The Effectiveness of Carbon Labels *

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Behavioral interventions to reduce carbon emissions are often evaluated in isolation, without considering the policy trade-off: if not implemented, equivalent reductions would likely require a carbon tax. This paper re-assesses carbon labels, showing that directly measuring intervention effectiveness relative to taxes leads to clearer policy recommendations. Using field experiments, I find that achieving similar reductions as produced by labels requires a €120 ($126) per tonne tax – over 150% higher than current EU-ETS prices. Based on experimental evidence and a structural model, I show that labels generate higher welfare gains and public support than an equivalent tax.

Keywords: carbon footprint, food consumption, welfare, behavioral intervention, field experiment

JEL Classification: D12, C93, Q18

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1 Introduction

The labeling of products with their associated carbon emissions has garnered considerable attention from policymakers, researchers, and private industry. Governments have incorporated carbon labeling into policy initiatives, such as the European Commission's Farm to Fork Strategy, while various companies have voluntarily adopted such labels. This interest is particularly pronounced in the food sector, which contributes an estimated 26% to 34% of global greenhouse gas emissions (Poore and Nemecek, 2018; Crippa et al., 2021). Given that dietary shifts toward lower-emission foods could substantially reduce these emissions,[1] carbon labels are seen as a potential tool for encouraging more sustainable consumer choices. While command-and-control policies and carbon taxes have faced significant political resistance in the food sector, carbon labeling has emerged as a more publicly acceptable alternative.[2]

Prior research suggests that carbon labels can influence consumer behavior, with estimated emission reductions ranging from 1% to 5% in various experimental settings, including student canteens, supermarkets, and online food delivery platforms (Brunner et al., 2018; Bilén, 2022; Lohmann et al., 2022; Ho and Page, 2023). While these findings demonstrate that labels have some effect, they do not establish whether such reductions are meaningful in policy terms. A 4% reduction in emissions may seem modest in absolute terms, but its policy relevance depends on how the political and welfare costs of achieving these reductions compare to those of alternative measures.

This paper leverages two field experiments to directly quantify the effectiveness of carbon labels relative to a carbon tax. Rather than dismissing a 4% reduction as small, I show that achieving the same outcome through taxation would require a carbon price of €120 ($126) per tonne – higher than most existing carbon taxes, including the current EU Emissions Trading System price (€70 per tonne) and Germany's carbon tax on gasoline (€55 per tonne). Having established the tax required to match the emission savings of carbon labels, I compare the two policies – carbon labels and the equivalent carbon tax – in terms of their effect on consumer welfare to assess their relative political feasibility.

To establish the equivalence, I conduct two field experiments, each designed to independently quantify the impact of carbon labels in terms of a carbon tax. These experiments employ different methodologies to ensure that results are not driven by a single approach. The first experiment, a

1. See Poore and Nemecek (2018), Kim et al. (2020), Grummon et al. (2023), and Scarborough et al. (2023).

2. Dechezleprêtre et al. (2022) show opposition to a regulation of the food sector in global survey data. Additionally, Douenne and Fabre (2020) document considerable opposition to meat taxes in France, and Fesenfeld (2023) outlines the political challenges of implementing such taxes in Germany.

framed field study ($N = 289$), directly measures how willingness to pay for meals changes when carbon labels are introduced. This within-subject design allows for a precise estimation of how carbon labels shift demand and how their impact compares to price changes resembling a carbon tax. The second experiment, a natural field study conducted in a university canteen ($N > 10,000$), evaluates the real-world purchasing behavior of thousands of consumers over seven weeks. Using a difference-in-difference design, I estimate the effect of carbon labels and compare it to an equivalent tax by leveraging natural price fluctuations in year-long dataset of canteen purchases. The consistency of results across these two experiments – despite their differing methodologies – adds credibility to my findings.

Beyond establishing the €120 per tonne tax equivalence, I estimate that carbon labels reduce emissions by approximately 4% – a figure consistent with previous studies.[3] Thus, the stronger policy recommendation in this paper stems not from an unusually responsive sample but from a more policy-oriented way of measuring and interpreting effect sizes. Moreover, I find that the impact of the labels remains stable over time and persists for at least three weeks after their removal, providing first causal evidence of the post-intervention effects of carbon labels.

Having established the tax-equivalent benchmark, I next examine how carbon labels and taxation compare in terms of consumer welfare and public support. To inform the analysis, I first quantify the relevance of different behavioral channels driving the effectiveness of carbon labels. A commonly proposed mechanism is that they correct misperceptions about the carbon footprint of different foods (Shewmake et al., 2015; Camilleri et al., 2019; Imai et al., 2022). To test this, I conduct an additional framed field experiment ($N = 444$), which shows that misperception correction accounts for only a small fraction of the labels' effect. Instead, the primary driver appears to be increased salience of carbon emissions at the moment of choice. This finding aligns with broader research showing that consumer attention plays a key role in shaping behavioral responses to informational interventions (Tiefenbeck et al., 2018; Rodemeier and Löschel, 2022), and helps reconcile the existing evidence on the effectiveness of carbon labels with Imai et al. (2022), who find no effect when misperceptions are corrected without altering attention.

To further inform my estimation of consumer welfare effects, I elicit participants' preferences for the presence of carbon labels across all three experiments. Results indicate strong consumer support, with fewer than 10% of participants expressing opposition and an average willingness to pay of 21 cents per meal for label presence.

3. The most comparable studies are Lohmann et al. (2022) and Brunner et al. (2018), which estimate reductions of 4.3% in British student canteens and 3.6% in a Swedish student canteen, respectively.

I introduce a simple discrete choice model of meal selection to formalize how carbon labels impact consumer behavior and welfare. In the model, a consumer chooses, from a set of meals, the meal that maximizes her perceived utility. Her perceived utility is a function of the consumption utility she obtains from the meal, her estimate of the emissions caused by the meal, the guilt she perceives per kg of emissions caused, and how salient carbon emissions are to her when making her choice. Carbon labels correct her misperceptions about the emissions caused by the meal and make emissions salient.

I structurally estimate the model using treatment effects from multiple experimental conditions – including carbon labeling, attention direction, carbon offsetting, participants' subjective emission estimates, and participants' willingness to pay for carbon labels. The model indicates that 79% of the emissions reductions from carbon labeling stem from increased salience, while only 11% result from misperception correction. An extension of the model to consumer welfare yields that the labels are welfare-improving, outperforming an equivalent carbon tax of €120 per tonne in net welfare impact. These welfare gains arise from two sources: first, the impact of carbon labels on purchasing decisions, and second, additional psychological benefits independent of behavior change. These could be driven by a preference for greater information availability or enhanced warm-glow effects among environmentally conscious consumers.

This paper contributes to three key strands of literature. First, this paper demonstrates that evaluating behavioral interventions in terms of their policy-relevant tax equivalents provides a more policy-relevant measure for evaluating intervention effectiveness. It provides a framework and experimental methods that can be applied to other consumption domains and interventions, and is thus relevant to the broad literature evaluating behavioral interventions aimed at reducing carbon emissions, in areas such as resource consumption (e.g., Allcott and Mullainathan, 2010; Allcott, 2011; Ferraro and Price, 2013; Brent, Cook, and Olsen, 2015; Tiefenbeck et al., 2018; Tiefenbeck et al., 2019; Goette et al., 2021; Fang et al., 2023; Byrne et al., 2024), purchasing behavior (e.g., d'Adda, Gao, and Tavoni, 2022; Rodemeier and Löschel, 2022), and food consumption (e.g., Kurz, 2018; Garnett et al., 2020; Meier et al., 2022; Jalil, Tasoff, and Bustamante, 2023; Lohmann et al., 2024). My approach goes beyond simply comparing effect sizes to price elasticities, which are often estimated from observational data and subject to endogeneity concerns. More importantly, price elasticities vary across contexts and consumer groups (Lusk and Tonsor, 2016), making it problematic to extrapolate from general population estimates to specific experimental settings. In contrast, my study estimates how consumers from the same group (students) respond to both price changes and carbon labels in the same consumption setting (a student canteen). This allows for a direct comparison between the two interventions within a controlled context, rather than relying on elasticities derived from different populations or market settings, such as grocery shopping.

More specifically, my findings inform our understanding of the effectiveness of carbon labels on food consumption products (Brunner et al., 2018; Bilén, 2022; Imai et al., 2022; Lohmann et al., 2022; Ho and Page, 2023). Beyond quantifying their effect sizes relative to a carbon tax, this paper provides the first causal estimates of their post-intervention effects, identifies the behavioral mechanisms driving their impact, and evaluates their efect on consumer welfare.

Second, this paper contributes to the literature on attentional biases in consumption decisions (e.g. Chetty, Looney, and Kroft, 2009; Busse et al., 2013; Taubinsky and Rees-Jones, 2018), in particular in environmentally relevant choices (Allcott and Taubinsky, 2015; Tiefenbeck et al., 2018; Rodemeier and Löschel, 2022). I provide the first causal evidence of attentional biases in food consumption and quantify the relative importance of attention versus misperception correction in determining the effectiveness of carbon labeling interventions.

Finally, this study informs the broader debate on the welfare implications of behavioral interventions (Allcott and Kessler, 2019; Allcott et al., 2022; Andor et al., 2023; List et al., 2023). By experimentally eliciting consumer preferences for carbon labels and incorporating them into a structural framework, this paper provides novel evidence on their welfare effects and directly compares them to carbon taxation.

The remainder of this paper proceeds as follows. Section 2 describes Experiment 1, which quantifies the effectiveness of carbon labels using a framed field experiment. Section 3 presents Experiment 2, a natural field experiment in a university canteen. Section 4 examines the behavioral channels driving the observed effects, drawing on data from Experiment 3. Section 5 shows reduced-form evidence from all three experiments concerning consumers' preferences towards the presence of the label. Section 6 introduces a theoretical framework, which is structurally estimated using data from Experiment 3. Finally, section 7 concludes.

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

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

2.1 Experimental design

Overview. To cleanly measure the impact of carbon labels and compare their effectiveness to a carbon tax, willingness to pay (WTP) for the same meal by the same individual should best be observed both with and without carbon labels. Experiment 1 is designed accordingly. Below, I summarize key design choices and provide details in the following paragraphs.

(1) Participants' lunch choices are moved to an online survey, completed just before lunchtime on the experiment day. Shortly after, they go to campus to receive their experiment payment and the meal corresponding to their choices.

(2) In the survey, participants indicate their WTP for different meals, totaling to 15 meal purchase decisions. One decision is implemented at payout.

(3) Participants are assigned to either the Label or Control condition. In Label, participants first state WTP for four meals without carbon labels and then for the same meals with labels. In Control, they state WTP twice without labels. The BDM mechanism incentivizes truthful responses.

(4) WTP is elicited relative to an alternative lunch: In each of the 15 decisions, participants state their WTP for a given meal relative to a cheese sandwich, reflecting the real-world fact that not eating one meal means eating something else.

