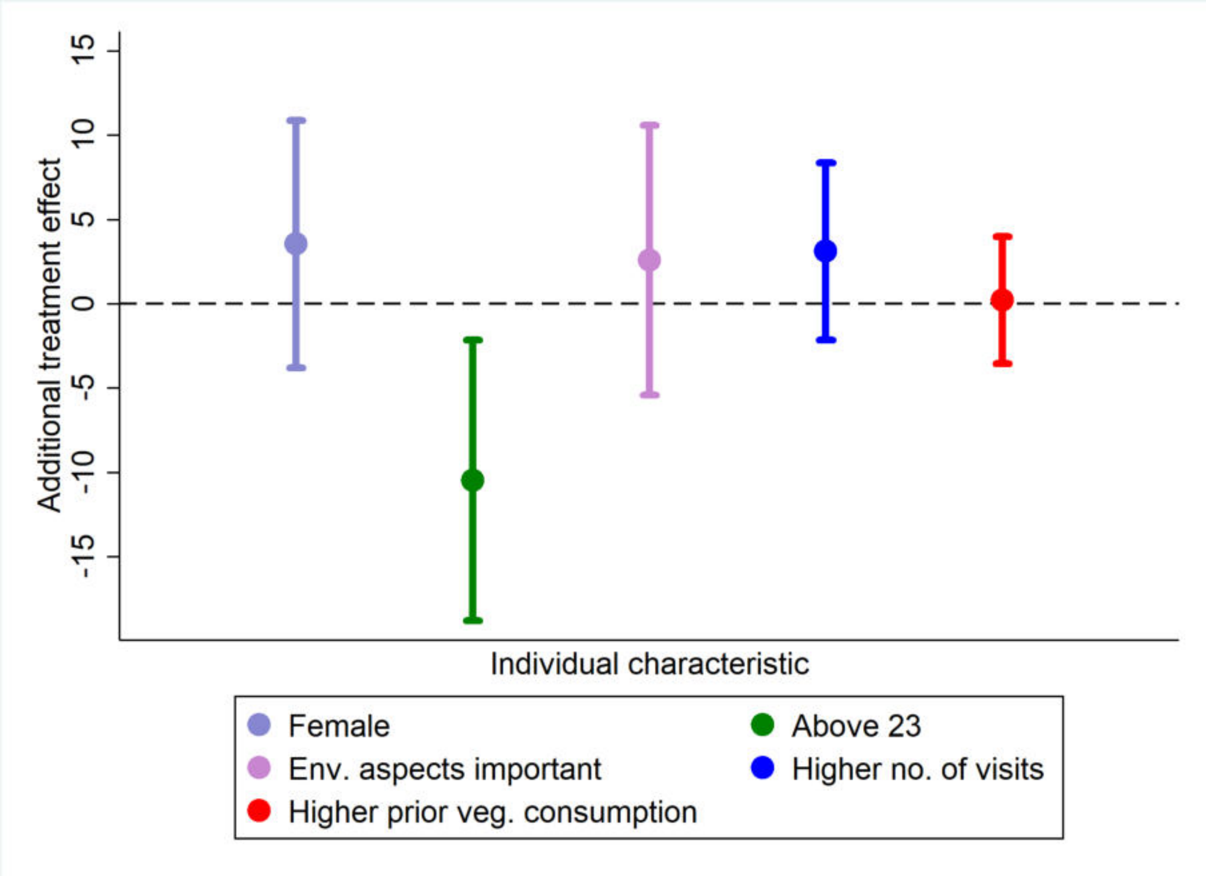

Figure 18: Graph visualizes the outcomes of five seperate regressions, all based on the main specification (see Table 6, spec. (3)). In the first regression, I additionally add the triple interaction "Treatment restaurant x Label period x Female", which is visualized in the plot, as well as the triple interaction "Treatment restaurant x Post period x Female", a control for female, and a control for "Treatment restaurant x Female". I control for Post period x Female by including a full set of date effects interacted with "Female". The other regressions follow the same pattern. The second regression examines being above 23. About a third of the survey sample are above 23. The third regression examines reporting that environmental aspects are important in the food consumption choice. This is the case for around 2/3 of the survey sample. The fourth regression examines the effect of visiting the student restaurant often in the label period. Around half of student restaurant guests visited the student restaurant at least three times. The fifth regression examines the effect of above-average vegetarian consumption in the pre-intervention phase (excluding guests consuming entirely vegetarian). The first three regressions draw on data of student restaurant guests who also provided demographics and attitude information (1,411 guests and 15,405 observations). The final two regressions draw on the full sample (9,676 guests and 80,061 observations).

Table 7: 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 8: 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 9: 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.

Table 10: 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	-12.17	-24.04**	-24.30**	-43.75
	(12.75)	(12.06)	(12.13)	(35.44)
Treatment restaurant	-42.76***	-35.21***	-25.72*	
	(10.72)	(10.16)	(13.73)	
Label period	-1.80			
	(6.58)			
Number of meal options			4.88	10.86
			(3.85)	(11.44)
Total daily sales			-0.21	5.87
			(1.71)	(9.83)
Date effects	No	Yes	Yes	Yes
Fixed effects	No	No	No	Yes
Guests control	5,096	5,096	5,096	160
Guests treated	1,974	1,974	1,974	33
Observations	23,091	23,091	23,091	2,223

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 11: 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***	6.33***	11.81**
	(1.43)	(1.43)	(1.53)	(4.85)
Treatment restaurant	4.57***	4.03***	6.77***	
	(1.38)	(1.37)	(1.99)	
Label period	-0.58			
	(0.73)			
Second veg. main			-1.95	-2.79
			(1.58)	(4.20)
Price difference			-18.43***	2.95
			(6.69)	(16.37)
Number of meal options			0.43	-1.21
			(0.58)	(1.44)
Total daily sales			0.10	-1.62*
			(0.21)	(0.89)
Date effects	No	Yes	Yes	Yes
Fixed effects	No	No	No	Yes
Guests control	5,096	5,096	5,096	160
Guests treated	1,974	1,974	1,974	33
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.