(5) Carbon labels display greenhouse gas emissions (kg), an ordinal ranking via a traffic-light system, and the distance a car would need to be driven (in km) to produce equivalent CO_2 emissions (see Figure 4). I designed the labels with Bonn's student canteens to ensure field implementation. Combining ordinal and quantitative rankings has been identified as effective in previous literature (Taufique et al., 2022; Potter et al., 2021).

(6) WTP to see or avoid carbon labels is also elicited: Before the final three purchase decisions (three new meals), participants choose whether they want to see carbon labels and indicate their WTP to enforce their choice. This elicitation is incentivized with a BDM mechanism.[4] These results are discussed in Section 5.

Experiment timeline. The online survey timeline is shown in Figure 1. First, participants receive an explanation of the WTP elicitation and answer comprehension questions.[5] Next, they indicate their baseline WTP for four canteen meals, incentivized by a BDM mechanism.[6] To create buffer time

4. See online Appendix C.3 for details.

5. Participants must answer correctly to proceed. Those requiring more than five attempts are excluded, as pre-registered.

6. See online Appendix C.3 for details.

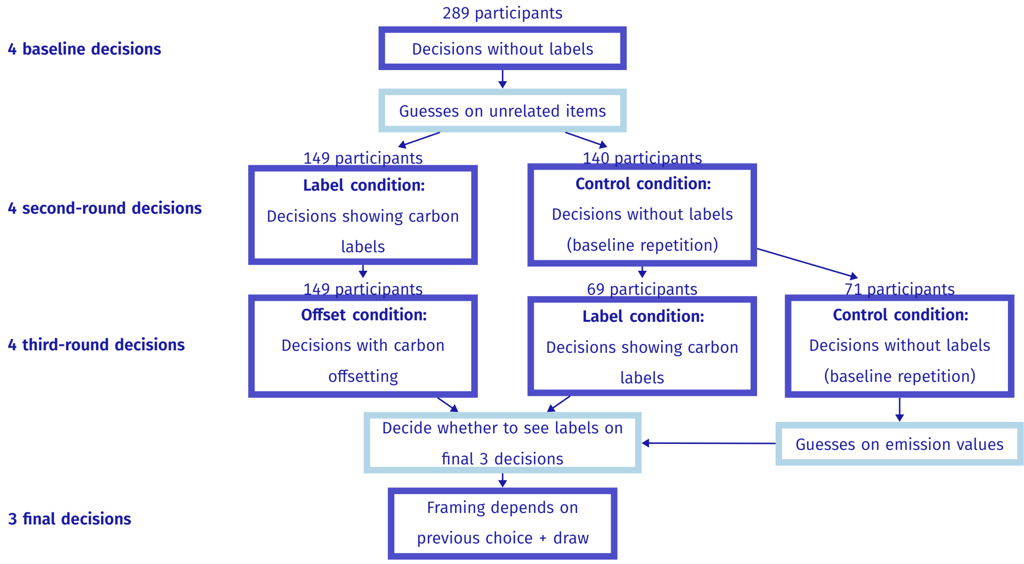


Figure 1. Experiment 1 schedule and treatment groups

before the second WTP elicitation, participants answer incentivized guessing questions on unrelated topics (e.g., the length of a popular running route in Bonn). In the second WTP elicitation, the framing of the decisions depends on the randomly assigned treatment:

- CONTROL: Decisions are identical to the baseline elicitation.
- LABEL: Participants see carbon labels.

For additional insights, WTP is elicited a third time,[7] with altered treatment conditions:

- Participants previously in the LABEL condition are in the OFFSET condition: Participants are informed that meal emissions (meal or sandwich) will be offset. This serves as a robustness check for Experiment 3 and is detailed in online Appendix* C.4, with reduced-form results in Table A.7.
- Half of the participants previously in the CONTROL condition receive the LABEL condition, and half of the participants previously in the CONTROL condition repeat the CONTROL condition. Afterward, this group estimates emission values.[8]

Each round includes four meal purchasing decisions (12 total). Additionally, three final decisions involve previously unseen meals. Before making these, participants indicate whether they want to see carbon labels and state their WTP to enforce their choice, incentivized via a BDM mechanism. Finally, participants answer questions on environmental attitudes and psychology, and their calorie estimates for each meal are collected for robustness checks.

7. Analyses control for third-round data. Main results replicate using only the first two rounds, see Table A.22.
8. Used in Figure 11. As these questions occur after the third WTP elicitation, they do not affect results in this section.

Details on the meal purchasing decisions. Participants who indicate they are vegetarian see only vegetarian meals.[9] In each of the 15 decisions, participants first choose between a specific meal and a cheese sandwich. An example decision is shown in Figure 2. The left option changes across decisions to cycle through four meals, while the right option (cheese sandwich) remains constant.[10]

After selecting a preferred option, a second window appears where participants indicate how much of their experiment payment they would forego to secure their preference (Figure 3). If they prefer the specific meal, they indicate WTP to receive it instead of the cheese sandwich. If they prefer the sandwich, they indicate WTP to ensure they receive it instead. Responses are made on a slider in five-cent intervals, between €0.00 and €3.00.[11] This procedure captures WTP for each meal relative to the cheese sandwich. If a participant prefers the specific meal in Step 1, their second-step amount represents WTP to receive it. If they prefer the sandwich, the amount represents WTP to avoid the meal (i.e., negative WTP). Participants are incentivized to report their true WTP using a BDM mechanism, as detailed in Appendix C.3.

In the four baseline decisions, participants see only the meal name and main ingredient, without carbon labels (Figure 2). This mirrors how meals are typically displayed on the student canteen website. The four second-round and four third-round decisions resemble the baseline, except for framing differences in some treatments. In the Label condition, emission values are added to meal options (Figure 4).[12] In the Control condition, framing remains unchanged. In the Offset condition, participants are informed that meal emissions will be offset through a donation to the nonprofit carbon offsetting service Atmosfair. The Offset condition is not further discussed here, but details are in online Appendix C.4, with results in Table A.7.

Participants and set-up. 304 participants from the BonnEconLab, the University of Bonn's behavioral experimental lab, took part in one of eight experimental sessions between October 26 and

9. Meals are detailed in online Appendix C.2. Participants with stricter dietary requirements (vegan, gluten-intolerant, lactose-intolerant, or halal) are excluded.

10. To control for left-right effects, positioning is reversed in half of the sessions, and meal order is randomized.

11. €3.00 is the maximum price students pay for any meal in the canteen. WTP values at the interval boundaries (±€3.00) occurred in less than 4% of observations. Figure A.1 in the online Appendix shows the distribution of baseline WTP values.

12. Meal emissions were calculated using the application Eaternity Institute (2020). The Bonn student canteen provided meal recipes for these calculations.

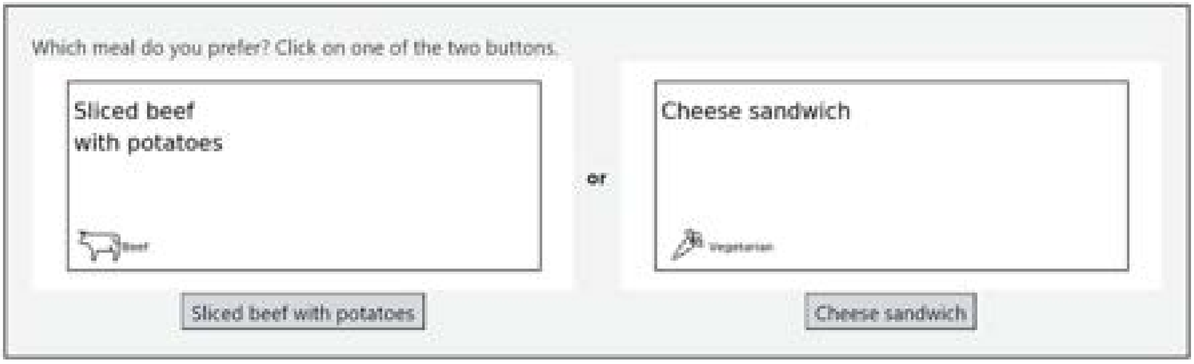


Figure 2. Meal purchase decision example step 1

Notes: Step 1 of the purchasing decision. Depending on participants' choice, Step 2 (Figure 3) asks for their WTP to receive or avoid the warm meal.

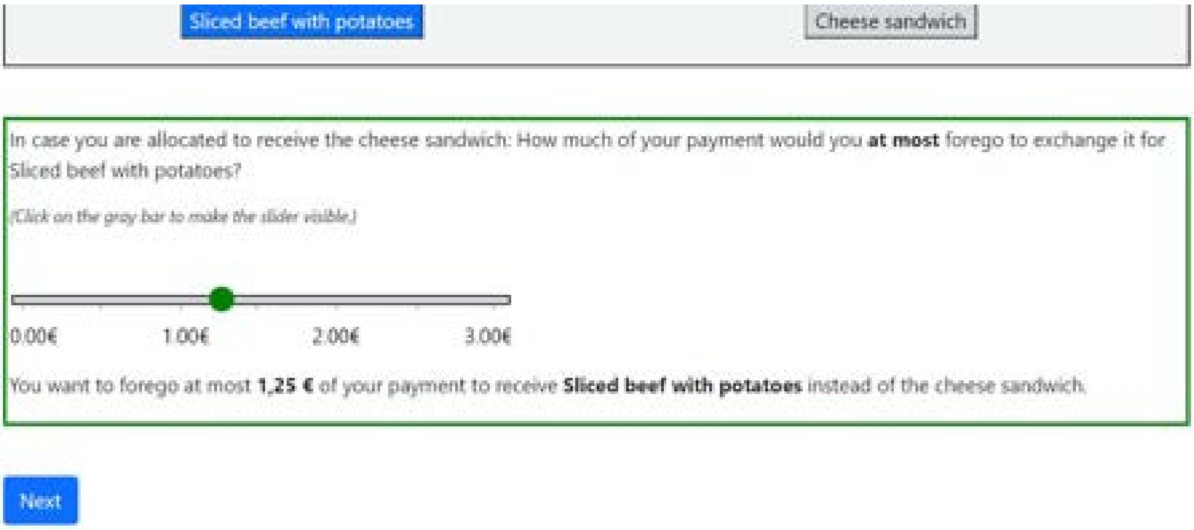


Figure 3. Meal purchase decision example step 2

Notes: Step 2 of the purchasing decision. If participants prefer the warm meal in Step 1, Step 2 is as shown above. If they prefer the cheese sandwich, Step 2 asks how much they would forego to receive the sandwich instead.

November 5, 2021. I pre-registered the experiment design and main outcomes shown in this section (Schulze Tilling, 2021b).[13]

Participants are informed that the experiment is conducted online but must visit campus directly afterward to collect their cash payment and lunch. They receive no further details on the experiment's purpose. The experiment is conducted using oTree software (Chen, Schonger, and Wickens (2016)).

Meals are provided by the student canteen. While all experiment meals are regularly offered by the canteen, they are not available on experiment days, meaning the canteen prepares them

13. See Table A.7 in the online Appendix for all pre-registered main results.

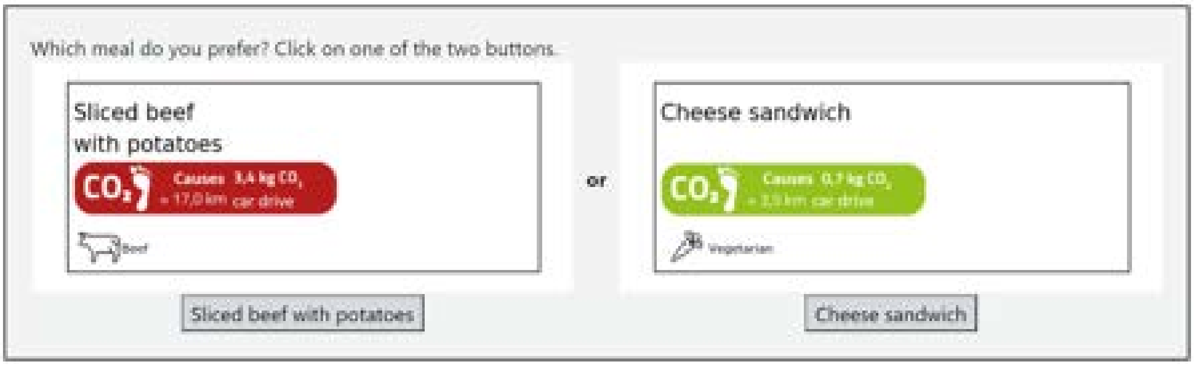


Figure 4. Meal purchase decision example: Decisions with labels

exclusively for participants. Participants receive a warm, ready-to-eat meal based on their online experiment choices and can consume it on-site.

2.2 Estimation strategy

Participants' WTP for a meal is influenced by various factors (e.g., taste, hunger, mood). To isolate the effect of labels, I analyze the **change** in WTP for a meal as the outcome variable: Instead of directly comparing WTP in the LABEL and CONTROL conditions, I subtract an individual's baseline WTP from their subsequent WTP for the same meal and examine the difference. This change reflects the impact of seeing carbon labels (LABEL condition) or simply being asked WTP a second time (CONTROL condition). The outcome variable can be interpreted as an individual- and meal-specific within-subject treatment effect, which I compare across treatment groups.

An alternative approach would use WTP as the dependent variable, including a fixed effect for each individual-specific meal choice. This yields similar results (Appendix A.9).

My main specification is:

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

where Δ_{ijm} is the difference between WTP of individual i in round j for meal m and their baseline WTP for meal m, always expressed relative to the cheese sandwich.

$High_m$ indicates whether the meal has higher emissions than the sandwich,[14] while Low_m indicates whether it has lower emissions. These variables capture any effect of repeated WTP elicitation.

14. For non-vegetarians, three of the four meals had higher emissions; for vegetarians, two of the four meals. See Appendix C for details.

$(Label_{ij} \times High_m)$ estimates the causal effect of labels on WTP for high-emission meals, while $(Label_{ij} \times Low_m)$ does so for low-emission meals. $ThirdRound_j$ controls for whether round j is the third decision round,[15] ensuring comparability across conditions.

2.3 Data and results

Of 304 participants, I exclude the 3% fastest respondents and those failing the comprehension check after five attempts, as pre-registered.[16] One incomplete response is also dropped, leaving 287 participants who were computer-randomized into treatments (randomization check in Appendix A.1). Participants are on average 24 years old; 67% are female, 80% are students, and 25% are vegetarians. The sample is roughly representative of regular student canteen guests, as discussed in online Appendix A.2, and results hold when restricting the sample to only students or only non-vegetarians (online Appendix A.7). Baseline WTP distributions are shown in online Appendix A.3: 22% of WTP values are 0 (indicating indifference between the meal and sandwich), 17% are negative (preference for the sandwich), and the rest are positive, with some bunching around €1. Less than 4% of WTP values are at the boundaries of the -€3 to €3 interval.

Table 1, Specification (1), shows the OLS estimation results for equation 1. For meals with lower emissions than the sandwich, WTP increases by €0.14 due to the labels. For meals with higher emissions, WTP decreases by €0.31. Changes in WTP in the Control condition are not significant and move in opposite directions, suggesting that repeated WTP elicitation does not significantly affect WTP. Figure 5 illustrates these effects by showing average changes in WTP for the Control and Label groups, for low- and high-emission meals.

Specification (2) in Table 1 regresses the change in WTP on the difference in emissions between the meal and sandwich (Emi_m):

$$\Delta WTP_{ijm} = \beta_1 Emi_m + \delta_1 (Label_{ij} \times Emi_m) + ThirdRound_j + \varepsilon_{ijm} \tag{2}$$

This specification estimates that WTP decreases by €0.12 per additional kg of emissions caused by the meal relative to the sandwich. This implies that a €120 per tonne carbon tax[17] produces

15. Since some participants experienced the Label condition in round two and others in round three, this controls for any differences. An alternative approach excludes third-round decisions entirely, yielding similar results (Table A.22 in the online Appendix).

16. See Schulze Tilling (2021a). Dohmen and Jagelka (2023) find that fast respondents are more likely to give random answers.

17. I refer to a tax added to a product's price without salient display, similar to value-added taxes or the EU-ETS trading scheme.

effects comparable to carbon labels. Figure 6 visualizes this comparison. The left column shows how carbon labels shift demand curves: for low-emission meals, demand slightly increases, while for high-emission meals, demand decreases. The right column shows a similar shift caused by a carbon tax of €120 per tonne.[18] This comparison conceptualizes the impact of carbon labels as a demand curve shift, analogous to a carbon tax.[19] Using experiment data, I simulate participants' choices in the student canteen with and without labels and estimate that labels reduce emissions by 4.8%. Details are in online Appendix A.4.

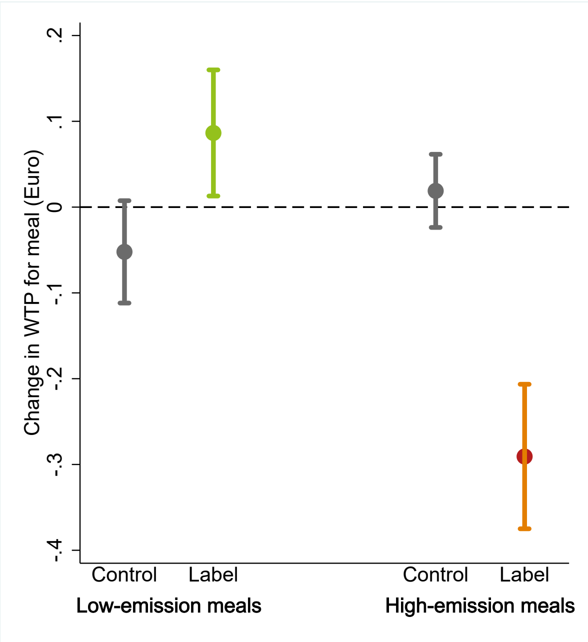


Figure 5. Within-subject change in WTP for meals by treatment condition.

Notes: Bars indicate 95% confidence intervals.

Table 1. Within-subject change in WTP for meals

	Change in WTP compared to baseline	
	(1)	(2)
High emission meal × Shown label	-0.31^{***}	
	(0.05)	
Low emission meal × Shown label	0.14^{***}	
	(0.04)	
High emission meal	0.01	
	(0.02)	
Low emission meal	-0.06^{*}	
	(0.03)	
Emissions(kg) × Shown label		-0.12^{***}
		(0.03)
Emissions(kg)		0.02
		(0.01)
Shown label		-0.08^{**}
		(0.03)
Participants control	139	139
Participants treated	217	217
Observations	1,704	1,704

Standard errors in parentheses

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

Notes: Standard errors are clustered at the individual level. Both columns additionally include a control for third-round decisions, and Col. (2) includes a constant term.

18. I construct these demand curves by deducting a €120 per tonne carbon tax from baseline WTP, with the tax calculated relative to the sandwich (negative for low-emission items).

19. See online Appendix A.10 for a detailed explanation of this comparison.

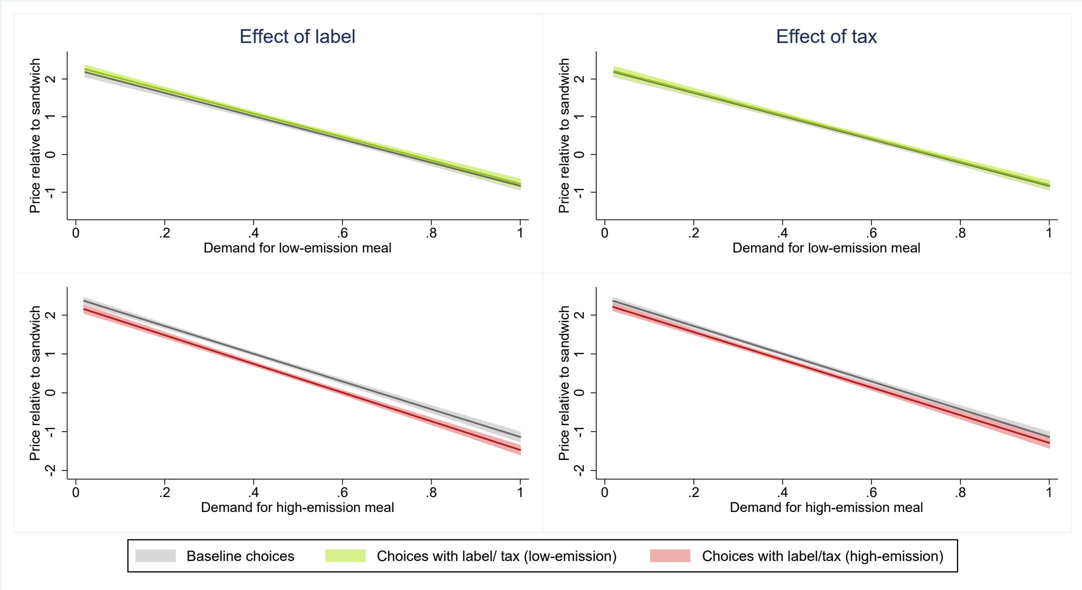


Figure 6. Demand curve shifts with labels vs. a carbon tax

Notes: Demand curves for low-emission meals (top, N=265) and high-emission meals (bottom, N=603) estimated using data from participants in the LABEL condition. Gray lines show baseline WTP, while green and red lines show WTP with carbon labels (left) and net WTP after deducting a carbon tax of €120 per ton (right). Shaded areas represent 95% confidence intervals using robust standard errors.

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

While Experiment 1 quantifies the effect of carbon labels relative to a carbon tax in a one-shot consumption setting, Experiment 2 provides an independent estimate based on longer-term consumption behavior in Bonn's student canteens. Subsection 3.1 examines the impact of carbon labels, while Subsection 3.2 compares the effects of price changes and labels.

3.1 The effect of labels

3.1.1 Experimental design. To identify the causal effect of carbon labels in the field, I leverage Bonn's multiple student canteens, which centralize meal planning. This means that on any given day, the same meals are offered across all canteens. Below, I summarize key details; further descriptions of the canteen setting are provided in online Appendix D. The experiment design and main outcomes are pre-registered.[20]

20. See AsPredicted#95108. I pre-registered outcomes including meat/vegetarian consumption, green-labeled meal consumption, greenhouse gas emissions, and canteen visits during and after the intervention period. Full pre-registered analyses are in online Appendix B.2.

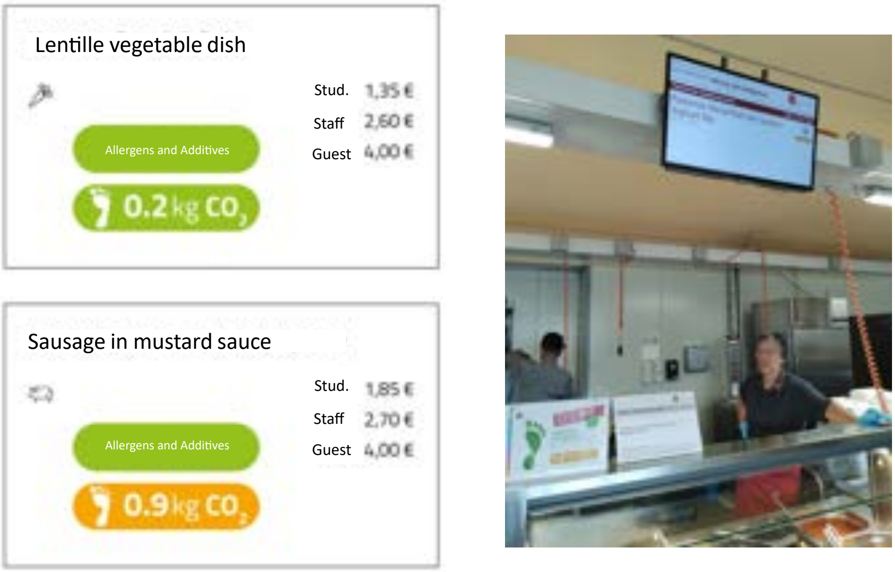


Figure 7. Labels in the canteen

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

(1) The experiment uses a difference-in-difference design: Purchasing behavior in all three student canteens is observed during three phases: (i) pre-intervention (4 weeks, no labels), (ii) intervention (7 weeks, labels installed in the treatment canteen), and (iii) post-intervention (3 weeks, no labels).

(2) Carbon labels display a quantitative and ordinal ranking, similar to Experiment 1. In the treatment canteen, labels are added to the online menu, digital billboards, and paper signs on meal counters. Examples are shown in Figure 7.[21]

(3) Labels are implemented only for the two main meal components (vegetarian and meat-based), not for sides or desserts, for ease of implementation and interpretability (details in online Appendix D). Main meal components cause, on average, 70% of lunchtime emissions. The vegetarian component consistently has lower emissions than the meat-based option.

(4) A pre-intervention (N>1,700) and post-intervention survey (N>900) accompany the field experiment. These surveys capture demographic characteristics and participants' opinions and are linkable to the purchasing data. Further details are in online Appendix D.8.

3.1.2 Estimation strategy. To estimate the causal effect of carbon labels in the student canteen, I use a difference-in-difference (DiD) approach with the choice of an emission-heavy main meal

21. Emissions are calculated based on canteen recipes and the Eaternity Institute (2020) database, as in Experiment 1.

component as the outcome variable. This controls for baseline differences in canteen consumption and time trends common to all canteens. The basic DiD specification is:

$$
\begin{aligned}
Meat_{it} = \alpha + \beta_1 LabelPeriod_t + \beta_2 PostPeriod_t + \gamma Treat_{it} + \\
+ \delta_1(Treat_{it} \times LabelPeriod_t) + \delta_2(Treat_{it} \times PostPeriod_t) + \epsilon_{it} \quad (3)
\end{aligned}
$$

Here, $Meat_{it}$ is a binary variable equal to 1 if individual i purchases the higher-emission, meat-based main meal component on day t, and 0 if they purchase the lower-emission vegetarian option. $LabelPeriod_t$ indicates purchases during the seven-week intervention, while $PostPeriod_t$ covers the three weeks following the intervention, before the summer break. $Treat_{it}$ indicates purchases in the treatment canteen. $(Treat_{it} \times LabelPeriod_t)$ is the variable of interest, identifying the DiD estimate of any change in purchasing behavior during the labeling period in the treatment canteen compared to control canteens. $(Treat_{it} \times PostPeriod_t)$ captures post-intervention effects.

Depending on the specification, I add granular time controls and controls for variations in canteen offerings. My preferred specification estimates intention-to-treat (ITT) effects at the guest level, using guest fixed effects to account for changes in canteen visiting behavior.[22]

3.1.3 Data and results. I include purchase data from April 4, 2022 (start of the semester) to July 8, 2022 (end of the semester). I drop data from seven days when the treatment and control canteen did not offer the same main meal components, as well as consumption by Ukrainian refugees, who received free meals from week 9 onward. For my main analysis, I exclude data from the first week of the label period (week 5) due to a concurrent "Healthy Campus" week, which could confound the increased vegetarian consumption observed during this week (Figure 8).[23] The results are robust to alternative exclusion criteria (online Appendix D.5). The final sample includes 121,371 observations from nearly 10,000 guests. For each purchase, I observe the meal, price, location, date, time, and whether the guest was a student (81%) or employee (17%). Additionally, 69% of purchases are linked to personalized payment cards, enabling guest tracking over time.

Using purchases linked to personalized payment cards, I construct an ITT sample restricted to guests who (i) visited the canteen regularly pre-intervention (at least five visits within four weeks)

22. While rare due to the 1.7 km (1.1 miles) distance between canteens, intervention-motivated switching is examined in online Appendix D. Using pre-intervention data, I identify a "home" canteen for each guest, and examine switching to "non-home" canteens. I find no evidence of increased switching due to the labels. Nevertheless, spec. (5) in Table 2 controls for such behavior and changes in guests' overall likelihood of visiting the canteens by assigning treatment as intent-to-treat and including guest fixed effects.

23. The "Healthy Campus" week affected both treatment and control canteens similarly and should not have caused differential effects. Nevertheless, I exclude it from the main analysis to be conservative.

and (ii) primarily visited the same canteen pre-intervention (at least 80% of visits to the same canteen). This allows me to classify guests as "intent to treat" based soley on their pre-intervention consumption behavior.[24]

Table 2 presents regression results. Column (1) estimates the basic specification from equation 3. Column (2) replaces the "Label period" and "Post period" indicators with weekly and day-of-the-week controls for finer time trend adjustments (e.g., semester and seasonal effects). Column (3) adds controls for whether a second vegetarian/meat main meal component was offered. Columns (4) and (5) analyze the ITT sample, estimating ITT effects and including guest fixed effects to account for changes in canteen visiting frequency or behavior.[25] Column (5) further includes date-specific time controls, which not only capture common time trends with higher precision but also control for the attractiveness of daily changing meal offerings, since the canteens centralize meal planning.

The final column estimates that the labels reduced meat consumption by three percentage points during the labeling period, corresponding to 6% of baseline meat consumption in the ITT treatment group. Post-intervention, meat consumption decreased by four percentage points, or 8% of baseline consumption. Figure 8 illustrates fairly similar pre-treatment trends, a relatively stable treatment effect during the intervention period, and sustained effects in the three weeks after the labels were removed. However, these effects do not persist into the subsequent semester (Figure B.2 in the online Appendix).[26] Post-intervention effects thus appear short-lived, aligning with the attention-habit model of Byrne et al. (2024), where the intervention raises awareness of carbon emissions and temporarily shifts consumption habits.

In the pre-intervention phase, the average emissions of a meat meal in the treated canteen were 2.2 kg, while those of a vegetarian meal were 0.4 kg. A back-of-the-envelope calculation (1.8 x 0.03) suggests that, without any changes in the meals offered, the intervention would have reduced greenhouse gas emissions by 54 grams per meal or 4.5% of baseline emissions (1.2 kg). However, meal offerings in the canteens changed between the pre-intervention and intervention periods, causing mechanical changes in average meal emissions. As detailed in online Appendix B.3, a simple

24. This classification is robust to alternative thresholds (online Appendix D.6).

25. For each individual, the "ITT guest" indicator is fixed based on pre-intervention behavior. Guests mainly visiting the treatment canteen pre-intervention are assigned an ITT value of 1, while those primarily visiting control canteens are assigned a value of 0. This ensures estimates are unaffected by guests potentially changing their canteen visits during or after the intervention. Control variables are also specific to the intent-to-treat canteen.

26. I cannot link individuals from the main data set to the subsequent semester's data. Thus, I cannot produce an ITT event plot or restrict the subsequent data to prior guests. The upward-sloping pattern in Figure B.2 suggests that null effects are not entirely attributable to new, untreated guests, but that effects likely did not persist among treated guests from May 2022.

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

	Likelihood of consuming meat				
	Base	Week FE	+Controls	+Guest FE (ITT)	+Date FE (ITT)
Treat × Label period	-0.02***	-0.02***	-0.02***	-0.03***	-0.03**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Treat × Post period	-0.06***	-0.06***	-0.06***	-0.06***	-0.04***
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)
Treatment restaurant	-0.10***	-0.10***	-0.07***		
	(0.01)	(0.01)	(0.01)		
Label period	0.01**				
	(0.00)				
Post period	-0.00				
	(0.00)				
Control for second veg. offered			-0.02***	-0.01	-0.03***
			(0.00)	(0.01)	(0.01)
Control for second meat offered			0.02***	0.02***	-0.00
			(0.00)	(0.01)	(0.01)
Constant	0.51***	0.49***	0.47***	0.49***	0.46***
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)
Week fixed effects	No	Yes	Yes	Yes	Yes
Guest fixed effects	No	No	No	Yes	Yes
Guests control	6,957	6,957	6,957	1,021	1,021
Guests treated	2,792	2,792	2,792	342	342
Observations	121,371	121,371	121,371	27,640	27,640

Standard errors in parentheses

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

Notes: Dependent variable: 0/1 indicator for consumption of the meat option. Spec. (1)–(3) assign treatment and controls at the canteen level, while Spec. (4) and (5) assign treatment and controls at the individual level (ITT). The standard errors of Col.(1)-(3) are robust. The standard errors of Col.(4)-(5) are clustered at the guest level.

difference-in-difference analysis using greenhouse gas emissions as the outcome would incorrectly attribute these changes to the carbon labels. To account for this, I control for the emissions of meals on offer in the analysis and estimate a treatment effect of 50 grams per meal in the ITT sample (Table B.4 in the online Appendix). These findings align with an additional check that avoids con-

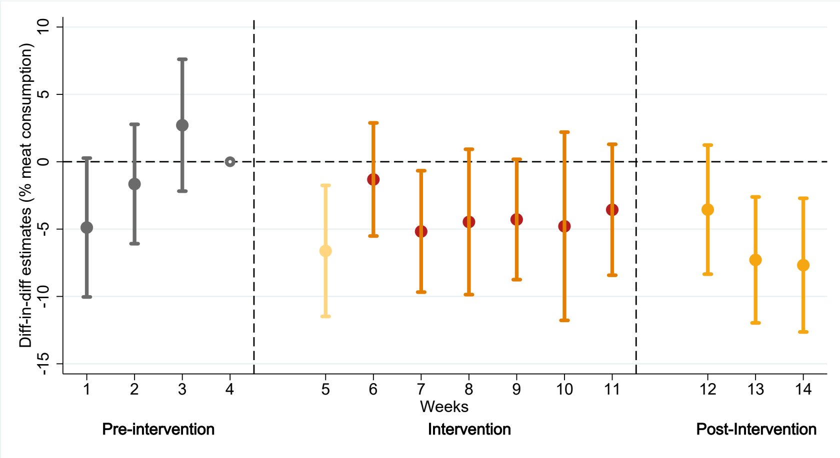


Figure 8. Event study: Difference-in-difference estimates

Notes: Difference-in-difference estimates of the likelihood of consuming the meat option (percentage points), using week 4 of the pre-intervention phase as the baseline. Weeks 1–4 represent the pre-intervention phase, weeks 5–11 the intervention phase, and weeks 12–14 the post-intervention phase. The regression specification follows specification (4) in Table 2 but estimates weekly effects and controls for weekly time trends. The corresponding regression table is in Table B.1. Week 5 is excluded from the main estimation in Table 2, as effects cannot be clearly attributed to carbon labels (see Appendix D for details). Standard errors are clustered at the guest level, and bars represent 95% confidence intervals.

trolling for meal changes by restricting the sample period to when pre-intervention and intervention offerings were identical (online Appendix B.3).

3.2 The effect of a carbon tax

3.2.1 Setting. To compare the effect of carbon labels to a carbon tax, I examine how canteen guests respond to price variations. In the canteens, the price difference between the main meat and vegetarian options is the relevant price figure driving the composition of meat and vegetarian purchases.[27] I focus on the effect of variations in this price difference, which I observe across a year of student canteen data due to two factors:

(1) The canteens vary the specific vegetarian and meat main meal components offered daily, resulting in price differences. Meat meal prices range from €1.85 to €2.5, while vegetarian options range from €1.35 to €2.4. Since meal pairings differ across days, the price difference between vegetarian and meat options varies accordingly.

27. Since canteen meals are cheaper than other lunch options, price-conscious students are likely to dine in the canteen. They then choose between meat and vegetarian meals based on their attractiveness and the price difference. See online Appendix D for further details on the canteens.

(2) A price increase in October 2022 raised both the general price level and the average price difference between meat and vegetarian options. From April to June 2022, this difference averaged €0.33 (around 20% of the price of a vegetarian meal). From October to December 2022, it increased to €0.50 (around 25% of the price of a vegetarian meal) and remained at this higher level.

Using these price variations, I approximate the effect of a carbon tax in the student canteens. During the period for which I have emissions data (April to July 2022), the meat option consistently had higher emissions than the vegetarian option offered on the same day. Average emissions for the vegetarian option were 0.4 kg per meal, compared to 1.6 kg per meal for the meat option.

3.2.2 Estimation strategy. I regress guests' decision to purchase the meat main meal component on the price difference relative to the vegetarian option, controlling for the meat option's attractiveness and time trends:

$$Meat_{pt} = \alpha + \beta_1 \Delta Price_{pt} + X_{pt} + \tau + \epsilon_{pt} \tag{4}$$

Here, $Meat_{pt}$ is a binary variable equal to 1 if purchase p on day t is meat-based, and 0 if it is vegetarian. $\Delta Price_{pt}$ describes the price difference between the two options in the canteen where purchase p is made on day t.

Since price differences may correlate with other factors affecting meal popularity, I include controls X_{pt} to account for the attractiveness of the options on offer. Specifically, I use 55 binary control variables for the specific meat meal available on day t in the relevant canteen. For vegetarian options, I group meals into three categories (fried/breaded, oven-baked, and curry/stir-fry) and include controls for these categories.[28] I also control for the labeling intervention in the treatment canteen. Weekly and day-of-the-week time controls (τ) are included.

3.2.3 Data and results. For this analysis, I use student canteen consumption data from April 2022 to March 2023.[29] I restrict the sample to purchases made by students, as employees and external guests face higher prices. Additionally, I drop 5% of purchases that are not from the standard two

28. Among 64 vegetarian meals offered during the period, 19 are classified as fried/breaded, 9 as oven-baked, and 36 as curry/stir-fry.

29. This larger data set combines two data sets provided by the student canteen: one covering April to July 2022 (the main data set for this paper) and another covering August 2022 to July 2023 (see Klatt and Schulze-Tilling, 2024). Data from April to July 2023 is excluded due to another intervention during this period (analyzed in Klatt and Schulze-Tilling, 2024). Individual guests cannot be linked across the two data sets, and emissions data is only available for meals served from April to July 2022.

meal options offered on a given day (e.g., leftover dishes) and 0.4% of purchases from two canteen-day combinations with no clear main meat or vegetarian option. This yields one meat and one vegetarian main meal component price per canteen and day, resulting in a single relevant price difference driving meat versus vegetarian purchases.

Table 3 estimates specification 4. Col. (1) estimates the average response to an increase in the price difference between the meat and vegetarian options, serving as a proxy for a general meat tax. A €0.10 increase in the meat meal price relative to the vegetarian meal is associated with a reduction in demand for the meat meal by 1.1 percentage points. Col. (2) estimates separate effects for different meat types to refine the approximation of a carbon tax. The effect is largest for pork meals, moderate for beef, and smallest for chicken, while demand for fish shows little response.[30]

In the framed field experiment in section 2, the effect of carbon labels is similar to that of a carbon tax of €120 per tonne. To understand whether my effect estimates in the natural field experiment (section 3.1) yield a similar equivalence, I use my regression estimates to approximate the effect of a €120 per tonne carbon tax in the student canteen. I use two approximations. First, assuming a general meat tax, the average emissions difference between meat and vegetarian meals (1.2 kg) translates to a price increase of €0.14 (€$0.12 \times 1.2kg$). Using the Col. (1) estimate, this implies a 1.5 percentage point decrease in meat consumption (€0.14×-0.11).

Second, assuming a tax by meat type, the price increase varies by emissions: €0.08 for chicken (€0.12×0.7 kg), leading to a 0.3 percentage point decrease in demand (0.08×-0.04); €0.14 for pork (€0.12×1.2 kg), with a 3.5 percentage point decrease;[31] and €0.59 for beef (€0.12×4.9 kg), with a 5.3 percentage point decrease.[32] Weighting by the percentage of days each meat type is offered (chicken: 41.6%, pork: 30.1%, beef: 10.2%, fish: 18.1%), I estimate a total reduction in meat consumption of 1.7 percentage points.[33]

In section 3.1 I estimate in the canteen-level analyses that the carbon labels decrease meat consumption by 2 percentage points. This is in a similar ballpark as my above estimates of the effects of a €120 carbon tax, corroborating my result from section 2 that a €120 per tonne carbon tax in this setting produces similar effects as the carbon labels. For context, I also calculate price elasticities. A €0.10 price increase corresponds to 4.3% of the average meat meal price (€2.30). Based on Col.

30. Fish is almost exclusively served on Fridays, making it a routine choice for some students. Effects may also be influenced by pescetarians or fish's "healthy" image, reducing price sensitivity. For robustness, I also estimate specifications without added controls or with only time controls. These yield weaker demand responses, consistent with higher prices correlating with higher meal attractiveness (Table B.7 in the online Appendix).

31. €0.12×1.2 kg $\times -0.24$.

32. €0.12×4.9 kg $\times -0.09$.

33. Weighted calculation: $0.416 \times 0.3 + 0.301 \times 3.5 + 0.102 \times 5.3 = 1.72$.

(1), this leads to a 1.1 percentage point (2.5%) decrease in meat meal demand (44%), implying an own-price elasticity of approximately -0.6. This is lower than the -0.9 elasticity estimated for German households by Roosen, Staudigel, and Rahbauer (2022) but consistent with elasticities for younger, lower-income individuals, which range from -0.4 to -0.8, depending on meat type.

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

	Likelihood of consuming meat	
	(1)	(2)
	Grouping all meat	By meat type
Price difference (in €)	-0.11***	
	(0.01)	
Price difference (in €) x Chicken		-0.04***
		(0.02)
Price difference (in €) x Pork		-0.24***
		(0.02)
Price difference (in €) x Beef		-0.09**
		(0.04)
Price difference (in €) x Fish		0.01
		(0.02)
Treatment restaurant x Label period	-0.04***	-0.04***
	(0.01)	(0.01)
Treatment restaurant x Post period	-0.02***	-0.02***
	(0.00)	(0.00)
Treatment restaurant	-0.02***	-0.02***
	(0.00)	(0.00)
Weekly time controls	Yes	Yes
Control for exact meat meal	Yes	Yes
Control for veg. meal type	Yes	Yes
Observations	360,699	360,699

Standard errors in parentheses

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

Notes: Dependent variable: 0/1 indicator for consumption of the meat option. Linear probability regression using student canteen data from April 2022–March 2023. The variable "Price difference" describes the price difference between the main meat and vegetarian meal components. Both columns include binary controls for week, day-of-the-week, and the specific meat meal offered, as well as two binary variables for the vegetarian meal type. See Table B.7 in the online Appendix for an estimation without controls. Standard errors are robust.

4 Experiment 3: Behavioral channels

Why do consumers react to carbon labels? Experiment 3 explores two behavioral channels: (1) labels inform consumers about emissions, correcting misperceptions; and (2) labels increase attention toward emissions at the moment of choice. Subsection 4.1 describes the experimental design, subsection 4.2 the experimental data, and subsection 4.3 the estimation strategy and results.

4.1 Experimental design

Overview. Experiment 3 investigates two mechanisms behind the effectiveness of carbon labels:[34] (1) labels correct misperceptions by informing consumers about the emissions caused by different items, and (2) labels direct attention toward emissions during the decision-making process. To analyze these channels, I conduct a framed field experiment similar to Experiment 1 with two key differences:

(1) To assess the role of correcting misperceptions, I track participants' initial estimates of meals' carbon footprints and compare them with their reactions to carbon labels in the reduced-form analysis.

(2) To evaluate the role of attention, I include an experimental condition that increases attention toward carbon emissions without providing any emissions information. I estimate treatment effects for this condition in the reduced-form analysis.

In addition to these reduced-form analyses, I estimate a structural model quantifying the contribution of each channel to meal choices. This model and its estimation are presented in section 6.

Experiment timeline. The experiment timeline is visualized in Figure 9 and follows a structure similar to Experiment 1, with one key distinction. Instead of answering unrelated guessing questions as in Experiment 1, participants in Experiment 3 estimate the carbon footprints of various meals, including the meals on which they make purchasing decisions and six additional meals (Figure 11). Participants receive emissions information for a single reference meal (Red Thai Curry with pork and rice, emitting 1.7 kg of CO_2) to guide their guesses. An example guessing screen is displayed in Figure 10. These guessing tasks are incentivized and timed as in Experiment 1.[35]

34. See the introduction for a detailed motivation.

35. Participants answer each of the ten guessing questions on separate screens, presented in random order. The emissions of the reference meal are consistently displayed. See online Appendix C.5 for screenshots of the guessing instructions.

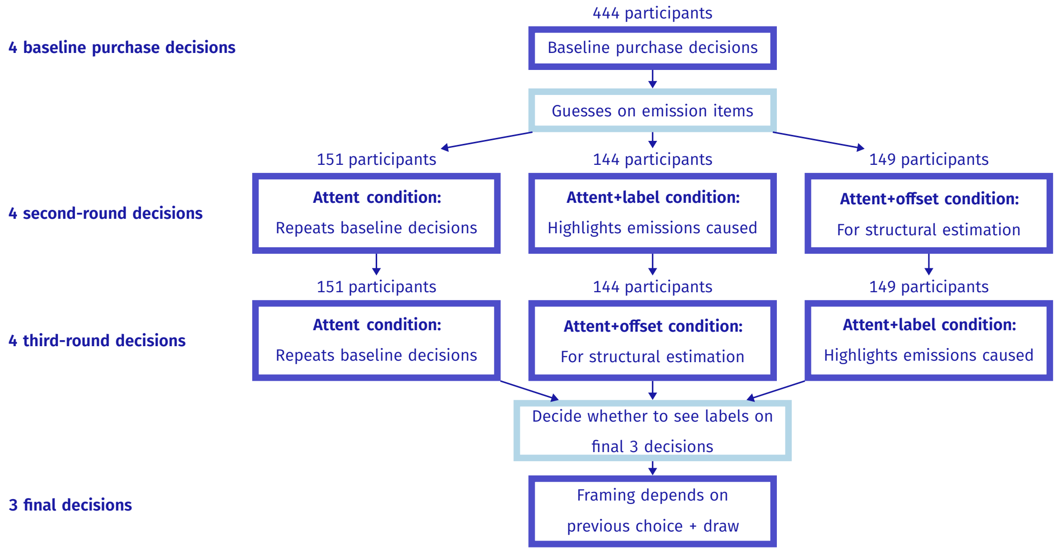


Figure 9. Experiment 3 schedule and treatment groups

The experiment then varies by treatment group, assigned via computer randomization. All participants indicate their WTP for the four meals, but decision framing and conditions differ:

- ATTENTION: WTP elicitation mirrors the baseline; however, prior carbon footprint guessing may enhance attention toward emissions during choices.
- ATTENTION+LABEL: Participants view carbon labels while indicating their WTP, providing both attentiveness and information (Figure 4).
- ATTENTION+OFFSET: Participants are informed that meal emissions will be offset, rendering choices carbon-neutral.[36]

To increase statistical power and gather additional insights, WTP for the same meals is elicited a third time,[37] with some treatment modifications:

- Participants in the ATTENTION+LABEL condition switch to ATTENTION+OFFSET and vice versa.
- Participants in the ATTENTION condition remain unchanged.

Subsequent procedures, the design of meal purchase decisions and the incentivization of WTP elicitation are as in Experiment 1.

36. Labels indicate carbon neutrality through CO_2 offsetting. The OFFSET condition is detailed in online Appendix C.4 and results are in Table A.7. This condition supports the structural estimation in Section 6 as discussed in Section 6.3.

37. Analyses control for third-round elicitations. Main results are consistent when using data from only the first two rounds.

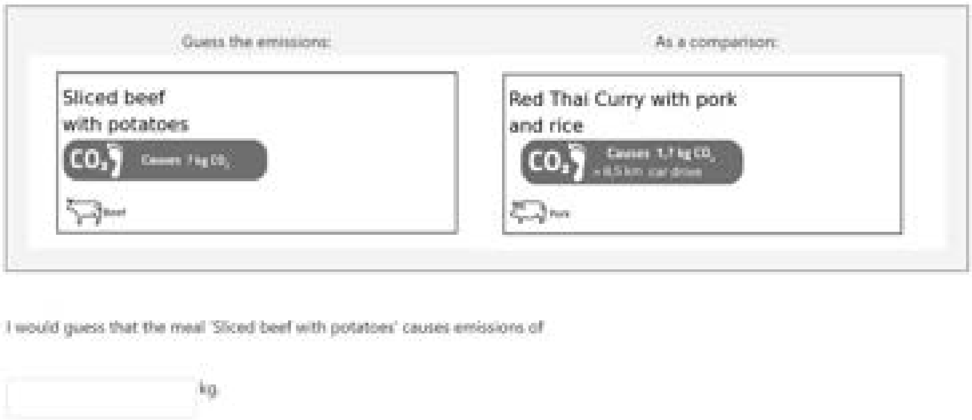


Figure 10. Example guessing questions

Participants and Set-Up. 476 participants from the BonnEconLab pool at the University of Bonn take part in one of 12 experimental sessions between June 22 and July 8, 2021. The experiment design, sample restrictions, and key analyses—including Figure 13 and Table 5—are pre-registered.[38] Participant recruitment and setup are as in Experiment 1.

4.2 Data

I exclude the 3% fastest participants and those failing the comprehension check after five attempts, as pre-registered. The remaining 444 participants are computer-randomized into treatments.[39] The sample is on average 26 years old, 55% female, 70% students, and 24% vegetarians. It is roughly representative of regular student canteen guests, as discussed in online Appendix A.2. Results are similar when restricting the sample to only (non-)students or (non-)vegetarians (Appendix A.7).

4.3 Estimation Strategy and Results

I analyze two main channels through which carbon labels may influence behavior:

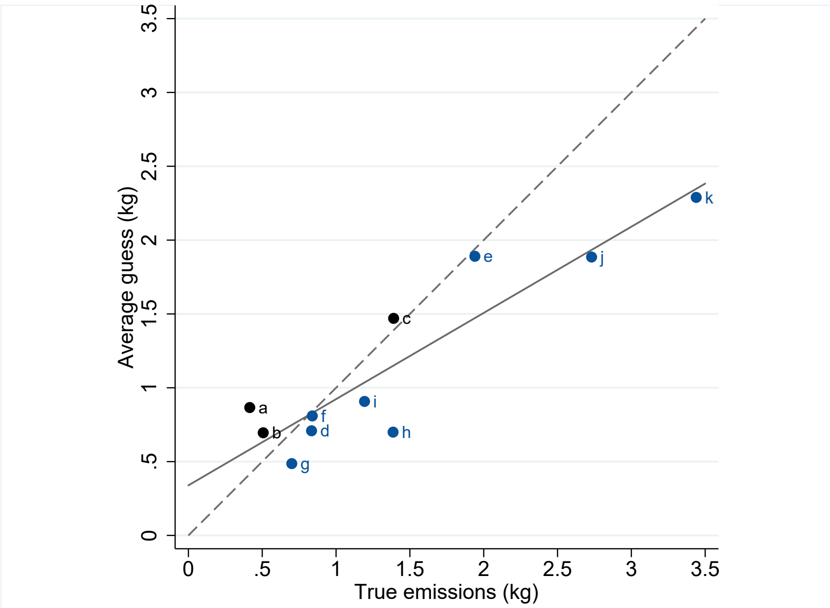
(1) Correcting misperceptions about carbon footprints.

(2) Directing attention to emissions at the moment of choice.

Descriptive statistics, estimation strategy and reduced-form results for each of these analyses are shown below.

38. See Schulze Tilling (2021b). Pre-registered main analyses are in Tables A.8 and A.9; Figure 11 was pre-registered as an additional analysis. Figure 12 was not pre-registered.

39. See online Appendix A.1 for a randomization check.



(a) Stir-fried veg.
(b) Vegetable pasta
(c) Chicken+rice
(d) Hash browns
(e) Kebap+rice
(f) Filled pasta
(g) Cheese sandwich
(h) Courgettes+fries
(i) Cheese pasta
(j) Spaghetti+meat
(k) Beef+potatoes

Figure 11. Average guess of the emissions caused by a given meal. Guesses are plotted against calculated emissions. Guesses closer to the dashed line are closer to calculated emission values. Meals corresponding to black scatter points are on average overestimated in their emissions, while meals corresponding to blue scatter points are on average underestimated. The dashed fitted line is described by $y = 0.39 + 0.57x$, with both the intercept and the coefficient significant at $p < 0.01$. Values are based on guesses made by the participants of Experiment 3, and 71 participants in the "Control, then Control" group in Experiment 1. The meal "Spaghetti with meat" was only guessed by the 71 participants of Experiment 1 guessing emissions. For each meal, the 10% most extreme guesses (in terms of deviation from the true emission value) are dropped. This leaves a total of 4,731 observations from 490 participants.

The Effect of Correcting Misperceptions. This section provides reduced-form evidence on whether treatment effects align with the correction of misperceptions as the main mechanism. The analysis draws on participants' carbon footprint guesses for different meals (step 2 in Figure 9, example in Figure 10). As descriptive evidence, Figure 11 shows that participants tend to underestimate emissions for high-emission meals (green dots) and overestimate emissions for low-emission meals (red dots).[40]

In the next step of the analysis, I combine individual and meal-specific treatment effects with participants' emission estimates for the respective meals. Using data from the ATTENTION+LABEL condition, I estimate:

$$\Delta WTP_{ijm} = \alpha Label_{ij} + \delta (Label_{ij} \times Under_{im}) + ThirdRound_j + \varepsilon_{ijm} \tag{5}$$

40. Further statistics on under- and overestimation appear in online Appendix A.11, including accuracy in meal ranking by carbon footprint.

where ΔWTP_{ijm} is the change in willingness to pay (WTP) for meal m by individual i in round j, relative to their baseline WTP for the same meal, as in Experiment 1.[41] Thus, the dependent variable directly captures subject- and meal-specific treatment effects for carbon labels.

$Under_{im}$ is an indicator for whether the individual underestimated the difference in emissions between meal m and the cheese sandwich. This is determined by comparing the individual's estimated difference in emissions with the true difference.[42]

$\alpha Label_{ij}$ captures the remaining effect of labels not explained by underestimation. $ThirdRound_j$ controls for whether round j is the third decision round.[43] This specification provides reduced-form evidence on whether correcting misperceptions is the main channel driving treatment effects. If it were, treatment effects should be proportional to a subject's underestimation of emissions.

Table 4.3, Spec. (1), presents OLS estimates of equation 5. Participants who underestimated meal m's emissions relative to the cheese sandwich decrease their WTP by an additional €0.13 when shown carbon labels. This suggests that part of the label effect stems from correcting misperceptions: learning that a meal has a higher carbon footprint than expected leads to lower WTP. Spec. (2) in Table 4.3 instead regresses the WTP change on the degree of underestimation (in kg). This specification suggests that carbon labels reduce WTP by €0.16 on average, with an additional €0.07 decrease per kg of underestimated emissions.

Strikingly, both specifications reveal a large negative constant term. In Spec. (1), labels reduce WTP by €0.10 even when emissions were not underestimated. In Spec. (2), this independent decrease is €0.16. Figure 12 further illustrates average effects split by prior under- or overestimation of emissions relative to the cheese sandwich. Participants also significantly reduce their WTP for meals where they had previously *over*estimated emissions. If correcting misperceptions were the sole mechanism at play, one would expect WTP to increase in such cases, not decrease. The observed pattern thus suggests a second mechanism—beyond misperception correction—driving treatment effects.

41. See Section 2.2 and online Appendix A.9 for details on the dependent variable.

42. This refers to the signed difference, not the absolute difference. For example, if a meal generates 0.2 kg more emissions than the cheese sandwich, but a participant estimates it to produce 0.3 kg less, this constitutes an underestimation. Results are similar when using only participants' estimates of meal emissions (Figure A.5 in the online Appendix).

43. This accounts for mixing data from second- and third-round decisions, as some participants experienced the ATTENT+OFFSET condition in round two and others in round three. Excluding third-round observations yields similar results (Table A.23 in the online Appendix).

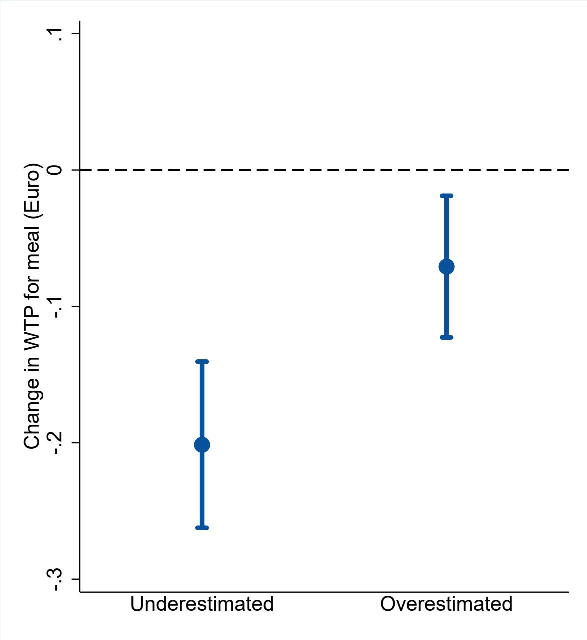


Figure 12. Within-subject change in WTP for meals when shown carbon labels, depending on previous estimation.
Notes: Bars indicate 95% confidence intervals.

Table 4. Within-subject change in WTP for meals when shown carbon labels, depending on underestimation.

	Change in WTP compared to baseline	
	(1)	(2)
Underestimated emissions × Shown label	-0.13^{***}	
	(0.04)	
Underestimation (in kg) × Shown label		-0.07^{***}
		(0.02)
Shown label	-0.10^{***}	-0.16^{***}
	(0.04)	(0.03)
Control for third round	0.05	0.07
	(0.05)	(0.05)
Participants	293	260
Obs. underestimate	555	515
Obs. overestimate	562	494
Observations	1,117	1,009

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

Notes: Analysis uses data from the Attention+Label condition. For each meal in spec. (2), the 10% most extreme guesses of the difference in emissions to the cheese sandwich (in terms of deviation from the true emission difference) are dropped. Standard errors are clustered at the individual level.

The Effect of Directing Attention. This subsection examines whether treatment effects can be explained by increased attention to carbon emissions. To estimate the magnitude of an attention effect, I analyze data from the Attention and Attention+Label conditions. Specifically, I estimate:

$$\begin{aligned}\Delta WTP_{ijm} &= \beta_1(Attent_{ij} \times High_m) + \beta_2(Attent_{ij} \times Low_m) \\ &\quad + \delta_1(Attent_{ij} \times Label_{ij} \times High_m) + \delta_2(Attent_{ij} \times Label \times Low_m) + ThirdRound_j + \varepsilon_{ijm} \qquad (6)\end{aligned}$$

where ΔWTP_{ijm} is defined as above. $Attent_{ij} \times High_m$ and $Attent_{ij} \times Low_m$ indicate whether a participant was made attentive to emissions for meals with a higher or lower carbon footprint than the cheese sandwich. $Label_{ij}$ is an indicator for whether individual i also saw carbon labels in round j. The interaction terms capture the additional effect of labels beyond mere attentiveness.

Table 5 presents the results, and Figure 13 illustrates average WTP changes in the Attention and Attention+Label treatments. Simply directing attention to carbon emissions reduces WTP for high-emission meals by €0.08 on average. Providing labels in addition further decreases WTP by €0.10 for high-emission meals. This effect in the Attention condition is primarily driven by decisions where participants already had a relatively accurate perception of the meal's emissions, as visualized in Figures A.6 and A.7 in the online Appendix. These findings suggest that increased attention alone accounts for a significant share of the treatment effect. Section 6 quantitatively assesses the relative contributions of attention and misperception correction to the label's overall effect.

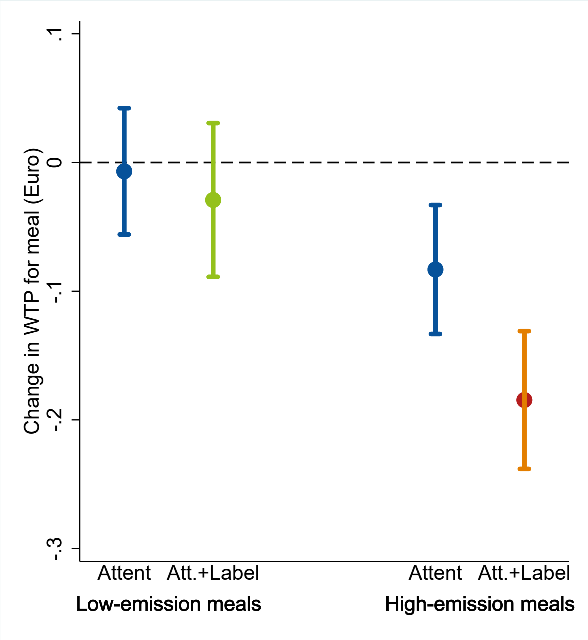


Figure 13. Within-subject change in WTP for meals in the Attention vs. Attention+Label condition.
Notes: Standard errors are clustered at the individual level. Bars indicate 95% confidence intervals.

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

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

Table 5. Within-subject change in WTP for meals in the Attention vs. Attention+Label condition

Notes: Analysis uses data from the ATTENTION and ATTENTION+LABEL condition. Standard errors are clustered at the individual level.

5 Consumer Preferences for Carbon Labels

This section presents experimental evidence on consumer preferences for carbon labels in consumption decisions. Section 5.1 examines findings from Experiments 1 and 3, while Section 5.2 discusses results from Experiment 2.

5.1 Evidence from the Framed Field Experiments

In Experiments 1 and 3, participants state their willingness to pay (WTP) for the presence or absence of carbon labels during their final consumption decisions. These elicitations are incentivized using a BDM mechanism.[44] About 50% of participants report a WTP of zero, indicating no strong preference. Fewer than 5% prefer labels to be absent (negative WTP). The remaining participants are willing to pay for labels, with 21% willing to pay €0.50 or more. WTP values are similar across

44. See online Appendix C.3 for details.

treatment groups, though slightly higher among those who had not previously encountered labels during the experiment.[45] The average WTP is 20.3 cents. While Thunström (2019) finds that calorie labels impose greater psychological costs on individuals with low self-control, I find no evidence of such a correlation, as shown in Table A.32 in the online Appendix. However, I find a weak positive correlation between preferences for the labels and perceived social norms for reducing carbon emissions in food choices, as well as between preferences for the labels and self-reported willingness to use info.

5.2 Evidence from the Natural Field Experiment

Following Experiment 2, student canteen guests participated in a follow-up survey assessing their preferences for permanent carbon label installation. As detailed in online Appendix D.8, the survey was framed as an opportunity for guests to provide feedback on various aspects of the canteen, with carbon labels being one of multiple topics. Carbon labels were not mentioned in the survey advertisement. Survey respondents were aware that their responses could impact student canteen policies, incentivizing them to report truthfully.

Among the 275 respondents who visited the treatment canteen at least once during the study period, 75% favored permanent label installation, 17% were unsure, and 8% opposed the measure. In contrast, a revenue-neutral carbon tax of unspecified magnitude,[46] was supported by 63%, with 14% unsure and 23% opposed. These results suggest that carbon labels enjoy greater support than carbon taxes, making their implementation more feasible.

6 Structural Model

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

In the model, a consumer chooses from a set of meals and selects the meal that maximizes her perceived utility. In general, the perceived utility of a meal may depend on a multitude of meal

45. See Figure A.8 for WTP distribution and Table A.30 for differences across treatments.
46. Survey participants were asked whether canteen prices should align with carbon labels (green-labeled meals being least expensive, red-labeled meals being most expensive).

attributes. The main attribute of interest in this model is the consumers' expectation of the carbon emissions caused by each meal. Ceteris paribus, the consumer has a higher valuation for a meal that causes fewer carbon emissions. How much the consumer cares about emissions depends on two parameters: the salience of carbon emissions at the moment of choice and the guilt the consumer perceives per kg of carbon emitted.[47]

6.1 Basic model

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

$$u(m) = v_m - p_m - \theta\gamma e_m. \tag{7}$$

Here, v_m is the *consumption utility* of meal m that is independent of emissions[48], p_m is the *price* of meal m, and e_m is the consumer's *estimate of emissions* caused by meal m at the moment of choice.[49]

The *salience* of carbon emissions $\theta \in [0, 1]$[50] and the consumer's *environmental guilt per perceived kg of emissions* γ jointly determine how much weight the consumer puts on carbon emissions when deciding.

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

$$e_m^{\text{info}} = (1-\kappa)e_m^{\text{true}} + \kappa e_m^{\text{prior}}. \tag{8}$$

Hence, the parameter $\kappa \in [0, 1]$ is a measure of the stickiness of the consumers' prior estimate of emissions, e.g. due to a lack of trust in the carbon footprint information provided.[51] If the con-

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

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

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

50. I hereby use a similar formulation as used in the literature on attentiveness to taxes and resource consumption (Chetty, 2009; DellaVigna, 2009; Byrne et al., 2024). In the framework of Bordalo, Gennaioli, and Shleifer (2022), a straight-forward reason why emissions might not be fully salient to consumers is a lack of prominence, as a meal's emissions are usually not (prominently) featured at the moment of choice.

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

sumer is *attentive* to emissions, this sets $\theta = 1$.[52] Introducing *carbon labels* makes the consumer both informed and attentive.

6.2 Extension to consumer welfare

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

$$u^{True}(m) \;=\; v_m - p_m - \gamma e_m^{\text{true}} \tag{9}$$

Accordingly, carbon labels increase the likelihood of the consumer choosing the meal m that maximizes her true utility.[53] If the consumer can make a choice $P \in 0, 1$ on the presence of carbon labels in her decisions, the *utility change she experiences from the presence of labels* is

$$u(P \;=\; 1) - u(P \;=\; 0) \;=\; u^{True}(m^L) - u^{True}(m^{prior}) + F \tag{10}$$

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

6.3 Identification of parameters

Experiments 1 and 3 represent a special case with binary choice set $\mathscr{M} = m, o$, where m is the meal option and o is the outside option (cheese sandwich). The WTP to exchange meals is given by:

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

The treatment conditions yield four equations with four unknowns:[54]

In the absence of any treatment, I assume $\theta \in [0, 1]$.

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

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

54. $v_m - v_o$ is treated as a single parameter; e_m^{prior}, e_o^{prior} are elicited, and e_m^{true}, e_o^{true} are known.

Baseline (No Treatment):

$$WTP^B = v_m - v_o - \theta\gamma(e_m^{\text{prior}} - e_o^{\text{prior}}) \tag{11}$$

The treatment condition ATTENTION directs participants' attention towards carbon emissions without providing information. Assuming this sets $\theta = 1$,

Attention Treatment:

$$WTP^A = v_m - v_o - \gamma(e_m^{\text{prior}} - e_o^{\text{prior}}) \tag{12}$$

Presenting carbon labels directs participants' attention towards carbon emissions, but also provides information on true carbon emissions. I assume this sets $\theta = 1$ and the participant updates as described in equation 8.[55]

Label Treatment (Informed and Attentive):

$$WTP^{A+L} = v_m - v_o - \gamma\left(\kappa e_m^{\text{true}} + (1-\kappa)e_m^{\text{prior}}\right) \tag{13}$$

In the OFFSET treatment, participants are informed that the carbon emissions caused by their choice, regardless of whether they choose the cheese sandwich or warm meal, will be offset. Assuming this sets $\theta = 1$, $e_o = 0$ and $e_m = 0$:

Offset Treatment (No emissions considered):

$$WTP^{A+O} = v_m - v_o \tag{14}$$

Finally, I assume that participants' WTP for the presence of carbon labels reflects their expectation of the true utility they obtain from the choice they make with carbon labels, m^L, versus the choice they make when merely attentive, m^A.[56]

WTP for the presence of labels:

$$WTP^P = \mathbb{E}[u(P = 1) - u(P = 0)] = \mathbb{E}[u^{True}(m^L) - u^{True}(m^A)] + F. \tag{15}$$

55. In Experiment 3, participants seeing carbon labels experience the LABEL treatment on top of the ATTENTION treatment. I assume the ATTENTION+LABEL, LABEL and ATTENTION treatment all set salience $\theta = 1$, without any additional attention-directing effect occurring from a combination of attention and labeling treatments. This assumption is in line with a comparison of effect sizes across experiments 1 and 3, where I see similar to larger treatment effects in the LABEL treatment in Experiment 1 than in the ATTENTION+LABEL treatment in Experiment 3 (Table A.7 in the online Appendix).

56. Allcott and Kessler (2019) and Butera et al. (2022) take a similar interpretation of their experiment participants' WTP to experience interventions. I assume that introducing participants to the concept of carbon labels and asking them for their WTP to see them will make them attentive to carbon emissions, so the relevant figure a participant will consider is choices with labels compared to choices when merely attentive.

Rewriting these equations for structural estimation (Appendix A.14), I estimate parameters via GMM, assuming γ, κ, and θ are homogeneous across participants. To mitigate the influence of outliers, I exclude, for each meal, the 10% of observations corresponding to the most extreme 10% of $e_m^{\text{prior}} - e_o^{\text{prior}}$ values.

6.4 Results

Estimated parameters are similar across the basic and extended model. I estimate θ at 16%, implying that participants behave as if carbon footprints are only 16% of their true size. This estimate is not statistically different from zero, suggesting emissions might not factor into decisions at all absent intervention. κ is estimated at 0.21 and insignificant, indicating that participants indeed take into account the emissions information provided on the labels. γ is estimated at €0.12 per kg CO_2 and significant at the 1% level. F is estimated at €0.21 fixed psychological benefit from seeing the labels and also significant at the 1% level. Estimates are similar across different specifications (See Table A.33 in the online Appendix).

I simulate how consumers would make choices under different types of canteen interventions, using their stated meal preferences, prior emissions estimates, the estimated behavioral parameters θ,γ, and κ, and assuming a typical student canteen offer and pricing structure.[57] The interventions I consider are:

(1) Knowledge intervention: Providing emissions information without increasing attention. Note that this is a fictional intervention for the purpose of decomposing the treatment effects of the labels. In practice, it is not possible to provide information without increasing attention.

(2) Attention intervention: Increasing attention without providing emissions information.

(3) Label intervention: Combining both knowledge and attention effects.

(4) Carbon tax: Pricing emissions at €120 per ton with lump-sum redistribution.

(5) Meat ban: Eliminating meat options from the menu.

Table 6 presents simulation results. The label intervention reduces emissions by 34g per meal, outperforming both attention (27g) and knowledge (4g) interventions. The carbon tax yields similar reductions but with greater dispersion in consumer welfare effects. The meat ban produces the largest emissions reduction but also the only net welfare loss.

The final four columns of Table 6 estimate how consumer welfare changes under each of the interventions. I find that carbon labels improve welfare by an average of 0.18 ¢ per meal, or 10 ¢

57. Details are shown in online Appendix A.14.4.

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

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

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

per meal affected by the intervention. The carbon tax also increases welfare but to a smaller extent, while the meat ban decreases welfare.

As a robustness check, I relax the assumption that the Attention and Label treatments purely increase salience and correct misperceptions without imposing costs. Instead, I assume they double all psychological costs consumers incur due to causing carbon emissions. This assumption increases the estimated psychological cost to 5¢ per choice (see online Appendix A.14.5). In this scenario, welfare losses occur even for choices unaffected by labels, as psychological costs rise regardless of behavior. However, the estimated 21¢ psychological benefit from labels (based on consumers' WTP for their presence) still outweighs these costs, leaving the net welfare effect positive, though smaller than in the baseline case. For carbon taxes, the adjustment results in a slight welfare loss, driven by a lower estimated environmental guilt parameter γ.[58] With lower perceived guilt per kg of emissions, shifting to lower-emission foods has a less positive effect on consumer welfare than in the baseline model.

7 Discussion

This paper provides causal evidence from a student canteen setting that carbon labels influence consumption behavior. I estimate that carbon labels reduce emissions by approximately 4%, a figure that

58. This is a mechanical result of the assumption that θ is set to 2 in the Attention and Label treatments, see model specification above.

might intuitively be dismissed as “too small”" to warrant policy attention. However, my findings show that achieving similar reductions through a carbon tax would require a price of €120 per tonne—– more than 150% above current EU ETS trading prices and three times the German carbon tax on gasoline. Both of these policies have faced substantial political resistance, whereas behavioral instruments, including carbon labeling, typically encounter less opposition (see, e.g., John, Martin, and Mikołajczak, 2023). Surveys conducted in the student canteen further confirm the relatively high acceptability of labels. These results suggest that emission reductions from behavioral interventions should be evaluated not in isolation but relative to what is politically feasible. While a 4% reduction in emissions alone is likely insufficient to address the climate crisis,[59] it represents a meaningful reduction within the scope of politically viable policies and in the context of the well-documented difficulty of shifting food consumption patterns (see, e.g., Guthrie, Mancino, and Lin, 2015).

More generally, my results highlight the importance of considering the effectiveness of single policies not in isolation, but relative to what is achievable with alternative policies. The methods I employ can also be applied to other consumption contexts and interventions to decrease carbon emissions. Quantifying effect sizes in tax-equivalent terms can help us understand the policy relevance of other behavioral interventions aimed at decreasing carbon emissions, such as changes in menu or meal positioning or information interventions.

My experiments also shed light on the behavioral mechanisms driving carbon labels' effectiveness. I find that while some of the observed effect can be attributed to correcting misperceptions about carbon footprints, a much larger portion stems from increased salience of carbon emissions at the moment of choice. This suggests that attention frictions play a significant role in limiting carbon-conscious decision-making in the absence of labels. While attention constraints have been shown to hinder sustainable behavior in energy and resource consumption (Allcott and Taubinsky, 2015; Taubinsky and Rees-Jones, 2018; Tiefenbeck et al., 2018), my findings extend this insight to food consumption. My results imply that the effect of carbon labels may extend beyond the target behavior, potentially generating attentional spillovers as described by Nafziger (2020). Future research should examine the scope and implications of such spillovers, particularly in domains where attention constraints influence decision-making.

Consumer support for carbon labels was strong across all three experiments. Fewer than 10% of respondents expressed opposition to labels, and participants revealed an average willingness to

59. Estimates for the social cost of carbon range from 50 USD (€49) per tonne and lower (some scenarios in Barrage and Nordhaus, 2024), €160 per tonne (e.g. Rennert et al., 2022), to substantially higher estimates (Bilal and Känzig, 2024; Moore et al., 2024). A carbon tax of €120, or a policy yielding similar effects, might thus be insufficient as a stand-alone policy to combat climate change.

pay of 20.3¢ per meal for their presence. Structural estimation indicates that carbon labels increase consumer welfare, both through their impact on choices and through independent psychological benefits. A carbon tax of €120 is also found to increase consumer welfare, but by a smaller amount. These results suggest that carbon labels are preferable to the €120 per tonne tax in the studied setting. While previous research has identified that consumers with low self-control in eating behavior are emotionally taxed by calorie labels (Thunström, 2019), I do not find similar evidence for carbon labels. However, such patterns may differ among other population groups, leaving room for future research.

The relevance of carbon labels as a policy tool is particularly pronounced in the food sector, where carbon taxes remain uncommon. For instance, the agricultural sector is excluded from the EU ETS trading scheme, and Dechezleprêtre et al. (2022) identify agricultural policies as among the least politically viable carbon mitigation measures. In this context, alternative interventions are especially needed. The student canteen setting is a particularly promising application: In Germany, 2.9 million individuals were classified as students in 2021 (Federal Statistical Office (Germany), 2023), with approximately 54% dining in student canteens at least once a week (Federal Ministry of Education and Research (Germany), 2023). My results also offer suggestive guidance for other relevant contexts, including corporate cafeterias, restaurants, and grocery retailing.

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